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C^0 SYMPLECTIC TOPOLOGY

FRANÇOIS LALONDE

Presented by George A. Elliott, FRSC

ABSTRACT. In this paper, I explain the emergence of Symplectic geometry and Symplectic topology, as it occurred historically, from three sources: classical and quantum physics, complex Algebraic geometry, and String theory. Symplectic topology is nowadays one of the most fascinating subjects in mathematics, and has reached since the 1980's a maturity that deserves the attention of all mathematicians and physicists. It combines topology, geometry, non-linear partial differential equations or relations, A^∞ algebras, and String theory in a powerful setting that addresses some of the most elusive questions of our times. It is made of soft h-principles coupled with hard transcendental, rigid, moduli spaces of solutions to PDE's on manifolds. I will end the paper with some conjectures in Symplectic topology, after explaining the difference between smooth and C^0 Symplectic topology.

RÉSUMÉ. Dans cet article, j'explique l'émergence de la géométrie symplectique et de la topologie symplectique, en suivant leur naissance et leur évolution au cours des siècles, à partir de trois sources: la physique classique et quantique, la géométrie algébrique complexe, et la théorie des cordes. La topologie symplectique est aujourd'hui l'un des domaines les plus fascinants de la recherche mathématique mondiale et a atteint, depuis les années 1980 une maturité qui mérite l'attention de tous les mathématiciens et physiciens. Elle rassemble la topologie, la géométrie, les équations aux dérivées partielles non-linéaires, les A^∞-algèbres et la théorie des cordes dans une théorie puissante qui s'adresse aux problèmes les plus subtils de notre époque. Elle est faite du h-principe topologique, une théorie "soft", couplée à un vaste ensemble d'espaces de modules d'EDP sur les variétés, que l'on peut qualifier de "hard" ou transcendental. Je conclurai cet article avec quelques conjectures en topologie symplectique après avoir expliqué la différence entre les topologies symplectiques lisse et C^0.

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1. Introduction

Symplectic[1] geometry was born in the early XVIII th century, just a few years after Newton's works, in the French school of mathematics and mechanics. After Euler's and Maupertuis' works, it became obvious that mechanics are just incalculable if one starts from Newton's laws. More deeply, the notion of Newton's forces was not commensurable with a mathematical vision of the physical world. One can see this immediately: the law $F = ma$ could be understood as just a definition of the force, not a law at all. It is obvious that, as long as gravitational forces are concerned, the only way to measure a force is by its acceleration a on a body of mass m. Therefore one could say that this tautology is of no use. In response to this lacuna, Maupertuis suggested that the physical nature of matter obeys a minimal principle, the principle of least action. This was just in time, because Euler and the Bernoulli family were precisely setting up the calculus of variations that is central to Maupertuis' point of view. In a nutshell, one could say that the breakthrough in mathematics justified and inspired the new vision of the physical world. In the last quarter of the XVIII th century, Lagrange gave a formal definition of what should be the ultimate understanding of the physical world in his treatise *Mécanique analytique*. Here I should comment on my statement that the law $F = ma$ is of no use. Actually, if the left hand side, the force, is electromagnetic, for instance an electron in an electric field, then the left hand side expresses a property of the electric charge, while the right hand side expresses the reaction of the inertial mass of the electron to that force, based on the mass of the electron. This is useful and leads to differential equations that will determine the motion of the electron. But if the left hand side is a gravitational force, say a spring in the terrestrial gravity field that we use to measure weight, then the mass on the spring measured on the left hand side is a gravitational mass, while the mass on the right hand side of $F = ma$ is an inertial mass that applies to all forces, not only gravitational forces. It turns out that both the gravitational mass and the inertial mass seem to be equal. It is still a mystery that both masses are equal, and every five years, classical and nuclear experiments confirm the extraordinary equality of these two distinct masses, up to 18 significant digits.

Now let us go back to Lagrange. When I read that treatise when I was young, I was shocked that Lagrange had hidden all main concepts. Like Newton who wrote his Principia in the form of Euclidean geometry, to avoid the critics, mainly the brillant bishop of Berkeley who criticized his differential calculus (and

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[1] The word "symplectic" is a modern version, introduced in the XX th century, of the word "complex" that has been used for complex numbers for the last five centuries and then entered the current language before being used in psychoanalysis. One should appreciate the word "symplectic" made of two Greek roots, since it is inappropriate to mix a Greek root with a Latin root to forge a new word. For instance, the American neologism "polyamory" is awful. Indeed, here the word "complex" comes from the adjunction of the Latin word "cum" (with) to the Greek word "plektos", while "symplectic" has both Greek origins: "sun" meaning "with" and "plektos". In both "complex" and "symplectic", the sense in ancient Greek or in Latin is "embrace" with the idea that we embrace something different from us, and therefore complicated, whence the notion of facts emerging from different elements, that could be intricated. And, as we will see in this article, this is indeed what Symplectic topology is.

he was right to do so), Lagrange did the same: instead of basing his theory on the least action principle, he set it up through a brilliant, but cumbersome, theory of statics. The point of view of Lagrange is to express all dynamical laws of physics through statics. For this, he introduced the first notion of differentiable manifolds, that he called "generalised coordinates", sixty years before Riemann. For example, a double pendulum, has coordinates in the product $S^1 \times S^1$, and Lagrange went far enough to express the configuration space of motions through arbitrary coordinates that were, in modern language, a quotient of Euclidean space constrained by some mathematical formulas. This indeed leads to the modern notion of manifolds.

The *Mécanique analytique* goes in this way: one starts with a physical system, expressed in generalized coordinates, and one assumes that the system is at equilibrium. Therefore, there is no motion at all. Then, by calculating the virtual motions of each of its parts, Lagrange computes the laws of dynamics!! It is a counter-intuitive way of considering dynamics, and it works. More important, and just in his notes at the bottom of some pages, Lagrange, in just a few sentences, says that this amounts to using the principle of least action. Nothing more. Just a remark.

Nowadays, the Lagrange theory of mechanics, assuming that we use it in the form of its least action principle, is the foundation of all physics, classical and quantum. The law is that any physical system minimizes the action integral which, in physical terms, is the integral over any path from a state a to a state b of the difference of potential and kinetic energies, computed along time. This integrand has now an intrinsic definition that does not require the limited concepts of potential and kinetic energies.

Twenty years later, Hamilton gave another definition of physics, which is equivalent to Lagrange's version, but this time using only the total energy of a system. This equivalence is given by the Legendre transform. It is much simpler conceptually. Although there is a least action principle version of Hamilton's theory (which was detailed in Arnold's book *Mathematical methods in classical mechanics*), Hamilton's theory cuts out the detour through the action integral and uses the Euler-Lagrange equations of the calculus of variations, to get immediately the right ordinary differential equations that govern a physical system. These equations are very simple: one starts with the Lagrange configuration space of the positions of the system, expressed as points on a manifold, and then extends that space to englobe the speeds of each point of the system, which would naturally lead to the tangent space of the configuration space, therefore doubling the dimension. But for mathematical reasons, it is natural, in Hamilton's theory, to take the cotangent space instead of the tangent Lagrange space. If there is a natural metric given on the configuration space, then there is a natural equivalence between tangent and cotangent spaces. So then, in this setting, Hamilton's theory just says that a point in the cotangent space (that is, a point for which we know the position and momentum, or equivalently, in the presence of a Riemannian metric, a point and its initial speed) follows the Hamiltonian equations of motion which, in a symplectic setting, just means that the dynamics is given by following the symplectic gradient (which, in the presence of a Riemannian metric compatible with the symplectic form, is just a kind of orthogonal to the ordinary gradient).

From a deep mathematical, and philosophical, point of view, the Euler-Lagrange equations are just incredible. They mean a kind of teleonomy of the universe. These equations mean that, if a particle wants to travel from a to b in minimal integral action over the path, the control for this is assumed pointwise in time and space, even if the particle does not know its goal! Of course, this apparent paradox is resolved by understanding that most particles will miss the goal that we have in mind. But every particle, independently of our will, will reach a random goal with minimum action integral. The first example of this is Fermat's discovery that the Snell-Descartes refraction law is just given by choosing the path that minimizes the time of the light path. Fermat is one of the greatest mathematical minds of all time: in 1625, twelve years before Descartes' Geometry (1637), he had arrived at most results that Descartes discovered. Moreover, he stated the first minimal action theory. He was the first to give a precise definition of the derivative of a function, exactly. Indeed, in his terms, the derivative of f is the limit of $f(x+E)-f(x)$ divided by E, when E tends to 0 (his letter E stands for "Error"). Moreover, he set up the theory of numbers, not in the sense of Diophantus, but analyzing numbers for their own sake.

What is most interesting here, from a historical point of view, is that Hamilton's discovery depends heavily on Lagrange's works of course, but also as deeply on Huygens' discovery of the notion of energy at the end of the XVII th century, just fifty years after Descartes, and before Newton. Huygens called energy "forces vives". It was the first time in the history of science that the notion of energy arose. Nowadays, this notion is used by all human beings, and nobody knows what it means: it is an integral of force over distance, and therefore its physical units are mass × acceleration × distance, so $kg\ meter^2/(sec)^2$, a very awkward concept for most people[2].

As an anecdote, the letter H is always used to denote the total energy of a physical system. And everybody thinks that H stands for Hamilton. But Hamilton was four years old when that letter for the energy was introduced! It was Lagrange who first used it in celebration of Huygens. Of course, one could

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[2] The same awkward understanding arose in the notion of *stress* that is so important in General relativity where the diagonal is made of energy while the off-diagonal terms are made of stress; Hans Selye in the 50's and the 60's has introduced the term "stress" as a purely scientific definition of stress in animals and human beings, a notion that was not pejorative. Today, like the concept of energy, nobody understands this as it should be understood. Another example is the very subtle notion of temperature: last week, when my daughter, ten years old, worked on an exercise in mathematics at school, she had to solve: 0 degree centigrade plus 32 degrees Farhenheit!! The authors of this monography of mathematics for kids expected the answer to be 0 degrees centigrade plus 0 degrees centigrade is equal to 0 degrees centigrade. But of course, even in Kelvin degrees, one cannot add temperatures. This has no physical meaning.

go far in the history and greet Aristotle for the first notion of energy. I do not think so. Aristotle, in his writings on physics (and on any other subject) is ambiguous and reading him is always disappointing. He developed the notion of "puissance dans l'acte" but this notion is far from the beauty and precision of Huygens' definition. The wonderful philosophy and mathematics in Ancient Greece spread over almost one millenium. Pythagoras, Zeno from Elea, Eudoxus, Archimedes, Apollononius. Here, in the context of this article, I mention the fantastic discoveries of Zeno, for whom Socrates and the peripathetician school had the highest admiration. Zeno is phenomenal. He responded to the everlasting question in Ancient Greece as whether the world is atomic in nature, or continuous. He knew that there was no accessible answer for this question in his current times. But, instead of taking a party, he destroyed both parties. He did that in two paradoxes. The first one destroys the thesis of continuity: this is the well-known paradox of Achilles and the turtle. The second one, much more important, and practically unknown, is his paradox destroying the quantum conception of matter and time. Here it goes: imagine three rows, each one made of equal squares, of quantum minimal length, each row one upon the other. Thus we have say the first row A_1, A_2, A_3, and then the row B_1, B_2, B_3, and finally the row C_1, C_2, C_3. They stand one over the other, and aligned. At minimal quantum time t, the row B moves by one quantum square to the right while the row C moves by one quantum square to the left. This has perfect sense seen from A. But then, from the C point of view, B has moved two quantum distances from C in one unit quantum time, that is one quantum distance in half a quantum time. Contradiction!!! In this reasoning, Zeno surpasses Galileo's relative theory, two millennia before Galileo. It is funny to close this introduction with Galileo because most scientists consider his theory of classical relativity or his theory of balls rolling on some inclined plane as the summum of his works. I do not believe so. His most brilliant observation is a purely intellectual experience: he proved that the acceleration of a falling body (a "grave" in the terms of the XVII th century) is independent of its mass. The experiment, that he has never needed to perform, is to imagine two masses, say two one-kilogram bricks, falling from the tower of Pisa. In the first experience, the two bricks are loosely attached by a string, and they fall as if they were independent. In the second experience, the string brings together the two masses, with no contraints. In the third experience, the two bricks are attached as to form a two-kilogram mass. It is clear that in all three experiences, the masses will fall with the same acceleration. Beautiful!

There is no better way to end this introduction than ending it with questions. Like all mathematicians, I ask myself hundreds of questions endlessly. Most of these questions cannot be understood by a random person whom I meet on the street. But some of them can. Here are four questions. The first one is "Why something exists instead of nothing?". Since mathematicians are always looking for the simplest solutions, it is obvious that it would have been much more "natural" that the universe, the world, had never existed. I asked that question to Gromov and his reply, in just a single line after five minutes, was the only one possible: “You ask the question, then you have got the answer!” Splendid!

Another question, that anybody can understand, is: “Why does a mirror reverse right and left, but not head and bottom?” Strangely, 99.9 per cent of the population do not even understand the question. I must say that it took me twenty minutes of hard reflection to solve it.

Another one, that I encountered in practice while working at home on renovations in my house is this: observing that wires and ropes have a fantastic ability to become a mess, I wondered if it were possible to imagine a rope, attached at its two ends, with a non-trivial knot on the left, and a non-trivial knot on the right, so that by manipulations, I could cancel both of these knots. This is again a question that anybody can understand. The answer is no. This can be proved by Seifert surfaces. Of course, this also answers the question that one cannot produce two genuine knots from nothing, whence the answer to my observation on the mess of wires and ropes. Actually, the mess is only apparent (it is unknotted). However, one should note here that this mess is always produced by the way we use long wires or ropes: the intrinsic torsion is tranformed into rotations. This is why, in navigation, one always stores a long rope in a figure eight around two pieces of stick, cancelling the torsion.

In the first question, about the existence of the universe (and therefore about my own existence), the question is not well-posed. However, in the last two examples, the questions are well-posed.

Here is my last question. It is well-posed. I consider that question as one of the most interesting ones and I do not expect a solution in the current century. This question is unfortunately not accessible to non-mathematicians. The question is: ”Why is it so that all rich geometric or physical theories always rely, ultimately, on 2-tensors?” In other words, the mathematical world, in all dimensions, always gives a special status to real dimension 2. Riemannian, complex, symplectic, algebraic structures always rely on something of dimension 2. One could of course argue that it due to the human nature, since we see things in dimension 3 which means that the only curved objects of intuitive interest are surfaces. But this is a wrong answer. Another guess would be to declare that the complex numbers, of real dimension 2, form the most wonderful field of numbers, or that real dimension 2 is the only dimension where conformality implies integrability. None of these answers is satisfying. Things seems to be much deeper.

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2. **Geometry and Topology**

What is the difference between geometry and topology? Suppose that you are given a differentiable manifold, or any space that you like, with some structure. Then this space, with its structure, is called a geometry if there are pointwise or local invariants. The best example is a

Riemannian manifold, or more generally a space with a distance (or metric, it is the same). In this case there are local invariants, for instance the curvature. Note that a Riemannian structure, which is by definition the data of a scalar product on each of the tangent spaces, data that vary smoothly with the point at which the tangent space is given, gives a notion of areas, volumes, and hypervolumes in all dimensions until one reaches the full dimension of the space under consideration. In particular, it gives the notion of geodesics, which is in General relativity exactly what one needs to compute the shortest path followed by any particle.

It turns out that geometry is far less present in mathematics than topology. A space, say a manifold, equipped with some structure, is called a topology if there are no local invariants. If one accepts that definition, then obviously most interesting spaces in the mathematical world are topological. Let's take some examples: a real differentiable manifold is by definition made of charts, locally diffeomorphic to some open subset of Euclidean space. And then, by definition, there are no local invariants. The same applies to complex manifolds, which are locally holomorphically diffeomorphic to subsets of complex Euclidean space. Another fundamental example is symplectic manifolds (I will get back to this in the next chapters).

Thus, it is far easier to work with geometry! For instance, one could use easily the integral of the curvature, or any Riemannian object, to distinguish between two geometrical spaces. The classification of these spaces, as hard as it can be, is easier. Actually, geometry is not made for distinguishing spaces. It is made to work on one space, which is given by some mathematical or physical considerations, and calculations, like the one given by General relativity. In a nutshell, geometry is a practical study, not a mathematical endeavour of classification.

Topology is completely different in its spirit. Without any local invariants, the classification of spaces is an incredible task. Let's take the recent fundamental works by Gromov, Donaldson, and Witten (and many others). The point in all of these works is to classify manifolds with a topological structure (in the above sense). In the absence of any local invariant, one needs to look at global invariants that cannot be the integral of local ones. What do you do? The first attempt was made by De Rham who introduced a cohomology, on any differentiable manifold, made of differential forms that should, a priori, detect the smooth structure of the manifold. It turns out that this cohomology is isomorphic to the cohomology of the topological underlying space. Therefore this cohomology cannot detect differential structures on the same underlying topological manifold. It was a failure. Of course, this isomorphism is now fundamental and De Rham and Hodge cohomologies have led to the most sophisticated researches nowadays, since Grothendieck, especially in algebraic geometry. Three of the last four Fields Medals were directly or indirectly awarded on this subject.

The reader should just understand that "differential geometry" should be called "differential topology" and that "complex geometry" should be called "complex topology". In the same vein, "symplectic geometry" should be called "symplectic topology".

3. Symplectic Topology Here again, one has the same question: what is the difference between symplectic geometry and symplectic topology?

The answer is still the same: symplectic geometry is a geometry, while symplectic topology is a topology, according to the definition above. But, historically, mathematicians worked on given Riemannian manifolds, endowed with a symplectic structure. In their minds, the symplectic structure was just a tool to compute, on a given Riemannian manifold, the laws of physics. The underlying symplectic structure was just an accessory tool, a tensor, that was useful for calculations. It was therefore a geometry because it was always associated to a Riemannian metric. Here the ambient Riemannian manifold was given, and the question of classifying the symplectic or Lagrangian objects within that ambient space was not on the radar, let alone the idea of classifying the ambient symplectic manifolds that was not considered at all. It is only in the 1970s and 1980s that this endeavour emerged, when the mathematical community realized that symplectic topology is a subject per se, for which one should define invariants that distinguish, and hopefully classify, all symplectic manifolds, and all relevant objects inside these manifolds.

It is time to give the definition. A symplectic manifold is a real smooth C^∞ manifold M (compact or not, with or without boundary) endowed with a symplectic structure, that is to say a real smooth 2-differential form ω that has two properties:

(1) ω is non-degenerate at each point p. Precisely, for each $p \in M$, the bilinear form ω_p on $T_p(M)$ is anti-symmetric and satisfies: the map that assigns to each $v \in T_p(M)$ the real linear form $w \mapsto \omega(v, w)$ in the cotangent space $T_p(M)^*$ is a real linear isomorphism. In other words, given v, the number $\omega(v, w)$ vanishes for all w only when $v = 0$.

(2) the differential form ω is closed: $d\omega = 0$, where d is the usual exterior derivative.

Note that the first condition is a pointwise condition on ω, while the second is a local condition expressed by a system of PDEs. It is trivial to construct a theory that obeys only one of these two conditions. But both together, they lead to a rich and very subtle theory, called Symplectic topology. Actually, the first condition is needed for the following simple reason. If $H : M \to \mathbb{R}$ is a Hamiltonian function, i.e any real smooth function (that, implicitly, one considers as the total energy of the system, but we do not care), then the differential of H at each point $p \in M$ leads to a unique vector given by the correspondence in the condition (1), called the symplectic gradient of H at the point p, noted $X_H(p)$. This is the same correspondence as the one given by the ordinary gradient, but it is morally orthogonal to it. This means for instance, that if H is defined on a real surface, the symplectic gradient is parallel to the levels of the function H, and therefore perpendicular to the ordinary gradient of H. This just expresses the fact that the motion of particles in physics preserves the total energy. Note finally that, by basic multilinear algebra, if a V is a real finite dimensional vector space, and if there is on V an anti-symmetric non-degenerate bilinear form, then the real dimension of V is even. This is the first hint of the rich relation between symplectic manifolds and complex ones.

The second condition is needed so that the Hamiltonian flow of H, which is just the flow generated by the vector field X_H, be such that its integral for each time t, $\phi_H(t) : M \to M$, be a symplectic diffeomorphism, i.e a diffeomorphism that preserves the form ω which means that $\omega(\phi_t(v), \phi_t(w)) = \omega(v, w)$ for all tangent vectors v, w belonging to the same tangent space of M at any point $p \in M$.

Examples: Note that any cotangent space $M = T^*V$ of a real smooth manifold V is naturally equipped with a symplectic form, which is simply the differential of the canonical obvious 1-form λ that assigns to each tangent vector $v \in T_{(q,p)}(T^*V)$ the number $p(\pi_*(v))$, where π is just the differential of the canonical projection of T^*V onto V. Here (q, p) is the base point of T^*V, where q belongs to V and p is the linear 1-form on the tangent space $T_q(V)$. This 1-form λ is the only canonical (I would even say tautological) form that one can imagine. One just applies the linear form to the projected vector. In local coordinates, that are made so as to extend the chart on T^*V as a differential of any chart of V, this is expressed as $\lambda = \sum p_i dq_i$, and therefore the symplectic structure is given in such a chart by $\sum dp_i \wedge dq_i$.

Another example is any Kähler manifold, and in particular any complex submanifold of complex projective space. I will go back to this example later. But at this moment, let's observe that the integral on a real surface, say the Euclidean plane, of the form λ on a loop in $\mathbb{R}^2$ is the area of the surface enclosed in that loop by Stokes' formula. Indeed, that integral of λ on the boundary is equal to the integral of ω (which in 2 real dimensions is just an area form) of the area of the surface enclosed by this loop. This is just an instance of Stokes' formula that says that if a bounded piece of a surface A has boundary ∂A, then the integral of $\omega = d\lambda$ on A equals the integral of λ on ∂A[3].

These two classes of examples cover most interesting examples in classical

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[3] One of my postdoctoral advisers, René Thom, told me that, in his opinion, De Rham theory and Stokes' formula, taken together, were the most beautiful achievements in the history of mathematics. Indeed that formula makes the interior accessible by the sole knowledge of the exterior, a theme that I will develop later in this article at a deeper level. As a personal note, when I was a NSERC-NATO postdoctoral fellow, René Thom and Henri Cartan, whose office I occupied at Paris-Orsay in 1983-1985, now called Paris-Saclay, were the last two scientists with whom I could discuss all mathematics, a large part of physics, some fundamental aspects of biology, a part of philosophy, with a deep knowledge of Latin, ancient Greek, several vernacular languages and their literatures. As an anecdote, when the newspaper *Le Monde* published an article on Henri Cartan's celebration of the 100 th anniversary of his birth, the journalist thought that he was dead; actually Cartan gave a splendid talk on this occasion!

mathematics (that is to say, before Symplectic topology).

Now let's play with multilinear algebra. Suppose that α is an anti-symmetric non-degenerate bilinear form on a real vector space V. Let W be a real vector subspace of V. Then we define the symplectic orthogonal $W^{\perp_\alpha}$ as the vector subspace of V that annihilates W: that is, v belongs to $W^{\perp_\alpha}$ if and only if $\omega(v, w) = 0$ for all $w \in W$. By the nondegeneracy of α, this orthogonal is of supplementary dimension. But, contrary to an ordinary orthogonal for a scalar product, this subspace may intersect W in a subspace larger than $\{0\}$. We say that W is α-symplectic if the restriction of α to W is non-degenerate, which implies that the intersection of W and $W^{\perp_\alpha}$ is $\{0\}$, like in the case of the scalar product. We say that W is isotropic if W is included in its α-orthogonal, and coisotropic if W contains its α-orthogonal. We say that W is Lagrangian if its orthogonal is equal to W. A Lagrangian subspace is always of half dimension of course. Note that any real hyperplane is automatically coisotropic.

Finally, going back to our symplectic manifold M, the same definitions hold for submanifolds N if they hold in each tangent space T_pN of the submanifold, viewed as a subspace of T_pM. In particular, any real codimension 1 submanifold N of a symplectic manifold is coisotropic (its symplectic orthogonal is included in itself, as a vector field, called the "kernel" of N).

Now let's go back to history. Until the 1970s, when only Symplectic geometry existed, all examples of symplectic manifolds were of just one kind. Indeed, from Lagrange and Hamilton considerations, they are always tangent (Lagrange) or cotangent (Hamilton) spaces of some smooth real manifold V, called the configuration space of the physical system, that simply encodes all possible positions of the system, like $S^1 \times S^1$ for the double pendulum or $(\mathbb{R}^3)^k$ for the position of k masses, like planets. With the works of Emmy Noether at the beginning of the the XX th century, it became clear that some "nice" systems obey some symmetries that one could quotient out (this is called today the Marsden-Weinstein quotient). The point is that, even if one starts from a cotangent space, which is obviously not compact, one can end up with compact manifolds, after this quotient. In many cases, these quotients were Kähler manifolds, which is just a generalization of complex projective varieties. The theory of this kind is called "Integrable Hamiltonian systems". The word "integrable" here is used to say that there are enough symmetries (called first integrals) that Poisson commute with H, so that one could compute the trajectories of the symplectic gradient of H, that gives the motion of particles, in simple ways, that are simple conceptually, and lead to elliptic integrals, studied by Jacobi. Precisely, a system on some cotangent space over a configuration space of dimension n, is integrable if there are real functions $H = H_1, H_2, \ldots, H_n$, where H is the total energy, such that all symplectic gradients are linearly independent at each point of the cotangent space (or at least in a subspace of full measure) and they all Poisson commute: the differential of any of these functions vanishes pointwise on the symplectic gradient of any of these functions. In clear terms, this means that any of these functions is preserved along the symplectic flow of any other. It is customary to call the $H_i, i \geq 2$, the momenta of H.

The first occurrence of a step toward Symplectic topology appeared when Poincaré, at the beginning of the XX th century, studied the analytic theory of deformations of integrable systems. I say "analytic" because he used only analytic functions. He was interested in the three-body problem. As everybody knows, Newton had solved the two-body problem. And one could expect a priori that the three-body problem would be as simple to solve. After all, we have the Hamiltonian equations and the full mathematical set-up to resolve it. What happened there is fantastic. We all know that a closed physical system has a total energy that does not vary in time, lets call it H, say, for the three-body problem. But the actual, and simplest, solution to the three-body problem involves a reference frame that requires the total energy to vary with time. Therefore, one gets a new Hamiltonian $H : I \times T^*V \to \mathbb{R}$, where I is a time interval that one could take as the entire real numbers. That new Hamiltonian is simpler to study, but now it depends on time. If we call a Hamiltonian autonomous if it is time-independent, then mathematicians shifted their attention to non-autonomous ones. And, as we will see, this is far more interesting and richer, mathematically speaking.

In the first half of the XX th century, most mathematicians, especially in pure mathematics, were driven by the new concepts introduced by Poincaré, not for the sake of Hamiltonian systems, but rather for Poincaré's introduction of Topology, that he had used in his study of mechanics. From that point, Topology was born, and it occupied an immense place in the first half of the century, forgetting the reason why Poincaré had introduced Topology. But Mechanics will take a revenge, and will come back in the 60's, 70's and 80's in the highest form of topology. This will give birth to Symplectic Topology, that one can certainly consider as one of the highest branches of topology nowadays, if not the richest and most profound, according to one's personal taste.

After a period of sleep in the first half of the century, Kolmogorov, Arnold and Moser established in the 1950s and 1960s the KAM theory that studied what remains when one slightly perturbs an integrable Hamiltonian system. Here the symplectic form remains the same, but the functions H_i, $i \geq 1$, are perturbed in a C^1-way, which means that both their values and their first derivatives stay close to the original ones. Their result is that some tori, on which the original motion of H took place, are preserved. But they did not study what happens if one goes far from the first integrals, let alone what happens if one perturbs the symplectic form itself. Moreover, their Hamiltonians were only autonomous.

What is fascinating here is that a non-autonomous Hamiltonian does not behave at all like an autonomous one. Actually, one could naively think, a priori, that the study of non-autonomous Hamiltonians could be easily reduced to autonomous ones by just adding the real coordinate that represents time. Or, in other words, one could just rely on the fundamental theorem of ordinary differential equations that applies as well to non-autonomous vector fields. But Arnold asked in the 1960's if a non-autonomous real function $H : I \times M \to \mathbb{R}$, where M is a closed symplectic manifold, generates a flow of symplectic diffeomorphisms at each time t, $\phi_H(t)$, that has always at least as many fixed points (that is closed orbits) as the sum of the Betti numbers of M. This is obvious for an autonomous Hamiltonian, because Morse theory shows that the number of critical points of a real Morse function is always larger than or equal to the sum of the Betti numbers. Since, in the autonomous case, all critical points of H are trivial fixed points of the flow, this establishes the result. But, in the non-autonomous case, it is obvious that all points of M could move under the time-varying flow of the symplectic gradients of H_t. Therefore Arnold's conjecture is far from being trivial, and the proof of it requires highly sophisticated tools, that were provided by Gromov and Floer in the 80s, leading to Floer's cohomology on holomorphic curves, that is to say open Riemann surfaces, with specific boundary conditions and deep regularity and compactness theorems on non-linear elliptic operators, similar to the non-linear Cauchy-Riemann operator. The main idea in Floer's theory is to build a Morse theory in infinite dimension on the space of all loops in M, and then consider on it the action functional whose critical points are exactly the closed orbits of each ϕ_t. The problem here is that the Morse index of each critical loop, as well as its co-index, is always infinite. But the relative index is finite (this was first observed by Atiyah, in another context, in his study of Gauge theory). The differential is built by considering cylinders that are pseudo-holomorphic for an associated almost complex structure compatible with the symplectic structure, going from a periodic orbit to another one. Floer then shows that the resulting cohomology is isomorphic to the ordinary singular cohomology of M (and is independent of the accessory almost complex structure). Whence the result.

So what we see here, historically, is a complete shift in mathematical research. Before the eighties, one was interested in studying very particular Hamiltonian dynamical systems, with a good grasp on calculations (this is Symplectic geometry). But, since then, one is also interested in Symplectic topology, in studying qualitative (and asymptotic) features of symplectic manifolds, and all questions of invariants and classification that apply not only to specific cases, but to all cases.

So let's go forward. It is difficult, and in some sense impossible, to classify symplectic manifolds, or their open subsets, or their Lagrangian or isotropic or coisotropic submanifolds, without adjoining accessory structures to them. Then, as usual, one will have to show that the invariants defined through these accessory structures, are independent of these, and therefore are genuine symplectic invariants.

We need to associate accessory structures to a given symplectic manifold (M, ω). Of course, the two best structures are complex and Riemannian structures. The choice of these structures is elementary and relies only on linear algebra (and Lie groups). If (V, α) is a real vector space endowed with a non-degenerate anti-symmetric bilinear form α (this α will be ω later when we pass to manifolds), hence of even real dimension, one can make this real vector space complex, by considering any J-structure compatible with α. Here J is a real automorphism of V that satisfies $J^2 = -identity$ (actually this requirement implies that J is one-to-one) such that α is J-invariant: $\alpha(v, w) = \alpha(J(v), J(w))$ for all $v, w \in V$. Such a J transforms V into a complex vector space by the rule: $i(v) = J(v)$. Since $J^2 = -identity$, like $i^2 = -1$, this gives a complex structure. Now, by elementary Lie group theory, one observes that the space of such α-compatible Js is non-empty and forms a finite-dimensional contractible space (it is a homogeneous space). And then one can define a scalar product by the rule $g(v, w) = \alpha(v, J(w))$, which is also J-invariant. Conversely, if J is given, one recovers α from g by $\alpha(v, w) = g(J(v), w)$. In summary, once α is given, one can endow (V, α) with a contractible space of compatible structures (V, α, J, g), where α is fixed. The fact that this space is non-empty and contractible is fundamental.

Now, after this interlude on linear algebra, one can consider symplectic manifolds. If (M, ω) is a symplectic manifold, one can always assign to ω an accessory set of structures (ω, J, g) on M that forms a non-empty contractible infinite-dimensional space. This is obvious because the space of all such ω-compatible structures (J, g) forms a fibration over M with contractible fibres, a Serre fibration. Thus a section always exists and all of these sections form a non-empty contractible space. With this in hand, the goal is to endow (M, ω) with an accessory structure (ω, J, g), then define invariants depending on the choice of J (and therefore on g since g is determined by both ω and J), and show finally that the invariants are independent of the choice of J, that is to say they are genuine symplectic invariants. Note here that such a section, i.e., the choice of a J-structure compatible with ω, is in general not integrable in the sense of complex structures. Indeed these J-structures are called almost complex structures, and they are integrable if, by definition, there are local diffeomorphisms that send J to the standard complex structure of $\mathbb{C}^n$, which means that, everywhere on this chart, J commutes with the multiplication by $i = \sqrt{-1}$ at the level of the differential of the diffeomorphism. The point here is that, although all symplectic manifolds carry a compatible almost complex structure, most of them do not carry an integrable one. The most obvious example is the cotangent space of a configuration space. In this case, which is the fundamental example of symplectic manifolds, there is in general no integrable J-structure, except on an infinitesimal neighbourhood of the zero section.

This is the point of view that Gromov introduced in his seminal paper in Inventiones Mathematicae in 1985. In this paper, Gromov developed the theory of almost complex structures compatible with a symplectic form, and showed that, as long as one restricts to complex dimension 1, all results that hold for integrable J-structures also hold for non-integrable ones, basically because the restriction of an almost complex structure to a sub-Riemann surface is always integrable (this is due to the well-known fact that all almost complex structures on a real surface are integrable; in other words, in real dimension 2, conformality implies integrability). Symplectic topology was born.

4. C^0 **Symplectic Topology** Here is the main starting point of this article: let (M, ω) be a smooth symplectic manifold, and consider the group of all symplectic C^∞-smooth diffeomorphisms of M onto itself. Consider a sequence $\phi_i, i \in \mathbb{N}$, of such maps, and suppose that it converges in the C^0-sense to some C^∞-smooth diffeomorphism ϕ. Then ϕ is also symplectic. This splendid theorem was proved by Eliashberg in the 70s. Note here that if the sequence were supposed to be convergent in the C^1-sense, the conclusion would be obvious since the definition of symplecticity refers to the pull-back by the differential of the diffeomorphism of the 2-form ω. However, here the convergence is only C^0, which means that we only know that the values of the sequence ϕ_i accumulate pointwise to the values of ϕ, while their derivatives might stay very far from the derivative of ϕ. Such examples of C^0-convergence, with C^1-divergence, are easy to construct, just by local C^0-close, but C^1-far, perturbations.

Another way, quite succinct, to state this theorem, is to say that the group of smooth symplectic diffeomorphisms of any smooth symplectic manifold is C^0-closed in the larger group of all diffeomorphisms. What this theorem really means is that, although the symplectic form is defined at the C^1-level by the differential, there is some C^0-content behind simplecticity, that needs to be addressed. Or, explicitly, there must exist C^0-invariants of subsets of a symplectic manifold that are such that if a diffeomorphism preserves all of these C^0-invariants (or maybe just one) on all subsets (or maybe just on open subsets), that smooth diffeomorphism is symplectic.

Pursuing these reflections further, one might as well redefine Symplectic topology just on homeomorphisms. Of course, this program has been successfully carried out in Riemannian geometry: it is indeed not difficult to define a homeomorphism that preserves lengths. But Symplectic topology is much more subtle, like Gauge theory, because contrary to Riemannian geometry, Symplectic topology has no local invariants. So one faces the following challenge: how to construct C^0-invariants on subsets, from a structure that has no local invariants? This is very similar to Gauge theory that produced invariants that can distinguish different smooth real structures on the same topological manifold[4].

There are many symplectic invariants that one can define when working with accessory structures. Some of them are stated through an accessory structure, and then one proves the independence. Others can be stated directly using only

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[4] Note that some of the best mathematicians working on Gauge theory (Donaldson, Taubes) have shifted their interests to Symplectic topology. In the case of Taubes, who has interpreted the Seiberg-Witten invariants as a wonderful symplectic invariant, and has devoted half of his life to symplectic topology, I could say that his passion emerged when I gave in 1989, as a young researcher, a talk in his seminar at Harvard on Symplectic topology, where I explained Gromov's theory just published a few years before in 1985, and my own work.

the symplectic form (and then one uses accessory structures in the proof of several results). One of the most important classes of symplectic invariants is the class of capacities. The term capacity comes from physics, in the form of a capacitor, where one produces a tension from electromagnetic fields. This concept of tension is fundamental, intuitively, in the theory of symplectic capacities.

A capacity c is a function from a symplectic manifold (M, ω) with or without boundary (for instance relatively compact open subsets of $\mathbb{R}^{2n}$) to the real numbers that satisfies the following three axioms:

(1) Monotonicity: if there is a symplectic embedding of (M, ω) into (N, ω), then $c(M, \omega) \leq c(N, \omega)$.

(2) Homogeneity: for all non-zero real numbers $r, c(M, r\omega) \leq \|r\| c(M, \omega)$.

(3) Non-triviality: the capacity c on the following two open subsets of $\mathbb{R}^{2n}$ is strictly positive:
$B^{2n} = \{x \in \mathbb{R}^{2n}, \|x\| < 1\}$
and
$B^2 \times \mathbb{R}^{2n-2}$.

One could add a normalization axiom that requires that the values of c on these two open sets be equal to π.

The two main examples of capacities are (1) the Gromov capacity, and (2) the Ekeland-Hofer capacity. The first one is defined as the supremum of πr^2 over all symplectic embeddings of the standard round ball of radius r, $B^{2n}(r) \subset \mathbb{R}^{2n}$, into (M, ω). The second one catches the smallest periodic orbit on the boundary of a relatively compact open subset of a symplectic manifold, where here smallest means that the area enclosed by that periodic orbit is the smallest. In a symplectic manifold, like $\mathbb{R}^{2n}$ or any cotangent space, where the symplectic form is the differential of the Liouville 1-form λ, this area can be computed just by Stokes' theorem by integrating the 1-form λ on the periodic orbit. But one cannot a priori assume that periodic orbits exist, and therefore the definition of the Ekeland-Hofer capacity is a projective limit of the behavior of Hamiltonian functions inside the open subset. A famous conjecture, by Viterbo, states that, whatever the capacity that you choose, the round ball in $\mathbb{R}^{2n}$ always minimizes the capacity on all convex open subsets of $\mathbb{R}^{2n}$ of the same volume. Although this conjecture has been proved in many cases, it is still unsolved. A stronger conjecture by Viterbo states that all capacities agree on convex open subsets of $\mathbb{R}^{2n}$.

The set of all capacities can be ordered, from the ones that assign the least values to the ones that assign the larger values. Whatever the one that we choose, the idea is to choose the finest invariant.

It turns out that these capacities, and other symplectic invariants of the same kind, are based on the relation between the interior of the open subset, and its boundary that naturally carries a symplectic dynamical system. Thus Symplectic topology is really a theory at the interface between algebraic geometry (in the interior) and Hamiltonian dynamical systems (on the boundary). The interior is detected by Gromov-Witten invariants and different sophisticated cohomologies or field theories, while the boundary is studied through the organization by closed periodic orbits related to each other through open Riemann surfaces for a J-structure compatible with the symplectic form, with ends on each of the periodic orbits.

Let us examine first the Gromov-Witten invariants. The idea is simple. Take a symplectic manifold (M, ω), that we consider closed to simplify. Then choose a 2-homology class A in M, and k points in M. The goal is to detect the symplectic structure by real closed surfaces in class A that pass through all these points. Of course, one could choose symplectic surfaces, but the problem is that the symplectic condition is an open one, and there are infinitely many such symplectic surfaces through these points. Thus, the natural way of proceeding is to choose an almost complex structure J compatible with ω, which defines a Riemannian metric. Then count the number of J-curves (curve in the complex sense, these are Riemann surfaces) with sign $+$ or $-$ in class A that pass through the k points. This count therefore belongs to the relative numbers $\mathbb{Z}$, and makes sense only when the Atiyah-Singer index of the set of such curves is of dimension 0, and thus the number k must be chosen to reduce the dimension of these J-curves to zero. One can generalize this and define the GW-invariants for J-curves in class A, in dimension 0, passing through k cycles in singular homology. It turns out that this counting is independent of J, and is therefore a symplectic invariant.

Three remarks are in order.

The first one is that since the group of symplectic diffeomorphisms acts transitively on k-tuples of points, the GW-invariant is just a symplectic invariant of the manifold, and not of the relative position of these points.

The second one is that, for a generic almost complex structure J on a manifold M, there is no J-invariant submanifold N of real dimension higher than 2 (that is to say only J-curves can exist). Actually, this holds even locally. By J-invariant, I mean that for each tangent space at any point of N, the action of the endomorphism J sends that tangent space to itself. The reason for this is that a generic almost complex structure is in general totally non-integrable, but the existence of some complex J-invariant submanifold of complex dimension greater than or equal to 2, even locally, would give some partial integrals of the structure J, a contradiction. Therefore, one cannot define GW-invariants using J-complex submanifolds of complex dimension higher than 1. In other words, if J is a generic almost complex structure given locally, say in a small neighbourhood of the origin in $\mathbb{R}^{2n}, n > 1$, there is no non-trivial (J, i)-invariant map from that neighbourhood to $\mathbb{C}$ endowed with its usual complex structure $J =$ multiplication by $i = \sqrt{-1}$ (for $n = 1$, we know that all almost complex structures are automatically integrable and therefore locally diffeomorphic to $\mathbb{C}$).

Thus Symplectic topology is a covariant theory, while algebraic geometry is a contravariant theory. By this, I mean that in algebraic geometry, there are lots of local holomorphic maps from $\mathbb{CP}^n$ to $\mathbb{C}$, and one can define projective varieties by the zero set of homogeneous polynomials. This is contravariant because one defines complex submanifolds as the zero locus of a set of maps. By the famous GAGA principle (a theorem), that states that Géométrie analytique is equal to Géométrie algébrique (whence the acronym $GA = GA$, or more simply GAGA, which by the way in the current French language means to be in love with someone like crazy, say in the expression "a father is gaga over his newborn baby"), or in other words that any complex closed submanifold of $\mathbb{CP}^n$ defined by complex analytic functions is actually defined also by polynomials, the a priori wider world of analytic functions on $\mathbb{CP}^n$ boils down to the usual theory of algebraic geometry defined by homogeneous polynomials.

To the contrary, Symplectic topology, which is based on generic almost complex structures, has no non-trivial local maps from the manifold to $\mathbb{C}$. Therefore, one can only consider complex maps from a Riemann surface to that manifold, whose images, which are J-curves, cannot be obtained by the zero set of maps from the manifold to $\mathbb{C}$, even locally. Thus Symplectic topology is a covariant theory in the sense that we can only study it by the injection of maps inside that manifold. To summarize, in a nutshell, Symplectic topology is covariant in the sense that we can only study maps from some accessory object R, say a Riemann surface, to (M, ω, J), while algebraic geometry is contravariant because one can study objects in $\mathbb{CP}^n$ by maps from $\mathbb{CP}^n$ to accessory objects. Note here that Donaldson's extraordinary theory in Symplectic topology produces real codimension 2 symplectic manifolds by a kind of contravariant theory similar to the classical Lefschetz pencils; however, it is not natural, neither from a symplectic point of view nor from a complex point of view, as it requires hard arguments of approximation, dilatation, and some forms of the homotopy principle.

This leads us to the question of parametrization. In algebraic geometry, since objects are given as the zero set of homogeneous polynomials, they are naturally unparametrized. But in covariant symplectic topology, objects are the images of maps, and therefore carry a natural parametrization. Sometimes, this parametrization is useful, but most of the time, it is useless, and one quotients out by the reparametrization group at the source of the map. This is sometimes a subtle task, when different parametrized objects intersect each other. When the source is the Riemann sphere $\mathbb{CP}^1$, this is not a problem since it carries only one complex structure. But for all Riemann surfaces of higher genus, this is a real problem because one has to consider all possible complex structures on the Riemann surface. Actually, in the seminal paper by Gromov in 1985 cited above, Gromov made only one mistake: he did not compute correctly the index for maps of Riemann surfaces of higher genus into the symplectic manifold, a mistake corrected later by Dusa McDuff.

This second remark leads us naturally to the relation between Symplectic topology and String theory in physics, since Gromov is a mathematician and

Witten a physicist. It is a remarkable accident of history that both theories were born at the same time in the 80s, are similar mathematically, but emerged from radically different origins. It took some time to realize that their mathematics are closely related.

The third remark is entirely general and applies to all (co)homology theories. To understand this, one should first observe that we only used 0-dimensional moduli spaces in our definition of the Gromov-Witten invariants. That is to say we only count over $\mathbb{Z}$. However, one could perfectly consider higher dimensional moduli spaces, not only 0-dimensional ones, and extract from them rings or other algebraic structures, that could be richer than the reduction to $\mathbb{Z}$. But it turns out, from intersection theory, that most higher algebraic structures can be reconstructed, by generic intersections, from a set of well-organized relative numbers. The danger, from a conceptual point of view, is that it is a huge task to make that reconstruction, and that one may lose the geometry. An example of this is that there are several functorial properties that the GW-invariants have, which are proved by considering the full topology of the moduli space of J-curves. Another simpler example is Morse theory where one could look only at trajectories, finite in number, from one critical point to another of relative index 1, and then try to recover the moduli space of trajectories between two critical points of higher relative index (the first work on this question was carried on by Barraud-Cornea in *Annals of Mathematics*). Morally speaking, this means that cohomology theories are made to be understood over $\mathbb{Z}$, and there is a beautiful theorem that tells us that if one knows a given cohomology on all prime numbers, as well as on the rational numbers, one can recover algebraically that theory over $\mathbb{Z}$.

5. Relation with String Theory

String theory was born to produce enough mathematical richness to encode the complexity of the physical world, and realize Einstein's dream of unifying the three quantum forces (electromagnetic, nuclear and weak) at low range with the large range gravitational force. Whence the name "Quantum gravity" for this endeavour. String theory, whose most brilliant advocate is Edward Witten (and Vafa), enriched physics by replacing a point-particle by a string, that is, a loop. Therefore a path in space of a point particle, which was a real curve, is replaced by a Riemann surface, that is the trajectory spread by a real loop in time. There are many reasons to replace a point by a loop. The first one, purely physical, is to encode flavours, tensions in that loop that encode some basic properties of the particle. The second one, which is of interest to mathematicians, is to replace an open real graph, made of edges and vertices, by a Riemann surface. Thus, when particles travel in time, after fusion or disintegration, the real graph is replaced by a Riemann surface (open or closed). This is actually needed for relativity: indeed a real graph is in contradiction with relativity (even with the basic linear relativity in Lorentz space). The reason is that the point in a classical real graph, where two particles make a fusion or disintegration, cannot be seen at the same point from different observers. But if one replaces this junction of two real lines by a pair-of-pants, then the point of junction is coherent with all observers simply because the central point of a pair of pants is not well-defined. Indeed, all observers in Einstein relativity theory will have a different look from different planes that will cross the "junction" of the pair of pants differently[5]. The second reason, more important, is that Riemann surfaces carry, with their moduli space of several different complex structures, a wealth of information much greater than real graphs.

Now let's look at the relation between the Gromov and Witten approaches seriously. In other terms, let's see why, in Witten's works, these GW-invariants appeared. Like all mathematicians, one never copies or reproduces something that one has written before. But here, I could not do better than what I wrote for the Bulletin of the American Mathematical Society in 1996 concerning the relation between Symplectic topology and String Theory, the only article up to now on this subject. I was helped by Lisa Jeffrey and John Harnad who were kind enough to respond to all my questions. For the sake of completeness, and to make this article as easy as possible to the reader, I copy here my paragraph from the Bulletin, with small changes. This paragraph might be a bit hard for some readers in mathematics and in physics, and the reader can ignore it and go to the next section of this article, without losing anything of the main content of this article. I encourage mathematicians to read the wonderful book by Rosen that, in just 100 pages, gives all four quantum field theories from Dirac to Feynman, in pure easy mathematics. This book is a jewel, made for mathematicians, that is so clear that it can be read in just a single day.

Let's recall what a *nonrelativistic Quantum Field Theory* (QFT) is, following Feynman's approach. First, a *classical mechanical theory* is determined by the choice of a Lagrangian (say the difference between potential and kinetic energies) on TV, the tangent space to V, or by the choice of a Hamiltonian function on T^*V, where, as usual, V is the configuration space of positions. A *classical field theory* is defined in a similar way by the choice of a bundle over V whose sections are the fields ϕ, and by the choice of a Lagrangian $L(t, q, \phi(q), \phi(q))$ whose integral over time for a given path ϕ_t is the *action.* In both the mechanical and field settings, the dynamics (or time evolution) is given by following the paths which are critical points of the action. Now a *quantum mechanical theory* assigns to V the space $L^2(V)$ of wave functions $\psi(q)$ with complex values. Its dynamics $\psi_t(q) = \psi(t, q)$ is governed by the following integral equation :

$$\psi(t', q') = \int_V K(q', q : t' - t)\psi(t, q)dq$$

assuming that an initial state $\phi(t, q)$ is given. Here the propagation kernel K reflects the density of probability that the particle be at position q' at time t

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[5] Payette, private communication, observed that this paradox in linear Einstein relativity is solved, even for real graphs, in general relativity simply because in this context, points are *events* in the four-dimensional space of general relativity, and therefore cannot be misunderstood.

given that it was at position q at time t. Here the kernel must satisfy the initial condition

$$\lim_{t\to 0} K(q', q; t) = \delta(q' - q)$$

where δ is the Dirac distribution that assigns, in a distribution sense, the number 0 except when $q = q$, in which case the value is 1. It must also satisfy the natural composition law:

$$K(q'', q; t'' - t) = \int K(q'', q'; t'' - t')K(q', q; t' - t)dq'$$

which just means that K is a group under time, like autonomous Hamiltonian systems, or any autonomous system of ODEs.

By iteration of this formula, as $\Delta t \to 0$, and based on considerations of Dirac, Feynman concludes that the kernel has the following functional integral representation:

$$K(q', q : t' - t) = \int_{\mathcal{C}} \exp^{2\pi i \mathcal{S}/h} \mathcal{D}Q$$

where the integral is taken over the space $\mathcal{C}$ of all paths Q from q, t to q', t' and where $\mathcal{S} = \mathcal{S}(Q)$ is the action of each path, that is, the integral of the Lagrangian over time from t to t'. This is the Feynman "sum over histories" formulation of the dynamical law of a system with a finite number of degrees of freedom (by finite degrees of freedom, I mean that Feynman's theory would not apply to, say, thermodynamics, at least as long as one considers thermodynamics as a theory of "continuous matter" like gases)[6].

The same quantization applies to field theory: here the unknown is the wave function $\Psi_t(\phi)$ defined on the space of fields, and the same considerations lead to a similar formula for the propagation kernel (or "partition function"):

$$K(\phi', \phi; t' - t) = \int_{\mathcal{C}} \exp^{2\pi i \mathcal{S}/h} \mathcal{D}\Phi,$$

but this time $\mathcal{C}$ consists of the paths Φ of space fields from $\phi,\ t$ to $\phi',\ t'$. Hence the quantum dynamics is determined by an integral over the space of classical fields.

Now, let's define a quantum field theory. It is simple. A $d+1$-quantum field theory assigns to a d-dimensional manifold W a complex vector space $\mathcal{H}(W)$, and to a manifold V of dimension $d+1$ with $\partial V = W$ an element $Z(W) \in \mathcal{H}(W)$, in such a way that:

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[6] Although the physical study of gases, in practice, relies on continuity, Boltzmann introduced it as a partition function on particles, and as Payette suggests rightly, one could in principle set Thermodynamics as a consequence of Feynman's QFT. There have also been some attempts to reduce thermodynamics to particles, for instance Smirnov's and Villani's works. This subject is far from the perspective of this article, and I will not comment on it here.

(1) $\mathcal{H}(W_1 \cup W_2) = \mathcal{H}(W_1) \otimes \mathcal{H}(W_2)$,

(2) $\mathcal{H}(-W) = \mathcal{H}(W)^*$,

(3) the following natural simple composition law holds: if $V = V_1 \cup_W V_2$ where $\partial V_1 = W_1 \cup W$ and $\partial V_2 = -W \cup W_2$, then $Z(V) = Z(V_1) \circ Z(V_2) : \mathcal{H}(W_1) \to \mathcal{H}(W_2)$. Here the *fields* on V can be functions, sections of a bundle, or maps from a manifold to a manifold M (in which latter case, the space of fields is obviously not in general a vector space, and then the theory is called a *non-linear sigma model.* Finally, the action $\mathcal{S}(\phi)$ is the integral over V of some Lagrangian depending on ϕ and its first derivatives.

Let's turn to *observables.* The classical observables are complex-valued functions defined on the space of all fields. The *partition function* (or propagation kernel when there is a time direction) is defined as above:

$$Z(M) = \int_{\phi s} \exp^{2\pi i c \mathcal{S}(\phi)} \mathcal{D}\phi$$

where $1/c$ is the *coupling constant of the theory.* The *correlation value* of the observables $\mathcal{O}_1, \ldots, \mathcal{O}_k$ is by definition

$$< \mathcal{O}_1, \ldots, \mathcal{O}_k >= \int_{\phi s} \exp^{2\pi i c \mathcal{S}(\phi)} \mathcal{O}_1 \ldots \mathcal{O}_k \mathcal{D}\phi$$

which is, as above, expressed uniquely in terms of classical fields. The theory is *topological* if the partition function and the observables do not depend on the choice of the metric on the manifolds involved.

Now Witten (but see also Beaulieu-Singer) considers the topological non-linear sigma model with V equal to a Riemann surface Σ (the Riemann sphere for instance), M equal to a symplectic manifold, and the fields being naturally the triplets (ϕ, ξ, ρ) where ϕ is a smooth map from Σ to M, and ξ, ρ belong respectively to $\Omega^0(\Sigma, \phi^*TM)$ and $\Omega^1(\Sigma, \phi^*TM)$, where here $\Omega^k(\cdot, \cdot)$ is the space of real differential k-forms defined on the first factor, with values in the second one. Then Witten shows that the only observables are those of the form $\mathcal{O}_\alpha(z)$ where $z \in \Sigma$ and α is a closed differential form: it is zero except when the map ϕ sends z to the Poincaré dual of α (in which case it is equal to the intersection number). Let me explain this. Physical considerations (BRST-invariance) suggest that the correlation functions $< \mathcal{O}_{\alpha_1}(z_1), \ldots, \mathcal{O}_{\alpha_k}(z_k) >$ are topological invariants in the sense that they are invariant under continuous changes of ω, J (and therefore of the Riemannian metric defined by ω and J), as well as changes in the k points on Σ. Mathematically speaking, the formula for the correlation of observables can be recast in the following form:

$$< \mathcal{O}_{\alpha_1}(z_1), \ldots, \mathcal{O}_{\alpha_k}(z_k) >= \int_{\phi, \xi, \rho} \exp^{-\mathcal{L}(\phi, \xi, \rho)} \mathcal{O}_{\alpha_1}(z_1), \ldots, \mathcal{O}_{\alpha_k}(z_k) \mathcal{D}\phi$$

where $\mathcal{L}$ is the Lagrangian. Here the number $i = \sqrt{-1}$ has disappeared from the exponential, since we look at the real part of the extension over $\mathbb{C}$ of the coupling constant $1/c$.

Witten then used the invariance of this expression with respect to changes in the metric, changing the Riemannian metric g to Cg, where C is a positive real number, as large as we wish. This change produces a similar change in the Lagrangian $\mathcal{L}$ that becomes $C\mathcal{L}$. Thus the above Feynman integral is dominated by fields that are minima of $\mathcal{L}$. These minima are easily seen to coincide with J-holomorphic maps $\Sigma \to M$. Hence, this integral boils down to the sum, counting multiplicities, of all rational J-curves that send the marked points $z_1, \dots, z_k$ to representatives of the Poincaré duals of the differential forms $\alpha_1, \dots, \alpha_k$. This sum splits into sums related to given homology classes of J-rational curves (this naturally appears when a J-curve in class A bubbles-off to two or more components in classes A_i whose sum is A), so that finally one is led to define the Gromov-Witten invariants, from Witten's point of view, as the number of rational J-curves in class A (or in any decomposition of that class) that send the fixed marked points of Σ to the given representatives of the Poincaré duals of the α_is.

6. The Interior and the Exterior in the Sciences

Here, let's start with philosophy. I first erased this paragraph, but under pressure ;-) of the Editor, I decided to keep it.

All field theories are based on the deep relation between the interior of an object and its boundary or exterior, that one could call the superficy. In mathematics and in physics, the exterior is the locus of dynamical phenomena while the interior belongs to geometry or topology, for instance algebraic geometry or symplectic topology. As I have explained above, in symplectic topology, the interior is detected by subtle invariants based on analytic geometry (when given accessory additional structures), while the boundary is the locus of Hamiltonian dynamical systems, as chaotic as they could be, i.e., as far as they could be from integrable Hamiltonian systems. From a philosophical point of view, one could ask the question: how well can one recover the geometric or topological nature of the interior of an object from its superficial dynamics, and conversely? This question is one of the most fundamental ones in philosophy, if not the most important one. Philosophy is the mother of all sciences. Before giving examples, I wish to say that I do not like the word "superficial" in the current language when it means a lack of understanding or an egoist behavior. Superficy is much higher, and this confusion of terms is damaging for everyone. Examples: philosophy's goal is to understand the sciences, the deep nature of things, from their surface. Philosophy is entirely based on the concept that the superficial behaviours, once studied correctly, are our only reach to the intrinsic nature of the universe, the world and ourselves. In philosophy, since its early ages, there are three levels: the highest one is esthetics, the "true" nature of things, which is the ultimate goal and where mathematics dominates as the only candidate of universal thinking[7]. The second level is phenomenology that corresponds to all physical and biological sciences, and also to mathematics inasmuch as we consider mathematics as the science of the human action on the world from prehistoric times[8]. The third and last level is ethics, or moral philosophy, which is an application of the first two levels. Here, I have no preconceptions: this apparent hierarchy is just pedagogical, as I admire all three levels in an equal way, as long as they are all understood. Of course, the most interesting problem emerges when the subject takes himself as an object. This is a current theme in logic and philosophy and led to paradoxes in the mathematical XIX th century. It also emerged in the XX th century when physicists realized, at the quantum level, that one cannot observe the state of a system without perturbing it. As a funny consequence, this fact, once placed in the world of intrication in quantum physics, led Bennet and Brassard to the theory of quantum cryptography that has resolved, once and for all, the question of secrecy in all communications. As a second example, let's take psychology. One must first define the object of psychology. Its object is the study of the personality. Then what is personality? I define it as the deeply organized set of behaviours that a child has built in his/her communication to the world, both in the reception of the world's stimuli and in his/her reaction and action on the world. Psychology's goal is not to respond to the deepest questions of humanity, but to be a practical science that studies the behaviours of human beings to solve day-to-day behaviour problems. For instance, psychologists cannot help a human being who suffers from deep philosophical unanswered contradictions. Psychology is purely a superficial science whose goal is not to infer the interior from the exterior. Here, once again, I use the word superficial in its highest sense. A third example is the justice system in our countries, which obviously stands only on the third, moral, level of philosophy. The problem here is that nowadays, the moral justice system, that should be the third level of philosophy, is no longer part of philosophy. It is influenced much more by lobbying or social networks than by esthetics and phenomenology.

Now let us examine the other family of invariants, the capacities. Let me first recall some basic bilinear algebra. Let (V, α) be a real finite dimensional vector space endowed with a real non-degenerate anti-symmetric bilinear form on V. Then, as I said above, the dimension of V must be even. Now consider any vector subspace W of V of real codimension 1, a hyper-subspace. As W is of odd real dimension, the restriction of α cannot be non-degenerate. Therefore there is in W, endowed with the restriction of α, a kernel K that we define

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[7] This highest level of philosophy is also called "Ontology" (la science de l'être) by Heidegger and Sartre.

[8] The term "phenomenology" was coined by Husserl and Merleau-Ponty; it could have been introduced by Auguste Comte a few decades earlier, or by Kant in its "Critique de la raison pure" where he opposes noumena and phenomena. One could go much earlier to the Greek philosophy in establishing this opposition. I use this term here in my own way. I should say that, while I admire German philologists (Nietzsche for instance), their philosophy is boring, as if the German tradition of philosophy was disconnected from the sciences. Like Spinoza, Nietzsche and Freud, I prefer Greek and early French philosophy.

as the vector subspace of W that is formed of all vectors of W that are such that $\alpha(k, w)$ is zero for any vector in K and all vectors of W. It turns out that this kernel K is always of real dimension 1 in W, whatever the hyper-vector subspace W. So this kernel gives a real line in W. This line will later be the motion of all Hamiltonian dynamics. It is now a trivial task to extend this observation to any real codimension 1 submanifold N in a symplectic manifold M. It shows obviously that any real codimension 1 smooth hypersurface N in M carries a vector field, given by the kernel K at each tangent plane. This vector field is called the Reeb vector field (Reeb was a Swiss mathematician from the French part of the country; his name is pronounced Rèb). It is a smooth vector field and the basic theory of ODEs tells us that we can integrate it (for all times if N is closed), which produces a dynamics on N: for each time t, there is a diffeomorphism of N onto itself that represents the integration of that Reeb vector field on N from time 0 to t. Now if M has a smooth closed boundary N, there is therefore an intrinsic flow on N, induced by the internal symplectic structure on M. The exquisite question is the following: it is obvious that the Reeb vector field just depends on a small neighbourhood of the symplectic manifold near N. And so, a priori, one could think that it is impossible to infer the totality of the interior from the Reeb flow on the boundary. This is true in all geometries, but it is false in Symplectic topology. Indeed, in Riemannian geometry, a very soft theory, one can change the metric in some part of the interior without changing its behaviour near the boundary. But symplectic topology is extremely rigid, because it is a topology. That's the point. Indeed, I conjectured that the only way to change the interior, without changing the exterior, is by the symplectic operation of blow-up, which is morally the same as the blowing-up in algebraic geometry. In symplectic topology, one can blow-up any symplectically embedded ball, and this will reduce the volume, but will be entirely local. As this operation is local, it does not affect the behaviour of the symplectic structure elsewhere, and therefore all theorems that refer to how the dynamics on the boundary determines the intrinsic symplectic topology inside must consider non-blow-up symplectic manifolds. Apart of the symplectic blowing-up of symplectically embedded balls, all other operations to produce a new symplectic manifold from two given symplectic manifolds are extremely rare and are all of global nature, not local. For example, I proved with Dusa McDuff that all symplectic fibrations, in some range (that does not preclude the possibility of a full generalization) are all cohomologically split, which means that the fibration operation does not in general produce new examples of symplectic manifolds, except when this is a direct product. Moreover, Gromov proved that the connected sum of two symplectic manifolds cannot lead to a new symplectic manifold. In summary, symplectic topology is so rigid that there is no local construction of new symplectic manifolds, except by blowing up, and that the rare operations of global operations on them are very limited. This is why the boundary of a symplectic manifold tells us so much about its internal structure.

One could argue here that one can change locally the symplectic form ω to $\omega + d\beta$, where β is any 1-form compactly supported in some bounded open subset of M, and sufficiently C^2-small. Indeed, in this case, $\omega + d\beta$ is still closed, cohomologous to ω, and non-degenerate since $d\beta$ is C^1-small. But there is a basic theorem in symplectic topology, called the Moser argument, that states that if one deforms $\omega = \omega_0$ along a one-parameter family of symplectic forms $\omega_t, t \in [0,1]$, all cohomologous (i.e., $[\omega_t] = [\omega_0]$ in the De Rham cohomology for all t), then there is a one-parameter family of smooth diffeomorphisms $\phi_t : M \to M$ such that the pull-back of ω_t by the differential of ϕ_t is ω_0. Thus, in this case, all of the structures ω_t are the same up to diffeomorphism (or in the language of physicists, they are all the same up to changes of "coordinates"). Furthermore, the diffeomorphisms ϕ_t can be chosen to be the identity outside the compact support of the deformation. Going back to our local change $\omega \mapsto \omega + d\beta$, one sees that the path $\omega + dt\beta$ between ω and $\omega + d\beta$ satisfies the hypothesis of Moser's argument, so that $\omega + d\beta$ is diffeomorphic to ω, with a diffeomorphism equal to the identity outside the compact support of β. Thus the structure of the symplectic form has not changed. This is why it is so difficult to change the internal symplectic structure of a symplectic manifold with boundary, without changing the dynamical structure of the boundary. In real dimension 4, there are results that show this for many convex subsets of $\mathbb{R}^4$, the boundary indeed determines entirely the symplectic structure inside. In other words, if one is given the smooth compact boundary W of some relatively compact open subset $U \subset R^4$, with the contact structure inherited from the standard symplectic form $dy_1 \wedge dx_1 + dy_2 \wedge dx_2$ on $\mathbb{R}^4$, then there are no other symplectic fillings of W, up to diffeomorphism and blow-up. This is what I mean by the statement "the boundary determines the interior", and since the interior determines the boundary, we have a sort of rational equivalence (in the sense of algebraic geometry) between boundaries and interiors. This will lead to a theory, to come, of bi-rational equivalence in symplectic topology, that will be much harder to develop than the bi-rational equivalence in algebraic geometry.

Now assume that one is given a homeomorphism $\phi : M \to M$, where (M, ω) is a symplectic manifold, that we can consider closed. Assume moreover that this homeomorphism is not differentiable, for instance non-differentiable almost everywhere, according to the volume measure given by ω^n. Then there are two ways to declare ϕ as a symplectic homeomorphism. The first way is to choose a capacity and check if ϕ preserves that capacity on all open subsets of M. The other way, probably simpler, is to require that ϕ be the C^0-limit of symplectic diffeomorphisms. In other terms, one considers the inclusion of the space of smooth symplectic diffeomorphisms as embedded in the group of all homeomorphisms, and then one takes the completion for the C^0 topology of the first into the second one. In order to understand this, let me recall that any symplectic diffeomorphism ϕ from a symplectic manifold to itself can be characterized as a Lagrangian submanifold L in the direct product $(M, \omega) \times (M, -\omega)$ (which is a symplectic manifold) that projects as diffeomorphisms on both factors, meaning that both maps $L \to M$ on the factors are diffeomorphisms. Here L is just the graph of the map ϕ. This establishes a one-to-one correspondence between symplectomorphisms and such smooth Lagrangian submanifolds in the cartesian product. Thus Lagrangian submanifolds are more general, for instance if we allow these submanifolds to interpolate between two Lagrangian graphs of ϕ_1 and ϕ_2 along a one-parameter family of Lagrangian submanifolds in the product $M \times M$ that are allowed at some times in the deformation not to respect the one-to-one projections onto both factors. This is reminiscent of caustics in optics, which are caused by this phenomenon.

Therefore, one is led to consider Lagrangian submanifolds inside any symplectic manifold M, not necessarily a product. And why not coisotropic submanifolds inside M? Recent results on this subject have proved that if a smooth manifold $N \subset M$ is the C^0-limit of smooth Lagrangian or coisotropic submanifolds, then N shares the same property. Actually, in the way I stated it, this is still a conjecture, that should and very likely will be proved. The right established statement is when the C^0 limit is taken of images of a coisotropic manifold by a sequence of ambient smooth symplectomorphisms.

The same C^0-nature of symplectic topology also appears in the Poisson bracket of two functions $f, g : M \to \mathbb{R}$ on a symplectic manifold (M, ω). It is defined by $\{f, g\}(x) = df(x)(X_g(x))$ where X_g is the symplectic gradient of g. This appeared above naturally for integrable Hamiltonian systems. I recall that a Hamiltonian $H = H_1$ on a symplectic manifold of real dimension $2n$ is called integrable if there are $n-1$ other functions, called momenta, $H_2, \dots, H_n$, such that all Poisson brackets $\{H_i, H_j\}$ vanish and their symplectic gradients are linearly independent almost everywhere (for the measure given by the volume form ω^n on M). In this case, the motion of the symplectic gradient of H is located on tori of dimension n given by the common zeros of all H_i's. Integrability is of course rare, and one is happy if one can find any number, less than $n-1$, of momenta, or if the momenta are such that the Poisson brackets $\{H_i, H_j\}$ are C^0-close to zero. In this respect, Cardin and Viterbo, Zaplosky, and Entov and Polterovich, proved that the functional associating to a pair of compactly supported smooth functions on a symplectic manifold the maximum of their Poisson bracket is lower semi-continuous with respect to the product C^0-norm on the space of pairs of such functions, a great result.

7. Concluding Remarks

Thus, C^0-symplectic topology is the same as smooth symplectic topology, it is a generalization to symplectic homeomorphisms. I am not aware of new results in C^0-symplectic topology that would prove something new in smooth symplectic topology. But there is hope because homeomorphisms are much more flexible. Currently, C^0-topology can be useful to yield simpler proofs of known results in smooth symplectic topology. But on the way, it could reveal a new world.

In a somewhat related subject, I work with my collaborators Dmitry Faifman, Jordan Payette, Egor Shelukhin and Jun Zhang on a project of Weighted Gromov-Witten invariants that would measure the volume of all J-holomorphic curves passing through k open subsets (or any other kind of subsets of interest) that would eventually be a unified theory of both GW-invariants and capacities.

There is an intrinsic metric, the only one possible, on the group of Hamiltonian diffeomorphisms of a symplectic manifold, and therefore on all Lagrangian and coisotropic submanifolds. Here the Hamiltonian diffeomorphisms are a subgroup of the identity component of the group of symplectic diffeomorphims equal to that identity component if the manifold is simply connected (or more generally if the first Betti number of M over $\mathbb{R}$ is zero). This metric is C^0 in nature; it was introduced by Hofer and was proved to be a genuine metric for all symplectic maniolds in a series of papers in 1995 in *Annals of Mathematics* and *Inventiones Mathematicae* by McDuff and me. This metric plays a fundamental role in smooth or C^0-symplectic topology. Indeed, as I said, no other intrinsically defined metric exists.

Finally, I have here a dilemma: either I build a reference section of about one hundred articles, or I leave it empty. In the context of this article, I prefer to leave it empty. But here are the names of the authors related to each subject treated in this article, that the reader can find on arXiv or elsewhere. For general knowledge, there are two books by McDuff and Salamon, one is a general introduction to symplectic topology while the other is the foundations of J-curves, GW-invariants and Quantum cohomology. In the theory of capacities, I wrote an article on the subject called Capacities in Symplectic topology. Nowadays, the best experts in C^0-symplectic topology, among others, are Viterbo, Humilière, Seyfaddini, Le Roux, Leclercq, Oh, Polterovich, Entov, Zapolsky, Buhovsky, Tanny, Logunov, Usher, Jun Zhang, Yaniv Ganor, Vukasin Stojisavljevic, Darko Milinkovic, Morimichi Kawasaki, Yusuke Kawamoto, Payette. On some conjectures by Viterbo on capacities, one could look at the very recent works of Hutchings and Shelukhin. On the physics side, there is a nice paper *The topological sigma model* by Beaulieu and Singer in CMP as well as the paper in CMP by Witten with the same title, and the unavoidable book by Feynman and Hibbs *Quantum mechanics and path integrals.* One should look at the long beautiful article by Witten *Two-dimensional gravity and intersection theory on moduli space*, as well as the synthesis *Mathematical aspects of string theory* edited by Yau.

Département de mathématiques et de statistique, Université de Montréal, Montréal, Québec, Canada
e-mail: lalonde@dms.umontreal.ca