AN ANALYSIS OF THE CONCEPT OF RETRODICTION

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ABSTRACT

AN ANALYSIS OF THE CONCEPT OF RETRODICTION

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In this dissertation, the concept of retrodiction is subjected to scientific, philosophical, and logical analysis in terms of the concepts of reversibility, irreversibility, t-invariance, t-non-invariance, trace, and the uniformity of nature.

It is argued that retrodiction is related with the distinction between reversibility and irreversibility of physical processes in the sense that the earlier states of reversible processes can be inferred from their later states by t-invariant equations, whereas the inference is extremely limited by t-non-invariant equations in the case of irreversible processes.

Since it is commonly believed that traces have an indispensable function in retrodiction, different accounts of traces are examined and rejected as philosophically implausible. Hence it is concluded that there is not a privileged class of things as traces that are distinguishable from other physical things.

The analysis conducted on the possibility of retrodiction suggests that there is a close connection between retrodiction and uniformity of nature, for, under the non-uniform conditions, retrodiction from any temporal position does not result in the same description, for the same regularities, boundary conditions and other physical evidence may not be available at every temporal position. So retrodiction as an inference from later to earlier states of the systems is not a sufficient account of retrodiction, since it presupposes strict uniformity. This conclusion emphasizes also the significance of the present in retrodictive inference.

The logical analysis consists of the examination of the logic of retrodiction in comparison with the logic of explanation and prediction. Retrodiction is distinguished from explanation and prediction by its direction. The overlap between explanation, prediction, and retrodiction that results from earlier accounts is solved by distinguishing prediction-in-the-past from explanation and retrodiction from present to the past from explanation that proceeds from the past to the present. As for the logical structure of retrodictive inference, it is concluded that retrodictive inferences are of either deductive nomological or inductive-statistical type.

Keywords: Retrodiction, Explanation, Prediction, Reversibility, Irreversibility, t-invariance, t-non-invariance, Trace, Uniformity, .

ÖZ

SONRADAN KESTİRİM KAVRAMININ ANALİZİ

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Bu tezde, tersinirlik, tersinmezlik, zamanın yönündeki değişime duyarsızlık, zamanın yönündeki değişime duyarlılık, iz, doğanın tekdüzenliliği kavramları kullanılarak sonradan kestirim kavramının bilimsel, felsefi ve mantıksal bir analizi yapılmıştır.

Sonradan kestirimin fiziksel süreçlerin tersinirliği ve tersinmezliği ayırımı ile ilişkili olduğu (bir başka deyişle, zamanın yönündeki değişime duyarsız eşitlikler aracılığıyla, tersinir süreçlerin erken durumları geç durumlarından kestirilebilirken, zamanın yönündeki değişime duyarlı eşitliklerle tersinmez süreçlerin erken durumlarının geç durumlarından kestiriminin çok sınırlı olduğu) iddia edilmektedir.

İzlerin, sonradan kestirimde vazgeçilemez bir rolü olduğu görüşü çok yaygın olduğundan, iz kavramını tanımlayan bir çok görüş incelenmiş, ancak tümünün de felsefi olarak savunulamaz olduğu görülmüştür. Bu nedenle, 'iz' olarak tanımlanabilecek ve diğer fiziksel nesnelerden ayırt edilebilecek özel bir nesne sınıfının olamayacağı sonucuna varılmıştır.

Sonradan kestirimin olanaklılığı üzerine yürütülen analiz, bu kavram ile doğanın tekdüzenliliği kavramı arasında çok yakın bir ilişki olduğunu göstermektedir. Çünkü aynı fiziksel düzenliliklerin, sınır koşullarının ve diğer fiziksel kanıtların her anda mevcut olmaması durumunda (tekdüzenli olmayan koşullar altında), farklı anlardan geriye doğru kestirim aynı sonucu vermez. Böylece, sonradan kestirimi, sonraki bir pozisyondan önceki bir pozisyona yapılan bir kestirim olarak değerlendirmek katı bir tekdüzenliliği varsaydığından, sonradan kestirimi ifade eden yeterli bir görüş olarak kabul edilemez. Bu sonuç, sonradan kestirimde bugünün öneminin vurgulanması bakımından önemlidir.

Yürütülen mantıksal analizde, sonradan kestirimin mantığı açıklama ve önceden kestirimin mantığıyla ilişkili bir şekilde incelenmiştir. Sonradan kestirim diğerlerinden ters yönde olması ile ayırt edilmiştir. Önceki görüşler sonucunda, açıklama, önceden ve sonradan kestirme arasında ortaya çıkan örtüşme sorunu, geçmişte-önceden-kestirim kavramını açıklama kavramından ve bugünden geçmişe doğru kestirimi de geçmişten bugüne doğru ilerleyen açıklamadan ayırtederek çözülmüştür. Ayrıca, sonradan kestirimin mantıksal yapısının ise tümdengelimsel-nomolojik veya tümevarımsal-istatistiksel türde olduğu sonucuna varılmıştır.

Anahtar Sözcükler: Sonradan kestirim, Açıklama, Önceden kestirim, Tersinirilik, Tersinmezlik, Zamanın yönündeki değişime duyarsızlık, Zamanın yönündeki değişime duyarlılık, İz, Tekdüzenlilik.

To My Son, Erkin Onat

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INTRODUCTION

When I first started to write a dissertation on the possibility of knowledge about the past, what I had in mind was an analysis of historical explanation, because I, as many others, thought that historical inquiry comprises only historical explanation. However, I later realized that before some past event can be explained it must first be discovered, i.e. reconstructed from the present evidence.

Discovering the past events as opposed to explaining them has been named postdiction or retrodiction. The distinction between retrodiction and explanation has, in fact, been noted by several authors, such as Hempel, Reichenbach, Grünbaum. But, as pointed out by Hempel, even some distinguished philosphers, namely Scriven and Rescher, confused retrodiction with explanation. It is, nevertheless, surprising to note that, contrary to Hempel's making a distinction between explanation and prediction and, furthermore, to his extensive analysis of the philosophy and logic thereof, Hempel spared only very little space for the analysis of retrodiction. It is also surprising that the most thorough analyses of retrodiction in literature are related with topics on time, entropy, reversibility and irreversibility. Nevertheless, even these analyses are not full in extent. Hence it appears that retrodiction has attracted little attention compared to its significance in philosophy of science and history.

Probably as a result of this neglect, its relationship with explanation and prediction, and its connection with reversibility, irreversibility, t-invariance and t-non-invariance of theories has not been sufficiently analyzed. In addition, its connection with uniformity of nature is left untouched. Although the significance of the so-called traces as retrodictive indicators has been repeatedly pointed out, the analysis of traces is made only in commonsensical terms, except the entropic account thereof. Therefore, in order to fill this gap, I intended to conduct an extensive analysis of retrodiction that covers its scientific as well as logical and philosophical aspects.

Our analysis begins with reversibility and irreversibility in chapter 2 because of their intimate connection with retrodiction. This chapter also includes the explication of t-invariant and t-non-invariant theories and their relationship with reversibility and irreversibility. Accordingly, chapter 3 is devoted to the concept of time which is related with both physical processes and retrodiction, since retrodiction is an inference with temporal dimension. In this chapter, the direction of time, related strictly with retrodiction, is also examined.

The first section of chapter 4 starts with a preliminary examination of the logic of retrodiction in comparison with explanation and prediction, with a special emphasis on the direction of retrodictive inference. This section is expected to provide the conceptual framework for the subsequent sections. The concept of trace, that is claimed to have the key role in retrodiction, is analyzed together with the critical examination of the earlier views in section 4.2. In the third main section, I analyze the conditions of retrodiction as an inference from later to earlier states of systems. For this purpose, three main groups of physical systems, namely classical mechanical, classical and statistical thermodynamic systems and the systems involving generation and destruction of things, are employed.

Although the objects of retrodiction may be quite recent events they are generally the events of the remote past that demand the consideration of the uniformity of nature. Thus in the fourth main section, I review and discuss the idea of uniformity to help to understand how it is related to retrodiction. If nature is not uniform in the most strict sense the time of retrodiction, thus, the present from which the past is inferred, becomes relevant. Hence I examine the idea of the present and the past, and the conditions of retrodiction from the present to the past in the fifth section.

Finally, in section 4.6. I analyzed the logical structure of retrodiction in order to see whether it fits into any of the logical models offered for explanation and prediction.

In addition, I provide three appendices that are important in understanding retrodiction. The first gives the logical models offered for prediction and explanation. The second is Watanabe's technical treatment of retrodiction for statistical systems. The third is about restricted laws that are important for retrodictive inferences in a non-uniform world.

As a final note, I deliberately exclude the examination of retrodiction for systems involving human conduct, because although I think that it is similar in essence, the notions, such as 'will,' 'action,' 'purpose,' 'ends,' and so on, complicate the matter in such a radical way that it goes beyond the scope of this study.

CHAPTER 2

REVERSIBILITY AND IRREVERSIBILITY

2.1. Reversibility

The reversibility concept is used to refer to some mechanical and thermodynamic processes as well as to a certain psychological behavior. However, in order to begin, a temporary definition, which is intended to refer only to physical (natural as well as artificial) systems, need be given: *A physical system is reversible if and only if it returns (or is returnable) to its initial state from a later state*. According to this definition, the motion of a simple pendulum constitutes a typical example (assuming no friction) since it attains its initial state periodically. However, it should be reminded that reversible processes need not necessarily be periodic, for there are other kinds of motions that are non-periodic but reversible. For example, the motion of a billiard-ball on a billiard-table is reversible (assuming that the billiard-ball and the billiard-table constitute a conservative system) since if a moving billiard-ball is given the same velocity in the opposite direction it follows the same path and reaches its initial position.

Reversibility can also be illustrated by a motion picture played in reverse. When a process is filmed and played in reverse, if the reverse playing displays an acceptable order of events the process is reversible, if not, the process is not reversible. For example, if the motion of a pendulum or a billiard-ball on a billiard-table is filmed and played in reverse it appears perfectly normal.

In addition to the reversible mechanical processes, there are some thermodynamic processes that also exhibit reversible behavior under specific conditions, e.g., Carnot cycle which can be described briefly as the following. The figure below represents a heterogeneous system (an idealized steam engine) of water and steam contained in a cylinder at a temperature T_1 under a movable piston. The piston is moved so as to allow the volume to increase isothermally and reversibly. The pressure, on the other hand, remains equal to the vapor pressure p_1 at T_1 due to evaporation of some water. In the p, V diagram below when the system follows the horizontal line 1-2 a quantity of heat Q_1 is extracted from the reservoir during which the temperature T_1 remains constant. At state 2, wall impermeable to heat is slipped around the cylinder and the volume is allowed to increase further adiabatically. From then on the temperature drops and volume continues to increase until state 3 at which temperature has dropped to T_2. Here the wall is removed to open the cylinder into the reservoir which causes a quantity of heat to be absorbed by the reservoir and the system moves along 3-4 at constant temperature T_2 and pressure which is equal to the vapor pressure p_2 with the decrease of the volume. When state 4 is reached, if the compression is continued adiabatically point one is reached and the cycle is complete.

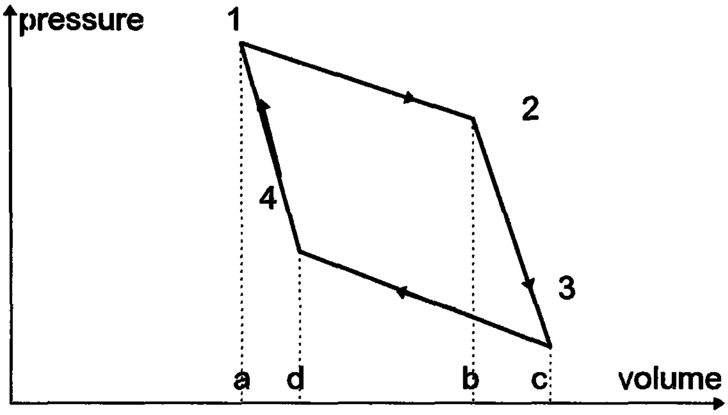

Figure 1: Carnot's Cycle

I should remind that reversibility as described so far does not suggest reversal of time as such because time continues to flow during the reversal of the processes. Popper (1974) stated this while discussing the asymmetry of time.

> Let there be a physical system *S* which undergoes a completely reversible physical process *P*, beginning at time t_1 and ending at the time t_2. Let us reverse this process at t_2: this means that after a certain time *S* will revert to the state it was in at t_1; it will not mean that the time has returned to t_1; when the reversed process comes to an end the time will be t_3, where, if the reversal was perfect, both processes took the same amount of time, $t_3 - t_2 = t_2 - t_1$. But the time will have elapsed (or moved, or run down, or whatever metaphor you like best) *without* running back, or returning in the opposite direction, or changing its "arrow". (p. 1141)

Furthermore, according to Mellor (1981), "Reversing natural processes means reversing the temporal order of *types* of events, not of *tokens* of those types" (p. 173; italics are ours). It appears then that reversibility has a close relationship with scientific theories, for types are accounted for by theories. The theories that describe reversible processes are said to be time reversal invariant (hereon t-invariant). The examples of t-invariant theories are, "the basic theories, quantum theory and relativity, [which] treat all event sequences as being reversible and therefore the theories in themselves ... offer no distinction between 'time forward' and 'time backward' " (Denbigh, 1981, p. 5).

Tolman (1938, pp. 102-104) endeavored to establish that classical mechanics is t-invariant by demonstrating that the standard Lagrangian equations of classical mechanics do not change when t is replaced by $-t$. Tolman considered an isolated conservative mechanical system of f degrees of freedom with the coordinates q_i and the generalized velocities $\dot{q}_i$. The behavior of the system can be described by the Lagrangian equations as the following.

$$\frac{d}{dt}\frac{\partial L}{\partial \dot{q}_i}-\frac{\partial L}{\partial q_i}=0 \qquad (i=1, 2, 3, \dots, f) \qquad (1)$$

The equation (1) can also be written in the form

$$\frac{d}{d(-t)}\frac{\partial L}{\partial(-\dot{q}_i)}-\frac{\partial L}{\partial q_i}=0 \qquad (i=1, 2, 3, \dots, f) \qquad (2)$$

For equations (1) and (2), with q_i as a function of time, there will be a solution in the form

$$q_i=\phi_i(t) \qquad (i=1, 2, 3, \dots, f) \qquad (3)$$

$$q_i'=\phi_i(-t) \qquad (i=1, 2, 3, \dots, f) \qquad (4)$$

Finally, in order to explain the above considerations, Tolman exemplified the theoretical issues as follows:

> Let us—for the sake of specificity—designate the possible behavior described by (3) a *forward motion* of the system and the behavior described by (4) as the corresponding *reverse motion*, and let us consider two systems of the kind under consideration, S and S', one carrying out the forward motion and the other reverse motion. (p. 103)

At $t=0$, the coordinates of S and S' would be in agreement, i.e., $q_i'(0)=q_i(0)$, whereas the velocities would be in reverse directions, i.e., $\dot{q}_i'(0)=-\dot{q}_i(0)$. At t = t, the coordinates and velocities become $q_i'(t)=q_i(-t)$ and $\dot{q}_i'(t)=-\dot{q}_i(-t)$ respectively. Hence,

> Corresponding to any possible motion of a system of the kind mentioned, that there would be a possible reverse motion in which the same values of the coordinates would be reached in the reverse order with reversed values for the velocities. (pp. 103-104)

Reichenbach (1956) also illustrated t-invariance of classical mechanics by a different set of examples. He stated that the laws of physics have functional relationships between variables in the form of $x_{n+1}=f(x_1...x_n)$ which "can be solved for any of the variables contained in it; for instance, we can write $x_1 = g(x_2...x_{n+1})$, g being a function resulting from f according to rules for mathematical equations" (p. 28). This means in general that if x is a function of y, y is a function of x, and thus the laws that have this form are symmetrical.

However, it may be objected that since most of the laws of physics are causal laws and the relationship between cause and effect is asymmetrical then the above consideration is fallacious. Reichenbach (1956) considered this objection but rejected it on the basis of the fact that the functional relationship which is symmetrical is compatible with the time directed causal relation.

> It is a familiar logical theorem that from a serial relation we can always construct a symmetrical relation. Similarly, we define a relation of causal connection as follows:
>
> DEFINITION. An event *A* is *causally connected* with an event *B* if *A* is a cause of *B*, or *B* is a cause of *A*, or there exists an event *C* which is a cause of *A* and *B*. (p. 29)

Reichenbach then pursued with the law of Boyle-Mariotte for perfect gases which is stated as $p.v = R.T.M / m$, where p is pressure, v volume, m molecular weight, M mass, T temperature, and R a constant. The law "does not tell us which change is the cause of which other change, or which quantity changes first so as to make the others vary" (p.29). Yet, he said, in the actual experiments, it is always possible to distinguish the cause from the effect. Therefore the directedness of causal relation is compatible with the symmetric nature of the laws.

However, Reichenbach was aware of that these laws, though abundant in science, describe relations and "refer merely to causal connection, without defining a direction or an order" (p. 29). But there are also two other kinds of laws that "describe physical processes, and do not merely lay down causal connections" (p. 29). He stated that the first group of laws describe mechanical processes which are reversible and the second describe thermodynamical processes which are irreversible. The former are symmetric in the sense that they are invariant with respect to the sign of time whereas the latter are asymmetric because they are of non-invariant nature. Below, I give a detailed report of Reichenbach's analysis of time invariance.

Suppose Q is a process which is described in positive time, t. Q can be described in negative time, t', which is related to t by the relation $t' = -t$ (i.e., the converse description). Furthermore the converse description may be regarded as a description in terms of positive time. "The question arises ... whether the new description denotes a process Q^* which is compatible with physical laws. If it is, we may say that the process Q is reversible, and call Q^* its reverse process. If it is not, we say that Q is irreversible" (p. 30). The example provided by Reichenbach is quite illustrative:

> Let the process Q consists in a ball being thrown from A to C (fig. 2) in the direction of the solid arrow. Its motion is given by the equations
>
> $$x_1 = a_1 t - (1/2)gt^2, \qquad x_2 = a_2 t, \qquad (4)$$
>
> where a_1 and a_2 are the components of the velocity with which the ball is thrown from A. Introducing negative time t' by the relation $t = -t'$, we find
>
> $$x_1 = -a_1 t' - (1/2)gt'^2, \qquad x_2 = -a_2 t'. \qquad (5)$$
>
> This is the description of Q in negative time. We now construct the description of the reverse process Q^* of Q by omitting the prime mark in (5):
>
> $$x_1 = -a_1 t - (1/2)gt^2, \qquad x_2 = -a_2 t. \qquad (6)$$

> Here t is, as in (4), positive time. Obviously, the process described in (6) is compatible with the mechanical laws and does occur; it is exemplified by a ball which is thrown from *C* to *A*, in the direction of the broken arrow. Throwing a ball is a reversible process. (p. 30)

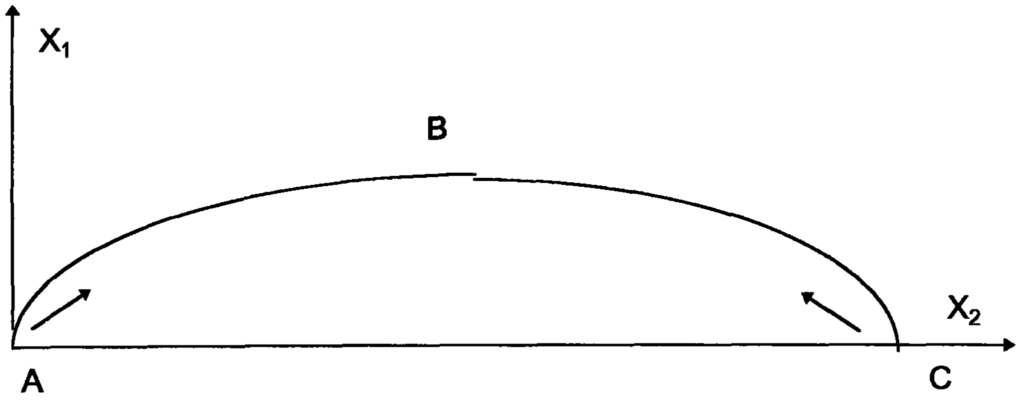

Figure 2: Path of a ball thrown from A to C. (Reichenbach, 1956, p. 31)

Finally, as affirmed also by Reichenbach, it may be said that classical mechanics is t-invariant because "the differential equations of mechanics are of the second order without first time derivatives. If $f(t)$ is a solution of such an equation, then $f(-t)$ is likewise a solution, because in the double differentiation a double multiplication by $d(-t)/dt = -1$ occurs, which leaves the result unchanged" (pp. 30-31). However, the question as to whether this fact establishes t-invariance of classical mechanics beyond any doubt is not obvious, for there have been serious objections which I discuss in section 2.3. below.

2.2. Irreversibility

We have seen above that a reversible system is that which returns to its initial state. *An irreversible system, on the other hand, is one that cannot* *(be) return(ed) to its initial state.* According to Denbigh (1989), "it [irreversibility] is best defined as the negation of reversibility which is an idealized and limiting kind of process, not capable of being fully realized" (p. 506). Thus "irreversibility is a matter of degree" (Ibid.) judged against this idealized process. Grünbaum's (1974) definition of an irreversible process is quite illustrative. "By an 'irreversible process' (*à la* Planck) we understand a process such that no counter-process is capable of restoring the original *kind* of state of the system at another time (p. 775)." We can again appeal to the motion picture example to illustrate irreversibility in common-sense terms. If lighting a match is filmed and played in reverse the film displays an awkward order of events following each other, e.g. charcoal and smoke concentrate to form an unburned match. So it can be said that a match burning into charcoal and smoke is an irreversible process. Perhaps, a better and often cited example may be formulated in terms of heat diffusion processes: if a metal rod hot and cold at opposite ends is left by itself completely isolated the warm end will cool while the cold end will warm up until the rod comes to an equilibrium. This process is irreversible because it is impossible[1] that one of the ends becomes hot and the other cold spontaneously in this isolated condition.

Historically, irreversibility was first defined in terms of macroscopic quantities, such as temperature and pressure by the second law of macrothermodynamics which states that the total entropy of a closed system increases with time. "In physical terms this means that if we encounter a closed system in non-equilibrium on a given occasion, then it will be close to equilibrium on another occasion sufficiently temporally separated from the first in one direction of time only" (Davies, 1975, p. 12).

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[1] It is possible, though overwhelmingly improbable, in statistical mechanical interpretation of thermodynamics.

Later, Boltzmann supplied the statistical mechanical interpretation of the second law in which he derived the second law from the laws of Newtonian particle mechanics together with the assumption of molecular chaos[2]. The application of the Newtonian particle mechanics to an idealized gas can be explicated as follows (Grünbaum, 1964): the molecules in an idealized gas can be defined by their positions and corresponding velocities in three dimensional space. To each molecule correspond total of six position and velocity values which are used to define the micro-state of the gas at any time where these six points describe "the cells of a six-dimensional position-velocity space or 'phase space'. And each of the n molecules will then be in some one of the finite number m of cells compatible with the given volume and total energy of the gas" (p. 237). Here, though the arrangement of the molecules depends upon the specific positions of each molecule, the macroscopic state of the gas at any time is independent of specific arrangements of the molecules but depends solely on the number of fast ("hot") and slow ("cool") molecules in this particular cell at the given time.

> Thus the macro-state depends on the numerical spatial and velocity distribution of the molecules, not on the particular *identity* of the molecules having certain positional and velocity attributes. It follows that the *same* macro-state can be constituted by a *number* of different *micro-states.* (Ibid., p. 238)

What is significant here is that "the number of micro-states W corresponding to a macro-state of near-equilibrium (uniform temperature) or high entropy is overwhelmingly *greater* than the number corresponding to a disequilibrium state of *non*-uniform temperature or quite low entropy" (Ibid.). In this way Boltzmann's interpretation transformed the second law into a

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[2] This assumption can be stated as follows: the positions and velocities of the particles are uncorrelated before they collide.

probabilistic form: entropy increase of a closed system with time is overwhelmingly *probable.* This is also known as Boltzmann's H theorem, H being a quantity "which depends upon the way in which the molecules are arranged" (Davies, 1978, p. 69) and is "related to the entropy S by the relation $S = -kH$, so that an entropy increase corresponds to a decrease in H" (Grünbaum, 1964, p. 239). This equation shows an inverse relation between S and H which means that H is negative entropy.

Loschmidt, Zermelo and Poincaré "questioned Boltzmann's derivation of *irreversible* behavior from fundamentally *reversible* laws of mechanics" (Davies, 1975, p. 12), which is called the reversibility objection. Loschmidt pointed out that,

> The *H* theorem as stated must be incorrect, because it contradicts the principle of microreversibility on which it is based. Because each individual collision is reversible, for every set of motions which decreases *H,* there will be a corresponding set which increases *H,* obtained from the set by reversing all the particle velocities simultaneously. This 'time reversed' system would then pass backwards through a reversed sequence of states; *H* cannot decrease in both cases" (Davies, 1977, p. 56).

Loschmidt's objection disclosed the paradox of explaining macroirreversibility in terms of microreversibility since most of the mechanical processes are reversible.

Another objection to Boltzmann came from Zermelo whose argument is based on Poincaré's *recurrence theorem* which states that "any mechanical system with a finite number of degrees of freedom approaches any state through which it goes an infinite number of times at a given approximation" (Costa De Beauregard, 1965, p. 313). In other words, "in an isolated system, *any* state will be revisited to arbitrary closeness an infinite number of times" (Davies, 1977, p. 56). Even though Poincaré's recurrence time is enormously high ($10^{10^{23}}$ years) it permits the occurrence of all low entropy states with very low probabilities. This means that "in a totally isolated system, anything that can happen, will happen, and infinitely many times!" (Davies, 1978, p. 71). Furthermore Zermelo contended that this fact could not be accorded with Boltzmann's claim that, in an isolated system, it is overwhelmingly probable that a low entropy state will be followed by a high entropy state. However, Ehrenfests showed that there is no incompatibility between Boltzmann's H-Theorem and the above objections

> When it is remembered that (i) the low entropy states to which the high relative probabilities of subsequent increase are referred are usually at the low point of a trajectory at which changes back to higher values are initiated, and (ii) the Boltzmann H-theorem therefore does not preclude such a system's exhibiting decreases and increases of S with equal frequency" (Grünbaum, 1964, p. 242).

This is shown schematically in the figure below:

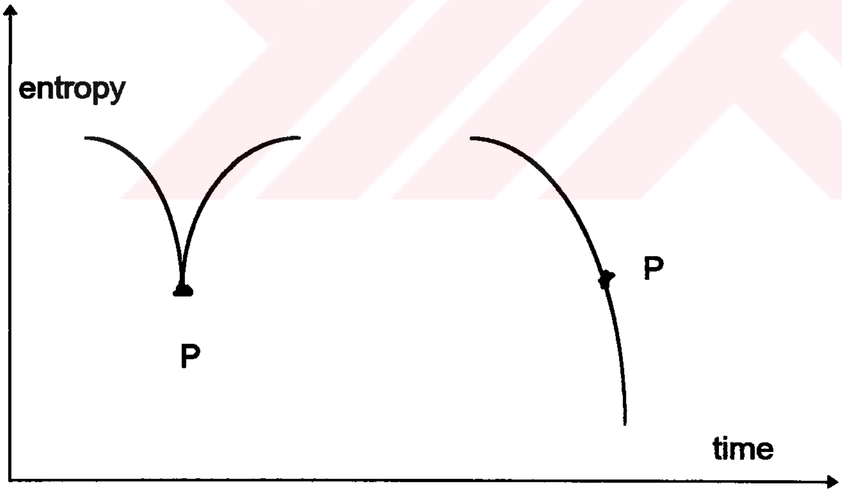

Figure 3: (Davies, 1975, p. 13)

The figure above shows that "should we encounter the system with an entropy value less than the maximum, it is far more likely that this value represents a minimum in a small fluctuation rather than the start (or finish) of a much larger one" (Davies, 1975, p. 13).

Thus the behavior of the system can be illustrated as showing consecutive higher and lower entropy states that is illustrated in Fig. 4.

Figure 4: The entropy curve of a closed system.

It can be observed in figure 4 that the entropy curve displays a series of quite small fluctuations indicating that entropy decreases as frequently as it increases. The interpretation of this fact is that "it is highly probable that a *low* entropy state will soon be followed by a high one. But it is no less true that a low entropy state was *preceded* by a state of high entropy with equally great probability" (Grünbaum, 1964, p. 242). However, on extremely scarce instants, a major fluctuation appears, which means a very significant drop of entropy. It is interesting to note that any randomly selected fluctuation will, with high probability, be a minor one rather than a major one, and therefore that the lower entropy state will mean an entropy minimum which is immediately followed by an entropy state close to the maximum. Hence, Boltzmann's H-Theorem can be saved with the above accounts and an additional proviso: "the affirmation of a high probability of a future entropy increase must not be construed to assert a *high* probability that low entropy values were *preceded by still lower* entropies *in the past* " (Grünbaum, 1964, p. 242). As Von Weizsäcker suggested

> In the absence of other grounds to the contrary, the statistical entropy law itself provides every reason for regarding a present ordered state in a system as a *randomly achieved* low entropy state rather than as a veridical trace of an actual past interaction. (in Grünbaum, 1964, p. 243)

However, the last statement, though a direct consequence of the above contention, is contradictory to our intuition that a lower entropy state always develops from much lower entropy state. In other words, our experience that the present lower entropy state has developed from still lower entropy seems to contradict the statistical formulation of the second law that a low entropy state was preceded, with very high probability, by a higher entropy state. This is a paradox which is also pointed out by Grünbaum (1964, pp. 281-282): for example, if a half destroyed footprint is found on a lonely beach, the common interpretation of this event is that footprint was in a better form earlier than the encounter. However, it should follow from the discussion on the statistical interpretation of the second law that the footprint was originally formed at this half obscured state because any lower entropy state constitutes an entropy minimum. Boltzmann seems to have predicted such objections and answered in advance.

> The fact that actually a transition from a probable to an improbable state does not occur as often as the opposite one, may be sufficiently explained by the assumption of a very improbable initial state of the universe around us. As a consequence, a given system of bodies entering into interaction is found, in general, in an improbable state. (in Reichenbach, 1956, p. 132)

According to Costa de Beauregard (1965), "The problem is thus an epistemological one, raising such questions as: can a finite system be considered as perfectly isolated when long time intervals are considered?" (p. 314).

The above paradox was solved by Reichenbach's (1956) theory of *branch systems,* according to which, branch systems are subsystems "that branch off from a comprehensive system and remain [quasi] isolated from then on for some length of time" and later they merge again with the larger system (p. 118). At the time of formation, entropy of such a branch system is relatively low which starts to increase as soon as it separates off from the larger system and reaches its entropy maximum. In the case of the footprint, the beach forms the larger system which is separated into a branch system when the footprint forms: the footprint + beach system forms a quasi-closed system whose entropy starts to increase as soon as the footprint forms.

Branch systems form both naturally and artificially by human intervention. (Both of these cases and other possible cases can be followed in Figure 5 below.) As for the naturally occurring branch systems consider a rock embedded in snow:

> [During day time] the system consisting of the sun and the rock increases its entropy because of the absorption of radiation by the rock. At night the rock and snow together form a system which, because of it inner temperature differences, has a low entropy; and the cooling of the rock and melting of the snow represent a transition to higher entropy. (Reichenbach, 1956, p. 118)

As for the artificial systems, on the other hand, dropping pieces of ice into a glass of water constitutes a good illustration. In the refrigerator + energy source + ice cubes system, entropy increases because ice cubes form at the expense of energy consumption; the glass of water + the room system, on the other hand, is in equilibrium (maximum entropy). When ice cubes are dropped into water the ice + water system whose entropy is low branches off from the wider systems and the entropy of the system starts to increase.

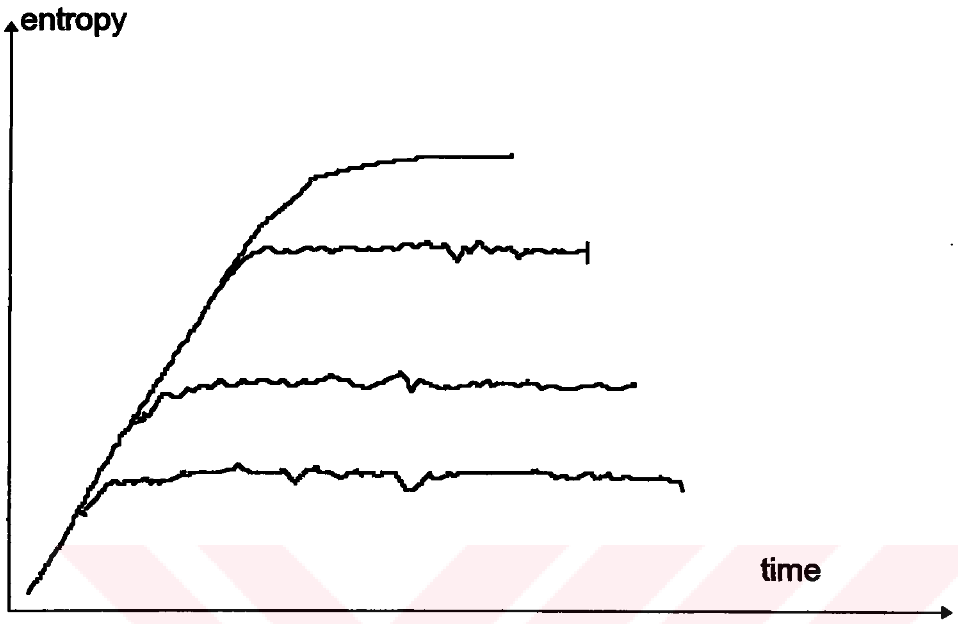

Figure 5: The initial upgrade of the entropy curve of the universe. Some isolated systems branch off, remaining for an infinite time. (Reichenbach, 1956, p. 119)

2.3. Non-entropic (Mechanical) Irreversibility

In this section, I examine mechanical irreversibility from three respects: expansion processes, the systems including non-conservative forces, and (ontological and epistemological) imprecision and unstability of equations of motions.

The discussion on the irreversibility of expansion processes was initiated by Popper (1956a).[3] He formulated his thesis of classical mechanical irreversibility by help of a simple example. When a stone is dropped into water, concentric water waves occur. Since this process is a classical mechanical processes, the reverse of expanding water waves, i.e., advanced waves, is theoretically possible by definition, for classical mechanical processes are time symmetrical which is evident from the invariance of classical mechanical laws with respect to change from $+t$ to $-t$.[4] However, formation of advanced waves is causally, i.e., physically, impossible because it would require a vast number of coherent oscillators to generate the contracting waves.

> If we look more closely at the way in which these oscillators are controlled so as to make them cohere, we find that they must, essentially, be controlled from one centre from which some signal—for example, a light signal—must run to the places where the oscillators are situated.
>
> Thus although the elementary classical physical processes involved in an expanding wave are reversible, we find that in order to obtain ... the contracting wave, we always need a wave that spreads from a centre. (Popper, 1958, pp. 402-403)

It should be noted that Popper did not claim that generation of contracting waves is impossible but he rather asserted that, in order to generate the advanced waves, the oscillators must be controlled from a common center, otherwise “causally unconnected oscillators can never be made to cohere” (Popper, 1957, p. 1297). But then we are forced back to the same problem, that is, reversing the signal that provided the coherence between the oscillators, which leads to an infinite regress.

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[3] Popper (1956b) admitted that Einstein had used a similar argument half a century ago.

[4] The wave equation, $(\partial^2 u / \partial x^2) + (\partial^2 u / \partial y^2) + (\partial^2 u / \partial z^2) = (1/c^2)(\partial^2 u / \partial t^2)$, is invariant under time reversal.

Hill and Grünbaum (1957)[5] attempted to develop Popper's thesis into a general principle. They contended that Popper's thesis of *de facto* irreversibility of expansion processes is true for open systems[6] without the spontaneity condition[7]: if the system were open then generation of the contracting waves would not be possible even by coherent oscillators because waves coming from infinity would lead to a position similar to that in Kant's fallacious First Antinomy: "the requirement that a process which has been going on for all infinite past time must have had a finite beginning (production by past initial conditions) after all" (as stated in Grünbaum, 1964, p. 268).

I now examine irreversible mechanical systems that contain forces that change with time and velocity, i.e., non-conservative forces. Schlegel (1956) claimed that certain solutions of Newton's second law do not result in reversibility. He based his argument on the following definition of time reversal. If equations of motion give symmetrical solutions for the normal and inverted motions of a particle for the same time interval then these equations are time reversal invariant (normal indicating the positive direction of time). He accepted that the second law, as expressed $F = md^2x/dt^2$, is reversible "if it is integrated with an F that is a function of x" (p. 381). However, if F is a function of t, i.e., if F changes with time, reversibility does not occur.[8] He finally concluded that "motions under various kinds of physical forces must be individually examined for their reversibility properties" (p. 382).

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[5] Grünbaum discussed this issue in more detail in his 1964 and 1974.

[6] They asserted that an open system is the system which is not confined by an outer wall in a finite region (p. 1296).

[7] Grünbaum (1964 and 1974) later claimed that Popper's thesis is not true for all finite systems even on the spontaneity condition because it is possible to produce nonspontaneously the initial conditions for advanced waves by dropping a large circular object onto the water surface that can start contracting waves.

[8] The full description of his argumentation is given in his essay.

Mehlberg (1961) also agreed with Schlegel that "there is nothing in Newton's laws of motion to prevent the asymmetrical occurrence of the time variable in mechanical magnitudes other than acceleration—for example, in a force whose value or direction varies as time passes" (p. 112). The crux of the point made by Schelegel and Mehlberg is that if force changes with time reversibility does not occur.

An alternative explanation was offered by Bosworth (1958) to account for the irreversible mechanical processes: irreversibility appears “by means of parameters which have only a statistical significance” whereas non-statistical classical mechanical interpretation results in reversibility due to the fact that the parameters defined within classical mechanical theory “give a complete description of the path of every particle of the system” and that the laws of motion are t-invariant (p. 402). However, Popper (1958) responded that irreversibility does not result from statistical interpretation which, he asserted, is obvious from the example of expanding waves which are elementary classical physical processes.

Popper (1956b) also argued against Schlegel’s (1956) account of irreversibility of mechanical processes that considering reversibility as the reversal of a single particle would not be sufficient because the whole system of forces must also be taken into account even in reversing a single particle because "in a classical model, the motion of one planet or of one particle cannot be reversed" (p. 382). In order to justify his point, Popper (1956b) considered the following case:

> If we reverse the velocity of one of the planets, at the time t_f and at the position of x_f, the planet will clearly not reverse its path precisely: upon reaching, at the time $t_f + \Delta t$, the position $x_f + \Delta x$, which we may assume to be very nearly the same as the one which it occupied at $t_f - \Delta t$, the planet will not be acted upon by the same force which acted upon it at the time $t_f - \Delta t$. If, however, we reverse the motions of all the planets in the system, then the force will be the same; the system is reversible. (p. 382)

Davies (1977) also considered the same problem for three possible cases of the irreversible behavior of non-quantum mechanical systems that appear to be irreversible. The first is "the motion of a charged particle with charge *e* in a magnetic field B. Such a system is described by the Lorentz law of motion $m\ddot{r} = e\dot{r} \times \mathbf{B}$ which changes under time reversal to $m\ddot{r} = -e\dot{r} \times \mathbf{B}$" (p. 24). The charged particle in a plane perpendicular to B follows a path (the arc of a circle) with a velocity *v*. If the velocity is reversed the particle does not travel backwards along the same path but rather follow another arc within the same plane.

The second is the motion of an object through a viscous medium or on a surface exerts friction. Frictional force is proportional to the velocity of the object, $\dot{r}$. Hence the equation of motion $m\ddot{r} = f(r)$ becomes $m\ddot{r} = f(r) - a\dot{r}$(here *a* is a positive constant). For damped motion, if the latter equation is integrated (for the cases that $f\ (r)$=0) the equation becomes $|\dot{r}| \propto e^{-(a/m)t}$ where $|\dot{r}| \to 0$ as $t \to \infty$. In the reverse case, i.e., when *t* is replaced by -*t*, the following equation obtained $|\dot{r}| \propto e^{(a/m)t}$. According to Davies, the resulting reverse cases "are clearly unphysical as they correspond to a body being spontaneously accelerated to an infinite velocity as a result of their contact with a viscous or frictional medium" (p. 25).

The third case is the motion of a charged particle coupled to its own electromagnetic field which is shown by the following equation $m\ddot{r} = f(r) - \frac{2}{3}e^2\dddot{r}$.

> In [this] equation ... the third term represents the damping, but depends on the *third* derivative of *r* with respect to the time. The loss in kinetic energy of the particle appears as retarded electromagnetic radiation flowing away into space. The opposite situation, in which the $\dddot{r}$ damping term is positive, corresponds to the convergence of advanced radiation onto the charged particle, and does not seem to occur in nature. (p. 25)

However, Davies did not conclude, in virtue of the above examples, that classical mechanics is t-non-invariant, but rather he offered an explanation similar to Popper's (1956b). He contended that the analysis of the concept 'the system' would reveal that it is in the construction of the system that make classical mechanics appear asymmetrical. If the system is construed as the whole system encompassing the processes that produce the interactions that a body involve, instead only of the body and its interaction with the media in which the body is emplaced irreversibility vanishes. As for the first example above, if the field is also reversed by reversing the charges that produce the field the asymmetry disappears.

In the case of the second example, if the kinetic energy dispersed into the environment as heat is considered in terms of individual atoms which can also be reversed (because atomic behavior is governed by t-invariant laws) and if the motion of the atoms are reversed this will cause "a cooperative transfer of momentum to the large body, which would then become exponentially accelerated" (p. 26).

In the third example, according to Davies, radiation streaming from the charged particles is assumed to be absorbed by the walls of the laboratory. If however, the atoms of the wall are also reversed they will give energy to the charged particles which eventually form an advanced radiation wave that will contract onto the charged particle.

Recently, Hutchison (1993) evaluated the question as to whether classical mechanical theory is t-invariant. This is similar to Schlegel's (1956) case. He attempted to demonstrate that classical mechanics does not necessarily give reversible solutions. But he did not conclude that classical mechanics is t-non-invariant but rather that it is neutral to reversibility and irreversibility.

To start with, Hutchison considered the oscillations of a particle hanging on a spring. It can be said that if the particle is pulled down it periodically returns to its initial position. Furthermore it is also a fact that classical mechanics provides the sufficient grounds to calculate the particle's route to its original position when the velocity of the particle is reversed. However, when examined closely enough it becomes evident that "we will see that its motions are neither periodic nor reversible, for their amplitude decreases with time" (p. 309). But the theory construes an idealized situation for perfect springs neglecting certain influences so that "theoretical particles on theoretical springs behave *like* real particles on real springs, but they also behave *unlike* them as well" (p. 310). Therefore the question is whether the irreversible behavior of real springs can be simulated (by a classical mechanical model) so as to predict the particle's slowing down in time. According to Hutchison, "[m]uch of the literature seems to say no here, for it insists that if a physical system is being simulated by a classical mechanical model, then the temporal evolution of the model cannot be anything but reversible" (p. 310). But against this, Hutchison insisted that "[i]rreversibility is a thoroughly normal feature of mechanical models" (p. 310). He provided two simulations of the motion of the particle hanging on a spring: the first is reversible and the second irreversible.

> Suppose the particle has mass m kg, and that, t seconds after release, its position is $x(t)$ meters above its equilibrium position. Then the reversible motion can be well modeled by presuming that the spring exerts a force of $k{\cdot}x(t)$ newtons downwards (k being a constant). Given the initial position of the particle, $x(0)$, and the initial velocity $\dot{x}(0)$, it is easy to find the later positions and velocities of the particle by solving the equation of motion provided by Newton's second law, *viz.*: $m{\cdot}\ddot{x}(t)+k{\cdot}x(t)=0$. Simple calculation shows this motion is reversible. (p. 311)

In this simulation, no external forces and detailed boundary conditions related to the spring are included. In addition, since the equation that describes the behavior of the spring has only time's second derivative ($\ddot{x}$ means d^2x/dt^2) it can be solved in both directions of time. As for the second simulation, on the other hand, conditions are changed.

> To simulate the irreversible motion, we need only make a small change to the above computation, for we can suppose that air resistance and the 'imperfection' of the spring, etc. exert yet another force on the particle, now proportional to its velocity, but in the opposite direction, say $-K{\cdot}\dot{x}(t)$, where K is another constant. We now have to solve a slightly different equation of motion, viz.: $m{\cdot}\ddot{x}(t)+K{\cdot}\dot{x}(t)+K{\cdot}x(t)=0$. When the simple calculation is done, the new solution can be easily seen to represent irreversible motion.

As it is evident, when other forces are also included the first derivation of time ($\dot{x}$) is added to the equation that makes the equation t-non-invariant. Hutchinson contended further that it is the active forces that are included which make the system reversible or irreversible. Furthermore, as for the nature of the forces, he stated that "the motions will be reversible if the forces depend only on geometric configurations; but when the forces vary with time, or the velocities of the interacting components, then irreversible motion results if the dependence is asymmetric" (p. 311). Nevertheless classical mechanics is capable of accounting for the irreversible as well as the reversible behavior. "So it is evidently wrong to interpret the claim that classical mechanics is reversible as if this meant that all mechanical systems were reversible" (pp. 311-312). Along with the same line of thought, Hutchison maintained that the second law of thermodynamics does not impose upon the systems irreversibility, it is also neutral with regard to reversibility and irreversibility. For "on its own it cannot tell us that things are irreversible. Indeed, the second law would not automatically be false in a universe so simple that all processes were reversible: *e.g.*, a universe containing only two Newtonian particles" (p. 313).

The crux of the matter that is emphasized by Hutchison is that reversibility and irreversibility are *not* in the theory but rather *in* the system that is described by the theory. This becomes evident when the influence of the environment and boundary conditions are taken into account. As for the example of the particle oscillating on a spring, it is said that the spring shows an irreversible behavior when the air resistance and other forces are included. A proponent of the reversibility would say either that these forces are neglected or that if all the molecules of air are also reversed the theory would account for the behavior of the spring as reversible (as, for example, Popper and Davies would say). However, it can be claimed that enlarging the system to include air molecules does not invalidate the irreversibility of the original case. It only supports Hutchison's assertion that irreversibility is "there in the system." Different systems can have different behaviors that can be accounted for by classical mechanics.

Against the view that, since the theories and simulations are only idealizations, some of the forces must be ignored Hutchison asserted that the same arguments can be used in favor of irreversibility. Suppose the resistance of the real air is replaced by an air simulator and the irreversible behavior of the spring is calculated. Now the proponent of the reversibility does not have the right to object that with regard to the real air the oscillation of the spring is in fact reversible because the irreversible behavior of the spring with the air-resistance simulator is only an idealization as the reversible behavior of the spring in the previous paragraph.

> The irreversibility is there in the system, whether or not it is present in the world that the calculation is intended to emulate, and whether or not our objector's belief about the cause of air resistance is correct. ... the objection is based on a confusion between characteristics of a theory, and characteristics of the world the theory is dealing with. (p. 315)

Furthermore Hutchison maintained that "the seemingly reversible Newtonian law of gravitation is [no] more correct than the second law of thermodynamics. It too can be reduced to a more fundamental theory, general relativity, and this reduction casts similar doubt on the absolute validity of the Newtonian law" (p. 316). He contended, therefore, that the requirement of absolute validity of t-non-invariant laws to accept irreversibility is not justified.

Hutchison also rejected the arguments asserted in favor of reversibility of classical mechanics. He considered Tolman's (1938) interpretation of the standard Lagrangian equations to show reversibility. He stated that "Tolman has presumed, in his choice of Lagrangian, an absence of the sort of forces that are known to produce irreversible motions. His result proves *at best* that if the forces acting are independent of time and velocity (etc.), then the motions are reversible" (p. 318). Furthermore if Tolman's assumptions as regards the nature of the forces is made explicit it appears that "it is assumed that forces acting between the particles depend only on the geometric configuration of the system" (p. 318). But this assumption does not hold for velocity-dependent forces as shown above. Thus he repeated that "an ontological presumption is the source of the alleged reversibility, not the laws of mechanics" (p. 318).

Savitt (1994) also considered the question as to whether classical mechanics is t-invariant in a reply to Hutchison (1993). He distinguished three senses of time reversal invariance.

(i) Time reversal invariance$_1$: Suppose that some set of laws, *L*, of a scientific theory, *T*, are differential equations that are functions of time, *t*, and let a time reversal transform be defined as the mapping *t* : $t \rightarrow -t$. "*T* is *time reversal invariant*$_1$ if and only if every solution of *L* is mapped to a (not necessarily distinct) solution of *L* under the time reversal transform, *t*" (p. 908).

(ii) Time reversal invariance$_2$:

> Suppose that the laws, *L*, of *T* concern some set of properties (or parameters) of a system, *S*, such as the positions or momenta at some time *t* of the set of particles making up *S*. A *state* of a system, S_k (relative to theory *T*), is some specification of values for all parameters of the components of the system. A sequence of states of a system is *dynamically* possible (relative to theory *T*) if the sequences of states $S_i \rightarrow S_f$ (indicating that S_i is before S_f) is consistent with the laws of *T* (is 'a permissible solution of the equations of motion' of *T*). Finally, let $(S_k)^R$ be the *time-reversed state* of the system S_k. (p. 910)

From the conditions specified above, "A theory *T* is *time reversal invariant*$_2$ under the following circumstances: a sequence of states $S_i \rightarrow S_f$ is dynamically possible (relative to the laws of *T*, of course) iff $(S_f)^R$ $(S_i)^R$ is dynamically possible (relative to *T*)" (p. 910).

(iii) Time reversal invariance$_3$: "A theory, *T*, is *time reversal invariant*$_3$ iff S_i evolves to S_f, according to *T*, then $(S_f)^R$ must evolve to $(S_i)^R$" (p. 911).

According to Savitt, Hutchison's definition of an irreversible process as a process the reverse of which is never witnessed is problematic, because the fact that the reverse of the process is never witnessed does not prove the claim that classical mechanics is time reversal non-invariant. Savitt admitted that the systems that involve dissipative forces appear to be irreversible according to Hutchison's definition. For example, as we have seen, Hutchison considered the damped motion of a particle moving through a viscous medium and concluded that the process is irreversible because time occurs in the first derivative and the reverse of the process is never witnessed. However, Savitt claimed on the basis of Davies' argument that the equations describing such motions are time reversal invariant$_2$ for, although the reverses of the motions seem "unphysical" (because they require that when the individual atoms in the medium be also reversed the particle in question be accelerated exponentially) it is dynamically possible (as appears from definition (ii) above). But Savitt accepted that no indeterministic theory would satisfy definition (iii). Therefore he claimed that "Hutchison's arguments show at most that CM [classical mechanics] is not time reversal invariant$_3$, but provide no support for the claim that CM is not time reversal invariant$_2$" (p. 912).

Hutchison (1995b) responded to Savitt that his criterion of reversibility "requires more than mere conformity to the general laws of classical mechanics, adding an additional insistence that the reverse motion also conform to the particular interactions that compose the dynamical system under consideration" (p. 342). Furthermore he claims that, for Savitt, reversibility from B^R to A^R is accomplished even when the forces in the system are not the same as the forces acting during the process from A to B; however, for him, the quality and quantity of the forces must remain the same during the forward and reversed motions.[9]

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[9] Hutchison also claimed that Savitt has not supported his view (that time reversal invariance$_3$ is the correct definition) with a proof. In addition, the literature is also not in favor of his criterion.

Callender (1995) also questioned Hutchison's view that time-dependent non-conservative forces must be taken into account in deciding whether classical mechanics is t-invariant by claiming that there are no non-conservative forces. The reason beyond this bold claim is Callender's distinction between 'phenomenal' and 'fundamental' forces. If it can be shown that non-conservative forces are of phenomenal type which can be reduced to fundamental forces then there would be no reason to base time 'non-invariance' of classical mechanics on 'secondary' forces because "when asking if the universe is TRI [time reversal invariant], we desire to know whether it is *at bottom* TRI. We make an ontological assumption" (p. 333). The ground of Callender's conviction that non-conservative forces are not genuine is the general belief in the global conservation of energy which suggests that dissipation of energy as heat during a damped motion (under frictional force which is a non-conservative force cited by Hutchison) is not lost but conserved within the larger system of which the damped motion is part. If considered in terms of classical statistical mechanics frictional force becomes the interaction of atoms which is accounted by t-invariant laws and are reversible (as explained by Davies (1977)). Similarly, according to Callender, there are no genuine non-conservative forces that cannot be reduced to particle positions and interparticulate forces because these forces supervenes on these fundamental variables. However, he admits that if there were a non-conservative force that does not supervene on fundamental variables then Newton's second law would not be denominated as the fundamental equation of classical mechanics. In such a case, a more fundamental law that joins the second law and this novel force would be the fundamental, but t-non-invariant, law. However, Callender challenged Hutchison that he has not provided any evidence that there are such irreducible non-conservative forces. Instead, Hutchison asserted that the possibility of non-conservative forces is sufficient. But Callender claimed that such possibility is not philosophically and physically interesting because we had known it all along that there are irreversible forces in other possible worlds. "The interesting question is whether classical mechanics is TRI in our world" (p. 337).

I have not so far explicitly considered non-deterministic (classical or non-classical) mechanical systems as regards irreversibility. According to Hutchison (1993), Tolman's (1938) conclusion that classical mechanical processes are reversible because differential equations describing these processes are t-invariant "is only valid if there is no other solution to the differential equations with the same initial conditions" (p. 319). He asserted that if the differential equations had more than one solution for the same set of initial conditions (that is, if they are indeterministic laws) the processes described by these equations cannot be said to be reversible. Furthermore he stated that the claim that a system is both reversible and non-deterministic is contradictory because a non-deterministic system can evolve from a state into different states but if it is reversible it must reverse into the same state from which it has evolved (p. 319). However, as we have seen above, Savitt (1994) charged Hutchison of having too a restrictive criterion for reversibility. By Savitt's time reversal invariance$_2$, and Callender's (1995) definition of time reversal invariance[10], a system can be both non-deterministic and reversible because dynamic possibility of the occurrence of the reversed state is sufficient.[11]

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[10] "(TRI) A process evolving from state I to state F is TRI iff it is dynamically possible according to the laws of nature for the image of F to evolve to the image of I after a temporal reflection that maps t: t→–t." Callender (1995), p. 332.

[11] We should note that if this rather relaxed definition of time reversibility were not accepted non of the classical statistical and quantum mechanical systems would probably be reversible because the reverse processes in these systems do not necessarily lead back to exactly the same physical conditions. (See Reichenbach, (1956, pp. 207-211), for a detailed analysis of reversibility of quantum mechanical processes.) Furthermore we should also note that Hutchison (1993) did not argue that non-deterministic theories are t-non-invariant. On the contrary, he distinguished between t-invariance of a theory and the reversibility of the processes that are described by this theory. That is, according to Hutchison, non-deterministic theories can be t-invariant.

Finally, I start to examine imprecision as a source of mechanical irreversibility. According to Hutchison (1995a), “the inexactitude (either ontological or epistemic) of physical parameters ... is certainly not built into the foundations of our mathematical physics” (p. 221). However, he reminded that quantum mechanics, “where the ‘uncertainty’ of every measurement is part of the official interpretation” and chaos theory, “where extreme sensitivity to initial conditions is a standard doctrine” are exceptions to this assessment (Ibid.). Classical mechanics, on the other hand, is claimed to presuppose an ontology of exactness, i.e., there is a one-to-one correspondence between physical quantities and numbers. Nevertheless Hutchison also asserted that this fact is not a final decree from above.

> Though classical mechanics is normally set out in a conceptual context which presumes an ontology of exactness, it is not absolutely committed to this conceptualization. For it is possible to take a few feeble steps in the direction of ontological imprecision by simply copying the relatively crude techniques already developed to partially accommodate epistemological imprecision. (p. 222)

As for irreversibility related with imprecision, it is evident that quantum mechanical and non-deterministic classical mechanical processes exhibit irreversible behavior due either to ontological or epistemological imprecision in reproducing the exact initial conditions. However, what is interesting, according to Hutchison, is the irreversibility that arise “when there is suitable instability in the equations of motion” in classical mechanics (Ibid. p. 224). This irreversibility is not the result of epistemological or ontological imprecision but rather of the equations themselves.

Two of the examples that Hutchison considered to support his view are the motion of a simple harmonic oscillator and inertial motion. He claimed that, since the equation describing the behavior of the former system is ‘suitably’ stable, incapacity in achieving initial conditions does not result in irreversibility[12] because the imprecision in the initial conditions is not magnified by the evolution of the system (Ibid., pp. 225-226). But in the case of inertial motion (Ibid., pp. 227-228) reversibility disappears because "*the uncertainties do not reverse themselves, only the precise values*" (Ibid., p. 227). According to Hutchison, reason that reversibility evaporates when a physical system is simulated differently is the fact that

> the original reversibility was not a permanent feature of the laws of motion: it was created by the way we modeled our physical system; it arose from the particular choice of idealizations adopted in relating the equations to a concrete situation, the fact that we decided to ignore uncertainties. (p. 228)

That is to say that as the system can be unstable so can the equations describing the system even for deterministic part of classical mechanics.[13]

2.4. Nomological and De Facto Irreversibility

As discussing the possibility of irreversible classical mechanical processes, Popper (1957) distinguished between theoretical reversibility and causal irreversibility: a process is "(a) 'theoretically reversible', in the sense that physical theory allows us to specify conditions which would reverse the process, and at the same time (b) 'causally irreversible', in the sense that it is causally, i.e. physically, impossible to realize the required conditions" (p. 1297).

Grünbaum (1964, 1967 and 1974) developed Popper's distinction for irreversible processes as weak and strong senses of irreversibility.

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[12] Hutchison defines irreversibility rather differently to accommodate imprecision: "if a conservative and deterministic system starts from a state B near A and evolves to B_T; then has its velocities reversed; will $(B_T^*)_T$ also be near $(A_T^*)_T = A^*$?" Hutchison (1995a), p. 223.

[13] Hutchison examined also several examples from conservative classical mechanical systems that exhibit irreversible behavior (pp. 227-231).

> The weak sense is that the *temporal inverse* of the process in fact never (or hardly ever) occurs with increasing time for the following reason: Certain boundary or initial conditions obtaining in the universe independently of any law (or laws) combine with a relevant law (or laws) to render the temporal inverse *de facto* non-existent or unreversed, although no law or combination of laws itself disallows that inverse process. The strong sense of "irreversible" is that the temporal inverse is impossible in virtue of being ruled out by a law alone or by a combination of laws. (Grünbaum, 1967, p. 160)

On the basis of Mehlberg's (1961) terminology, the strong sense can also be called, "nomological" because the reversal of the process is forbidden by laws of nature and the weak sense "contingent," "nomologically contingent" or "de facto" (Grünbaum, 1964, p. 211) because it "arises from boundary conditions which are *contingent* with respect to the laws of nature" (Ibid.).

It follows from above that there are two classes of processes, the first of which is also divided into two subclasses.

1. Classical mechanical processes.

1.1. Nomologically and de facto reversible processes, e.g., celestial mechanical processes.

1.2. Nomologically reversible-de facto irreversible (non-entropic) mechanical processes, e.g., wave expansion.

2. Thermodynamic processes. Nomologically and de facto irreversible (entropic) processes.

2.5. Philosophical Background of the Connection Between Reversibility-Irreversibility and Time Invariant and Time Non-invariant Theories

Costa de Beauregard (1968) contended that "physical irreversibility is never deduced, but truly *postulated* at the very root of any theory dealing with it" (p. 188). He further argued that this is manifest from the two aspects of Carnot's postulates:

> 1. That heat will flow from a place at high temperature to a place at low temperature, not the other way.
>
> 2. That in a monothermal situation work is convertible into heat, not the other way.
>
> If the time arrow were reversed in these two Carnot postulates, it would also be reversed in all the rest of phenomenological thermodynamics. Thermodynamical irreversibility is thus not spontaneously generated in the course of the deduction, but is rather built in the so-called Second Law at its very root. (Ibid., p. 189)

It may be asserted in support of macroscopic irreversibility that there are macroscopic (phenomenological) observables, such as pressure, temperature, etc., and that the thermodynamic laws which are based on these observables are t-non-invariant. However, then, the lawlikeness of macrothermodynamics has been questioned by some authors. For example, Earman (1974) stated that "The relevant 'macrolaws' are not true laws in the true sense of the word; these so-called laws are either false lawlike statements ... or else they are non-lawlike statements which are summaries of human experience" (pp. 36-37). The conflict that seems to occur between observation of irreversible macroprocesses and reversibility implied by the mechanical theory of microprocesses is denied by Earman.

> For reversibility does not mean that in any given model the reverse process which occur must also occur. *We live in only one model, and any given model can be as radically asymmetric with respect to past and future as you like while at the same time all of the relevant laws are time reversal invariant.* (Ibid., p. 37)

Harrison (1975) also maintained that the second law "is an empirical generalization from macroscopic experience" (p. 45). Thus it seems to lead to the conclusion that the second law is spatio-temporally restricted, i.e., it is true only for the limited regions of space and time and is confined only to certain processes. However, as Denbigh (1981) stated to the contrary, "it can be contended equally well that t-invariance is nothing more than a peculiarity of certain theories. These are humanly constructed and, as such, their feature of t-invariance could well be an idealization which overlooks some important aspects of reality" (p. 101).

In order to dissolve this dilemma, Denbigh (1989b) argued that t-invariant theories do not actually describe irreversible processes of the real world but "what they can be used to describe is the idealized limiting case" (p. 508). For example, Newtonian mechanics construes the motion of bodies in a frictionless environment without inelastic collision between the bodies, which is an abstraction (or idealization). He also maintained that those who reject irreversibility conceive theories as laws of nature: if the laws are t-invariant so must the physical processes be invariant to the direction of time.

> The effect of such a claim is to make the notion of irreversibility appear entirely foreign to physical science. Yet this is not only contrary to the reality of irreversibility in human experience but is also entirely contrary to what is accepted about the objective world in those sciences—notably biology, geology and astrophysics— which deal with evolving systems. (p. 508)

Denbigh's objection suggests that the issue as regards t-invariant and t-non-invariant theories and their connection with reversibility and irreversibility should also be considered for the real systems which are open and therefore vulnerable to external influences. Within this respect, the significance of the environment, the initial and boundary conditions need be evaluated.

Denbigh (1981) examined Loschmidt's reversibility objection.

> According to Loschmidt's objection the inverted state $\overline{S}_2$, having the inverse vectors $-v_1$, $-v_2$, ... $-v_n$, should have exactly the same probability of occurrence as S_2 and therefore, if the H-theorem were not fallacious, we could observe a gas '*withdrawing*' to the state $\overline{S}_1$, which is the velocity inverse of S_1, just as often as we observe a gas 'expanding' into a vacuum. (p. 98)

However, although the inverse process of gas diffusion seems theoretically possible, in actual systems, in addition to the inverse vectors of the gas molecules "the molecules of the containing vessel would also need to have exactly inverted motions, as otherwise the collision of the gas molecules with the walls would not be the temporal mirror images of the original ones" (Ibid.). This criticism is similar to Popper's (1956b) argument (see above) regarding the planetary system that reversing only the velocities of the planets would not be sufficient because other forces exerted upon the system must also be taken into account. If otherwise, even the planetary systems are irreversible because the planets would not retrace their paths to return to their original positions. However, it may be said that this irreversibility does not arise as a result of the classical mechanics since all influences of the mega-environment cannot be included within the system, because all theories are, to a certain degree, idealizations that must ignore certain effects exerted upon the system by the environment. However, as we have seen in section 2.3., Hutchison

(1993) asserted that irreversibility can also be defended by similar arguments.

With regard to the significance of initial and boundary conditions, Popper's (1956-58) consideration of non-entropic irreversible processes are perfectly relevant. As we have seen above, the possibility of advanced waves can be contemplated from two perspectives: first, advanced waves are theoretically possible due to the wave equation; second, they are, nevertheless, physically impossible. The first condition indicates that there is no law that forbids the advanced waves; the second condition, on the other hand, indicates that certain boundary conditions do not allow the occurrence of the advanced waves.

Grünbaum (1964), in discussing factlike and lawlike nature of irreversibility, also debated that we have no more warrant for the time-symmetric laws to be universally true than for the factlike conditions for experienced irreversibility to be universally true.

> What is decisive for the obtaining of that anisotropy [of time] is not whether the non-existence of the temporal inverses of certain processes is merely de facto rather than nomological; instead, what matters here is whether the temporal inverses of these processes always (or nearly always) fail to occur, whatever the reason for that failure! (p. 211)

However, the stronger position that may be held is to insist that irreversibility does not arise in virtue of the interaction of the system with the environment, or of the statistical effects or even of the boundary conditions but rather as a result of the fact that irreversibility is more fundamental than reversibility. Whyte (1955) started with this presupposition and endeavored to develop an alternative conception that covers and is more fundamental than both reversibility and irreversibility. He thus stated that

> We should give up the long struggle with the question: 'How does irreversibility arise if the basic laws are reversible?', and ask instead: 'If the laws are of a one-way character, under what spatial and temporal, quantitative and sterile conditions can reversible expressions provide a useful approximation? (p. 110)

Denbigh (1981) also confirmed that "only very special, and unreal, initial conditions will result in reversible behavior; an overwhelmingly large number of the physically possible sets of initial conditions will give rise to irreversibility" (p. 100). So it is tempting to conclude that reversibility is merely a useful idealization which can only be approximated, whereas irreversibility is the true nature of the processes. Furthermore de facto irreversibility need be extended to cover all mechanical processes, because although mechanical processes are reversible in idealized simulations these are, as a matter of fact, irreversible when initial and boundary conditions are taken into account. The significance of the initial and boundary conditions will be significant when I examine retrodiction in chapter 4.

CHAPTER 3

NATURE OF TIME

3.1.A Brief Review of Some Theories on Time

The problem of time has always perplexed even the greatest minds throughout history. While some of them have sought for the essence of time through metaphysical, conceptual or scientific analysis others have regarded time merely as a given feature of nature and treated it together with space as the background to everything else. Every philosophical work on the possibility of the knowledge of the past must include, at least, a review of some basic concepts as regards time.

As for the nature of time, perhaps, the first question that is always asked is as to whether time as time has an independent reality of its own, or it is solely a relational concept that is defined in terms of things and events. The common notion is that change is the essence of time, i.e., time without change is inconceivable (for example, Aristotle, Hume, Aune (1962), Bennet (1966, p.175), Mellor (1981, p. 47). For example, Aristotle stated that "time is not independent of change. Thus, when we have no sense of change or are inattentive to any change, we have no sense of passing of time" (*Physics,* Bk. IV, 218b, 20-22). According to van Fraassen (1970), "The argument Aristotle uses to establish this is phenomenological: we cannot perceive time as such; we are conscious of the passing of time only through discerning change or movement" (p. 15). Hume (1739) also contended that "'tis impossible to conceive ... a time when there was no succession or change in any real existence" (p. 40).

That we derive the concept of time from the relationship between events and that time is measured against change do not justify the claim that time is only a relation and has no existence of its own. For example, Shoemaker (1969) asserted that it is possible to conceive of time without change which is a conclusion that he arrived at in virtue of a thought experiment regarding a period of time during which no change occurs. Furthermore assuming that time has a real existence of its own has important consequences as pointed out by Shoemaker (1969): "It implies, for example, that the universe cannot have had a temporal beginning unless time itself had a beginning and that the universe cannot come to an end unless time itself can come to an end" (p. 64).

Another possibility for holding the claim that time has no real existence of its own is to make a Kantian assertion that time is only a form. Rotenstreich (1958), for example, contended that "[t]ime is a form of the relation of succession" (p. 51). If time is merely a form then it, as form, can be investigated in an *a priori* manner which is objected to by Newton-Smith (1980), who thought that "the investigation of the structure of time is an empirical matter and as such cannot be conducted in an entirely *a priori* manner" (p. 4). Although Rotenstreich thought that time is a form he distinguishes pure time from empirical time which is not, however, identical with change that takes place in time. The conception of pure time can provide answer to such questions as "whether time flows or whether the events occurring in time flow, whether time depends on events or events upon time" (Ibid., p. 60) for "[p]ure time does not flow, for the simple reason that it is not a reality but rather a form used for the cognition of reality" (Ibid.). Rotenstreich's understanding of pure time is obviously Kantian since for Rotenstreich as for Kant time is a pure form, "the form of flow free from flow" (Ibid., p. 61). In contradistinction to material time, i.e., empirical time, pure time is not experienced as material time, for "pure formal time is a condition of experience" (Ibid., p. 74). Rotenstreich also spoke of time as empirical time, material time, psychic time or historical time which are not, as he admitted, time as form but are empirical and presuppose time as form.

Even if it is possible to speak of time as independent of change, change is the measure of time. With regard to the nature of change, Aristotle distinguished between essential and accidental changes. An essential change is either from one substantive (yellow) to a contrary substantive (red) or from one substantive (man) to a contrary non-substantive (not-man) (*Physics,* Bk. V, 224b, 28-30, and 225a, 4-19). Furthermore, movement as well as generation and destruction is an essential change. On the other hand, a change is accidental if it is described in terms of relations of comparisons. For example,

> If Peter is taller than Paul ... a change might occur that results in Peter being shorter than Paul. But this kind of change is called accidental change when Peter is taken as the subject of the change, because it would occur as a result of Paul's growing up. (van Fraassen, 1970, p. 12)

McTaggart (1908) distinguished two series of time: A and B series. In B series, "positions in time ... are distinguished in two ways. Each position is Earlier than some and Later than some of the other positions" (p. 24). In A series, on the other hand, "each position is either Past, Present, or Future" (Ibid.). It is obvious from above that "the distinctions of the former class are permanent, while those of the latter are not" (Ibid.). That is to say that if two events have 'earlier than' or 'later than' relation such relation never changes but the status of an event considered in A series change from future to present and to past. For example, the death of Queen Anne "was once an event in the far future. It became every moment an event in the nearer future. At last it was present. Then it became past, and will always remain past, though every moment it becomes further and further past" (Ibid., p. 26).

McTaggart's purpose was to demonstrate the unreality of time by showing that change and time require the existence of the A series which leads to a contradiction. Thus he rejected the reality of the A series together with the reality of time and change. He furthermore claimed that the B series must also be rejected. All this leads to the conclusion that "Nothing is really present, past, or future. Nothing is really earlier or later than anything else or temporarily simultaneous with it. Nothing really changes. And nothing is really in time" (p. 34).

This negative, apparently Parmenidian, conclusion regarding the reality of the A and B series and thus reality of change and time has not gone without objections. For example, Mellor (1981) remarked that "[w]hat is wrong with McTaggart's prosecution of time is not his prosecution of tense but his contention that disposing of tense disposes of change" (p. 92). He then went on to prove the unreality of tense which means the rejection of the A series. He concluded that all tensed sentences, i.e., the A series sentences, can be stated in terms of tenseless statements, i.e., the B series sentences. For example, a past event is earlier than, a future event later than, and a present event simultaneous with a particular moment of time. However, the question regarding A and B series is not a linguistic matter, i.e., whether one can be stated in terms of other, for the B series statements can also be translated into the A series statements. What is important for these two series is which of the two is the more primitive. Hence the problem is ontological not linguistic. However, as Denbigh (1981) noted, "neither the A-theory nor the B-theory ... is properly speaking a *scientific* theory–not at least in Popper's sense. There appear to be no empirical means by which either of them might be refuted" (p. 54). But it is still a persuasive fact that scientific laws and theories being generally tenseless statements lend support to B series rather than to A series.

As for the objectivity of time, Denbigh (1981) distinguished two senses; objectivity$_1$ which is conventional, and objectivity$_2$ which is independent of public agreement.

> This [objective$_2$] relates to statements about things which can be held to exist, or about events which can be held to occur, quite independently of man's thoughts or emotions, or of his presence in the world ... For instance a proposition to the effect that the Cambrian rocks were formed before the Devonian would refer to a supposedly objective$_2$ event occurring previous to man's own existence. (p. 11)

The general scientific attitude towards objectivity of time corresponds to Denbigh's objectivity$_2$ because in science objectivity of time is considered to be independent of human consciousness.

> The objective$_2$ concept of time,... was thus a late development. One of its origins was the theory of mechanics; another was the understanding of geology and of Darwinian theory which allowed it to be seen that the temporal order exists quite independently of man's own existence. (p. 12)

3.2. Direction of Time

Time as experienced produces in us the indispensable feeling of flowing which is perhaps the foremost evidence of the direction of time that can be asserted. However, we need more than a subjective feeling for justifying such a claim. Therefore I review the arguments for and against the direction of time that can be derived from the anthropocentric stand, logical analysis, physical processes and scientific theories.

3.2.1. Anthropocentrism and the Direction of Time

According to Grünbaum (1974), "the inspiration for speaking about 'the' direction of time derives from the supposition that there is a transient 'now' or 'present' which can be claimed to shift so as to single out the future direction of time as the sense of its 'advance' " (p. 777). He further alleged that the supposed 'now' or 'present' has no place in science except perhaps psychology because of its anthropocentric origin.

Popper (1974) claimed that although the words 'here' and 'now' are anthropocentric, these words are not expungable from science. He maintained that the statements 'The present state of the surface of the moon suggests that ...' and 'The present age of the universe' are perfectly suitable for cosmology (perhaps also for geology or with a new name 'the planetary science') if not for theoretical physics. He admitted that astronomers use space coordinates instead of places and times in terms of here and now; however, it is obvious that the coordinates are specified with reference to the Earth and dates with reference to conventional and anthropocentric 'now.'

> Thus my thesis is that the notions like 'the present' are needed, if not in theoretical physics, at any rate in physical science. But I want to claim even more. Theoretical physics uses all the time spatio-temporal variables; and without applications in which these variables are specified (in the last instance with the help of 'here and now'), they would have no reasonable function whatever. (p. 1143)

I should elaborate on the words 'past,' present' and 'future' which are distinguished by human consciousness. We commonly believed that we can be more sure about our knowledge about the past but not about the future (this is also related with another contention that the past leaves traces but the future does not or, in other words, we remember the past but not the future) whereas we can influence the future but not the past.

Consciousness divides the past and the future by the present ("specious" present or now). The present has a very significant role in human consciousness because our experience is always at the present. Even our memories belong to our present. But this specious present is continually changing in the sense that a future event, i.e., a possibility, becomes a present event and then a past event. So the feeling of the flow from future to past supports the direction of time. However, within the conception of physics that treats time as a coordinate, past, present and future have no significance because they are points on the time coordinate that are distinguished by the relations 'earlier than' and 'later than.'. But considering the future as real as the past seems strange because there are other evidence (e.g., existence of memories and traces, etc.) that the past and the future are different. For example, in the case of geology, biology and human history past, present and future have significance due to the existence of traces because although the past events leave traces the future events do not and these sciences are based primarily on the interpretation of traces.

Related with McTaggart's A and B series, two theories of time can be distinguished: A- and B-theories. According to the A-theory, 'now' or 'the present' is a real feature of time and the world. According to the B-theory, on the other hand, events occur tenselessly in the relation 'later than,' 'earlier than' and 'simultaneous with' which are fully objective in contrast to 'past,' 'present' and 'future.' They further claimed that 'past,' 'present' and 'future' can be reduced to 'later than,' 'earlier than' and 'simultaneous with.' Which of the two theories describe time and the world more precisely is beyond the scope of this study. What is important for us is that both of these theories can be significant for retrodiction of the past because these series of time enter into the description of the past in the sciences and history.

3.2.2. Logical Analysis of the Direction of Time

Reichenbach (1956) conducted a logical analysis which, he expected, would help to distinguish between order and direction. For this purpose, he examined and contrasted the order constituted by the points on a Euclidean straight line and the order of the real numbers. As a result, he concluded that an analogy can be drawn between the latter and temporal direction.

As for the points on a straight line, he observed that "the points on a straight line, which is infinite in both sides, are arranged in a certain order; but the line does not posses a direction. When the points are in a linear order, or serial order, they are governed by an asymmetrical and transitive relation” (p. 26). This relation can be described by referring to the figure below: the relation *to the left of* is asymmetrical, since even though A is to the left of B, B is not to the left of A. This relation is also transitive, since if A is to the left of B, and B is to the left of C, then A is to the left of C. Since the relation is asymmetrical, connected and transitive, the line is serially ordered. But Reichenbach asserted that the order is not directed because “there is no way of distinguishing structurally between left and right, between the relation and its converse” (p. 26). The conclusion and its justification needs elaboration: first, the relation *to the left of* is extrinsic in the sense that it is imposed upon the points externally. That is, the properties ‘left’ and ‘right’ do not belong to the points themselves; when the point of reference changes left and right also change. A structural property, on the other hand, is intrinsic to the elements. Thus, an intrinsic relation is that which orders the elements by their structural properties. Reichenbach seems to have assumed that the direction of a serial order can be distinguished *only* intrinsically, i.e., by structural properties and without an observer.

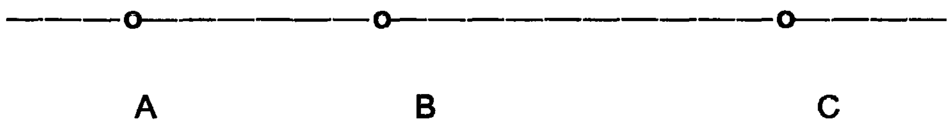


Figure 6: The order of points on a line (from Reichenbach, 1956)

According to Reichenbach the real numbers, on the other hand, "are governed by the relation *smaller than*, which is [also] asymmetrical, connected, and transitive, like the *relation to the left of;* therefore the numbers have an order" (p. 26). It should be noted that this order is also a *serial* order due to asymmetry, connectedness, and transitivity. He also maintained that "the relation *smaller than* has a direction; that is, it is structurally different from its converse, the relation *larger than* " (p. 26). He explained how positive and negative numbers are discriminated in the following way.

> The square of a positive number is positive, and the square of a negative number is also positive. We therefore can make this statement for the class of [negative] real numbers: Any number which is the square of another number is larger than any number which is not the square of another number. (p. 26)

Reichenbach's conclusion can be explained in the following way: the direction of a serial order can be distinguished only intrinsically, and two opposite orders are distinguished intrinsically for the real numbers, therefore the order of the real numbers is directed.

Finally, he drew an analogy between the real numbers and time. "The relation *earlier than* is regarded as being of the same kind as the relation *smaller than,* and as not being undirected like the relation *to the left of*" (pp. 26-27). This also means that "the relation *earlier than* differs structurally from its converse, the relation *later than* " (p. 27). Therefore time has also both order and direction

Denbigh (1981) also agreed with Reichenbach that the points on a straight line have no direction whereas the real numbers and time as experienced by human consciousness have direction. He also stated that since the relation 'to the left of' determines a serial order for the points on a straight line only *extrinsically* no direction can be distinguished, for "a 1 D[imensional] being who lived in the line presumably could not tell the one direction from the other; the line's seriality is *not intrinsic* " (p. 93). The series of real numbers, on the other hand, ordered by the intrinsic relation 'greater than' which allows to distinguish between one direction from the other. Similarly, for the temporal order, Denbigh (1981) stated that “in human experience the two directions along the temporal order are intrinsically distinguishable–they are distinguishable from within the order itself” (Ibid. p. 32). It seems that this asymmetric ordering relation that is provided by human sense of ‘later than’ is similar to the relation ‘greater than.’ However, Denbigh noted that the relation ‘later than’ is different in an essential way from the relation ‘greater than.’ Although each number has the same logical status and is predefined each temporal instant does not have the same logical status and are not predefined because “there is no *logical* necessity that all change in the universe, including the ongoing of clocks, will not suddenly cease” (Ibid., p. 63).

Mehlberg (1961) criticized Reichenbach’s differentiation of the relations ‘greater than’ and ‘smaller than’ for the real numbers and the relations ‘later than’ and ‘earlier than’ for time on the basis of transitivity, asymmetry, and connectedness. As for the numbers, “[i]t is not only the relation ‘larger than’ ... that has the three characteristics of order [i.e., transitivity, asymmetry, and connectedness]; the same holds for the converse relation ‘smaller than.’” (p. 109). Along the same line of thought, the above argument can be reasserted for time as well: "since the relation 'after' defines a temporal order of events, then so does the converse of succession, the relation 'before' " (p. 109). Therefore Reichenbach's arguments, to this extent, do not establish that one of the two directions has a privileged status.

Mehlberg then continued to analyze as to whether one of the directions has a different quality that is not shared by the other direction which would help to distinguish one direction as "the" direction. This also amounts to saying that the directions are structurally different. As we have seen above, Reichenbach provided such a structural difference (i.e., the concept of the square) that holds between the two relations for the negative real numbers which, if true, would also establish a structural difference for the temporal order as well. Mehlberg found this attempt unsatisfactory for the following reasons. First, it is possible to invent other numbers some of which would pertain to the relation 'larger than' while others to the converse of that relation. "Consequently, there is no reason whatsoever for associating the concept of 'direction' with one of these relations on the basis of its monopoly on a group of 'structural' properties allegedly enjoyed by this relation" (p. 110).

Secondly, Mehlberg asserted that if Reichenbach retreated to the position that the two orders can at least be distinguished from each other on the basis of one structural property without identifying one direction as "the" direction the main term would be left unexplained. "For we would be offered some clue to understanding the claim that a given continuum 'has a direction' ... without receiving any hint as to what 'the direction of this continuum' actually means" (p. 111).

Grünbaum (1964 and 1974) also objected to Reichenbach's interpretation of the direction of time. First, the order of points on a straight line: Grünbaum compared the two ordering relations, namely 'o-betweenness' and 'to the left of.' "The points of the straight line form a system of o-betweenness. This order is intrinsic to the straight line in the sense that its specification involves no essential reference to an external viewer and his particular perspective" (p. 214). He also contended that this relation is a triadic relation and that the order with respect to this relation is *not* a serial order. Conversely, however, the relation 'to the left of' is a dyadic *serial* ordering relation, and the serial order on the basis of this dyadic relation is extrinsic since it requires an external point of reference. In addition, "To say that a given serial order on the line with respect to the relation 'to the left of' is conventional is another way of saying that it is extrinsic in our sense" (p. 215). He agreed with Reichenbach that "the serial ordering of the real numbers with respect to 'smaller than' is intrinsic in our sense, since for any two real numbers, the ordering with respect to magnitude requires no reference to entities outside the domain" (p. 215). However, he disagreed with Reichenbach's contention that a serial ordering established extrinsically is undirected for "a serial ordering always establishes a difference in direction independently of whether it is intrinsic or extrinsic!" (p. 215). On the basis of the above contention that a serial ordering established extrinsically is undirected, Reichenbach differentiated between the relations 'to the left of' and 'smaller than' in that the former is not unidirectional whereas the latter is unidirectional. Grünbaum stated that the reason that Reichenbach ended up in this assertion is his failure "to note that, by being asymmetric, a serial relation is automatically a directed one even when the seriality has an extrinsic basis" (p. 215). Moreover, "this oversight led him to distinguish relations which are serial while allegedly being undirected from directed serial relations" (p. 215). It is this error, according to Grünbaum, that led Reichenbach to distinguish between 'order' and 'direction.' But Grünbaum contended that this false distinction “should be replaced by the distinction between intrinsically isotropic and anisotropic kinds of time” (p. 216).

It appears that a mere logical treatment of the structure of time is not sufficient for determining the direction of time because the relation ‘later than’ establishes merely that time has two direction but cannot single out one direction as “the” direction of time.

3.2.3. Physical Processes and the Direction of Time

We have seen in section 2.4. that irreversible processes are distinguished as ‘de facto’ and ‘nomological.’ In the case of de facto irreversible processes, the temporal inverses of the processes never occur due to certain initial and boundary conditions that render the reverse of the processes nonexistent. In the case of nomologically irreversible processes, on the other hand, the reverse of the processes is forbidden by a law or a combination of laws. I can thus proceed to define nomological reversibility in the following way: *reversal of a process is rendered possible by a law or a combination of laws*. In this case, there is no guarantee that the actual reversal of the process in fact occurs because certain initial and boundary conditions may render it impossible. De facto reversibility can also be defined similarly: *de facto reversibility is made possible not only by laws but also by the necessary initial and boundary conditions that permit the reverse of the processes*. “That is to say the temporal inverses would not only be allowed by the relevant laws but would actually exist in virtue of the obtaining of the required initial (boundary) conditions” (Grünbaum, 1964, p. 212). The connection of reversibility and irreversibility with the direction of time can now be analyzed.

“If all kinds of natural processes were actually de facto reversible, time would indeed be *isotropic*” (Grünbaum 1964, p. 212). For, as the points on a straight line that constitute merely an order with respect to the relation 'o-betweenness,' "De facto reversible processes intrinsically define a temporal order of mere o-betweenness under suitable boundary conditions, but the symmetric causal relation associated with these processes provides no physical basis for an intrinsic serial order of time" (p. 216). However, as it is possible for the points on a straight line to be serially ordered by the extrinsic relation 'to the left of,' "so also it is possible to choose two reference states in a time that is intrinsically merely a system of o-betweenness, and extrinsically render this time serial by making one of these two states later than the other through the assignment of suitable real numbers as temporal names" (p. 216). Therefore a universe in which all the processes are exclusively de facto reversible can be described by a serial time, which is, however, defined only extrinsically.

Nevertheless, if it can be shown that there is a property on the basis of which a dyadic relation can be defined intrinsically between the pairs of states time can be said to have serial order. Such a property may be provided by irreversible processes, e.g., entropy. Thus in a universe in which some of the processes are de facto or nomologically irreversible, two adverse time direction may be structurally differentiated:

> There are certain kinds of sequences of states of systems specified in the order of increasing time coordinates such that these same kinds of sequences do *not* likewise exist in the order of decreasing time coordinates. Or, equivalently, ... there are certain kinds of sequences of states of systems specified in the order of *decreasing* time coordinates such that these same kinds of sequences do *not* likewise obtain in the order of increasing time coordinates. (Grünbaum, 1964, p. 776)

That is, "such a world is temporally anisotropic: its time exhibits a special kind of *difference in direction* arising from the directed, intrinsically grounded serial relation of 'later than' " (Grünbaum, 1964, pp. 216-217). However, Grünbaum (1974) claimed that the fact that two opposite directions of time are structurally different "provides no basis at all for singling out one of the two opposite senses as '*the* direction' of time. Hence the assertion that irreversible processes render time anisotropic is *not at all* equivalent to such statements as 'time flows one way'" (p. 776). Grünbaum's distinction between temporal anisotropy and temporal one-wayness is worth emphasizing: although the anisotropy concept recognizes two distinct and opposite directions this does not mean that one direction is "the" direction of time because "the instants of anisotropic time are ordered by the relation 'earlier than' no less than by the converse relation 'later than' " (1974, p. 777).

Popper (1974) argued against Grünbaum (1974) that time has a direction in addition to its anisotropy because "there is not only the relation of betweenness in its topology, but also the relation of before and after" (p. 1141). However, this objection misses Grünbaum's point since he did not reject these relations or their objectivity but asserted that neither of these have a privileged status to determine "the" direction of time. As we have seen above, no independent evidence can be provided so as to single out one direction as "the" direction. Although irreversible processes exist there are doubts about the claimed universal status of these processes. The existence of irreversible processes can be accepted without accepting the direction provided by them as "the" direction, for such processes may be local that can help to determine time's arrow only locally.

Another physical asymmetry proposed for establishing unidirectionality of time is the expansion of universe. Denbigh (1981) draws attention to the fact regarding the expansion of universe that although the cosmological 'arrow' may be more fundamental, "cosmological theories [e.g., big-bang theory] are much more uncertain ... than are the theories of thermodynamics and statistical mechanics" (p. 116). Hence even if it were possible to base the direction of time on the current big-bang theory it would still constitute a poor evidence. But it should be remembered that the expansion of universe derives not from the current big-bang theory but from a more fundamental experimental data, i.e., the Doppler effect. However, as Denbigh notes "the conclusion that the red-shift implies expansion is not entirely secure, since some other explanation of the experimental results may be found [e.g., the 'tired light' hypothesis]" (Ibid.).

Furthermore, there are also cyclical big-bang theories that conceive the universe as having repetitive contractions following expansions which thus do not permit defining 'later than' on the basis only of expansion, for, during the contraction phases, 'later than' means contraction. Therefore, since the possibility of cosmological arrow depends on one of the current cosmological hypothesis and there are other models that may support adverse directions the cosmological determination of time's arrow supplies no more certain evidence than other theories.

3.2.4. Scientific Theories and the Direction of Time

The earliest theories of modern science are the mechanical theories, such as celestial mechanics and dynamics. It is believed that these theories that purport to explain reversible processes are time reversal invariant. The theories that are concerned with irreversible processes, e.g., macrothermodynamics, and are time reversal non-invariant came as late as the nineteenth century. This historical fact played a significant role in viewing time as a coordinate similar to space coordinates because of the noted fame of the mechanical theories. But Denbigh (1981) noted that the roots of time-as-coordinate are deeper in the history of human thought. “It appears to have been Aristotle (*Physics*, Book VI) who first represented ‘time’ as a straight line and subsequently the notion of time *as a coordinate*, much like the three space coordinates, was widely adopted in classical physics" (p. 61). In the Cartesian system, space is represented by three coordinates each of which indicating one dimension. Objects are defined in three dimensions by points on each coordinate. The points on the coordinates have no intrinsically determinable direction, but rather the order is merely extrinsic as mentioned earlier in the case of the points on a straight line. Similarly, time is constructed as a coordinate in mechanical theories that allows to locate events on this coordinate just as physical objects are positioned in space coordinates. In the space-time system in the theory of relativity, space and time are fused into a four dimensional coordinate system in which points on the coordinates define the events. So constructed, time has no intrinsically determinable direction because in this coordinate time is ordered by the relation of temporal betweenness. The two directions on time coordinate can be distinguished only extrinsically by assigning increasing numbers to one direction and decreasing numbers to the other.

On the other hand, t-non-invariant theories enable to distinguish the two directions of time, for example, by entropy increase. However, it has been argued that since classical thermodynamics is reformulated by statistical mechanics, t-invariant theories are more fundamental. But, as we have seen in section 2.3., this conclusion is still a matter of dispute because there are arguments in favor of genuine character of t-non-invariant theories. Wave propagation and damped motion are the examples given for t-non-invariant formulation of classical mechanical processes. Furthermore general increase of probability is also cited in support of the direction of time. However, all these suggest at most a factlike rather than lawlike character of anisotropy of time.

CHAPTER 4

RETRODICTION

4.1. What Is Retrodiction? A Preliminary Discussion on the Logic of Retrodiction as Compared with Explanation and Prediction

In this section, I analyze the concept of retrodiction with respect to the temporal direction of retrodictive inference in comparison with explanatory and predictive inferences.

The logical structure of deductive nomological explanation (given in detail in Appendix A) is claimed to represent also those of retrodictions and predictions. (Hempel, 1962, p. 116)

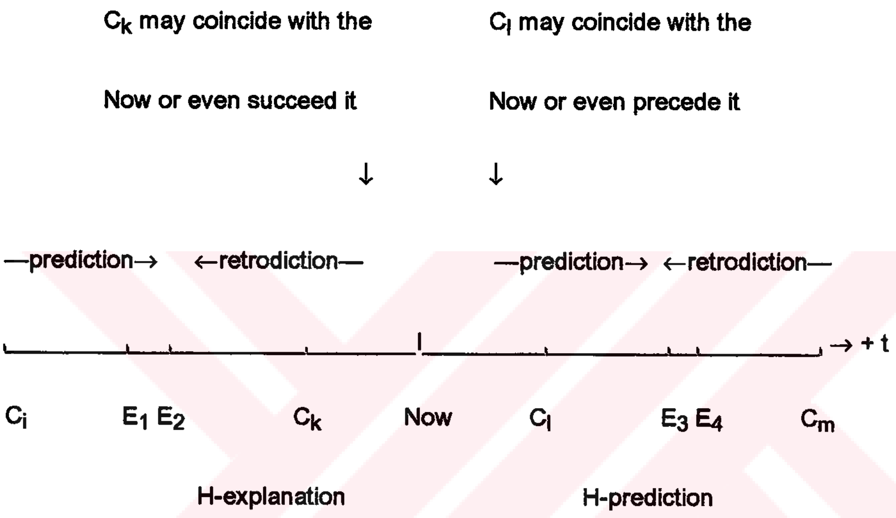
	$C_1, C_2, \ldots, C_k$	Statements of antecedent conditions
	$L_1, L_2, \ldots, L_r$	General laws
Logical deduction	___________________	
	E	Description of the empirical phenomenon to be explained

In the case of predictions or retrodictions E represents the predictive or retrodictive statement. According to Hempel and Oppenheim (1948), since explanation and prediction have the same logical structure "[t]he difference between the two is of a pragmatic character" (p. 11). Pragmatic in the sense that *E* belongs to the scientist's past in explanation and to the scientist's future in prediction.

However, Rescher (1958) argued against Hempel (1962) that the thesis that explanations and predictions have the same logical structure "rests upon a tacit but unwarranted assumption as to the nature of the physical universe" (p. 282). It appears that the primary rationale behind his assertion is that "the explanation of events is oriented (in the main) towards the past, while prediction is oriented towards the future" (p. 286). This view was also shared by Scriven (1959): "to predict, we need a correlation between present events and future ones—to explain, between present ones and past ones" (p. 479). However, Hempel (1962) responded that Rescher's view as regards the opposite directions of explanation and prediction is the result of his confusion of explanation with retrodiction. He also noted that this confusion may, in part, be the result of a footnote of their 1948 essay: "The logical similarity of explanation and prediction, and the fact that one is directed towards past occurrences, the other towards future ones, is well expressed in the terms 'postdictability' and 'predictability' used by Reichenbach in [Quantum Mechanics]." Later, in his 1962, Hempel distinguished retrodiction from explanation and prediction by C_i being later than E in retrodiction (p. 116).

Grünbaum (1964) interpreted Hempel and Oppenheim's (1948) and Hempel's (1962) thesis of symmetry between explanation and prediction in the following way: Since they asserted that the explanandum (E) belongs to the scientist's past in the case of explanations and the scientist's future in the case of predictions, "the particular conditions C_i ($i = 1.2. ... n$) ... may be *earlier* than E in *both* explanation *and* prediction or the C_i may be *later* than E in *both* explanation *and* prediction." (p. 291) Grünbaum is justified in this interpretation because they had not mentioned any rules distinguishing between the order of C_i and E for explanation and prediction.

Nonetheless, Grünbaum noted that Hempel (1962) expressed a different rule in contrasting retrodiction and prediction: "a *retrodiction* is characterized by the fact that the C_i are *later* than E, while the C_i are *earlier* than E in the kind of *prediction* which is antithetical to retrodiction but *not* identical with Hempelian prediction. (Ibid.) By the help of a graph, Grünbaum compared Hempel's two criteria.

> In the accompanying diagram, the i, k, l, m, may each range over the values 1,2 ... n.
>
> 
>
>
> If we use the prefix "H" as an abbreviation for "Hempelian," then two consequences are apparent. Firstly, a retrodiction as well as a prediction can be an H-prediction, and a prediction as well as a retrodiction can be H-explanation. Secondly, being an H-prediction rather than an H-explanation or conversely depends on the transient homocentric "now," but there is no such "now"-dependence in the case of being a retrodiction instead of prediction, or conversely. (p. Ibid.)

I assert that the conflict between the accounts of 1948 and 1962 that appears in the above scheme and stated as "the first consequence" above can be resolved by rejecting that C_i may be later than E in *both* explanation *and* prediction which makes room for retrodiction as C_i being later than E, independent of the transient homocentric "now." By this, the overlap of Hempel's 1962 retrodiction with H-explanation and H-prediction disappears, for prediction and explanation can be defined as C_i being earlier than E, and retrodiction as C_i being later than E. I think that there is a good reason for doing so. First, although it is true that retrodiction qualifies logically and methodologically as H-explanation and H-prediction for *some* physical systems, e.g. conservative classical mechanical systems, for which the earlier and later states can be used interchangeably to infer from earlier to later and from later to earlier,[14] it does not qualify methodologically for some irreversible systems, as claimed by Rescher (1958) and accepted by Grünbaum. Hempel (1962) constructed an artificial system in order to illustrate this fact:

> Consider a model "world" which consists simply of a sequence of colors, namely, Blue (B), Green (G), Red (R), and Yellow (Y), which appear on a screen during successive one-second intervals i_1, i_2, i_3, ... Let the succession of colors be governed by three laws:
>
> (L_1) B is always followed by G.
>
> (L_2) G and R are always followed by Y.
>
> (L_3) Y is always followed by R.

---

[14] Even though the logical structure of retrodiction is the same as those of explanation and prediction for such systems it does not follow that retrodiction is identical with explanation and prediction, for, in explanation and prediction, we infer from earlier to later states of such mechanical system, whereas in retrodiction we infer from later to earlier states. For example, the motion of a billiard-ball on a billiards table, without friction and time and velocity dependent forces (that is, a conservative system), we can infer the explanandum describing the position and velocity of the ball at $t=t_2$ from the C_i describing the position and velocity of the ball at $t=t_1$ ($t_1< t_2$) together with the laws of mechanics. This would constitute an explanation if E were in our past or a prediction if E were in our future. However, retrodiction is the inference of C_i (E in retrodiction) from E (C_i in retrodiction), that is, from t_2 to t_1. Although E is inferred from Ci the direction of the inference is the opposite. Therefore these inferences are not identical in every respect, though structurally the same.

> Then, given the color of the screen for a certain interval, say i_3, these laws unequivocally determine the "state of the world," i.e., the screen color, for all later intervals, but not for all earlier ones. For example, given the information that during i_3 the screen is Y, the laws predict the colors for the subsequent intervals uniquely as RYRYRY ...; but for the preceding states i_1 and i_2, they yield no unique information, since they allow here two possibilities: BG and YR. (pp. 114-115)

It is obvious that the system above permits predictions but not retrodictions. However, Hempel demonstrated also that the above case allows explanation.

> Suppose that during i_3 we find the screen to be Y, and that we seek to explain this fact. This can be done if we ascertain, for example, that the color for i_1 had been B; for from the statement of this particular antecedent fact we can infer, by means of L_1, that the color for i_2 must have been G and hence, by L_2, that the color for i_3 had to be Y. (Ibid., p. 115)

Furthermore although prediction that overlaps with H-explanation in Grünbaum's diagram is an inference of the past states of things it is not a retrodiction, because the direction of the inference is from earlier to later. I agree with the diagram that it is, in fact, a kind of prediction which, however, conflicts Hempel and Oppenheim's original definition of explanation and prediction. This conflict can be dissolved by modifying this definition.

The overlap between prediction and H-explanation that appears on Grünbaum's (1973) diagram in section 4.1. can be eliminated by the following modifications. At this part of the diagram, the inferences, i.e. prediction and H-explanation, are from past (earlier) initial conditions either to a past (later) or to a present (later) final condition. Neither the initial nor the final conditions are directly observable in the case of inferences from the past to the past, and thus must be retrodicted from the present evidence. In the case of inferences from the past to the present, on the other hand, the initial conditions are not directly observable, whereas the final conditions are observable. We can state this matter formally. Suppose that the thing x evolved from t_1 to t_n (t_n being the present of the inferring scientist) that can be described as x at t_1, x at t_2, x at t_3, ... x at t_n. Suppose also that x at t_1 constitutes the initial conditions and x at t_2 the final conditions. As for the inference from the past to the past, there are two possibilities. First, if the initial conditions (x at t_1) are known by the inferring scientist through retrodicting either from x at t_n (assuming that the system is isolated and reversible) or from other present things, and the final conditions, which are not known at the time of inference, are inferred from the initial conditions together with the relevant laws this inference is a prediction. This situation can be described by an example. Suppose that it is retrodicted from the present evidence, e.g. a crater, that there was an asteroid impact on earth 65 million years ago. From this particular initial condition and by the relevant laws, it is inferred (hypothesized) that certain plants and land animals became extinct during the following few decades. This prediction can be tested (confirmed or falsified) indirectly, i.e. not by observation of the extinction directly, but rather by retrodiction from the present evidence, i.e. from the present fossils. (However, I should note that, strictly speaking, the confirmation takes place at the future of the scientist, and thus the confirming evidence is not a present but rather a future evidence with respect to the utterance of the predictive statement; however, what is confirmed is the statement describing the extinction in the past.)

Second, if the final condition (x at t_2) is already known, and is inferred from possible or already known initial conditions (x at t_1) together with the relevant laws (that is, brought under laws) the inference is an explanation, because explanation (at least deductive nomological explanation) is bringing the final condition under a law(s). The above example can be adapted to illustrate this case. Suppose this time that we already know that there was an extinction 65 million years ago which we discovered through retrodicting from the present fossils. In addition, we also retrodicted from an impact crater that there was an asteroid impact approximately 65 million years ago. If it is possible to infer the extinction from this particular initial conditions (there might have been other possible initial conditions) together with laws we can be said to *explain* the extinction by the impact.

As for the inferences from the past to the present, there are also two possibilities. First, if the final condition (x at t_n) is not known (but, in principle, directly observable) the inference from the initial conditions (for instance, from x at t_1) is a prediction. For instance, it may be the case that a past volcanic eruption at a certain place was retrodicted from certain present volcanic rocks. It can be predicted from this initial condition that this eruption has had a certain influence on the present distribution of a certain species of plants at that particular region which is not known at the time of the utterance of the predictive statement. This prediction can be verified or falsified by direct observation of the distribution of that particular species.

Second, if the final condition is already known the inference of this final condition from the possible past initial conditions is an explanation. Again the same example can be used. This time, the particular distribution of the species, which is already observed, need an explanation. Upon the retrodiction of the volcanic eruption from the present geologic evidence, the distribution of the plants can be explained.

As regards the modification of Hempel and Oppenheim's (1948) distinction between explanation and prediction, while their distinction requires that the state of the system described by the predictive statement be ontologically undetermined due to its being in the future our distinction requires merely an epistemological indeterminacy. That is, the lack of knowledge by the predicting scientist about the state of the system described by the predictive statement is sufficient to distinguish prediction from explanation. (However, I do not assert that this distinction is necessary, because in the case of “ordinary” predictions, Hempel and Oppenheim’s criterion applies.) Although I agree that prediction-in-the-past may be a peculiar type it is not contrary to the intuition about predictions.

4.2. Traces

Traces and records are generally considered to constitute an ontologically and/or epistemologically distinct class of objects and to have a distinct irreplaceable function in inferring the past. It is also a common notion in the literature of science and philosophy that traces and records have the same ontological and/or epistemic status. The use, even metaphorically, of the terms ‘archives,’ ‘monuments,’ ‘antiquities of the earth’ and ‘texts,’ that are “testimony” of the past, for non-human traces and ‘trace’ for human records reveals the idea that the emphasis is on the interpretative function of traces. Franklin (1984), for instance, stated that “there is a long, if now dormant, tradition that regards the entire world as a book written by God.” (p. 510) If so, *every* thing can be interpreted according to the Book to unreveal the purpose of God. On the other hand, modern hermeneutics gives autonomy to the text which “raises the possibility either that the existence of the author is not necessary at all, or at least that the assumption of his existence performs no theoretical function.” (Franklin, p. 510) Hence, according to this interpretation, if the intentions of the author plays no role in interpretation, there is no reason not to regard nature, which has no author, as a text waiting for interpretation.

Denbigh (1981) also claimed that “most records and traces are not distinguished as such in any objective or physical sense; it is rather what *we* read into them that constitutes them as records or traces.” (p. 127) For example, “if it is an oak tree it provides evidence that a sapling took root in this place, and furthermore that the particular tree survived and was not felled." (Ibid.) In the next paragraph, he provided a definition that does not demarcate between traces and records.

> [A] record or trace is an entity which recognizably persists as such (i.e. it is genidentical, although not necessarily changeless) during some temporal interval. If we come across it at a moment *t* during that interval we may be able to infer, from its state and location at *t*, and by use of our accumulated experience, which particular processes occurring earlier than *t* have contributed to its state. (p. 127)

Reichenbach (1956) also used the terms alternately. "Our knowledge of the past is based on records, whether they be documents written by a chronicler, or fossils included in geological strata, or traces of blood on a garment." (p. 21) Grünbaum (1964), on the other hand, included traces, records and memories under the general class of later-indicators or post-indicators.[15]

As regards other characteristics of traces, there is a general agreement that they are remains of the past in the sense that they come into existence as a result of past events or processes, i.e. they are *after* the events, endure up to the present, and furthermore they are thought to be evidence of the past events or processes (that is, they convey information about these events and processes) and therefore can be interpreted to know the past (for example, Reichenbach, 1956, van Bemmelen, 1961, Hooykaas, 1963, Grünbaum, 1964, Danto, 1965, Riceour, 1976, Gould, 1987, Jenkins, 1995). (In this chapter, I do not deal with the issues of interpretation, but rather are focused on the attempts to provide ontological accounts of traces and/or records.) However, Grünbaum cautioned that although indicator-states[16] occurs, with very high probability, after the

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[15] Grünbaum did not use the terms 'later-indicators' or 'post-indicators' directly he used 'later' and 'indicators' to mean 'later-indicator' and 'post-record' to include also traces and memories.

[16] We should remind that he was thinking in terms of entropy (he used the term 'low entropy indicator-states).

interactions which form the indicators "it is not an a priori truth." (p. 283) (As we see in the next section, that is why he endeavored to define traces and records in terms of entropy which would eliminate the a priori acceptance of them as later-indicators.) He provided two exceptional cases of pre-recordability, that is, the record being before the event which it pre-records. (pp. 284-286)

However, only very small part of the literature, incomparable with their so-called importance, is spared for the analysis of the nature of traces and records. The only possible reason may be that the concept of trace and record seems intuitively so clear that most people have not bothered with the matter. Only two trends in philosophical literature are interested in rather detailed analysis of traces and records. The one is the attempt to explain traces and records in terms of entropy (for example, Reichenbach, Grünbaum and Smart), and the other is the attempt by some philosophers of hermeneutics who argued in detail what a 'text' is (for example, Derrida, Riceur and their followers).[17] In addition to entropic and hermeneutic attempts, no serious effort is spent in defining traces. The authors merely list and classify traces. The definitions, if given at all, are rather imprecise. They do not offer any ontological accounts of traces like the entropic explanation. It is never clear why the examples that they provide have a privileged status over other similar objects.

In section 4.2.1., I examine the entropic account of traces and records and reject it on the ground that the concept of entropy is used beyond its legitimate limits of application. In section 4.2.2., I examine the views that cluster around the claim that traces are remains of the past events, but I reject them as philosophically untenable. In this section, I also consider the

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[17] There is also a specific literature on the concept of Trace originating with Heidegger (perhaps even far earlier) and followed, for example, by Derrida and Levinas. However, we can find no connection between the way that they have used the concept and with our intentions in this study.

view that traces or records are those objects that have retrodictive significance in the sense that those things that can be utilized for retrodiction but not prediction are traces or records. However, I reject also this view on the basis of the fact that the retrodictive significance of a thing is context dependent, because every thing is, in principle, can be used for retrodiction. Hence I conclude that there is no privileged class of things that are traces or records. Every thing can play the role of a trace or record which is dependent upon the field of interest. Our construction of retrodiction construes all things as its object. Furthermore the logic of retrodiction is the same for every kind of thing.

4.2.1. Is There an Entropic Foundation of Traces and Records?

The view that traces and records are low entropy states has been developed by Reichenbach (1956), Grünbaum (1964) and Smart (1967). This view is based on Reichenbach's theory of branch systems according to which branch systems are subsystems that branch off from the wider system, remain isolated for some time and join again the main system. It is postulated that the traces form within the subsystem as low entropy states as a result of the interaction with the wider system. Smart (1967) explained this idea quite clearly.

> The formation of a trace is the formation of a subsystem of temporarily lower entropy than that of its surroundings, and the trace is blotted out when the entropy curve of the subsystem rejoins that of the larger system. A footprint in sand is a temporarily highly ordered state of the sand; this orderliness is bought at the expense of an increased disorderliness (metabolic depletion) of the pedestrian who made it....

> On investigation it will be seen that all sorts of traces, whether footprints on sand, photographs, fossil bones, or the like, can be understood as traces in this sense. In deed, so are written records. The close connection between information and entropy is brought out in modern information theory, the mathematics of which is much the same as that of statistical thermodynamics. (pp. 131-132)

Although both Smart and Grünbaum agree that traces are low entropy states they differ as to whether entropy of the system is lower than the surrounding (Smart) or than the earlier state of the system itself (Grünbaum). Furthermore they construed entropy as a measure of disorder, and concluded that traces or records are ordered states. For example, Reichenbach (1956) stated that records are frozen order (p. 151) meaning that they record the past interactions that produced the ordered state.

It appears that Reichenbach, Grünbaum and Smart seem to postulate that there is a connection between thermodynamic entropy and disorder (and order). However, according to Whyte (1955), this supposed relationship between entropy and disorder is the result of the ambiguity of the terms like ‘order’, ‘disorder’, organization,’ etc. He further contended that “both tendencies [that is, ordered and disordered states] display a movement towards equilibrium” (p. 120).

In order to show the erroneous connection between entropy and disorder Wright (1970) considered several examples that exhibit increase of entropy and order at the same time (pp. 584-586). Then he concluded that "Intuitive qualitative ideas of 'disorder' correlate closely with these quantities [that is, quantities measured by the entropy law] in many cases, and this fact is important and informative—but is a matter of CORRELATION and not of IDENTIFICATION" (p. 586).

Denbigh (1975 and 1989a) also analyzed the relationship between entropy, on the one hand, and disorder and disorganization on the other.

He noted that there are apparent problems with equating the increase of entropy with the increase of disorder. Denbigh's concern was to show that the views that regard entropy as disorder or as a measure of disorganization is untenable. He maintained that disorder or disorganization are not related to the entropy defined by the classical and statistical interpretations of the second law. " 'Entropy' is a term which belongs firmly within science whereas the terms 'order' and 'organization', and their negations, do not" (1989a, p. 328). In order to make his point, he proceeded to consider a set of objects or events that are distributed in space and/or time.

> For example a set of three or more objects, A, B, C, etc., will display a certain kind of orderliness if they exist in a linear arrangement. Thereby the objects obey the rule that B is to the right of A, that C is to the right of B, etc., when viewed from *anywhere* on one side of the line. The same objects will display an additional kind of orderliness if there is also a relationship (*e.g.,* of equality, of doubling, etc.) between successive separations, AB, BC, etc. There is then a more comprehensive state of order. (1989a, p. 328)

He maintained further that natural objects are not arranged exactly as having geometrical shapes but they can only be approximated to such figures. But then the question arises as to "how large a standard deviation (or comparable measure) is permissible for a given set of entities to qualify as orderly in some particular respect" (1989a, p. 328). He noted that there is often a subjective character in such determinations.

> There is certainly no difficulty in appreciating that a crystal is an orderly arrangement of its component particles in so far as these lie at positions close to the intersection points of a geometric lattice. The question is: How orderly is it? For the supposed interpretation of entropy as orderliness is useless unless the concept of orderliness is capable of being made at least as quantitative as is that of entropy itself, and this has not been achieved in a relevant scientific manner. (1989a, p. 329)

He reminded also that W in the equation $S = k \ln W$ can be calculated for a perfect lattice as well as for a crystal with imperfections. But he contended that "there is no theory of orderliness independent of statistical mechanics which would provide the means of calculating a crystal's degree of orderliness" (1989a, p. 329).

A good example provided by Denbigh in order to show the erroneous connection between entropy and disorder is "the spontaneous crystallization of a super-cooled melt. Under adiabatic conditions the entropy of this system increases, but it would involve special pleading to substantiate a claim to the effect that its disorder also increases!" (1989a, p. 329).

As regards the relationship between 'order' and 'organization' that are used as synonyms in the biological literature, Denbigh provided some counter-examples.

> [i] For instance a patterned wallpaper is surely much more orderly, in having almost exact repetition, than is, say, a Cezanne, but is much less highly organized. Any great painting displays organization to a high degree but its parts are not related to each other by a rule, such as is characteristic of a state of order ... [ii] Similarly a living cell is more highly organized than is a crystal even though the latter is much more orderly, at least in the spatio-temporal context. ... [iii] Just as the items depicted in a painting are spatially organized, so also the notes and bars in a musical composition are temporally organized. Similarly the component parts of a scientific theory or a mathematical treatise are organized in a logical space. (1989a, pp. 329-330)

A further aspect of an organized system is that "the system in question *has a function*" (1989a, p. 330).

Lastly, with the above considerations in mind, Denbigh moved on to analyze the supposed relationship between entropy and disorganization. He furnished his analysis with another counter-example: hatching a fertile egg inside an incubator which is provided with sufficient air and temperature and then isolated. Since the incubator is isolated its entropy can only increase or remain constant. From then on, there are two possible consequences that are evaluated by Denbigh.

> (1) the egg dies; (2) the egg lives and eventually gives rise to a live chick. Now it is true that in case (1) there is an entropy increase accompanied by a process of *disorganization*, localized in the egg. But the opposite is the situation in case (2): For although the egg is certainly a highly organized system, the live chick must surely be deemed to be much more so. Entropy again increases but now there is an increase in the degree of organization as well. (1989a, p. 331)

Therefore Denbigh concluded that thermodynamic entropy, disorder, disorganization and their negations are mutually independent. However, he maintained that these counter-examples do not show that such systems are contrary to the Second Law, but they rather indicate that "the Second Law is a good deal less restrictive than is commonly supposed" (1989a, p. 331).

I can now examine the claim that traces or records are states of low (thermodynamic) entropy. This claim can be objected on two grounds. First, Reichenbach, Grünbaum and Smart accounted for the formation of traces or records by the interaction of an isolated system with something outside itself. This idea involves the application of the second law to open systems. However, Denbigh (1989a) stated that "the Second Law does not apply directly to 'open systems',"[18] for measuring or calculating entropies of open systems is extremely difficult. (p. 324) For example, celestial bodies lose and gain matter and energy continuously, or living organisms and their cells

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[18] However, Denbigh admitted that open systems "may actually undergo decrease of entropy, due to an outward passage of heat and/or matter across the boundary surface." (p. 324) Furthermore Prigogine (1980) also showed how low entropy states can occur and become stabilized within an open system by energy flow.

are in continuous exchange of energy and matter with the environment. So it is not simple to determine precisely whether their entropies are low or high. But he also stated that this does not mean that occurrence of such systems is contradictory to the second law, because the second law can account for these systems if these systems are considered as sub-systems of sufficiently enlarged systems that become isolated. Then "a decrease of entropy in the sub-system is compensated by an equal or greater increase of entropy of the environment." (p. 324).

The second ground for objection can be based on the claim that the systems bearing traces or records are not thermodynamic systems; therefore the second law cannot be applied to such systems. Wright (1970) distinguished three distinct concepts of entropy: (i) the thermodynamic entropy invented by Clausius: (ii) the statistical mechanical entropy of Boltzmann which explains macro-states in terms of micro-states; and (iii) the intuitive qualitative ideas of disorder (pp. 582-583). He asserted that between (i) and (ii), "there is a precise and quantitative correspondence. ... However, between concepts of types (i) and (ii) on the one hand, and concepts of type (iii) on the other, it is not reasonable to expect any more than a rough correspondence." (p. 583)

Penrose (1970) also discussed comparatively entropies that are provided by statistical mechanics, such as, Gibbs entropy, observer statistical entropy, molecular entropy, fine-grained entropy, entropy of a probability distribution, entropy as lack of information (information entropies), entropy as the degree of disorder, etc. However, Denbigh (1989a) stated that "none of them is precisely the same as the thermodynamic entropy and none of them is identical with the others" (p. 326).

However, Reichenbach (1956) attempted to connect microentropy with macroentropy by applying microstatistics to macroscopic systems.

A distribution of molecules is a class of the arrangements of these molecules. In this sense,

> For instance, if a certain molecule in the cell *i* and another one in a different cell *k* exchange their places, we obtain a different arrangement, or "complexion", whereas the distribution remains the same. However, if two molecules in the same cell *i* exchange their places, even the arrangement remains unchanged, since the size of the cell defines the limit of exactness within which we wish to specify an arrangement. (p. 57)

A microstate is the same as an arrangement of molecules. On the other hand, "A macrostate *A* is ... a class of microstates *a* which are *macroscopically indistinguishable* from one another." (p. 71) Hence the same macrostate can be realized by various microstates; hence while microstates can change continually macrostate remains unchanged.

Now, an unordered state is a "well-shuffled state" in the sense, for example, that all molecules of a gas in a chamber are evenly (or uniformly) distributed in the chamber. An ordered state, on the other hand, is the state that has a structure or pattern in the sense that the gas molecules, for example, are crowded in one side of the container. Hence, "By "order" we understand here the degree of separation; therefore, a well-shuffled distribution possesses the lowest degree of order whereas a partial separation has some higher degree of order." (p. 72). Furthermore, according to Reichenbach, "macroscopically indistinguishable arrangements possess *the same degree of order*. ... The concept of *order* is therefore definable in terms of the concept *same order*, which in turn is reducible to the concept of *macroscopically indistinguishable*." (Ibid.)

When the elements are macroscopic objects, such as grains of sands or playing cards, instead of molecules, and the elementary arrangements are macrostates rather than microstates all arrangements are macroscopically distinguishable (remembering that, in microstatistics, microscopic arrangements cannot be distinguished, but only the macrostates that are composed of microstates are). Then how is it possible to distinguish the order of macroarrangements?

> With reference to the example of the deck of cards, we may call attention to the fact that we distinguish between ordered and unordered arrangements. If all red cards are on top and all black cards below them, we speak of an ordered arrangement, whereas we regard the kind of arrangement usually resulting from shuffling as unordered. The reason is that the first kind of arrangement can be characterized by a simple rule, whereas that the latter kind cannot. (Ibid., p. 146)

Then it appears that the order of a macroarrangement can be determined with regard to a rule. Hence Reichenbach defined the concept "rule-indistinguishable." "We will call two arrangements a_1 and a_2 rule-indistinguishable if every simple rule satisfied by a_1 is also satisfied by a_2, and vice versa." (It also follows that these arrangements are rule-distinguishable if every simple rule satisfied by a_1 is not satisfied by a_2, and vice versa.) Thus "Two arrangements that are rule-indistinguishable are said to have the *same order*. (p. 148) When these definitions are obtained, it follows that the methods of microstatistics applies also to macroarrangements. Therefore "An ordered arrangement of cards has a low macroprobability, the logarithm of which may be called the macroentropy of that arrangement." (p. 148)

Earman (1974) stated that "statistical-thermodynamic entropy is what we might call microentropy since it concerns microscopic arrangements of atoms and molecules." (p. 40) On the other hand, even if macroentropy could be defined for macroscopic objects, such as grains of sand, so as to account for the formation of traces by a decrease of macroentropy, "there is no guarantee that there will be any direct relation between macroentropy so defined and microentropy." (p. 40)

Against Barret and Sober (1992), who defended the idea that had been developed by Reichenbach, Grünbaum, and Smart and applied it to an evolving population of organisms in terms of the probability equation $\sum p_i ln p_i$, Denbigh (1994) asserted that "[w]hereas in statistical thermodynamics the p_i refer to the probabilities of molecular states, in the context of information theory they usually refer to the probabilities of certain macroscopic states." (pp. 709-710)

From above it may be concluded that the formation of traces or records cannot be connected to thermodynamic entropy. It is then possible to retreat to another position that a trace or record is an *ordered* (but not necessarily low entropy) state that forms as a result of the interaction of an isolated system with wider environment. However, even this rather weaker version of the hypothesis for explaining the formation of traces and records can be challenged. First, as Earman (1974) asserted, a trace formed by an interaction is not necessarily ordered.

> When a bomb is dropped on a city ... the explosion may leave traces. But in what sense is the formation of the traces of the explosion (the ruined buildings, etc.) the formation of temporarily highly ordered states or the formation of subsystems of temporarily lower entropies than their surroundings? (p. 40)

Earman is right in asking in what sense these traces are ordered states, because although thermodynamic entropy has a well defined meaning these concepts do not. It may be responded that every explosion has its own pattern and thus is ordered in some sense. But attributing order to the trace of an explosion makes unacceptable the intuitive meaning of order all together so that it becomes meaningless to use these terms to distinguish any state as ordered or disordered.

It may be claimed that order can be defined more objectively in terms of probability. We know that, in an isolated system, say, a chamber of gas, if the number of arrangements of gas molecules comprising a certain distribution is small the relative probability of the occurrence of this distribution is also low (assuming that all arrangements have equiprobability). It is observed in general that the low probability distributions of gas molecules seem to correlate well with the intuitive idea of order. That is, the low probability distributions are ordered states. However, it is obvious that the good correlation between order defined intuitively and low probability does not imply a logical connection between the two. But even on the acceptance of this connection between order and low probability for isolated systems, the application of this idea to open systems in which traces or records form as a result of the interaction of an isolated system by something outside is dubious because low probability of the occurrence of an "ordered" state in an isolated system is not low when the system is open or semi-closed due to the fact that the interaction of the isolated system with something outside makes the probability of this "ordered" state quite high. For instance, while probability of the formation of a footprint like shape on an isolated beach by wind and waves is extremely low the probability of the formation of this figure on a semi-closed beach is not low because of the possible intervention of human beings. It also explains why the hypothesis that the footprint was formed by a stroller is a very good hypothesis for accounting for the footprint since it increases the probability of the occurrence of the footprint. Therefore I conclude that explanation of order by low probability is applied illegitimately beyond its scope to account for the formation of traces or records in open systems.

In addition, the postulation that ordered states have low probability does not hold even for isolated systems because apparently ordered states (as order defined intuitively) can form quite frequently in isolated systems. For example, in a desert environment, that can be said to be an isolated system for similar reasons as the isolated beach above, where there is only wind activity, the formation of cross-beds that are geological structures is quite frequent and the cross-beds are stable structures.

We have so far seen that neither entropy nor orderliness is a necessary condition of the formation of traces or records. Finally, I can examine as to whether interaction of an isolated system with the environment is necessary for the formation of traces or records. According to this condition, a state that forms within an isolated system as a result of the random internal processes is not a trace or record. However, this condition is too restrictive because it excludes many structures that are commonly considered as traces by sciences like geology. For instance, the cross-beds mentioned above, are accepted as geological traces even though they form in isolated systems only by the random activities of wind.

4.2.2. Traces as Physical Remains of the Past

In addition to the entropic model and the related discussions, no detailed analysis of traces are found in the literature of analytic philosophy. The authors merely list and classify traces. The definitions, if given at all, are imprecise. They do not provide an ontological account of traces as the entropic explanation. It is never clear why the examples that they provide have a privileged status over other objects. For this reason, I have to derive some definitions from these unclear notions and work with them. I start with a short list of some paradigmatic cases of "traces" that are found intuitively and/or scientifically clear: a fingerprint on a coffee cup, a footprint on a beach, a fossil of a Cambrian trilobite (an ancient organism), a diabase dike

(an intrusive rock), fusion tracks in a rock, a (geological) fault, track of a jet airplane, and so on. Since these are scientifically/intuitively "unambiguous" cases they must have some properties in common. For instance, it appears at first sight that they all are individual physical things[19]. Although this is

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[19] There are two main theories of physical things, namely the substance theory, starting with Aristotle, and the bundle theory, starting with Hume (1888). Aristotle distinguished between substance (necessary) and attributes (accidental). According to Körner (1983), the substance and attributes can be demarcated respectively as instantiating and being instantiated.
Locke (1689) also accepted the existence of material substance supporting the qualities. Similar to Aristotle, Locke believed that substances are particular things. "... when we speak of any sort of Substance, we say it is a *thing* having such or such Qualities, as Body is a *thing* that is extended, figured, and capable of Motion." (Ibid. 3, 297)
Quinton (1973) found Aristotle's explanation of the individuation of things in terms of matter and form vague. He also asserted that qualities do not individuate either, because it is always possible that two things be qualitatively identical. Against individuation by spatial positions, Pivcevic (1990) argued that the problem remains as the individuation of places. According to classical atomism, no two things can be made of the same atoms without being the same thing. The question as to how to distinguish between two atoms with identical spatial form is left unanswered.
Attributive change is defined as a thing having incompatible properties at two different times by the substance theories. The definition requires that while the thing in question changes, i.e. it gains or loses attributes or parts, it retains its identity. According to Locke's (1689) compositional theory, if "no Addition or Substraction of matter being made [to a thing or Body] it is the same." (Book II, ch. XXVII, 2, p. 329) However, Quinton (1973) claimed that inorganic things as well as organic things can survive the continuous replacement of their components. He also contended that simultaneous replacement of all the components of a thing will result in the lose of identity. The problems with the compositional theory is that it shifts the problem of identity to a deeper level (atoms).
Another attempt to account for identity through time is made in terms of reducing identity to spatio-temporal continuity which is defined, for instance, as 'continuous line' (Shoemaker, 1963, p. 5), 'continuous set of places' (Strawson, 1959, p. 37), 'contiguity' (Swinburne, 1968, p. 22-23), or more sophisticated formulations (Coburn's, 1971). Oderberg (1993) claimed that none of these capture some of the cases and counter-cases, such as radical disassemble, disappearance/reappearance of things, fusion or fission of objects (or persons), protean change, and so on.
Bundle theory is an empiricist reaction to the unanalyzable notion of substance (for instance, Hume, Russell, Quine, Strawson, Goodman). The recent empiricists maintained that the substance concept is derived from the subject-predicate form of most languages, and that if a language without singular terms were made possible it would undermine the notion of substance.
The idea of things as bundles of qualities is based on the interpretation of qualities as general classes that are *instantiated* at a position indefinitely many times. In this sense, an instance of a quality is not different from an instance of "man" because both of them are complexes of qualities.
The bundle theory has led to the conception of four-dimensional entities that are composed of temporal parts. According to Russell (1948), Broad (1949), Carnap (1958), Goodman (1951) and Quine (1953), temporal parts are events that make the four-dimensional things also long events or processes. According to Oderberg, "The strictly event-based metaphysics ... is no longer in great favour." (p. 67)
More recent theories are rather vague with regard the nature of temporal parts. Lewis (1971), for example, stated that "enduring things such as persons and bodies ... [are] aggregates–sets, mereological sums, or something similar–of momentary stages." (p. 203) Sider (1996) defended a more radical view: "not only do I accept person stages; I claimed that we *are* stages." (p. 433) The most explicit expression is given by Thompson (1983), who stated that since we are interested in physical objects and their parts "is *not* every part of a physical object itself a physical object? I should think so." (p. 206)
As for individuation of things, Oderberg claimed that "the concept *temporal part* has a built-in method of individuation" which is suggested by the definition of a temporal part. (p. 73) He also contended that the TP theory has advantages as regards problems, such as radical disassemble,

never explicitly stated it seems that there is a general agreement on the status of traces as physical entities. As I said at the beginning of section 4.2., there is a general agreement as regards the fact that traces are remains of the past in the sense that they come into existence as a result of the past events or processes, i.e. they are *after* the events and have endured up to the present. Below I endeavor to show first that the fact that traces are the products of the past is not a necessary but only a contingent fact; second that every trace is not necessarily produced by an event (and conversely every event does not necessarily produce a trace); third that there are no intuitively and/or scientifically clear special class of physical entities that are traces (in other words, every physical thing can intuitively be considered as a trace); and fourth that physical status of traces is not necessary, because there can be intuitively and/or scientifically clear cases of non-physical traces.

I first examine the claim that traces are the relics of the *past*, i.e. the past produces traces, but not the future. It is generally presupposed that the traces as relics are the effects of the past causes that are events, processes or interactions. So the event that produces the trace is the cause and the trace is, in a sense, the effect. For example, when someone walks on the smooth surface of the beach sands his feet push "the sand grains, they pick up kinetic energy, which is transformed into heat when the grains come to rest." (Reichenbach, p. 151) As a result, the sand grains are rearranged in the shape of a human foot.

Though the idea that only the past produce traces seems intuitively clear, it is not an a priori truth and need be qualified. This problem is related strictly with the notion of the direction of time. Although it is a common

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disappearance and reappearance of things, fusion or fission of objects (or persons), protean change, and so on.

notion that the word 'cause' is a term referring to the past we have seen in chapter 2 that causality is inadequate in defining the direction of time. Reichenbach (1956) considered this problem. He restated the footprint example in another language which presupposes reverse time direction.

> First, there are smoothed-out imprints in the sand, somewhat resembling the shape of a human foot. Then winds begin to blow, carrying grains of sand back and forth, with the effect that the shapes in the sand become more distinct and eventually are formed into the exact molds of human feet. Finally a man arrives, walking backward. He puts his feet one after the other into the imprints in the sand, which fit his feet perfectly. When he lifts his foot, sand pours in form all sides and fills the imprints completely, so that this spot of sand no longer differs from its environment. (p. 153)

According to Reichenbach, there are two points about these strange happenings that need be emphasized: first, "the order of the smoothed-out footprints is not regarded as a record; it does not follow, but precedes, interaction, and it represents not a *postinteraction* state, but a *preinteraction* state." (R., p. 153) Second, the footprints in distinct shape cannot be explained in terms of past events because the impact of air particles on sand grains in such an order that can form the footprint is to explain "one state of order [i.e. the footprints] in terms of another one," (p. 153).

A possible account for the events described in this language is to explain "the improbable coincidences by their *purpose* rather than by their cause. The wind transforms the molds in the sand into shapes of human feet *in order that* they will fit the man's feet when he comes." (p.153) This means that the world as described is governed by *finality* not causality. This is the result of changing the direction of time. Reichenbach concluded that although both finalistic and causal explanations are equally plausible our world is a causal world, for the second law of thermodynamics provides the direction of time. So, according to Reichenbach, "it is a physical law that causality, and not finality, governs the universe." (Ibid., pp. 154-155) It follows from this conclusion that “the past *produces* the future, and not vice versa.” (Ibid., p. 155) In other words, “the cause produces the effect. ... the cause leaves traces in the effect.” (Ibid., p. 155)

However, we have seen in chapter 2 that, contrary to Reichenbach’s conviction, the second law of thermodynamics is not adequate in determining *necessarily* the direction of time, because the statistical formulation of the law, the de facto character of retarded waves, Bayes’ principle, and so on, all suggest that irreversibility with which the direction of time is determined is of fact-like rather than law-like. Hence that our universe is governed by causality rather than finality is not a necessary but rather a contingent fact deriving from the initial and boundary conditions of the universe that were determined at the time of “generation.” Then it appears that since causality is not necessary, the claim that traces are the effects of the past causes is also a contingent fact.

I can now analyze the claim that traces are produced by events in terms of the ontology of events. Events are generally defined in terms of other “more basic” elements. For example, Wilson (1974) said that “there is no such thing as an event distinct from a fact;” (p. 317) “an event is a truncated fact.” (p. 304) Sellars (1973, p. 197) also stated that “events are species of [true] proposition.” Kim (1973) defined an event “as a concrete object (or *n* -tuple of objects) exemplifying a property (or *n* -adic relation) at a time. (p. 222) Charlton (1983) proposed to explain changes in terms of causal agents causal action, and causal conditions. (p. 143)[20]

As for the relationship between things, events, and changes, Prior (1968) contended that "it's *things* that change, and events don't change but *happen*" (p. 36). Mellor (1981) claimed that while events have temporal

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[20] There are objections to such interpretations. For instance, Thalberg (1978 and 1980) argued in favor of irreducibility of events.

parts things do not. Hence "[a]pparent changes in events are no more than differences between temporal parts, analogous exactly to differences between spatial parts of things." (p. 104) However, Pivcevic (1990) objected to this distinction as unreliable, because he contended that events also change. For instance, "A concert performance may start off well, then flag and lose sparkle as it goes along, or even veer off the rails altogether and end as a fiasco." (p. 47)

Although Kim did not distinguish between event and change Mellor (1981) defined change "as the thing having different properties at different times, provided the difference constitutes an event." (p. 125) He also stated that "all events have causes or effects, but causes or effects need not all be events." (p. 127) For, according to Mellor, certain unchanging "stages" of things, though causally connected, are not events. It can be said that events may produce all kinds of effects, at least, in their immediate surroundings.

A trace then can be defined in terms of change which is produced by a causing event: *a trace is a thing with a new property*. However, there are two problems with this definition. First, since Mellor's definition of change does not include the so-called existential change, i.e. a thing coming into being or going out of existence, a fingerprint which is a thing that comes into existence by someone's holding a coffee cup with his dirty hands is not a trace, which is contrary to intuitive and scientific understanding of traces. Second, according to this definition of trace, the remain of the past cause (e.g. a thing with a new color) is not a thing but rather a property (new color) which, however, is against the above idea that traces are physical things (recalling that, according to the substance theory of things, I excluded properties from the class of physical things). Then I must define traces in terms of all events not only the changes: *a trace is a thing (that comes into existence) or a "non-thing" (that is, that does not exist as a thing* *but, nevertheless, ceasing to exist is an event), or a thing with a new property.* This definition also sounds odd. I provide a counter-example against the formulation that every event produces, at least, one trace, and conversely that every trace is produced by an event. Suppose that we have a fantastic machine that can do all sorts of things, such as creating things from nothing and making things vanish, that are not come across in daily life. At t_1, we push one of its buttons, a green crystal comes into existence *ex nihilo*. At t_2, we push another button and the color of our green crystal turns into red without changing its sortal. At t_3 ($t_1 < t_2 < t_3$), we again push a third button then our crystal goes out of existence, *ex nihilo*. Finally, after a sufficiently long time, say at t_{10}, we go into the next room and there appears a blue crystal of different sort without our pushing any button and without any contribution of our machine. Now the green crystal at t_1 is a trace because it is produced by an event (our pushing the first button). At t_2 as well as at t_1, there is an event producing a change (event), i.e. a thing with a new property (red crystal). According to the above schema, the second event (pushing the second button) produces also a trace at t_2. But is the second trace the-now-red crystal or just its red color? If it is the color then, as I said above, trace is a property which is inadmissible. If the trace is the red crystal then, at t_2, it is a trace both of the first event and the second one. But at t_1, as a green crystal it was already the trace of the first event. At t_3, there is a third event which has an effect, i.e. disappearance of the crystal, however, we have no trace (a physical thing), unless we want to say that non-existents are physical things and thus traces. Furthermore, at t_{10}, the appearance of the blue crystal in the next door has no earlier (or "later") event to produce it. So it shows that there can be traces that are not produced by some (earlier or later) events. (Although this counter-example is of a fantastic sort, these are not illegitimate even in contemporary physics which construes things that are created or destroyed, *ex nihilo*.) So it appears that every event, though producing an effect, does not leave a

trace as a physical thing, and conversely every trace does not necessarily have a causing event.

There is one more issue as regards the alteration of non-causal relational properties of things.[21] If such alterations are changes or events then from the cases such as that 'Xantippe's becoming a widow' as a result of 'Socrates' death' it would follow that Xantippe, a widow, is a trace, if it is held that traces are the remains of the past events (or changes). In order to

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[21] Kim (1974) claimed as regards the alterations of noncausal relations that they are changes. However, Helm (1975), for example, argued that they are not even events. Mellor (1981) also asserted that such alterations are not real changes. According to Kim (1974), "There appear to be dependency relations between events that are not causal." (p. 41) Such noncausal dependency appears to be the case between the events like 'Socrates' death' and 'Xantippe's becoming a widow,' for Socrates' death is not a cause of Xantippe's widowhood even though the relationship between the two events makes true the counterfactual 'if Socrates had not died at *t*, Xantippe would not have become a widow at *t*.' He claimed that however close we look at the chain of events between Socrates' death and Xantippe's becoming a widow we cannot find a continuous causal chain because "the connection between the death and Xantippe's becoming a widow isn't causal." (p. 49)
Helm (1975) argued against Kim that ""Cambridge" events [e.g. Xantippe's becoming a widow] are not events, and *a fortiori* cannot stand in a relation of dependence to other events, whether of causal or non-causal dependence." (p. 140) The reason given by Helm is that Kim's "Cambridge" criterion allows such events to happen to non-existent things. For example, although Socrates' dying is an event Xanthippe's becoming a widow is not an event because, if this is allowed, it would also possible to say about Xanthippe in the case of her having never married to Socrates that if she had been married to Socrates she would have become a widow after Socrates' death. (pp. 140-143) Helm also stated that although causal consequences of an event are events the logical consequences of events are not events. Helm (1977) explicated this matter on two examples: 'Jones is watching Robinson' and 'Jones is kicking Robinson' both of which are real events. However, we should note that 'being watched by' is different from 'being the widow of' because being watched can, in fact, cause changes in the object that is being watched. For example, it can cause some psychological consequences on the one who is being watched, which are mental events; in addition, it can, in fact, change the object (for example, a subatomic particle) that is being observed.
Mellor (1981) asserted that "Not every alteration in *prima facie* properties of things can count as change." (p. 107) He defined "real changes" causally such that "Real changes ... must have effects." (p. 108) For example, the alteration of the property of Mellor's being an only child or his relative position to his house is an event but not a real change because his parents having another child or his leaving his house does not produce any immediate effect on him or on the house. (p. 107) Moreover he claimed that "This [causal] test of change also provides a test for real properties of things, namely properties whose alteration is a real, i.e. a contiguously effective, change." (p. 108) For instance, 'fame' is not a real property of people but is ascribed by other people. This means that someone can become famous without being aware of it. In such a case, becoming famous can have no effect in that person. Hence the definition of change is transformed into thus: "A change ... is a thing having incompatible real properties at different times." (p. 110)
In addition, Mellor denied also that the events indicating the beginning or ending of a thing are changes. That is, he renounced existential change. He also claimed that "some events are not even beginnings or endings of things, let alone changes in them; some are not part of the history of any thing. Of what thing's history is a flash of lightning a part." (p. 120) From these he concluded that "there are more events than changes in the world." (p. 120)

make the case more comprehensible, let us consider the following example which appeals to commonsense better than the case of Xantippe. Suppose that there are two physical objects, A and B, separated with a distance of two meters. By the movement of B one meter away from A, which is an event, the relational property of A (being two meters from B) has been altered. That is, A was two meters from B, now it is three meters from B. If A's being three meters away from B is a change, or at least an event, then A with its relative new position from B is a trace of the event of B's moving one meter away from A. But this is absurd for the reason that, in that case, as a result of any motion in the universe, everything else must be said to change in relation to that motion. Therefore everything else in the universe is the trace of that particular change. This would require such a strong organismic view of the world that everything is organically related to every other thing, so that any change in any part of the world be considered as changes everywhere else. If this view is accepted it can be said that A with its new relative position is a trace.

I said above that all the paradigmatic cases of traces are individual physical things. We first should distinguish, if possible, between material and physical things, and then ask as to whether all physical/material things are traces or there is a special class of physical/material entities, as exemplified by the above instances, that have a distinct status among others. The need for distinguishing between physical and material things arises from the fact the material content of some of the traces mentioned above is not so obvious. For instance, a diabase dike is a material thing in the sense of being composed of matter; however, it is not so clear at all that a footprint and a geological fault are made of matter. Superficial entities[22],

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[22] Superficial entities are holes, surfaces, corners, any kind of discontinuity, etc. They are, as Leonardo da Vinci said once, things without substance, they need other entities to exist (in Cassati and Varzi 1994, p. 10).

such as those above, have bothered some philosophers (namely, Lewis and Lewis, 1970 an Casati and Varzi, 1994) who, nevertheless, endeavored to explain them as material entities. But I believe that the arguments asserted in favor of materiality of these entities are in suspect. For this reason, I prefer to use the term 'physical thing' (following Joske, 1967, who distinguished between physical and material things to include, however, not the superficial entities but rather force fields, etc.) which I define negatively as excluding physical properties, abstract (fictional, mental, universal, etc.) and spiritual entities.[23]

I can now attempt to answer the question as to whether the examples of traces given above constitute a special class of physical things that are distinguishable from other physical things. For example, a tree, a house, a piece of rock, a billiard-ball are also individual physical things. Except the tree which is a living organism and hence is different from the earlier examples, all the others are indistinguishable in this respect. But there are non-trivial examples of biological organisms that are considered as fossils in geology, namely, living fossils. Since the concept of living fossils is not intuitively very clear I need provide a definition: "An animal or plant that lives at the present time, is also known as a fossil from an earlier geologic time, and has undergone relatively little modification since that earlier time." (*Glossary of Geology*, p. 365;1980) Furthermore, since fossils are traces[24] then it follows that living fossils are also traces.[25]

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[23] Even though the substance theory of things does not permit to include physical properties as physical things the bundle theory does allow including physical properties in the class of physical things because any instantiation of a physical property is necessarily a thing.

[24] The definition of fossils is given by *the Glossary* as the following: "Any remains, trace, or imprint of a plant or animal that has been preserved in the Earth's crust since some geologic or prehistoric time; loosely, any evidence of past life." (p. 243)

[25] It may be objected that the term 'living fossil' is used only metaphorically, and thus does not refer to real fossils. However, we claim that living fossils meet all the other properties that "real" fossils possess.

The definition provides additional conditions that must be examined. I argue that although these conditions are useful for practical/scientific purposes they are philosophically inadequate. First, it states that there is an organism living at the present which is too general, because then all the organisms living at the present turn out to be candidates. Second, according to the definition, the organism has been living since an "earlier" geologic time, which is too imprecise, because it does not specify how earlier the geologic time should have been. For example, there are lungfishes from Devonian (the Devonian Period starting 408 million years ago and ending 360 million years ago), conifers (trees) from Carboniferous (between 360 million years and 286 million years), ginkgos (trees) from Triassic (248 million years and 213 million years), and marsupials (the early mammals) from Cretaceous (144 million years and 65 million years) (Stanley, 1986, pp. 369, 406, 453, 503). As it appears, there are hundreds of millions of years between these geologic times. Hence it is as plausible to assert that more recent animals or plants can also be regarded as living fossils. Third, it is said that the organism has changed little which is too vague because 'relatively little modification' can be interpreted to cover many other organisms. How little a modification is little enough? So these conditions are not conclusive to distinguish, at least, certain groups of biological organisms from others.

It may be argued that although the paradigmatic cases include human "traces," e.g. footprint, there are no human artifacts as trace. Therefore human artifacts constitute a distinct class of physical things that are not traces. Even though I have not provided human artifacts in the set of paradigm examples I can think of a few of them: for example, an ancient cave drawing or a child's carving on a tree appear to be intuitively clear cases of traces.

I have said that traces are remains of the past in the sense of being end products of the past events or processes. For example, a fingerprint forms when someone holds a coffee cup with his dirty hands, or a fault comes into being when certain rock formations are fractured and these parts are relatively displaced as a result of tectonic activities, or fusion tracks are produced in non-conducting solids by charged particles, and so on. It can be argued on a similar line of thought that, including biological organisms, all physical things are, in a sense, remains of the past and that they come into existence as a result of past events. Every individual creature has a beginning and a life however short it may be. Although the organism changes something always remains from the past. A house also is the end product of construction having taken place in the past. All artificial things are also relics of the past. Even so are the human beings traces in this sense.

It may still be maintained that traces or records are those entities that have retrodictive but not predictive significance. That is, traces or records can be used to attain information about the past but not about the future. For instance, the classical mechanical particles are not considered as traces, because they are utilized to obtain information about the past and future symmetrically. On the other hand, a fossil is a trace, because it has a retrodictive value but cannot be used to predict the future events. Even though this argument seems to have force and is, perhaps, one of the reasons behind the intuitive understanding of traces and records I show below that it does not work.

The apparent fact that a classical mechanical particle seems to be a clear case of "non-trace" but a fossil a clear example of traces is determined by the domain in which these entities occur and has nothing to do with the things themselves. That is, the same thing may be a clear case of traces in one domain but not in the other. For instance, the asteroid belt in our solar system is considered as a physical thing with a mass, position, and velocity that has both retrodictive and predictive function in celestial mechanics (classical astronomy), and is thus a non-trace, whereas the same asteroid belt is a trace to a geologist/cosmologist, because it is interpreted as a remain of a planet that has exploded many million years ago, and thus has only retrodictive significance.

The reason of this difference lies at the very foundations of these sciences. Celestial mechanics, or astronomy, at least, as understood classically, is concerned with the spatial and temporal alteration (i.e. motion) of things. As we have seen in chapters 2 and 3, it construes physical entities as constituting reversible systems, and time as symmetric. Moreover since the motion of the bodies are time symmetric within this interpretation, their earlier (past) and later (future) motions are equally knowable. However, the sciences of geology and human history are virtually about the past[26], so that the entities that fall within these domains are interpreted as significant for the past but not for the future. It does not, nonetheless, mean that these entities cannot have predictive significance in different interpretation as the asteroid example clearly indicates. Since these sciences are concerned primarily with the past of things a footprint or a fossil, for instance, seems to be a trace and is significant only for the past.

Moreover the so-called historical sciences are concerned mainly with the origin of things which lies in the past not in the future. In other words, the entities within (classical) astronomy are described non-genetically, that is, only by their positions, velocity, mass, etc., in the historical or genetic sciences, on the other hand, the things are described genetically, in addition to chemically, physically, etc. For instance, a fossil or a rock is

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[26] Although both geology and human history may be thought of concerned with predicting the future, such as predicting the future earthquakes or the social developments as claimed by Marxist historical materialism these are merely practical consequences of such sciences.

defined by its origin as well as it chemical, mineralogical, taxonomic features. So, at least, part of the problem is semantic which, nevertheless, has roots in the foundation of these sciences. At this point, we should note that although, for instance, contemporary physics is concerned with the generation and destruction of things (that is, with the origins) the terms 'trace' and 'record' do not have a common technical use but are perhaps utilized metaphorically. However, I claim that this use is not illegitimate in physics as it is in astronomy. The reason that such concepts are not used need be sought in the traditions from which these sciences have originated. That is, traditionally such concepts have had no place in these sciences, but this does not mean that they cannot be used. Nonetheless I think that dispensing with the concept of trace or record all together is philosophically more reasonable, since no special class of physical things can be distinguished from other physical things.

I can now examine the status of non-physical entities, such as mental entities[27] universals, spiritual entities, and so on. It is not too fanciful to say that what a dream or a hallucination is to Freudian psychoanalysis a fossil is to geology. Such mental objects can be said to be caused by some mental or physical events and are considered by the psychoanalyst as signs referring to past causes or events in the life of the owner of the dream. The distinction between signs and traces is not so obvious. It is a matter of tradition rather than a philosophically well-established fact.

We have seen that a footprint, a fossil, a fault are traces as individuals. Are classes of which these particulars are part also traces?

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[27] As regards the ontological status of memory, the views are various. The general commonsensical view is that it is a mental entity, whereas many philosophical theories, the first of which is Plato's as discussed in Theatetus as a slab of wax and an aviary, construes memory as a physical trace causally mediating between learning and subsequent remembering. If memory is understood as a physical trace it can be included in physical entities and become part of that class. Nevertheless one way or the other, memory has a special place in knowing the past as it is obvious to everyone.

Since universals are atemporal entities they cannot be said to be caused by events. Furthermore, intuitively, traces are thought to be individuals rather than classes (universal entities) and we cannot provide intuitively and/or scientifically clear cases of universal entities that are considered to be traces.

Therefore it follows from the above discussion that even though the concept of trace is scientifically useful it is not possible to obtain a precise ontological definition of trace that is philosophically tenable. Nor is it possible to base the concept of trace on its retrodictive significance. Hence I intend to examine retrodiction without presupposing a special class of objects but use the term 'thing' or 'physical thing' indiscriminately for all the objects of sciences from physics to human history.

4.3. Retrodiction from a Later to Earlier State

In section 4.1., I defined retrodiction as a kind of inference that is directed from later to earlier states of systems which is opposite to the directions of explanation and prediction. No reference to a homocentric 'now,' i.e. to a subject and a present, is made in this definition. This analysis is in accordance with our earlier discussion that most of the basic laws do not discriminate any privileged moment that can correspond to "the present." At most, some of the laws do discriminate two different time directions without, however, picking out one as "the" direction of time.

In this section, I explore the limits of retrodiction for various systems with respect to the following conditions: (i) initial conditions (ii) uniformity of nature (iii) time reversal invariance and non-invariance of the equations, (iv) determinism and non-determinism of laws, (v) reversibility and irreversibility of the processes, and (vi) imprecision in determining the initial conditions and stability or unstability of the equations.

4.3.1. Classical Mechanical Systems

I examine retrodiction for conservative and non-conservative classical mechanical systems separately on the above conditions, because these systems require distinct treatment.

The law statements (equations) describing the behavior of conservative classical mechanical systems are t-invariant in the sense that these equations produce symmetrical solution for $+t$ and $-t$. This derives from the fact that time appears in the second derivative, i.e., d^2x/dt^2, in these equations. The initial conditions of retrodiction are determined as the state of the system at a chosen time, $t = 0$, which is, at the same time, the inverted final state of the system proceeding in the positive direction of time. That is, the direction of time and the velocity vectors are reversed. I can now examine retrodiction for the above conditions.

I distinguish two types of uniformities: uniformity of regularities as described by the laws and the constancy of the factual conditions. (I analyze this distinction in detail in sections 4.4.2 and 4.4.3.) The former consists of the assumption that the classical mechanical laws have unrestricted scope. The latter, on the other hand, consists of the assumption of the constancy of the following boundary conditions: the system, which is a universal system, is a conservative classical mechanical system earlier than the time of retrodiction, i.e. $t = 0$. I examine this system for several initial conditions.

First, the system consists of the body x at rest at a position p at $t = 0$ and the law statement for retrodictive inference, which is Newton's first law: "a body remains at rest or, if already in motion, remains in uniform motion with constant speed in a straight line, unless it is acted upon by an external force."[28] Then we can retrodict from the above that the body is at rest for all time values before $t = 0$.

Second, the system consists of the body x in uniform motion with a constant velocity in a straight line in a specified direction at a position p at $t = 0$ and the law statement is the same as the first system. Then we conclude that the body is in uniform motion with constant speed in a straight line in the specified direction before $t = 0$. Of course, the initial conditions can be such that there may be more than one body that may not come from infinity. One of the possible cases may be a system like our solar system. In this case, we would conclude that the planets have been rotating around the sun for infinitely long time.[29] Another possibility is that the bodies may be coming from a common origin, if the past courses of the bodies merge into a center. This is the case of an expanding universe. But I should note that the universe at such a singularity may not obey the laws of classical mechanics. So this may be the limit of retrodiction by classical mechanical laws.[30] Furthermore if we want to know the exact positions of the body for different time values earlier than $t = 0$ an additional law is required. This law which is Newton's second law stating that "The acceleration produced by a particular force acting on a body is directly proportional to the magnitude of the force and inversely proportional to the mass of the body (that is, $F = ma$, where F being force, m mass of the body and a acceleration)" provides all the necessary equations for calculating the positions earlier than $t = 0$ on

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[28] Since the laws of mechanics do not distinguish between the two directions of time we can equally say that if a body found at rest at $t = 0$ it has been in the state of rest for all times earlier than $t = 0$.

[29] Of course, we know that our solar system is not a conservative system in the strict sense and has a beginning. But by the present positions and velocities and upon the assumption that it has been conservative we necessarily end up the above conclusion.

[30] In the above cases, we assume that there is no collision of the bodies; however if there were collision we would retrodict that the bodies have been crushing into each other for an infinite time in the past. (Of course, we assume that the bodies are perfectly elastic, otherwise the system would not be conservative, because the collisions would have a damping effect. For a conservative system, at the time of collisions the bodies would have zero velocity which, however, would reach up to the original uniform motion due to the conservation of the momentum.) We need not to remind that the idea of an expanding universe is equally applicable in this case.

the basis of the given initial and boundary conditions. Since time occurs as the second derivative in the acceleration (d^2x/dt^2), all the equations have symmetrical solutions for $+t$ and $-t$. This operation is possible for all types of motion for conservative systems. Then it can be concluded that, for conservative systems, retrodiction of the states of the systems before $t = 0$ by Newtonian laws of motion on the condition of the above uniformities is, in principle, possible. Here I should note that retrodiction is symmetrical with prediction in the sense that from the same initial and boundary conditions together with the laws (on the assumption of the above uniformity conditions), both the earlier and the later states of the system can be inferred equally well, and are symmetrical, except that the velocity vector is inversed.

We have seen in section 2.3. that Hutchison (1993) questioned Tolman's (1938) demonstration of time invariance of classical mechanics by the standard Lagrangian equations of motion on two grounds: first, Tolman's proof does not apply to the classical mechanical systems that include non-conservative forces; second, even if it is accepted that Tolman's argument established reversibility of conservative mechanical systems it can be objected that Tolman presupposed that classical mechanics is strictly deterministic. However, the classical mechanical systems described by the differential equations that do not have unique solutions cannot be said to be reversible. Therefore we should consider the differential equations that do not have unique retrodictive solutions. In this case, at least two possible retrodictive statements would follow from the same initial conditions. At most, it may perhaps be said that one of the statements indicates to a more probable path that does not obviously imply that this path is to be necessarily followed. If we turn to an actual system it appears that, in order to decide as to which one of the possibilities has actually occurred which cannot be provided only on the ground of the initial conditions and the nomological evidence, i.e. the laws, we must have extra-nomological evidence (that is, information attainable from things other than that is available from the specified system, e.g. an eye witness). However, this means that we already have the knowledge about the earlier states of the system.

I now examine the correlation of retrodiction by classical mechanical laws with reversible and irreversible classical mechanical systems. We have seen in section 2.3. that Popper (1956a) questioned the common view that all classical mechanical systems are reversible by providing expansion of water waves as a counter-example. He concluded that although expansion of waves are theoretically, i.e., nomologically, reversible they are physically, i.e., causally, or de facto, irreversible. I should also note that Popper seems to have presupposed that the system is conservative and deterministic.[31] The significance of the distinction between theoretical and physical reversibility vanishes with respect to retrodiction, because theoretical reversibility is sufficient for retrodiction by the relevant laws. This can be illustrated by the equation of wave expansion. As we have seen, this equation is t-invariant, deriving from the fact that time occurs as the second derivative that results in the indifference of this equation to the sign of time. Hence the attempt to retrodict the earlier states of an expanding wave before $t = 0$ is made possible by replacing $+t$ by $-t$ in the wave equation.

Imprecision has a close connection with retrodiction by the laws. First, it can make retrodictions quite unreliable in the case of classical mechanical systems in which imprecision is not magnified by the equations of motion. On the other hand, in the case of the systems in which imprecision is magnified by the equations, retrodiction may become impossible. We can

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[31] However, this does not mean that a classical mechanical non-conservative and non-deterministic system of wave expansion cannot be constructed.

see this on the example of inertial motion given by Hutchison (1995a) and cited in section 2.3. Hutchison stated that

> If we considered a slightly modified system [inertial motion] in which the particle was confined to a finite box, the uncertainty could easily mean that we knew no more about the position of the particle than that it was somewhere in the box. So in its reversed motion the particle would get no closer to its initial configuration than could be achieved by choosing a state totally at random. (p. 228)

Therefore as reversibility becomes impossible, so does retrodiction of the earlier configuration of the particle by the equation of motion in a finite system.

Now, I can explore the possibility of retrodiction for non-conservative classical mechanical systems in which forces change with time or velocity. As reviewed in section 2.3., there is an ongoing dispute as to whether non-conservative classical mechanical systems are reversible or irreversible and as to whether classical mechanics is t-invariant or t-non-invariant. Hutchison (1993) argued that classical mechanics is neutral to time reversal invariance on the ground that classical mechanics can account for reversible and irreversible physical systems. However, Popper (1956b) and Davies (1977) had already asserted that non-conservative systems become reversible if the non-conservative forces are also reversed.[32] We have also seen that, according to Savitt's (1994) t-invariance$_2$ and Callender's (1995) definition, classical mechanics is t-invariant. Furthermore Callender also rejected the existence of non-conservative forces on the ground that they are reducible to fundamental ones. However, I have also asserted with Hutchison (1993) that explaining phenomenological irreversibility away by

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[32] On the condition that the non-conservative systems are considered as part of larger systems in which dissipative forces are reversed by reversing all the atoms in the larger system that are disturbed from their original positions by the movement of the body so that dissipative force becomes a positive force contributing to the movement of the body and increasing with time in the negative direction.

reducing it to the atomic level does not invalidate the irreversibility appearing at the phenomenological level. Therefore we must consider all these cases for the possibility of retrodiction. Furthermore if irreversibility remains even as a phenomenological phenomenon, the law equations that describe this phenomenon remain as t-non-invariant because these equations are sensitive to the substitution of *+t* by *-t*.

Let us consider non-conservative systems in which dissipative forces are also included. One of such forces is frictional force that appears as a negative damping force in the equation of motion, $m(d^2x/dt^2) = f(x) - f(x,t)$, that can also be written as $m\ddot{x} = f(x) - \alpha\dot{x}$. If this equation is integrated (for the cases that $f(x) = 0$) the equation becomes $|\dot{x}| \propto e^{-(\alpha/m)t}$ where $|\dot{x}| \rightarrow 0$ as $t \rightarrow \infty$, i.e., as time increases the particle slows down. In the reversed case, i.e., *t* is replaced by *-t*, the following equation obtained: $|\dot{x}| \propto e^{(\alpha/m)t}$. (It is obvious that this equation is t-non-invariant because changing the sign of time changes the equation.) Now, let us investigate the possibility of retrodiction for this system. First, if the particle is at rest at $t = 0$ it is impossible to retrodict the earlier states of the system, because it cannot be known for how long the system has remained at rest and what the particular route the particle has followed before it stopped. Second, if the particle is found in motion in a certain direction at $t = 0$, the equation of motion can be solved on the condition that the frictional force is reversed, and that the boundary conditions are specified. Therefore we need examine as to whether these requirements can be met.

If we remain at the phenomenological level (as Hutchison, 1993, asserted as a valid irreversible system) the damping force as a positive force cannot be determined, because the kinetic energy dissipates into the environment as heat. Determination of the force would require transformation of heat into work which is forbidden by the second law of classical thermodynamics. In terms of statistical mechanics, however, we may consider that the motion of the particle is slowed down by the dissipation of the kinetic energy to the atoms in the medium in the form of heat, and thus if the motions of the individual atoms are also reversed, which is made possible by the t-invariance of the laws that govern the atomic interaction, the frictional force is said to be reversed. Then the reversed paths of the atoms can be retrodicted as contributing to the particle to speed up as a positive force. (Of course the problem as to how large the environment should be remains, because the larger environment would interact with still larger environments, unless full isolation were presupposed.)

The problem of the determination of the damping force is thus related also with the boundary conditions. If it is assumed that the system is a very simple one, the damping force may be estimated roughly (by assuming, for instance, a uniform decrease of the velocity of the particles) and the equation can be solved for the earlier time values. Even so, as time goes to infinity in the negative direction of time the velocity of the particle goes also to infinity which means that the particle has come from infinity with an infinite velocity which is unphysical. In order to avoid such unphysical solutions, it may be presupposed that the system has had a beginning at a certain time before $t=0$, and the equation can be solved with this restriction. However, although such presuppositions are possible for hypothetical systems, for actual systems, a presupposition of this kind requires that we already have some information about the system. But this information can be obtained only by extra-nomological evidence (for instance, by the notes of the scientist who designed this experiment, but has left it to someone else at $t=0$), because neither the equation nor the initial conditions can provide any information about the earlier condition of the system. Furthermore the system may have been more complex in the sense that it may have an irregular behavior in the past. Nor is this information provided by the equation or the initial conditions. It may be objected that, as in the case of predictions, the boundary conditions for earlier times can be predetermined at $t = 0$, and the earlier states of the system can be inferred on these conditions. However, even though the truth of a prediction is tested by later observations, the truth of a retrodictive inference can never be tested directly, but only indirectly by extra-nomological evidence.

If the universe is an expanding universe (that is, the earlier courses of the particles merge into a common origin) then the velocities would not blow up, for all bodies would merge into a common origin. If the system is collisional, retrodiction is possible if the dissipation of energy at each collision is uniform, or if the rate of change in energy dissipation through time is uniform. However, retrodiciton is possible only for smaller time values, for the system blows up also for this case. For an expanding universe, on the other hand, the system may end at a singularity before blowing up. Finally, as indicated above, if the bodies are motionless retrodiction becomes impossible, because it is not even possible to know for how long the system has been at rest.

As for the conditions of determinism and non-determinism influencing retrodiction, it is obvious that the differential equations describing non-conservative systems can also be non-deterministic. In the case of statistical theories and laws non-determinism is built in these theories that would restrict retrodiction by such laws.

Imprecision arising from human incapacity and the unstability of the equations is more severely observed in the case of non-conservative and statistical systems which have been treated by the theories of chaos.

I have so far investigated the conditions of retrodiction for the whole universe as a single system. Upon the acceptance of the conservation of energy and a finite and closed universe, it follows that the quantity of matter (or energy) has always been the same at any slice of time. So Laplace's ideal of knowing the past or future from the present evidence becomes possible, as we have seen above, at least for certain cases at determined uniformity conditions, if no ontological or epistemological indeterminacy is involved. However, this is an overestimation of the real cases, specifically the local systems. Even in the case of a deterministic conservative classical mechanical universe, retrodicting the earlier states of a small portion of the universe requires further uniformities in addition to the above uniformities, such as that the system is isolated and that no forces act upon the system before $t = 0$, or the uniformity of the rate of the matter or energy flow into and out of the local system, and so on. If not, (that is, if that part of the universe is in interaction with the other parts of the universe) retrodiction would fail. For instance, suppose that our solar system is a deterministic conservative classical mechanical system, and we want to retrodict the past of this system from the present initial conditions together with the relevant laws. This retrodiction is possible so far as the system is isolated or matter and energy flow is uniform. Otherwise, the possibility of some unestimated body entering into the system would disturb the system, thus fail the retrodiction, for our retrodiction would not "sense" the interception of the wandering celestial body. But the expectation of the uniformity of isolated conditions for local systems is not well grounded, for our experience suggests that local systems are, at most, quasi-isolated systems that branch off from the environment and then converge into it (remember Reichenbach's theory of branch systems in chapter 2).

4.3.2. Thermodynamic Systems (Classical and Statistical)

As for classical thermodynamic systems, the situation is different, because presupposition of the uniformity of the isolation condition of the systems before $t = 0$ creates additional problems. I can show this on an example from heat diffusion.[33] Suppose that there is a metal bar which is hot at one end and cold at the other at $t = 0$. (I remind that the system is completely insulated from the environment for all values of time before and after $t = 0$.) It is simple to predict the later state of the system by diffusion laws. For example, we can simply say that diffusion will continue until both ends of the bar reach the equal temperature and remain there forever as long as the system remains isolated. But suppose that both ends of the bar have equal temperature (state of equilibrium) at $t = 0$. Is it possible to retrodict the state of the system for all time values before $t = 0$? The answer is in the negative. First, it is impossible to know whether the system has been in the equilibrium state for only a second or for hours, for there is nothing in the equation of heat diffusion that grants this information. Secondly, the state of equilibrium may have been reached by (at least) two equally probable non-equilibrium states in the case of simple systems, such as the one we are considering, i.e. either one end was hot and the other cold or conversely. Therefore we cannot infer by the equation of heat diffusion as to which one of the possibilities has been the case. (In the case of more complex systems, the possible non-equilibrium states can be countless in number.)

On the other hand, suppose that at $t = 0$ we found our system in a non-equilibrium state, i.e., one end of the bar is hotter than the other. Is it then

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[33] Similar examples have been given in order to explain the limitations of retrodiction of thermodynamic systems by several authors, for example, Grünbaum (1964), Rescher (1958), Denbigh (1994).

possible to infer the earlier states of the system? We presupposed that the system is isolated for all earlier time values, that suggests that the diffusion undergoes for all time values before $t = 0$ which is unphysical, because this would mean that the system has come from infinity with one end of the bar having infinitely higher temperature and the other having infinitely lower temperature. (Retrodiction is possible for smaller time values on the assumption of the uniform cooling rate.) Furthermore since we cannot put any restriction with regard the system's earlier states (because this would mean that we have some extra-nomological evidence regarding these periods), retrodiction of this type for classical thermodynamical systems is impossible. I should emphasize that this impossibility arises not as a result of the impossibility of the mathematical operation in solving the equations.

Now, I examine thermodynamical systems at molecular-atomic level (that is, thermodynamics as interpreted in terms of statistical mechanics). As above, I presuppose that the system is isolated for all time values before $t = 0$. The motions of molecules and atoms also obey classical mechanical laws, and thus considered as reversible. Hence it should be expected that retrodiction of the earlier states of the systems that are constituted by molecules and atoms be, in principle, possible. However, I should also remind that behavior of each individual molecule and atom cannot be described, due to the practical impossibility of making observations at that level. Therefore statistical mechanics has been developed to treat these particles in large numbers. The statistical behavior of the particle groups creates another problem with regard to retrodiction that I examine shortly in terms of Bayes' probability theorem, which shows that retrodiction is not as successful as prediction.

Now, suppose that there is a container divided into two parts by a removable partition and one of the parts is filled with a gas. When the partition is removed it can be interpreted in terms of classical thermodynamics that the gas will expand into the chamber until it reaches equilibrium which is determined by uniform distribution of the gas in the chamber. The evolution of the system can be described also in terms of entropy. It is said that the system in which the gas fills one half of the chamber has a lower entropy state. When the divide is removed, the system evolves into higher entropies until it reaches the maximum entropy and remains there so long as the system remains isolated.

When this system is described in terms of statistical mechanics it is said that the system evolves from lower entropy to a maximum, but the system does not remain in this equilibrium state, because there are fluctuations of entropy, i.e., the entropy of the system sometimes decreases from maximum entropy. (This is described in section 2.2. and Figure 4.) Above, I have defined this system as having a beginning; however, I must examine the behavior of the system on the boundary condition that the system is isolated for all time values earlier than $t = 0$. Now, suppose that we find the system at entropy maximum at $t = 0$, is it possible to retrodict the system's earlier states? When the system is at an entropy maximum at $t = 0$ we can say that the system has just evolved from a lower entropy state, because, as seen in figures 3 and 4 in section 2.2., there are countless fluctuations indicating lower entropies. We cannot know the exact path of the evolution of the system that resulted in this particular maximum entropy state, because there may be countless number of possibilities (depending upon the number of molecules in the system) that can result in that particular equilibrium state.

On the other hand, suppose that the system is in a low entropy state at $t = 0$. Then we would infer that this is a small fluctuation (as we have seen in section 2.2. figure 4 larger fluctuations are very rare and the probability of being at the increasing slope of a large fluctuation is, therefore, quite low), and hence an entropy minimum preceded by a higher entropy state. But again, the particular path from which the system has evolved cannot be known.

I have said earlier in this section that retrodiction by statistical laws poses another problem that can be treated in terms of Bayes' probability theorem which was discussed by Watanabe (1955 and 1969) and Costa de Beauregard (1968) through Bayes' formula for conditional probability: $p'_i=q_ip_i/Eq_ip_i$ where p_i's indicate the intrinsic (or prior) probabilities of physical occurrences and q_i's extrinsic (or conditional) probabilities. Intrinsic probabilities depend on the internal dynamics of the system while extrinsic probabilities depend on the external dynamics. The conclusion drawn from this formula is that blind statistical prediction is possible whereas blind statistical retrodiction is impossible. What is meant by 'blind' prediction and retrodiction can be explicated thus: "for prediction, equal q_i's, entailing $p'_i=p_i$, will do, while any sound retrodictive estimation will imply an *a priori* estimation of the extrinsic q_i's" (Costa de Beauregard, 1968, p. 191). That is to say that retrodictive solutions require an assumption of assigning equal values to prior probabilities which is not generally a valid practice (because equal prior probabilities means that the system was isolated), for it may very well be the case that the prior probabilities are different (that is, the system was not isolated and the initial probabilities were determined selectively). This kind of retrodiction is called "blind retrodiction." In this sense, "blind" retrodiction (that is, retrodiction by assigning equal values to prior probabilities) is impossible (with some minor exceptions). (See Appendix B for Watanabe's (1969) detailed analysis of the limits of inference of the prior probabilities from posterior probabilities.) I need to elaborate why assigning the same value to all prior probabilities is not generally unacceptable. Suppose that the physical system in question is in a low entropy state at present. According to Loschmidt's objection, it is equally probable that the system was and will be in a high entropy state in the past and in the future. However, this conclusion presupposes that the system has been isolated in the past and will be so in the future (as we have seen above). If it is certainly known that the system has been isolated assigning the same values to all prior probabilities is valid since all microstates has equal probability (which is shown in Boltzmannian statistical thermodynamics). However, since the knowledge about the state of isolation cannot be known by the laws but only by extra-nomological factors assigning equal values to initial probabilities can be justified conditionally: if the system had been isolated the state of the system at certain time is so and so. But such isolated systems are quite rare in nature and the actual systems are semi-isolated as shown by Reichenbach (1956). Thus if the system had not been isolated or there is not certain evidence that it has not been isolated, supposition of equal prior probabilities is not valid because an intervention may have probably been selective for the initial probabilities. This can be illustrated on the half-destroyed footprint example. If a half-destroyed footprint found at a lonely beach we do not assume equal initial probabilities and conclude that the footprint formed as a result of random processes of waves and wind. We rather argue that the beach has not been isolated all along in the past and a human beachwalker caused the footprint in full shape which has been eroded to gain its present half-destroyed shape.[34]

4.3.3. Systems Involving Primarily Generation and Destruction of Things

In the previous sections, I have explored the limits of retrodiction for the systems that do not involve things coming into and/or going out of existence, but only spatio-temporal and qualitative alteration of things. However, nature abounds with systems having quite many types of change,

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[34] Nevertheless this does not mean that we choose the hypothesis of a beachcomber, because the footprint has a small probability but rather that the hypothesis of a beachcomber increases the probability of the formation of the footprint.

generation and destruction that cannot be accounted for by mechanical laws, because these laws presuppose the existence of certain types of entities and thus is concerned, essentially, with their motion. Hence in this section, I examine the systems involving generation and destruction of things.

Furthermore the entities that are generated may be altered by other forces through time. This alteration can be so radical that may transform the entity into other types of entities, or the entity may retain its identity through time. So there can also be regularities about the alteration of things that can also be formulated as generalizations (or laws).

All these laws that range over the above-type entities can be used in retrodicting the past; however, the problem with this type of retrodiction is that, since the natural systems are mainly quasi-isolated local systems, the entities may be lost from the system that results in loss of information, because the entities as evidence may be beyond reach, or can become unrecognizable due to alteration or overprinting by the later activities. Hence retrodiction by such laws may not be complete.

Below, I examine several artificial systems for different uniformity conditions in order to determine the limits of retrodiction for such systems. The reason that I prefer artificial systems rather than the actual systems is that the actual systems are so complex, and the laws about these systems are so imprecise that analysis of retrodiction becomes extremely difficult and unclear. However, the artificial systems that I produce are simulations of the real systems, so that what I say about these systems go also for the real systems.

CASE I: This is the simplest case that makes retrodiction fully possible. (I should note that the systems I consider are universal systems, if not otherwise indicated.) We have the following boundary conditions[35]: the universe is finite and closed; the number of things in the universe is finite; thus the types of things in the universe are constant through time; the number of generation of things at every interval is constant. The laws are the following: nothing is created or destroyed ex nihilo (the law of conservation); things are generated *only* by fusion of other existing things; the simplest entities are not generated, only destroyed by fusion to make of other complex things (however, not all the simplest things are used at ones to make other things, they are used in an "economical" way); there are deterministic special laws of fusion for each type of thing, such that no thing is generated by more than one way.

Let us now illustrate this case, so that it can be understood better. The first and second boundary conditions are clear enough. As for the third one, we can say, for example, that there are only five types of things, A, B, C, D, E, available at time $t = 0$ (the time of retrodiction). As for the special laws, let us say that B's are always generated by the fusion of two A's, and the boundary condition that 10 A-fusions are possible at every time interval t; C's are always generated by the fusion of one A and one B, and 5 AB-fusions at every t; D's are always generated by the fusion of one B and one C, and 3 BC-fusion at every t; and finally E's are always generated by the fusion of one C and one D, and 1 DC-fusion at every t. There can be many different sets of special laws and the boundary conditions about the frequency of the fusion type, but what is important here is that there is only one fusion type corresponding to the generation of every thing. I should

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[35] We feel to emphasize once more that demarcation between the boundary conditions and laws is not always clear, but in the above cases the distinction seems to us clear enough to keep the discussion going.

make a last remark: I presuppose that A's are in sufficient number, because there is no generation of A's. So A's seem to be the "ultimate" particles of this universe. As regards retrodiction from any time $t = 0$, I also assume that these laws and boundary conditions are unrestrictedly uniform through space and time.

It is obvious that from any time we can retrodict the composition of the universe back in time step by step without running into any difficulty. For instance, if we have 41 B's, 19 C's, 27 D's, and 12 E's (and A's) at $t = 0$, we can infer that at the last interval before $t = 0$ there were 25 A's in addition to other A's at $t = 0$, 39 B's, 18 C's, 25 D's, and 11 E's. This is, in a way, similar to the case of the conservative classical mechanical case. However, I should note that I do not discriminate between the things generated at earlier and later intervals. So we can retrodict the earlier compositions of the system, but not the temporal order of the generation of things.

It is clear that the above system is irreversible, because there is only fusion of the particles, so that the particles that are generated do not decay back to their constituents. Although the system is irreversible and the laws are t-non-invariant (because the solutions of the laws are not symmetrical: see the example of radioactive decay at ft. nt. 36) both retrodiction and prediction is possible, because the laws are deterministic which is evident from the fact that there is only one way in generating things. The system is also deterministic due to the deterministic laws and the uniformity of the boundary conditions determining the number of fusions. For instance, from the same initial conditions (41 B's, 19 C's, 27 D's, and 12 E's (and A's) at $t = 0$) we can retrodict as well as predict the composition of the system for earlier and later time values. It should be noted that although the system is deterministic for the generation of the types of things, it is non-deterministic in the sense that the things that fusion at one interval is random within the limits of the number of the things. However, we cannot know which of the

A's will make B's. Accordingly, if we have B's we cannot identify the A's of which they are made. So retrodiction and prediction are possible within these limits. This is similar in a way to radioactive decay, for although the number of the decaying atoms are constant within the duration of the half-life of that element which atoms decay within that interval is indeterminate.

This is a good point to return to the discussion of traces in section 4.2.2. where I argued against the attempt to define traces in terms of their retrodictive significance. I asserted that the fact that the so-called traces are significant only for retrodiction but not prediction is determined by the field of interest in which the things are interpreted. Case I above and the example of radioactive decay as used in age determinations are excellent example to defend this assertion, because both retrodiction and prediction are equally possible for these systems, and also the entities involved meet the requirements of the intuitive understanding of traces. For instance, B in Case I can be defined genetically as an entity that is generated by the fusion of two A's. This definition can be compared, for instance, with the definitions of meteorite crater (a meteorite crater is an entity generated by meteorite impacts on ground). In addition, the case of radioactive disintegration clearly shows why prediction, though as accurate as retrodiction, does not seem so significant.[36] The reason is that geology,

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[36] The law of radioactive decay states that the number of atoms disintegrating per unit time, $-\mathrm{d}N / \mathrm{d}t$, is proportional to the number of atoms present, N. Thus, $-\mathrm{d}N / \mathrm{d}t = \lambda N$ (1) where λ is the decay constant. The integration of equation (1) gives $N = N_0 e^{-\lambda t}$ (2) where N_0 indicates the number of atoms at time $t = 0$. As for the decay of a parent to a daughter, the decay law for the parent is $-\mathrm{d}P / \mathrm{d}t = \lambda P$ (3) where λ is the total disintegration constant and P the atomic abundance of the parent. For the daughter, on the other hand, the law is expressed as $\mathrm{d}D / \mathrm{d}t = \lambda_D P$ (4) where λ_D is the disintegration constant for the decay $P \rightarrow D$, and D is the atomic abundance of the daughter. Upon the integration of (3) and (4), we obtain the following two equations for the parent and daughter from the initial to the present moment: $P_i = P_p e^{\lambda t}$ (5) and $D_p - D_i = P_p \frac{\lambda_D}{\lambda}(e^{\lambda t} - 1)$ (6) where i and p refer to the initial and the present moments. It is evident that the law described by the equation (1) is t-non-invariant, because time is in the first derivative indicating that it is sensitive to the change of the sign of time. We can see this also from

human history and archaeology in which this method is used are constructed primarily to discover what has happened in the past, not what will happen in the future. So knowing for how long it will take for certain amount of, say uranium, to decay in a certain rock or pottery is of no concern.

As a last remark, the information attainable from the so-called traces is primarily about their origin of these things (in addition to their subsequent alteration). This information is obviously about the past not future; however, if the concern were about their ends prediction rather than retrodiction would be important.

CASE II: If the boundary conditions about the number of fusions at every interval is not constant, while every other boundary conditions and laws are remaining the same as CASE I, retrodiction of the earlier compositions of the system may become extremely difficult, even impossible, depending upon the change in the boundary conditions. If the change is, nevertheless, uniform, (for instance, at every successive interval, the number of fusion of each type is increased in a regular way) retrodiction would be possible. However, if the change has no determinable pattern we would not be able to retrodict (or predict) how many things were (or will be) generated at each time interval. But it cannot be said that retrodiction of the earlier state is completely impossible, because we would at least know, for instance, that B's are generated by the fusion of two A's, and C's by one A and one B, and so on.

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the equations (2), (5), and (6) where the change of the sign of time results in different solutions in two directions of time. However, what is significant for us is that these equation can be solved for both directions equally well. That is, if the present abundance of an isotope is known we can calculate the earlier amounts of it through equation (2) (retrodiction). On the other hand, if we had the initial amount (i.e. the earlier amounts) we can calculate the later amounts of this isotope (prediction). It is obvious that both retrodiction and prediction are equally possible. This is also similar for the equations (5) and (6).

CASE III: In addition to the change in CASE II or to CASE I, if there are more than one way to make the same entity (that is, if the systems, and thus the laws are non-deterministic which is implied by multiple solutions of the law equations) retrodiction is extremely restricted. For instance, the above example can be altered in the following way: one B is made of by the fusion of two A's, one C is made of the fusion of by either one A and one B or three A's, a D is made of by either B and C or two A's and a C or three A's and a B, or five A's, and so on. If we have a C and a D at $t = 0$ the composition of the system can be A, 2B's, C or 3A's, B, C or 4A's and 2B's or 3A's, B, C or 5A's and C or 6A's and B. (For the sake of simplicity, we disregard other things that can be in the system.) So there are six possible compositions at one step back that would increase geometrically. However, retrodiction is possible with some extra laws in very special and numerous cases: for instance, if there is an order in generation of things from simpler to complex ones, or if the more complex things are not generated before the necessary number of the simpler things is reached. Suppose that we have a C at $t = 0$. Now according to the generation law, the earlier step may be 3A's or A and B; however, according to the new law we just provided, 3A's are not possible, because the system must make a B before making a C. So the earlier step is one A and one B.

CASE IV: Suppose furthermore that there is fission (decay) of particles in addition to fusion. In this case, since the complex particles may also decay into their constituents retrodiction of the earlier compositions of the systems from the composition at $t = 0$ is possible, though more complicated than retrodiction without fission, on the condition that the number of fissions at each interval is constant, or changes uniformly. However, if the number of fissions at each interval is not constant, retrodiction becomes impossible. Another comment to be made about this case is that the system is physically reversible due to fission. But, as I said earlier, although we can retrodict the earlier compositions we cannot know which of the particles are generated at each interval.

As would be seen, it is possible to complicate the situation by adding the above-mentioned restriction conditions by which the system approaches the actual systems. Furthermore, in order to see how cases can get more complicated we need consider several other possibilities that are involved in actual cases.

We know that in the real world while things come into being or decay they may also change location, which adds another difficulty in following the earlier cases. In such cases, mechanical laws are also involved in retrodiction in addition to the laws provided above. Furthermore things may also alter in shape, size, color, smell, etc., without transforming things into new sorts. There may be, at least, two consequence of alteration: first, it makes recognizability of the things difficult; second, the order of the generation of things can be retrodicted by such alterations. I said in discussing the above cases that although we can retrodict the earlier compositions of the systems we cannot know the exact time interval at which things are generated. However, if we had regularities of alteration in addition to other regularities the order of generation, at least to a limited degree, can be inferred from the state of the things at the time of inference. For instance, if we knew the original shape of the things in question and also had the regularity of the deformation of the shape of things (e.g. the more deformed things are generated earlier) we could infer that things more deformed were generated earlier. For instance, if have 17 B's, 7 C's, and 3 D's at $t=0$ with the conditions are as described in CASE I, we can retrodict the earlier composition for the last two intervals as 15 B's and 5 C's at $t=1$ and 10 B's at $t=2$. Since at $t=1$ there were no D's we can be sure that all D's were generated by the fusions at the last interval; however, as for C's and B's the situation is different, because since there were already 5 C's and 15 B's at $t = 1$ and 7 C's and 17 B's at $t = 0$, two of C's and B's were already in the system. But, as I said above, we cannot distinguish between the "old" and the "new" C's and B's. Nevertheless, if there were alteration of some kind, e.g. deformation of the shape of the things, and a regularity about it (for example, "things that are more deformed are older") then we can point to two C's and say that they were the new ones, and the others were old.

Another regularity that can provide information about the order of generation may be the fact that in our universe things that are generated form strata, so that those that are below are older and those above are younger. (Of course, these strata may be destroyed to generate other things, but at least some of the strata may be preserved.) In such a case, from this principle of "superposition" we may be able to infer the order of the generation of things.

We have so far assumed that the regularities and the factual conditions as described by the boundary conditions in the universe are spatio-temporally unrestricted; however, it is possible that some of the regularities or the factual conditions be restricted. In such cases, retrodiction beyond the limits of regularities and factual conditions may be extremely difficult or even impossible. Let us consider some of the possibilities on artificial universes.

We supposed in the earlier cases that there is a continuous generation and/or destruction of things at every interval. That is, the generation types (i.e. fusion and fission) and types of things are unrestrictedly uniform. However, it is possible that the generation processes and/or types of things cease at some point of time or new processes and/or completely new types of thing come into existence. For instance, we may first start with A's within the universe, and after certain moment all A's are transformed into other types of things by fusion, so that there may be no A's and A-type fusion. If the laws were formulated in accordance with the processes after the annihilation of A-type fusion they would not describe the generation of such things. On the other hand, it may be possible that, due to the stratification mentioned above, A-type of things may be preserved in some of the strata but they do not interact with other things to allow fusion. So there would be A's available but no A-type fusion at some point of time. In the former, retrodiction is impossible, unless some indirect evidence can be found about A's or A-type fusion. As for the latter, on the other hand, since A-type is an extinct type of fusion no direct inference by laws ranging over A-type fusion is possible; however, by analogy with other fusion types, inference may be possible.

It should be evident from the discussion so far that the rigor of the retrodictive inference is lost as new variables are added to the cases. We go from retrodicting the complete history about the earlier states of the system (CASE I) to only a faint idea about the earlier states. Further difficulties arise if the systems are considered as open local systems instead of single universal systems, for, in such cases, things are lost from or gained into the system. For example, if CASE I is considered as an open local system interacting with the wider environment the laws that are used to retrodict the earlier states of the system becomes highly unreliable, because the addition or subtraction of new things, unless it is regular, cannot be accounted for by these laws that are not designed to trace back these random interactions, which is similar to some of the cases that I discussed for the classical mechanical systems.

4.4. Uniformity of Nature

As we have seen, retrodiction as an inference from later to earlier states presupposes the uniformity of nature, because it means that any point on time coordinate has equal status in retrodicting the earlier states. That is, no period or slice of time should have a privileged status over others in retrodicting the earlier stages. I think that even though the concept of uniformity of nature is used quite often it is not a transparent concept as regards its philosophical nature; hence I examine the idea of uniformity both historically and conceptually in order to reveal its philosophical character in section 4.4.1. Clarification of the philosophical nature of the concept is not sufficient to have an understanding of its extent, because it might mean the uniformity of some deeper level regularities or, in addition, higher level regularities, such as human behavior and thinking. Thus I devote section 4.4.2. to a discussion on the scope of uniformity of nature. Finally, in section 4.4.3., I examine some classifications of uniformity and present a brief account of our understanding of the uniformity of nature.

4.4.1. The Philosophical Character of the Idea of the Uniformity of Nature

There are different conceptions regarding the idea of the uniformity of nature. For some it is a metaphysical presupposition, whereas for others it is a methodological principle. Still others think that it is a verifiable or falsifiable scientific hypothesis, and so on.

Hume (1777) considered the idea of uniformity in connection with causality. As is well known, he rejected causality as an ontological claim, and concluded that the idea of cause is derived from experiencing certain types of events followed regularly by similar types of other events. So it is merely a presumption “that there is some connection between them; some power in the one, by which it infallibly produces the other, and operates with the greatest certainty and strongest necessity.” (p. 88) Accordingly, the idea of the uniformity of nature is also a supposition rather than a conclusion that is not justifiable by experience. However, Kant, by redefining experience, concluded that uniformity as well as causality are contributed by the organizing categories of the human mind. Mach (1893) also asserted that uniformity, which is "the essence of the connection of cause and effect," should not be expected to be found in nature, because "[R]ecurrences of like cases ... exist but in the abstraction which we perform for the purpose of mentally producing the facts." (p. 580)

Whitehead (1925) found the origin of the idea of uniformity in modern science in "fate in Greek Tragedy," i.e. Aristotle's final cause, which was transmitted through the medieval conviction of the rationality of God and Order of Nature. However, he cautioned that,

> I am not arguing that the European trust in the scrutability of nature was logically justified even by its own theology. ... My explanation is that the faith in the possibility of science, generated antecedently to the development of modern scientific theory, is an unconscious derivative from medieval theology. (p. 14)

The view that the uniformity is a metaphysical presupposition is shared by recent philosophers of geology and biology. For example, Simpson (1963) stated that it is a postulate regarding the immanent (as opposed to configurational or contingent) characteristics of the world. (p. 33) Hooykaas (1963) found it as a methodological principle "underlying modern geology and evolutionary biology." (p.1) By distinguishing two types of uniformitarianism[37], namely, substantive and methodological uniformitarianism, Gould (1965) seems also to emphasize its metaphysical/methodological character. Albritton (1967) noted that there is no agreement on the nature of the principle of uniformity, i.e., whether it is an axiom, a postulate, a hypothesis or a law. The opponents of substantive uniformity call it "an a priori assumption, or an anti-historical dogma." (p. 1)

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[37] Uniformitarianism is Charles Lyell's formulation of the uniformity of nature in combination with presentism.

Albritton (1967) himself claimed that the principle of uniformity can be reduced to the principle of simplicity that things are not to be multiplied without necessity (Occam's Razor). When the actualistic model (see James Hutton below) cannot explain some phenomena a new theoretical model is invented, but models are not to be multiplied without necessity. This view is also shared by Goodman (1967), who seems to interpret uniformity as a supposition underlying geology. Şengör (1991) argued in favor of the uniformity as a metaphysical presupposition, because he believed that this principle cannot be empirically falsified.

J. S. Mill (1862) found an intimate connection between induction and uniformity of nature. "[T]here is a principle implied in the very statement of what Induction is; an assumption with regard to the course of nature and the order of the universe." (p. 340) He also asserted that uniformity of the course of nature "is our warrant for all inferences from experience." (p. 340) However, he rejected the Kantian response that uniformity is "[t]he disposition of the human mind to generalize from experience." (p. 342) Furthermore he also objected to the view that "the proposition that the course of nature is uniform, is the fundamental principle, or general axiom, of Induction. ... On the contrary, I hold it to be itself an instance of induction." (p. 342) Then if uniformity of nature is an inductive generalization, the verification problem connected with induction also applies to uniformity. That is, uniformity of nature cannot be verified.

According to Russell (1917), even though philosophy has kept a continuous interest on causality as "one of the fundamental axioms or postulates of science, yet oddly enough, in advanced sciences, such as gravitational astronomy, the word 'cause' never occurs." (p.173) Instead, science accepts on "inductive grounds" the uniformity of nature which, however, "does not assert the trivial principle 'same cause, same effect', but the principle of the permanence of laws." (p. 188) So he seems to think that the uniformity of nature is not an a priori principle but rather an inductive generalization, that, therefore, "cannot be considered certain, but only probable to a degree which cannot be accurately estimated." (p. 188) Although he did not rejected the uniformity of nature he objected the idea that it is a universal principle. He rather thought that there are uniformities of smaller scope that are more probable than those with larger scopes. So both Mill and Russell believed that the uniformity of nature is a verifiable general principle.

A related view is derived from the attempt to reduce the principle of uniformity to the principle of simplicity which is not peculiar to geology (Gould, 1965, Albritton, 1967, Goodman, 1967). It is said of the uniformity of nature that the general features of the universe at the present hold for all space and time values is the principle of simplicity as defined by Occam's Razor: do not multiply the variables beyond necessity.

Simpson (1970), contrary to his earlier view, argued that the principle of uniformity as understood in geology is confirmable by the astronomical evidence as well. He based this change of opinion on McCrea (1968), who stated that we have no evidence contrary to the idea of uniformity which is brought to us by cosmic rays and particle emission from other parts of the universe. Şengör (1993), arguing along the same line of thought, confessed that his earlier opinion of the uniformity of nature as a metaphysical presupposition had been wrong, for the astronomical evidence that come from not only from other parts of the universe but also earlier *times* of the universe due to our chance to see the past of the universe as far back as 10^4 million years shows that the principle of uniformity is a falsifiable scientific hypothesis.

I should mention two more geologists who introduced the idea of uniformity of nature explicitly into geology and used it systematically to solve geological problems. Although they did not discuss the philosophical aspects of uniformity we can derive their views from their works. The first is James Hutton, a distinguished eighteenth century geologist, who proposed the principle of actualism that the present is the key to the past, considered uniformity as a presupposition that makes geology and all other sciences possible. Charles Lyell, one of the major nineteenth century geologists, on the other hand, who defended a strict uniformity (at least, until his later years), seems to have thought of uniformity as a confirmable hypothesis, because he presented countless evidence to support this hypothesis in his three volumes of *Principles of Geology.*

4.4.2. The Extent of Uniformity

In this section, I review briefly the views of several scientists and philosophers as regards the extent of uniformity. James Hutton, whose earth model is mechanistic, stated that lands are eroded and washed away into the sea and the material that forms strata at the bottom of the sea are elevated above the sea by the heat of the earth (volcanism) to complete the circle. This process goes on and on forever with *no vestige of a beginning and no prospect of an end.* According to Simpson (1970), since there is not a beginning, nor and end, but unending repetition, this "is indeed a doctrine of uniformity, of a historical steady state overall, although it describes a dynamic, cyclic equilibrium and not a static one. " (p. 260)

Hutton's view of uniformity includes not only the fundamental physical uniformities as described in physics and chemistry, but also most of the basic geological processes. Nevertheless although he did not exclude the possibility of some "catastrophic" changes he thought that even they are uniform, thus accountable by the present data and laws. Hooykaas (1963) argued that, according to Hutton, "the principle of uniformity really meant that the thing that is, has always been. ... This uniformity was extended also to the organic world, Man excepted." (pp. 143-144) So it seems that Hutton did not go so far as to extend uniformity to the human domain which diverts Hutton from the extreme position. Furthermore I think Hutton's actualism envisage a dynamic progress within the same cycle which, nevertheless, is repeated to establish an overall steady state if all cycles are taken into account.

Charles Lyell, on the other hand, was more strict in his understanding of the uniformity of nature. He assumed that geological processes, e.g., erosion, sedimentation, and the material of processes, e.g., amount of total volume of water in earth, composition of atmosphere had been the same as they were at Lyell's time. They had to be constant, otherwise they could not be stated as repeatable laws. However, of course, there was change but not in those that made change possible. He also conceived very long duration of time for the earth and very slow changes which would make contemporary land-sea distribution and other geological monuments, such as mountains possible.[38]

> [I]f in any part of the globe the energy of a cause appears to have decreased, it is always probable, that the diminution of intensity in its action is merely local, and that its force is unimpaired, when the whole globe considered. (pp. 164-165)

According to Hooykaas (1963), "Lyell expressly denied that he was contending for the absolute uniformity throughout all the sublunary events. ... But ... he [did not wish] ... to deny that the order of nature has always been uniform." (p. 144)[39] Simpson (1970) also emphasized that by uniformity, Lyell "meant more than constancy of applicable physical laws or

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[38] Although his strict uniformity did not even allow a Darwinian type of biological evolution he later softened his views to allow organic evolution.

[39] As regards Georges Cuvier, who was an opponent of Lyell and uniformity in geology and biology, Hooykaas (1963) contended that he "considered the history of humanity as more uniform than the history of nature." (p. 150) However, since the last revolution, i.e. the last catastrophe, " 'the present causes' are sufficient to explain geological changes." (p. 150)

invariance of immanent characteristics of the universe. ... he also meant that present causes are sufficient to explain past physical changes." (p. 262) Gillispie (1959) also stated that Lyell "did not, of course, deny the reality of change, but he insisted that all change had been uniform, proceeding in cycles in time rather like the orbits in space through which the planets swing." (p.127)

We have seen in the last section that, according to Whitehead (1925), the idea of the order of nature is founded on the medieval conviction in the rationality of God and Order of Nature. It is interesting to note that one of the sources of the idea of non-uniformity in geology and biology is also religion. The idea was derived from the Biblical Genesis that the world had undergone several world-wide revolutions, and has remained unchanged since the last revolution, namely the Mosaic flood. The biblical interpretations were easily accorded with Abraham Gottlob Werner's, the great eighteenth century mineralogist, neptunist hypothesis that all rocks were formed by successive precipitations or sedimentations from the primordial ocean that had once covered the earth. For example, according to Richard Kirwan, the Irish Chemist, "it is easy to reconcile the neptunist succession of rocks with the mosaic account; geological exegesis unfolds the Genesis story step by step." (Hallam, 1984, p. 34) This view implies a radical sort of non-uniformity as regards geological and biological uniformities, for each epoch has its peculiar type of objects and relations (both synchronic and diachronic) between objects. Nevertheless, it should be noted that they did not deny the unrestrictedness of the fundamental regularities.

Another source of non-uniformity is the world-wide geological unconformities (i.e. discontinuities in rock and fossil record) that were interpreted by Georges Cuiver as mass extinctions that were caused by catastrophic floods. These cataclystic events were connected with the religious revolutions by, for instance, Rev. William Buckland, one of Cuvier's pupils and the most influential geologist at his time (Gillispie, 1959, pp. 98-120). However, Buckland thought that "the six days of creation were not to be taken figuratively, and long periods of time might have elapsed between successive acts of creation." (Hallam, 1984, p. 41) His was an "empirical" theology. Adam Sedgwick believed that the truth of the Deluge and evidence for it should be established scientifically. Non-uniformity is more radical in biological cases (for instance, Cuvier's recreation of new species at different epochs); however, even their catastrophist view allow some sort of uniformity. For instance, Cuvier did not deny that there may have been a uniformity since the last catastrophe.

In the twentieth century, a somewhat opposite route was followed by Velikovsky, who attempted to explain the recent history by the "evidence" that he found in common mythology and scripture. The idea is that prehistoric records must have been influenced by the observed world-wide (mainly extraterrestrial) catastrophic events. Furthermore a contemporary hypothesis regarding the discontinuity in fossil records at the Cretaceous-Tertiary boundary is similar in the spirit. It is claimed that this discontinuity points to a world-wide extinction which was caused by a large meteorite impact. The views of the latter type, i.e. neo-catastrophism, are not altogether against the idea of uniformity in geological and biological domain, for they allow even the continuity of some species. Non-uniformity may not be so radical at least for the kinds of things, but the general attitude against the uniformity of the rates of processes is more prominent.

The idea of uniformity is not, in general, a respectable notion in the historical realm; nevertheless there have been supporters of this view. For instance, Hume (1777) said that "Would you know the sentiments of inclinations, and course of life of the Greeks and Romans? Study well the temper and actions of the French and English" (p. 83) Von Leyden (1984) asserted that "one can explain Hume's stance as an expression of the doctrine of uniformity, a doctrine widespread in the eighteenth century in the wake of Machiavelli's and Guicciardini's belief that "the world was always of a kind"." (p. 64) Furthermore the proponents of the so-called historicists and the positivists in the nineteenth century (for instance, Comte, J. S. Mill, Marx) argued that, in addition to the fundamental regularities of the physical and chemical world, there are historical regularities similar to the astronomical laws.

The contemporary forms of historical uniformity thesis are more moderate. It is claimed that there are limited generalizations or even laws about the behavior of single persons and of societies that are formulated by the human sciences, such as psychology, and sociology. It is also asserted, though not explicitly stated, that there are limited historical generalizations in the sense of past sociology and psychology (for instance, Popper, 1945, Joynt and Rescher, 1961, Gardiner, 1961, Hempel, 1965, Murphey, 1973). If such generalizations or laws are possible then it can be said that there are certain uniformities of human behavior, action, etc., though of restricted in scope. However, it does not follow that there is a single universal law accounting for the evolution of human society in general. For instance, Popper (1960) asserted that "[Darwin's evolutionary hypothesis] is not a universal law, even though certain universal laws of nature, such as laws of heredity, segregation, and mutation, enter with it into the explanation." (pp. 108-109) He grounded this assertion on the claim that "[t]he evolution of life on earth, or of human society, is a unique historical process." (p. 108) So although there are regularities of human or social behavior these do not add up to a single comprehensive law as claimed in Marxist historical materialism.

The idealist philosophers of history, e.g. Croce and Collingwood, on the other hand, claimed that historical events are unique. What is intended by 'all events are unique' is that there are no regularities whatsoever in the historical domain. There is a difference between saying that 'life on earth is a unique process' and 'all historical events are unique,' for, as Popper noted, even if the whole process may be unique there can be restricted or unrestricted regularities governing the relations of the objects contributing to this process.

As a final remark, although the view that the fundamental regularities are uniform throughout the past is a common conviction and is presupposed above in examining retrodiction for mechanical and thermodynamic systems there have been objections to this view. It is argued that the natural laws or the regularities are not uniform, because if the cyclical big bang theories are correct then these laws of nature apply only to this cycle, and thus are not uniform (for example, Nagel, 1961, p. 57, Kitts and Kitts, 1979, p. 618, and Bunzl, 1993, pp. 96-97; see also Appendix C, 'Restricted Laws'). Furthermore Balashov (1992) examined the idea of the evolution of the laws of nature.

> As an instance, the laws of chemical phenomena could not *come into existence* until the objects had been formed that were capable of interacting chemically. It happened obviously during the definite cosmic epoch when the temperature fell sufficiently to enable formation and stable existence of neutral atoms and then molecules of various substances. Prior to this epoch there were no chemical laws. (p. 346)

Against the objection that such restricted laws as chemical, biological, etc., are not fundamental laws that do not evolve or do not have a beginning, Balashov responded that "[t]he laws of nature in no sense 'precede' the Universe," for "[t]here is only one universe, and its evolution is perhaps the unique cosmological event." (p. 355) Moreover "[n]ot only are the substrate properties of the Universe subject to particular laws of evolution. The laws themselves may depend on the result of their own action, that is on the particular physical state the Universe happens to be in as it undergoes its evolutionary development." (p. 356)

The above idea of the evolution of the laws of nature goes back to P. A. M. Dirac's hypothesis that the cosmological constants have been changing. Jordan (1971) and Carey (1976) claimed that this hypothesis can be tested on geological evidence, for geology is concerned with a very long span of time that may allow to observe some changes that may have resulted from the changes in the cosmological constants. The general idea of testing this claim is that the decrease in the gravitational constant would result in the expansion of the earth during the last 4.5 billion years. They believed that they found this in the fragmented nature of the earth's crust that has been accepted as a fact in the last a few decades and has given rise to the theory of plate tectonics.

4.4.3. Classification of Uniformity

In this section, in order to determine the meaning of uniformity, I critically examine different classifications of uniformity. At the end, I provide my interpretation of the meaning of uniformity.

Hubbert (1967) ordered four nonequivalent answers to the question of the principle of uniformity of nature as understood in geology.

> (1) The present is the key to the past.
>
> (2) Former changes of the earth's surface may be explained by reference to causes now in operation.
>
> (3) The history of the earth may be deciphered in terms of present observations, on the assumption that physical and chemical laws are invariant with time.

> (4) Not only are physical laws uniform, that is invariant with time, but the events of the geologic past have proceeded at an approximately uniform rate, and have involved the same processes as those which occur at present. (p. 4)

As it is evident from the above classification, uniformity of nature is sometimes used synonymously with presentism (or "actualism" as called in geology), i.e. acquiring the knowledge of the past by the present evidence. The first three provide different conditions of presentism. Type (1) does not even presuppose any uniformity of nature but just indicates that the past can be known on the basis of the present evidence. Although uniformity of nature is implicitly assumed in similar definitions many philosophers of history reject uniformity in historical domain, while using the present as a key (to a very restricted sense) to know (or understand) the past. Nevertheless, we know that the dictum 'the present is the key to the past' was proposed by James Hutton together with a presupposition of the uniformity of nature. Type (2) is a more explicit version of type (1), though still unclear, for the extent of the presupposed uniformity is not so clearly determined. In type (3), on the other hand, some kind of uniformity is presupposed; however, it is not clear as to whether 'laws' refers to natural or scientific laws. If it refers to natural laws then 'invariance' refers to the uniformity of recurrent phenomena. However, if they are scientific hypotheses then 'invariance' may refer to the survival of these hypotheses in scientific practice which is not relevant here. Type (4) shows that by 'invariance or uniformity of laws,' Hubbert meant the physical regularities, because the second part provides other conditions of physical uniformity. I need elaborate on this type of uniformity that is not very clear. If it means that the present geological regularities about the rates and types of geological events were the same in the past it is not different from the uniformity of the so-called natural laws as mentioned in the first half of the expression. If, however, it means that some of the present geological things or events have been constant it is about the continuity of the very same thing or event. For example, if it is said that the present polar ice-caps have always been at the same place with approximately the same form and quantity, or that the salinity of the oceans have been constant it is not about the uniformity of a certain regularity but rather about the uniformity of a particular thing. (I return this matter again in this section.)

Gould (1965) distinguished two types of uniformitarianism: *Substantive uniformitarianism,* that geologic processes of the past have proceeded at relatively uniform rate as those which occur at the present, and *methodological uniformitarianism,* that physical and chemical laws are invariant in time. Albritton (1967) asserted that "proponents of substantive uniformitarianism are inclined to interpret geologic history in terms of changes that are more even and cyclic than episodic or variable." (p. 1)

Gould's substantive and methodological uniformitarianisms seem to be about the uniformity of nature, if physical and chemical laws are understood as natural laws or regularities. However, if methodological uniformitarianism is interpreted as "comparing past with present" (Rudwick, 1972, p. 188) it is a presentist notion. Furthermore substantive uniformitarianism includes the constancy of the particular conditions as mentioned above.

Hooykaas' (1963) classification which distinguishes seven types of uniformity-non-uniformity in geological and biological domains goes continuously from the strict uniformitarianism (uniformity) to catastrophism (non-uniformity). These types are not presentist ideas but about the uniformity of nature.

> I. What happens now, happened always in the same manner, at the same tempo and on the same level. ... Animal species either remain the same or are replaced by species belonging the same genus. ...

II. There is a gradual evolution with a constant velocity. ... The uniformity is in the *rate of change* of the situation ... ; the uniformity does not refer to the state of being ... but to the act of becoming. ...

III. ... The rate of geological change (the tempo) and the velocity of evolution are slackening ... The idea that the intensity of geological changes is gradually decreasing is not strictly uniformitarian, but, of course, there could be a *uniformity in the retardation* of the rate of change.

IV. Geological change is supposed to happen in alternate periods of orogenetic activity and periods of rest in which erosion preponderates. ... The repetitive unit consists of a period of activity and a period of rest. ...

V. In paleontological history periods of rapid evolution ... or even extremely rapid evolution ... alternate with epochs of gradual orthogenetic development. ...

VI. If no progression or evolution be supposed, it may be held that the *character* of geological changes is permanent, but their *energy* has abated. ...

kind and energy, or in kind but not in energy. As for the latter, the present causes differ from the past causes not in kind but in energy, or they do not differ in any respect at all.

Rudwick (1972, pp. 164-217) distinguished four types of uniformities in Lyell's *Principles* that are more explicitly stated by Gould (1987), who discussed them at length. These are the uniformity of law, the uniformity of process, uniformity of rate, or gradualism, and uniformity of state, or nonprogressionism. (Gould, 1987, pp. 119-123) According to Gould, the first states that "[n]atural laws are constant in space and time." (p. 119) He interpreted the second as a methodological principle. "If a past phenomenon can be rendered as the result of a process now acting, do not invent an extinct or unknown cause as its explanation." (p. 120) This means that the present causes or processes do not differ in kind from the past ones which is similar to the first type of Hooykaas' (1970) actualistic conceptions. So Gould asserted that "[t]he first two uniformities are geology's version of fundamental principles—induction and simplicity." (p. 120) The distinction between the two is that the first is about regularities at a deeper level (as explored by physics and chemistry) the second is about geological regularities. Many conceive physical and chemical regularities as natural laws, geologic regularities, on the other hand, as accidental. (I argue against this notion in Appendix C.) However, both types of uniformity are, in principle, the same, for they are about regularities. The third means, according to Gould, that "[t]he pace of change is usually slow, steady, and gradual." (p. 120) The fourth is the most controversial claim of Lyell's uniformitarianism.

> Change is not only stately and evenly distributed throughout space and time; the history of our earth also follows no vector of progress in any inexorable direction. Our planet always looked and behaved just about as it does now. Change is continuous, but leads nowhere. The earth is in balance or dynamic steady state; therefore, we can use its current *order* (not only its laws and rates of change) to infer its past. (p. 123)

The fourth type is typically the constancy of a particular condition, for it asserts that the present state of the earth has always been the same, or approximately the same.

I can now introduce my interpretation of the idea of the uniformity of nature. Uniformity is generally used in two related meanings, namely regularity and continuity. Recurrence of phenomena is a regularity that constitutes the first meaning of uniformity. Regularities (uniformities) may range over different sorts of things and relations, such as, atomic and sub-atomic particles, molecules, layers of rock, genes, human behavior, social groups and behaviors, and so on. Regularities may extend unrestrictedly through time which is uniformity as the spatio-temporal *continuity*. According to this formulation, the uniformity of nature can be construed as spatio-temporal continuity. However, I need emphasize that continuity or uniformity does not necessarily imply spatio-temporal unrestrictedness, because regularities or uniformities may also be spatio-temporally restricted. It can then be asserted that there may be regularities of nature ranging over different things and relations with varying degrees of spatio-temporal continuity. In other words, regularities can have different scopes.

Accordingly, regularities or uniformities are the objects of different sciences from physics to chemistry, geology, and biology to human sciences and history. Hence the counterparts of regularities or uniformities in science are laws or generalizations. That is, there is a correspondence between regularities and laws or generalizations; however, this is not a one-to-one correspondence, because we cannot be sure that our constructions *really* represent the reality as it is. In addition, as regularities or uniformities have different scopes, so do generalizations or laws. That is, some laws may be spatio-temporally unrestricted, whereas other may be restricted (Appendix C).

Moreover I need emphasize that scientific laws are not the laws of nature. Regularities only make the laws possible, they do not guarantee their accuracy. Hence I do not use the term 'uniformity of laws,' which is common in the literature (for instance, Hubbert, 1967, Gould, 1965, 1987, Rudwick, 1972). However, it is not always clear whether regularities (i.e. the so-called laws of nature) or scientific laws[40] are intended. If it means the laws of nature the uniformity of laws means spatio-temporal continuity of the regularities. So uniformity in this sense is an ontological notion. If it means the uniformity of scientific laws it becomes confusing, because scientific laws are constructions and thus can have scope not uniformity. As Goodman (1967) stated, the laws that we think as uniform are our constructions and may change through time or from phenomenon to phenomenon. Nature does not behave according to our formulated laws but we hope to understand and formulate laws to be true, at least, for some time or for some phenomena. Therefore we should rather speak of the scope, not the uniformity, of scientific laws.

Furthermore there are also constancies of particular conditions that can be considered within the context of the uniformity of nature. In this sense, uniformity means continuity, not regularity or recurrence. These constancies or uniformities of particular conditions constitute the boundary

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[40] We should make it clear that, in our usage, 'regularity' corresponds, more or less, to 'laws of nature' or 'natural law.' On the other hand, a scientific law is a statement or formulation of a regularity, because we think that laws, i.e. scientific laws, are general scientific hypotheses constructed by us to account for the natural phenomena. (We also admit the view that the natural phenomena are also constructed with respect to the theory that one favors.)

conditions of retrodictions. Since laws cannot be applied for undetermined conditions the initial and boundary conditions must be specified, so that the laws can work within these limits. For instance, the statement that 'the salinity of the ocean waters has been the same for such and such period' directs the application of the relevant law that it is restricted to this particular condition. However, I am aware of the problem that there is not a universal definition of the term 'boundary condition' that would help to distinguish laws and boundary conditions. In many cases, laws and boundary conditions are distinguished intuitively; however, some boundary cases make it difficult to demarcate between the two. For instance, is the speed of light in vacuum a law or a boundary condition? There are proponents of both views. So for this reason, I discriminate laws and boundary conditions intuitively, and for each case, so that if a statement does not seem to be a law it is a boundary condition.

4.5. Significance of the Time of Retrodiction

In the last three section, I considered retrodiction as an inference from later to earlier states which presupposes that the system during its career is so uniform that the temporal points are equally suitable for retrodiction by laws and the initial and boundary conditions. However, it does not follow that every temporal point is the same, for the things and relations that exist are available only at a particular point of time. It might only mean that this difference may not be significant in retrodicting the earlier states of certain systems, because from each temporal positions the same history, except an excess of states associated with the temporal position, can be retrodicted. For other systems, on the other hand, the time of retrodiction becomes significant, because part of the history of the systems cannot be reached from one temporal position with the available things and relations but can be retrodicted from (probably an earlier) position with different things and relations. Below I illustrate this matter through several examples.

As we have seen above, for conservative classical mechanical systems, the time of retrodiction is insignificant so far as the necessary boundary conditions and the laws are unrestricted, because we can retrodict the same history equally well from any moment with the relevant initial conditions. This can be illustrated thus: retrodiction from $t = t_n$ results in the earlier states, s_n, s_{n-1} s_{n-2}, s_{n-3}, ... s_1 (s_n being the state of the system at t_n), while from $t = t_{n+1}$ it is s_{n+1}, s_n, s_{n-1}, s_{n-2}, s_{n-3}, ... s_1 (s_{n+1} the state of the system at t_{n+1}). The inference from $t = t_{n+1}$ gives additional information which is the result of the fact that the system has evolved from s_n to s_{n+1} from t_n to t_{n+1}. Despite this difference, which is the result of different initial conditions for each retrodiction, it is obvious that the time of retrodiction is insignificant, because both inferences give the same history, except the interval between the two time points. So retrodictions for such systems can be treated as inferences from later to earlier states.

I said in section 4.3.1. that if a non-conservative classical mechanical system is in a state of rest we cannot retrodict the earlier states of this system *by* classical mechanical laws. That is, the moment of retrodiction is important for non-conservative systems, because if, for instance, a body is at rest at $t = t_2$, and in motion at $t = t_1$ ($t_1 \langle t_2$) we can retrodict, with some restrictions, from $t = t_1$ but not from $t = t_2$. The reason for this is that as a non-conservative system proceeds energy is dissipated from this system as a result of non-conservative forces, such as frictional force. That is, the momentum is lost (in other words, energy is not conserved), for mechanical energy is transformed, in part, into heat that is no longer within the domain of classical (macro) mechanics.[41] Energy transformed into heat may either

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[41] Heat is accounted for by statistical micro-mechanics as motion of the particles.

leave or remain within the system. If it leaves the system it is out of reach. But even if it remains within the system, it is not any more the object of classical mechanics, because heat is studied in classical thermodynamics. So as the system, e.g. several billiard-balls in motion, loses all its energy by dissipation the system comes to a halt. Now if a system is found in such a state, and there is no information as to whether the system is a conservative classical mechanical system (if so, retrodiction is possible) retrodiction of the earlier states of the system cannot be possible by the classical mechanical laws, because these laws are not so designed as to apply to equilibrium states, unless dissipated energy could be somehow accounted for as a positive force accelerating the bodies at rest. Now I should remind that if the system were found in a state of motion, and if it were a rather simple system, retrodiction of the positions of the bodies could be possible for smaller time values (for greater time values the system blows up), if, of course, the non-conservative forces can be reversed as positive forces. The difference between the two systems (i.e. at rest and in motion) is that we can estimate the non-conservative forces by observing the system in motion, whereas we do not have this chance if the system is at rest, because the non-conservative forces, even as negative forces, cannot be observed for this system. Now it follows that the temporal position at which the system is found determines retrodicting the earlier states. The situation is very similar for classical thermodynamic cases.

Nevertheless this does not mean that we cannot know anything about the past of such systems. "Extra-nomological factors" may help in retrodicting the past to a certain extent. Part of the heat (energy), that is generated as a result of the action of the non-conservative forces, do *work* which may generate new entities that might survive long enough to be observed. For instance, when a car suddenly stops, mechanical energy at the wheels are transformed into heat by the friction between the wheels and breaks and wheels and the ground. Some of this heat is "lost" by dissipation into the environment, while part of it generates "tracks" on the ground. It is similar for the classical thermodynamic systems for which retrodiction of the earlier states is extremely difficult, even impossible, especially when the system is in equilibrium conditions. However, as in the case of the non-conservative system, i.e. the car, retrodiction can be possible to certain extent on the basis of "extra-nomological" evidence that may occur within the systems. Let us illustrate this by an example. If there is hot magma that intruded into cold rocks it cools by diffusion of heat into the surrounding rocks and finally reaches the equilibrium condition. According to our discussion in the previous section, retrodiction of the earlier states of this system, if found in this equilibrium state, by classical thermodynamic laws is impossible. Nonetheless this is not the whole truth about retrodiction of the past state of this system. Even though the diffusion laws do not provide any answer other regularities about the magmas can be of help. It has been observed that as hot magma cools down certain minerals start to form in a determined order. The existence of some of these mineral, that are called "geothermometers," can be used to estimate the initial temperature of the magma that may then be used to retrodict development of the magma by the diffusion laws. The regularities about the generated entities, such as tracks and minerals, can be formulated as laws that may or may not be explained by more fundamental physical and chemical laws.[42] The question as to whether the time of retrodiction is significant can be answered by

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[42] At this point, we should say a few words about "extra-nomological evidence," such as tracks and minerals. The claim that tracks and any other entities are "traces" or "remains" of the past is groundless, for tracks are extra-nomological only relative to classical mechanics, because there can be theories relative to which these so-called traces are not extra-nomological (e.g. geological theories). Hence they are not, in principle, different from Newtonian particles. Interpreting tracks or minerals as remains of the past and Newtonian particles as "ordinary" things is derived from an incorrect view that there is a privileged class of things that are traces. Both the Newtonian particles and tracks are physical entities that are the objects of different theories that construe things as temporal objects with "histories" of their own. Retrodicting the past of these different types of thing is, in principle, the same.

examining the systems involving generation and destruction of things that I investigated in section 4.3.3.

If the systems are like the Case I in section 4.3.3. temporal positions are not significant, because the earlier states can be retrodicted from any temporal position uniformly. However, there are situations for which temporal position becomes important. For instance, types of things may not be uniformly distributed in time. That is, certain types of things and/or processes (e.g. fusion or fission) may be restricted only to a certain temporal period. In this case, retrodiction from a later position beyond the "extinction" of the things and generation types is not possible. This fact suggests that, at least for such cases, the time of retrodiction becomes significant, because the temporal dimension in this respect is not homogeneous. Hence the time of retrodiction must be taken into account. That is, it is possible that inferences from two distinct temporal positions lead to different histories. This can be illustrated in the following way. From t_n we may be able to infer the states s_n, s_{n-1}, s_{n-2}, ... s_1, whereas from t_{n+1}, before which there was total destruction of certain types of things, we may infer only a portion of the states, such as s_{n+1}, s_n, s_{n-1}, ... s_{10}. The states from s_9 to s_1 may remain unknown or unkowable to us from t_{n+1}. The reason for this is that things, or relations (and the laws formulated on the basis of the observed regularities at that point of time) that are necessary to infer the states from s_{10} to s_1 may not be available at t_{n+1}.

It follows from the above that if the time of retrodiction is significant, at least, for certain types of systems the present at which we are given the things, relations, regularities, and so on, is also important, for from the present history of certain time periods may be completely out of reach or extremely limited. For this reason, retrodiction from the present to the past requires a special treatment. However, we ought to examine the concepts of the present and the past before analyzing retrodiction for the present and the past.

4.5.1. Sense and Content of the Present

I noted in section 3.1. that McTaggart (1908) distinguished between A and B series of time. In the former, "each position [in time] is either Past, Present, or Future," whereas in the latter "[e]ach position is Earlier than some and later than some of the other positions." (p. 24) However, the distinction between past, present, and future derives its force not from the conceptual analyses like McTaggart's, but rather from experience. According to Rotenstreich (1958), "[t]he present offers a meeting place for man and his world. The present is an occurrence, and act of meeting, and the moment when the meeting takes place." (pp. 79-80)

I hold the empiricist position that "[t]he concept of presentness is built into the concept of experience, so that it is analytically necessary that one can only experience the present." (Danto, 1965, p. 92) Grünbaum (1970), on the other hand, stated that

> M's experience of the event at time *t* is coupled with an awareness of the temporal coincidence of his experience of the event with a state of *knowing* that he has that experience at all. In other words, M experiences the event at *t and* knows that he is experiencing it. Thus, presentness or nowness of an event requires conceptual awareness of the presentational immediacy of the experience of the event. (155-6)

Denbigh (1981) also asserted that experience by itself is not sufficient. "When we are attending to our affairs, just living, temporality is not at all noticeable; we don't even experience a present." (p. 16) Furthermore although he agreed with Grünbaum he contended that "the present owes much of its particular character to ... the existence of memory." (p. 50) For "in the absence of memory, I would have no power ... for comparing what *is* with what *was*. All earlier experience would be wiped out at the very moment of its ceasing to be 'present' and the present would thus not stand out as being the present." (p. 50)

I agree with Grünbaum that an awareness of the temporal coincidence of the experience is necessary for the sense of presentness. However, I do not think that neither the sense of presentness nor the awareness requires memory. For we can think of a creature that lives in a world in which there are only a few different types of stimulus to which the being can respond in accordance with its built-in structure (for example, genetic structure). It can react automatically to every type of stimulus as a unique event, and thus can survive in this world, unless there is change in the conditions that are not included in its built-in structure. Furthermore self-awareness can also be built-in like a computer command that is activated each time by the experience, so that the entity can be said to be aware of the temporal coincidence of its experiencing a thing. In this sense, its awareness and sense of present are discontinuous, like "blinking." The consequence of the above view is that the psychic or subjective present is a specious moment of experience. It is like a point on a line. Any point on this line is counter-factually a present even though the temporal positions of a subject is a contingent matter.

Even though the psychic present is a specious moment and apprehended immediately by the awareness and experience the historical present is not a specious moment of the psychic experience, for "[u]nlike the psychic present, the historical present is not a dimension of *experience*." (Rotenstreich, 1958, p. 100) It is the dimension of the datum that is not restricted to a single subject. It may be defined by a generation or even by an era. This is evident from the terms like "modern age," "twentieth century." Since such long periods could not have been experienced by a single subject the historical present, as an "extended" present, appears to be a constructed notion. Rotenstreich (1958) also emphasized this: "Since there is no subject taken for granted in the historical realm, the historical present is totally constructed." (p. 101) The extension of the present is determined relative to the field of interest in such a way that any point within these periods is equivalent in the sense of being indistinctive (but not indistinguishable).

The content of the present is constituted by physical things (including memories as physical "traces") and spatio-temporal relations between things. More precisely, a thing is the object of psychic present which is a specious moment. However, I said above that historical present is not a specious moment, but rather is extended to cover many non-contemporary psychic presents. Nevertheless, this duration does not prevent us from treating every point in this interval as temporally equivalent in the sense of being the same present. Accordingly, a thing may have only negligible differences within a time interval as regards its qualitative features and spatial positions which cause us to experience this thing within this interval as one and the same thing. In this sense, a physical thing can be defined by an interval not by an instant. So a thing within an interval, which may correspond to an extended present, is the same for that interval. In other words, the thing is considered unchanged within the extended present, so that it is also, in a sense, an "extended" object. For instance, a mountain appears the same even to successive generations taking up a very long temporal interval.

On the other hand, a thing within a time interval may change spatial positions and/or alter qualitatively. Furthermore these changes in spatial position and qualities can be so radical that it may not be possible to trace the history of a thing as the same thing from a certain time point forward. This is a thing going out of existence. In such cases, either this disappearance may lead to appearance of a new sort of thing, or no new thing may be detectable. These things and relations that are presented to us within the present are the source of the regularities that are formulated as laws, because we observe within the extended present that certain things and relation recur. These recurrences are regularities that can be formulated as laws which are utilized in retrodiction.

4.5.2. The Idea of the Past

I said that the idea of present is immediately given by experience, and all we have are the things and relations manifested to us within the present. How then is it possible to make judgments about the past that is beyond reach? What is the origin of the idea of past? In order to overcome this problem, C. I. Lewis offered the term 'marks of pastness' that the present objects bear (from Danto, 1965, p. 39). Similarly, Russell (1921), on the other hand, suggested that "[t]here may be a specific feeling which could be called the feeling of "pastness," especially where immediate memory is concerned." (p. 162) Danto claimed that this problem arises from the empiricist assumption that our experience is temporally neutral, which, he thought, is unrealistic. (pp. 92-93) However, he maintained that

> [J]ust to apprehend something as *evidence* is already to have gone beyond the stage of merely making statements about *it:* to count something as evidence is already to be making a statement about something else, namely, that for which it is taken as evidence. And taking *E* as evidence for *O* is to see *E* differently from the way we would if we had no notion at all about *O.* Thus, just to see something as *evidence* is already to be 'looking' through the fabric and beyond. (p. 89)

Now if this argument is extended to comprehend the statements about the past it can be asserted that "to see something *E* as evidence for one of them is to be seeing *E* in a certain temporal perspective." (p. 89) From this, he concluded that

> [H]ow, if we did not know the past, could we possibly experience the present world as we do? For in fact, and our language shows this, we are always experiencing the present world in a logical and causal context which is connected with past objects and events, and hence with reference to objects and events which we cannot be experiencing at the time when we are experiencing the present. (p. 93)

C. I. Lewis and Russell's solutions of the problem, that if things are manifested to us within the present how is it possible to derive the idea of pastness from the present things and relations, by inventing "the marks or feeling of pastness" originate from an unwarranted presupposition that there is an ontologically privileged class of things that are traces, or records, or memories. If this presupposition were true it would be correct to speak of the marks of pastness, because these things would, in fact, bear such marks or provide a feeling of pastness as a result of being the remains of the past. In section 4.2., I reject this idea that there is a distinct class of things as traces, records, or memories. If all things have the same ontological status as regards "tracehood" speaking about the marks or feelings of pastness only for certain things becomes meaningless, because then everything bears the marks of pastness that makes them useless. Furthermore I also asserted that things, in general, are the source of attaining the knowledge of the future as well as the past which is in accordance with the basic laws that do not distinguish any preferred direction of time. Hence both the past and future are respectively retrodicted and predicted from the present things. It is we not the things that distinguish between the past, present, and future. Nevertheless although things do not have marks of pastness some words, such as 'trace,' 'remain,' 'record,' etc., have past connotations. In this respect, I agree with Danto that our languages are not temporally neutral in the sense of referring only to the present. Moreover I agree with Danto also on his claim that our present experiences are conditioned by our past experiences. However, I contend that the primary source of pastness is memory, and the role of language in developing the idea of past is only secondary, because temporally neutral languages are possible.

We experience the present things with the perspective of the past which is supplied by the memory. However, memory, as a physical thing, is not different from other physical things as regards pastness. The role of memory as a conveyor of pastness is contingent upon the human nature (including perhaps most of the animals, or even some plants) which is the result of a particular evolution. The biogenetic mechanism is probably responsible for distinguishing the memory-images derived from the present experiences of memory "traces" from other images derived from other than the memory traces. However, this is not a necessity, because, as we have seen in the last section, we can imagine a being that has only present experiences due to its lack of a memory, so that it does not experience things with a past perspective. Furthermore it is even possible that some persons, e.g. mentally retarded or brain damaged, can, in deed, confuse the memory images with the other images. [43]

The fact that our languages are not temporally neutral in the sense that some words have past connotation while referring to the present things enables us to experience the things in a temporal perspective.[44] This is different from the memory case in which while the experience of memory traces is immediate via the built-in mechanism the experience of other things as temporal objects is learned. Some words, such as 'trace,' 'record,'

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[43] We should also note that the idea or sense of past given immediately by memory does not logically imply that there in fact was a past, which was expressed by Russell (1921). "It is not logically necessary to the existence of a memory-belief that the event remembered should have existed at all. There is no logical impossibility in the hypothesis that the world sprang into being five minutes ago, exactly as it is then was, with a population that "remembered" a wholly unreal past." (p. 159)

[44] At this point, we should remark that retrodiction from the present things to the past is a special case of retrodiction from later to earlier by scientific laws, most of which are temporal languages in the sense that they contain time as a variable, and the things over which they range are construed as temporal objects. So as we retrodict from the position and velocity of a particle back in time by classical mechanical laws we already assume that the particle has an earlier history

'footprint,' etc., are defined genetically, i.e. by their past causes. So that they have past connotations while referring to the present objects, i.e. these words are not temporally neutral. Hence the objects referred by the above words are learned to be experienced with a perspective of the past, because a trace is always a trace of something past. The source of their temporality is also memory, for their causes, or constant conjunction with earlier things, are observed and retained in the memory which enables to define them genetically (temporally). So the sense of pastness is prior to temporal languages which are not, however, necessary consequences of the sense of pastness, for some mathematical and logical languages are temporally neutral. Hence it would be possible to have the idea of pastness and temporally neutral languages.

Furthermore some words are more neutral in the sense of requiring a special learning procedure. For example, the word 'rock' does not have a past connotation to the non-geologists, because 'rock' is generally defined by its qualities only, e.g. a thing with extension, impenetrability, color, etc. Thus rock that is referred by this word and as defined above is not conceived as a temporal object, but rather as a present object by the non-specialists. Someone who has the idea of past would be able to distinguish the rock in the past and the rock in the present by his memory, but the word itself would not give any sense of pastness. However, what 'rock' and rock are to a geologist is the same as what 'footprint' and footprint are to most people, for geologically 'rock' is defined genetically as having been formed by a past cause, etc. Hence the geologist learns to see this object as having a history as a footprint is understood by almost every one as having been imprinted in the past by some person. However, for a child, for example, a footprint is probably not a temporal object until he masters his language, thus learns how to see things around. Although it is possible to define 'footprint' in a temporally neutral terms, e.g. by its shape, extension, etc. some other words, such as 'trace,' 'record,' and so on, are strictly temporal, because these words become meaningful only in a temporal perspective. Atemporally they make no sense. Probably the source of the idea that there is a special class of things that are traces and records is in the language. Some words, though, in principle, definable atemporally, are generally defined temporally. These words correspond to the objects that are thought to be traces or records.

So far we have seen that the idea of pastness has a distinctive subjective nature which is derived from the memory; however, it is also inter-subjective which is evident from the common language that enables a common sense of temporality. These two conclusions, nevertheless, indicate that the idea of pastness is contingent upon the human nature and language.

The substance theory of things also contributes to the idea of present and past. According to the theory, things are three dimensional entities that "move" in time. If the big bang is that which started the time and matter this beginning is also the "first" present, and the universe as a three dimensional entity has been "moving," i.e. changing, since then by passing through the successive presents. The times of our experiences coincide happily with these presents. Before us or any creatures to have experiences there were still presents. Hence the past is the earlier times of the universe, and the present universe bears the marks of its history, i.e. the past changes. Although it is a present thing it is also a past thing, because it retains its identity. This provides an intrinsic pastness and presentness to things, so that things are both present and past in this sense.

A final note regarding the past is that psychic past is given immediately by the collective contribution of memory and the built-in structure. Historical past, on the other hand, is constructed in relation to the historical present as being beyond it. Furthermore since the historical present is a construction, and the past is constructed via the present, what is the difference between the two? According to Rotenstreich (1958), "there is no essential distinction between the present and the past. The difference between them depends on the status assigned to these dimensions from the point of view of historical knowledge itself, i.e. of what is to be explained and what explains." (pp. 102-103) That is, the present is explained by the past. In this sense, the past exists only ideally as explaining the present. Rotenstreich called this past *the past of the present*. What is meant by the past explaining the present is that the past causes of the present situation are discovered. But he found this insufficient because this interpretation leads to the conclusion that the past has no real existence. Thus he stated that the past was once a present. "Therefore if the past was once a present, i.e. an occurrence bound up with an actual sensation, there is no power capable of abolishing its reality." (p. 113) He called this past *the present of the past*. To sum up, "The past qua explanation of the present exists ideally, but the past as having a present of its own appears as a self contained domain, at a distance from the present but really existing nevertheless." (p. 112)

4.5.3. Retrodiction from the Present to the Past

I said at the beginning of this section that temporal position of retrodiction becomes significant if the nature is not uniform. Furthermore our temporal position, namely our present, in which we are presented things, relations, and the regularities about the things and relations, becomes significant. We can also ask as to whether the present is *necessary* for retrodiction. I earlier treated retrodiction as an inference from later to earlier states which disregards the present. Nonetheless, although this treatment is logically valid we should be aware of the fact that, even in the case of inferences for which the initial conditions may be earlier or later than the present, these initial conditions are assigned from the present. For instance, even if these initial conditions might have been obtained by an earlier study which may be beyond the present we interpret these initial conditions with the perspective of our own which is necessarily at the present. In the cases at which the laws and the boundary conditions are unrestrictedly uniform, this present perspective concerns only the initial conditions. So although the present appears necessary it is insignificant for such systems, because the same history can be inferred from the initial conditions that have different temporal positions. But, in addition to different initial conditions, if the laws are restricted that cannot be applied uniformly to all periods the present becomes significant, because it determines how the history is to be narrated, and how far we can infer back in time. That is to say that if everything that is necessary for retrodiction is provided at the present retrodiction, with the exception of certain systems for which the temporal positions are not significant, is dominantly presentist. Hence retrodiction cannot be treated as an inference from later to earlier states in most of these cases but the present must also be taken into account.

Moreover I also maintain that presentism is the primary way of knowing the past even if the past could be directly observed, for instance, by time-traveling. For even if we could traveled back in time and observed a certain period directly (that is, the things, relations, and regularities) our present perspective would still be inherent in this observation due to our belonging to *this*, not that, present that determines our understanding to a certain extent. Furthermore I even contend that the past *ought to* be known from a present perspective. We should understand the past as we are, not as someone having lived in the period that is desired by us to know. Even in emphatic understanding, as a mode of scientific understanding, one does not become the object that he wants to understand. He must keep a distance between him and his object.

However, if knowing the past is and ought to be presentist there arise problems, since I assumed that nature is not uniform. If different types of things are continuously generated and destroyed they may be available at one period of time but not at another. For instance, if there had been revolutions in the past, so that *every thing* before and after the revolutions were radically different, the types of things, relations, regularities, and so on, that we observe in our present would not have been in the past. In such a radical case, there would be no way of knowing the past. There is at least one radical case, namely the cosmological big bang, that is the limit of all retrodiciton, because it indicates to a total creation (or destruction). As I mentioned in section 4.4.2., Cuvier claimed that the present day geological causes are useful only after the last catastrophe, because before it every thing was radically different. If there had been, in fact, such radical turning points throughout time retrodiction would, indeed, be limited to these periods. Fortunately, revolutions that are mentioned in geology, biology or human history are not so radical.

Nevertheless it has been claimed against the possibility of historical knowledge that there arise some paradoxical cases even at less radical situations. The argument goes that if the only evidence about a past era at the present is an unusual type of thing that must have been formed by a presently extinct or unknown generative process (e.g. some unfamiliar rock strata)[45] it is not possible to know either about the era or about the things, for in order to know about the era we have to know about the things that can be known only through the extinct process which is confined to this era. I admit that as stated thus it is a genuine paradox, for, in principle, there can

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[45] In fact, there are such rock strata that are extremely folded. Since these rocks are relatively recent no known deformation processes seemed to have been responsible for such deformed rocks. However, later when observations in the formation of the deep sea sediments revealed that these rocks having formed at the slopes of continental margins "flowed" in the sea like plastic material to become folded. But before this observation it was a complete mystery to the geologists.

be such cases; however, the most of the cases constitute solvable dilemmas, rather than genuine paradoxes, because, in addition to the things cited as to be the only evidence, there can be other evidence either about the era or about the things, or even about both. For instance, the catastrophe (if there had been a catastrophe at all) at the Cretaceous-Tertiary boundary about sixty million years ago killed only seventy percent of the living beings. Some of them had the chance to survive that can provide information about this period. Those that went extinct were not vaporized but some parts of them, e.g. bones, have been preserved. However, it may be claimed that even though, for instance, the bones of the dinosaurs have been protected the information obtainable from these "remains" is about some of their physical structure, but, for example, no remains of their sounds have survived. So how is it possible to "reconstruct" their sounds at the present?[46] This is not so bizarre as it seems, because the dinosaurs have some relatives in the animal kingdom that provide such information. This so-called catastrophe did not change other physical processes that can be utilized to attain information about dinosaurs and the environment at that time.

As for human history, we can think of how radical the French revolution was. Did it end all types of institutions, social organizations, traditions, behaviors, psychologies of the individuals, etc. that are different from the present. Some of the things, relations, and regularities might have survived to the present. At this point I should warn that in more complex cases, such as human conduct, the regularities may, in fact, be less uniform both spatially and temporally, so that societies that are spatially and temporally distanced may not display similar regularities, and that all the

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[46] In fact, this problem was discussed publicly when the motion picture "The Jurassic Park" was played. The geologist argued that the dinosaurs could not possibly have the sound that was represented in the movie.

generalizations that are based on the present regularities may not be applicable to every society. For instance, the Athenian democracy cannot probably be understood simply by the present-day western democracy; however, some estimation about the past can still be made on the basis of the present things and relations. In addition, in more complex domains of nature including the human society, the details that diverts things from each other become more important. So it may be claimed that although there may be regularities in the geological or historical domain they are not sufficient for the task of the geologist or the historian, because he wants more than generalizations can offer, i.e. the details and deviations from the typical that may not be covered by generalizations. So the general knowledge is not satisfactory for either the historian or the geologists. Of course, this should not be understood that history is strictly the knowledge of the particular. For if every thing were described in its full particularity, history would require an infinite vocabulary for describing every single thing, because no two things would have any common characteristics with each other to allow any generalizations. This view does not conform to the actual practice of historians. As Joynt and Rescher (1961) noted "[l]ike the scientist, the historian resolves the dilemma of uniqueness by the use of a variety of classes in his discussions: "nations", "wars", "revolutions", "assassinations", "budgets", and the like" (p.151). Even a simple chronology of events requires types for ordering the events.

The final issue regarding the problems of retrodiction from the present to the past is that as the time distance between the time of retrodiction and the period to be retrodicted increases retrodiction becomes more difficult. This has, at least, two consequences: first, the chances of the things that existed in the further past to survive up to the present are lower, because the things become more vulnerable to dissipation of energy, alteration, destruction, and so on; second, the regularities may not be continuous throughout such long periods. As a result, the present evidence is fragmentary, thus retrodiction can proceed step-wise from one piece of available evidence to other. The blanks are filled by retrodicting from the results of the earlier retrodictions. For instance, we retrodict from the present evidence that there was a meteorite impact sixty million years ago. Even if we had no direct evidence about later events after the impact, such as fossils indicating an extinction, because of erosion that might have erased all the evidence about the extinction we would observe only different fossil flora and fauna of a new era that are separated from the impact by millions of years, and be able to make estimations about the effects of the impact to fill the gap between the impact and the new era. In fact, it is how geologists and human historians actually work: they endeavor to unfold the past from fragmentary evidence going back and forth step by step via evidence and the results of the earlier retrodictions. Complete and continuous retrodiction is only an ideal and cannot be obtained even in astronomy which is a more or less classical mechanical system, for, as I said earlier, some unexpected bodies might have bothered the solar system. Hence such interventions should be accounted for, in order to eliminate the interruption in the retrodiction about the periods before these interferences. Some of these interventions can, in fact, be detected from things that might have been generated as a result of these events.

4.6. Logical Structure of Retrodiction

In this section, I analyze the logical structure of retrodiction that I have so far examined. Since I include a detailed review of the logical models of explanation and prediction in Appendix A, I do not discuss the models that I mention in this section. In section 4.1., I noted that Hempel (1962) stated that "one important variety of it [retrodiction] is the deductive-nomological one." (p. 116) In addition to deductive-nomological model, there are, at least, two varieties of retrodiction, namely deductive-statistical and inductive-statistical (probabilistic), into which most of the other retrodiction types fall.

Deductive-nomological model suits well in accounting for retrodiction by deterministic laws, for the explanandum statement, E, describing the earlier part of the sequence, is inferred deductively from the explanans including, at least, one deterministic law, L, and the statements of the initial conditions, C_i, that describe the later part of the sequence. Retrodiction for conservative classical mechanical systems, that are reversible, by the classical mechanical laws of motion, which are t-invariant, is one example of deductive-nomological retrodiction, since the earlier positions and velocities, E, of a body are inferred deductively from a later position and velocity, C, of the body together with the laws of motion, Li.

Nonetheless retrodiction by t-non-invariant laws for irreversible systems can also be of deductive-nomological type so long as the laws are deterministic. For instance, although I showed that retrodiction by classical thermodynamic laws for classical thermodynamic systems is extremely limited and gives unphysical results it satisfies deductive-nomological model, because the earlier states of the system are inferred from the later states together with the classical thermodynamical laws, e.g. the diffusion law. Furthermore although the artificial system (Case I) as constructed in section 4.4.3. is an irreversible system (because there is no fission) and the laws are t-non-invariant retrodiction is deductive nomological. In the example, one of the laws is ‘B’s are generated by the fusion of two A’s’ (L) that allows retrodiction (on the assumption of the boundary condition that there are determined number of A-type fusions at every interval). If there are B’s (C_i) we can retrodict that a determined number of them were generated by the fusion of two A’s (E) at the last interval. This can be illustrated thus:

Two B's at t_2 (C)
All B's are generated by the fusion of two A's (L) $(t_1 \langle t_2)$

There were four A's at t_1 (E)

It is perhaps the right moment to examine the connection between retrodiction and causality. That is, are there causal retrodictions similar to causal explanations? In other words, are retrodictions by causal laws causal retrodictions? It may be claimed that since the inference proceeds from the effect to the cause retrodiction in such cases is teleological. This is not correct, because causality and teleology are determined by the nature of processes or laws, not by the direction of inference. If a sequence is said to be causal it does not turn into a teleological sequence by a reversed inference. The sequence is still from the cause to the effect but the inference is from the effect to the cause. So if there are ever "teleological" retrodictions this would be as a result of teleological sequences and laws. Hence it is correct to speak of causal retrodiction, in order to distinguish it from statistical retrodiction.

In Appendix A, I said that statistical explanation is divided into deductive-statistical and inductive-statistical (probabilistic) models. As for the former, although there is, at least, one statistical law in the explanans, which contains also the statements of the initial conditions, the inference of the explanandum from the explanans is *deductive*. In section 4.4.2., I investigated the limits of retrodiction for classical statistical mechanical systems and concluded that retrodiction by statistical laws is possible only for very special cases in which the prior probabilities are assumed to be equal. Hence these special cases fall in the deductive-statistical model.

Inductive-statistical (probabilistic) model, on the other hand, is an inference of the explanandum, which is a singular statement describing the phenomenon to be explained, from the explanans that contain, at least, a probabilistic law and singular statements describing the initial conditions. Inductive-probabilistic explanation is similar to deductive-nomological and deductive-statistical model, except that the inference is inductive rather than deductive. Accordingly, since retrodictive inference has inverse direction relative to explanatory (and predictive) inference, the inference of the explanandum of retrodiction, describing some *earlier* state of a system, is from the explanans, with probabilistic laws and the statements of the initial conditions, describing the later state of the system.

Inductive-probabilistic retrodiction constitutes a very large class including most of the geological, biological, and human historical systems. Retrodiction in these domains can be described in a general way in the following form: there are statements describing physical things (the so-called traces or records) found at the present and generalizations ranging over these entities which are used to make *estimations/assertions* about the past that the things are supposed to be about. Below I describe inductive retrodiction on a geological example, which was described by Gilbert (1896).

In his essay, Gilbert discussed the case of The Limestone Crater of Coon Butte, Arizona. The description of the crater is given as follows.

> In northeastern Arizona there is an arid plain beneath whose scanty soil are level beds of limestone. At one point the plain is interrupted by a bowl-shaped or saucer-shaped hollow, a few thousand feet broad and a few hundred feet deep; and about this hollow is and approximately circular rim, rising 100 or 200 feet above the surface of the plain. In other words, there is a crater; but the crater differs from the ordinary volcanic structure of that name [i.e. volcanic crater] in that it contains no volcanic rock. The circling sides of the bowl show limestone and sandstone, and the rim is wholly composed of these materials. On the slopes of this crater and on the plain around about many pieces of iron have been found, not iron ore, but the metal itself, and this substance is foreign to the limestone of the plain and to all other formations of the region. (pp. 5-6)

Furthermore Gilbert stated three facts that need explanation: “First, the crater composed of non-volcanic rock; second, the scattered iron masses; third, the association of crater and iron. (p. 6)

In the essay, he cited four hypotheses invented to explain the above facts and the formation of the crater. The first is that of the shepherds: “The crater was produced by an explosion, the material of the rim being thrown out from the central cavity, and the iron was thrown out from the same cavity at the same time.” (p. 7) Later, a mineralogist, Dr. A. E. Foote, who investigated the origin of the iron and he found out that the iron has a celestial origin, i.e. meteoric, which is accepted as the final account. The second was a tentative hypothesis by W. D. Johnson, who asserted that “the rim might be the remnant of the dome of strata over a laccolite.” (p. 9) Third, Johnson constructed another hypothesis that explains the crater by an explosion from within that was caused by steam produced by volcanic heat. “The fall of iron was independent, and the association of the two occurrences in the same locality is accidental.” (pp. 9-10) The fourth is the impact theory that interprets the crater as having been generated by the impact of an iron meteorite. Of course, there are other possible hypotheses, for instance, accounting for the crater by a volcanic explosion (because there are, in fact, volcanic craters) or by a collapse into a cavity within the rocks.

According to Gilbert, calling hypotheses “scientific guesses” is appropriate, because they are invented to discover the cause or origin of “a fact or group of facts.” It seems that since he, as many others, did not distinguish between explanation and retrodiction he thought that the above hypotheses, as hypotheses in general, were asserted to explain a past phenomenon.

Furthermore Gilbert stated that "hypotheses are always suggested through analogy." (p. 5) For instance, when shepherds asserted that the crater was formed by an explosion Gilbert believed that they had done so on the basis of their earlier experience with blastings in mining. That is, they constructed their hypothesis in analogy to the totality of knowledge that they had. Similarly, the other hypotheses were based on analogical reasoning but, at this time, the object of the analogy was scientific knowledge, such as knowledge about laccolites, meteorite craters, hydrothermal steam explosions, and so on. I assert that analogical reasoning is another name for inductive inference, for assertions based on analogical reasoning are inductive, unless they are soothsaying based on coffee remains or other non-empirical experience.

Hence the above six hypotheses are retrodictive, rather than explanatory, inferences, since they are directed towards the past. The above are asserted on the basis of the present evidence, i.e. the crater and its characteristics, and some "implicit generalizations" or analogies in order to discover the origin of the crater. Even the hypothesis of the shepherds is an inductive inference that has the following form: first, description of the hole that constitutes the statements of the initial conditions of the retrodiction; second, the generalization about the holes of the described type which is stated in terms of the words, 'most,' 'frequently,' 'very often,' and so on; then comes the inductive inference: if such and such type of hollows are produced by explosions, and the hollow here is similar to this type of hollows then it was probably produced by an explosion. I should also note that a hypothesis may be result of a series of inductive inferences, not necessarily a single one. It is also similar for the other hypotheses. For instance, the claim that the crater was produced by a meteorite impact is based on the similarities with the impact craters, or that it was of laccolitic origin was suggested by the outward dip of the rocks in all directions. These analogies serve as generalizations of the inductive inference.

The origin of the wide variety of hypotheses in the above case has two sources. The description of the thing is incomplete due to its complexity, and, accordingly, the generalizations about such phenomena are not very precise and restricted in scope. In the case of such complex things distinguishing between impact and explosion craters may be extremely difficult, or impossible, or the probabilities of the formation of the crater by an explosion or a meteorite impact may be quite close, so that preferring the one instead of the other may not be rationally justified. But in any case, the hypothesis is constructed on the basis of inductive reasoning however crude it may be. By series of things that are fragmentary, the past is constructed step-by-step as I stated in the last section.

CHAPTER 5

CONCLUSIONS

The methods that I employed in the present study involve scientific as well as philosophical and logical analyses. The scientific analysis consists of the investigation of the nature of physical processes in terms of the concepts of reversibility and irreversibility, the direction of time with respect to logical structure, physical processes and scientific theories, and the possibility of retrodiction for different physical systems including both artificial and real systems. The philosophical analysis, on the other hand, includes the concepts of trace and uniformity of nature that are intimitaley connected with retrodiction. Finally, the logical analysis comprises the examination of the direction and the logical structure of retrodictive inference in comparison with those of explanation and prediction.

I analyzed retrodiction in terms of four (groups of) concepts: reversibility and irreversibility connected with the physical processes, t-invariance and t-non-invariance related with scientific theories, physical things constructed independently of the concept of trace, and the uniformity of nature.

I endeavored to show that retrodiction is related strictly with the distinction between reversibility and irreversibility of physical processes. In addition, a third type of processes appears at the intersection of these types, viz., theoretically reversible and physically irreversible. Furthermore reversible and irreversible processes are described in terms of t-invariant and t-non-invariant theories respectively. I showed on certain artificial as well as real systems that the earlier states of reversible processes can be inferred from their later states, whereas the inference is extremely limited in the case of irreversible processes. The reason for this limitation is that although t-invariant equations describing reversible processes can be solved symmetrically by replacing positive time values with the negative ones the solution of t-non-invariant equations gives, if ever, asymmetrical solutions due to their sensitivity to the change of the sign of time. Nevertheless I should note that there are, at least, two exceptions to this rule: the simple deterministic irreversible processes, that are described by t-non-invariant equations, and theoretically reversible but physically irreversible processes. As for the former, I provided several artificial systems that can be solved to retrodict the earlier states of the systems. Some real systems, e.g. those involving the decay of radio-isotopes, that are analogous to my artificial systems also allow retrodiction. I ought to emphasize that the restriction imposed upon retrodiction does not arise as a result of mathematical impossibility but rather is contingent upon the boundary conditions, for the equations are, in principle, solvable. As for the latter, although the processes are irreversible, since they can be accounted for by t-invariant equations the change of the sign of time do not alter the solutions of these equations. For instance, even though expanding waves are physically irreversible mechanical processes the equations describing these processes are t-invariant which means that these processes are theoretically reversible. So if + t is replaced by - t in the wave equation it is symmetrically solvable, because time is in the second derivative in the equation. Furthermore it was also observed that non-determinism involved in the equations and the imprecision arising as a result of the equations and the human incapacity (the problem of measurement) can make retrodiction quite unreliable.

In order to establish a clear and distinct idea of physical things that are the objects of retrodiction, I examined the possible accounts of traces, for it is a common belief that traces (or records) have a distinct function in retrodiction that is different from the function of other physical things. As a result, I concluded that there is not a previliged class of things as traces that are distinguishable from other physical things, because, on final analysis, every physical thing can be said to play the role of a trace. Thus I asserted that the difference between the things is determined by the theory that may construe certain things as traces. However, it does not follow that they are ultimately different from other physical things. In this sense, a newtonian particle is the same as (or has the same function as) a fossil bone as regards retrodiction. This leads to the fact that also retrodiction distinguished on the basis of traces and other physical things is not a defendable position. So I concluded that all physical things are on equal footing as regards retrodiction.

The analysis conducted on the possibility of retrodiction suggests that there is a close connection between retrodiction and uniformity of nature that I examined in terms of regularities (laws) and particular conditions (boundary conditions). we have seen in virtue of the examination of the idea of the uniformity of nature that strict uniformity conditions cannot be realized in general. Thus, under the non-uniform conditions, retrodiction from any temporal position does not result in the same narrative, for the same laws, boundary conditions and other physical evidence may not be available at every temporal position that restricts retrodiction. So retrodiction as an inference from later to earlier states of the systems is not a sufficient account of retrodiction, since it presupposes strict uniformity. Furthermore the fact that the time of retrodiction is significant leads to the fact that the present, as a particular temporal position, is also significant for retrodiction for certain systems.

The logical analysis consisted of a preliminary examination of the logic of retrodiction in comparison with the logic of explanation and prediction. I noted that retrodiction is distinguished from explanation and prediction by its direction. I solved the overlap that results from Hempel's earlier and later accounts of explanation, prediction, and retrodiction by redefining explanation and prediction to make room for retrodiction. This consists of distinguishing prediction-in-the-past from explanation and distinguishing retrodiction from present to the past from explanation that proceeds from the past to the present. As for the logical structure of retrodictive inference, I concluded that the logical structure of retrodiction can be described by deductive nomological, deductive-statistical, or inductive-statistical model.

APPENDICES

A. LOGICAL MODELS OF EXPLANATION AND PREDICTION

In this appendix, I provide a detailed review of the logical models of explanation and prediction, for I discuss the logic of retrodiction in comparison with and contrast to explanation and prediction, since these three types of inference are intimately related to each other.

A.1. Deductive-Nomological Explanation

According to this model, explanation is divided "into two major constituents, the *explanandum* and the *explanans* "(Hempel and Oppenheim, 1948, p. 10). The explanandum is "the sentence describing the phenomenon to be explained" (Ibid.). The explanans, on the other hand, is "the class of those sentences which are adduced to account for the phenomenon" (Ibid.). The explanans are further differentiated into sentences describing antecedent conditions ($C_1, C_2, ..., C_k$) and general laws ($L_1, L_2, ..., L_r$). A schematized representation of an explanation is supplied in the below format:

$C_1, C_2, ..., C_k$ Statements of antecedent conditions

$L_1, L_2, ..., L_r$ General laws

Logical deduction ____________

E Description of the empirical
phenomenon to be explained

It has been claimed also that one of the substantial features of deductive-nomological (and statistical) explanations is the symmetry between explanations and predictions. The only difference is of a pragmatic character not of logical one.

> If *E* is given, i.e., if we know that the phenomenon described by *E* has occurred, and a suitable set of statements C_1, C_2, ... , C_k, L_1, L_2, ..., L_r is provided afterwards, we speak of an explanation of the phenomenon in question. If the latter statements are given and *E* is derived prior to the occurrence of the phenomenon it describes, we speak of a prediction (Ibid., p. 11).

That is, while in explanation, the phenomenon given by the explanandum statement is already known and the explanans, containing general laws and initial conditions, are used to derive the event described by the conclusion statement, in prediction, the statement describing the possible occurrence of the event is derived from the known initial conditions and the laws. It is further maintained that this symmetry holds for probabilistic explanations as well as D-N explanations.

The predictive and explanatory power is due to the existence of general laws that make predictions as well as explanations possible. For if there were no general laws the predictions would be impossible.

> [For example], it may be explained that a car turned over on the road 'because' one of its tires blew out while the car was traveling at high speed. Clearly, on the basis of this information, the accident could not have been predicted, for the explanans provides no explicit general laws by means of which the prediction might be effected (Ibid., p. 12).

According to Hempel (1965), the thesis of structural identity between explanation and prediction can be divided into two sub-theses, "namely (i) that *every adequate explanation is potentially a prediction* ... (ii) that conversely *every adequate prediction is potentially an explanation* " (p.367).

Hempel believed that the first sub-thesis is sound whereas the second is open to discussion. For the first thesis is trivially true since the conclusion is acquired via logical deduction from the premises and, what is more, a more general principle is also in support:

> Any rationally acceptable answer to the question 'Why did event *X* occur?' must offer information which shows that *X* was to be expected—if not definitely, as in the case of D-N explanation, then at least with reasonable probability. Thus, the explanatory information must provide good grounds for believing that *X* did in fact occur; otherwise, that information would give us no adequate reason for saying: "That explains—that does show why *X* occurred." And an explanatory account that satisfies this condition constitutes, of course, a potential prediction in the sense that it could have served to predict the occurrence of *X* (deductively or with more or less high probability) if the information contained in the explanans had been available at a suitable earlier time (Ibid., pp. 367-368).

However, it is different for the second sub-thesis. "For example, certain empirical findings may give excellent grounds for the belief that the orientation of the earth's magnetic field shows diurnal and secular variations, without in the explaining why" (Ibid.).

The Hempel and Oppenheim model of explanation was designed to satisfy four conditions of adequacy; the first three are logical conditions and the last is an empirical condition:

> (R1) The explanandum must be a logical consequence of the explanans....
>
> (R2) The explanans must contain general laws, and these must actually be required for the derivation of the explanandum....
>
> (R3) The explanans must have empirical content....
>
> (R4) The sentences constituting the explanans must be true. (Hempel and Oppenheim, 1948, pp. 10-11)

The reason for (R4) is discussed at length. Suppose that the explanans were not required to be true but only highly confirmed on available evidence at that time. However, it is possible that, by further evidence, the explanans were highly disconfirmed. In such a case, it would be concluded that the explanandum was correctly explained at the beginning with the available evidence but not explained later. They find this contrary to common usage since the correctness of an explanation must be time independent.

However, Hempel (1962) in his later article relaxed the D-N model because of the vagueness of the notion of correctness. He argued that correctness may be construed in two different ways: "as truth in the semantical sense, which is independent of any reference to time or evidence; or as confirmation by the available relevant evidence—a concept which is clearly time dependent" (Ibid., p. 102). Therefore, he distinguished three types of explanation (pp. 102-103):

1. Potential explanations whose explanans and explanandum may be false.

2. Potential explanations with highly confirmed explanans.

3. Potential explanations with true explanans.

Even though Popper also contributed to D-N Explanation he called it causal explanation. According to Popper (1972), "To give a *causal* explanation of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws,* together with certain singular statements, the *initial conditions*" (p. 59)

As it is clear, his formulation is almost identical with that of Hempel and Oppenheim (1948). The general laws in explanans of Hempel and Oppenheim become *universal statements,* "i.e. hypotheses of the character of natural laws" (Popper, 1972, p. 60) and antecedent conditions in explanans are called the ***initial conditions***. The explanandum is prediction in Popper's formulation. As in Hempel and Oppenhheim, the symmetry between explanation and prediction is welcomed also by Popper.

Although Popper's causal explanation is parallel to D-N model, according to Hempel (1963), causal explanation is a variety of D-N Explanation, because it presupposes that the event described by the particular conditions (antecedent conditions or initial conditions) is prior to, or at most simultaneous with the event to be explained. "But some specific explanations also invoke, in the explanans, certain occurrences that are later than the explanandum-event" (p. 109). He also presented an example concerning such a case that does not fit into the causal presuppositions of priority or simultaneity of the event described in the antecedent conditions (Ibid.).

However, for Hempel and Oppenheim (1948) as for Popper (1972), deductive-nomological explanations are chiefly in causal character. They alleged that;

> If E describes a particular event, then the antecedent circumstances described in the sentences C_1, C_2, ..., C_k may be said jointly "cause" that event, in the sense that there are certain empirical regularities, expressed by the laws L_1, L_2, ..., L_r, which imply that whenever conditions of the kind indicated by C_1, C_2, ..., C_k occur, an event of the kind described in E will take place. (p.12)

The laws given in D-N model are called causal or deterministic, contrary to the statistical laws in probabilistic explanation "which assert that in the long run, an explicitly stated percentage of all cases satisfying a given set of conditions are accompanied by an event of a certain specified kind" (Ibid., p. 13). However, this notion equalizes 'causal' with 'deterministic.' As Hempel (1965) later admitted, statistical explanations have a probabilistic concept of cause, "in contradistinction to a strictly deterministic one" (p. 393). It might even be argued that, considering quantum physics and other contemporary statistical theories of science, there are no deterministic causes at all but only probabilistic ones.

The simplest formulation of a causal connection is frequently presented in the following form: between two events event *a* is the cause of event *b*. However, although every single event is unique in its individuality what is actually meant by such a formulation is that event *a* is an instant of A type events and event *b* is an instant of B type events. Moreover, as put by Scriven (1958), further conditions must also be specified. "To say *X* causes *Y* is to say that under proper conditions, an *X* will be followed by a *Y* " (p. 185). Hempel (1965) conceived this statement and particular events being instants of types to be the indication of causal explanation as conforming to the D-N model. For, first, the relation holding between A type events and B type events is expressible only by general laws from which particular cases can be deduced. Second, determination of "proper conditions" mentioned by Scriven (1958) leads to the specification of general laws. If these conditions are otherwise indeterminate the causal connection attempted to be specified becomes a vague claim.

> Thus the causal explanation implicitly claims that there are general laws—let us say, L_1, L_2, ..., L_r —in virtue of which the occurrence of the causal antecedents mentioned in C_1, C_2, ..., C_k is a sufficient condition for the occurrence of the explanandum event. ... Causal explanation is, at least implicitly, deductive-nomological (Hempel, 1965, p. 349).

According to Hempel, the best examples are produced by deterministic theories, such as classical particle mechanics. In such theories, the selected system is an isolated one and its state at a particular time is specified for example by the position and momento of the particles. From this deterministically described state, the behavior of the system at different times is also specifiable with accuracy. Described in this way, "the notion of a cause as a more or less narrowly circumscribed antecedent event has been replaced by that of some antecedent state of the total system, which provides the 'initial conditions' for the computation, by means of the theory, of the later state that is to be explained" (Hempel, 1962, p. 107). However, if the system is not isolated and some external forces are in operation then these influences are also expected to be included in the explanans as boundary conditions.

Hempel related non-causal deductive-nomological explanation with laws of coexistence (or laws of state, instead). Causal laws are laws of succession (or laws of change) in the sense that they account for the changes that the systems undergo. Laws of coexistence, on the other hand, do not concern temporal changes but rather elucidate a relation such as the period of the swing of a pendulum which is explained "by reference to its length and the law that the period of a mathematical pendulum is proportional to the square root of its length" (p. 108). In this explanation, no antecedent causal conditions are mentioned. The fact that is referred to is contemporary to the event to be explained.

The other examples to non-causal laws suggested by Hempel are Boyle's, Charle's, and Van der Waal's laws for gases, Ohm's law, and the law of Wiedemann and Franz, "according to which, in metals, electric conductivity is proportional to thermal conductivity" (p. 108).

A.2. Statistical Explanation

Statistical explanation may be divided into two types: deductive statistical and inductive-statistical (probabilistic) models of explanation. As for the deductive model, Hempel (1962) described it as the explanation of general regularities, which is similar to deductive-nomological explanation explaining both particular facts or events as well as general regularities expressible as laws.

> [Explanations of this kind] involve the deduction of a statement in the form of a statistical law from an explanans that contain indispensably at least one law or theoretical principle of statistical form. The deduction is effected by means of the mathematical theory of statistical probability, which makes it possible to calculate certain derivative probabilities (those referred to in the explanandum) on the basis of other possibilities (specified in the explanans) which have been empirically ascertained or hypothetically assumed. (Hempel, 1965, p. 381)

Therefore, although the explanans and the explanandum may be statistical in form the conclusion is obtained deductively from the premises. Hempel (1965) furnished two examples regarding this type: the experiment of flipping a fair coin, in which the probability of coming heads or tails is explained deductively from a general statistical hypothesis and that, which is more interesting for empirical sciences, is found in the disintegration of radioactive substances in which the probability of disintegration of a radioactive substance in a unit of time is explained deductively from some complex statistical hypotheses regarding radioactive decay.

Inductive-statistical model, on the other hand, has been labeled by various authors as probabilistic, inductive-statistical, and, simply, statistical explanation. According to Nagel (1961), "Probabilistic explanations are usually encountered when the explanatory premises contain a statistical assumption about some class of elements, while the explicandum is a singular statement about a given individual member of that class" (p. 22).

One important point about statistical probability is its distinguishing feature from inductive or logical probability. Although they have common mathematical characteristics "Logical probability is a quantitative logical relation between definite *statements;* ... [while] statistical probability is a quantitative relation between repeatable *kinds of events*" (Ibid., p. 32).

Inductive-statistical explanation, therefore, may be schematized similar to deductive-nomological explanation:

$C_1, C_2, \ldots, C_k$ Statements of antecedent condition

$L_1, L_2, \ldots, L_r$ Statistical Laws

=========== [makes highly probable]

E Description of the empirical phenomenon to be explained

(The double line here is proposed by Hempel (1966) for distinguishing inductive-statistical explanation from deductive-nomological explanation.)

As would be seen from the above-scheme, both types of explanation share a common form. It has been even claimed that statistical explanations are incomplete versions of deductive-nomological explanations such that replacing the statistical generalization in the premises by a strictly general statement would make it deductive nomological. However, Hempel (1962, 1965 and 1966) argued against this notion that statistical explanations have a logical character different from deductive model.

> A probabilistic explanation of a particular event shares certain basic characteristics with the corresponding deductive-nomological type of explanation. In both cases, the given event is explained by reference to others, with which the explanandum event is connected by laws. But in one case, the laws are of universal form; in the other, of probabilistic form. And while a deductive explanation shows that, on the information contained in the explanans, the explanandum was to be expected with "deductive certainty," an inductive explanation shows only that, on the information contained in the explanans, the explanandum was to be expected with high probability, and perhaps with "practical certainty." (Hempel 1966, p. 29)

Another version of statistical explanation is causal-statistical explanation, theory of which has been developed by Hans Reichenbach, Wesley Salmon, Michael Scriven, and Richard Jeffrey: "explaining an event scientifically is neither a case of deducing nor inducing (providing a high probability argument for) that event from anything, let alone from a combination of facts and laws" (Lambert and Britton, Jr., 1970, p. 20). The causal-statistical theorist rejects the classical theory, which is simply formulated as giving rationally acceptable answer to 'Why' questions, on the basis of the fact that not all acceptable scientific explanations are answers to 'why so and so was to be expected.'

> There are rationally acceptable answers to why-question about unexpected events. For example, recall again the question 'Why did that atom decay in a certain one minute interval?' The answer 'Because it was an atom of U 238, etc.' though scientifically (and thus rationally) acceptable does not provide information showing why this quite unexpected event was to be expected (Ibid., p. 21).

Although, in many cases, a scientific explanation is of deductive or inductive type this is not as such in all the cases. For the causal-statistical theorist "to explain an event scientifically is to present both the set of factors statistically *relevant* to that event and the causal network underlying the statistical regularities involving those factors and that event" (Ibid.).

Statistical relevance is described as the probability of A given B, compared to the probability of A without B given. For example, injection of ampicilin for recovering from bacterial pneumonia is statistically relevant in contradistinction to wearing a gray hat because, whereas, for the latter, the probability of recovering from the disease in the case of wearing a gray hat is the same as in the case of not wearing a gray hat, for the former, the probability of recovery by ampicilin injection is higher than the probability of recovery by no injection (Ibid., pp. 21-22). Statistical regularity, in this example, may be stated as "the probability of recovering from bacterial pneumonia given seven 1,000 milligrams injection of ampicilin is greater than 0.98" (Ibid.).

On the causal-statistical theory, to explain scientifically requires also that the causal processes and interactions behind the statistical regularities be known.

In order to avoid the Humean criticism of causation that two subsequent isolated events do not imply a causation between them, the causal-statistical theorist defines a causal process as a spatio-temporally continuous process. For example, the transmission of radiation between an atomic explosion and a person who is exposed to it. "A *causal interaction,* [on the other hand], is a relatively brief event at which two or more causal processes intersect" (Ibid., p. 23).

The advantages of the causal-statistical model are that "it accounts neatly for the explanation of unexpected events. ... because of its sophisticated treatment of statistically relevant factors, it excludes irrelevant explanations, such as the 'explanation' of a man's avoidance of pregnancy via consumption of birth-control pills" (Ibid., p. 24).

A.3. Historical Explanation

Although it has been claimed that history deserve a distinct model of explanation because these areas are qualitatively distinct, i.e. historical phenomena are unique, unrepeatable, highly complex, etc., the covering-law theorists (for example, Hempel (1962 and 1942) and Popper (1960 and 1945) claim that D-N explanation is suitable also for historical phenomena. "Some historical explanations are surely nomological in character; they aim to show that the explanandum phenomenon resulted from certain antecedent, and perhaps, concomitant, conditions; in arguing these, they rely more or less explicitly on relevant generalizations" (Hempel, 1962, p. 108). However, since the objects of history are more complex, historical explanations given by the historian do not always explicitly mention generalizations--because either that these generalizations are simple truisms about human and social psychology or that generalizations are difficult to state explicitly and precisely--and thus are only *explanation sketches* (Hempel, 1965 and 1962). But this does not mean that historical explanations differ in quality due to the fact that history is concerned mainly with particular facts because any causal explanation can be regarded as historical in so far as it is applied to some particular event (Popper, 1960). Furthermore, even though some idealist philosophers of history, e.g. Croce, Collingwood, Oakeshott, claimed that historical events and conditions are unique, it can be shown very easily that all events are unique in principle. It is also evident that physical explanation of such unique events and objects is possible, but not in the sense of complete explanation of the phenomena in all their concrete detail because, as Hempel (1965) stated, "*A fortiori*, it is impossible to give a *complete explanation* of an individual event in the sense of accounting for *all* its characteristics by means of universal hypotheses" (p. 233). This statement is true for physical as well as historical explanations. Then the question remains as to whether the historical scientist is satisfied with such "incomplete" explanations as the physicist is. Although Hempel and Popper claimed that historical explanations, in principle, involve laws, Dray's (1957) account of historical explanations is worth to consider: "showing that there is no metaphysical barrier to bringing historical events under laws is not the same as showing that the laws are in fact used, or that they are in practice available, or that they must function in the covering law way" (p. 46).

It is claimed that animate entities are characteristically different from inanimate objects because the latter, though must obey laws of nature, act/function on some purpose toward an end, can plan for the future, may have free will as in the case intelligent animals like men to act irrationally as well as rationally, and so on.

A.4. Functional or Teleological Explanation

According to Nagel (1961), the reason that biological and human phenomena are thought to require a distinctive model of explanation lies in the so-called peculiarity of these areas.

> In biology and in the study of human affairs, explanations take the form of indicating one or more functions (or even disfunctions) that a unit performs in maintaining or realizing certain traits of a system to which the unit belongs, or of stating the instrumental role an action plays in bringing about some goal (Nagel, 1961, pp. 23-24).

As Nagel observed many biologists are aware that living phenomena as well as nonliving ones obey physical and chemical laws. However, some biologists believe also that, due to its different character, understanding living processes necessitates a fundamentally different explanatory analysis (Ibid., p. 398).

The main distinctive future of teleological or functional explanations, among others, is the fact that these explanations are future oriented in the sense that rather than a previously occurring event causing the event in question as in the case of causal explanation, a future goal, event, or state determines or at least affects the present event, which was stated as early as by Aristotle as final cause. However, in Aristotelian science "since nonliving as well as living phenomena were thus analyzed in teleological terms—an analysis which made the notion of final cause focal—Greek science did not assume a fundamental cleavage between biology and other natural science" (Ibid., p. 401).

Although purposes and deliberate goals play a distinctive role in human affairs, in biology such a deliberation would not be the case unless a supernatural design or goal is assumed. Before modern biology appeared such explanation models were common among biologists. But Darwin's theory of organic evolution changed the course of the evolution of biology. For Darwin's account is, in a sense, mechanical with the important role played by random selection. According to evolutionary theory, "Evolution has neither purpose nor goal, and yet it results, often though by no means always, in adaptation, i.e., in fitting the organism to its environment" (Dobzhansky, 1963, p. 210). However, even some contemporary theories, namely orthogenesis, nomogenesis, aristogenesis, hologenesis, or in general, autogenesis, have been proposed as alternatives to evolutionary theory claiming that "evolutionary changes are performed in the organism itself. The genetic basis of the organism changes, perhaps gradually but relentlessly, in a definite direction" (Ibid., p. 212).

Although biologists employ physical methods for analyzing macro and micro structures of organisms and their parts they must also account for the functions of these organisms as a whole since vital processes have a purposive character which can only be expressed in a teleological language. For example, a biological statement may be constructed in a teleological language in the following form: "The function of chlorophyll in plants is to enable plants to perform photosynthesis" (Nagel, 1961, p. 403).

Other arguments asserted by the proponents of teleological explanation in the study of human affairs can be expressed in three groups (though this is not exhaustive):

1) Phenomena describing human activities are unique and unrepeatable, therefore, unlike the physical phenomena, they cannot be accounted for by causal explanation.

2) Human behavior cannot be covered by general principles because, besides the present situation in which the action takes place, the personal history of the individual must also be taken into account.

3) Purposive behavior can be explained only with reference to the motives involved, therefore causal explanation cannot account for such motivations.

A.5. Genetic Explanation

Genetic explanation has been offered to apply to animate as well as to inanimate phenomena dealt by the studies of human affairs, biology, and historical geology. "Historical inquiries frequently undertake to explain why it is that a given subject of study has certain characteristics, by describing how the subject has evolved out of some earlier one" (Nagel, 1961, p. 25). That is, the event to be explained is presented as the final stage of the evolutionary sequence. In doing so, certain singular statements, descriptive and explanatory ones together, are given in the order of appearance. However, not all events are described in that order except those that are supposed to be related to the phenomenon to be explained. "Those events which are mentioned are selected on the basis of assumptions (frequently tacit ones) as to what sorts of events are causally relevant to the development of the system" (Ibid., p. 25). According to Nagel (1961) and Hempel (1965), general statements, either universal or probabilistic or quasi-probabilistic, are also included in the premises.

According to Hempel (1965), the explanatory role of genetic account is nomological in nature because "each stage must be shown to 'lead to' the next, and thus to be linked to its successor by virtue of some general principle" (pp. 448-449). He thought, however, that, in this sense, many of the physical phenomena are explained genetically also by physics, e.g., free fall of a stone (Ibid.). The difference between purely nomological explanations and genetic ones is that the latter "combine a certain measure of nomological interconnecting with more or less large amounts of straight description" and "no overall law which, ... links the final stage of the process immediately to the initial one" as in the case of physical explanations (Ibid., pp. 449 and 453).

In order to explain a certain event at time t, an initial term describing the first event in the order together with other terms describing the events between the final event and the initial one are introduced. Those events in between may or may not be successive because some of them may overlap or be simultaneous with the others. Among these terms, two classes may be distinguished: "one ... describes those facts about the given stage which are explained by reference to the preceding stage; the other ... constitutes information about further facts which are adduced without explanation" (Hempel, 1965, p. 450).

As for the causal connection between events it may be said that although there may be a causal connection between each subsequent event within a series of events, for example, between a_1 and a_2 within a_1,

a_2, ..., a_{n-1}, an, cause of the event, an, is not the one just before it, a_{n-1}, but rather the whole series of events from a_1 to a_{n-1}.

B.THE SIGNIFICANCE OF BAYES' PROBABILITY THEOREM IN RETRODICTION

Since Bayes' probability theorem is an excellent example of the asymmetry between prediction and retrodiction for statistical systems, and shows the limitations of retrodiction for such systems, I provide a brief review of Watanabe's (1969) discussion thereof.

Watanabe started with a logical spectrum $\mathrm{G} = \{G_k\}$ which is a product of $\mathrm{E} = \{E_i\}$ and $\mathrm{F} = \{F_j\}$ such that $G_k = E_i \cap F_j$ and $\mathrm{G} = \mathrm{E} \otimes \mathrm{F}$. From the general formula of conditional probability which is given as

$$p(A|B) = p(A \cap B)/p(B) \qquad (1)$$

where $p(A|B)$ is conditional probability of A given B, the following mathematically symmetrical equation can be derived:

$$p(E_i)p(F_j|E_i) = p(E_i \cap F_j) = p(F_j)p(E_i|F_j) \qquad (2)$$

However, Watanabe stated that this symmetry disappears in most of the practical cases. For example, in the physical sciences in which E and F correspond respectively to the initial and the final states, E_i may be a proposition describing the physical system as being in state i at $t = 0$ and F_j as being in state j at $t = t$. "A physical law usually gives a probabilistic prediction such as 'if E_i is true, then there is such-and-such probability of F_j being true.' This means that the physical law gives only $p(F_j|E_i)$ "(p. 104). However, other four probabilities, $p(E_i)$, $p(E_i|F_j)$, $p(F_j)$, and $p(E_i \cap F_j)$, in the above equation cannot be determined from $p(F_j|E_i)$ alone, for "These depend on how the collection of samples is prepared for the experiment, or with what probability each initial state is presented" (p. 104). In addition, in the physical sciences, E and F correspond to cause and effect, respectively, and when the effect is guessed from a cause event it is called prediction, i.e., $p(F_j|E_i)$ in the above sense, and the converse is called retrodiction, i.e., $p(E_i|F_j)$.

According to Watanabe, retrodiction is impossible in general because the retrodictive probability $p(E_i|F_j)$ "cannot be derived in the general case from $p(F_j|E_i)$" (p. 104). However, there are special cases for which retrodictive probabilities can be obtained.

I reproduce below two equations given by Watanabe in his discussion of prediction and retrodiction.

$$p(E_i|F_j) = p(E_i)p(F_j|E_i)/p(F_j) \qquad (3)$$

$$p(F_j) = \sum_i p(E_i)p(F_j|E_i) \qquad (4)$$

Equation (3) is derivable from Equation (2) and Equation (4) on the basis of conditions specified above. As it is evident from (3), $p(E_i|F_j)$ cannot be determined when $p(F_j|E_i)$ is given only because $p(E_i)$ and $p(F_j)$ are also required. (This is the impossibility of retrodiction in general.) However, in a special case in which $p(E_i)$ and $p(F_j)$ are not separately known but $p(E_i)/p(F_j)$ is given the retrodictive probability $p(E_i|F_j)$ can be obtained. "This exception happens in an extraordinary case when the predictive probability $p(F_j|E_i)$ is 0 or 1 in such a way that for a given F_j there is only one E_i that makes $p(F_j|E_i)$ unity" (p.105). In this special case, $p(E_i)/p(F_j) = 1$ which, in turn, makes $p(E_i|F_j) = p(F_j|E_i)$ for all i and j. Watanabe called this special case bilaterally deterministic case, since prediction and retrodiction are symmetrical, and stated that "[e]xcept for this special case, pure retrodiction, or derivation of the retrodictive probabilities from the predictive probabilities only, is impossible" (p. 106).

It can be seen from the equations (3) and (4) that if $p(E_i)$ is known in addition to $p(F_j|E_i)$ the retrodictive probability $p(E_i|F_j)$ can be obtained. However, in general, $p(E_i)$ is not known unless there are extra-evidential factors, such as traces or records. The only possible way, according to Watanabe, to get around this problem is "to invoke the principle of ignorance and give the same value to all $p(E_i)$, although the validity of this principle is questionable" (p. 106).

Another possibility regarding the limitations of retrodiction arises when "the retrodictor knows, besides $p(F_j|E_i)$, the probability distribution of $p(F_j)$" (p. 106). This may occur when the retrodictor has the chance to observe the effects so often that he may determine $p(F_j)$ approximately. According to Watanabe, the question as to whether successful retrodictions can be made without the knowledge of $p(E_i)$ "amounts to asking if one can solve m equations

$$p(F_j) = \sum_{i=1}^{n} p(E_i)p(F_j|E_i)$$

for n unknowns $p(E_i)$. If n is larger than m, of course, it is generally impossible to determine $p(E_i)$, and therefore $p(E_i|F_j)$. Even when $n = m$, this is not possible if det $\left[p(F_j|E_i)\right] = 0$" (p. 106).

So it can be summarized that (i) when only $p(F_j|E_i)$ is given retrodiction, i.e., $p(E_i|F_j)$, is impossible except in the bilaterally deterministic case; (ii) when $p(F_j|E_i)$ and $p(F_j)$ are given retrodiction is sometimes possible and sometimes impossible; (iii) when $p(F_j|E_i)$ and

$p(E_i)$ are given retrodiction is possible to a full extent, however, the knowledge of $p(E_i)$ cannot be obtained quite generally from the nature of physical system alone (that is, from the physical laws governing that physical system and from the evidence, F_j) but only by the help of extra-nomological evidence.

C. RESTRICTED LAWS

C.1. Introduction

According to the classical model, laws are unrestricted true universal statements. Accordingly, the generalizations that make reference to particular places, times and proper names are not laws. Hence all the generalizations in geology, biology, the social sciences and history cannot be laws, since they are spatio-temporally restricted.

Below I endeavor to establish that, upon the acceptance of this theory, no generalizations, including laws of physics and chemistry, can satisfy this condition. The attempts to show that there are geologic laws fail, for they also presuppose the condition of unrestrictedness. Therefore I propose to relax the classical model by rejecting the unrestrictedness requirement. Furthermore I assert that laws are not isolated statements but rather are part of coherent wholes, namely theories, that provide indirect evidence, explanatory and predictive power, and power to support counterfactual conditionals to the restricted generalizations.

C.2. Classical Model of Laws

Hempel (1942) stated that a law of nature is “a statement of universal conditional form which is capable of being confirmed or disconfirmed by suitable empirical findings" (p. 231). Later, Hempel and Oppenheim (1948) suggested that the conditional form is not essential "since any conditional statement can be transformed into a non-conditional one” (p. 20), and also that the requirement of high confirmation is inadequate since a proposed important feature of laws cannot be their literal truth, since this rarely exists. It is not their closeness to the truth which replaces this, since far better approximations are readily constructed" (p. 100). Van Fraassen (1989) also rejected the truth condition by giving two parallel examples with exactly the same categories of terms and of logical form, although both are true, one is only accidentally true (p. 27).

As for universal form, it is, in principle, possible that any statement can be formulated in the universal form, e.g. 'Ahmet is ten years old' can be formulated as 'For every x, if x is Ahmet then x is ten years old.' Thus, it is obvious that many generalizations which are against the intuition of scientific laws would satisfy this definition. For example, the statement 'All men are mortal' satisfies the above definition, because it has a universal conditional form ($\forall x(Fx \rightarrow Gx)$) and empirical content, i.e. it can be confirmed or disconfirmed by empirical evidence.

Since every statement can be stated in universal form, Hempel and Oppenheim (1948) suggested that laws are those true universal statements that are unrestricted in scope. Hence, although the statement 'Every apple in basket *b* at time *t* is red' is universal in form, since it makes reference to a class of particular object it is restricted in scope and, thus, not lawlike (Ibid. p. 20).

C.3. Generalizations in geology, biology, and history With Respect to the Classical Model

The view that laws are unrestricted true universal statements has resulted in a commonly held opinion that geologic generalizations are borrowed from theoretical sciences or are accidental generalizations. It is also held that geology acquires much from physics and chemistry but it also has a historical core which is independent of these sciences, because geology, on the one hand, must account for what is particular, on the other hand, must find what is typical (Bucher, 1936; Bradley, 1963, Simpson, 1963, Kitts, 1963a, 1963b, 1974; Laudan, 1987). However, according to the classical model of laws, laws are unrestricted true universal statements, and since the historical generalizations necessarily make reference to particular places, times and proper names, there cannot be true geologic laws.

As for biology, for instance, Hull (1976, 1978 and 1981), by making an ontological assertion that particular species are individuals, contended that since laws are spatio-temporally unrestricted and no uneliminable reference can be made to proper names, individuals, or to particular places and times in laws, species as individuals cannot perform the proper function in laws. Furthermore Hull (1976) maintained that the species-as-individuals thesis "poses no threat to the status of evolutionary theory as a spatio-temporally unrestricted scientific theory because no version of evolutionary theory actually refers to particular species anyway" (p. 189). He alleged that genuine biological laws can be and are, in fact, formulated in terms of entities that are unrestricted in scope, e.g. species *category,* DNA, etc. Caplan (1981), though agreeing with Hull that many of the biological laws make reference to species category not to particular species, rejected this as a conclusive evidence for ascribing particular species a different ontological status because he averred that species are spatio-temporally unrestricted classes that can function in laws (p. 138). If otherwise, all social sciences and history, which are concerned with human beings (*homo sapiens*), would cease to be sciences, which is unacceptable (Caplan, 1981, p. 137; also Ruse 1989, p. 111). However, Hull (1981) responded that "any generalization in which the names of particular species function is at best an empirical generalization" (p. 141). This view is also shared by Dray (1989), who, accepting that historians may formulate generalizations that can help to explain particular occurrences that fall under them, maintains that since these generalizations are derived from particulars that happened in particular places and times “they will be quite incapable of explaining what occurred beyond the places and times of which they originally asserted” (p. 181).

So upon accepting that laws are spatio-temporally unrestricted, in order to show that geologic, biological, and historical generalizations that are restricted are laws, there are two possible ways: eliminating the reference to particular places and times in these generalizations or demonstrating that restricted generalizations are readily deducible from unrestricted laws.

C.4. Eliminating the Reference to Particular Places and Times in Restricted Generalizations

Accepting that geologic generalizations are restricted, because they contain implicit or explicit reference to the planet earth, Kitts (1963a) asserted that it is, in principle, eliminable (p. 56). This conclusion was also reached by Watson (1966), who contended that the restricted scope is the result of men’s observational incapacities and thus “such law-like generalizations do not differ in logical form nor in explanatory nature from the laws of, e.g., elementary chemistry" (p. 177). I should note that both Kitts and Watson seem to have agreed about the classical model of laws.

However, although the elimination of the reference to particular places and times provides unrestrictedness it jeopardizes the truth of the generalizations. Smart (1963) also considered this possibility for biological generalizations: 'mouse' in the generalization 'Albinotic mice always breed true' may be defined in a general way without making reference to any particular place and time. Smart, nonetheless, maintained that there is no reason to believe that the supposed law 'Albinotic mice always breed true' is true since a mouse-like creature from another planet might possibly falsify it. There arises a paradoxical dilemma for the biologist: if s/he wants biological generalizations to be true they become restricted in scope, but if s/he transforms them into universal statements then they become false.

The problem is similar with other biological classes. For instance, Bunzl (1993) accepted that species category or DNA are, in principle, unrestricted but he maintained that truth of the generalizations about these entities is highly doubtful. Suppose that we found an extraterrestrial counter-instance against our biological generalizations (for example, "hereditary material came in triples instead of pairs" (p. 92) or "DNA played a different functional role" (p. 94)) then the generalizations would be false.

C.5. Deduction from Unrestricted Laws

Another possible way to retain geologic laws by keeping the unrestrictedness condition is to show that although they are restricted in scope they are deductive consequences of higher level unrestricted laws. This is similar to Hempel and Oppenheim's (1948) distinction between fundamental and derivative laws which was proposed to account for the lawlikeness of the restricted generalizations, such as Kepler's laws. Thus

Kepler's laws, though making reference to particular objects, can be derived from Newtonian mechanics and gravitational theory, which have purely qualitative predicates, whereas accidental generalizations, such as 'Every apple in basket *b* at time *t* is red', cannot be deductively articulated to fundamental laws (pp. 20-21). However, Nagel (1961) pointed out that Kepler's laws are not direct consequences of Newton's theory, but are derivable only by conjunction with additional premises. Therefore many universal generalizations that are apparently non-lawlike qualify as laws by such derivations with additional premises (p. 58). Furthermore in deduction of Kepler's laws from Newton's theory certain initial conditions must be presupposed. For example, elliptic orbits that are required by Kepler's laws are dependent strictly upon the history of the planets (that is, they are contingent). In the case of geologic, biological, and historical generalizations, it is extremely difficult, if not impossible, to show that geologic generalizations can be deduced from the laws of physics and chemistry.

Furthermore the conviction that the higher level hypotheses, i.e. the laws of physics and chemistry, are unrestricted is highly questionable. First, all statistical macrophysical laws are obviously restricted in scope. Second, even the fundamental laws disqualify as laws, because they do not satisfy the unrestrictedness requirement. For example, as Nagel (1961, p. 57), Kitts and Kitts (1979, p. 618) and Bunzl (1993, pp. 96-97) noted, if different cosmic periods are determined by different regularities as suggested by some cyclical big bang theories and no underlying unrestricted laws are possible to explain the laws of each cycle, i.e. each cycle is causally independent and the laws that governed each cycle are incompatible then all laws of physics would turn out to be restricted in scope. As in the case of geology, it can be claimed that the observed regularities are only accidental and restricted to *this* cycle. Hence if the lawlikeness of the fundamental laws cannot be established how is it possible to establish lawlikeness of other lower level generalizations on the basis of such "fundamental" laws.

C.6. Other Conditions of Lawlikeness

If it is not possible to demonstrate that generalizations of geology, biology, or history are unrestricted another possible way is to drop the unrestrictedness condition. However, it can be objected that if this claim were rejected it would not be possible to distinguish between laws and accidental generalizations. Below I discuss several conditions that are asserted to distinguish laws from accidental generalizations.

According to Goodman (1947), while the generalization "Everything in my pocket on V-E day was silver" (Ibid., p. 123) does not support the counterfactual conditional "If *P* had been in my pocket on V-E day, *P* would have been silver" (Ibid., p. 122), the counterfactual "If that piece of butter had been heated to 150° F., it would have been melted" (Ibid., p. 113) is supported by the universal statement "All butter melts at 150° F" (Ibid., p. 123). But what makes us so confident that the latter support counterfactual conditionals? Before going any further in answering this question we will see how historical generalizations respond to counterfactuals.

Joynt and Rescher (1961, pp. 156-158) claimed that even though the historian produces only *limited* generalizations these are not simple descriptive statements but, though restricted in scope, i.e. making explicit reference to proper names, places, groups of person, periods of time, customs and so on, help the historian to explain historical events. For instance, the statement "In the seafights of sailing vessels in the period of 1653-1805, large formations were too cumbersome for effectual control as single units" (p. 156) is not a mere description of historical facts but has counterfactual force because "it asserts that in literally any large-scale fleet action fought under the conditions in question ... effectual control of a great battle line is hopeless" (Ibid.). White (1965, pp. 47-53) and Murphey (1973, pp. 76-86) also argued that historical generalizations, though spatio-temporally restricted, have predictive and explanatory power and support counterfactuals. They also contended that there are laws for individuals. White maintained that the statement "Whenever John eats spinach, John breaks out in a rash" (p. 48) is a law because although it has an uneliminable reference to John it is universal as obvious from its logical formulation: "for every time t, if John eats spinach at time t, then John breaks out in a rash at t+24 hours" (p. 49). The statements such as this has both a proper name and the words 'whenever,' 'always,' etc. and are therefore universal and singular. He also claimed that this statement, which is not only a summary of its instances, has explanatory and predictive power, and supports counterfactuals.

Braithwaite (1946) argued that in order for a true universal hypothesis *h* to be lawlike it "either occurs in an established scientific deductive system as a higher-level hypothesis containing theoretical concepts or that it occurs in an established scientific deductive system as a deduction from higher level hypotheses which are supported by empirical evidence which is not direct evidence for *h* itself" (pp. 301-302). For example, the statement 'All men are mortal' is not a law because it is supported only by its instances. However, Braithwaite was aware of the fact that if 'All men are mortal' is considered as a deductive consequence of 'All animals are mortal' it will be lawlike. But if 'All animals are mortal' is the highest level hypothesis in this deductive system it will be non-lawlike since it is supported solely by its own instances. At this point, Dretske (1977) correctly questioned how a statement that is not itself a law can secure lawlikeness of others (p. 261). However, Braithwaite had maintained that the lower level hypotheses are lawlike because they are deductive consequences of higher level hypotheses, the higher level hypotheses are also lawlike because they explain lower level hypotheses (pp. 303). The problem with this account is that it is circular and cannot distinguish between laws and accidental generalizations "for every generalization implies another of smaller scope" (Dretske, 1978, p. 261). Braithwaite (1946) maintained in response to such a criticism that if the evidence for a higher-level generalization were obtained only from its instances, i.e. from lower level hypotheses that are deductive consequence of it, the hypothesis would be said to explain its instances poorly because the only evidence at hand is direct evidence (p. 302). Therefore only on the condition of indirect evidence from the same-level hypotheses would provide independent knowledge for the truth of a hypothesis (Ibid., p. 303).

Ayer (1956) and Nagel (1961) also offered indirect evidence from other hypotheses to establish lawlikeness of general statements. According to Nagel (1961, pp. 64-65), indirect evidence for a hypothesis may be in two senses. First, the hypothesis, h_1, may be derivable from the higher level hypothesis, L, together with other hypotheses, h_2, h_3, ... which are at the same level with h_1 but whose scopes are different. The direct evidence for h_2, h_3, ... would count as indirect for h_1. Second, h_1, h_2, h_3, ... can be derived from L with some additional assumptions, a_1, a_2, a_3, ... (L plus a_1, a_2, a_3, ...). The direct evidence for h_1, h_2, h_3, ... would count as indirect for L. Consider, for example, Ayer's (1956, p. 52) argument regarding the statement 'all the cigarettes in my case are made of Virginian tobacco', which is similar to Goodman's (1947) example of silver coins. Ayer admitted that if someone told him that he absent mindedly put Turkish cigarettes in his case the generalization would not hold. However, he maintained that if his case has a peculiar property that transforms all cigarettes put into his case into Virginian tobacco then the statement would not be false. Of course this assumption requires other generalizations or facts that provide evidence for such a property. Nevertheless our total knowledge is not capable of supporting Ayer's sentence that his case has such power to turn all cigarettes into Virginian tobacco. Therefore the statement fails to be lawlike.

C.7. Arguments in favor of Restricted Laws

I assert that laws should not be considered as isolated statements, because they become meaningful so far as they are part of a theoretical system, and lawlikeness of general statements are established by their relation to the other generalizations and facts within the same system, and even by their relation to other established theoretical systems. The relationship between the items of the system is not of necessarily deductive type. Hence I defend a non-deductive holistic model of laws. Goodman's claim that laws support counterfactuals should be considered in this respect. The statement "All butter melts at 150° F" seems to support the counterfactual "If that piece of butter had been heated to 150° F., it would have been melted"; however, this generalization is also supported by all kinds of physical and chemical generalizations (at the same and higher level) and facts that al together support the counterfactual. The case of 'the coins in Goodman's pocket' and Ayer's example of Virginian tobaccos do not support the relevant counterfactuals, because there is no support from any part of our knowledge that support such generalizations. The key here is the indirect evidence provided by other general and particular hypotheses. However, I should note that I do not interpret theories as deductive systems but rather as coherent wholes, because deductive relationship is too strong a relationship to demonstrate.

If the relationship between theories and laws is construed in this way it can be argued that, at least, some geologic, biological, and historical generalizations are laws since they are not isolated statements, as those accidental generalizations that are proposed for consideration (see for example Goodman's and Ayer's examples), but are coherent with other facts within the systems to which they belong, i.e. mutually supporting. Furthermore geology, biology, and history are also expected to be coherent with the same and higher level theoretical sciences. But since construing this relationship as deductive requires a reductionist stand, which is difficult to show, I prefer a coherentist relationship both intra- and inter-science which also permits local deductive relationships. For example, Kitts (1963a) also stated regarding geology that, despite the abundance of geologic generalizations, "geology is not a discipline unto itself." (p. 60) But not in the sense that geologic generalizations can be readily articulated into the general physical-chemical theory (because this is still an open question) but rather that while formulating geologic generalizations geologists keep an eye on the physical-chemical theory in order not to violate them (Kitts, 1974, p. 3). For example, no historian or geologist would assert any statement that would violate the law of gravity.

On the face of it, consider for example the geologic generalization "Major streams of the northern Appalachian region rise in the Allegheny Plateau and flow southeastward to the Atlantic directly across the northeast-southwest grain of rocks and structures ranging in age from Precambrian to Tertiary" (Kitts, 1963a, pp. 54-55). According to Kitts, this statement is non-lawlike because it refers to a finite class of specific object. However, except being restricted in scope which does not hinder a generalization from being lawlike, as I have established above, this statement is lawlike for the following reasons: first, although this statement appears to refer to a finite class of objects, it is different from, for example, Goodman's (1947) example 'All coins in my pocket on V-E day were silver' because the geologic example is not asserted as an isolated statement to account for certain isolated phenomenon. It is meaningful in relation to total geologic knowledge; second, it supports the counterfactual conditional 'if S were a major stream in the Appalachian region it would flow toward the Atlantic' not in virtue of an irreducible notion of necessity but as a result of its relation to other geologic and physical facts. What makes the streams flow toward the Atlantic is probably the fact that the region has an inclination toward the Atlantic. This fact can be formulated as 'under gravity, fluids flow from higher to lower levels', which is a physical law. However, it may be argued, as Flichman (1995, p. 39) does on a similar example, that what supports the counterfactual is not the geologic generalization but the underlying law. But the geologic generalization is not directly deducible from the physical law because other geologic and morphological assumptions must be taken into account. For example, although this region may be inclined towards the Atlantic, in the presence of a depression in that area could change the course of streams. Furthermore, some morphological and geological factors, such as the configuration of mountain ranges and rock types, may affect drainage system. Therefore additional assumptions as in the case of the deduction of Kepler's laws from Newtonian mechanics and gravitational theory would be necessary. However, it is true that this geologic statement has only a very limited scope of application and thus is an extremely low level generalization, but, I maintain, it is *still* lawlike. Within this regard, White's (1965) example 'Whenever John eats spinach, John breaks out in a rash' could be a law if it were shown that it occurs in an established system as a meaningful constituent. However, even in history such statements so specific as this one are not usually functional, except perhaps when concerning great historical characters. Therefore such a statement may be lawlike in one context while non-lawlike in another.

Another geologic example may clarify the relationship of a generalization with other generalizations and facts and the significance of indirect evidence. This example, I believe, shows that at least some geological, biological and historical generalizations which are not readily reducible to laws of physics, i.e. they are not derivative laws of Hempel and Oppenheim, are lawlike due to the support they acquire from the other same level generalizations. It has been observed that all modern coral reefs form only around the margins of islands and continents between the tropics. (This statement can be formulated more generally to include other coral reefs throughout the earth history: 'Coral reefs indicate only island and continental margins within the limits of warm waters.') The generalization about the modern coral reefs is empirically established and would be considered as an accidental generalization in the standard account of laws like those given above regarding silver coins, Virginian tobacco, etc. However, its difference from such accidental generalizations is that it is supported by indirect evidence and have explanatory and predictive power. Coral reefs are built primarily by corals and the green alga Zooxantellae as a result of their symbiotic relationship in which "the alga contributes to the calcification capability of corals by extracting carbon dioxide from the animals' body fluids, thus increasing the concentration of the carbonate ion needed for the precipitation of calcium carbonate" (Burchfiel et al., 1982, p. 419). Other generalizations in support are the following: coral reefs are restricted to warm and shallow water, i.e. island and continental margins between the tropics because (i) sunlight required for the algae to photosynthesize is available only at shallow depths, (ii) shallow water environments are the places where strong water movement is present which brings corals planktons in large amounts, (iii) warm water enables corals to secrete calcium carbonate that forms the structure of the reef. (However, reefs do not build in warm and shallow water near deltas of large rivers because water in such areas is sediment-laden that shields sunlight and hinders corals to feed on small floating animals.)

So far, I have given arguments that unrestrictedness is not a necessary condition for lawlikeness and also given evidence that, on the basis of the foregoing, there can be laws with restricted scope, and that geology can, in principle, have laws. I have also provided more evidence that the difference between the generalizations of physics and geology is only a matter of degree. That is, even though laws of physics and chemistry are more general in scope, have more theoretical terms and so on they are not different, in principle, from laws of geology.

From the foregoing we can conclude that: the claim that those sciences, such as geology, biology, human history, cannot have laws is the result of the classical model that construes laws as unrestricted true universal statements; if the standard model is accepted no scientific laws are possible at all; there is no qualitative distinction between the generalizations of more theoretical sciences and geology, biology and human history.

REFERENCES

Albritton, Jr., C. C. 1967. Uniformity, the Ambiguous Principle, in *Uniformity and Simplicity*, C. C. Albritton, Jr. (ed.). GSA Special Paper No: 89. GSA: New York, 1-2.

Aristotle. 1961. *Physics.* Tr. by R. Hope. University of Nebraska Press.

Aune B. 1962. Fatalism and Professor Taylor. *Philosophical Review* 71/4, 512-519.

Ayer, A. J. 1970 (1956) What is a Law of Nature, in *Readings In the Philosophy of Science*, B. A. Broady (ed.). Prentice Hall: Englewood Cliffs, p. 39-54.

Balashov, Y. V. 1992. On the Evolution of Natural Laws. *Brit. J. Phil. Sci.* 43, 343-370.

Barret, M. and Sober, E. 1992. Is Entropy Relevant to the Asymmetry Between Retrodiction and Prediction? *Brit. J. Phil. Sci.* 43, 141-160.

Bennet, J. 1966. *Kant's Analytic.* Cambridge University Press: Cambridge.

Bosworth, R. C. L. 1958. Irreversible Processes in Physical Theory. *Nature* 181, 402.

Bradley, W. H. 1964 (1963). Geologic laws, in *The Fabric of Geology*, C. C. Albritton, Jr. (ed.). Freeman, Cooper, & Company: Stanford, 12-23.

Braithwaite, R. B. 1964 (1947) *Scientific Explanation.* Cambridge University Press: Cambridge.

Broad, C. D. [1949] 1959. *Scientific Thought.* Littlefield, Adams & Co: Paterson.

Bucher, W. H. 1936. The Concept of Natural Law In Geology. *Ohio Journal of Science* 36/4, 183-194.

Bunzl, M. 1993 *The Context of Explanation.* Kluwer: Dordrecht.

Burchfiel, B. C., Foster, R. J., Keller, E. A., Melhorn, W. N., Brookins, D. G., Mintz, L. W., and Thurman, H. V. 1982. *Physical Geology: The Structure and Process of the Earth.* Charles E. Merill: Columbus.

Callender, C. 1995. The Metaphysics of Time Reversal: Hutchison on Classical Mechanics. *Brit. J. Phil. Sci.* 46, 331-340.

Caplan, A. L. 1981. Back to Class: A Note on the Ontology of Species. *Philosophy of Science* 48, 130-140.

Carey, W. S. 1976. *The Expanding Earth.* Elsevier: Amsterdam.

Carnap, R. 1958. *Introduction to Symbolic Logic and Its Applications.* Tr. by W. H. Meyer and J. Wilkinson. Dover: New York.

Casati, R. and Varzi, A. C. 1994. *Holes and Other Superficialities.* The MIT Press: Cambridge.

Charlton, W. 1983. Causation and Change. *Philosophy* 58, 143-160.

Coburn, R. C. 1971. Identity and Spatiotemporal Continuity, in Identity and Individuation, M. K. Munitz (ed.). New York University Press: New York, 51-101.

Costa de Beauregard, O. 1965. Irreversibility Problems, in *Logic, Methodology and Philosophy of Science,* Y. Bar-Hillel (ed.). North-Holland Publishing Co: Amsterdam, 313-346.

Costa de Beauregard, O. 1968. On Time, Information and Life. *Dialectica* 22/3-4, 187-205.

Danto, A. C. 1965. *Analytical Philosophy of History.* Cambridge University Press: London.

Davies, P. C. W. 1975. Cosmological Aspects of Irreversibility, in *Entropy and Information in Science and Philosophy,* L. Kubát and J. Zeman (eds.). Elsevier: Amsterdam, 11-28.

Davies, P. C. W. 1977. *The Physics of Time Asymmetry.* University of California Press: Cambridge.

Davies, P. C. W. 1978. *Space and time in the modern universe.* Cambridge University Press: Cambridge.

Denbigh, K. G. 1975. A Non-Conserved Function for Organized Systems, in Entropy and Information in Science and Philosophy, L. Kubát and J. Zeman (eds.). Elsevier: Amsterdam, 83-92.

Denbigh, K. G. 1981. *Three Concepts of Time.* Springer-Verlag: Berlin.

Denbigh, K. G. 1989a. Note on Entropy, Disorder and Disorganization. *Brit. J. Phil. Sci.* 40, 323-332.

Denbigh, K. G. 1989b. The Many Faces of Irreversibility. *Brit. J. Phil. Sci.* 40, 501-518.

Denbigh, K. G. 1994. Comment on Barret and Sober's Paper on the relevance of Entropy to Retrodiction and Prediction. *Brit. J. Phil. Sci.* 45, 709-711.

Dobzhansky, Theodosius. 1963. Scientific Explanation—Chance and Antichance in Organic Evolution, in *Philosophy of Science, The Delaware Seminar, 1961-1962, Volume I,* B. Baumrin (ed.). Interscience Publishers: New York, 209-222.

Dray, W. H. 1964 (1957). *Laws and Explanation in History.* Oxford University Press: Oxford.

Dray, W. H. 1989. *On History and Philosophers of History.* E. J. Brill: Leiden.

Dretske, F. I. 1977. Laws of Nature. *Philosophy of Science* 44, 248-268.

Earman, J. 1974. An Attempt to Add a Little Direction to 'The Problem of the Direction of Time.' *Philosophy of Science* 41, 15-47.

Edel, A. 1975. Aristotle's Categories and the Nature of Categorial Theory. *Review of Metaphysics* 29/1, 45-65.

Flichman, E. H. 1995. Hard and Soft Accidental Uniformities. *Philosophy of Science* 62, 31-43.

Forbes, G. 1987. Is There a Problem About Persistence, in *Identity*, H. Noonan (ed.). Dartmouth: Aldershot, 291-301.

Franklin, J. 1984. Natural Sciences as Textual Interpretation: The Hermeneutics of the Natural Sign. *Philosophy and Phenomenological Research* 44/4, 509-520.

Gardiner, P. 1961. *The Nature of Historical Explanation*. Oxford University Press: Oxford.

Gilbert, G. K. 1896. The Origin of Hypotheses. Presidential Adress of 1895. The Geological Society of Washington: Washington.

Gillispie, C. C. 1959 (1951). *Genesis and Geology*. Harper & Row: New York.

Glossary of Geology. 1980. R. L. Bates and J. A. Jackson (eds.). American Geological Institute, Falls Church.

Goodman, N. 1947. The Problem of Counterfactual Conditionals. *The Journal of Philosophy* 44/5, 113-128.

Goodman, N. 1966 (1951). *The Structure of Appearance*. The Bobbs-Merrill: Indianapolis.

Goodman, N. 1967. Uniformity and Simplicity, in *Uniformity and Simplicity*, C. C. Albritton, Jr. (ed.). GSA Special Paper No: 89. GSA: New York, 93-99.

Gould, S. J. 1965. Is uniformitarianism necessary? *American Journal of Science* 263, 223-228.

Gould, S. J. 1987. *Time's Arrow Time's Cycle.* Harvard University Press: Cambridge.

Grünbaum, A. 1967. The Anisotropy of Time, in *The Nature of Time,* T. Gold and D. L. Schumacher (eds.). Cornell University Press: Ithaca, 149-186.

Grünbaum, A. 1970. The Meaning Of Time, in *Essays in Honor of Carl G. Hempel,* N. Rescher et al. (eds.). D. Reidel: Dordrecht, 147-177.

Grünbaum, A. 1973 (1964). *Philosophical Problems of Space and Time.* Dordrecht Reidel: Dordrecht.

Grünbaum, A. 1974. Popper's views on the Arrow of time, in *The Philosophy of Karl Popper,* P. A. Schilpp (ed.). Open Court: Lasalle, 775-797.

Hallam, A. 1984. *Great Geological Controversies.* Oxford University Press: Oxford.

Harrison, M. J. 1975. Entropy Concepts in Physics, in *Entropy and Information in Science and Philosophy,* L. Kubát and J. Zeman (eds.). Elsevier: Amsterdam, 41-59.

Helm, P. 1975. Are "Cambridge" Changes Non-events? *Analysis* 35, 140-144.

Helm, P. 1977. Detecting Change. *Ratio* 19, 34-38.

Hempel, C. G. 1962. Deductive-Nomological vs. Statistical Explanation, in *Minnesota Studies in the Philosophy of Science, Vol. III,* H. Feigl and G. Maxwell (eds.). University of Minnesota Press, Minneapolis, 98-169.

Hempel, C. G. 1963. Explanation and Prediction by Covering Laws, in *Philosophy of Science, The Delaware Seminar, 1961-1962, Vol. I,* B. Baumrin (ed.). Interscience Publishers: New York, 107-133.

Hempel, C. G. 1965. *Aspects of Scientific Explanation and other Essays in the Philosophy of Science.* The Free Press: New York.

Hempel, C. G. 1966 (1962). Explanation in Science and in History, in *Philosophical Analysis and History,* W. H. Dray (ed.). Harper & Row: New York, pp. 95.126.

Hempel, C. G. 1970 (1966). Probabilistic Explanation, in *Readings in the Philosophy of Science,* B. A. Brody (ed.). Prentice Hall: Engelwood Cliffs, 28-38.

Hempel, C. G. and Oppenheim, P. 1970 (1948). Studies in the Logic of Explanation, in *Readings in the Philosophy of Science,* B. A. Brody (ed.). Prentice Hall: Engelwood Cliffs, 8-27.

Hill, E. L. and Grünbaum, A. 1957. Irreversible Processes in Physical Theory. *Nature* 179, 1296-1297.

Hooykaas, R. 1975 (1970) Catastrophism in Geology, Its Scientific Character in Relation to Actualism and Uniformitarianism, in *Philosophy of Geohistory: 1785-1970*, C. C. Albritton, Jr. (ed.). Dowden, Hutchinson & Ross: Stroudsburg, 310-356.

Hooykaas, R. 1963. *The Principle of Uniformity in Geology, Biology and Theology*. E. J. Brill: Leiden.

Hubbert, M. K. 1967. Critique of the Principle of Uniformity, in *Uniformity and Simplicity*, C. C. Albritton, Jr. (ed.). GSA Special Paper No: 89. GSA: New York, 3-33.

Hull, D. L. 1976. Are Species Really Individuals? *Systematic Zoology* 25, 174-191.

Hull, D. L. 1978. A Matter of Individuality. *Philosophy of Science* 45, 335-360.

Hull, D. L. 1981. Kitts and Kitts and Caplan on Species. *Philosophy of Science* 48, 141-152.

Hume, D. 1978 (1739). *A Treatise of Human Nature.* Clarendon: Oxford.

Hume, D. 1992 (1777). Enquiries Concerning Human Understanding and Concerning the Principles of Moral. Clarendon: Oxford.

Hutchison, K. 1993. Is Classical Mechanics Really Time-reversible and Deterministic? *Brit. J. Phil. Sci.* 44, 307-323.

Hutchison, K. 1995a. Temporal Asymmetry in Classical Mechanics. *Brit. J. Phil. Sci.* 46, 219-234.

Hutchison, K. 1995b. Differing Criteria for Temporal Symmetry. *Brit. J. Phil. Sci.* 46, 341-347.

Jenkins, K. 1995. *On 'What is History?'* Routledge: London.

Jordan, P. 1971. *The Expanding Earth: Some Consequences of Dirac's Gravitation Hypothesis.* Tr. by A. Beer. Pergamon: Oxford.

Joske, W. D. 1967. *Material Objects.* MacMillan: London.

Joynt, C. B. and Rescher, N. 1961. The Problem of Uniqueness in History. *History and Theory* 1, 150-162.

Kim, J. 1973. Causation, Nomic Subsumption, and the Concept of Event. *The Journal of Philosophy* 70/8, 217-236.

Kim, J. 1974. Noncausal Connections. *Nous* 8, 41-52.

Kitts, D. B. 1963a. Historical Explanation in Geology. *The Journal of Geology* 71, 297-313.

Kitts, D. B. 1963b. The Theory of Geology, in *The Fabric of Geology*, C. C. Albritton, Jr. (ed.). Freeman, Cooper, & Company: Stanford, 49-68.

Kitts, D. B. 1974. Physical Theory and Geological Knowledge. *The Journal of Geology* 82, 1-23.

Kitts, D. B. and Kitts, D. J. 1979. Biological Species as Natural Kinds. *Philosophy of Science* 46, 613-622.

Körner, S. 1983. Thinking, Thought, and Categories. *The Monist* 66/3, 353-366.

Lambert, K. and Britton, Jr., G. 1992 (1970). *An Introduction to the Philosophy of Science.* Ridgeview Publishing Company: Atascadero.

Laudan, R. 1987. *From Mineralogy to Geology.* The University of Chicago Press: Chicago.

Lewis, D. 1971. Counterparts of Person and Their Bodies. *The Journal of Philosophy* 68, 203-211.

Lewis, D. and Lewis, S. 1970. Holes. *Australasian Journal of Philosophy* 48/2, 206-212.

Locke, J. 1979 (1689). *An Essay Concerning Human Understanding.* Clarendon Press: Oxford.

Mach, E. 1974 (1893). *The Science of Mechanics.* Tr. by T. J. McCormack, The Open Court: La Salle.

McCrae, W. H. 1968. Cosmology after half a century. *Science* 160, 1295-1299.

McTaggart, J. M. E. 1993 (1908). The Unreality of Time, in *Philosophy of Time,* R. Le Poidevin and M. MacBeath (eds.). Oxford University Press: Oxford, 23-34.

Mehlberg, H, 1961. Physical laws and time's arrow, in *Current Issues in the Philosophy of Science,* H. Feigl and G. Maxwell (eds.). Holt, Rinehart and Winston: New York, 105-138.

Mellor, D. H. 1981. *Real Time.* Cambridge University Press: Cambridge.

Mill, J. S. 1862. *A System of Logic.* Parker, Son, and Bourn: London.

Murphey, M. G. 1973. *Our Knowledge of the Historical Past.* The Bobbs-Merrill Company: Indianapolis.

Nagel, E. 1982 (1961). *The Structure of Science.* Routledge & Kegan Paul: London.

Newton-Smith, W. H. 1980. *The Structure of Time.* Routledge & Kegan Paul: London.

Oderberg, D. S. *The Metaphysics of Identity over Time.* St. Martin's Press: New York.

Penrose, O. 1970. *Foundations of Statistical Mechanics.* Pergamon: Oxford.

Pivcevic, E. 1990. *Change and Selves.* Clarendon Press: Oxford.

Popper, K. R. 1956a. The Arrow of Time. *Nature* 177, 538.

Popper, K. R. 1956b. *Nature* 177, 382.

Popper, K. R. 1957. *Nature* 179, 1297.

Popper, K. R. 1958. *Nature* 181, 402-403.

Popper, K. R. 1960 (1957). *The Poverty of Historicism.* Routledge & Kegan Paul: London.

Popper, K. R. 1962 (1945) *The Open Society and Its Enemies.* Routledge & Kegan Paul: London

Popper, K. R. 1972 (1959). *The Logic of Scientific Discovery.* Hutchinson: London.

Popper, K. R. 1974. Replies to my critics. in *The Philosophy of Karl Popper,* P. A. Schilpp (ed.) Open Court: Lasalle, 961-1201.

Prigogine, I. 1980. *From being to becoming.* W. H. Freeman and Company: San Francisco.

Prior, A. N. 1993 (1968). Changes in Events and Changes in Things, in *Philosophy of Time,* R. Le Poidevin and M. MacBeath (eds.). Oxford University Press: Oxford, 35-46.

Quine, W. V. O. 1964 (1953). *From A Logical Point of View: 9 Logico-Philosophical Essays.* Harvard University Press: Cambridge.

Quinton, A. 1973. *The Nature of Things.* Routledge & Kegan Paul: London.

Reichenbach, H. 1971 (1956). *The Direction of Time.* University of California Press: Berkeley.

Rescher, N. 1958. On Prediction and Explanation. *Brit. J. Phil. Sci.* 8. 281-290.

Ricoeur, P.1976. History and Hermeneutics. *The Journal of Philosophy* 73/19, 683-695.

Rotenstreich, N. 1958. *Between Past and Present: An essay on History.* Yale University Press: New Haven.

Rudwick, M. J. S. 1976 (1972). *The Meaning of Fossils.* Neala Watson Academic Publications: New York.

Ruse, M. 1989. *The Darwinian Paradigm: Essays on its history, philosophy and religious implications*. Routledge: London.

Russel, B. 1948. *Human Knowledge: Its Scope and Limits*. George Allen and Unwin: London.

Russell, B. 1968 (1921). *The Analysis of Mind*. George Allen & Unwin: London.

Russell, B. 1994 (1917). Mysticism and Logic. Routledge: London.

Savitt, S. F. 1994. Is Classical Mechanics Time Reversal Invariant? *Brit. J. Phil. Sci.* 45, 907-913.

Schlegel, R. 1956. Irreversibility and Mechanics. *Nature* 178, 381-382.

Scriven, M. 1958. Definitions, Explanations, and Theories, in *Minnesota Studies in the Philosophy of Science II*. H. Feigl, M. Scriven and G. Maxwell (eds.). University of Minnesota Press: Minneapolis, 99-195.

Scriven, M. 1959. Explanation and Prediction in Evolutionary Theory. *Science* 130/3374, 477-482.

Scriven, M. 1962. Explanation, Prediction and Laws, in *Minnesota Studies in the Philosophy of Science III*, H. Feigl and G. Maxwell (eds.). University of Minnesota Press: Minneapolis, 170-230.

Sellars, W. 1973. Actions and Events. *Nous* 7, 174-202.

Shoemaker, S. 1963. *Self-knowledge and Self-identity*. Cornell University Press: Ithaca.

Shoemaker, S. 1993 (1969). Time Without Change, in *Philosophy of Time,* R. Le Poidevin and M. MacBeath (eds.). Oxford University Press: Oxford, 63-69.

Sider, T. 1996. All the World's a Stage. *Australasian Journal of Philosophy* 74/3, 433-453.

Simpson, G. G. 1975 (1970). Uniformitarianism. An Inquiry into Principle, Theory, and Method in Geohistory and Biohistory, in *Philosophy of Geohistory: 1785-1970*, C. C. Albritton, Jr. (ed.). Dowden, Hutchinson & Ross: Stroudsburg, 256-309.

Simpson, G. G. 1964 (1963). Historical science, in *The Fabric of Geology*, C. C. Albritton, Jr. (ed.). Freeman, Cooper, & Company: Stanford, 24-48.

Smart, J. J. C. 1963. *Philosophy of Science.* Routledge and Kegan Paul: London.

Smart, J. J. C. 1967. Time, in *Encyclopedia of Philosophy* 8, P. Edwards (ed.). MacMillan: New York, 126-134.

Stanley, S. M. 1986. *Earth and Life Through Time.* W. H. Freeman and Company: New York.

Strawson, P. F. 1959. *Individuals.* Methuen: London.

Swinburne, R. 1968. *Space and Time.* St. Martin's Press: New York.

Şengör, A. M. C. 1991. Timing of orogenic events: a persistent geological controversy, in *Modern Controversies in Geology*, D. W. Müller, J. A. McKenzie and H. Weissert (eds.). Academic Press: London, 405-473.

Şengör, A. M. C. 1993. Tarihsel Disiplinlerin Bilimselliği Üzerine. *Bilim Tarihi* 25, 11-30.

Thalberg, I. 1978. The Irreducibility of Events. *Analysis* 38/1, 1-9.

Thalberg, I. 1980. Can We Get Rid of Events? *Analysis* 40, 25-31.

Thompson, J. J. 1983. Parthood and Identity Across Time. *The Journal of Philosophy* 80, 201-220.

Tolman, Richard C., 1938, *The Principles of Statistical Mechanics.* Clarendon Press: Oxford.

Van Bemmelen, R. W. 1961. The Scientific Character of Geology. *Journal of Geology* 69, 453-463.

Van Fraassen, B. C. 1970. *An Introduction to the Philosophy of Time and Space.* Random House: New York.

Van Fraassen, B. C. 1989 *Laws and Symmetry.* Clarendon Press: Oxford.

Von Leyden, W. 1984. Categories of Historical Understanding. *History and Theory* 23, 53-77.

Watanabe, S. 1955. Symmetry of Physical Laws. Part III. Prediction and Retrodiction. *Review of Modern Physics* 27, 179-186.

Watanabe, S. 1969. *Knowing and Guessing: A Formal and Quantitative Study.* John Wiley & Sons: New York.

Watson, R. A. 1966. Is Geology Different: A Critical Discussion of "Fabric of Geology." *Philosophy of Science* 33, 172-185.

White, M. 1982 (1965). *Foundations of Historical Knowledge.* Greenwood Press: Westport.

Whitehead, A. N. 1952 (1925). *Science and the Modern World.* The New American Library: New York.

Whyte, L. L. 1955. One-Way Processes in Physics and Biophysics. *Brit. J. Phil. Sci.* 6, 107-121.

Wilson, N. L. 1974. Facts, Events and Their Identity Conditions. *Philosophical Studies* 25, 303-321.

Wright, P. G. 1970. Entropy and Disorder. *Contemporary Physics* 11, 581-588.

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2. 1995. Bilimsel Devrimler *Radikal Kavramsal Değişimler*'e İndirgenebilir mi? (Can Scientific Revolutions Be Reduced To *Radical Conceptual Changes*). *Felsefe Dünyası* 16, 100-111.

3. 1996. Matematik Tüm Bilimler İçin En Uygun Dil Midir? (Is Mathematics the Most Suitable Language for All Sciences). *Felsefe Dünyası* 19, 75-80.

4. 1997. Levha Tektoniği: Jeolojide Bir Devrim mi? (Plate Tectonics: A Revolution in Geology?). *Yerbilimleri Dergisi* 8(1-2-3), 117-123.

5. (with D. Grünberg) 1993. Jeolojinin Bilimsel Niteliği Üzerine Tartışma (Discussion on the Scientific Character of Geology). *Bulletin of the Geological Congress of Turkey* (The Chamber of Geological Engineers), 85-89.

6. (with H. Ünder) 1998. A Model for the Conservation of Geological Remains as Documents. *Environmental Geology* (to be published).

Publications - Abstracts

1. 1991. Jeoloji Felsefesi ve Bir Örnek (Philosophy of Geology and a Case Study). *Abstracts of the Geological Congress of Turkey* (The Chamber of Geological Engineers), 17.

2. 1991. Alt Mesozoyik Yaşlı Dört Kuzey Karolina Diyabaz Daykında İzlenen Kimyasal ve Petrografik Değişimler (Chemical and Petrographic Variations Across Transverse Profiles of Four Early Mesozoic Diabase Dikes from North Carolina). *Abstracts of the Geological Congress of Turkey* (The Chamber of Geological Engineers), 22.

3. 1996. Yerbilimlerinin Geleceği (Future of the Earth Sciences). *Abstracts of the Geological Congress of Turkey* (The Chamber of Geological Engineers), 44.

4. (with H. Ünder) 1997. Preservation of Geological Remains: A Philosophical Approach to the Foundations. *Abstracts of The International Symposium on Geology and Environment* (UCTEA and The Chamber of Geological Engineers), 138.

Publications - Translations

(With D. Bayrak) 1995. Yerbilimlerinin Felsefi Yönleri. *Jeoloji Mühendisliği*, 46, 64-69 (translated from A. F. Hagner, "Philosophical aspects of the geological sciences," in C. C. Albritton, Jr. (ed.), *The Fabric of Geology* (Freeman, Cooper, & Company, 1964), 233-241.).

Publications - Popular Articles

1. 1993. Objektif bilim mi yoksa ideolojik ve ahlakçı bilim mi? (Objective Science or Ideological and Moralistic Science?). *Jeoloji Mühendisleri Odası Haber Bülteni* 93(6), 36.

2. 1994. Son danışma kurulunda söylediklerim ve söylemediklerim (What I said and did not say at the last meeting of the Counselors Committee). *Jeoloji Mühendisleri Odası Haber Bülteni* 94(4), 14.

3. 1994. Nasıl bir demokratik kitle örgütü? Nasıl bir JMO? (How ought a Non-governmental Organization be? How ought JMO be?). *Jeoloji Mühendisleri Odası Haber Bülteni* 94(5-6), 29-30.

4. 1994. Andre Cailleux'nün *Jeoloji Tarihi* 'nin çevirisinin değerlendirilmesi (Review of the translation of Andre Cailleux' *Historie de la géologie*). *Jeoloji Mühendisleri Odası Haber Bülteni* 94(5-6), 42.

5. 1995. TE-JE-KA veya JE-ME-O? (TE-JE-KA or JE-ME-O). *Jeoloji Mühendisleri Odası Haber Bülteni* 95(1), 15-16.

6. 1996. Vedat Oygür'ün "Madenciliğin sonu mu?" başlıklı makalesinin düşündürdükleri: Teknik bir yazı mı, yazı tekniği mi? (On Vedat Oygür's "End Of Mining?" Science or Rhetoric?). *Jeoloji Mühendisleri Odası Haber Bülteni* 96(3), 20-25.