Science and Cultural Theory

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Growing explanations

Historical perspectives on recent science

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1 Mirror symmetry: persons, values, and objects

Peter Galison

At the millennium, there was much talk about "the end of physics." Many physicists believed that their enterprise was coming to a final phase of its history, but they interpreted "the end" in numerous ways. At the end of the cold war some made dire predictions about the discipline because military research and development were downsized, the $15 billion superconducting supercollider was canceled in 1993, the National Science Foundation offered less to physics, and the Department of Energy budget was significantly reallocated. These same words—"the end of physics"—took on a second meaning in the 1980s and 1990s, when some high-energy particle physicists turned against a small but growing minority of theorists who embraced string theory. For these critics string theory appeared a false salvation, a mathematical chimera that abandoned experiment, tempted the young, distorted pedagogy, and ultimately threatened the existence of physics as science. A third meaning of the "end of physics" emerged within string theorists' own ambitions: many argued that a remarkable series of discoveries within the mathematical physics of strings provided grounds, the best ever, for an account of all the known forces including gravity, completing the historical mission of fundamental physics. It would be, its most enthusiastic backers argued, a "theory of everything."

Physics at the millennium

Hopes and fears for finality in physics are not new. Distinctive is the string-theoretical vision in which mathematics itself came to stand where experiment once was: the view that the powerful constraints of mathematical self-consistency would hem theory in so tightly that, at the end, only one theory would stand, and that an elegant, compact theory would cover the world by predicting all the basic forces and masses that constitute and bind matter. Theoretical physics would end, in this third sense, because the ancient search for physical explanation in terms of basic building blocks had reached its last station.

Rejecting the doomsayers, the new alliance between physics and mathematics saw the opportunity to restructure the bounds of both fields, opening a window on twenty-first century mathematics that might, imminently, produce a unified account of gravity and particle physics. But the optimism came with warnings from some quarters of both math and physics—what would be the proper standards of demonstration in this new interzone between disciplines? Would this new "speculative mathematics" sacrifice experiment on the physics side and intrude physical argumentation into the heartland of mathematics on the other? Would it compromise both the physicists' demand for laboratory confirmation and the mathematicians' historical insistence on rigor?

These discussions over the place of string theory are enormously instructive. They offer a glimpse into theoretical physics during a remarkable period of transition, for in the realignment of theory toward mathematics the meaning of both *theory* and *theorist* were in flux: First, in the 1990s a new category of theorist was coming into being, part mathematician and part physicist. Second, theorists ushered a new set of conceptual objects onto the stage, not exactly physical entities and yet not quite (or not yet) fully mathematical objects, either. Finally, alongside the shift in *theorist* and *theory*, there arose, in the trading zone between physics and mathematics, a style of demonstration that did not conform either to older forms of physical argumentation familiar to particle physicists or to canonical proofs recognizable to "pure" mathematicians.[1]

By introducing new categories of persons, objects, and demonstrations, string theory made manifest conflicts over the values that propel and restrict the conduct of research. These debates, joined explicitly by both mathematicians and physicists, focused on the *values* that ought to guide research, and the disagreements were consequential. At stake were the principles according to which students should be trained, how credit for demonstrations should be partitioned, what research programs ought be funded, and what would count as a demonstration. For all these reasons, the status of claims and counterclaims about string theory mattered. They were not "just rhetoric" constructed after the fact; they were, in part, struggles over the present and future of physics. Values, in the conjoint moral and technical sense I have in mind, were not, as one historian derisively called them, mere "graffiti." Nor is it the case that these values respected a distinction between "inside" and "outside" science. These were not values in the sense of "propriety," such as whether or not honorary authorship would be countenanced on a group paper. Rather, I will argue that the "values" in debate crosscut through the central, guiding commitments of physics and mathematics into the wider, everyday sense of the term. For this reason, dismissing the role of values in shaping theoretical research makes it impossible to understand both the moral passion behind these debates over demonstrative standards, the participants' own understanding of their distinctive scientific cultures, and ultimately, the scientific persona of the physicist.

The argument will follow this order: section two tracks the reaction by particle physicists to the shift by string theorists away from the constant interplay between theory and experiment. Sections three through five home in on the creation during the late 1980s and early 1990s of a striking "trading zone" between physicists and mathematicians around a variety of developments including that of "mirror symmetry," a remarkable development at once physical and mathematical. Mirror symmetry offered insight both into the shape of string theory after the higher dimensions curled up and provided new ways of understanding not only fundamental features of algebraic geometry, but also reshaped the status of geometry itself. By following the ways mathematicians and physicists saw one another in the episode of mirror symmetry—and the ways each side came to understand aspects of the other's theoretical culture—it becomes possible to characterize the broad features of a new place for theory in the world. Finally, in light of the tremendous impact of this hybrid physics-mathematics, section six analyzes the mathematicians' sometimes conflicted reaction: a response mixing enormous enthusiasm with grave reservations about the loss of rigor that accompanied the mathematicians' collaboration with the physicists. In a sense the physicists' and mathematicians' anxieties mirrored one another: both saw danger in parting from historically established modes of demonstrations that gave identity to their fields. These discussions were hard fought and explicit: in the midst of remarkable new results, claims, and critics, string theorists never had the luxury of being unself-conscious: the purpose, standards, and foundation of their budding discipline were *always* on the table.

At the end of the millennium, string theory resided, both powerfully and precariously, in the hybrid center between fields, bounded on one side by the continent of physics under the flag of experiment and on the other side by the continent of mathematics under the flag of rigor. To understand the aspirations and anxieties of this turbulent territory is to grasp a great deal of where theory stood at the end of the twentieth century.

Theory without experiment

In 1989, David Gross, from Princeton, chose physicists' favorite site for metaphors (the romantic mountains) on which to erect a new relation between theory and experiment, one far different from the close cooperation that had marked the mid-1970s. "One of the important tasks of theorists is to accompany our experimental friends down the road of discovery; walk hand in hand with them, trying to figure out new phenomena and suggesting new things that they might explore."[2] Burnt into the collective memory both of pro- and antistring theorists was the example of the J/psi and other "charm" discoveries of November 1974.[3] During the frenetic days of that late fall, and the months that followed, experimentalists tossed new particles into the ring, so to speak, and theorists worked furiously to explain them; theorists postulated new particles, new effects, and new theories—experimentalists responded with tests that could be prosecuted almost immediately. At the time Gross was writing, in the late 1980s, that highly responsive dialogue had fallen into silence—few experimental results were coming out of the accelerators, and the discoveries that were being reported had a wickedly short life: the neutrino oscillations (indicating that the neutrino might have a mass) came and went; proton decays were reported and retracted; monojets spurted momentarily from CERN, then vanished; the fifth force grabbed attention for a while and then loosened its hold. Under these circumstances some theorists—Gross included—were less and less inclined to theorize furiously after each new sighting. These were no longer the days of new, hot experimental news and papers written on airplanes returning from the accelerator laboratories, of quick phenomenological calculations using a couple of Feynman diagrams and Lie group calculations that could be done on one's fingers. Looking back in 1989, Gross put it this way:

> It used to be that as we were climbing the mountain of nature the experimentalists would lead the way. We lazy theorists would lag behind. Every once in a while they would kick down an experimental stone which would bounce off our heads. Eventually we would get the idea and we would follow the path that was broken by the experimentalists. Once we joined our friends we would explain to them what the view was and how they got there. That was the old and easy way (at least for theorists) to climb the mountain. We all long for the return of those days. But now we theorists might have to take the lead. This is a much more lonely enterprise.[4]

Without knowing the location of the summit, or how far it was, the theorists could promise little reassurance to themselves or to the experimentalists. In the meantime, experimentalists were not only left behind; they were left out altogether.

Not surprisingly, many experimentalists were shocked by the theorists' string ambition, not so much by the idea of theorists leading the way on an uncertain trek up an uncharted mountain, but because the experimentalists did not see how they could even gain a toehold in the foothills. Carlo Rubbia, who only a few years before had taken home a Nobel Prize for his team's 1983 discovery of the intermediate vector bosons, the W and the Z, lamented the loss of contact between experiment and theory at a meeting on supersymmetry and string theory:

> I am afraid I am one of the few experimentalists here. In fact, I can see we are really getting fewer and fewer. I feel like an endangered species in the middle of this theoretical orgy. I am truly amazed. The theories are inventing particle after particle and now for every particle we have there is a particle we do not have, and of course we are supposed to find them. It is like living in a house where half the walls are missing and the floor only half finished.

After the bruising W and Z search, and a contentious struggle with the top quark, Rubbia had little appetite for a zoo of unknown particles as numerous as the known. Even one or two particles were terribly hard to find—Rubbia's UA1 collaboration had employed some 150 physicists for years at a cost of hundreds of millions of dollars to find the W and Z. Now this new breed of theorists was ordering a supersymmetric partner for every known entity: a selectron as partner to the electron, and so on all the way down the line.[5]

Not only was this half-missing world overwhelming in its mandate for experimental discovery, but the very motivations cited by the theorists had moved ever further from the accelerator floor. Gosta Ekspong, a senior European experimentalist who often worked at CERN, addressed the purported aesthetic satisfactions of the string theorists:

> I would like to address the question of truth and beauty; truth being experiment, beauty being theory. . . . The problem is that the latest [superstring] theories are so remote from experiment that they cannot even be tested. Therefore they don't play the same role as Dirac's equation. . . . I hope that this search for beauty does not drive theorists from experiments, because experiment has to be done at low energies, from one accelerator to the next and so on. Big jumps do not seem to be possible.[6]

In the 1970s and 1980s, *theory* and *experiment* were categories (better: subcultures) with an intermediate trading zone of phenomenologists straddling the fence. Ahmed Ali was one of these, having worked at both DESY (*Deutsches Elektronensynchrotron*) and CERN, and he invoked the idiom of the fund-seeking experimentalist when he declaimed, "The present superstring theories are like letters of intent written by a lobby of theoretical physicists. They are very good in intent; but often what is said in the letter of intent and what is measured in the experiment are two very different things. The figure of merit of a theory is its predictive power which could be tested in an experiment in a laboratory."[7]

In a sense, the discomfiture of experimentalists, and those working hand in glove with them, could be expected. New techniques in theory had left experimentalists ill at ease with gauge theories in their early stages, though by 1974 experimentalists had found the gauge theories suggestive of a wide range of predictions, tests, and directions for empirical work. The case of superstrings seemed much worse; there was no clear avenue for the accelerator laboratory to follow, and the theories themselves offered precious little to hold on to in the way of physically "intuitable" entities. Less expected, perhaps, was the vehement reaction against string theory from *within* the theoretical high-energy physics community.

I now turn to that part of the opposition that was certainly *not* grounded on a hostility to wide-ranging claims about unification, nor on a broad opposition to the disproportionate resources allocated to particle physics. (That is, I am not focusing here on criticisms mounted over the 1970s and 1980s by condensed-matter physicists such as Philip W. Anderson, who had in mind both a defense of emergent properties in many-body physics and an argument for a reallocation of human and material backing.)[8] Far from being outsiders to the tradition of "fundamental" physics, Howard Georgi and Sheldon Glashow were as central to the gauge revolution of the 1970s as anyone. No, the dispute hinged on something even deeper, I believe, than the relative fundamentality of physical domains. It circled around differing visions of what theoretical physics should be.

Before the 1984 *annus mirabilis* of strings, Georgi opened the 1983 Fourth Workshop on Grand Unification with a transparency of a recent advertisement he had spotted:

> HELP WANTED
> Young Particle theorist to work on Lattice Gauge
> Theories, Supergravity, and Kaluza-Klein Theories

Here, Georgi asserted, was a telling sign of the times, a position caught between "chemistry" (particle physicist nomenclature for a calculational activity in which fundamental principles were no longer at stake) and "metaphysics and mathematics" (particle physicist jargon for work without experiment).[9] Superstrings had not yet emerged with the force it would a few years later, but the problem of choosing between truth as beauty and truth as experiment had, and Georgi sided squarely with those who demanded accessible measurements as a sine qua non of desirable theory.

The next years polarized the situation further. In 1986, Paul Ginsparg, who had himself contributed to the Alvarez-Gaumé and Witten no-anomaly demonstration in superstrings, collaborated with his Harvard colleague Sheldon Glashow to bemoan the loss in superstrings of the historically productive conflict between experiment and theory:

> In lieu of the traditional confrontation between theory and experiment, superstring theorists pursue an inner harmony where elegance, uniqueness and beauty define truth. The theory depends for its existence upon magical coincidences, miraculous cancellations and relations among seemingly unrelated (and possibly undiscovered) fields of mathematics. Are these properties reasons to accept the reality of superstrings? Do mathematics and aesthetics supplant and transcend mere experiment? Will the mundane phenomenological problems that we know as physics simply come out in the wash in some distant tomorrow? Is further experimental endeavor not only difficult and expensive but unnecessary and irrelevant?[10]

This was an altogether different view than that which Glashow had taken in the euphoric moments after he and Georgi had produced the first grand unified theories (GUTs), theories that while leaving aside gravity, aimed to bring together the strong, the weak, and the electromagnetic forces. GUTs, at least the SU(5) and SO(10) versions, did have very high energies—only a few orders of magnitude less than the Planck scale. And like the string theories, GUTs too forecast a "desert" in which no new experimental results could be found. But, Glashow and Georgi believed, several crucial items differentiated GUTs and superstrings. GUTs forecast a crucial (and measurable) parameter in the electroweak theory that measured the relative strength of the weak and the electromagnetic forces—$\sin\theta_W$. Further, SU(5) and SO(10) both predicted a decay of the proton that ought to have been measurable in deep mine experiments; finally, by construction the new grand unified theories reproduced all of the known phenomenology of both the electroweak and quantum chromodynamic theories. Strings could do neither; that is, they could not make new predictions (such as proton decay and $\sin\theta_W$), nor could they reproduce the known phenomenology of the standard model. Closing the August 1987 Superworld II conference, Glashow remarked that the proliferation of superstring theories undermined claims for a unique "theory of everything." Despite such setbacks, Glashow continued, "stringers enthusiastically pursue their fascination with ever purer mathematics, while some survivors grope towards the baroque to the beat of their superdrums. Perhaps we unstrung and unsung dinosaurs will have the last laugh after all."[11]

By 1989, for Georgi, the proliferation of GUTs, especially their assimilation into the even larger unification schemes of the superstring, was a source of a mighty ambivalence: "I feel about the present state of GUTs as I imagine that Richard Nixon's parents might have felt had they been around during the final days of the Nixon administration. I am very proud that the grand unification idea has become so important [but] I cannot help being very disturbed by the things which GUTs are doing now." GUTs, he insisted, had been motivated by the desire to complete the unification of forces by accounting for the weak mixing angle and explaining the quantization of charge.

> They [GUTs] were certainly not an attempt to emulate Einstein and produce an elegant geometrical unification of all interactions including gravity, despite the parallels which have been drawn in the semipopular press. Einstein's attempts at unification were rearguard actions which ignored the real physics of quantum mechanical interactions between particles in the name of philosophical and mathematical elegance.[12]

Imitating the aging Einstein in his failed quest for a completely unified theory, Georgi contended, was a losing proposition.

As far as Georgi was concerned, at stake was not an incidental question of style or philosophy, but rather the defining quality of the discipline. The nature of physics itself was in contest. "Theorists," Georgi insisted, "are, after all, parasites. Without our experimental friends to do the real work, we might as well be mathematicians or philosophers. When the science is healthy, theoretical and experimental particle physics track along together, each reinforcing the other. These are the exciting times."[13] When experimentalists get ahead, as the bubble chamber experimentalists did in the 1960s, the discipline becomes eclectic, overrun with results without order or explanation. At other times theory gets ahead and the path is strewn with irrelevant speculations out of touch with reality.

The string theorists read history differently; they foresaw a different future, and they found in the new theory not a violation of the defining values of physics, but rather their instantiation. Far from being a decline from "the exciting times" of the past, so they argued, these were the golden days of physics, days like none the discipline had seen since the founding years of quantum mechanics in the mid-1920s. If such struggles over values seemed to go beyond typical debates within physics, it was because more than the choice of the right Lagrangian was in play. In contemplating the fate of string theory, theorists, experimentalists, and mathematicians saw that the constitutive practices of their scientific cultures were at stake.

What theorists want

Why, one might well ask, would any theorist want anything beyond the standard, gauge theory model of the electroweak and strong nuclear forces? For, try as they might, the powerful accelerator laboratories at CERN, DESY, and SLAC had only found ever-greater confirming evidence for the gauge theories—ever-more precise measurements that in every way seemed to celebrate the powerful work accomplished from 1967 through 1974. Contra Kuhn there was no experimental anomaly gnawing at the theorists—no aberrant motion of the perihelion troubled them, no unexpected spectral-line splitting, no failure to find a phenomenon like aether drift. The issues in play were fundamentally *intra*theoretical. How, theorists asked, could a theory be considered complete when it held (not including the neutrino masses) some nineteen free parameters that were utterly unfixed by basic principles and could only be inserted by hand? How could a theory whose nineteen parameters had to be tuned to a precision of ten or more decimal points be right? Surely this required "fix" hinted at something more natural, more fundamental that lay beneath the surface. How could gravity and the particle theories be reckoned in utterly different ways in disconnected theories? After all, in the highly curved spacetime near a black hole, quantum effects must enter into the picture. It was not just unaesthetic to banish gravity from the theory of matter; it was manifestly inconsistent—and straightforward attempts to make a quantum field theory for gravity led to an inconsistent, nonrenormalizable theory. It seemed as if the infinities that plagued the quantum field theories signaled a pathology in the theory, suggesting that something was wrong with reasoning that allowed lengths to exist down to their vanishing point.

String theory seemed to bridge these gaps. It would start from a single scale—the Planck scale formed of the various fundamental constants of quantum mechanics (h), gravity (G), and relativity (c). This quantity, $(hG/c^2) = 10^{-33}$ centimeters, set the fundamental length, the "Planck length" of the theory. Out of this single parameter, so the hope was, would follow *all* the parameters of the standard model—including the masses of the quarks and leptons, the coupling constants of the forces. No host of seemingly arbitrary values, no fine tuning of their ratios to get sensible answers that could be reconciled with experiment. Instead of building physics out of point particles, there would be, at the root of all things, *finite* strings of Planck length: all currently known "fundamental" particles of string theory would be no more than the low-lying excitations of these strings. Instead of a Keplerian music of the spheres, forces and masses would be the music of these tiny strings, vibrating under an intrinsic tension of some 10^{39} tons.

Born in the context of strong interaction theory—a long way from unified theories of everything—string theorists got far enough to show that the theories would only exist in 10 or 26 dimensions, far enough, that is, to seem irrelevant for real world 4-dimensional physics. Then, in 1984 three things happened: Michael Green and John Schwarz showed that the theory was probably finite to all orders of perturbation theory—*finite*, not renormalizable.[14] Second, the Princeton "string quartet" of David Gross, Jeffrey Harvey, Emil Martinec, and Ryan Rohm exhibited a particular model that actually seemed a candidate for unifying gravity and known particle physics. And third, Philip Candelas, Gary Horowitz, Andrew Strominger, and Edward Witten provided a picture of what string theory might look like once the "extra" dimensions had curled up (compactified), giving a glimpse of how "low-energy" (accelerator-accessible) particle physics might fit into the theory. String theory exploded. From less than a hundred titles a year between 1974 and 1983, the number skyrocketed to over twelve hundred in 1987.[15]

With Planck-scale physics looking promising in 1985, string-friendly physicists hoped that, with the advances of the high-energy theory, there might be a way to pick out the low-energy consequences of the theory and so meet experiment. The basic strategy for this procedure was this. It was argued that the Planck-scale string theory would "compactify"; that is, 6 of its 10 dimensions would curl up—they had better do so since the world we live in has but 4 dimensions of space and time. After this compactification, the "effective theory" defined on this new space—4 space-time dimensions plus the compactified 6-dimensional real space (equivalent to 3 complex dimensions)—could then be analyzed to determine what particles were predicted to exist and how they should interact.

There are stringent requirements on the nature of this complex 3-dimensional manifold. (An n-dimensional space is a manifold if it can be covered by patches of Euclidean space $\mathbb{R}^n$ or, if complex, patches of $\mathbb{C}^n$.) Most importantly it still had to have a minimal supersymmetry, the pairing of particles like the electrons that obey the Pauli exclusion principle with ones that do not (in the case of an electron, its supersymmetric twin was a postulated entity known as the selectron). Conversely particles (bosons) that tend to bunch together like photons are postulated to have twins that do obey the exclusion principle (the photon's hypothetical twin that *would* obey the exclusion principle was dubbed the photino). The demand for supersymmetry restricted the structure of the 6 "curled-up" dimensions to a particular kind of complex manifold. Not too many years earlier, Eugenio Calabi, a mathematician at the University of Pennsylvania, and Shing-Tung Yau, a mathematician at Harvard, had explored these manifolds and their various properties: in a certain sense flat, closed, and configured so that a vector transported around a closed loop exhibited very special characteristics. Supersymmetry made these mathematicians' speculations the perfect home for curled-up dimensions in string theory. The physicists named them, eponymously, Calabi-Yau manifolds.[16]

At first, everything the physicists knew about these objects came directly from Yau: he had provided a single example in his original paper and in later conversations told Andrew Strominger there were at least four more. For a brief moment, the physicists hoped that all but one would be ruled out, and that the single remaining space would give the true theory. But even before examples had multiplied greatly, Yau began to suspect there were tens of thousands.[17] Still, the requirements the physicists placed on such spaces were stringent and the number of candidates was sure to decrease when those constraints were imposed. In particular, and to the great surprise of many of the string theorists, it turned out that the number of particle generations was tied to a *topological* feature of the Calabi-Yau space. For a 2-dimensional closed surface, the topological Euler characteristic is defined as 2 (1-g) where g is the number of handles in that surface. In higher dimensions, the Euler characteristic is a means of identifying the topological complexity of the manifold. Old-style gauge physics provided no reason for the existence of this multigenerational repetition of particles: electron and electron neutrino, for example, were repeated as the muon (just a heavier version of the electron) and its own associated (muon) neutrino. This same structure repeated a third time with yet a heavier version of the electron known as the tau and *its* associated (tau) neutrino. Here was a clear example of a way in which the mathematics of algebraic topology fixed a physical feature (number of generations) that had absolutely no constraint in gauge theory other than brute, experimental measurement.[18] The hope, then, was that one day a unique string theory in 10 dimensions would be found such that, when compactified, it would issue in a Calabi-Yau space *predicting* the number of generations to be three. It might then, after all these years, be possible to answer I. I. Rabi's longstanding question about the muon, "Who ordered that?," with an answer: whoever chose the number of holes in the manifold. Or one better: "she who ordered the original 10-dimensional theory that compacted into a Calabi-Yau with Euler number plus or minus 6 was, by doing so, ordering the muon." But for now, absent that final theory, theorists could restrict their attention to those Calabi-Yau spaces with the right Euler number—that is, with the right number of generations.

$$|X| \text{ (the Euler characteristic)} = 2 \times \text{(number of generations)}$$

So to match the known world of three generations, string theorists began hunting for 3-dimensional Calabi-Yau manifolds with Euler characteristic plus or minus 6. (Six divided by two gave the three generations, and it was generally thought that the sign of the characteristic could be conventionally fixed later.) With a more or less well-articulated problem, physicists began calling in the mathematicians. David Morrison, a Harvard-trained mathematician at Duke, found the encounter unnerving:

> Physicists began asking, "Can you algebraic geometers find us a Calabi-Yau with Euler characteristic plus or minus 6? It was a pretty interesting experience being asked this. . . . We were asking questions for internal mathematical reasons. Suddenly some physicist knocks on your door and says: if you can answer this it might be a solution to the problem of the universe. But the communication barriers were immense. A parody of the interaction was this. A physicist asks a mathematician: "Can you find me an X?" The mathematician (after many months): "Here's an X." Then the physicist says, "Oh that. Actually we wanted Y not X." *Ad infinitum.*[19]

Israeli physicist Doron Gepner was at Princeton from 1987 to 1989, where he was struggling to understand the structure of 2-dimensional quantum field theories—field theories defined by one time and one space dimension. These are well-studied objects, simpler in many ways than full-blown 4-dimensional theories. But for string theorists the 2-dimensional theory also had the virtue of representing the world-sheet swept out by a string. As Gepner examined these 2-dimensional theories, he began to ask an intriguing question. Suppose, as a number of string theorists did at the time, that one divided the 10-dimensional string space-time into a 4-dimensional part and the compacted 6-dimensional part. It is possible to think of the 6-dimensional part as parameterized by six fields, one for each dimension. For a 2-dimensional field theory to represent this 6-dimensional space, it would have to satisfy two constraints. First, it would need to register the six fields with what is called a "central charge" proportional to 6. And second, the 2-dimensional theory would have to be free (or almost free) of anomalies—a quantum effect that spoils the good behavior of the theory. Gepner considered such a minimal model. It was a 2-dimensional, conformal field theory with a weak anomaly and the right central charge.

What Gepner suggested, in particular, was that there was a geometrical interpretation of the particular minimal model he had written down that had an actual, explicit manifold associated with it. In other words, like any quantum field theory, the minimal conformal field theory specified all the configurations the objects it described could take. And the space of those configurations was, for Gepner's minimal conformal field theory, a completely specifiable Calabi-Yau manifold. More precisely, the Calabi-Yau manifolds that Gepner had in mind were spaces defined by solutions to a polynomial in five complex variables, $z_1^5 + z_2^5 + z_3^5 + z_4^5 + z_5^5 = 0$, known as Fermat hypersurfaces. Just as a quadratic equation $x^2 + y^2 = 9$ (with x and y real) defines a one-dimensional curve in 2 dimensions, the Fermat defining equation with z_1 through z_5 picks out a (complex) 4-dimensional hypersurface in 5-dimensional complex space (C^5). Gepner's argument identifying the minimal model and the Fermat hypersurface proceeded in two steps. First, he showed that both had certain quantities in common (the Hodge numbers, which I'll describe in a moment). And second, he pointed out that the Fermat hypersurface and the minimal model exhibited the same discrete symmetries. If, for example, one multiplies, z_1 (or z_2 or z_3 or z_4 or z_5) by a fifth root of 1, call the root α, then since $(\alpha z_1)^5 = z_1^5$ nothing would change in the equation specifying the manifold. Third, he showed that, for certain points in the algebraic formulation of the field theory and certain points in the Calabi-Yau manifold, the spectrum of particles would be the same.

Finally, Gepner went out on a limb, conjecturing that the link between his particular minimal 2-dimensional conformal theory and a Calabi-Yau space was no accident. He simply declared that *every* minimal model would have a corresponding Calabi-Yau space. No proof, just a strongly held hunch.

An aside: it turns out, quite generally, that there was an enormous simplification in mathematical analysis that could be had by moving from a complex space of n + 1 dimensions ($\mathbb{C}^{n+1}$) to a "complex projective space" of one fewer dimensions ($\mathbb{CP}^n$) that consists of all the lines through the origin in $\mathbb{C}^{n+1}$.[20] So instead of looking at the Fermat hypersurface in complex 5-space ($\mathbb{C}^5$), Gepner turned to the equivalent problem in the projective space, $\mathbb{CP}^4$. It was clear to both physicists and mathematicians that, while $\mathbb{CP}^4$ by itself is not a Calabi-Yau space, degree 5 hypersurfaces embedded in it would be. Starting with this embedded degree 5 complex space, the Fermat-type polynomial took the problem down one dimension and Gepner could lop off another dimension by posing the analysis in projective 4-space. Together this left a space of 3 complex dimensions—and it was this space that Gepner conjectured was the explicitly given Calabi-Yau manifold in which the compactified 2-dimensional string theory lived. So in the end Gepner had a few promising examples but a larger hope. That hope was for a particular construction of a conformal field theory that would be both physically realistic and uniquely attached to a geometry; in short, there would be a correspondence:[21]

> Conformal Field Theory $\Leftrightarrow$ Calabi-Yau.
> Geometry might parallel algebra.

Gepner's 1987 claim about geometry and minimal conformal field theories was heard, used, and challenged. Cumrun Vafa, at the time Harvard's lone string theorist, was also after the links between algebra and geometry. He and colleagues Wolfgang Lerche and Nicholas Warner began with the algebraic relations of the 2-dimensional quantum field theory (just as Gepner had) and they too asked, What must the geometry be that would produce these algebraic relations? But what they saw surprised them: there was an ambiguity on the geometry side—that is, there appeared to be *two radically different geometries* either of which seemed as if it could be associated with essentially the same conformal field theory. (Conformal field theories are the quantum field theories used to represent the strings; they are, by definition, left invariant under transformations that preserve the flat [Minkowskian] metric up to a position-dependent rescaling.) This went considerably farther than what Gepner had in mind, and in conversation Gepner criticized Vafa for treating size- and shape-changing parameters as if they were linked. Lance Dixon too began to wonder about this geometrical ambiguity.[22]

We shift scenes now, from Cambridge to Texas. Philip Candelas, based in the physics department at the University of Texas, Austin, had entered the field through astrophysics (having completed his doctoral dissertation on Hawking radiation in 1977), had then begun studying quantum gravity theories, and had turned to string theory in the mid-1980s. By spring 1988, Candelas and his students Rolf Schimmrigk and Monika Lynker were struggling to understand what it was that Gepner had, in fact, done—what exactly was he saying about the relation between conformal field theory and geometry? After giving a seminar to the physics group in Austin during the spring of 1988, Dixon crowded into Candelas's office along with the students to discuss the meaning of Gepner's analysis. Dixon showed the Austin group how in Gepner's scheme labeling a set of particles a generation or an antigeneration was arbitrary. But on the geometrical side, he pointed out, there was a huge distinction between the geometries that would supposedly correspond to the two alternatives. In particular it was puzzling that finding one geometry (with negative Euler numbers) was easy but finding the geometry corresponding to the antigeneration, a supposedly equivalent field theory, was impossible. (The Austin group had practically no relevant manifolds with positive Euler numbers.)[23] Later on, Candelas recalled, "I remember being in seminars where Gepner would say that these Calabi-Yaus came in pairs with opposite Euler numbers. I remember saying: 'Gepner is crazy.' "[24] Crazy because the pairing Gepner conjectured demanded the so-far unseen manifolds with positive Euler numbers. Here it is worth being more precise about the characterization of a manifold.

Since the nineteenth century, mathematicians have had many ways of understanding the topological properties of a manifold—properties such as the genus, the number of holes—that were independent of the particular metric (a rule for calculating distances) imposed on the space. One of the most basic topological characteristics is captured by the Betti number, that is, the number of independent cycles of various dimensions that could be defined on the space. Intuitively (by-passing the precise definition of cycle) think of a torus. It has two closed curves that can be neither smoothly contracted to a point nor deformed to one another: the cycle that goes the long way around a donut and the cycle that goes around the circular cross-section. Since the Betti number is a topological characteristic, so are any linear combinations of Betti numbers, and, in particular, the Euler characteristic can be defined as the alternating sum of Betti numbers. For a real 3-dimensional manifold, for example,

$$X = b_0 - b_1 + b_2 - b_3.$$

Generalizing to the case of complex manifolds, mathematicians defined the generalized Betti number (known as the Hodge number, $h^{p,q}$) to be a count of the number, p, of complex cycles and the number, q, of complex conjugate cycles. For the class of manifolds considered here (Kähler), the relation between the Hodge numbers and the Betti numbers is just one of addition: b_2, for example, is just a sum of all the Hodge numbers such that the total number of cycles is 2, that is, where $p + q = 2$. So here $b_2 = h^{1,1} + h^{2,0} + h^{0,2}$.

In general, one could say a great deal about a 3-dimensional complex manifold by writing down all its Hodge numbers, a task conveniently displayed in the Hodge Diamond:

$$\begin{array}{ccccccc}
 & & & h^{0,0} & & & \\
 & & h^{1,0} & & h^{0,1} & & \\
 & h^{2,0} & & h^{1,1} & & h^{0,2} & \\
h^{3,0} & & h^{2,1} & & h^{1,2} & & h^{0,3} \\
 & h^{3,1} & & h^{2,2} & & h^{1,3} & \\
 & & h^{2,3} & & h^{3,2} & & \\
 & & & h^{3,3} & & &
\end{array}$$

Simple complex conjugation takes a complex variable into its conjugate (as in $x + iy$ goes to $x - iy$). Accordingly there is a symmetry around the vertical axis: $h^{1,0}$ must equal $h^{0,1}$, for example. Other symmetries enforce an identity of terms flipped over the horizontal axis, for example, $h^{2,0} = h^{3,1}$. The conditions that define a Calabi-Yau manifold set certain Hodge numbers equal to 0 and others to 1. When the dust settled, there were only two surviving, independent terms ($h^{1,1}$ and $h^{1,2} = h^{2,1}$) in the Hodge Diamond of a Calabi-Yau manifold:

$$
\begin{array}{ccccccc}
 & & & 1 & & & \\
 & & 0 & & 0 & & \\
 & 0 & & h^{1,1} & & 0 & \\
1 & & h^{2,1} & & h^{2,1} & & 1 \\
 & 0 & & h^{1,1} & & 0 & \\
 & & 0 & & 0 & & \\
 & & & 1 & & &
\end{array}
$$

Roughly speaking, the Hodge number $h^{1,1}$ counts the number of nontrivial 2-surfaces on the Calabi-Yau manifold (that is, the number of 2-dimensional surfaces); $h^{1,1}$ also counts the number of parameters that rescale the manifold without changing its shape. By contrast, $h^{2,1}$ leads easily to the number of 3-surfaces (the number of 3-surfaces is $2\ h^{2,1} + 2$); $h^{2,1}$ also gives the number of parameters that change the shape (complex structure of the manifold) without altering its topology. Summing up the Hodge numbers in an appropriate way gave the Euler number and, as discussed above, the Euler number is directly proportional to the number of generations. For the 3-dimensional Calabi-Yaus

$$|X| = 2\,|(h^{1,1} - h^{2,1})| = 2 \text{ (number of generations).}$$

So by late spring 1988, Candelas and his group understood Gepner's conjecture as posing a well-defined puzzle: if a family of particles was defined on a Calabi-Yau of Euler number X, then the antifamily would be defined by a manifold of Euler number $-X$. If this was right, Calabi-Yau manifolds that corresponded to conformal field theories came in pairs—one with X and one with $-X$—both of which essentially corresponded to the same (conformal) string theory.

"You see," Candelas mused, "the mathematicians were never after thousands of examples of something like Calabi-Yau manifolds—they knew half a dozen. You don't get promoted in math for such things."[25] By contrast, the physicists *were* after thousands of these manifolds; they were precisely interested in the grubby details of their internal geometric structure—for in one of those thousands of manifolds, in some set of geometrical relations (so they hoped), was more than a mathematical example. Somewhere among these imagined structures was the one manifold that would yield an effective theory corresponding on one side to a compactification of a still-to-be found Planck-scale string theory, and on the other side to the zoo of observed particles that came flying out of particle colliders. Somewhere in the panoply of manifolds might lie the one solution to the theory of everything.

Brian Greene was one of the physicists knocking on mathematicians' doors. Greene had been an undergraduate at Harvard, then pursued a doctorate at Oxford with Roger Penrose, finishing in 1986. With a National Science Foundation (NSF) postdoc in hand, he then returned to Harvard to work with Vafa in the hopes of doing something "more physical" than the cosmological work he had followed in England. Greene sought to exhibit, explicitly, the manifold pairings that Gepner, Dixon, and Vafa thought existed. By sewing together pieces of manifolds in a suitable way, Greene and a graduate student, M. Ronen Plesser, found explicitly the missing manifold of opposite Euler number to the ones that Gepner had explored.

Remarkably, one member of the pair $h^{1,2}$ corresponded to the $h^{1,1}$ of the other (and vice versa)—a mirror flip, if you will, across a diagonal axis on the Hodge Diamond. And since this flip interchanged those two quantities, $X = 2\,(h^{1,1} - h^{2,1})$ switched sign. For the well known Calabi-Yau with $X = -200$, Greene and Plesser now could offer one with $X = +200$; for the one with $X = -88$, there now was a twin with $X = +88$.[26] And this sewing and gluing process would, by construction, leave the basic physics (the conformal field theory) unchanged in all its predictions despite the fact that seemingly very different properties of the twin manifolds had switched roles. In short,

$$\text{Calabi-Yau}\ \underline{\quad}\ \text{Conformal Field Theory} \Leftrightarrow \text{Mirror Calabi-Yau}$$

This was a remarkable state of affairs: two different manifolds—different in their very topology—were indistinguishable in terms of physical predictions. While $h^{1,1}$ counted the ways in which size could be altered (rescalings of the metric), $h^{2,1}$ did something completely different—it altered the *shape* of the manifold by changing its complex structure. Nothing in general relativity prepared the physicists for this twinning: in general relativity, to have a radically different geometry meant a different physical situation.

Meanwhile, unknown to Greene and Plesser, Philip Candelas was exploring the same territory, but using very different lines of reasoning. Candelas developed a way of constructing large classes of Calabi-Yaus and finding their Hodge numbers, and from that, extracting their Euler numbers. Up to that point most of the known Calabi-Yau manifolds had negative Euler numbers, so it seemed highly unlikely that they actually existed in pairs. But in the spring of 1989, to check the conjecture Candelas, along with Lynker and Schimmrigk, did what physicists do when encountering a question of large numbers of entities. They tried lots of examples. More specifically, they generated some six thousand examples of these newly discovered Calabi-Yaus on the computer and printed out a scatter plot. "We did not believe they came in pairs," Candelas noted. "We wanted to know if we could find Calabi-Yaus with positive Euler numbers. Could there be a large number of Calabi-Yaus with positive X? Could the computer do the job in less than the age of the universe?" As it turned out, yes and yes.

When Candelas and his collaborators saw the Euler number printout of their various Calabi-Yaus, they were floored. Roughly speaking, the number of positive and negative Xs were equal. Moreover, there were some twenty-five hypersurfaces with Euler number 6 (or −6) meaning there would be, as needed, three generations. Candelas: "When I got this graph, I brought it to Brian Greene and said, 'You're going to fall off your chair when you see this.' He said 'we know that—we know they [Calabi-Yau manifolds] come in pairs.' "[27]

Greene, as it turned out, remained upright in his chair because he, too, had been grappling with Gepner's wild idea. Having taken a smaller class of surfaces than Candelas and Xenia de la Ossa had considered (Greene and Plesser were using the equation $z_1^5 + z_2^5 + z_3^5 + z_4^5 + z_5^5 = 0$, with no cross terms between the different z's), the two Harvard postdocs had begun building up conformal field theories on these Calabi Yau manifolds and discovered, to their immense surprise, that they indeed could get pairs with opposite Euler numbers. So when Candelas came to his pairing conclusion on the wider set, Greene was ready to believe. Now there were two pieces of evidence for what Greene had begun calling "mirror symmetry." From Candelas came a rough argument (same number of positive and negative Euler numbers for a wide class of manifolds) and from Greene a precise argument restricted to a narrower class of manifolds: Greene and Plesser could show exactly that one and the same conformal string theory could sit on Calabi-Yau manifolds with opposite Euler numbers.

With this concordance in hand, Candelas and Greene began showing their results to both physicists and mathematicians. Not surprisingly, Vafa liked the result—it confirmed his earlier conjecture. But when Greene walked his result the fifty yards or so from the physics department to Shing-Tung Yau in the Harvard mathematics department, the reception was quite different. As far as Yau was concerned there simply was no reason for this pairing; he was sure they had simply made a mistake. Candelas too found the mathematicians highly dubious. Yau just did not think that his manifolds were twinned. "I was beginning to believe that they came in pairs," Candelas said. "But the mathematicians were not having any of it. We published the diagram and I began phoning up the mathematicians—we got no response. There just wasn't any reason why they *should* come in pairs. The first time I mentioned it to Yau, he said it must be bullshit."[28] To other mathematicians the arguments by Greene, Candelas, and their collaborators seemed dreamlike, incomprehensible. For the mathematicians around 1990, the two Calabi-Yaus seemed utterly unrelated. First, the conformal field theory itself meant nothing to them—the fact that the same theory could be defined equivalently on the two manifolds simply did not move them one way or the other. Second, almost all the Calabi-Yaus known had negative Euler numbers, so the idea of each Calabi-Yau having a twin of opposite Euler number seemed preposterous. Third, the manifolds themselves seemed unrelated. The moduli space (the space defined by the parameters that define the vacua of the theory) on one side involved deformation of the complex structure of the Calabi-Yau and had a special geometry (variation in shape). On the other side, the moduli space was parameterized by deformations in the Kähler structure (variation in size).

For the physicists around 1990, the mirror symmetry conjecture seemed puzzling for completely different reasons. In the moduli space defined by the parameters that varied complex structure, describing quantum corrections to scattering of strings was particularly easy—it could be shown that these were not quantum corrected. The geometry was exact. On the other side (the moduli space built on the size-changing, "Kähler" deformations), the scattering amplitude was corrected. Because of the quantum corrections, the geometrical picture, it seemed, would be lost. So how, the physicists asked, could these two different realizations of the theory possibly be physically equivalent?

To understand why the result was, nonetheless (physics notwithstanding), so entrancing to mathematicians, we need to turn away from the physics of strings, away from conformal field theories and scattering amplitudes, and toward the mathematicians' own world of algebraic and enumerative geometry.

When mathematicians approach systems of algebraic equations in several variables, they are after the structure of the solutions. One way to get that is to consider the space of solutions to the equations as a topological manifold, a manifold in which one does not assume the existence of a distance but only the topological properties such as the number of holes in a surface. Algebraic geometers, by contrast, consider the equations as having coefficients in (an arbitrary field) and the solutions to these equations lie (in its algebraic closure). As one textbook put it, "the arguments used are geometric, and are supplemented by as much algebra as the taste of the geometer will allow."[29]

There are high-brow problems in algebraic geometry—problems of classification of manifolds, for example. But it was not the high flyers that made first contact with the physicists but rather the mathematicians who aimed at high-level classifications by studying specific examples. Herbert Clemens loved examples. Having taken his doctorate in complex geometry with Philip Griffiths in 1966, Clemens was after variations of the complex structure of 3 (complex) dimensional manifolds. For odd dimensions a simplification exists because the local spatial structure (cohomology) has a simple extra symmetry. And for 3-folds there is a further reduction in difficulty if the Hodge numbers at the far left and far right of the Hodge Diamond are zero. That is, if the Hodge numbers $h^{3,0} = h^{0,3} = 0$, then, roughly speaking, the 3-dimensional manifold is called "of Fano type" and behaves in certain respects as if it were a *one*-dimensional object. A particularly simple Fano-type 3-fold is the cubic hypersurface in projective 4-space. And it is there that Clemens did his work. An old conjecture stipulated that this hypersurface was equivalent to an affine space (a space without a metric but where distance could be defined along parallel lines). Clemens and Griffiths proved it was not.

Over the years, Clemens continued studying these Fano spaces, exploring the structures of these simplified 3-dimensional entities and how they related to the one-dimensional case. "I much preferred working with concrete examples," he said, "and then, after many years, trying to see generality." The holy grail of people in the field was to prove Hodge's conjecture that, for Fano-type 3-folds, translates to the assertion that the homology—the set of nonequivalent cycles that specify the topology of the manifold—would be parameterized by closed loops of algebraic curves. "There are many out there who would sacrifice an arm and a leg and probably more if they could prove the Hodge Conjecture, even in a restricted setting close to the Fano 3-fold case."[30]

One way for Clemens to pursue this purely mathematical goal was to examine the simplest case where one could *not* move the curves around. And the Calabi-Yaus defined by a quintic hypersurface in projective 4-space seemed to fit that bill: it was the lowest degree hypersurface that was not nearly Fano and it certainly had a nontrivial homology. If Hodge was right, then the rational curves (the special category of curves that might parameterize the general curves) would have to be rigid for a general quintic—that is, not distendable into one another.

So for the physicists, Calabi-Yau 3-manifolds were the conceptual site on which they expected to describe a realistic, three-generation string theory at low energies. But for the mathematicians, Calabi-Yau manifolds were to be a site for exploring the Hodge Conjecture in the particular case of Fano-like 3-folds. Now, if the rational curves really were rigid—and Clemens had shown that some were rigid in arbitrarily high degree—then a homogeneous coordinate z_i could be restricted to a function of arbitrarily high degree on a rigid curve. So one ought to be able to count curves of fixed degree.[31] While there would still be much else to understand, a determination of their number would say at least *something* about these curves. So Clemens encouraged Oklahoma mathematician Sheldon Katz to take a look.

Counting geometrical objects like this—known as enumerative geometry—was a modest corner of algebraic geometry. Katz vividly remembered advice he got soon after graduate school at Princeton: building a reputation as an enumerative geometer might kill his budding career.[32] Although mathematicians had long admired the tantalizing numbers of enumerative geometry, these numbers were by no means the most highly prized, most abstract results of algebraic geometry. One old result, due to the magician-like Hermann Schubert in the nineteenth century, showed that exactly 27 lines could be drawn on a general cubic surface.[33] Next on the totem pole of such counting problems was the task of finding the number of rational curves of higher degree that could be put on a quintic 3-fold. The simplest case, lines (curves of degree one), had been calculated in 1979; it turns out, as Joe Harris showed, that there are 2,875 of them. The next case, that of conics (curves of degree two), fell to Sheldon Katz in 1986, who had met Clemens's challenge: there were 609,250 of them.[34] What Katz really wanted to know was what lay behind these numbers: "I wanted to know what makes Schubert's ideas work. What deep unexpected relationships were there on higher-dimensional manifolds?"[35] By the end of the 1980s, it was well known among enumerative geometers that the next case, the tally of curves of degree three, was going to be vastly harder to compute. Two Norwegian mathematicians were hard at work on it; I will return to them in a moment.

Suddenly, in late 1990, Candelas and his collaborators Xenia de la Ossa, Paul Green, and Linda Parkes (COGP) saw a way to use mirror symmetry to barge into the geometers' garden.[36] From Brian Greene and Ronen Plesser, Candelas knew there was, in principle, an equation that allowed one to pass from calculations of string interactions on a manifold to the same calculation on the mirror. Greene and Plesser thought actually computing some key quantities would simply be too hard—Candelas, who liked calculating things, thought maybe it was in fact tractable using some algebra and a home computer. Candelas and his younger colleagues reasoned this way. Suppose three strings interact. Since the conformal theories are essentially the same whether described in terms of a Calabi-Yau manifold, M, or its mirror, W, the amplitude for the string collision must be the same whether computed on M or W. On one manifold this expression turned out to be relatively easy to evaluate. On the mirror manifold, the expression for the amplitude amounted to the number of rational curves. So suddenly the computation of a seemingly impossible quantity became nearly trivial.

This is worth explaining in more detail. A string traveling through space-time carves out a surface; a closed string depicts a cylindrical surface (world sheet or surfaces of higher genus). Quantum mechanically, a particle takes all possible paths, as Richard Feynman taught back in the 1940s. So in computing an amplitude—the probability of a three-string interaction—one must consider all the appropriate world-sheet embeddings in the Calabi-Yau manifold. Now this manifold could well have uncontractible holes in it (hollowed out spheres), and so the interaction among strings must be corrected by terms corresponding to the string world-sheet wrapping, in a minimal and smooth way, around these spheres one, two, three, or more times. Such uncontractible topological defects are known as instanton corrections, and increasing the number of wrappings corresponds to making higher order corrections. From the mathematicians' perspective, each winding number around a defect corresponds to a particular degree curve on the quintic hypersurface. So what for the physicists was an expansion yielding the quantum (instanton) corrections was for the mathematicians a series giving the number of curves from lines to conics, to cubics, and so forth.[37] But this identification while interesting was not yet news. Neither physicists nor the mathematicians could compute the series.

Mirror symmetry broke the computational blockade. Suddenly the computation on the size-changing (Kähler) manifold could be equated to the shape-changing (complex structure) mirror manifold. All at once Candelas and his collaborators had an answer to the enumerative geometers' dream: Even if the problem was intractable on the quintic hypersurface, any conformal field theory calculation there could be converted to the mirror manifold that could then be deformed in shape as desired. But just this shape invariance kept calculations on the mirror from being quantum corrected: calculations on *that* side of the mirror equation could be done easily, and explicitly, with some algebra and a Mac.[38]

Astonishingly, at the end of a brief calculation, Candelas and crew had the next member of the series, the till-then elusive number n_3, that they assessed at 317,206,375. But the mathematicians were dubious—the methods the physicists used corresponded to nothing remotely comprehensible to the mathematicians. Conformal field theories, Feynman path integrals—as far as the algebraic geometers were concerned, these were ill-defined concepts plucked like a clutch of rabbits out of thin air. So COGP went back to their Mac and offered the mathematicians the next number, n_4, and the next after that, n_5, all the way up the series to n_{10}. To the mathematicians the whole procedure, from start to finish, seemed dubious indeed.

Meanwhile, the Norwegian mathematicians, Geir Ellingsrud and Stein Arild Strømme, were struggling with a direct, mathematically grounded, *geometrical* computation of the number of rational curves. Both had taken mathematics degrees in Oslo, both had completed doctorates with the same advisor (Olav Arnfinn Laudal), and they had been collaborating since 1978 on a range of mathematical problems surrounding the moduli spaces for various geometric structures and in particular on cohomology or intersection theory. By 1987, they had cranked up a crude desktop computer, a Sinclair Spectrum, to help with their computations, and, over the course of the next years, they studied the space of twisted (rational) cubic curves. Then in 1989 Ellingsrud and Strømme met Clemens who, fresh from his success in motivating Katz to count the curves of n_2, now gave similar encouragement to the Norwegians to reckon n_3. Ellingsrud and Strømme thought they could do it—after all, having completed years of work on twisted cubics and their mobilized Spectrum, they thought they could find intersections of the twisted cubics with just about anything. It was equally obvious that the calculation of n_3 would be far too difficult to undertake by hand. And so, though both were relative neophytes on the computer, they pounded out a new computer package, using an algebraic program known as Maple. For almost a year they worked at it until, in June 1990, the computer displayed the fruits of its labor: $n_3 = 2{,}682{,}549{,}425$.

Instantly, they shot the ten-digit number over the Internet to Sheldon Katz, Herb Clemens, and others; Katz electronically relayed the news to Candelas:[39]

> Dear Sheldon Katz, June 6, 1990
>
> We finally got a number for the number of twisted cubics on the general quintic threefold. I know that Geir saw you not long ago, but he is out of reach for the moment so I ask you directly this way what you know about the number. We get 2 682 549 425 = 5^2*17*6311881. I get the impression from your 1986 papers that you only claim that the number is divisible by 5, but Herb Clemens thought perhaps it should be 3*5^3. Obviously there is the possibility of a programming error on our part (we used Mathematica, Maple and Macaulay, and MPW to shuffle results back and forth between these), but it would be most interesting if you can say right away that this number has to be wrong. . . .
>
> Best regards,
> Stein Arild Stromme

Now Candelas found himself in a complicated position. On the positive side, he had a computer program that had produced a scatter plot strongly indicating that mirror symmetry should work over a large class of manifolds and the mirror symmetry hypothesis squared with the older conjectures and the more recent constructions by Greene and Plesser. On the negative side, he had a bevy of very dubious mathematicians. On the positive side again, he could reproduce the mathematicians' n_1 (2,875) and even the three-year-old result of Sheldon Katz for n_2 (609,250). All this was remarkable. But the third member of the series, the long-sought n_3, clashed directly. The Norwegians posted 2,682,549,425, and that was not the same as physicists' 317,206,375 no matter how you sliced it. These were numbers that *had* to be the same, but they were not. Candelas and his collaborators went over the calculation again and again. There would be no middle ground: either the physicists or the mathematicians were wrong.

Between physics and mathematics

In part propelled by the clash over n_3, a corps of mathematicians and physicists scheduled a workshop at Berkeley's Mathematical Science Research Institute for 6–8 May 1991. From the physics side were Greene, Plesser, Candelas and his collaborators, Vafa, and Witten among others; among those from the mathematics side Yau, Katz, Ellingsrud, Strømme, and Shy-Shyr Roan. A lingua franca did not come easily, and each day's lectures were followed by intensive question sessions during the evenings. As Yau put it, both mathematicians and physicists "each attempted to grasp the vantage point and conceptual framework of the other." Language as well as specific expertise made communication exceedingly difficult. "As with any important new development which straddles traditional academic disciplines, two pervasive obstacles facing prospective adherents are the differences in language and assumed knowledges between the respective fields."[40] The "language gap" was echoed throughout the papers, as Greene and Plesser stressed in their contribution: "At present, mirror symmetry finds its most potent expression in the language of string theory," for it was principally the fact that both manifolds contained identical physical theories—the same conformal field theory—that vouchsafed (for physicists) the equivalence of the underlying manifolds.[41] Mathematicians, by contrast, simply took "string" to be a "mnemonic" for a more precise definition.[42]

Despite—or perhaps because of—the numerical clash, David Morrison, a mathematician from Duke University, saw in Candelas's form of argumentation something important for mathematicians. Exactly what was harder to say. "The language problem," as Candelas put it, "is a very difficult barrier to surmount."[43] Some of the problem revolved around specific concepts, as Candelas noted:

> There were aspects of this that had been terribly mysterious—very hard to think of modularity space and how you moved around it. Monodromies—to a physicist a complex structure was to be understood by hypergeometric functions represented by integrals. The really fundamental thing would always be these integrals, though as the calculations got more complex, one encountered higher-dimensional versions of these hypergeometric functions. For the physicists the fact that if you walked around a singularity, it was mysterious that quantities altered in certain ways.[44]

Mathematicians saw not mystery in the movement of these points around the surfaces but straightforward geometry. Monodromies—well known geometric entities—were quantities that were locally single-valued but changed values if one took them around a nontrivial closed path. Conversely, for the mathematicians the existence of mirror symmetry itself was mysterious. For the physicists, however, it was not—it came out of "natural" assumptions about the conformal field theory—and the key quantitative features, coefficients of various quantities, were merely factors that had to be introduced to keep track of coordinate changes.

For the algebraic geometers the idea of treating key coefficients as if they were a coordinate transformation appeared unrigorous, even arbitrary, as Morrison made clear in a talk during July 1991, a talk in which he deliberately tried to place the physicists' result in a language familiar to mathematicians from standard techniques in number theory:

> By focusing on this q-expansion principle, we place the computation of [COGP] in a mathematically natural framework. Although there remain certain dependencies on a choice of coordinates, the coordinates used for calculation are canonically determined by the monodromy of the periods, which is itself intrinsic. On the other hand, we have removed some of the physical arguments which were used in the original paper to help choose the coordinates appropriately. The result may be that our presentation is less convincing to physicists.[45]

Indeed, while to a physicist the invocation of a patchwork of plausibility arguments was entirely reasonable, to the mathematicians, the physicists' formulations looked ill-defined, practically uninterpretable. Morrison continued his quotation from COGP to show the mathematicians just how cryptic it looked in the original: "To most pairs (X,S), including almost all of interest in physics, there should be associated . . . ." What, Morrison asked, was the scope of "almost all," especially when applied to a category picked out by the (mathematically) incomprehensible property "of interest in physics"? To this he added: "To be presented with a conjecture which has been only vaguely formulated is unsettling to many mathematicians. Nevertheless, the mirror symmetry phenomenon appears to be quite widespread, so it seems important to make further efforts to find a precise formulation."[46]

Again, Morrison took an extract of the COGP paper, and while emphasizing throughout his argument that the physicists' conjecture was of great interest, he concluded that the formulation struck mathematicians as completely unrigorous. From COGP, he reproduced, verbatim, the justification for their formula that counted rational curves on quintic threefolds:

> These numbers provide compelling evidence that our assumption about the form of the prefactor is in fact correct. The evidence is not so much that we obtain in this way the correct values for n_1 and n_2, but rather that the coefficients in equation [$K_{ttt} = 5 + 2875\ e^{2\pi i\ t\ n} + 4876875\ e^{4\pi i\ t} + \ldots$] have remarkable divisibility properties. For example the assertion that the second coefficient 4,876,875 is of the form $2^3\ n_2 + n_1$ requires that the result of subtracting n_1 from the coefficient yields an integer that is divisible by 2^3. Similarly, the result of subtracting n_1 from the third coefficient must yield an integer divisible by 3^3. These conditions become increasingly intricate for large K. It is therefore remarkable that the n_k calculated in this way turn out to be integers.[47]

"These arguments," Morrison added, "have a rather numerological flavor" reminiscent of the physicists' speculations about the "monster group" that mathematicians had at first labeled "moonshine."[48] Maybe this case of mirror symmetry would turn out to be more than moonshine, too. But, as Morrison noted, there remained the stubborn numerical obstacle presented by the Norwegian mathematicians: "Unfortunately, there seem to be difficulties with n_3."

As Ellingsrud and Strømme mentally stepped, line by line, through their program, a glitch suddenly leapt out at them. Two subroutines, "logg" and "expp" figured in the calculation, routines related to ordinary logarithmic and exponential functions. Just as in ordinary logarithms on a slide rule, these little programs sped up the manipulation of truncated power series expressions, reducing multiplication and division to simple additions and subtractions. But where, ordinarily, these functions were applied to terms that had constant terms equal to 1 (where the log of 1 is 0) in this application, the constant term, W1, was *not* unity. And since "logg" ignored the constant terms, any information contained in W1 was lost. The next line's routine, "expp" could not retrieve it. The instant that logg hit W1, the calculation was doomed.

Swapping out the faulty lines, they reran the program. And on Thursday, 31 July 1991, the mathematicians' barrier vanished: their computer too spat out n_3 = 317,206,375. Herb Clemens received an email from the Norwegians and immediately forwarded it to Candelas with its white-flag header: "Physics wins!"

> Date: Wed, 31 Jul 91 11:06:34 MDT
> From: Herb Clemens ⟨xxx@xxx.edu⟩
> To: candelas@YYY.edu
> Subject: Physics wins!
> Message-Id: ⟨AAA@XXX.edu⟩
>
> To: Herb Clemens ⟨xxx@xxx.edu⟩
> From: stromme@zzz (Stein Arild Stromme)
> Dear Herb,
> we just discovered the mistake in one of the computer programs. Once that was corrected, our answer is the same as that of Candelas & Co. I am glad we found it at last!
> Best regards,
> Stein Arild

Returning to the electronic preprint file, Morrison drew a line through the words "Unfortunately, there seem to be difficulties with n_3" and inscribed, "Not any more!"[49] To Joe Harris at Harvard, Geir Ellingsrud shot a similar electronic concession, probably also that same day:

> We found an error in the program we use computing the number of twisted cubics on quintics a few days ago. After having fixed it, we now get the same number as the physicists. I feel a little bad about not having discovered the error before, but that's life and for mathematics I think the outcome is the best. Please tell Yau and the others about it.
> Best regards, Geir.[50]

It was a hard turn of events for Ellingsrud and Strømme. Harris offered some consolation in a return email: "Don't feel bad about the miscalculation, though—it seems to me that the point of all this is not the number but the ideas and techniques, and those are if anything vindicated. Joe."[51]

During the summer of 1991, while Morrison was struggling with the gap between mathematicians' and physicists' views, Sheldon Katz arrived at Duke for a previously scheduled, year-long visit. The two of them began interrogating the physics graduate students and inscribed lists of terms on the blackboard, a list beginning "conformal field theory," "correlation functions," and continuing on from there. The next year, Morrison shared an office with Brian Greene at the Institute for Advanced Study, and they set aside an hour each day to lecture to each other. By the time Greene and Yau had assembled a second major mirror symmetry volume in 1997,[52] the joining of mathematics and physics was legible in the author list: now instead of explaining the physicists' work to the mathematicians, Morrison and Greene, among others, were coauthoring papers. "One of my roles," Morrison commented a few years later, "is as an interpreter between the two communities. They are after different things and I've tried hard to maintain the distinctions. Mathematicians want to know which parts of this stuff are proven rigorously and which parts are conjectural. Physicists don't see that—they don't like to be told something is not a theorem. They have an argument and think it is so. Different standards."[53]

The borderland prospered. Physicists and mathematicians alike began talking about geometry in a radically new way. Ordinary geometry—the geometry built up out of points—held a special relation with a physics predicated on point particles. But now that strings were beginning to take over, it was becoming apparent that the point-based geometry was only a limit, just the way at distances large compared to the Planck scale of 10^{-33} cm, space "looked" as if it were made up of points. So it was imagined to be in geometry: another geometry, the hidden half, so to speak.[54] String theory, on this interpretation, offered that generalized geometry and reduced in an appropriate limit to classical geometry. The situation was analogous to a noncommutative algebra of quantum operators that reduced to the commutative case as the Planck constant h headed to zero.

Mirror symmetry was but one of the boundary regions increasingly populated by both string theorists and mathematicians. Edward Witten used the conformal field so essential to string theory to prove novel results in the theory of knots, and string theory led to a new understanding of an enormous simple finite discrete group dubbed the Monster and a host of new insights into, inter alia, Donaldson theory of 4-manifolds and a new proof of the Atiyah-Singer index theorem.[55] Writing to the National Science Foundation in 1994, Witten and mathematician Pierre Deligne, his colleague at the Institute of Advanced study, outlined a plan designed to capitalize on this new domain. "For most of the past hundred years," they wrote, "the role of theoretical physics has been to explain known experimental or observational phenomena and to make predictions that then lead to further experimentation." Such ventures covered a broad range of phenomena from quantum mechanics to general relativity, all of it borrowed from already extant mathematics, including Riemannian geometry. "In such significant historic examples, the mathematics did not drive the physics, but it was ready at hand and utilized by the physicists with little need for reference back to the mathematical foundations."[56] Now that was changing, the authors argued. With physicists digging deeper into string theory, it became apparent that the required mathematics did not exist—though isolated pieces of recent math had proved useful.

Because the larger part of what they required was not available, they have pushed the mathematics, frequently on an ad hoc basis, leading to startling predictions. Physicists' arguments do not automatically translate into mathematical proofs but have yielded striking new mathematical ideas and results. These results have usually involved what physicists perceive as manifestations of the unknown new conceptual-geometical framework of string theory.

The structure of this common cause would involve senior mathematicians who "can listen to the physicists and communicate their ideas to [other] mathematicians in a way that captures the physical context of their thought." The geographical scope of possible mathematical recruits included MIT, IHES, Tohoku, Oxford, and Cambridge. Physicists would come from Harvard, UCLA, the University of Chicago, Tokyo, Texas A&M, and UCSB. Four to six "younger researchers" would enter the program to "develop in ways noticeably different from those of their colleagues who are more traditionally focused, either in mathematics or physics [the new breed of researcher] should feel equally at home in both worlds."[57]

Contested boundaries

The National Science Foundation replied, with regrets. It is of great interest to understand why. One referee began by lauding the contributions of the principal investigators, and celebrated the increased use of physics by mathematicians. But "I do not think that this activity [on the border between mathematics and physics] should result in the production of a large number of physics students who would work in this field." Others reiterated that sentiment: excellent investigators but the NSF should pause before further "populating" the border region. "I fear," another wrote, "that in this case the results may be analogous to those obtained by axiomatic field theory in the past—which did not further physics understanding of field theory in a substantial way." The harshest critic agreed in his praise for the leaders (Witten "is probably the best person in the entire galaxy to lead the proposed program") but the program itself raised fundamental questions about the direction of physics. "My conscience would not rest if I did not record those doubts here, even though I am fully aware that my opinion is highly contrarian." The referee continued:

> I tend to think that the most conspicuous development of the last decade is the training of a generation of very bright young theorists who know and care more for geometry and topology than for the standard model and current experimental efforts to discover the next step beyond it. Since I am convinced that the key advances in physics emerge from physical rather than mathematical insight, I must view this as a negative development. I think that theoretical physics would be in better shape if this group of very capable people had been taught

to practice research with better balance between physical fact and mathematical intuition.[58]

Ultimately, this evaluator's greatest concern was not for the mathematicians but for the physicists, especially young ones, whom the program "would tend to subvert." Mathematics, the referee continued, was a tool, but one that must be secondary to the concerns of a fundamentally physical nature. "The main goal of theoretical physics is to understand the laws of nature, and for most of the 20th century this has involved a closer connection between the most capable theorists and experiment than exists today in particle physics."[59] What was needed, this reviewer argued, was an amplification, not a diminution of that bond between laboratory and blackboard.

After revision, Witten and Deligne resubmitted the proposal, armed with a more detailed exposition of new results—this time successfully. Again they aimed to create an environment that would allow mathematicians to explore links between previously disconnected mathematical domains, fields that, to the physicists, appeared manifestly linked: "before the current period, mathematicians have tended to try to treat each idea coming from physics as a separate, isolated phenomenon, with proofs to be given in each case in an *ad hoc* fashion, unrelated to the context in which the ideas appeared." It was necessary, Witten and Deligne argued, for the mathematicians to step beyond such a piecemeal approach, to see the physicists' problematic in its "natural context," not in vitro. For the string physicists over the two years prior (1993–95), it had become common wisdom to see the various different string theories as asymptotic versions of some unknown, underlying theory. And grasping the mathematics of this theory—in the absence of the fundamental symmetries, variables, and geometrical ideas governing it—was exceedingly difficult and would surely involve both new physical ideas and "mysterious new mathematical structures." At stake, the authors contended, was the future: "The extent of success in understanding what that theory really is will very likely shape the fate of physics in our times."[60] The referees concurred; funding followed.

Once approved, prospective applicants found descriptions of the planned math-physics collaboration on the Web, including one posted on 15 December 1995 that sought to differentiate the current collaboration from previous uses of physics by mathematics:

> It is not planned to treat except peripherally the magnificent new applications of field theory. . . . Nor is the plan to consider fundamental new constructions within mathematics that were inspired by physics, such as quantum groups or vertex operator algebras. Nor is the aim to discuss how to provide mathematical rigor for physical theories. Rather, the goal is to develop the sort of intuition common among physicists for those who are used to thought processes stemming from geometry and algebra.[61]

I have used the IAS as an example but it was not alone in its search to create a new kind of scientist, a new personhood in science, if you will, one not only with particular problems and procedures but with a hybrid way of thinking. At stake were not only "thought processes" and "intuitions" but ultimately identity. Small wonder that the move met resistance. Some particle theorists—several of whom had been coauthors with some of the principal string theorists—took the withdrawal of string theory from experiment as the harbinger of a dark age of speculation. Charges of "theology" echoed off the walls, and the battles were fought over positions from graduate student through postdoc, junior, and senior faculty positions. The first round of NSF referees' responses to the Institute for Advanced Study's "Integrated Program" was but one site that revealed these tensions; similar battles erupted in physics departments across the country, precipitating a far-reaching debate over the nature and meaning of theoretical physics.

If the string theorists were to use mathematics as their new constraint structure, they had to find a modus vivendi with the mathematicians. And here the results were contradictory, informatively contradictory, forcing to the surface long-dormant resentments and ambitions. That they would need the mathematicians, however, was clear. Precipitating this stage of the debate was an article published by Arthur Jaffe from Harvard and Frank Quinn from Virginia Polytechnical Institute—both mathematical physicists heading mathematics departments in the early 1990s. Titling their July 1993 piece, " 'Theoretical Mathematics': Toward a Cultural Synthesis of Mathematics and Theoretical Physics," in the *Bulletin of the American Mathematical Society*, they unleashed a torrent of response by public and private email and in print—and in the process surfaced views about the nature and importance of theoretical and mathematical culture. Jaffe and Quinn began by recounting how the string theorists had lost their historical tie to experiment and then continue:

> But these physicists are not in fact isolated. They have found a new "experimental community": mathematicians. It is now mathematicians who provide them with reliable new information about the structures they study. Often it is to mathematicians that they address their speculations to stimulate new "experimental" work. And the great successes are new insights into mathematics, not into physics. What emerges is not a new particle but a description of a representation of the "monster" sporadic group using vertex operators in Kac-

> Moody algebras. What is produced is not a new physical field theory but a new view of polynomial invariants of knots and links in 3-manifolds using Feynman path integrals or representations of quantum groups.[62]

Here we have the crux of the issue. Physicists were using their standard set of tools (such as Feynman path integrals, vertex operators, and representations of quantum groups) to solve mathematicians' problems—and not trivial ones, either: representations of the Monster sporadic group, polynomial invariants of knots and links in 3-manifolds. Suddenly, the hard-won theorems of mathematics were being exceeded by methods the mathematicians found utterly lacking in rigor.

Rather than reject this incursion into mathematical territory out of hand, Jaffe and Quinn wanted a "dignified" name for the activity that would nonetheless isolate it from the mainline of rigorous mathematics. Borrowing from physics itself, they chose the name "theoretical mathematics": "The initial stages of mathematical discovery—namely, the intuitive and conjectural work, like theoretical work in the sciences—involves speculations on the nature of reality beyond established knowledge. Thus we borrow our name 'theoretical' from this use in physics."[63]

By employing the term *theoretical mathematics* in this way, they deliberately displaced two older, competing notions of *theoretical*. On one hand, they refused to identify "theoretical" in mathematics with the "pure" in contrast to "applied" mathematics. On the other hand, they refused to employ "experimental" mathematics to designate computer simulations—for in fact they took such speculative explorations to be under their categorization, "theoretical."[64]

So far, just a redefinition. But words alone could not efface the different ways in which the two groups treated some of the same sets of symbols. Both mathematicians and physicists might want to characterize the knots and kinks in 3-manifolds, but the methods that each group used led to a direct confrontation:

> Theoretical physics and mathematical physics have rather different cultures, and there is often a tension between them. Theoretical work in physics does not need to contain verification or proof, as contact with reality can be left to experiment. Thus the sociology of physics tends to denigrate proof as an unnecessary part of the theoretical process. Richard Feynman used to delight in teasing mathematicians about their reluctance to use methods that "worked" but that could not be rigorously justified. He felt it was quite satisfactory to test mathematical statements by verifying a few well-chosen cases.[65]

Complementing Feynman's views, Glashow lambasted overly mathematical string theorists: "Until the string people can interpret perceived properties of the real world, they simply are not doing physics. Should they be paid by universities and be permitted to pervert impressionable students?" Conversely—and Jaffe and Quinn did not hesitate to raise the point—mathematicians had no great respect for the weightiness of physicists' contributions to knowledge. In one anecdote that resonates on a gendered as well as epistemological level, they likened the physicist's proof to the woman who traced her ancestry to William, the Conqueror . . . with only two gaps.

Exaggerating for emphasis, many mid–twentieth-century physicists thought that mathematicians were supererogatory and mathematicians thought that physicists were superficial. But by 1992, neither side could so lightly dismiss the other; in the past their fiefdoms had been at sufficient remove that each could polemicize at a distance. Now they overlapped on territory vital to both.

Branches of algebraic geometry had become a trading zone—with each side contributing to it, each interpreting joint results differently. For the physicists, mirror symmetry along with other dualities promised to become some of the most powerful theoretical tools they had available. It showed how some of the string theories might be further reduced by demonstrating their equivalence—and even offered a deep geometrical understanding all the way down to Lagrangian quantum field theory and classical electrodynamics. On the mathematical side, new forms of calculation corrected their own work and extended it in certain cases infinitely beyond their previous capacities.

Stepping between the fields was delicate work, as Morrison indicated in his contribution to the 1997 mirror symmetry volume. He argued that the enumerative geometry results gained through the new techniques were physically powerful, but that they should be used with mathematical caution: "The calculations in question can often be formulated in purely mathematical terms, but it should be borne in mind that the arguments in favor of the equivalence of the answers . . . rely upon path integral methods which have not yet been made mathematically rigorous. For this reason, mathematicians currently regard these calculations as *predicting* rather than *establishing* the results."[66]

One mathematical response to the unlocking of these mirror equivalents and similar string theory successes was a full-tilt emulation by some senior mathematicians of physicists' style of work. It was a route, Jaffe and Quinn cautioned, that was a minefield, littered with danger. Imitation had

> [Jaffe's and Quinn's] comparison of proofs in mathematics with experiments in physics is clearly faulty. Experiments may check up on a theory, but they may not be final; they depend on instrumentation, and they may even be fudged. The proof that there are infinitely many primes—and also in suitable infinite progressions—is always there. We need not sell mathematics short, even to please the ghost of Feynman.

Since World War II, he contended, physics had played the dominant role in American science—but the discipline was itself now in trouble. Mathematicians need not, ought not, pine after the methods of this crepuscular science. "Mathematics does not need to copy the style of experimental physics. Mathematics rests on proof—and proof is eternal."[75]

Tied to pedagogy, credit, and epistemic security, ultimately the culture war over theory had consequences for "reality," as Morris W. Hirsch, the Berkeley algebraist and differential geometer, made clear. His claim was that however much Jaffe and Quinn protested that they wanted to eschew terminology per se, they still wrongly spoke about "mathematical reality": "It is important to note at the outset that their use of 'theoretical' is tied to a controversial philosophical position: that mathematics is about the 'nature of reality,' later qualified as 'mathematical reality,' apparently distinct from 'physical reality.' They suggest 'Mathematicians may have even better access to mathematical reality than the laboratory sciences have to physical reality.' " On Hirsch's view, mathematics was not a theory *of* anything, and certainly not of a special branch of reality. Neither poems nor novels "referred," and mathematics was no different. True enough, Hirsch said, physicists used mathematics as a tool with which to construct "narratives" (his term). They might be telling a story about how a certain system worked and would use the concepts of mathematics to continue, as for example they characteristically would do when invoking the assumption of equilibrium, nonzero determinates, or the independence of random variables. But the uses of mathematics (on Hirsch's view) spoke not a word about the intrinsic referentiality of mathematics itself, and if, as he believed, "mathematical reality" was idle chatter, then the very category "theoretical mathematics" ought cede to the ontologically neutral one: speculative mathematics.[76] For many, including James Glimm from Stony Brook, more was at stake in this struggle than the referential structure of mathematics:

> It bears repeating that the correct standards for interdisciplinary work consist not of the intersection, but the union of the standards from the two disciplines. Specifically, speculative theoretical reasoning in physics is usually strongly constrained by experimental data. If mathematics is going to contemplate a serious expansion in the amount of speculation [it will] have a serious and complementary need for the admission of new objective sources of data, going beyond rigorously proven theorems, and including computer experiments, laboratory experiments and field data. [The] absolute standard of logically correct reasoning was developed and tested in the crucible of history.

Such standards, Glimm concluded, had to be preserved and defended in the rapid expansion of mathematical horizons.[77]

On the cultures of theory

String theorists, prominently among them Edward Witten, took their history from the young Einstein—the Einstein who constructed general relativity with the barest of experimental ties, such as the precession of the perihelion of Mercury. Georgi and Glashow, arguing against string theory, called to the stand a different historical Einstein—the aging hermit chasing after the illusion of unification, self-blinded to the worlds of Lagrangian quantum field theory, meson exchange, and new particles.

Mathematicians and mathematical physicists including Quinn, Jaffe, Mac Lane, Glimm, and Atiyah also battled over histories—the collapse of a brilliant but in some mathematicians' view too speculative Italian algebraic geometry versus the path-breaking formulae of Euler's divergent series or Ramanujan's number-theoretical insights. These wars over the past were tightly coded interventions aimed more at shaping the future than on chronicling the past. Would physics students learn about cross sections, Lagrangians, and particle lifetimes? Or would they train in Calabi-Yau manifolds, knot theory, and topological invariants? Would mathematicians learn to think in terms of physics categories like vertex operators, conformal field theories, and Feynman integrals? Or would they guard the proof structure of Bourbakian morality, "family values," and disciplinary traditions? These prophesies and evaluations were not "outside" the creolized physicomathematics of strings—they helped, in no small measure, to constitute it. Through shared institutions and intuitions, mathematicians and physicists are constructing a conjoint field of inquiry, whether one calls this trading zone "quantum geometry" or situates it within string theory or algebraic geometry. Joint appointments, common conferences, research collaborations, and training methods all have created a substantive region of overlap in practices and values. From the perspective of this account, it is perhaps not surprising that by 1999 the mathematics department at Columbia began demanding that its graduate students take a course in quantum field theory. Even ten years earlier, such a requirement would have been unthinkable.

By creating such a substantial border region, more than results have shifted. As the "contrarians" rightly noted, these alterations signaled a shift or perhaps an expansion in the meaning of theoretical physics and algebraic geometry. And with these changes, what it means to be a geometer or a theoretical physicist altered as well. The next generation of mathematicians and physicists would know a different world from their elders: in addition to physical sites (such as the Institute for Advanced Study at Princeton), they would also frequent virtual places, such as the joint mathematics and physics "Calabi-Yau Home Page" that, by the end of the twentieth century were already joining activities and techniques in the border zone. Trained differently from physicists in the 1970s or 1980s, working to different standards with different tools, it became possible for a young investigator to say: "I don't know whether I am doing physics or mathematics," an utterance either unthinkable or unacceptable even a few years earlier. With the new sense of theoretical physicist and geometer came also a new object of inquiry: in its present form not quite mathematical and not quite physical, either. One day pieces of such entities may be folded back into physics or into geometry, but at century's end they were *conceptual objects*, hugely productive and yet seen with discomfort by purists in both camps.

In the late twentieth century, understanding the shifting cultures of theory was not just of abstract significance. It was a problem of urgent concern to leading physicists and mathematicians; these debates would shape central features of their disciplines into the next century. Because this dynamic so thoroughly mixed constitutive values with the disciplinary identity of the practitioner and the allowed objects of inquiry, the story of this hybridized mathematics and physics raises equally pressing concerns for historians, sociologists, and philosophers of science. Our histories and our shifting present are always reconstituting the triad: persons, values, and objects.

Notes

1 On trading zones as sites of local coordination between different scientific and technological languages, see Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

2 Antonio Zichichi (ed.), *The Superworld I* (New York: Plenum Publishing, 1990), 237.

3 Galison, *Image and Logic*, chapter 6; Galison, "Pure and Hybrid Detectors: Mark I and the Psi," in Lillian Hoddeson, Laurie Brown, Michael Riordan, and Max Dresden (eds.), *The Rise of the Standard Model* (Cambridge: Cambridge University Press, 1997), 308–337.

4 David Gross, "Superstrings and Unification," in R. Kotthaus and J. H. Kühn (eds.), *Proceedings of the XXIV International Conference on High Energy Physics: Munich, Federal Republic of Germany, August 4–10, 1988* (Berlin: Springer-Verlag, 1989), 328.

5 Zichichi, *Superworld I*, 235.

6 Ibid.

7 Ibid.

8 Philip W. Anderson, "More Is Different: Broken Symmetry and the Nature of the Hierarchical Structure of Science," *Science* 177 (1972): 393–396; reprinted in Anderson, *A Career in Theoretical Physics* (Singapore: World Scientific, 1994), 1–4. For a good discussion of various condensed matter theorists' views on unity and disunity in physics, see Jordi Cat, "The Physicists' Debates on Unification in Physics at the End of the 20th Century," *Historical Studies in the Physical and Biological Sciences* 28 (1998): 253–299.

9 P. J. Steinhardt (ed.), *1983 Fourth Workshop on Grand Unification* (Boston; Birkhäuser Boston, 1983), 3.

10 Paul Ginsparg and Sheldon Glashow, "Desperately Seeking Superstrings?" *Physics Today* 39 (1986): 7.

11 Glashow, "Closing Lecture: The Great LEP Forward." in Antonio Zichichi (ed.), *The Superworld II* (New York: Plenum Press, 1990), 540.

12 Howard Georgi, "Effective Field Theories," in Paul Davies (ed.), *The New Physics* (Cambridge: Cambridge University Press, 1989), 446.

13 Ibid., 452.

14 Michael Green and John Schwarz, *Physics Letters B* 149 (1984): 117–122.

15 Gross, J. Harvey, E. Martinec, and R. Rohm, "Heterotic String Theory," *Nuclear Physics B* 256 (1985): 253–284; P. Candelas, G. T. Horowitz, A. Strominger, and E. Witten, "Vacuum Configurations for Superstrings," *Nuclear Physics B* 258 (1985): 46–74.

16 Candelas et al., "Vacuum Configuration for Superstrings," 46–74.

17 Shing-Tung Yau interview, 20 August 1998; Strominger interview, 2 April 1999.

18 Candelas et al., "Vacuum Configurations for Superstrings," 46–74.

19 David Morrison interview.

20 A simpler, visualizable analogue to projective space can be imagined if we start with R3, real 3-dimensional space. Then RP2, the real projective 2-space, is defined by identifying (x_1, x_2, x_3) with $\lambda(x_1, x_2, x_3)$ where λ is a real number. The elements of this real projective 2-space—the lines through the origin—can be represented by the intersection of such lines with a real 2-sphere around the origin. Each such pair of points (located opposite each other on the sphere) picks out a single element of the space, namely a line. The goal in shifting attention to projective space is that now many limiting processes contain the asymptotic limit within the space rather than convergent to a point *outside* the space. That is, the space is *compact*. If x,y has a point (1,3) in it, then (2,6) is too, as is (x, 3x) in general for any real number x. So suppose we start with (1,x), a point that looks like it will head off to (1,•) for x growing indefinitely. By the rule that scaling does not change the identity of the point, we can always identify (1,x) with 1/x(1,x) = (1/x, 1). And 1/x, 1 behaves just fine as x gets really large. It is this feature of containing its limit—being compact—that makes projective space so useful to mathematicians.

21 D. Gepner, "Exactly Solvable String Compactifications on Manifolds of SU(N) Holonomy," *Physics Letters B* 199 (1987): 380–388; and "Space-Time Supersymmetry in Compactified String Theory and Superconformal Models," *Nuclear Physics B* 296 (1988): 757–778. Gepner's conjecture was later proven by B. Greene, C. Vafa, and N. P. Warner, "Calabi-Yau Manifolds and Renormalization Group Flows," *Nuclear Physics B* 324 (1989): 371–390.

22 W. Lerche, C. Vafa, and N. P. Warner, "Chiral Rings in N = 2 Superconformal Theories," *Nuclear Physics B* 324 (1989): 427–474; L. Dixon, lectures at the 1987 ICTP Summer Workshop in High Energy Physics and Cosmology; see also "Some World-sheet Properties of Superstring Compactifications, On Orbifolds and Otherwise," in G. Furlan et al. (eds.), *Superstrings, Unified Theories, and Cosmology 1987* (Singapore: World Scientific, 1988), 67–126.

23 Email message, Rolf Schimmrigk to author, 17 March 1999.

24 Candelas interview, 29 May 1998.

25 Ibid.

26 B. R. Greene and M. R. Plesser, "Duality in Calabi-Yau Moduli Space," HUTP-89/A043, printed as *Nuclear Physics* B 338 (1990): 15–37.

27 Candelas interview, 29 May 1998; also Candelas, M. Lynker, and R. Schimmrigk, "Calabi-Yau Manifolds in Weighted P_4," *Nuclear Physics* B 341 (1990): 383–402.

28 Candelas interview, 29 May 1998; also Yau interview, 20 August 1998.

29 Serge Lang, *Introduction to Algebraic Topology* (Reading, Mass.: Addison-Wesley, 1972), v.

30 Both citations from Herbert Clemens interview, 31 October 1998.

31 H. Clemens, *Some Results on Abel-Jacobi Mappings*, in *Topics in Transcendental Algebraic Geometry* (Princeton, N.J.: Princeton University Press, 1984).

32 Sheldon Katz interview, 30 October 1998.

33 See, e.g., Phillip Griffiths and Joseph Harris, *Principles of Algebraic Geometry* (New York: John Wiley, 1978, 1994), 480–489; Steven L. Kleiman, "Problem 15. Rigorous Foundation of Schubert's Enumerative Calculus," in *Proceedings of the Symposium of Pure Mathematics* 28 (1976), part II, 445–482. Cf. Hermann Schubert, *Kalkuel der Abzaehlenden Geometrie* (Leipzig: B. G. Teubner, 1879).

34 S. Katz, "On the Finiteness of Rational Curves on Quintic Threefolds," *Composition Mathematics* 60 (1986): 151–162; For n_1 see J. Harris, "Galois Groups of Enumerative Problems," *Duke Mathematical Journal* 46 (1979): 685–724.

35 Sheldon Katz interview, 30 October 1998.

36 P. Candelas, X. C. de la Ossa, P. S. Green, and L. Parkes, "An Exactly Soluble Superconformal Theory from a Mirror Pair of Calabi-Yau Manifolds," *Physics Letters* B 258 (1991): 118–126, hereafter cited as COGP-1; for the first ten n_k, see ibid., "A Pair of Calabi-Yau Manifolds as an Exactly Soluble Superconformal Theory," in Shing-Tung Yau (ed.), *Essays on Mirror Manifolds* (Hong Kong: International Press, 1992), 31–92.

37 The degree of a map is different from the degree of a curve. Because of this, extracting the number of fixed-degree curves from string theory computations is quite difficult. For example, the multiple winding of string world-sheets contributes multiple covers of lower degree curves—and these count in the enumeration of higher-degree maps.

38 Why is the complex structure manifold not quantum corrected? In string theory there is only one free parameter, α', the string tension. So a string expansion on the complex side would necessarily involve α'/R^2, the R^2 entering to make the expansion parameter dimensionless. But R is a parameter that changes the size of the Calabi-Yau manifold, and supersymmetry guarantees that the parameters that change size and the parameters that change shape will not mix—so R cannot occur on the shape-changing side of the equation. Therefore, no string corrections exist for the shape-changing (complex structure) side of the mirror pair.

39 Stein Arild Strømme, email message to Sheldon Katz, 6 June 1990.

40 Yau, "Introduction," *Essays on Mirror Manifolds*, iv.

41 B. R. Greene and M. R. Plesser, "An Introduction to Mirror Manifolds," in Yau, *Essays on Mirror Manifolds*, 2–3.

42 P. S. Aspinwall and C. A. Luetken, "A New Geometry from Superstring Theory," in Yau, *Essays on Mirror Manifolds*, 318.

43 Candelas interview, 29 May 1998.

44 Ibid.

45 David R. Morrison, "Mirror Symmetry and Rational Curves on Quintic Threefolds: A Guide for Mathematicians," DUK-M-91-01; July 1991, alg-geom/9202004 10 Feb 92. Note that this citation can also be found in Xenia de la Ossa, "Quantum Calabi-Yau Manifolds and Mirror Symmetry" (Ph.D. diss., University of Texas, Austin, 1990), 2.

46 Morrison, "Mirror Symmetry," 13.

47 Ibid.

48 Ibid., 18.

49 Ibid., 17.

50 Geir Ellingsrud, email message to Joe Harris, n.d. [probably 31 July 1991].

51 Email message, Harris to Ellingsrud, 1 August 1991, 18:07 PDT.

52 B. Greene and S.-T. Yau (eds). *Mirror Symmetry II* (Providence, R.I.: American Mathematical Society, International Press, 1997).

53 Morrison interview, 4 November 1998.

54 See, e.g., B. Greene and H. Ooguri, "Geometry and Quantum Field Theory," in Greene and Yau, *Mirror Symmetry II*, 26.

55 Some relevant papers by Witten include "Quantum Field Theory and the Jones Polynomial," *Communications in Mathematical Physics* 121 (1989): 351–399; "Fional Gauge Theories Revisited," *Journal of Geometry and Physics* 9 (1992): 303–368; "Supersymmetry and Morse Theory," *Journal of Differential Geometry* 17 (1982): 661–92.

56 E. Witten and P. Deligne, "Mathematical Sciences: An Integrated Program in Mathematics and Theoretical Physics," NSF-DMS 9505939, submitted 18 November 1994.

57 Ibid.

58 Referee reports for Witten and Deligne, "Mathematical Sciences."

59 Ibid.

60 E. Witten and P. Deligne, "An Interdisciplinary Program in Mathematics and Theoretical Physics," NSF DMS 96-27351.

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