Structural Stability, Catastrophe Theory, and Applied Mathematics: The John von Neumann Lecture, 1976

Rene Thom

STRUCTURAL STABILITY, CATASTROPHE THEORY, AND APPLIED MATHEMATICS*

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RENÉ THOM†

Abstract. We give a brief description of catastrophe theory, and of its applications; to my view, it is a fundamentally qualitative, interpretative theory, and, by itself, it has no ability to predict. Examples are given of interpretations of singularities in statistics and in geophysics (plate tectonics).

It is perhaps ironical that I am now assessing the importance of such ideas and theories as structural stability and catastrophe theory in front of an audience of applied mathematicians. Why? Because in its very intention catastrophe theory emphasizes the qualitative aspect of empirical situations, whereas applied mathematics is fundamentally devoted to computation.

Of course, applied mathematics cannot exclude qualitative thinking, as its problems are originally given in ordinary language—in a qualitative way. But most applied mathematicians would say—(I believe)—that "modelization" is nothing but translating this qualitative problem into a quantitative model, which then has to be confronted with experiment. On the contrary, catastrophe theory would say that quantitative studies—inasmuch as they are possible and reliable— may help in defining local morphological elements (singularities), from which a global qualitative construction may be built.

Moreover, the truth is that I do not have myself a very clear picture of the activity of a professional applied mathematician. Hence it is quite possible that some of the ideas I express here may seem to you a bit out of the field, as I never had the opportunity of working myself on a very quantitative basis—even in pure mathematics. But as many people—especially in popularization articles—have expressed tremendous hopes about the pragmatic possibilities of catastrophe theory, I think it is time to come back to a more sober appreciation of its impact.

Perhaps a very important cause of the ambiguity, when dealing with catastrophe theory, is its radically novel epistomological status. You read frequently, in these popular articles about catastrophe theory (here abbreviated C.T.), that "Catastrophe theory is a mathematical theory". The truth is that C.T. is not a mathematical theory, but a "body of ideas", I daresay a "state of mind". As soon as the ideas developed by C.T. have reached a very rigorous mathematical status, then these ideas have been incorporated in specific branches of mathematics: singularities of smooth mappings, stratified spaces, singularities of differential forms, bifurcation theory, qualitative dynamics, etc. Hence, strictly speaking, C.T. is not a mathematical theory. Of course, C.T. arose from mathematics, and it has led to important progress in mathematics itself; and we may hope that this

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contribution is not finished. On the contrary, the "practical" results of C.T. are, up to now, not very striking; evaluated by the strict–positivist–criterion of the discovery of "new phenomena", they reduce to a few (not too surprising) facts in geometric optics elaborated by M. Berry at Bristol in his work on caustics.

In the minds of most people, C.T. reduces to what I call "elementary catastrophe theory" (E.C.T.) involving the (too) celebrated seven catastrophes on $\mathbb{R}^4$. For the sake of completeness, let me perhaps recall the basic schema of an "elementary catastrophe". Suppose we have a system (S), the states of which are parametrized by a point ξ in a (smooth) manifold M. Suppose that we may act on this system by varying a point u in a space U of control parameters. Suppose this determination is defined by a map (system of equations)

$$(\Sigma) \qquad\qquad\qquad F(\xi, u) = 0,$$

or

$$F_i(\xi_j, u_k) = 0,$$

$$i = 1, 2, \cdots, n \quad j = 1, 2, \cdots, n, \quad k = 1, \cdots, m.$$

In practically all cases considered in control theory, one admits that this system of equations may be solved—through the implicit function theorem, with the assumption that the Jacobian $D(F_1, \cdots, F_n)/D(\xi_1, \cdots, \xi_n)$ is not zero; hence the possibility of solving (Σ) with respect to $\xi_1, \cdots, \xi_n, \xi = \varphi(u)$, through any point of the graph of the map F in the product space $M \times U \xrightarrow{F} \mathbb{R}^n$. This amounts to saying (cf. $n \geqq m$) that this graph is transversal to the fibers of the projecting map $\Phi: M \times U \to U$ (Condition τ). Control theory then tries to define functions $u_i(t)$ in such a way as to have the corresponding functions $(\xi(t) = \varphi \circ u(t))$ satisfy some optimality condition.

Let us suppose first that $n = 1$; then (Σ) reduces to only one equation $F(x, u_i) = 0$. If $\partial F/\partial x \neq 0$ we are in the standard situation (τ).

Elementary catastrophe theory deals, on the contrary, with situations where the transversality condition (τ) fails; that such situations cannot be avoided—in general—is shown by the following picture (Fig. 1) where for $n = m = 1$ the graph of (Σ) is a compact curve. Any small deformation of F will have points with vertical tangents (a, b), projecting to α, β. Of course, from a "control" point of view, we will do all that we can in order to avoid (in our space U) the "bad" points α, β, where the variation of our system is no longer smoothly controllable. Catastrophe theory—in contrast—tries to study such situations. Here enters the idea that there are two types of lack of transversality: the "finite codimension type", and the "infinite codimension" type. The finite type is exemplified by the case where $\partial F/\partial x = 0$, but there exists a higher order derivative $\partial^k F/\partial x^k$ which is not zero (with $\partial^i F/\partial x^i(0) = 0$, for $i < k$). Then using the Weierstrass–Malgrange preparation theorem, F may be locally replaced by a distinguished polynomial in x:

$$\lambda \cdot F(x, u) = x^k + a_1(u)n^{k-1} + \cdots + a_k(u), \qquad\qquad \lambda(0) \neq 0,$$

and this local situation is now well understood, as we have for it a polynomial
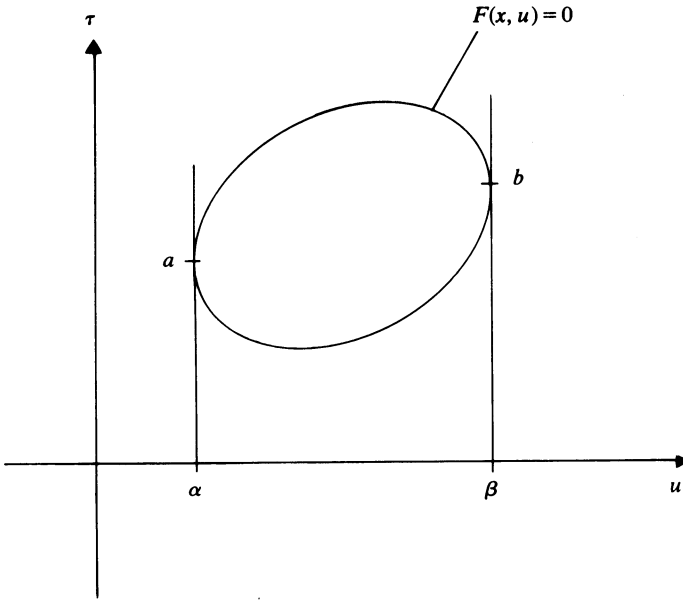
FIG. 1

model. Moreover, if the set of coordinates u is such that the mapping $u \rightarrow v$ $u \rightarrow a_i(u)$ into the space v of coefficients of the generic equation of degree k $x^k/k + v_2 x^{k-2} + \cdots + v_n$ is surjective (the $(k-1)$ coefficient v_1 being annihilated through translation on x), then the singularity will be "unavoidable", i.e., structurally stable under any small C^k deformation of the equation $F(\xi, u) = 0$. This is a consequence of the global continuity of the distinguished polynomial as defined by the local germ of functions F, a fact expressed in the Weierstrass–Malgrange theorem.

The infinite codimension case is exemplified by $F(x, u) = u - \exp(-1/x^2)$, a curve with a *flat* contact (infinite order). No embedding of F into a finite parameter family $F(x, v)$ can make it stable. Such singularities are avoidable (structurally unstable) by a small deformation of F. It is the result of the theory of maps (J. Mather) that for a given set of integers (n, m), there exists only a finite number of "unavoidable" singularities, although the definition of local equivalence has to be purely topological and no longer a local C^∞ isomorphism (at least, in general).

Hence this general idea that there are "unavoidable" catastrophe situations, and that it is of fundamental importance to know all of them; whereas control theory up to now tries to avoid them (not always, as in fact control theory knows the need of shifting strategies discontinuously). It seems clear that if we want to understand biological phenomena, we have to understand these catastrophic effects: for life itself shows a great mastery in dealing with these phenomena, as shown by physiological events such as heart beat, nerve influx, and by morphological events, such as gastrulation, in embryology.

Now in E.C.T. we suppose that the local state ξ in $F(\xi, u)$ is itself given by an optimality principle of type $G(\xi, u)$: that is, (Σ) reduces to: ξ minimizes $G(\xi, u)$ for a given $u \in U$, hence to system (Σ) of type

$$(\Sigma) \qquad\qquad \frac{\partial G}{\partial \xi_i}(\xi, u) = 0,$$

which defines the critical points of $G(\xi, u)$ when ξ varies. Caution should be applied to distinguish between situations which are described by an optimality principle and those governed by an "extremality" principle. Although formally they are directed by the same mathematics, they exhibit a fairly different character. One could say that situations governed by an optimality principle exhibit a fundamentally *irreversible* character, whereas those governed by an extremality principle depend on a *reversible* dynamics. Typical of this last situation is the Hamilton–Jacobi theory which describes the propagation of a wave front, and which leads to an extremality principle on the space (p) of normalized covectors. (The G function itself is defined in terms of the initial data.) The global theory of singularities of functions, which arises out of E.C.T., is now fully grown. It has shed light on many points of wave optics (Saddle-method: the idea of unfolding is very useful here), and the theory has now reached a stage (due to the work of V. Arnold and the Moskow school) where its capabilities exceed the need for it. (The singularities of codim < 16 are now known, and it has developed into a very beautiful theory, which touches many branches of mathematics: algebraic geometry, Platonic solid classification, simple Lie group theory, etc.).

For most people, C.T. is identified with E.C.T. The reason for this is obvious: the table of the seven elementary catastrophes appears as an immediate way of classifying natural situations, and beyond that classification, little is known. Nevertheless, the objection that very few natural dynamics are gradient dynamics remains valid. To that objection it may of course be answered that near any attractor A of any dynamical system there is a local Lyapunov function. Hence in some sense any asymptotic régime is defined by a Lyapunov function around its attractor. In fact, if one allows some C^k-noise, then only the Lyapunov function may be said to have a meaning, as the flow remains entering into the level variety of such a Lyapunov function. But then the question arises whether such Lyapunov functions do exhibit the bifurcation phenomena exhibited by the E.C.T. The answer is obviously no, as the simplest of all possible bifurcations, the so-called Hopf bifurcation, which transforms a source into a repeller, creates—in general— an attracting cycle around the repeller. Then the corresponding Lyapunov function exhibits (on a straight line section) a behavior of type $x^4/4 + x^2/2$ to $x^4/4 - x^2/2$, bifurcation among all *even* functions. Hence, in such a case, the cyclic, nongradient-like character of the bifurcation is expressed by the fact that the corresponding Lyapunov function undergoes a catastrophe which is not elementary, but is defined only among functions which are equivariant with respect to some action of a symmetry group. This simple example shows that there is no chance that E.C.T. alone may provide a sufficient set of possible bifurcations in nature (an objection already made by J. Guckenheimer, who showed that bifurcations among gradient-like fields is not the same as the bifurcation of

gradient fields). It is quite obvious that other types of bifurcations, such as symmetry equivariant, composed map bifurcations, etc., have to be taken into consideration. Moreover we still know very little about the global problem of catastrophe theory, which is the problem of *dynamic synthesis*, i.e., how to relate into a single system a field of local dynamics.

Perhaps the most complete studies which deal with this problem are those concerned with the modelizing of embryological processes. Here perhaps the most novel idea is that when we model such processes, there is no point in looking—as for ordinary dynamical systems—for a flow varying in a fixed phase space, but, instead, one should look for a fixed dynamics defined on a succession of phase spaces: a degree of liberty being at some times triggered, and at some other times, damped out and nonexistent. But of course these ideas have not yet reached the stage of a possible experimental confirmation, and hence are still in an infancy stage.

From my viewpoint, C.T. is fundamentally qualitative, and has as its fundamental aim the explanation of an empirical morphology. Its epistomological status is the one of an interpretative–hermeneutic theory. Hence it is not obvious that it will necessarily develop into new pragmatic developments.

The importance of C.T. in applied mathematics. The idea of structural stability, although proposed originally by technicians in applied mathematics, Andronov and Pontryagin, does not seem to have elicited a tremendous enthusiasm among specialists in computing techniques. I believe that there is a fundamental opposition between the structural stability approach and the "computing viewpoint". When a situation is structurally stable (and very strongly so), this means that this situation may be reached regardless of the initial data, or with a very poor approximation on these data. Hence computing is in that case not necessary, and the situation might be easier to describe through geometrical description, or in general with ordinary language.

In contrast, when we deal with an unstable, threshold-like situation where we have the choice between several outcomes (success or failure), then it is of fundamental importance to determine the precise position of the threshold which separates the initial data which lead to success from those which lead to failure. Hence, in such circumstances, a highly accurate description of the underlying dynamics is necessary. Such a precise description in general involves using physical laws, which are the only possible tools to get such precise dynamics. This shows why the structural stability requirement actually means very little for the technician who has to give a reliable answer to a technical problem.

There is also the philosophical problem pointed out by D. Berlinski in "Synthese": Why is it that physical laws do not themselves obey the structural stability requirement? Classical mechanics, for instance, has to do with Hamiltonian systems, which are very far from being structurally stable. This is a huge problem, which I would like to discuss relatively briefly, as a complete treatment would require lengthy developments.

I would claim—as a principle—that any phenomenon is associated with some kind of irreversibility: for a phenomenon has to *appear*, hence it has to emit something which can be seen (or detected through some apparatus amplifying

human vision). Then the apparent time reversibility of physical laws only shows that these laws do not describe phenomena by themselves, but more accurately change of frames between observers. They describe, so to speak, how the same local irreversible phenomenon may be perceived by different observers. For instance Newtonian dynamics, which gives rise to Kepler's laws and planetary motions, does not in itself describe a phenomenology. For the motion of celestial bodies is a phenomenon only inasmuch as these bodies are lighted by the sun—a typically irreversible phenomenon due to the irreversible transfer from gravitational energy to radiative energy. Moreover, even a planet which turns around an obscure sun does not lead to a phenomenon: for, in Keplerian terms, there is always a canonical transformation of phase space which maps a planet at time t to the same planet at time $t' > t$. This canonical transformation, it is true, is not compatible with the projection $(q_i, p_i) \rightarrow (q_i)$ on the usual configuration space. But such incompatibility may lead only to phenomenology if one is able to fix the (q_i) coordinate, that is, to localize the planet. Such a localization operation (an idempotent projection) always requires coupling with a source of energy (the emission of which is irreversible).

Putting things more abruptly, I would dare to say that the time reversibility of physical laws is probably no more than the expression of a sociological constraint, namely communication between several observers. For this constraint is nothing more than the linguistic constraint between members of the same linguistic community: when people speak the same language, they share the same semantic universe: because, to the same sentence, they have to put the same meaning (or at least, approximately the same). In fact, any observer has to communicate with himself—with his own past. Hence he needs to have the possibility of comparing his way of looking at the universe at time t_1 with the look he had at time $t_0 < t_1$. This requires a common standard of description, a permanent way of parametrizing the states of the world. Hence reversibility of the dynamics.

The same fact can be expressed in another way: Suppose we have a dynamical system (S) described by a flow X on a smooth manifold M. What does it mean to speak of a stationary—or an asymptotic—régime of this dynamic? This basic problem has no satisfactory answer. But let us define this régime by the attractor A of the dynamic towards which the given trajectory is tending with t tending to infinity. Then the asymptotic dynamics on the attractor A has to be time-reversible, for the operation of taking the limit for $t \rightarrow +\infty$ admits only stationary dynamics as solutions. Hence if we want to describe stable "objects or situations" in our irreversible universe, only those which carry a time-reversible dynamics are possible candidates. It is also possible that this dynamics is not smooth, that it owes its reversibility to compensation between two antagonistic irreversible dynamics. This suggests that the motivation for Hamiltonian dynamics may perhaps be found in quantum mechanics; namely observe that the harmonic oscillator flow, associated with the Hamiltonian $H = p^2 + q^2$ is also the *gradient* of the function $V = pq$ with respect to the hyperbolic metric $ds^2 = dq^2 - dp^2$. This can be viewed as the result of a conflict between two antagonistic dynamics supported on the p, q axes respectively (or as in a zero-sum game between two players). If such a view is correct, only very few of the possible Hamiltonian systems could be "quantized", namely those for which H is the square of the modulus of a holomorphic function

of the complex variables $z_j = q_j + ip_j$. Only those could be considered as "natural". In some sense, the lack of structural stability exhibited by the physical laws is nothing but the expression of constraints, namely those associated with the global conservation of space-time, despite the ceaseless changes in space due to local interactions.

The quantitative aspects of catastrophe theory. Because of the relation of E.C.T. to Hamilton–Jacobi theory, it is obvious that some physical phenomena are amenable to the catastrophic schema. I quoted earlier the caustics of geometric optics. Some partial differential equations lead directly to the same E.C.T. scheme; Riemann's equation $u_t = f(u)_x$ has solutions given by the E.C.T. schema as shown by Peter Lax much before catastrophe theory existed. But generalizations of such results require caution. As soon as the underlying substrate has local symmetry properties, then the theory has to be changed accordingly in order to take account of the constraints due to this symmetry. The phenomena of phase (and phase transitions) are not amenable directly to catastrophe theory. One could say that E.C.T. describes the behavior of a sort of *ether*, a *materia prima* without any specific property. For that reason, on any medium E.C.T. still keeps some validity (at a sort of global, qualitative viewpoint). But the precise quantitative laws will in general not exist. Typical examples of that are the critical phenomena, where E.C.T. is nothing but the classical Landau (or mean field) theory, theory which leads to incorrect characteristic exponents. Here again, this failure of E.C.T. is due to the fact that the thermodynamic functions have to satisfy—precisely because of their statistical definition—subtle requirements when they are considered as functions defined on spatial coordinates. They are always very near harmonic functions, as they have to damp their variations through diffusion.

In fact, the physical applications of C.T. are not very important, because precisely on a physical substrate, the evolution of which is very accurately described by physical laws, the presence of catastrophes may be immediately deduced from the equations. Hence C.T. itself is dispensable.

It is important to know now to what extent the local polynomial models of E.C.T. may be fit into empirical data. It is very tempting to do so on a precise quantitative basis. Of course such a fit may be done; it has already been done in biochemistry (protein denaturation, Kozak), and also for some sociological phenomena (modeling of prison riots) by E. C. Zeeman and his coworkers. I must confess that such attempts of applying quantitatively E.C.T. (particularly on a very "soft" substrate such as sociology) do not seem to me very reliable. I have some "a priori" objections towards giving an explicit quantitative expression for the unfolding parameters of a catastrophe. My objection stems from the fact that the homogeneity requirement is in general not satisfied for the modeled phenomenon; hence it will be preposterous to expect for the u functions explicit homogenous expressions, needed if the phenomenon is invariant with respect to space dilatations. And if we do not have such a homogeneity property, then the u functions have ad hoc expressions which will depend in an arbitrary way on the units used to measure the magnitudes entering in the formula.

Now it will probably be excessive to deny the possibility of applying a quantitative fit of a catastrophe scheme to empirical data. But such a procedure

may not be more (nor less) trusted than any other approximation procedure applied in numerical analysis (such as the interpolation of a continuous function by a polynomial by the least square method) and its validity should be confirmed in each specific case; and obviously, we cannot expect such approximating devices to throw any light on the mechanisms underlying the studied phenomena. The ad hoc—and probably illusory—character of such technics is particularly obvious when the underlying coordinates describe psychological feelings such as aggressiveness or frustration, for which no obvious way of measurement can be found.

Nevertheless, we cannot dismiss the possibility that a fine quantitative study—on a relatively "hard" substrate, close to physics—may help to decide between several catastrophe models. Very frequently, there is no uniqueness of the generating catastrophe, although up to now no example is known of such a study. A precise quantitative study of such a case may help to make a choice between models, and decide for a further qualitative modeling. This may be true also for interpreting statistical data, as explained below.

It is my conviction that the domain amenable to quantitative analysis has been—in recent years—grossly exaggerated (the interests of the computer industry are perhaps not entirely foreign to this state of affairs). Even for a problem of physical nature, the physical laws give you constraints which have to be satisfied, but do not suffice in general to determine the evolution of the system. For if we submit any physical medium in a domain D to boundary conditions, then it will be false in general that its further evolution will everywhere satisfy a global partial differential equation (E). What happens—in general—is that there will be an open dense subdomain (D^0) where the equation (E) is satisfied, the remaining complementary set $(D - D^0)$ will be a set of "catastrophe" points, otherwise stated, "defects" of the structure; this set will contain "regular" points forming an open set D^1, where some system of P.D.E.'s (E_1) associated with (E) will have to be satisfied; and then on $\overline{D^{-1}} - D^1$, defects of the defects, some other system (E_2) will have to be satisfied, etc. Up to now, no algorithm exists to determine this sequence (this stratification) of defects, nor the associated sequence of P.D.E.'s. This is why we can sensibly believe that a complete qualitative classification of "defects" will have to be found before a satisfactory quantitative approach may be realized. (Of course, it is always possible in concrete cases to rely on a statistical analysis, but then we can never be absolutely sure that a global failure (catastrophe!) of our given system will not happen.)

The qualitative analysis. Catastrophe theory is not a mathematical theory; it is also not a "scientific theory", as we have seen that by itself it cannot provide any clue to external reality. Nevertheless, it claims to have something to say about phenomena. The statements that C.T. allows one to produce are of the following nature: "If, in the interval of time (t_0, t_1), the system exhibited some morphology (M_0^1), then *one has to expect* that in a further interval (t_1, t_2) it will exhibit some morphology (M_1^2)."

Such a statement can never be considered as an absolutely certain prediction, such as the ones derived from physical laws. The future morphology (M_1^2) derives from (M_0^1) by an hypothesis about the simplicity of the underlying dynamics. If the prediction is realized, then there is nothing to be surprised about. If the prediction

fails (that may happen) and a morphology M_{12} different from M_1^2 does appear, this is interesting, because it shows that our original assumptions were too simple, and some new element of complication has to be introduced into the picture. Paradoxically, one could say that C.T. is more interesting when it fails than when it is successful. This type of "qualitative" reasoning, which allows one to reconstruct piecewise the underlying dynamic of a system is basically an "interpretative" theory (an hermeneutic theory, pedantically). To my view, this is the most interesting element brought in by C.T. Whether such an approach—which by itself does not lead to prediction—may be termed scientific or merely "philosophical", is a matter of definition. It is certainly scientific by the techniques used—if not by its practical results.

Although no clear-cut example of this procedure is known up to now, one may hope that C.T. may be very useful in statistics. For it is fair to say that statistics presently is very far from a complete theory. What finally is statistics about? If we submit a system to a series of experiments, we end finally with a cloud of points in the Euclidean space U of observables. Statistics is nothing but the interpretation of clouds of points. Now the most obvious approach to interpret such a cloud in U will be to construct a space M of hidden parameters, and then in the product $M \times U$, to form a deterministic (ergodic) dynamical system Γ such that the projection on U of an invariant equiprobable distribution on $\Gamma \subset M \times U$ may generate the given distribution of points in U. Strangely enough, it does not seem that statistics did conceive its task that way. Usually statistics oscillates between two viewpoints: either the given cloud should be concentrated along a submanifold of U, thus describing a system of quantitative laws governing the observables, or the cloud should be concentrated in a central point, and noise only explains the deviation from this center. But what happens if the cloud exhibits some clear-cut morphology, such as boundary lines, corners, or triple points? Statistics is completely hopeless in front of these situations, and the specialist has to devise "ad hoc" explanations for these singularities of shape of the distribution. Catastrophe theory which knows how the projection of critical values looks generically, may interpret these accidents as singularities of a projection from $M \times U \to U$, and may devise the simplest possible model (M) accounting for these singularities. For instance, if in a plane $\mathbb{R}^2$ a cloud of points exhibit a sharp border line: with increase of the density towards the border (Fig. 2) instead of a diffuse pattern on both sides (Fig. 3), then it will be more natural to define the cloud of case 1 as coming from the projection of a "fold", whereas such an interpretation would be irrelevant in case 2. Moreover, the interpretation of data in a high dimensional space U may be made difficult by the fact that points are given by their coordinates as a table of real numbers; hence the morphology of the cloud may well be very difficult to recognize.

The role of the statistician—in front of these data—is fundamentally the same as the role of the diviner in the primitive societies. He has to find the reasons for such and such surprising phenomena. Hence his "interpretative", "hermeneutic" function. Now such a task may always be considered also in a game theoretic framework. In any game, it is of fundamental importance for a player to guess the strategy of his partner. The hermeneutic interpretation may be regarded as an attempt to play against a malevolent devil who wants to hide from you his trump
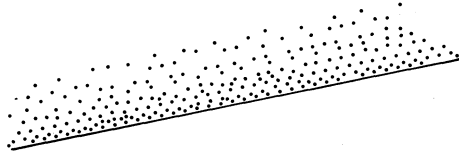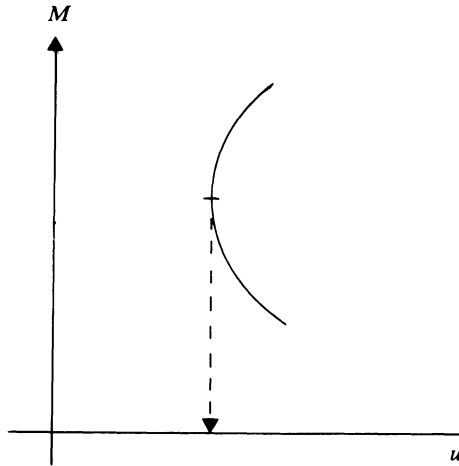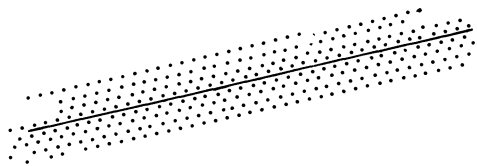
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Fig. 2A.

M

u

Fig 2B. *Fold associated with Fig. 2*

Fig. 3

cards, his strategy, and his rules of behavior. Now the usual probabilistic approach with his emphasis on standard distributions (Gauss, Poisson laws), assumes that the devil only has a very rudimentary brain, such as the drunkard's behavior which generates Brownian motion. A complete statistical theory will necessarily call for intermediate situations between a strict determinism, and a completely incoherent behavior. Catastrophe theory, with its fundamentally morphological approach may be very useful in trying to cope with such situations, where our devil has a more coherent behavior, a global finality for instance, eventually mixed with noise.

But perhaps one of the most interesting aspects of C.T. is its indifference with respect to the properties of the underlying substrate. It may give quite a lot of insight without involving the need of a specific information on the substrate itself.

In that respect, catastrophe theory may seem very arrogant to specialists, who have taken great pains to acquire a rich and detailed information on this substrate itself. This is perhaps a reason why C.T. did make so little progress among biologists, where nevertheless its prospects look among the most promising. I want to give here another example, borrowed from geology.

As an example of this type of morphological analysis, let me describe here the "tectonics of plates" in geology from the catastrophe theory point of view. Suppose that on the surfaces S^2 of the earth, there exists a velocity flow which sometimes may be multivalued; each determination being associated with a minimum of a potential function $V(x; u)$ as in the E.C.T. scheme. Let us admit first the validity of a Maxwell's rule: in each point u, the local minimum (hence the local velocity) is the vector x which gives the absolute minimum of $V(x, u)$. Then, "generically", the set of catastrophe points where two velocity flows collide will be a graph having as only singularities free end points (Fig. 4), boundary lines (Fig. 5), and triple-points (Fig. 6). Moreover there will be basically two types of boundary lines: after subtraction of the average velocity, we get either converging lines (Fig. 7), or diverging lines (Fig. 8). The first one gives rise to orogenesis, as the compressed material tends to be lifted upwards in the third dimension (height).
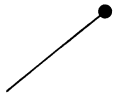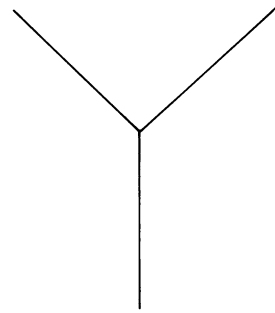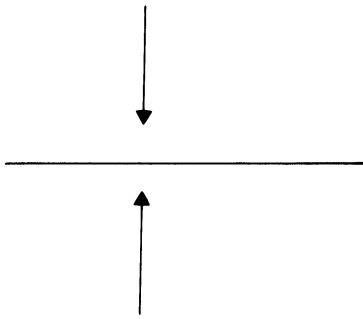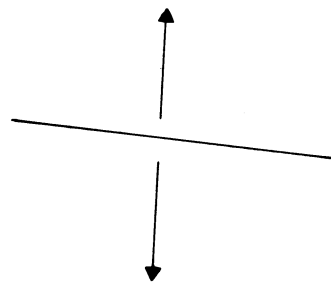

FIG. 4   FIG. 5

FIG. 6

FIG. 7

FIG. 8

The second gives rise to "rifts", deep furrows in the earth's crust. This very simple interpretation already explains quite a lot of the earth morphology; in particular, it settles by the negative the tricky question—still debated among specialists— whether plates have to be considered as "solid". As soon as one gets a rift ending

onto a free end (like the southern extremity of the African rift), there is no point in believing that plates may be undeformable.

If we look at the situation more closely, we will find out that a situation like that in Fig. 9 where the shock line is not orthogonal to the compression (or dilatation) direction is obviously unstable. The only way to make it stable is to get
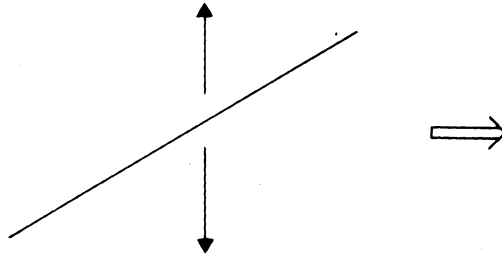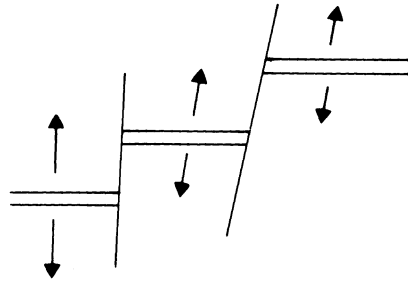

FIG. 9

FIG. 10

to a broken set of segments perpendicular to the compression (or dilatation) direction joined by fault lines along which the two flows slip along each other. Such a stepwise structure is found precisely along the Mid-Atlantic Ocean ridge. Such slip discontinuities are found in fluid dynamics (Mach reflexion), and they occur each time velocity plays a fundamental role in the local state. This example clearly shows what can be expected from the use of C.T. (and in particular E.C.T.). A global understanding of the morphology, subject to local refinements taking into account the precise particularities of the medium. A recent result of V. Kléman and G. Toulouse (two Orsay physicists) may go a long way towards the classification of structurally stable defects. If a medium has local symmetries (described by a pseudogroup (G)), then the function $V(x, u)$ reaches its minimum in x along an orbit (homogeneous space) H under the G isotropy group. Hence E.C.T. is no longer valid, but the stable defects are submanifolds W of codim K such that the link σ of W, which is a $(K-1)$ sphere, is mapped on the local minimum orbit H by a map $\sigma \to H$ which is not homotopic to zero (in general a fibration). This principle provides a very powerful way to classify stable defects in ordered media—liquid crystals, superfluid helium, etc. It shows the profound relation between stability and transversality.

Conclusion. Perhaps the most interesting feature of C.T. is its ability to describe analogies. In some sense, C.T. could be called the theory of analogies; by associating with a local situation a singularity of the local dynamics, it provides an algebraic way of formalizing the intuitive notion of analogous situations. Even the notion of "program" may be to some extent put into a purely geometric framework. This theory is admittedly incomplete, it deals only with "actions" described grammatically by verbs. Analogies dealing with nouns are far more difficult to analyze: they would imply a general theory of *regulation* for systems: external objects and concepts referring to them. Of course, the problem of regulation, which was the main aim of N. Wiener's *Cybernetics*, is still very far out of sight. But as the catastrophe theoretical approach does not exclude the possibility of considering the temporal evolution of a structure (and of its regulation itself), it provides a better way to approach the problem of formalizing life dynamics: for the purely technological approach of cybernetics does not allow such genetic considerations: a machine has no embryology. At least catastrophe theory does not preclude such an attempt. The study of analogy did not progress since Aristotelian logics, as the Boolean approach, with its purely extensional way of defining logic, has turned its back to a true synthetic logic allowing the consideration of meaning. It is there, on this very philosophical ground that I see the main interest of catastrophe theory. If at the end of the 17th century, somebody would have come, exhibiting Taylor's expansion theorem, and saying that such a theorem would help science in finding new phenomena, such a claim would have appeared ridiculous. But nevertheless, Taylor's expansion formula is a highly useful theorem, even if, in itself, it never did lead to any specific experimental discovery. I am inclined to believe that the status of C.T. is of the same nature (if not of the same importance).