CHAPTER VI: QUALITATIVE DYNAMICS

Au moment où tant de savants calculent de par le monde, n'est-il pas souhaitable que d'aucuns, qui le peuvent, rêvent?
—René Thom.[1]

The next great era of awakening of human intellect may well produce a method of understanding the qualitative content of equations. Today we cannot see that the water flow equations contains such things as the barber pole structure of turbulence that one sees between rotating cylinders. Today one cannot see whether Schrödinger's equation contains frogs, musical composers, or morality—or whether it does not. We cannot say whether something beyond it like God is needed, or not. And do we can all hold strong opinions either way.
—R. Feynman.[2]

1. INTRODUCTION: THE MODELING PRACTICE, OR PRACTICES, OF 'APPLIED TOPOLOGISTS'

At the General Assembly of the contributing members of the Institut des hautes études scientifiques, on December 10, 1969, Director Léon Motchane proudly emphasized that Professor René Thom had started fulfilling the hope that he, Motchane, had placed on him when he hired him. Thom was in the process of building his own research school:

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[1] "At a time when so many scholars in the world are calculating, is it not desirable that some, who can, dream?" R. Thom, *SSM,* 325.
[2] R. Feynman, R. Leighton, and M. Sands, *The Feynman Lectures in Physics,* 2 (Reading: Addison-Wesley, 1964): 41-12; quoted by R. H. Abraham, "Introduction to Morphology;" repr. *On Morphodynamics: Selected Papers by Ralph Abraham on Models for Pattern Formation Processes, Morphogenesis, and Self-Organizing Systems Showing the Evolution of the Complex Dynamical System Concept over a Fifteen Year Period,* "The Science Frontier Express Series" (Santa Cruz: Aerial, 1985), 9.

> Under the influence of Thom among others, a progressive *conversion* of topologists is being felt—a conversion toward the study of *stable* structures, forgoing purely topological themes. This problem is not new (think of Poincaré), but it has until now been treated like a poor relative, being often considered as belonging to the domain of engineers. But these problems are very close from those dealing with the stability of dynamical systems (Poinsot, Poincaré, Birchoff [*sic*]). Thus, besides Grothendieck's school of geometric algebra and all the ramified algebraic *works* spurred by it, this new tendency emerged, whose *best* representatives, Pugh, Mather, etc. ... come work with us.[3]

Starting January 5, 1970, Thom's seminar took the name of "Séminaire de dynamique qualitative." For the first time since he had joined the IHÉS, the seminar he led was devoted to a topic beyond purely mathematical themes.[4] For this seminar, he secured the collaboration of Steve Smale and his best students from Berkeley: Michael Shub, Charles Pugh, Robert Williams, John Guckenheimer, and Nancy Koppel. Other participants included David Chillingworth from University of Warwick, at Coventry, England (from the Mathematical Institute headed by E. C. Zeeman, which also came to Bures that year), and the young Floris Takens from Amsterdam.[5] Since the quality of this seminar was exceptional, it attracted other permanent professors of the IHÉS, such as Alexander Grothendieck and David Ruelle.

That year, Ruelle and Takens jointly wrote their famous paper on the nature of turbulence, which sparkled later studies on the chaotic behavior of dynamical systems in

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[3] Léon Motchane, *Notes pour le rapport scientifique 1968* (10/12/69), 2. Arch. IHÉS. Original emphasis.

[4] Thom's previous seminars were titled: "Structure des ensembles analytiques et des applications différentiables," with Bernard Malgrange (starting 13/11/64); "La stabilité structurelle des applications différentielles" (starting 8/11/65); "Propriétés tangentielles des ensembles analytiques" (starting 3/10/66); "Singularités des applications différentiables et des champs de vecteurs" (starting 9/1/67); "Analyse différentielle" (starting 6/11/67); and others on differential geometry and cobordism theory in 1968-1969. *Rapports scientifiques,* 1964-1969. Arch. IHÉS.

[5] *Rapport scientifique 1970* (2/6/71). Arch. IHÉS.

physics. That year, while at the IHÉS, Smale wrote articles where he first articulated his views on the possible uses in other fields of topological results of his dynamical systems theory. Around the same time, Ralph Abraham also visited the IHÉS and developed his own understanding of catastrophe theory. So did Christopher Zeeman.

For the first time, coalesced a new form of modeling practice of which Thom's book *Structural Stability and Morphogenesis* became the manifesto. While the mathematical developments that led to the emergence of this modeling practice partly stemmed from elsewhere, in particular Lefschetz's group and above all Smale's school, the impetus to turn these mathematical ideas into proficient tools for the modelization of natural phenomena came from the IHÉS at Bures-sur-Yvette.

Therefore, the modeling practices of those I call "applied topologists" bore the mark, not only of the discipline it came from (differential topology), not only of its main initiator (René Thom), but also of the institute where it was articulated (the IHÉS). In particular, since for financial and ideological reasons, the IHÉS could not allow incursions in the experimental domain, the idea that models had to be experimentally tested hardly surfaced at the time. As we saw in Chapter III, Thom therefore strongly argued for the total independence of his models from experimentation. Moreover, conforming with the ideology of fundamental research promoted by the IHÉS, the applied topologists' modeling practice was articulated at a highly abstract level, with the feeling that concrete applications had to be developed by "interpreters" coming from various disciplines. Throughout this period, the IHÉS remained, as an institution, focused on what it called fundamental research, i.e. pure mathematics and theoretical physics.

Most importantly, the IHÉS provided a meeting ground for, on the one hand, applied topologists and, on the other, physicists whose education and research programs were mathematically speaking sophisticated enough. A key figure in this rapprochement, David Ruelle was among the permanent faculty of the theoretical physics section of the IHÉS. Only this institution, perhaps, could have played this role. In any case, it played it magnificently for many reasons. It was small enough so that people met across disciplinary boundaries; it was prestigious enough to attract leaders in their fields; and, admittedly without much concrete effect until then, an ideology of cooperation between mathematicians and theoretical physicists permeated the discourses of its upholders.

Soon enough, however, some divergence started to be felt among the promoters of these new practices. By the second half of the 1970s, Thom, Smale, Zeeman, and Ruelle had all gone in diverging directions. Leaving mathematics and the building of particular models to others, Thom grew ever more concerned with philosophy. With some success, Smale catered his highly mathematical approach to mathematical economists who, like Gérard Debreu, had had a Bourbakist education. Very actively developing models inspired by catastrophe theory and insuring their wide promotion, Zeeman saw his approach the more harshly criticized. Developing models for turbulence, Ruelle followed an approach which, by far, was the most fruitful in terms of the consequences it had in provoking the emergence of chaos. Ruelle's work will be addressed in detail in Chapter VII. For the time being, I tell the story of how topologists, who all often visited Bures, became modelers and then disagreed on what it meant for a topologist to model natural phenomena.

2. THOM'S PROGRAM: THE EARLY YEARS, 1964-1966

Research schools in the experimental sciences have proved a useful tool for historians to think about the roles of institutions in bringing about scientific change and the emergence of new specialties.[6] To speak of a research school in mathematics is less obvious for the main reason that, at first glance, no focus on simple and rapidly exploitable experimental techniques seems possible. But, as this dissertation amply shows, the practices of mathematicians can also provide practitioners with techniques and tacit knowledge that can be learned through sustained interaction. In this chapter, I explore the possibility of thinking of Thom's school as a research school in Geison's sense.

Moreover, with the parallel development of groups working on global dynamics and catastrophe theory at Berkeley around Smale and at Warwick around Zeeman, the concept of a research network as developed by Teresa Hopper may apply.[7] Of course, no material (like in her case radium) could be exchanged in the network, but students and mathematical techniques could. The sustained interaction, not only among the leaders of these different schools, but also their students who for the most part toured these sites supports this hypothesis. But before thinking about building such school and network, Thom had to come to terms with his new, original setting where he started working in October 1963.

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[6] See in particular G. L. Geison, "Scientific Change, Emerging Specialties, and Research Schools," *History of Science,* 19 (1981): 20-40; and "Research Schools and New Directions in the Historiography of Science," *Research Schools: Historical Reappraisals, Osiris,* 8, ed. G. L. Geison and F. L. Holmes: 227-238.

[7] T. Hopper, *Science at the Boundary: Discipline Building and Community Making in Radioactivity and Early Nuclear Science, 1919-1939,* Ph.D. Thesis (Princeton University, 1997).

a) Settling in at the IHÉS

In January 1964, René Thom went to Bombay for an international colloquium on differential topology, where he talked with mathematicians John Milnor, Steve Smale, and Jürgen Möser, all of whom he of course already knew and had several occasions to meet.[8] Already in December 1957, after he had had a chance to discuss with Smale while at Chicago, Thom presented Smale's Ph.D. thesis at the *Séminaire Bourbaki.*[9] Similarly, Thom had just presented Möser's work on KAM-theory in the same setting.[10] The Bombay Colloquium was the first important conference Thom attended since he had joined the IHÉS. Director Léon Motchane specifically asked him to invite Smale and Milnor to Bures.[11] At a meeting of the Scientific Committee in October, invitations were indeed sent to Möser and Smale, while Thom still "dream[t] of a differential geometer."[12]

This was the first step in the direction of establishing close contacts between Smale's Berkeley school and the IHÉS. Strangely, Thom hardly seemed to have been the main artisan for the weaving of this link. The impetus more or less came from the Institute itself which, always on the look for the best mathematicians, could not help be enticed by men of Smale's or Möser's stature. In 1962, Smale's name had already appeared on Grothendieck's permanent list of correspondents, and as a handwritten

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[8] See *Differential Analysis: Papers Presented at the Bombay Colloquium, 1964,* Tata Institute of Fundamental Research (Oxford: Oxford University Press, 1964).
[9] R. Thom, "La classification des immersions (selon Smale)," *Séminaire Bourbaki,* 10 (December 1957), exposé #157.
[10] R. Thom, "Travaux de Möser sur la stabilité des mouvements périodiques," *Séminaire Bourbaki,* 16 (December 1963), exposé #264.
[11] Lettres de R. Thom à L. Motchane (6/1/64); de Motchane à Thom (15/1/64). Arch. IHÉS.
[12] Calabi, Chern, or Spencer. *Comité scientifique* (1/10/64). Arch IHÉS.

addition to a list of invitations proposed by Dieudonné.[13] Among the big mathematical news of those years were KAM-theory in which Möser was involved and, above all, Smale's proof of the Poincaré conjecture.[14]

(i) Singularities Versus Dynamics

For his part, starting in the mid-1950s, René Thom was mainly interested in the search for a generic classification of mappings from $\mathbf{R}^n$ to $\mathbf{R}^p$, which he undertook as a follow-up to Hassler Whitney's work. As I have describe in Chapter III, he was quickly led to focus on these mappings' singularities, and conjectured his famous classification theorem. Already in 1956, he had summarized it in a table "whose exactitude [was] not totally guaranteed," but which would evolve into his list of seven elementary catastrophes.[15]

This research program had led Thom to consider the exact same problem that Smale had called the "topological conjugacy problem" in 1961. As Albert Tucker once said, "much of topology has to do with *description* and *classification.*"[16] Thus, topologists Thom and Smale both defined their problem as the search for a topological classification of mappings. Inspired by the concept of genericity, they looked for an open dense subset (or more accurately a Baire subset) of all diffeomorphisms. But, although both were

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[13] Lettre de Alexander Grothendieck à Annie Rolland (21/2/62); *Liste d'invitations de M. Dieudonné* (30/8/62). Arch. IHÉS.

[14] See S. Smale, "The Story of the Poincaré Conjecture."

[15] R. Thom, "Les singularités des applications différentiables," *Séminaire Bourbaki,* 8 (May 1956), exposé #134; repr. *Séminaire N. Bourbaki, Volume 3, Années 1954/55, 1955/56, Exposés 101-136* (Paris: SMF, 1995): 357-369, 363-364. See also R. Thom, same title, *Annales de l'Institut Fourier de Grenoble,* 6 (1955-1956): 43-87; and "Un lemme sur les applications différentiables," *Boletín de la Sociedad matemática méxicana,* 2nd ser., 1: 59-71.

[16] A. W. Tucker, *History of Mathematics,* Course II-1962, NSF Institute, mimeographed lecture notes by Alvin K. Funderburg, 180. Original emphasis.

specialists in topology, they came to this problem from different angles. Smale attacked the conjugacy problem from the standpoint of qualitative dynamics. Following Peixoto, he focused on structural stability as a candidate for a generic property. Following Birkhoff, he chose to use the classic tools of qualitative dynamics (limit sets and soon nonwandering points), rather than singularities, as a way to characterize diffeomorphisms. Moreover, as we have seen in the previous chapter, Smale approached the problem by trying to *guess* which simple axioms could generate an open dense subset of diffeomorphisms. Instead, Thom chose to attack the more difficult problem of classifying generic singularities. The different tools Thom and Smale used in their attempts at classifying mappings led them to follow different paths.

While Smale developed a general theory of dynamical systems, Thom struggled with difficult technical concepts in order to formalize the intuition he had summarized in his table of singularities. In 1959, as a *Gastprofessor* in Bonn, he taught a course on the singularities of differential mappings, in which he conjectured that "almost all" mappings were topologically equivalent to mappings close to them.[17] Similarly, he conjectured that "almost everywhere," an infinitely differentiable mapping was topologically equivalent to a polynomial.[18] Exhibiting in 1960 a "pathological" example, Thom engaged in "the long

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[17] The maps f and g from Euclidean space E to E' are said to be *topologically equivalent* if there exist homeomorphisms h on E and h' on E' such that $hg=fh'$. If their singularities are preserved under all such homeomorphisms, the maps will be said to be *structurally stable.*

[18] His lecture notes, taken and arranged by Harold I. Levine, "Singularities of Differentiable Mappings," were widely circulated in preprint format. They were later published in the *Proceedings of Liverpool Singularities Symposium I,* ed. C. T. C. Wall (Berlin: Springer, 1971): 1-89. See Thom's introduction, 2-3.

story of the edification of the notion of stratified sets and morphisms."[19] It was his battle with these concepts that led him to wish for a differential geometer to come to the IHÉS.

In a didactic article on the theory of envelops published in 1962, Thom explained what his approach was and where he hoped it led him. Educated by Bourbakis, Thom did not hide his struggle to adapt older geometrical traditions to the dominant attitude. Recent reforms of higher education evacuated from the mathematics curricula the theory of envelops, which dealt with the set defined by the tangents of a curve. This was "quite unfortunate," Thom estimated. We may recall that at around this time he had started to experiment with caustics, which, he contended, only the theory of envelops could account for. Why was it condemned, he asked? Because of it "insufficient, non-rigorous character," he explained.

> For any professor enamored with clarity and precision, the theory of envelops, with its long list of exceptional phenomena: fixed points, singular points, stationary curves, etc., quickly becomes a torture [*tourne rapidement au martyre*]: for nothing insures that all pathological phenomena have effectively been cataloged.

With this paper, Thom's therefore aimed at providing the classic theory of envelops with the "theoretical foundations which have until then been lacking." For this, it had to be put in a more general setting: namely, singularity theory. Despite his protests against educational reforms, Thom's enterprise was a singularly Bourbakist one. Using

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[19] R. Thom, "Problèmes rencontrés dans mon parcours mathématique: un bilan," *Publication mathématiques de l'IHÉS,* 70 (1989): 199-214, 201; see also "Notice académique (1976)," Thom Arch. Thom's example was a family of mappings, parametrized by a real number k, whose topological features changed continuously with the parameter k: "La stabilité topologique des applications polynomiales," *L'Enseignement mathématique,* 8 (1962): 24-33. He gathered his results on stratification in "Ensembles et morphismes stratifiés," *Bulletin of the American Mathematical Society,* 75 (1969): 240-284.

highly technical tools (especially Ehresmann's theory of jets and stratified sets), Thom hoped that "these considerations . . . indicates the way to follow in order to get the theory out of the chaos where it is now struggling."

He distinguished clearly between two types of singularities: "stable" (or "generic") ones, which were unchanged by sufficiently small deformations, and an infinite number of unstable ones, which he assumed were nongeneric. "A sane pedagogy shall limit itself to the description of stable singularities, which are (*as far as we may guess*) in finite number." There lay the essential assumptions that made Thom's research agenda possible: stable singularities were generic and in finite number.[20] One may be struck, once again, by its similarity to Smale's convictions.

(ii) Malgrange's Preparation Theorem

During his first years at the IHÉS, Thom saw his agenda dominated by these difficult problems which, as he described John Mather's closely related work, "demanded an almost universal competence: a good topological vision, deep knowledge of analysis, a never-failing algebraic technique."[21] He did succeed in gathering a rather thriving group focusing on these problems, but soon his main interests would lead him elsewhere.

Indeed, in his Scientific Report for 1964, Motchane mentioned an "École Thom" working on the structure of analytic sets, but with few activities compared with Grothendieck's school of algebraic topology which was then flourishing. The group of

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[20] R. Thom, "Sur la théorie des enveloppes," *Journal de mathématiques pures et appliquées,* 9th ser., 41 (1962): 177-192. All quotes above are from pp. 177-178. My emphasis.

[21] Lettre de recommendation pour John Mather de René Thom à W. T. Martin, Princeton University (17/11/67). Arch. IHÉS.

mathematicians to whom Thom belonged was vaguely termed: "differential geometers and those assimilated to them, and topologists." The next year, Thom's school was demoted to a mere "group" (while Grothendieck's remained a "school"). But it now included as much as 14 mathematicians.[22]

While teaching in Bonn, Thom made a conjecture that proved easier to attack, namely that every differentiable mapping could be exactly approximated by a polynomial. This generalization of Weierstrass's preparation theorem (*Vorbereitungssatz*) was proved by Orsay mathematician Bernard Malgrange.

> I consider my duty to state that one of the main results, 'the preparation theorem for differentiable functions,' was proposed to me as a conjecture by R. Thom, and that he had to make a great effort to overcome my initial scepticism.[23]

More significantly, Thom saw a way to use the preparation theorem in order to prove Whitney's classification of the singularities of maps from the plane to the plane.[24] Starting in November 1964, Thom and Malgrange directed a seminar (Thom's first

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[22] *Éléments de rapport scientifique à l'assemblée* [pour 1964] (10/2/66); *Éléments de rapport scientifique sur l'exercice 1965* (1/12/66). Arch. IHÉS.

[23] B. Malgrange, *Ideals of Differentiable Functions* (Oxford: Oxford University Press, 1966), Introduction. See B. Malgrange, "Le théorème de préparation en géométrie différentiable," *Séminaire Henri Cartan 1962/63. Topologie différentielle.* Exposés #11, 12, 13 and 22 (February 4, 11, 25, and May 20); "The Preparation Theorem for Differentiable Functions," *Differential Analysis, Bombay Colloquium* (Oxford, 1964): 203-208. On the real axis, the preparation theorem answered the following simple question in the affirmative: Let $f(x)$ be a real, infinitely differentiable, function, on an interval; does a polynomial $P(u)$ and a diffeomorphism $u=h(x)$, $h'(x)$ never zero, exists, such that for all x in the interval, we have: $f(x)=P(h(x))$? Thom solved this problem by making a wide usage of sophisticated topological techniques. R. Thom, "L'équivalence d'une fonction différentiable et d'un polynôme," *Topology,* 3, suppl. 2 (1965): 297-307. Manuscript sent to Mr. Phillips (31 May, 1964). Thom Arch. References are: Chlander Davis, Problem 4714, *American Mathematical Monthly,* 64 (1957): 679; and N. Levinson, "The Canonical Form for an Analytical Function of Several Variables at a Critical Point," *Bulletin of the American Mathematical Society,* 66 (1960): 68-69.

[24] See Chapter III above.

seminar at the IHÉS) devoted to the preparation theorem. Sponsored by the military, this seminar dealt with stratification. Thom described it as such:

> the properties of *structural stability* of differential mappings are dealt with and in particular preliminary notions on the local differential properties of analytic sets are given. It essentially is a research seminar.[25]

For the first time, this placed the Institute at the center of essential developments occuring in analysis.

(iii) Dynamics and Structural Stability

On February 12, 1965, René Thom wrote a brief note for the director of the IHÉS. We should invite Vladimir Arnol'd, "a young Russian presently in Paris," it said. At the Institut Henri Poincaré, Arnol'd was giving a series of lectures, devoted to the ergodic properties of "unstable" motions, at the *Séminaire* of Jacques-Louis Lions and Laurent Schwartz. No doubt inspired by his own work on KAM-theory, and his encounter with Smale in 1961, these lectures were essentially published in a seminal book he wrote with André Avez.[26]

According to Thom's student Jean Petitot, the numerous conversations Thom had with Arnol'd during that spring played an important role in shaping what became

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[25] Lettre de Léon Motchane à Pierre Aigrain (8/12/64). My emphasis. *Rapport scientifique 1964;* lettre de Léon Motchane à Lucien Malavard (23/11/64); contract with DRME n° 167/65 approved (2/4/65) for 20,000 F. Notification d'une commande de travaux sur mémoire, DRME: "Organisation de séminaires de physique théorique et de mathématiques" (25/5/65). Arch. IHÉS.

[26] V. I. Arnol'd and A. Avez, *Problèmes ergodiques de la mécanique classique* (Paris: Gauthier-Villars, 1967); *Ergodic Problems of Classical Mechanics* (Reading: Addison-Wesley, 1968).

catastrophe theory.[27] Even while extending Thom's singularity theory, Arnol'd remained ever skeptical of catastrophe theory. On March 7, 1967, having received a copy of Thom's *Structural Stability and Morphogenesis,* he wrote Thom: "I thank you for your biological book, where one finds *one* well-hidden page of good mathematics."[28]

Peixoto's 1964 seminar on structural stability, Thom's seminar with Malgrange, his conversations with Arnol'd all show that by that time structural stability, which Thom did not consider in his early investigations of singularities, was quickly becoming, for him and people around him, an important category of thought. Although stability had already surfaced in various forms, only in 1964 did the phrase 'structural stability' appear in Thom's work. But still, it then came up at the end of an argument, as a property exhibited by generic maps, and not as a guiding tool for his investigation.[29]

Thom's work increasingly addressed dynamics, with which he had already flirted for some time. In 1951 he was given Birkhoff's ergodic theorem as a topic for the second thesis of his *thèse d'État,* traditionally imposed by the faculty.[30] As a consequence, Thom devoted his first exposé at the *Séminaire Bourbaki* to Eberhard Hopf's work on geodesics

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[27] J. Petitot, "René Thom," *Encyclopaedia Universalis, vies et portraits,* 636-638. See V. I. Arnol'd, *Catastrophe Theory* (Berlin: Springer, 1992), x-xi, 26. At the IHÉS, on April 4, 1965, Arnol'd gave a talk titled: "La stabilité structurelle du courant géodésique." Arch. IHÉS.

[28] Lettre de V. I. Arnol'd à René Thom (7/3/67), P.S. de la copie d'une lettre du Président de la Société Mathématique de Moscou à Thom. Arch. IHÉS. My emphasis. For his critique of catastrophe theory, see V. I. Arnol'd, *Catastrophe Theory,* first published in 1979.

[29] R. Thom, "Local properties of Differential Mappings," *Differential Analysis, Bombay Colloquium* (1964): 191-202, 201.

[30] R. Thom, *Espaces fibrés en sphères et carrés de Steenrod,* doctoral thesis, Faculté des sciences, Université de Paris (Paris: Gauthier-Villars, 1952). Jussieu Lib. Defended on October 13, 1951 in front of G. Valiron (pres.), Henri Cartan, and André Lichnérowicz.

on manifolds with negative curvature.[31] This certainly helped him later take notice of Hopf's bifurcation, the rediscovery of which is attributable to Thom's conversations with Arnol'd.[32]

In 1963, Thom was chosen to present Möser's work, also at the "Bourbaki show," to use one of Tits's words.[33] There, he insisted on the following aspect: "for the first time, in the study of a diffeomorphism, the existence of a globally invariant manifold which presents a certain stability vis-à-vis perturbations of this diffeomorphism has been exhibited." This was the kind of problems Smale was interested in, but in the dissipative case as opposed to the conservative one studied by Möser. In Thom's exposé, structural stability came up as an interesting property, but not as a guideline. Moreover, the fact that Thom spoke of the structural stability of a *manifold*, which had no clear definition, indicated that he had not yet studied the concept very extensively.[34]

Thom spent the months of August and September 1965 at Berkeley, where he no doubt familiarized himself with Smale's most recent work on dynamical systems. It probably was through his interaction with Peixoto, Arnol'd, and Smale that he acquired the conviction that structural stability was an essential property for the modeling of nature, but his own previous interests had prepared him to reach this conclusion.

By 1965, the stage was almost set for Thom's theory of morphogenesis. Thom now believed that there was a great task awaiting topologists:

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[31] R. Thom, "Les géodésiques dans les variétés à courbure négative (d'après E. Hopf)," *Séminaire Bourbaki,* 2 (March 1951), exposé #28.
[32] V. I. Arnol'd, *Catastrophe Theory,* 26.
[33] Lettre de Jacques Tits à Annie Rolland (37/10/69). Arch. IHÉS.
[34] R. Thom, "Sur les travaux de Möser," 12.

> Besides classical analysis, which is essentially linear, there is the practically unexplored domain of nonlinear analysis; there the topologist may hope make an even better use of his methods, and perhaps his essential quality, namely the intrinsic vision of things.[35]

Three elements however were still missing for a new modeling practice to coalesce: Thom's assimilation of Zeeman's ideas about modeling; a satisfactory proof of his classification theorem soon to be provided by John N. Mather; and his contact with embryologist Waddington. All of these encounters took place thanks to the IHÉS.

b) Zeeman: Topology for Mathematical Modeling

On July 7, 1965, Warwick mathematician Eric Christopher Zeeman came back elated from his annual visit at the IHÉS. He wrote Motchane: "I think the single most exciting conversation I had was with Thom on his new theory of creods."[36] A most fruitful alliance was born from which catastrophe theory would emerge in the course of the following decade. Lecturing on "Piecewise Linear Transversality," Zeeman was however not counted among Thom's group in Motchane's subsequent *Scientific Report.*[37]

While Thom defined in mathematical terms what he meant by a "catastrophe," it was Zeeman who introduced the catchy phrase of "catastrophe theory."[38] Thom credited

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[35] R. Thom, "Sur les travaux de Smale," 26-27.
[36] Lettre de E. C. Zeeman à Léon Motchane (9/7/65). Arch. IHÉS.
[37] *Eléments de rapport scientifique sur l'exercice 1965* (1/12/66), 4-5. Lettre de E. C. Zeeman à Annie Rolland (13/10/64). Arch. IHÉS.
[38] The term "ensemble de catastrophe" appears for the first time in print in Thom's "Une théorie dynamique de la morphogénèse," *Towards a Theoretical Biology, I. Prologomena,* ed. C. H. Waddington (Edinburgh: Edinburgh University Press, 1968): 152-166; repr. *MMM,* 13-38; and in a rather obscure publication: "A Dynamical Theory for Morphogenesis," *Katada Symposium on Topology* (private edition, February 1967): 1-57. Thom Arch. "According to [Claude-Paul] Bruter, Thom would have found the word [*catastrophe*] in Goethe." J. Largeault, "René Thom et la philosophie de la nature," *Critique,* 36 (1980): 1055-1060, 1058.

Zeeman for using the theory to make sense of arbitrary sets of inputs and outputs, whereas Thom had thought of his morphogenetic theories as arising in four-dimensional spacetime $\mathbf{R}^4$ only.[39] As I mentioned in Chapter III, Zeeman's attempts at utilizing topology in biology predated Thom's. Audaciously he then developed many famous (and infamous) models in physics, politics, sociology, neurology, animal behavior, etc.[40] It was Zeeman who, almost single-handedly, attracted the most attention on catastrophe theory, always careful, however, of acknowledging his debt to Thom.

The archives of the IHÉS shed a new light on the interplay between Thom and Zeeman at the roots of catastrophe theory. Indeed, they show that, far from resulting from his mere reading of an "underground" copy of Thom's manuscript, Zeeman's interest in Thom's ideas emerged through constant personal contacts between the two mathematicians.[41] By the same token, Thom picked up some of his colleague's ideas. Zeeman once explained how he saw this process working out for mathematicians:

> Essentially [mathematics] is created by individuals working alone, but often the creation is inspired by talking. *Informal discussion is the secret;* . . . it is the only medium by which one can convey *intuition* without details. . . . The intuitive idea of a paper, which might take a month to read, can often be explained in 10 minutes

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[39] See Zeeman's popular presentation of "Catastrophe Theory," *Scientific American,* 234(4) (1976): 65-83; original extended version in E. C. Zeeman, *Catastrophe Theory: Selected Papers, 1972-1977* (Reading: Addison-Wesley, 1977): 1-64. See also I. Ekeland, "La théorie des catastrophes," *La Recherche,* 81 (1977): 745-754.

[40] For a compilation of his articles in several disciplines, see E. C. Zeeman, *CT;* Woodcock and Davis present several applications in an accessible manner, *Catastrophe Theory,* 76-145.

[41] R. Thom often mentioned "la carrière souterraine" of *SSM.* See, e.g., "Exposé introductif," *Logos et théorie des catastrophes,* ed. J. Petitot (Geneva: Patiño, 1988): 23-39, 33.

> on a blackboard. This is the secret of gregariousness, and of the success of the Institutes at Princeton and Paris."[42]

In short, it was thanks to these sustained personal contacts which were favored by the IHÉS's structure and setting, that emerged a modeling practice that topologists could call their own.

(i) 'The Perfect Environment Both to Think and to Write'

As early as 1961, Dieudonné invited Zeeman, who had received his Ph.D. in 1954, to come to the IHÉS. Arriving in October 1962, he gave a series of lectures on the "Foundations of Combinatorial Topology," and spent the whole spring semester at Bures-sur-Yvette. Starting an IHÉS tradition, he brought a student with him.[43]

Because of its atmosphere and the possibility to work in a quiet setting, Zeeman very much liked the IHÉS. After he came back from his first visit, he sent his "many thanks" to Annie Rolland, the General Secretary, "for creating the familyness of the Institut." Adding: "we realise (although keep it a secret) how much nicer is Bures than Princeton."[44] Having written eight papers during his stay (including 4 jointly, and 2 on physics), he could not help noticing that it had been a "tremendously rewarding year" for him. "I found the place of Bois-Marie a perfect environment both to think and to write."[45]

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[42] E. C. Zeeman, "How to reverse the brain drain in Maths," *New Scientist* (4 May 1967): 263-264. My emphasis.
[43] The student was W. B. R. Lickorish. *Comité scientifique* (8/7/61); lettres de Jean Dieudonné à E. C. Zeeman (31/5/61); de E. C. Zeeman à Jean Dieudonné (6/6/61); de E. C. Zeeman à Léon Motchane (8/1/62); de Annie Rolland à E. C. Zeeman (23/1/62); de E. C. Zeeman à Jean Dieudonné (26/3/62); de Jean Dieudonné à Léon Motchane (8/4/62); *Rapport scientifique sur l'activité de l'IHÉS en 1962* (30/4/63). Arch. IHÉS.
[44] Lettre de E. C. Zeeman à Annie Rolland (26/9/63). Arch. IHÉS. Original English.
[45] Lettre de E. C. Zeeman à Léon Motchane (26/9/63). Arch. IHÉS. Original English.

During the following years Zeeman became a fixture of the IHÉS, paying at least a month-long visit almost every spring. In 1963, following the reform of the British university system, the University of Warwick was set up at Coventry, where Zeeman accepted a chair. It was, he wrote, "a difficult decision – which I entirely made by instinct rather than by reason." Occupied with the demanding task of building a new department from scratch, he was "afraid of being drowned in administration." Therefore, he welcomed the opportunity the IHÉS offered him to work on mathematics *per se*. The "only solution is that every now & then I shall have to flee to Paris . . . to the peace of le Bois-Marie, in order to recover my wits!"[46]

Zeeman also pleased Motchane, and received from him an offer that he did not want to accept. Motchane invited him to stay for a longer period but Zeeman refused in the short term, because, he said, his image of himself was "primarily that of a teacher."[47] Because of his ability to mix highly abstract topological theories with reflections about other subjects, because he collaborated with physicists such as François Lurçat then at the IHÉS, Zeeman might have been the kind of mathematician Motchane ideally was on the look for.

(ii) Topology of the Brain

Indeed, Zeeman was unusual for pure mathematician. While he was interested in abstract theories, he also thought that they provided him with new tools to think about the world. Throughout the 1960s, Zeeman published several papers on relativity and on what he

[46] Lettre de E. C. Zeeman à Annie Rolland (18/12/63). Arch. IHÉS. Original English.
[47] Lettre de E. C. Zeeman à Léon Motchane (26/9/63). Arch. IHÉS. Original English.

called the "topology of the brain."[48] These papers were characterized by a creative use of mathematics in order to achieve better *explanations* of natural phenomena in biology and physics.

Mathematics, Zeeman believed, exhibited a number of specific characters, which distinguished it from other human endeavors. It was both an art *and* a science. As a science, "mathematics—and by that I mean pure mathematics—is the most original and most creative of all the sciences." As "the unique subject that is independent of humanity and of the universe," mathematics was "inhuman." Its result were universal; its history continuous. As opposed to other sciences, its truth was based on proofs, rather than experiments. But mathematics was also an art, because the only criterion for determining worthwhile subject matters was "*taste*" as opposed to external reality, i.e. "what is there."

> Mathematics has gradually been shaking itself free from the rest of science, and the revolution of the last two decades has witnessed the final escape. It is now recognised that we choose what mathematics to do, not for its usefulness or applicability, but for its 'elegance, intrinsic beauty, profundity, generality, simplicity, depth, subtlety and economy.'[49]

Mathematics, by detaching itself from the real world, strove for absolute, universal truths. And there lay "the outstanding quality of mathematics: . . . the final arbiter of what lives

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[48] E. C. Zeeman, "The Topology of the Brain and Visual Perception," *Topology of 3-Manifolds and Related Topics,* ed. M. K. Fort (Englewood Cliffs: Prentice Hall, 1962): 240-256; "Topology of the Brain," *Conference on Mathematics and Computer Science in Biology and Medecine (MRC, Oxford, July 1964),* (London: Her Majesty's Stationary Office, 1965): 277-311; "Causality Implies the Lorenz Group," *Journal of Mathematical Physics,* 5 (1964): 490-493; "The Topology of Minkowski Space," *Topology,* 6 (1967): 161-170.

[49] E. C. Zeeman, "Les mathématiques et la pensée créatrice." *Sciences et l'enseignement des sciences,* 5(34) (November-December 1964): 11-14. Original English from a typed manuscript "Mathematics and Creative Thinking" (n.d. [around 1962-1963]). Arch. IHÉS. Zeeman was here quoting from M. L. Cartwright, *The Mathematical Mind* (Oxford, 1975).

and dies in mathematics is 'elegance and taste'." Simplicity was the judge for this. "At the same time as complicating itself mathematics is paradoxically simplifying itself . . . in a constant process of self-refinement."

Another paradox was that, in spite of—and indeed because of—these very qualities, mathematics was nonetheless extremely useful for other sciences. Citing the classic examples of quantum mechanics and matrices, of relativity and tensors, as well as the more recent use of number theory for computer science, Zeeman contended that "sooner or later it seems that every branch of mathematics will be used by science." Using an expression later taken up by Thom, the intelligibility of the experimental "chaos," Zeeman claimed, depended on mathematics' very "power to simplify and explain."

This amounted to the standard credo of a Platonist mathematician raised in the Bourbakist age. But Zeeman went further than other mathematicians in seriously trying to work out applications of his theories to other sciences. What he did not say in the article quoted above, but expressed clearly elsewhere was that he thought not only that mathematics could help organize experimental data, but that it moreover offered new insights for some natural phenomena.[50] In his "Topology of the Brain," acknowledged by Thom as an important source of inspiration, Zeeman plainly stated his goals:

> We use mathematics to try and explain the relationship between mind and brain, between memory and anatomy, and between thinking and the electrochemical activity of the cortex. The mathematical tool used is algebraic topology, because

[50] Compare Chevalley's philosophical articles quoted in Chapter II, Ruelle's Bourbakist view of physics in Chapter VII below, and the analysis of Debreu's work in mathematical economics by E. R. Weintraub and P. Mirowski, "The Pure and the Applied: Bourbaki Comes to Mathematical Economics," *Science in Context,* 7 (1994): 245-272.

> this is a branch of mathematics *well adapted to ignore local variations and capture global properties.*[51]

This was quite an original way to envisage the classic problem of emergent properties. As Zeeman explained:

> Up till now most theories of the brain, of thinking and of memory have studied the local behaviour of molecules and neurons by using equations. It was thought that to study the global behaviour we should have to solve all the equations for all the neurons (of which there are 10,000,000,000 in each brain). Not only is this impossible, but misleading because it includes the irrelevant local randomness. The power of algebraic topology was to admit that the global activity depends entirely on the local activity, but yet, at the same time, furnish a technique for capturing only the global activity.[52]

Christopher Zeeman modeled the brain by a cube of 10^{10} dimensions, which he called "the thought cube." Each vertex of the cube corresponded to a neuron which could be in either of two states: 0 or 1. To work with such a formidably complicated construct, recent techniques of topology (homology theory) were called to the rescue. The standard problem Zeeman investigated was: How does the brain perceive a visual image starting from the nervous impulses coming from the retina?

> the message is never reconstructed into a picture, but is dissipated into a 'wave pattern' over the cortex, which bears little resemblance to the original picture." Zeeman's most important suggestion was that "the set of all such patterns has a *global mathematical structure similar to that of the sets of all pictures.*[53]

Within a certain tolerance limit in order that concepts (and pictures) be represented not by points but by small regions of the thought cube, Zeeman introduced an analogy between states of the cube and the perception of pictures. He claimed that an isomorphism existed between pictures and states of the brain, and that this could be

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[51] E. C. Zeeman, "Topology of the Brain," 277. My emphasis.
[52] E. C. Zeeman, "Mathematics and Creative Thinking."
[53] E. C. Zeeman, "Topology of the Brain," 278.

expressed mathematically. Zeeman's underlying assumption was that, granted the mechanism of the mind relied on the electrochemical interactions of neurons, new (mathematical) tools were necessary in order to understand how it worked. The "brew of electrons," as Thom wrote, provided no explanation at all.[54] Zeeman underscored that his model "was based on the well known anatomical structure of the brain."[55] But even while affirming this, he had trouble convincing biologists that his topological model—a cube in 10^{10}-dimensional space—bore any relation to the actual anatomy of the brain.[56] Zeeman's approach was moreover difficult to test in the laboratory.

> The results are expressed in geometrical language, and are *qualitative* rather than *quantitative*. This means that so far the theory to be described in this paper has attempted to *explain* phenomena rather than *predict* the measurements that experiment would obtain.[57]

Here lay a source for Thom's later pronouncement: "To predict is not to explain."[58] But Zeeman did not give up all hopes. He would "like to use the model to make predictions," he confessed, "but so far it had only given qualitative explanations of familiar phenomena."[59]

In brief, we may characterize Zeeman's approach as an exploitation of recent topological tools in order to make sense of the way the brain worked. But it did so without paying attention to the physical and electrochemical processes of neural activity. This disjunction between these processes and his topological model hardly mattered since

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[54] R. Thom, "Topologie et signification," *MMM,* 174.
[55] E. C. Zeeman, "Topology of the Brain," 291.
[56] See Sir Edward Collingwood's comment at the end of E. C. Zeeman, "Topology of the Brain," 292.
[57] E. C. Zeeman, "Topology of the Brain," 277. Original emphasis.
[58] R. Thom, *Prédire n'est pas expliquer* (Paris: Eshel, 1991). This theme was already raised in *SSM,* 5-6.

an isomorphism was postulated between the outer world (i.e. a mathematical model of our perception of it) and the structure of the brain (i.e. a mathematical model of it). As I discussed in Chapter III above, the main lesson about Zeeman's modeling practice later taken up by Thom was that recent advances in topology offered powerful new ways to model mathematically the global behavior of complex systems.

c) Convergence: The Spring of 1966

At the General Assembly of the IHÉS on April 6, 1967, Francis Perrin was intrigued by the title of a lecture given by Thom on May 2: "Topologie comparée de la gastrulation chez les vertébrés," probably a practice for the talk he would give the next summer at the Bellagio conference organized by Waddington. For the first time, he introduced the notion of "catastrophies" (as he then wrote in English). Although it did not show in his Scientific Report, Motchane was enthusiastic about it. He replied Perrin that Thom had written a book to be published by Benjamin [*SSM*], which perhaps set the first stone of the Third Section of the IHÉS.[60] Already on December 1, 1966, at an earlier General Assembly, where Motchane had presented his Scientific Report for 1965 (but which Perrin did not attend), the director had lauded, without mentioning the book, his mathematics professor:

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[59] E. C. Zeeman, "Topology of the Brain," 292.

[60] *Notes de séance* manuscrites de Annie Rolland, *Assemblée générale* (6/4/67); *Eléments de Rapport scientifique sur l'exercice 1966, Année 1966 - Séminaires et conférences* (6/4/67), 2. Arch. IHÉS.

> Thom, a Fields Medalist full of genius, disgusted by topology, did remarkable things in differential geometry [and concerning the] generalization of the stability of trajectories.[61]

With the repute of the IHÉS ever growing, a foremost American algebraicist, Saunders Mac Lane, decided to visit the Institute in the spring of 1966, even though, in Thom's word, "he interested no one" there.[62] A Christmas card he and his wife sent around provides an unusual snapshot of the atmosphere reigning at the IHÉS:

> [It] has a simple but idyllically beautiful setting, coupled with hard and concentrated thinking—an excellent luncheon daily with a lively group of incisive Physicists, plus catastrophies in Mathematical Biology and schemas in Algebraic Geometry.[63]

The months of April and May 1966 had indeed witnessed a singular convergence of people and topics at the IHÉS. With Zeeman present, but still addressing issues of algebraic topology, there were, besides Thom's, two more talks that would have an important impact on the subsequent activities of the Institute. On April 19 to 22, Edinburgh embryologist Conrad H. Waddington visited the IHÉS. In May, French mathematician Bernard Morin, discussed the "Weierstrass-Malgrange Preparation Theorem According to J. Mather." The same week, Steve Smale gave a seminar on

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[61] *Notes de séance* manuscrites de Annie Rolland, *Assemblée générale* (1/12/66). The first version of Thom's book was by then finished: lettre de W. A. Benjamin à Annie Rolland (2/12/66); the revised version would not be ready before the end of May 1967: lettre de René Thom à Miss Shapiro, assistant-editor at Benjamin (14/3/67). Arch. IHÉS.

[62] Note manuscrite de Annie Rolland (25/8/65). Arch. IHÉS.

[63] Carte de Noël de Saunders et Dorothy Mac Lane à Annie Rolland (21/12/66). The Mac Lanes arrived at Bures on March 8, 1966. Note manuscrite de Annie Rolland (10/6/65) and (25/8/65); lettre de Saunders Mac Lane à Annie Rolland (3/4/66). Arch. IHÉS.

"Differentiable Dynamical System."[64] As a result, when he got back to Warwick, Zeeman wrote to Motchane that as usual the Institute had provided him

> an escape from administration and an opportunity to do mathematics again, but I find the Institute a very stimulating environment. I was particularly delighted this time to have sessions with Thom and Waddington and gain deeper understanding of Thom's book.[65]

The invitations envisaged by Thom during the year 1966 reflected his preoccupation: Ralph H. Abraham (who was teaching at Princeton at the time, after having worked with Smale at Columbia); Solomon Lefschetz (who however never came); D. Anosov (whom Thom invited upon caming back from Moscow); and John N. Mather (who had just finished his Ph.D. at Princeton).[66]

3. THE EMERGENCE OF A MODELING PRACTICE, 1966-1970

Between 1966 and 1970, the research conducted around Thom at the IHÉS was mainly directed towards achieving a proof of his old conjecture on singularities. But at the same time, Thom was moving away from purely mathematical concerns. Having written the first version of his book, he worked on developing catastrophe models for embryology and linguistics, which, as we saw in Chapter III, informed the shape of catastrophe theory. Simultaneously, Thom started to advocate it on a wide variety of stages.

Around Thom, some important scientists became interested in his ideas. Smale, Zeeman, Abraham, Ruelle, Grothendieck, and even Motchane recognized in Thom's

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[64] *Eléments de Rapport scientifique sur l'exercice 1966, Année 1966 - Séminaires et conférences* (6/4/67), 2-3. Arch. IHÉS.
[65] Lettre de E. C. Zeeman à Léon Motchane (5/5/66). Arch. IHÉS.
[66] *Petit comité scientifique* (31/5/66); lettre de Ralph Abraham à Léon Motchane (24/10/66); de Léon Motchane à Solomon Lefschetz (2/6/66); carte postale de René Thom

approach ideas they could combine with their own previous theoretical practices in order to come up with new kinds of models. These years witnessed the establishment of an international network of scientists interested in global analysis and qualitative dynamics. Besides the IHÉS, the most important nodes of this network were Smale's group at Berkeley, and soon the Warwick Mathematical Institute headed by Zeeman. It was at Bures, however, that the push away from pure mathematics towards applications seemed the strongest.

a) The Stability of C^∞-Mappings

On November 1, 1966, Gil Hunt wrote to Orsay mathematician Jacques Deny about the Princeton people who might spend a sabbatical year in Paris in 1967-68. Among them was John Mather. "[C]onsidered by Milnor and others to be the best graduate student in several years [he] will receive his doctorate in June and would like to spend a year or two in Paris. Malgrange already heard of him."[67] Indeed, following Morin's presentation of his work, Mather was extremely welcome at the IHÉS. Motchane immediately invited him and Mather accepted to come.[68] Arriving at Bures in September 1967, Mather presented the "machinery" he had used for proving the Weierstrass-Malgrange preparation theorem

à Léon Motchane (n.d. between his trips to Moscow and Bellagio); lettre de Léon Motchane à John Mather (5/12/66). Arch. IHÉS.

[67] Lettre de Gil Hunt à Jacques Deny (1/11/66). Arch. IHÉS.

[68] Lettres de Léon Motchane à John Mather (5/12/66); de John Mather à Léon Motchane (31/1/67). Arch. IHÉS.

and suggested that it could be used for the classification of catastrophe. He was followed by Polish mathematician Stanislas Lojasiewicz's description of his own proof.[69]

Spending two years at the IHÉS, Mather wrote a series of technical papers which, not quite entirely avoiding Thom's heavy arsenal of stratified sets, did succeed in providing a satisfactory proof of his classification theorem.[70] As result, Thom wrote on November 17, 1967:

> I have the *highest* regard for John Mather. He entirely solved a problem of a great difficulty (and probably of a great importance): that of the structural stability of differentiable mappings.[71]

b) Consequences of Smale's Counterexample

Meanwhile, in the spring of 1967, previous collaborators of Smale's, important for the subsequent history of 'applied topology', lectured at the IHÉS in the *Séminaire Thom*. In particular, came Ralph Abraham (from Princeton) who talked about Anosov flows, and Nicolaas H. Kuiper. Most important was the semester-long visit of Charles Pugh (from Berkeley). He gave several talks at the Institute, and on February 27, 1967, introduced Smale's counterexample. I do not know if Ruelle attended Pugh's lectures. They predated his initial interest in dynamical systems, and while he did emphasize some of Thom's and Smale's lectures, Ruelle never mentioned Pugh's. As will be made clear in Chapter VII,

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[69] *Elements de rapport scientifique* (8/5/68), *Année 1967 - Séminaires et conférences,* 6. Arch. IHÉS. Mather used the term "machinery" in "On the Preparation Theorem of Malgrange," Princeton University preprint (April 1966).

[70] J. N. Mather, "Structural Stability of Mappings," Princeton University preprint (November 1966), both in Fine Arch.; "Stability of C^∞ Mappings," Parts I and II, *Annals of Mathematics,* 87 (1968): 89-104; 89 (1969): 254-291; Parts III and IV, *Publications mathématiques de l'IHÉS,* 35 (1968): 127-156; 37 (1969), 223-248; Part V, *Advances in Mathematics,* 4 (1970): 301-336; and Part VI, *Proceedings of the Liverpool Singularities Symposium,* Lecture Notes in Mathematics no. 192 (Berlin: Springer, 1971): 207-253.

Ruelle crucially depended on Smale's counterexample as a source for his later concerns with fluid dynamics.

Logically, Smale's counterexample—showing that that structurally stable systems were not generic—should have stricken a deathblow to Thom's hopes as put forward in *SSM*. "This result," Thom later acknowledged, "was a kind of catastrophe for my view of things."[72] He nevertheless chose to push forward for reasons he mentioned at the Moscow Congress:

> The very notion of structural stability however is far from having lost all interest: first, because there exists, in the functional space of vector fields on a manifold, a 'relatively important' generic open set of vector fields of gradient type (without recurrence) and, probably, a class of fields, defined by Smale (the so-called Morse-Smale vector fields) which exhibit recurrence (with closed trajectories) but under a benign, severely controlled form."[73]

Thom still believed that structural stability could serve as a useful guideline for his modeling practice. One reason, as he explained above, was that he emphasized gradient systems, conjectured by Max Delbrück to be at the roots of cell differentiation, which, generically, were structurally stable. This explains why much of catastrophe theory focused on elementary catastrophes. This also became a major weakness of Thom's philosophy since, clearly, not every system was a gradient system. "As we are far from understanding" generalized catastrophes, Thom acknowledged, "the models that will be presented . . . are bound to be imprecise."[74] Because of this mathematical dead end, Thom moved away from a classification of models based solely on topological premises back to philosophy.

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[71] Lettre de recommendation de René Thom à W. T. Martin (17/11/67). Arch. IHÉS.
[72] R. Thom, "Exposé introductif," 32.
[73] R. Thom, "Sur les travaux de Stephen Smale," 27.

In 1967, after a visit to the IHÉS, Abraham wrote the introduction to the lecture notes of a course on mechanics given in the spring of 1966 at Princeton University. He explicated a bit of Thom's philosophy. Using arguments similar to those of Andronov's and Lefschetz's, and citing Pierre Duhem, he explained, that the usefulness of a theory, the adequacy between models and experimental data, hinged on the a criterion of *stability*. "Although this criterion has not been discussed very explicitly by physicists, it has functioned as a tacit assumption, which may be called the *dogma of stability*."[75] Abraham claimed that, by postulating structural stability in a specific theory, Thom offered an alternative to the dogma of stability.

c) May 68 at Bures: 'Le Bois-Marie Never Looses Her Magic'[76]

"It is useless to tell you that Bures was quiet, but you may guess how much this turmoil touched us."[77] Thus did Annie Rolland, the General Secretary of the IHÉS, comment the student revolts and general strikes that shook the country in May 1968. Its financial consequences aside, these hardly seemed to have impacted the Institute.[78] The following months witnessed a striking increase of Zeeman's and Ruelle's interest in Thom's theory.

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[74] R. Thom, *SSM*, 28.
[75] Ralph Abraham and Jerrold E. Marsden, *Foundations of Mechanics: A Mathematical Exposition of Classical Mechanics with an Introduction to the Qualitative Theory of Dynamical Systems and Applications to the Three-Body Problem* (New York: W. A. Benjamin, 1967), 3-4; this introduction is repr. in R. Abraham, *On Morphodynamics*, 1-4. See P. Duhem, *The Aim and Structure of Physical Theory*, transl. P. P. Wiener (Princeton: Princeton University Press, 1954).
[76] Lettre de E. C. Zeeman à Léon Motchane (18/8/68). Arch. IHÉS.
[77] Lettre de Annie Rolland à Jacques et Marie-Jeanne Tits (1/7/68). Arch. IHÉS.
[78] Lettre de Léon Motchane à Edgar Faure, Ministre de l'Education nationale (29/11/68). Arch. IHÉS.

On January 28, René Thom discussed "Qualitative Dynamics and Morphogenesis" in front of a distinguished audience, following the general assembly of the Société mathématique de France (SMF). During the winter term, however, most of talks at Thom's seminar, given by Thom himself, Lojasiewicz, Mather, Malgrange and Jean-Claude Tougeron, were devoted to the stability of differentiable applications and to Thom's theory of stratification.[79]

(i) Zeeman Dives In

In April, Christopher Zeeman visited the Institute for one month, leaving just before the beginning of the strikes. For the first time in years, he did not lecture. He was starting to study Thom's ideas very carefully. In August, he wrote Motchane:

> In particular, I am getting very excited about dynamical systems. The coming year we are having a year-long symposium in the subject at Warwick. . . . The following year I hear there will be much activity in Paris. [While at Bures,] I spent the whole month digging into Thom's ideas on catastrophes which was a particular pleasure, and since then I have lectured on his work in several places, and consequently I am beginning to understand it. I gave a summer course at Aarhus on dynamical systems and am devising some lecture notes giving, I hope, an elementary armchair introduction – I will send you a copy when they are finished to test out in your armchair!

Zeeman then explained how he literally got into the water. Catastrophe theory was starting directly to inform his own modeling practice.

> One facet on Thom's ideas I found very intriguing is the application to breaking waves. To the amusement of the family, I spent several hours of our summer holiday up to my neck in the sea attempting to photograph the waves as they break [!!!]. The shape is quite unlike what I had previously imagined – it is as if a thin jet stream shoots out from the crest, confirming Thom's prediction that it has to do

[79] *Rapport scientifique 1968.* Arch. IHÉS. Note that Thom became presdent of the SMF in March.

> with his hyperbolic umbilic catastrophy [*sic*]. As far as I can discover there has been no satisfactory explanation previously.[80]

In his reply to Zeeman, Motchane revealed that he also had become interested in Thom's ideas about "dynamical systems." He was reading an article by Arnol'd, and was "eager and anxious to read to receive [Zeeman's] elementary arm-chair introduction, since sitting in my arm-chair I can afford all catastrophies."[81]

(ii) The Road to Ruelle's Turbulence

From May 16 to 18, 1968, there was a meeting of mathematicians and physicists at the University of Strasbourg, where David Ruelle spoke on statistical mechanics. Jean Leray, a professor of mathematics at the Collège de France, who in his 1931 doctoral thesis had proposed a mechanism for the onset of turbulence, was also present (Chapter VII). I do not know if Ruelle discussed turbulence with him at the time, but coming back from Strasbourg, Ruelle read Landau and Lifschitz's *Fluid Mechanics.*

In August 1968, Ruelle put the final touch to a book which gathered several years of extremely successful work on the mathematical foundations of statistical mechanics.[82] Feeling done with this, he was looking for another research topic. During the fall semester, 1968, Ruelle visited the University of California, Irvine.[83] When he got there, he wrote to Motchane:

> It seemed to me that the time had come for me to try and do something other than statistical mechanics, and that my stay here was particularly appropriate for such a

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[80] Lettre de E. C. Zeeman à Léon Motchane (18/8/68). Arch. IHÉS. Original English. See E. C. Zeeman, "Breaking of Waves," *Proceedings of the Symposium on Differential Equations and Dynamical Systems,* ed. D. Chillingworth (Berlin: Springer, 1971): 2-6.
[81] Lettre de Léon Motchane à E. C. Zeeman (16/10/68). Arch. IHÉS. Original English.
[82] *Statistical Physics: Rigorous Results* (New York: Benjamin, 1969).
[83] Lettre de David Ruelle à Léon Motchane (5/2/68). Arch. IHÉS.

> change. For the time being, I am therefore trying to look at some problems of hydrodynamics or, more generally, of 'dissipative phenomena' from a physical point of view analogous to Thom's. These phenomena are fascinating, but one does not see very clearly what can be done mathematically. *There is a very good chance that nothing will come out of all this, in which case I would stop there* [je tirerai une ligne], *I'll give a seminar and move on* [j'arrêterai les frais].[84]

This letter of Ruelle's shows not only that he started to think about applying Thom's ideas to fluid mechanics as early as 1968,but also that he considered the general problems of dissipative systems.[85] It moreover shows how uncertain he was about the fruitfulness of this approach.

That physics could benefit from the modeling technology develop around Thom, Smale and Mather was a natural idea to come up with. Indeed, as early as during the summer of 1967, a meeting had been held at the Battelle Research Center at Seattle in order to get mathematicians and physicists to speak to one another. Organized by Cecile DeWitt and John A. Wheeler, both relativity theorists, it gathered many IHÉS visitors. Significantly, on the mathematics side, topology was emphasized, and Thom, Smale, and Mather tried to convince physicists of the importance of their ideas.[86]

Because he saw Ruelle as attacking some of the very issues that had motivated him in founding the IHÉS in 1958, Motchane was delighted about Ruelle's new direction of research. "Your letter greatly pleased me. Specially what you said about your 'change in orientation'." He added his own advice about the problems he might face:

> If you have in mind 'dissipative phenomena' in a very general sense, there do *not exist as yet,* to my knowledge, good tools, nor adequate method to tackle the study of [these] physical phenomena. You fall back on '*the* great problem' presently,

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[84] Lettre de David Ruelle à Léon Motchane (7/10/68). Arch. IHÉS. My emphasis.
[85] D. Ruelle, *Chance and Chaos,* 53.
[86] C. M. DeWitt and J. A. Wheeler, eds., *Battelle Rencontres: 1967 Lectures in Mathematics and Physics* (New York: Benjamin, 1968).

> namely: the search for a mathematical instrument *specifically adapted* to the interpretation of physical phenomena.[87]

The interest of some physicists for Thom's ideas had become clear. In March 1968, Malgrange addressed the IHÉS physicists about the stability of differential mappings, while, two weeks later, Thom spoke of "Resonances and catastrophes."[88] Meanwhile Thom was pursuing his crusade for the popularization of his ideas. In May, he participated to the *journées* on differential analysis at Rennes. In the fall, he gave talks at Orsay and the École polytechnique. Still in 1968, he published his first article on linguistics. At the same time, he was preparing the invitations for the next year, thinking of Palis, Williams, Peixoto, Smale, Shub, Milnor, etc.[89]

(iii) Motchane: Formal Structures of Real World

Besides playing an important institutional role, Léon Motchane must have also contributed to the reflections on the nature of modeling taking place at the IHÉS in the late 1960s. In his yearly *Scientific Report,* he often briefly insisted on the methodological revolution that new Bourbakist conceptions of mathematics brought about. For Motchane, Bourbaki was now allowing the mathematician to meddle with vast areas of the sciences.

> In a large number of cases, a kinship of structures in extremely diverse domains has been noticed. This allows today's mathematicians, without for all this becoming an expert in a branch that is not the object of his study, to understand its essential [features].[90]

The Bourbakist reordering of mathematics which emphasized the structure concept provided mathematicians with tools that would not suddenly make experts in a

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[87] Lettre de Léon Motchane à David Ruelle (16/10/68). Arch. IHÉS. Original emphasis.
[88] *Rapport sur le séminaire de Physique théorique* (31/3/68). Arch. IHÉS.
[89] *Petit comité scientifique* (25/6/68). Arch. IHÉS.

foreign field of them, but which enabled them to grasp the deep structures, the essence, of these other fields. In the best of cases, a dialogue could then be established between the mathematician and the expert.

In the archives of the IHÉS, I discovered a reprint of Motchane's, probably dating from around the same period, that considerably complicated some of Zeeman's reflections above.[91] Plainly, he stated that he "identified *scientific knowledge* and *structure*." Any structure of the (physical) universe U, he argued, was induced by a structure (in Bourbaki's sense) on an appropriate mathematical space K, induced by the inverse image of the elements of F, a set grouping "all of our means of observation in a large sense," i.e. all the functions $f \in F, f: U \rightarrow K$. More concretely, an element f may represent a specific natural law, any of which being a mapping of the world to a mathematical space.

Motchane defined the "domain of scientific knowledge" as being a subset of the universe U determined by the inverse images $f^{-1}(K)$ of some mathematical spaces K. Taking the real line $K=\mathbf{R}$ as an example, Motchane asked: what subset of the universe does it represent? "This domain of the universe," he answered, "will contain physics, certain portions of chemistry and physical chemistry, but also domains belonging to other sciences that could be dealt with by using mathematics, like biology, sociology, economics, etc." This domain of the world therefore was a new concept, which did not exactly recover the "classic ideas of the exact sciences." He suggested to designate it by the outmoded term of *natural philosophy*, also picked up by Thom:

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[90] L. Motchane, "Éléments de Rapports scientifique [1967] à l'Assemblée [générale du 8/5/68]," 4. Arch. IHÉS.

[91] L. Motchane, "Structures formelles du monde réel," reprint (pp. 105-110). Arch. IHÉS. Unfortunately, I have been unable to locate neither its place, nor its date of publication.

> "Natural Philosophy" therefore is the domain of our observations susceptible of being studied, analyzed, endowed with structures, by the mathematical method.

As a radical consequence of this view of things, Motchane claimed, this "classification is done by the means of the method, without being too much concerned with the nature of the phenomena." This method for the modeling of the world's phenomena chopped off the universe in different domains that were defined by the kinship of their mathematical structures alone, rather than by phenomena or substrata. These vague considerations, Motchane believed, shed a new light on the process of scientific thought:

> This process makes clear the fact that the very structure of the world of phenomena—provided that this term had a precise meaning—does not influence the formation of scientific laws at all. More exactly, one should say that the scientist is scarcely concerned with this structure, knowing that he does not have a chance to grasp it [directly].

Motchane's conception of the Bourbakist impact on scientific modeling indicates that he shared similar concerns as Thom's, Ruelle's, and Zeeman's. Moreover, they underscore some of their common assumptions. They held that the ontology of the world was unreachable directly, and that our knowledge of it could be mediated by mathematical models. As a consequence, the modeling practice informed by this belief could not be dictated by the phenomena themselves. The main guideline for the modeling of natural phenomena rather were mathematical objects. The tools of global analysis, structural stability and genericity, by Motchane's inverse mapping process, revealed a whole new domain of the world that now could be modeled. This philosophy explains why, in the late 1960s and early 1970s, 'applied topologists' felt entitled to tackle so many

different subjects without taking the time to become experts in any of them. Mathematics alone dictated what could be studied.

d) Mathematics versus Rhetoric: The Case against Deligne

In April 1968, a first "crisis," in Motchane's word, erupted at the IHÉS. It was triggered by Thom's opposition to the hiring of Pierre Deligne as a permanent professor of the IHÉS. Born in 1944, Deligne was a young Belgian mathematical genius who, for the last two years, had been working on algebraic geometry at Bures in close collaboration with Grothendieck. This episode highlights an aspect of Thom's view of the role of mathematics.

In May 1966, Thom, Motchane, and Grothendieck had agreed to invite Deligne to IHÉS "for 3 or 4 years."[92] Less then two years later, Grothendieck estimated that he had obtained six results "each one of which could constitute the central result of an excellent doctoral thesis."[93] Grothendieck, like Dieudonné when he presented him with his Fields Medal in 1966, lavishly compared him with none less than David Hilbert. "The mathematical center to which he will belong will become a radiating center like Göttingen."[94] Considering that Grothendieck's predominant influence on Deligne might unbalance the Mathematics Section of the IHÉS, Thom was not convinced, but proposed that the IHÉS keep him as a temporary member for still a few years.[95]

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[92] *Petit comité scientifique* (31/5/66). Arch. IHÉS.
[93] *Note sur les travaux de P. Deligne,* par Alexander Grothendieck (mars 1968). Arch. IHÉS.
[94] *Procès-verbal du Comité scientifique du lundi 1er avril 1968* (11/5/69). Arch. IHÉS.
[95] *Notes pour le Rapport scientifique 1968* (10/12/69); lettres de Léon Motchane à Pierre Deligne (20/5/68); de Pierre Deligne à Léon Motchane (22/5/68). Arch. IHÉS.

The following year, on May 11, 1969, after a particularly fruitful year for him, the matter of Deligne's hiring came up once again. But Thom remained skeptical. He was too young; he had never taught, never traveled; he was not ready for this kind of position, Thom demurred. He feared that with Deligne's nomination, he would be in minority.[96] But his main objection remained a matter of mathematical style. Deligne was very gifted, but solely in algebraic geometry.

> What counts is not really what a mathematician finds, but the problems he raises. . . . On the list of [Deligne's] works, there are only three or four [of them] that do not appear to me as pure rhetoric.[97]

Thom's position cannot be separated from the opposition to the contemporary Bourbakist-algebraicist attitude, which he expressed over the following years.[98] For Thom, any question in algebra was "either trivial or impossible to solve."[99] This underscores Thom's belief that mathematics needed to break away from its isolation. Neither a lonely queen living in an ivory tower, nor submissive servant to other sciences, mathematics was the means for achieving new intelligibility of the world.

Eventually Deligne was hired without Thom's consent. "Objectively," Motchane said, his nomination "was imperative."[100] Like Poincaré a "monster of mathematics," to

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[96] "Il y aura deux géomètres algébrique contre un seul Thom [!]." *Notes de séance manuscrites du Comité scientifique* (11/5/69). Arch. IHÉS.
[97] *Notes de séance manuscrites du Comité scientifique* (11/5/69). Arch. IHÉS.
[98] R. Thom, "Les mathématiques modernes: une erreur pédagogique et philosophique?" *L'Âge de la science,* 3(3) (1970): 225-242; repr. *Pourquoi la mathématique?,* ed. R. Jaulin: 57-88; transl. "Modern Mathematics: An Educational or Philosophical Error?" *American Scientist,* 59 (1971): 695-699; and "Mathématiques modernes et mathématique de toujours," talk given at the Mathematical Pedagogy Congress of Exeter (1972); repr. *Pourquoi la mathématique?,* ed. R. Jaulin: 39-56.
[99] R. Thom, "Modern Mathematics," 696.
[100] *Notes pour le Rapport scientifique 1968* (10/12/69). Arch. IHÉS.

use Dieudonné's phrase, "Deligne proved the Weil Conjecture on July 2, 1973."[101] For this, he was awarded the extremely rarely conferred *Henri Poincaré* Medal from the Académie des sciences, and the Fields Medal at the Congress of Mathematicians in 1978. Once again, Motchane's intuition had served the IHÉS well.

e) The Network in Full Swing

In the summer of 1968, a symposium on global analysis placed under Smale's aegis was organized in Berkeley by the American Mathematical Society.[102] The next year, Zeeman's Mathematics Institute at Warwick held a year-long symposium and a summer school devoted to similar topics.[103] These meetings saw the final convergence of people and topics which would form the dynamical systems background for later excitement about catastrophe and chaos theories. As Lawrence Markus explained, the purpose of the Warwick Symposium was twofold:

> i) research – to draw together the leaders in the fields of differentiable dynamics and the more classical parts of the qualitative theory of ordinary differential equations, such as oscillation, stability and control theory, for an extended duration in an atmosphere of active and creative research.
>
> ii) education – to encourage the consolidation of the new developments in differentiable dynamics and its application after a decade of profound but rather frantic and *chaotic* investigation, and to disseminate this information among

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[101] *Communication by Directeur* (Nicolaas Kuiper), *Comité scientifique* (16/11/73); *Rapport sur les travaux de P. Deligne* (médaille Henri Poincaré), par Jean Dieudonné (9/12/74). Arch. IHÉS.

[102] Shiing-Shen Chern and Stephen Smale, eds., *Global Analysis: Proceedings of the Symposium in Pure Mathematics (Berkeley, 1968),* 3 vols. (Providence: American Mathematical Society, 1970).

[103] D. R. J. Chillingworth, ed., *Proceedings of the Symposium on Differential Equations and Dynamical Systems: University of Warwick, September 1968 - August 1969, Summer School, July 15-25, 1969,* Lecture Notes in Mathematics, 206 (Berlin: Springer, 1971).

> mathematicians and scientists of the UK through seminars, instructional courses and schools.[104]

Pushing for greater support from the SRC for the development of differential equation studies, Christopher Zeeman was especially pleased with this:

> The Warwick Symposium and Summer School on differential equations went like a bomb, and may have interesting repercussions on the number of research students opting to go into the field.[105]

The attraction of the IHÉS was being felt especially strongly at this time. In January 1969, Zeeman wrote to Motchane a letter that could only make him proud of his achievement. "Princeton [i.e. the IAS] has been pressing me to go there, but I would much prefer to come to you, especially as I understand that both Smale and Thom will be with you."[106] At least in one field, the pupil had outdone its master. Letters asking to come and work with Thom became more and more frequent. One example was a letter Karl Sigmund, from Vienna, in which he indicated that, waiting for the publication of his book, he was reading Abraham, Smale and Zeeman.[107] In 1969-1970, the topic of Thom's seminar became "Qualitative Dynamics" at large. Collaborators included Smale's students (including Rufus Bowen, his "best student," according to a handwritten note of Rolland), Smale himself, Peixoto, Takens, and Zeeman (who was accompanied by David Fowler,

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[104] L. Markus, "Dynamical Systems – Five Years Later," *Dynamical Systems – Warwick 1974,* ed. A. Manning (Berlin: Springer, 1975): 354-365, 354. My emphasis.
[105] PS d'une lettre de E. C. Zeeman à I. M. Sneddon, Glasgow (19/9/69).
[106] Lettre de E. C. Zeeman à Léon Motchane (3/1/69). Arch. IHÉS.
[107] Lettres de Karl Sigmund à René Thom (21/5/69); de Léon Motchane à Philippe Husson (29/5/69). Arch. IHÉS.

Figure 12: René Thom Lecturing on Catastrophe Theory at the IHÉS in the Early 1970s. Copyright © Arch. IHÉS.

future translator of Thom's *SSM*).[108] For some, it must have felt as if Berkeley had moved to Bures-sur-Yvette!

[108] Lettres de Léon Motchane à R. F. Williams (17/3/69); de Nicolaas Kuiper à René Thom (16/4/69); de Floris Takens à René Thom (27/6/69); de E. C. Zeeman à Annie Rolland (10/7/69); de E. C. Zeeman à Annie Rolland (22/7/69); de Rufus Bowen à Annie Rolland, et à Léon Motchane (5/11/69); Note manuscrite de Annie Rolland (31/10/69). Arch. IHÉS.

4. EXTERNAL SUCCESS AND INTERNAL CRISES, 1969-1972

While "its situation on the scientific level seems to me more brilliant and promising for the future than it ever was," Grothendieck wrote Motchane on January 26, 1970, "the IHÉS goes through a period of crisis of which it is difficult to foresee the outcome."[109] Indeed, financial stability due to the Europeanization of its pool of sponsors was finally in view. But never this young institution had been so close to explode because of internal divisions. The crises that shook the IHÉS in 1970 were to affect the Institute deeply.

Paradoxically, it was at the same time that the attraction of the modeling practices of qualitative dynamics promoted by Thom, exerted the strongest attraction on members of the permanent faculty. Particularly striking was Grothendieck's involvement in biology based on his interest for Thom's ideas. One of the purest Bourbakists was turning to applications. This highlights the exceptional cooperation across disciplines that then characterized the IHÉS. But in order to do full justice to Grothendieck's change of interests, it is necessary to digress somewhat and go into the political issues that underlay his metamorphosis. After he quit the IHÉS, nothing would ever be the same. In the following years however, catastrophe theory had finally matured enough so that Motchane thought he could at last build the Third Section on this basis.

a) First Skirmish

The crisis erupted on October 1, 1969. In a letter to Motchane signed by the four of them, the permanent professors of the IHÉS (Grothendieck, Michel, Ruelle, and Thom) expressed their unease about some rumors that had been floating around. "We have been

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[109] Lettre de Alexander Grothendieck à Léon Motchane (24/1/70). Arch IHÉS.

surprised not to learn from you" of a project for modifying the bylaws of the Institute and of the formal proposal made to Res Jost (theoretical physicist from the ETH, Zürich, and Ruelle's mentor) to become its next director. They demanded that a meeting of the Scientific Committee (CS) be convoked to discuss these matters.

At that time, Motchane was 70 years old, and might have been already looking for his successor for after his retirement, normally four years away. But the big project he was working on was the transformation of the IHÉS in order to be supported by different national research foundations in Europe. Motchane's direction style had always been authoritarian, and he was somewhat taken aback by his professors' reaction. In addition, the information they had might not have been quite exact. On November 3, he replied them: "I did not understand - and I still do not understand - [your] affirmations."[110]

Unfortunately, Grothendieck's impatience had quickly turned this lack of communication into a personal conflict. On October 24, Grothendieck escalated the dispute. Reiterating his demand for a meeting of the CS, he accused Motchane of conspiracy.

> (faithful to your bizarre principle that the most shriveled line is the shortest way between two points) you make phone calls right and left to insure to which extent it would be possible for you to spread confusion [*brouiller les cartes*].

Grothendieck expressed the wish that Motchane saw in the professors "competent, good-willing collaborators in your task of insuring the survival and continuity of the IHÉS,

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[110] Lettre de Léon Motchane à Alexander Grothendieck, cc: Michel, Ruelle et Thom (3/11/69). Arch. IHÉS.

rather than a gallery to circumvent."[111] Motchane could quickly lose his temper, and on this occasion, he did.

> Your letter is of monumental thoughlessness [*un monument d'inconscience*]. How dare you, whose competence outside of mathematics is limited, all judge, give your appreciation on everything? . . . You should meditate on the parable of the straw and the beam.

A touching handwritten addendum, which I ignore whether it was sent, however tried to mend the disagreement.

> My dear Grothendieck,
>
> I would like to had a few words to this letter, and these few words may well be what is essential.
>
> It is true that friendship between men cannot be dictated; it comes and goes through unpredictable affinities. But the converse is also frequent and still more damageable when a current of sympathy is forbidden to be established for fortuitous reasons, because of one's tactlessness, or - worst still - for no reason at all.
>
> Nothing is more painful to me than to feel a hostility of which ignore the cause, and even more so when it thwarts my spontaneous feeling of sympathy. . . . While we may be different, coming from different countries, with a different education, many things, and the most important ones, unite us, for we have the same moral reactions to grave problems. Then, [there is] our shared responsibility in our common endeavor: to safeguard the Institute for the future as a refuge where man's freedom and dignity is respected. To reestablish the climate is all this for me.
>
> Sincerely yours, L.[112]

On November 12, a meeting indeed took place between Motchane and the four permanent professors whose minutes I could not find in the archives. This meeting reestablished a more healthy atmosphere at the IHÉS, Motchane promising to involve his

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[111] Lettre de Alexander Grothendieck à Léon Motchane (24/10/69). Arch. IHÉS.

professors in the transformation processes. In particular, he promised to send them a copy of the budget.

b) Grothendieck, the IHÉS, and The Military

Three days later, Grothendieck wrote him an indignant letter which started another affair. "You omitted to send me a copy of the budget of the IHÉS. This 'neglect' nonetheless did not forbid me to learn that 5% of the budget of the IHÉS are presently provided by the French Military [*forces armées françaises*]." He moreover claimed that Motchane was informed since the beginning that he, Grothendieck, was not ready to keep on working at the IHÉS if a portion, no matter how small, of its finances came from a military source, and that Motchane has assured him that this was not, and would never be, the case. From this fact, "I immediately and irrevocably draw the necessary consequences on a professional, as much as on a personal, level."

Grothendieck gave Motchane an ultimatum. He asked for the written assurance that no portion of the IHÉS budget would come from the military for the following year and for as long as Motchane would remain director. "Short of receiving this assurance, I cannot keep on occupying my functions at the IHÉS." In addition, he promised to give to this question "all the publicity it deserves in my opinion."[113]

On November 24, Motchane sent Grothendieck, and the other professors of the IHÉS a long letter in order to set the records straight: "in ten years, you never one single

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[112] Lettre de Léon Motchane à Alexander Grothendieck, avec addendum manuscrit (28/10/69). Arch. IHÉS. For an expression of Motchane's Gaullist socialist beliefs, see Thimerais [Léon Motchane], *La pensée patiente* (Paris: Minuit, 1943).
[113] Lettre de Alexander Grothendieck à Léon Motchane (15/11/69). Arch. IHÉS.

time came to tell me about your opinions and convictions, nor to inquire of the Institute's position. . . . But of course, I did not ignore that you shared with the majority of scientists a concern for not working for war and, at the occasion of your trip to Vietnam, you were able to learn of my position on this matter which, for that matter, does not differ from yours."[114] Indeed, the archives of the IHÉS preserve little evidence to support Grothendieck's claim that he had in the past voiced his opposition to certain sources of funding.

(i) Grothendieck's Politics and Biology

In 1960 however, when Motchane discussed with the European Atomic Organization (Euratom) a possible contribution in return for the participation of one of its scientist to the Scientific Committee, Grothendieck had expressed his opposition. The reasons he gave had nothing to do with moral concerns about nuclear issues, but only questioned the principle by which Euratom's financial support was linked to its having a word to say in the scientific activities of the IHÉS.[115] Motchane might have learned from this not to involve Grothendieck in administrative matters, which according to the bylaws were none of his business anyway.

That Grothendieck had liberal views, however, was a secret for no one. During the Algerian War, "because of the internal political situation in France," he even envisaged to move to the US.[116] In August 1966, Grothendieck who was to receive the Fields Medal at the Moscow Congress, refused to go, for political reasons. When Motchane, who had

[114] Lettre de Léon Motchane à Alexander Grothendieck (24/11/69). Arch. IHÉS.
[115] Lettre de Alexander Grothendieck à Léon Motchane (3/10/60). Arch. IHÉS.
[116] Lettre de Alexander Grothendieck à Léon Motchane (22/1/62). Arch. IHÉS.

accepted the award in Grothendieck's name, delayed his handing-in of the award, in order to prepare a ceremony, Grothendieck took the delay as an affront.[117] The relationship between Grothendieck and Motchane periodically passed through several tense episodes over the years of their collaboration. In 1967, upon learning, one month after the fact that a secretary had quit, Grothendieck accused Motchane: "you seem to adopt an attitude of sabotage regarding my work."[118] Several years of laudatory *Rapports scientifiques* by Motchane however can only bear witness to the fact that he was extremely proud of the work of his professor. That he was among the greatest mathematicians of his generation was recognized by everyone.

But starting in 1967, Grothendieck became involved in political matters. In November, responding to an invitation of the Mathematical Society of Vietnam he himself had solicited, Grothendieck went to Hanoi, where saw the ravages of the war and experienced bomb alerts, during one of which a mathematics teacher was killed. Upon coming back, he gave an exposé of the mathematical life in Vietnam at the Sorbonne. Detailing, with much sympathy and some naiveté, the extreme difficulties of their daily life, he testified to the Vietnamese mathematicians' courageous efforts to pursue their

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[117] Lettres de Alexander Grothendieck à I. G. Petrovskii (28/2/66); de Alexander Grothendieck à Léon Motchane (5/7/1966); and the following exchange of letters in August 1966. Arch. IHÉS.

[118] Lettre de Alexander Grothendieck à Léon Motchane (18/1/67). Arch. IHÉS. A good example of what can be termed his paranoia is displayed in A. Grothendieck, *Récoltes et semailles. Réflexions et témoignage sur un passé de mathématicien,* 7 vols. (Montpellier: Université du Languedoc and CNRS, 1985). Manuscript, Fine Arch.

mathematical activity. He called for the organization of a support movement, which was to have some concrete effects.[119]

After May 1968, the mathematicians of the University of Orsay set up a committee in charge of evaluating the education and recruiting of mathematics professors, in view of addressing the "fundamental vice of the present system."[120] As a result, Grothendieck wrote a widely circulated report, which he asked *Le Monde* to publish. This highly elitist text was inspired by the Bourbakis' notion of a resonance box [*caisse de résonance*]: those "second-rank researchers" who according to Weil, "play a smaller role in [mathematics] than elsewhere, the role of a resonance box for a sound that they do not contribute to make."[121] Earlier in 1968, Dieudonné had acknowledged that "resonance boxes" might also play a role in mathematics below the 150 "great mathematicians" of the century.[122] A "Questionnaire sur la recherche" circulated by Orsay mathematicians at the end of 1967 expressed a similar point of view:

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[119] A. Grothendieck, "La vie mathématique en République démocratique du Vietnam," typed manuscript, 22 pp.; lettre de Alexander Grothendieck à Léon Motchane (29/9/67). Arch. IHÉS. In particular, it led to a trip of Bernard Malgrange and Alain Chenciner to Vietnam, in October 1974, see A. Chenciner, B. Malgrange, Lê Dûng Tráng, and F. Pham, "Les mathématiques en République démocratique du Viet Nam," *Gazette des mathématiciens,* 3 (February 1975): 26-31. For the memoirs of another activist mathematician who went to Vietnam in 1968, see L. Schwartz, *Un mathématicien aux prises avec le siècle* (Paris: Odile Jacob, 1997), chap. 11, and esp. 454-462.

[120] Lettre de Alexander Grothendieck à Hubert Beuve-Méry, directeur du *Monde* (4/6/68). A. Grothendieck, "Le maître-enseignant et le maître-chercheur dans l'Université d'aujourd'hui et de demain," typed manuscript (June 1968).Arch. IHÉS.

[121] A. Weil, "The Future of Mathematics" (transl. Arnold Dresden), in *Great Currents,* 321-336, 333; "Le futur des mathématiques," in *Grands courants,* 317-318. My translation.

[122] For his point of view, see J. Dieudonné, "Orientation générale des mathématiques pures en 1973," *Gazette des mathématiciens,* 2 (October 1974): 73-79; and J. Dieudonné, "Lettre à Marcel Berger (17/4/80)," *Gazette des mathématiciens,* 14 (July 1980): 138.

> Are useless and even harmful all the papers whose only aim is to prove the ability of the authors to solve certain exercises, or to provide these so-called researchers with positions in teaching or research.[123]

Inspired by the IHÉS's ideology, Grothendieck voiced the opinion that two akin, but distinct, roles were confused in the present situation: the mathematician had "to transmit already acquired knowledge," while at the same remaining the "intellectual creator contributing to the deepening, widening, and renewal of this very knowledge." In spite of the present confusion, the system only worked because the number of students was small enough. With the coming of mass education, the situation was changing. He believed that some professors should devote their energy to the education of future "users of mathematics" (scientists, engineers, businesspersons), rather than future mathematicians.[124] Grothendieck however loathe this type of teaching which, in his opinion, should not be imposed to the great mathematicians.

> As a general rule, except for rare exceptions, the greatest the worth of a researcher, . . . the greatest will be the effort he will have to make in order to be snatched away from [his] problems, and the difficulty he will feel to put himself into the different state of mind which the training of students who do not care at all for this science as such, demands.

Grothendieck envisioned a system where "great mathematicians" would be given a freedom comparable to the one he enjoyed at the IHÉS. The person who, "once his thesis completed, which will have demanded from him (supposing himself or his thesis advisor took seriously the traditional criteria of the worth for a thesis) a very considerable

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[123] For a summary of the above positions and comments about these issues, see P. Samuel, "Buts d'un mathématiciens," *Gazette des mathématiciens,* G2(5) (June 1970): 39-46. Quote on p. 40.

[124] Motchane also was concerned with the problem of adapting universities to mass education, lettre de E. C. Zeeman à Léon Motchane (19/8/68). Arch. IHÉS.

effort, unique in his life," the person who unfortunately was not a "great mathematician," should be charged to train mere users of mathematics.

In this text, Grothendieck wrote a surprising sentence which, taken seriously, could soon have been used to disqualify him as a serious mathematician.

> The most characteristic test to distinguish the mathematical re searcher from any species of individuals, is that when he meets one of his colleagues, . . . he starts right away to discuss neither politics, the weather, his colleagues, nor his boss, . . . but rather of mathematical problems.[125]

René Thom, who quite early stopped considering himself as "true" mathematician once voiced a similar opinion. "If one truly is a mathematician in the soul, one is not very much concerned with philosophy; and if this happens, it is a sort of derailment. I remember what happened to Grothendieck."[126] Real mathematicians could not be concerned with anything other than mathematics.

During the summer of 1970, Grothendieck engaged in an iconoclastic political crusade for raising consciousness about the responsibility of scientists. This evolution, of course, hardly was independent from the conflicts that opposed him to Motchane at the IHÉS. The most surprising, however, is that Grothendieck's metamorphosis was also due to the development of Thom's ideas. Indeed, on November 14, 1969, the day before he wrote his infuriated letter to Motchane about the military credits, Grothendieck sent a request the secretary for ordering a dozen biology books. "One of the most terrible attackers [*pourfendeurs*] of applied mathematics," Grothendieck was getting interested in

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[125] A. Grothendieck, "Le maître-enseignant et le maître-chercheur."
[126] R. Thom, *Prédire n'est pas expliquer,* 14-15.

biology.[127] This was an immediate consequence of the activity that was taking place around Thom.

In a long letter he sent to Motchane on January 26, 1970, Grothendieck explained what had happened.

> [W]hile in the past the relations on a scientific level among the diverse permanent [professors] of the IHÉS, between physicists and mathematicians as much as between Thom and me, were practically nil, this situation has been changing for the last few months. As it was, the 'catalyzers' have been Zeeman and [Mircea] Dumitrecu (a Romanian biologist, friend of [Valentin] Poenaru). Zeeman is making great efforts to popularize Thom's ideas on universal mathematical models (the "catastrophes") in the natural sciences, and he convinced me of the importance of these ideas and of the necessity to assimilate them.

Alexander Grothendieck later fondly remembered the ambiance of "scientific incubator [*étuve*]" which reigned at the IHÉS in 1969-1970.[128] Dumitrescu's seminar on molecular biology were sometimes attended by Ruelle and Thom, while the latter explained his geometric ideas on morphology to Dumitrescu and his audience. Grothendieck saw vast new fields of research opening to him.

> I am seriously thinking of taking advantage of the exceptional work conditions I enjoy [at the IHÉS] in order to devote a few years to acquire some basic knowledge in the natural sciences, notably in physics and biology, with the hope of later contributing to an interdisciplinary attack of certain problems, *presently all too much subjected to the sole specialists.*[129]

Grothendieck's attitude hardly pleased Motchane and contributed to the escalation of the conflict that opposed the two of them. Meanwhile, however, they had been able to reach some kind of compromise about the affair of the military credits.

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[127] Jacques Baron, "Au premier rang mondial, l'École mathématique française," *Entreprise,* nos. 726/727 (9-16 août 1969), 24-33, 25.
[128] A. Grothendieck, *Récoltes et semailles,* 3, 170, n.42.
[129] Lettre de Alexander Grothendieck à Léon Motchane (26/1/70). Arch. IHÉS. My emphasis.

(ii) Military Credits at the IHÉS

In his reply to Grothendieck's ultimatum, on November 24, 1969, Motchane had done little to bridge the rift. He explained how himself had cleared his conscience. Grothendieck's analysis, Motchane judged, was superficial. It was common knowledge that military funding were distributed over civil budgets. To forgo all credits coming from specifically military agencies, he thought, was a Pharisaic attitude. He had adopted a solution that controlled at the level of the Institute the use made of the funds, always insisting on total scientific independence. Motchane was nonetheless ready to discuss these matters with Grothendieck, and a few days later, sent him the few documents he had demanded.[130]

Receiving Motchane's first letter, Grothendieck reiterated his accusations: "If I stayed at the IHÉS while a portion of its budget came from a [military] source, it is solely because you thought best to decide to leave me ignorant of this fact, and that I committed the unforgivable fault of trusting you on his point." He wrote that having failed to get the assurances he had asked for, he was preparing his leave from the IHÉS, albeit keeping a door open for discussion.[131]

In view of the imminent inflow of foreign money, Motchane now thought Grothendieck's concerns a "purely academic" question. While calling for a frank discussion, he insisted on the fact that the professors needed not be involved in administrative matters.

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[130] Lettres de Léon Motchane à Alexander Grothendieck (24/11/69); and (28/11/69). Arch. IHÉS.

[131] Lettre de Alexander Grothendieck à Léon Motchane (1/12/69). Arch. IHÉS.

> As you know, the absolute, without precedent, scientific independence enjoyed by the Scientific Committee is counterbalanced by its total non-interference in the administrative domains.[132]

On December 16, 1969, Grothendieck once again escalated the conflict. "I immediately stop [my] activity. In the coming days, I will come to vacate my office at the IHÉS." He did not resign, but rather asked for a leave of absence. And he insisted on attending the next meetings of the CS to discuss these issues.[133]

The same week, Motchane granted his leave to Grothendieck, but only if some conditions were respected by him. In particular, he insisted on Grothendieck's having to attend the annual spring meeting of the CS, a condition never imposed previously. Grothendieck was outraged. While saying that his intent indeed was to participate to this meeting where important issues would be raised, he stated that he had "no intention to set foot again at the IHÉS, as long as it would be supported by funds of a military origin."[134] This was a dangerous sentence to write. As Motchane replied,

> this would constitute a unilateral rupture without advance notice of contract with the Institute, whose sole immediate consequence would be pure and simple exclusion.[135]

Over the Christmas break, Motchane had however come up with an appropriate response to his professors' worries. Meanwhile, Grothendieck had obtained from his colleagues to write a joint letter to Motchane expressing their concerns regarding

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[132] Lettre de Léon Motchane à Alexander Grothendieck (12/12/69). Arch. IHÉS.
[133] Lettre de Alexander Grothendieck à Léon Motchane (16/12/69). Arch. IHÉS.
[134] Lettres de Léon Motchane à Alexander Grothendieck (19/12/69); de Alexander Grothendieck à Léon Motchane (20/12/69). Arch. IHÉS.
[135] Lettre de Léon Motchane à Alexander Grothendieck (9/1/70). Arch. IHÉS.

Grothendieck's imminent leave, and demanding that the IHÉS renounce any type of military funding. In substance, Motchane agreed to this request.[136]

He explained that the majority of the so-called military funding that the Institute had received since 1964 had been coming from the discretionary funds of the Minister of Armies, M. Pierre Messmer. This amount of 500,000 F over several years (1965 to 1967) could not, properly speaking, be labeled as military, since the Minister himself was a civil. The Minister had also contributed around 100,000 F in 1968 and 1969. These amounts represented from 13% to 4,35% of its budget. Motchane did not foresee that this exceptional help would be renewed for 1970 and after. Papers preserved at the IHÉS show that, albeit vague, Motchane's affirmations were correct.

But Motchane twisted the truth when addressing the more delicate case of the *Direction des recherches et moyens d'études* (DRME), the organism in charge of scientific research in view of military applications. Those were "the only truly military funds that the IHÉS has ever received." In 1966, indeed, the DRME had provided the IHÉS with 20,000 F, a mere 1% of its budget that year. He wrote his professors that he had had to provide a report, a requirement he found unacceptable.

What Motchane did not say, and about which he in fact lied, was that the main problems as for continuing this contract and obtaining another one from the DRME had not come from him and the IHÉS, but rather from the military agency itself. In 1964, upon arriving to the IHÉS, Ruelle had indeed written an extensive proposal for a contract to the amount of 350,000 F over two years. Finally, despite the favorable opinion of

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[136] Lettres de René Thom, David Ruelle, Louis Michel et Alexander Grothendieck à Léon Motchane (19/12/69); de Léon Motchane à René Thom, David Ruelle, Louis Michel et

Pierre Aigrain, then scientific advisor to the DRME, this contract was rejected, because not fitting its ordinary framework.[137] The other contract, which was accepted, concerned research seminars in mathematics, including Thom's seminar with Malgrange and Grothendieck's. Strangely, as opposed to Thom, Grothendieck apparently was not asked by Motchane to provide a description. Motchane, on the other hand, was obligated to ask for the DRME's permission to publish its proceedings, including Grothendieck's famous *Séminaire de géométrie algébique* XIX to XXV.[138]

Neither was Motchane truthful when he insured his faculty members that "[f]aithful to the spirit that presided over the foundation of the IHÉS, . . . we never neither solicited contributions from military agencies, nor accepted those from politico-military organizations, such as NATO."[139] Indeed, while the possibility of looking for NATO support was raised at the foundation session, but apparently not pursued, the above shows that Motchane's efforts at getting DRME support were important in the dire years of 1962-1964.[140] Moreover, on several occasions throughout the late 1960s, Motchane,

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Alexander Grothendieck (8/1/70). Arch. IHÉS.

[137] Lettres de Léon Motchane à Lucien Malavard (17/11/64); de Léon Motchane à Pierre Aigrain (8/12/64); de Annie Rolland à Léon Motchane (19/1/65). Arch. IHÉS. Cf. also *Note manuscrite* de Annie Rolland suite à un coup de téléphone de André Grandpierre (12/1/65), et lettre du Général Lavaud au Général René Cogny (15/5/62): La DRME "s'interdit toute action qui pourrait l'assimiler à un mécénat."

[138] Lettre de Léon Motchane à Jacques Dubois (21/6/65). Lettre de Léon Motchane à Lucien Malavard (23/11/64); contract with DRME n° 167/65 approved (2/4/65) for 20,000 F. Notification d'une commande de travaux sur mémoire, DRME: "Organisation de séminaires de physique théorique et de mathématiques" (25/5/65). Arch. IHÉS.

[139] Lettre de Léon Motchane à René Thom, David Ruelle, Louis Michel et Alexander Grothendieck (8/1/70). Arch. IHÉS.

[140] Lettre du Général René Cogny, Commandant en Chef en Afrique Centrale, à François Le Lionnais (15/5/62). Arch. IHÉS.

helped by François Le Lionnais, attempted to transform the exceptional support of the Minister of Armies into a permanent subvention.[141]

On January 8, 1970, with the promise that foreign subventions should come in the near future, Motchane gave his "moral assurance" to the permanent faculty members that the IHÉS would not receive, nor solicit funds from "military or pseudo-military" sources. This was the most he could do, and it satisfied Grothendieck and the others.[142] The incident was closed.

(iii) Jalousie? Better a Good Divorce than a Bad Union[143]

During the spring term, 1970, the Minister of Armies however informed the IHÉS that he was granting another subvention of 80,000 F. The finances of the IHÉS remaining stretched, the administrators accepted the subvention, even if it hardly represented more than 3,5% of its budget.[144] On March 5, in order not to cut their invitation budget, while allowing Grothendieck to stay, the physics professors, Michel and Ruelle, asked that these funds be exclusively devoted to their Section.[145] President André Grandpierre of the

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[141] E.g. Lettre de André Grandpierre à Pierre Messmer (3/3/66); *Notes sur les rapports de l'IHÉS avec le Ministère des Armées and the DRME* (9/3/66); de Henri Domerg à Léon Motchane (18/4/67); de Léon Motchane à Jacques Ballet (12/1/68); de Jacques Ballet au Général Fourquet, délégué général à l'Armement (22/2/68): "Expriment le souhait que l'aide du Ministère des Armées devienne permanente et prenne la forme d'une subvention annuelle." *Note sur les relations de l'IHÉS avec le Ministère des Armées* (14/5/68). Arch. IHÉS.

[142] Lettres de Alexander Grothendieck à Léon Motchane (13/1/70); de Alexander Grothendieck, Louis Michel, David Ruelle et René Thom à Léon Motchane (16/1/70). Arch. IHÉS.

[143] Lettre de Jacques et Marie-Jeanne Tits à Annie Rolland (13/6/70). They wrote: "un bon divorce vaut mieux qu'un mauvais ménage." Arch. IHÉS.

[144] *Notes manuscrites d'Annie Rolland, Assemblée générale* (15/12/70), where president Jacques Ballet said: "Toujours sur la corde raide." Arch. IHÉS.

[145] Lettre de Louis Michel et David Ruelle à André Grandpierre (3/5/70). Arch. IHÉS.

IHÉS replied that this solution was not administratively possible, but by informal agreement, they could be attributed only to physicists.[146] Failing to gather the support of his colleagues to oppose in block the acceptance of this subvention, judging the IHÉS's compromise as being "incompatible with the responsibility of a scientist [*savant*] vis-à-vis society," Grothendieck resigned from the IHÉS on May 25, 1970.[147]

But the affair of the military credits might have only been a façade. Personal conflicts between Grothendieck and Motchane soon resumed after the January arrangement. Moreover, Grothendieck's avowed interests, as well as his activities, had perhaps started diverging from what Motchane had in mind for him. This incident reveals some other aspects of the atmosphere reigning at the IHÉS in 1970, which helped shape the modeling practices being promoted at the same time by Thom and his clique.

As early as January 26, Grothendieck wrote a long letter to Motchane in order to make his position clear for the future. There, as described above, he talked about his new interest for the natural sciences. But mostly, he reproached Motchane of not having involved the faculty enough in the administration of the IHÉS, especially with regards to the choice of the new director and the change of status linked with the Europeanization of the IHÉS. In unmistakable terms, he also accused him of incompetence, "due to the overwork and nervous fatigue [*usure*]" of the twelve tears he spent as the head of the IHÉS. He was showing him the door. "Since in a few years at the latest, we will have to

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[146] Lettre de André Grandpierre à David Ruelle (4/5/70). Arch. IHÉS.
[147] Lettres de Alexander Grothendieck aux membres du Comité scientifique (25/5/70); de Alexander Grothendieck à Léon Motchane (9/6/1970). Arch. IHÉS.

leave to others the task of pursuing this enterprise (If it survives until then), is it not better that you accept it right now?"[148]

Indeed, other problems between Motchane and the faculty, already brought up during the previous semester, had started to occupy the foreground. On January 18, the four permanent professors, without Deligne just officially hired, again addressed a joint letter to the director. It concerned the change of status of the IHÉS and the choice of its next director. The professors asked for the establishment of formal procedures for the CS, so as to keep a consultative voice in these negotiations. Again, the atmosphere was rapidly deteriorating.[149]

"We always lose something when a private association is transformed into an organization whose patrons are States."[150] The professors were endeared to the bylaws Motchane himself had written for the IHÉS, and the prospect of important changes scared them. For Motchane, this was a "true wind of craziness [which] started to blow among the permanent [professors]."[151] But soon, he felt that the cohesion among the professors was not so strong as he feared.

> The "united front" of the permanent [professors] seems shaken up. I saw MICHEL this morning, who does not blindly walk with GROTHENDIECK anymore. THOM of course remains correct and normal.[152]

On November 30, 1969, E. C. Zeeman, answering a verbal offer by Motchane, had written that, for the time being, he did not want to be considered for directorship, but that

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[148] Lettre de Alexander Grothendieck à Léon Motchane (26/1/70). Arch. IHÉS.
[149] Lettre de Alexander Grothendieck, Louis Michel, David Ruelle et René Thom à Léon Motchane (18/1/70); de Léon Motchane à René Thom (28/1/70). Arch. IHÉS.
[150] Lettre de Léon Motchane à Victor Weisskopf (5/2/70). Arch. IHÉS.
[151] Lettre de Léon Motchane à André Grandpierre (13/2/70). Arch. IHÉS.
[152] Lettre de Léon Motchane à André Grandpierre (18/2/70). Arch. IHÉS.

he might reconsider his decision at the end of Motchane's mandate in 1974.[153] In February, as a way to insure the future of the IHÉS, Motchane intensified the search for his successor. His preference went to Res Jost, while Zeeman remained his second choice.[154] But the former apparently was not interested.

On April 6, 1970, at 10 o'clock, a fateful meeting of the CS took place, attended by the five permanent professors, Motchane, Montel, and British physicist Rudolph Peierls.[155] A motion was unanimously voted in favor of formally asking Zeeman to become the next director of the Institute. Motchane then explained how negotiation with foreign governments were going. Invitations for the next year were discussed.

Suddenly, Grothendieck mentioned his desire of inviting molecular biologist Mircea Dumitrescu for the whole year. He had already given a seminar at the IHÉS, underscoring, Grothendieck thought, the "positive character of the emergence of common interests among permanent [professors]," upon which other professors more or less protested. Grothendieck had solicited the opinion of eminent biologists in favor of Dumitrecu.[156] Thom's opinion however was not as favorable. The biologist possessed

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[153] Lettre de E. C. Zeeman à Léon Motchane (30/11/69). Arch. IHÉS.

[154] Lettres de Léon Motchane à Rudolph Peierls (5/2/70); de Léon Motchane à Victor Weisskopf (5/2/70). Arch. IHÉS.

[155] This memorable session of the CS was reconstituted starting from the *notes de séances manuscrites de Annie Rolland*; and *Procès-verbal du Comité Scientifique du 6 avril 1970* (dated 29/5/70), which however differ on some points. Also *Lettre* de Léon Motchane à E. C. Zeeman (22/4/70). Arch. IHÉS.

[156] Lettres de Alexander Grothendieck à E. Wollman (12/3/70); de E. Wollman à Alexander Grothendieck (17/3/70); de Alexander Grothendieck au Dr. C. Ropartz (26/3/70); de C. Ropartz à Alexander Grothendieck (2/4/70). Dumitrescu's doctoral thesis in medical sciences, *Genetic Mechanisms of Evolution and Analysis of the Consistency of Some Usual Biological Concepts*, prepared while he was at the IHÉS, was defended at Bucarest on June 17, 1971. See *Rapport sur les activités scientifiques à l'IHÉS en 1972—*

"very original [and] advanced conceptions, some of which are perhaps valuable, but [he was] too speculative for an experimenter and too experimental for a speculative." But Thom did not mind his coming to the IHÉS. Motchane however seemed opposed to it. His reasons were not exactly clear. Perhaps did he not think that Grothendieck should devote so much energy to biology; more probably, as he said during the meeting, did he feel uncomfortable with inviting a biologist when this clearly was not the role of the IHÉS. True, Waddington had been invited by Thom to spend three days at Bures in 1966, but the present proposal had a totally different scale.

Since the handwritten minutes taken by Annie Rolland and the proceedings established by Motchane differ at this point, what happened next is not totally clear. In any case, Ruelle suggested that, since the Institute was supposed by its bylaws to promote research in "all discipline connected with" its main fields of inquiry, Dumitrescu could be invited to organize a "pluri-disciplinary seminar on the relations between mathematics and physics." According to the minutes, Motchane agreed to this formula, upon which Grothendieck interjected that he had always considered him an "arrant liar [*un fieffé menteur*]." Motchane was enraged. Grothendieck said: "If you were looking for a grave incident, you got it!" Following this, Rolland simply wrote "Strife [*Bagarre*]."

Three days later, at 9:35, the CS went on discussing modification to the bylaws. "M. Grothendieck left the session at 10:05. He had been reading Bourbaki for the whole

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Ouvrages de mathématiques et de physique élaborés à l'IHÉS (February 1973). Arch. IHÉS.

time."[157] The relations between him and Motchane had suffered beyond repair. From then on, Motchane would work on ways to get rid of Grothendieck.

On April 14, Peierls wrote that he thought Motchane had not appreciated "the strength of the feeling amongst the permanent members of the Institute, and their near-unanimity in spite of the divergence in their view on some matters." Peierls did not see that there was enough ground to dismiss Grothendieck. True his letter stated vague intentions of devoting himself to subjects other than mathematics. True his attitude at the CS had been reprehensible. But this was no ground for firing him.

On April 22, Motchane exposed Grothendieck's case to the Administrative Board of the IHÉS. I have not found the proceedings, nor the minutes of this meeting in the archives, but I hit upon non-dated handwritten notes prepared by Motchane for an exposé which I suspect took place on this occasion. For Motchane, after the crisis linked with military credits, there was a "violent action of G trying to take control, to kick me out."[158] Apparently, a *motus vivendi* was obtained, Motchane declared, but a desire remained "to install an *oligarchy*." In his view, Grothendieck's departure had become unavoidable, since Thom and Michel would otherwise leave. Motchane believed that this crisis hid something deeper. Grothendieck's crazy ideas [*lubies*] were dangerous. For him, Thom's

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[157] *Notes de séances manuscrites du Comité Scientifique de Annie Rolland* (9/4/70). Arch. IHÉS.

[158] *Notes manucrites de Léon Motchane* (n.d.). Here is the complete text: "Nov. 69 = crise des crédits militaires. Solution? Position de l'IHÉS. Crise liée aux transformations. Pourparlers avec les allemands, lié avec ma retraite: action violente de G essayant prendre le dessus, me mettre dehors, joue un rôle[?] de dictateur, opposition de G et [?]... En apparence un motus vivendi est obtenu mais le désir d'installer une oligarchie. Quelques profs sont venus me trouver pour dire que c'est imparable = le départ de G devient nécessaire. Raison[?] Thom et M. partiront[?]. Cette crise cache quelque chose de plus

success in biology had "engendered a humility" from Grothendieck's part. Obviously, he, or Motchane, had to leave. It was in this context that the administrators accepted the subvention from the Minister of Armies.[159] Since not anymore devoting his energy to mathematics, Grothendieck was dispensable.

Besides the personal reasons that Grothendieck might have had, the attraction of Thom's ideas, and the activities going on around him, thus were powerful enough to contribute greatly to his quitting not only the IHÉS, to which he had contributed so much, but also mathematical research. Was Grothendieck envious of Thom? The count of their approximate number of lines in the *Science Citation Analysis,* as I found in the archives of the IHÉS, would hardly substantiate this claim. Above everyone else, Grothendieck had 520 lines, while Thom and Ruelle, respectively, only had 95 and 119.

The best analysis of the crises that shook the IHÉS in 1969-1970 may well have been that provided by Peierls. Having just married his daughter, he wrote that Motchane found himself in "the familiar situation of the parent who is taken by surprise by the fact that his children have grown up and has to adjust himself to this new situation."[160] The Institute had acquired a life of its own. Motchane could leave it the next year, to be replaced by Nicolaas Kuiper, a Dutch mathematician specializing in fields close to

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profond = Ces lubies peuvent être dangereuses = Thom biologie = a engendré une humilité[?]."

[159] Communication de Léon Motchane (15/5/70). Arch. IHÉS.

[160] Lettre de Rudolph Peierls à Léon Motchane (12/4/70). Arch. IHÉS.

Thom's. He was chosen only after Christopher Zeeman and André Maréchal both declined.[161]

As for Grothendieck, a new period in his life began during which he got involved in anti-war, then ecological, activism. In June 1970, he founded at Montréal the movement *Survivre,* (later, *Survivre et vivre*), which counted, besides Grothendieck's own highschool nephew, among its French members and fellow travelers Claude Chevalley, Pierre Samuel, Denis Guedj, Daniel Sibony, all of whom would play prominent roles in debates about the social role of mathematics during the next decade.[162] Attempting to use the International Congress of Mathematicians, held in Nice in August 1970, to make mathematicians more aware of their responsibility vis-à-vis society, Grothendieck only succeeded in infuriating his old colleague Dieudonné.[163] This marked the sad end of a very successful mathematical career.

c) The Birth of Catastrophe Theory

Just as reminder that the political situation was not tense only for IHÉS scientists, let me quote from a letter sent by Oscar E. Lanford on May 7, 1970. At Berkeley, he also found

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[161] Lettres de E. C. Zeeman à Léon Motchane (30/6/70); de E. C. Zeeman à André Grandpierre (30/6/70); de E. C. Zeeman à Annie Rolland (30/6/70). About Maréchal, lettre de David Ruelle à Léon Motchane (12/11/70). Arch. IHÉS.

[162] A. Grothendieck, *Récoltes et semailles,* tome 3, n.6, p.143. A. Grothendieck, "Responsabilité du savant dans le monde d'aujourd'hui: le savant et l'appareil militaire," typed manuscript, 63 pp. Arch. IHÉS. See e.g. D. Sibony, "À propos des mathématiques modernes," *Tel quel,* 51 (1972): 87-103; repr. *Pourquoi la mathématique?,* ed. R. Jaulin, Robert (Paris: UGE, 1974): 100-130; and P. Samuel, *Séminaire "Mathématiques, mathématiciens et société."* Publication mathématiques d'Orsay n° 86-74.16 (1974). Jussieu Lib.

[163] A. Grothendieck, "Reportage: Le Congrès international des mathématiciens de Nice (1-10 septembre 1970)," typed manuscript., 7 pp. Arch. IHÉS.

himself in the midst of a political crisis. The Governor has just closed the University of California for four days "to preempt Faculty-students strike against Nixon's adventure in Cambodia." Lanford acknowledged that: "Evidently, no one finds much energy to spend on scientific work."[164]

Apparently, despite all the strife, this was not so much the case at the IHÉS. In 1969-1970, the Institute witnessed an unprecedented convergence of dynamicists. The following years would see these relations being sustained. In particular, Smale and his students may sometimes have found the atmosphere of Bures-sur-Yvette more conducive to work than Berkeley's. It was while he was at Bures that Smale wrote one of his first papers, titled "Topology and Mechanics," which did not deal with purely mathematical themes.[165] A modeling practice inspired by Thom's was attracting the best representative of dynamical systems theory. Important contributors would be, among others, Ralph Abraham, Jerrold Marsden, Christopher Zeeman, David Ruelle, and René Thom himself.

In 1971, Motchane proudly stated that "the IHÉS is one the very rare places [in the world] where physicists and mathematicians successfully interact."[166] Throughout the 1970s, sometimes under Thom's impulsion, and more and more under Ruelle's, the IHÉS remained one of the main research centers where dynamical systems theory, and of course catastrophe theory, were pursued always with an eye intensely focused on applications, not only in physics, but also in biology, linguistics, economics, and psychology. Taking

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[164] Lettre de Oscar E. Lanford, III, à David Ruelle (7/5/70). Arch. IHÉS. In October, Ruelle witnessed that the "political atmosphere was heavy" in California: Lettre de David Ruelle, from Irvine, à Léon Motchane (14/10/68). Arch. IHÉS.

[165] *Rapport Scientifique, Année 1970 - Travaux de mathématiques et de physique théorique,* 1. Arch. IHÉS. It was published in *Inventiones Mathematica,* 10 (1970): 305-331; 11 (1970): 45-64.

up his directorship on October 1, 1971, Nicolaas Kuiper wrote: "M. Thom keeps on attracting the attention of young mathematicians and others on many open problems and he is an 'attractor' and a source of inspiration for many visitors." He greatly inspired Zeeman's and Abraham's applied work. At the same time,

> the work of M. Ruelle is of mathematical and physical interest and contributes to the coherence of the scientific activities of the IHÉS, in particular between physics and mathematics, an exceptional thing in the world."[167]

Throughout these years, René Thom traveled the world "to spread the gospel," giving talks on a wide variety of topics (mathematics, biology, linguistics, and physics) to a wide variety of audiences.[168]

In 1970-1971, Thom devoted many sessions of his seminar to his "Mathematical Models of Morphogenesis," writing up the first chapters of his second book first published as a pocket book in 1974. In the following years, Thom held a Monday seminar whose designation varied from applied global analysis to structural stability and bifurcation theory, or qualitative dynamics. In particular, in February and March, 1972, Thom's seminar welcomed several talks given by Abraham, Marsden and Ruelle, on hydrodynamics, Smale's econometric theories, and bifurcation theory.[169]

But despite all his attraction power, Thom hardly seem to have been interested in building a true research school. Too much of a dreamer and moving away from pure mathematics, he lacked the necessary administrative skills. True, a young French

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[166] *Note pour le rapport scientifique 1970* (2/6/71) Arch. IHÉS.
[167] *Note pour le rapport scientifique 1971* (18/5/72). Arch. IHÉS.
[168] "je m'arrêterai à Buffalo (Center for Theoretical Biology), et à Chicago, pour y répandre la bonne parole sur la Morphogenèse." Lettre de René Thom à Léon Motchane (21/6/67). Arch. IHÉS.
[169] *Rapports scientifiques* (1971 to 1973). Arch. IHÉS.

mathematician working on his Ph. D. thesis, Alain Chenciner, was especially attracted by Thom's ideas in the early 1970s. Thom invited him to speak at his seminar, and was on his thesis committee.[170] Unfortunately, Chenciner remained an exception, and Thom was not able to build at Bures anything approaching Smale's Berkeley global analysis group of the late 1960s.

On September 25-28, 1972, a "catastrophic seminar" was held at Bures-sur-Yvette, celebrating "Catastrophe Theory and its Applications."[171] Coinciding with the publication of *SSM,* which had been awaited for so long, this symposium may well be considered as the official birth of catastrophe theory as such. Besides talks by Abraham, Smale, Takens, Zeeman, and a series by Thom, it also included talks by French mathematicians, B. Teissier (a frequent speaker in Thom's seminars), D. Thillaud, and F. Pham, which may serve to underscore the attraction of Thom's ideas, but mainly the difficulties of integrating them within the dominant Bourbakist mold. In 1973, Thom recruited another man from the French quarters in the person of Ivar Ekeland, who would play an important role in the diffusion of catastrophe theory in France.

In 1974, in numerous lectures, Thom addressed issues related to qualitative dynamics, differential topology, catastrophes and the human sciences, language and catastrophes, and theoretical biology. That year, other people who would be quite vocal in

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[170] A. Chenciner gave talks were "Catastrophes élémentaires de Corang deux: Ombilics" (25/1/71), "L'ombilic parabolique" (1/2/71). His thesis, *Sur la géométrie des strates de petites codimensions de l'espace des fonctions différentiables réelles sur une variété,* doctoral thesis (Faculté des Sciences d'Orsay, Université de Paris), was defended on June 6, 1971, in front of H. Cartan (president), J. Cerf, L. Scwhartz, and R. Thom.

[171] *Journées sur la théorie des catastrophes and morphogenesis: Summaries and References Concerning the Given Lectures.* Lettres de Nicolaas Kuiper à Floris Takens et

their defense of catastrophe theory for the first time spoke at Thom's seminar. In particular, one notes Claude-Paul Bruter's talk on solitary waves, and Jean Petitot's on "The Pictorial Representations of the Myth of St. George."

Two other factors besides Thom's own reticence, further hindered the establishment of a true research school in qualitative dynamics at Bures-sur-Yvette. First, not being a university, the Institute had no obligation to accept students (in fact, Thom could not head a thesis committee). Therefore, although students were always present and welcome at the IHÉS, either as auditors for seminars or, more rarely, as paid visitors, their flow seldom was important enough. Second, albeit not requiring anything close to the financial needs of an experimental facility, the constitution of a research school would at least have involved the payment of a few salaries, while throughout its existence, the IHÉS struggled to keep afloat. There was therefore always a question of how best to distribute scarce resources. When new credits started coming from different national science foundations in the early 1970s, a debate between Ruelle and Motchane underscored that a feeling, shared by Thom, existed among the faculty that too much was spent on the salary of permanent professors as opposed to that of visiting scientists.[172]

Overall, the variety of topics treated around Thom probably was too wide to avoid a feeling of spreading out. Only a mind such as Thom's might have been able to keep up with it, but only at the crucial cost of often remaining superficial. "In a sense," as Zeeman put it, "Thom was forced to invent catastrophe theory in order to provide himself with a

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autres (7/2/72); de René Thom à Nicolaas Kuiper (5/7/72 et 31/2/72) where the "séminaire catastrophique" is mentioned. Arch. IHÉS.

canvas large enough to display the diversity of his interest."[173] With the lack of a clear focus, Thom's school might have been bound to remain an exciting maelstrom with no actual impact on the practice of mathematically modeling natural phenomena. In short, to use a word dear to Motchane's ideology of fundamental research, Thom needed "interpreters" in different disciplines for adapting his ideas, for producing concrete modeling practices. The IHÉS, especially with Ruelle's impulse, provided a perfect place for this to happen in physics. But, as their interests and methods diverged, the contacts between Thom and Ruelle became sparser. Catastrophe theory, as a theory of modeling practice, was born at the IHÉS, but perhaps was it only destined to die in infancy.

d) A Research School for Thom in the Methodology of the Sciences of Man?

This needed not to be so. After his retirement in 1971, Léon Motchane engaged in the new project of, at long last, setting up the Third Section of the IHÉS. Already in 1958, he had wanted his institute to include a Section dealing with the "Methodology of the Sciences of Man." The very day he left the directorship of the IHES to Kuiper, on October 1, 1971, Motchane wrote to Chen Ning Yang, professor of theoretical physics at SUNY, Stony Brook, about the Third Section. Again, we may note that he was addressing a physicist rather than a specialist in the humanities and social sciences. Believing that the time had "come to try some new experiences in the research of human sciences,"

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[172] Lettres de René Thom à Léon Motchane (12/5/70); de David Ruelle à Léon Motchane (12/4/71); de Léon Motchane à David Ruelle (3/5/71); de Louis Michel à David Ruelle (28/4/71). Arch. IHÉS.

[173] E. C. Zeeman, "Catastrophe Theory: A Reply to Thom," *Manifold,* 15 (1974); repr. *Dynamical Systems, Warwick 1974*, ed. A. Manning (Berlin: Springer, 1975): 373-383, 373.

Motchane solicited Yang's advice on the directions that such experiences should take. His letter appeared somewhat jumbled, and, as far as I was able to determine, received no answer. It however shows that he now thought of this Section as one where the mathematicians' and physicists' tools, their "structures," might be used for attacking problems in biology, sociology, and linguistics. In particular, one was to look "for some hints to a structural stability. The recent work of Thom and others," he added, "gives us some hope."[174]

Motchane hoped that the activities of the different Sections, including the humanities, be formalized. In January, 1972, however, the members of the Scientific Committee expressed the opposed opinion. . On February 7, Ralph Abraham introduced Smale's theories in economics. Thom and Michel suggested that a meeting be organized on economics, and on March 3, 1972,economists Gérard Debreu and Werber Hildebrand came to the IHÉS.[175] These modest activities, in addition to the "catastrophic symposium" of the next September, were felt by Kuiper as a partial fulfillment of the IHÉS mandate in the humanities.

For Motchane, it seemed that multidisciplinarity had become essential in order to broaden the financial basis on the IHÉS. During the following summer, having been named on the Administrative Board of the Institute, he warned Kuiper:

> In the future it will not be accepted that government's money is spent on scientific work, in case that work does not contain components in the direction of human

[174] Lettre de Léon Motchane à C. N. Yang (1/10/71). Arch. IHÉS. Original English.
[175] *Rapport scientifique, Année 1969 - Séminaires et conférences,* 5. Arch. IHÉS.

> sciences. . . . Too great specialization will not be accepted. . . . If we want to obtain a high level, this entails multidisciplinary activities.[176]

Motchane discussed the matter further with Thom and Michel, and on October 19, 1972, he wrote to Jacques Ballet, the new president after Grandpierre's death in July, that the moment had come to set the Third Section up. He suggested that Thom, who had accepted, be nominated as permanent professor in the methodology of the sciences of man. In a note written at the same moment, he explained that setting up the Third Section was becoming imperative in view of "the tendency to establish pluridisciplinarity as a general rule in Universities and research institutions and the disposition to favor budget priorities in the same direction." The work of Thom and others represented, for Motchane, "real progress" that had come in the last few years. The IHÉS should capitalize on its success.[177]

On October 6, at meeting between Motchane and Thom, a commission including them both as well as Lévi-Strauss was envisioned to study the feasibility of the Third Section.[178] For Thom, the principal difficulty was to find the right persons for this endeavor.

> It is difficult to find [in the human sciences] elements that are simultaneously *serious* and *brilliant.* One will perhaps find good, serious specialists (like Debreu in Mathematical Economics) or, with more difficulty, brilliant, but not too serious, people (like certain fashionable structuralist, not to name anyone). . . . Sooner or later, we will have to chose between the 'serious' and the 'brilliant'. Our scientific training would surely make us prefer the former to the latter. But the necessity of

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[176] *Rapport d'un entretien téléphonique Nicolaas Kuiper-Léon Motchane* (19/6/72). Arch. IHÉS.

[177] Lettre de Léon Motchane à Jacques Ballet (19/10/72); *Note de Léon Motchane* (23/10/72). Arch. IHÉS.

[178] N. Kuiper, *Note sur les activités de l'IHÉS dans d'autres domaines (Méthodologie des sciences de l'homme)* (14/11/72). Arch. IHÉS.

> impressing potential contributors might lead us temporarily to carry out the converse policy.[179]

In this case, Thom noted, a problem raised by Louis Michel would make this solution difficult to contemplate. Indeed the theoretical physicist incisively noted that the Yvette valley truly was a "desert" as far as the human sciences were concerned. How, then, could the IHÉS hope to attract a flamboyant intellectual humanist in the French tradition?

Motchane was almost alone in truly wanting to devote much effort to this matter. While Kuiper accumulated arguments working against a formalization of the IHÉS activity in the humanities, Thom surprisingly remained unreceptive to the idea. He found that "the social sciences as a science, or better what concerns their methodology, are practically non-existent." But "content with the present situation," he preferred "the silent, humble way of development."[180]

This sheds light on Thom's attitude regarding the establishment of a research school around himself. For all his projecting the image of a public intellectual, in the French sense of the term, Thom tried to remain, to use his expression, a "serious" scientist more committed to the building of abstruse theories expressed in highly technical mathematical language, if not always using its techniques, rather than to the gathering of an entourage of devoted disciples. Because of his scientific training, Thom would not commit the same mistakes as those he called "fashionable structuralists."

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[179] Lettre de René Thom à Léon Motchane (10/11/72). Arch. IHÉS.
[180] N. Kuiper, *Note sur les activités de l'IHÉS dans d'autres domaines (Méthodologie des sciences de l'homme)* (14/11/72). Arch. IHÉS. Original English.

In December, the Administrative Board of the IHÉS charged Motchane, Kuiper, and Thom of examining the question of the Third Section. The report Kuiper produced on September 26, 1973 voiced the opinion that a clearer conception of the position of the Third Section had to be found. He opposed Thom's nomination on the ground that his theories still needed "considerable work before they are ready for more general 'consumption' by scientists of other disciplines." In short, he was proposing to let die the issue. Thom agreed to give more rigor, "i.e. by not accepting any crazy people anymore," to his theoretical biology seminar. He indicated that he was looking for a permanent professor to occupy a chair in the Third Section.

> The ideal case would be to find a personality coming from the Human Sciences per se (Sociology, Ethnology, Linguistics?) but with a solid mathematical training, and the ability to apply mathematics judiciously. . . . No need to say that I have not yet found this rare bird.[181]

In this circumstance, it is not surprising that nothing was done regarding the Third Section until the early 1980s.[182] In summary, the above has shown that while Thom hardly was interested in establishing a research school either focusing on mathematical problems or applications of catastrophe theory to the human sciences, the IHÉS nonetheless served as a catalyst for the development of new modeling practices that attracted the attention of some important mathematicians, including Grothendieck and

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[181] R. Thom, *Note sur la 3e section* (27/9/73). Arch. IHÉS.

[182] Thom only became Professor of mathematics and methodology of the sciences of man in 1980. *Comité scientifique* (15/3/80) and *Conseil d'administration* (5/5/80). With the help of Jean Petitot, he then elaborated a detailed research program for the humanities. R. Thom, *rapport sur la 3e section;* and *Programme de recherche présenté par Jean Petitot dans le cadre de la 3e section* (n.d. [1980]). Arch. IHÉS. About Petitot's early involvement with Thom's catastrophe theory, see the two special issues of *Mathématiques et sciences humaines,* 15(59) (Fall 1977); and 16(64) (Winter 1978).