The art of mixing with an admixture of art: Fluids, solids, and visual imagination[a)]

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(Received 12 January 2010; accepted 13 January 2010; published online 25 February 2010)

Although creativity is often equated with art, it is just as present in science and technology. The Otto Laporte Lecture provides a forum to cover some of this ground—an opportunity to offer observations about scientific imagination, the role of collaborators and environment, creative processes in general, and even how science evolves. Very little has been written about these topics in the context of fluid dynamics, but I believe that such a viewpoint—understanding why and how scientific discoveries are made—provides significant insights which go far beyond my specific work. © *2010 American Institute of Physics*. [doi:10.1063/1.3323087]

I. PRELIMINARIES: OBJECTIVE AND POINT OF VIEW

It is a wonderful honor to be recognized by your peers. I would like to thank the committee for awarding me the Fluid Mechanics Prize for the year 2008. I would like to thank those who nominated me for this award and also express my deep gratitude to my collaborators and students over the years; thanks to them all. I greatly appreciate being selected.

There is a significant benefit associated with this award and it is that the accompanying Otto Laporte Lecture gives the recipient of the Fluid Mechanics Prize a wonderful platform to address peers across topics. So let me start by stating my objective.

The implicit expectation in these kinds of talks, and the requisite written text that follows, is to explain *what* one has done, in my case, according to the citation, to go over "*chaotic mixing and mixing and segregation in granular flow*." The temptation is therefore to summarize a career or, at least, a big part of it. I want to do something else. I want to go into elements of *why and how* rather than explaining simply the *what*. That is, I will focus briefly on why I did what I did and what were the surrounding ideas that influenced me, rather than simply presenting the final results. This will allow me to make general observations about scientific imagination, the role of collaborators and environment, creative processes in general, and even about how science evolves. I believe that these are issues of interest to people in the Division of Fluid Dynamics and to the readers of *Physics of Fluids*, but are topics which one has rarely the opportunity to discuss in this forum. Moreover, and this is the main motivation for my point of view, I believe that a broadly construed *why and how perspective* holds significant lessons, lessons which go far beyond my specific work.

Three points first. The first is that I will dwell on history. But I will not attempt to satisfy the standards of rigorous historical research. There are a few books focusing on the history of fluid dynamics, notable among them is the recent work of Darrigol.[1] Nor will I get into material connected with the structure of scientific revolutions.[2] My viewpoint is of someone looking out from the inside, that of the practitioner, someone involved in the craft; if there is advice it is to individuals also working on the craft, on how to move ideas forward, on how to recognize hurdles, and how to view opportunities. I recognize that this may be a feeble defense. David Hockney, an accomplished contemporary painter, was taken to task by art historians by suggesting that old masters used a *camera obscura*, notwithstanding his argument that being an actual painter should give some credibility to his arguments. The second point is that the accent of this paper is on words whereas the accent of the actual talk, given in San Antonio, was on figures. The arrow of time in a talk goes in one direction only; putting the accent in words on a printed page allows for a deeper scrutiny of the arguments, a back and forth if you will. The third point is that I am aware also that pursuing the "why and how" avenue brings me into dangerous territory. I know the risks of being an explainer. The English mathematician Hardy put it perhaps a bit too strongly: "There is no scorn more profound, or on the whole more justifiable, than that of the men who make for the men who explain. Exposition, criticism, appreciation, is work for second-rate minds."[3] I will take the risk. My argument is that being aware of the arc of history and seeing that the "arrow of progress" is far from the tidy summary presented in textbooks has tangible benefits. Studying, contemplating, and even admiring how an idea gets put together does not reduce creativity; it sharpens creativity (Fig. 1).

This is the plan: I start by making some general observations about creativity in science and art, how are they commonly viewed, including what I see as misconceptions, the uniqueness of their products, the flow and evolution of creative processes, and the ability or inability to retrace steps leading to a creative output. I then get into the issue of how creative ideas are accepted as part of a domain and the interaction between the domain and the practitioners of the domain, that is, the field. All this forms a rather lengthy intro to my own work, where I get into issues having to do with fluid

[a)]This paper is based on the Otto Laporte Award Lecture, presented by the author at the 61st Annual Meeting of the Division of Fluid Dynamics of the American Physical Society, held 23-25 November 2008 in San Antonio, TX. Dedicated to the memory of Professor William Ranz.

FIG. 1. (Color online) Hardy (left) who despised explainers and Immanuel Kant, who offered explanations (right).

dynamics, Lagrangian views, mixing and dynamical systems, my search for visual inspiration, the value given to pictures through time, and, finally, a few aspects of spatial imagination. I conclude by focusing on granular mixing which allows me to get into issues of context and the importance of fitting with the canonical knowledge of the times. The closing section distills some of the ideas into lessons which may be of value to those who I believe form the bulk of the readership of *Physics of Fluids*, those involved in the creation of ideas in the domain of fluid dynamics.

II. CREATIVITY IN SCIENCE AND ART: UNIQUENESS, FLOW OF IDEAS, AND MISCONCEPTIONS

How do people, especially those that defined a discipline, pick problems to work on? It would have been wonderful to peek into the brain processes of someone like G. I. Taylor. How did he come up with ideas? Unfortunately we have little record of that. There is little tradition of this type of activity in fluid mechanics. However, several notable people have thought about creativity—philosophers and, notably, a few distinguished mathematicians.[4] Some of these accounts involve Euler, Lagrange, and other people who were at the center of the birth of fluid dynamics as a discipline.

There is creativity in art, science, and technology. But let us start at the beginning, with what is a popular view: That artistic creativity is at the top of all creative acts. This may be traced back, though not uniquely, in large part to the philosopher Immanuel Kant (1724–1804) and his conception of genius. According to him, genius exists in the arts but not in the sciences—while scientists can teach others their work, artists produce original works, the secret of the creation of which is unknown. "Newton could show how he took every one of the steps he had to take to get from the first elements of geometry to his great and profound discoveries," Kant wrote, "not only to himself but to everyone else as well, in an intuitively clear way, allowing others to follow." This is not the case with Homer and other great poets, he added. "One cannot learn to write inspired poetry… however superb its models."[5]

This argument has been articulated many times since. The science historian Cohen, in his book *Franklin and Newton*,[6] cites a remark attributed to Einstein: "…even had Newton or Leibnitz never lived, the world would have had the calculus, but that if Beethoven had not lived, we would never have had the C-Minor Symphony." The argument is that even though Leibniz and Newton did not work together they occupied a common intellectual milieu. "All the basic work was done—someone just needed to take the next step and put it together," Jason Bardi writes in *The Calculus Wars*, a history on the development of calculus.[7] "If Newton and Leibniz had not discovered it, someone else would have."

Do Kant and his recent followers have a point? We face in fact two classical, time-ingrained arguments; first, the magic associated with the inability of retracing steps in truly creative areas is, in fact, something that becomes a definition of creative output according to this group, and, second, is the inevitability angle. But significant exceptions are easy to find; here I am taking the point of view of the practitioner, someone who has seen things from the inside. Anybody involved in a creative activity knows that the evolution of ideas is messy.

A. Retracing steps

Consider the argument of retracing steps, reversing the flow of ideas. Are all the steps in Newton's arguments actually ordered in a perfectly logical sequence? Let us take an extreme position, steps in the most logically based all of disciplines, mathematics. A mathematical proof has to be flawless; step n has to be logically derived from the $n-1$ previous steps. From this viewpoint one can follow the logical argument of a proof. But this hardly means that the *idea* behind the proof can be traced, or that the origins of the theorem can be traced back at all. One has to guess a theorem before proving it. Mathematics is inductive, not deductive. Once built, the nth theorem follows logically from a subset of the $n-1$ previous theorems. But this does not mean that logic itself governs the growth of mathematics. That is why we have conjectures, statements that cannot be proved or disproved, like Fermat's last theorem—conjectured by Pierre de Fermat in 1637 but not proven fully until 1995 by Andrew Wiles. One guesses a theorem—this has nothing to do with logic and all to do with informed intuition—and then one goes on to prove the theorem, or not. This is where the logic goes, but the logic is only seen *post facto*. Most guesses go nowhere.

Polya said it best: "…we secure our mathematical knowledge by demonstrative reasoning (the logical part) but we support our conjectures by plausible reasoning."[8,9] The proof rests on logic, but the insight that leads to the "magical" QED is based on intuition.

B. Inevitability

And then there is the larger question of whether math is invented or discovered. "My attitude towards mathematics is that most of it is lying out there, sometimes in hidden places, like gems encased in a rock. You do not see them on the surface, but you sense that they must be there and you try to imagine where they are hidden. Suddenly, they gleam brightly in your face and you do not know how you stumbled upon them. Maybe they always were in plain view, and we all are blind from time to time," Enrico Bombieri reputedly said.

Bombieri's view hits on the second argument, the one about inevitability. Some people have gone far in this direction, this being the "*in the air* theories" argument.[10] If person "X" had not discovered "D," person "Y" would surely have arrived at a similar result, maybe not exactly D, but something close—a pedestrian version of the lofty Platonic quote by Enrico Bombieri above. Michelangelo puts it more succinctly: "The marble not yet carved can hold the form of every thought the greatest artist has."

The important point here is that there is more similarity between art and science than people think. One can make the argument that creativity in science ranks with art. I would go even further and argue that mathematical creativity probably ranks at the very top along with the most perfect works of art produced by humanity.

Let me elaborate on the evolution of ideas and the retracing steps argument. Retrospective analyses show that artistic masterpieces do not suddenly appear from thin air—the final painting can be deceptive when presented without context. Paradoxically, even though bursts of inspiration are associated with visual art, visual arts is the domain where the evolution of an idea—all the painstaking process that goes behind what may appear as flashes of genius—is the most transparently documented. In the visual arts, the entire creative process matters. For example, Picasso did 43 sketches for Guernica (and, remarkably, all survive). The documentation of the creative process in visual arts is meticulous, examples of which are retrospectives and books of art history. Creative processes in science involve as many steps. However, we do not routinely see this level of documentation in theoretical developments in science or in mathematics.

C. Evolution

An excellent example of evolution in visual art is provided by etchings. Unlike an oil painting or a watercolor, there is no retracing of steps in etching. There is no retracing in marble sculpting either, but unlike marble sculpting etching leaves a recordable irreversible timeline, with intermediate stopovers, proofs, leading to what may be the "final" result. In fact the last result may have gone too far and the best and "final" result may be one of the previous steps. Rembrandt provides some of the best examples in this area, as for example, in the etchings "Christ crucified between the two thieves." Some artists have purposefully explored this idea. Picasso did a series of 11 lithographs in a time span of two weeks at the end of 1945, going from a realistic-looking bull to one showing only its essential elements, captured by just a few lines.[11] There is always this evolution in painting. But sometimes it is hidden from view.

The second point is the uniqueness of an idea. "Genius" is associated in the popular mind with the initial burst, the germ of an idea often believing that the idea emerges fully formed. Few people in science buy into this idea; few people in art buy into this idea as well. It is probably safe to say that artists do not understand how scientists think and *vice versa*.

FIG. 2. (Color) Primo pensieri. First ideas were highly prized in the Renaissance. Jacopo da Pontormo (1494–1557) Two Studies of Male Figures, Collection of The Uffizi Gallery, Florence Italy.

There is a historical underpinning associated with equating creative burst and genius, and this comes largely from art. In the Renaissance, the first idea, the initial sketch, the quick few lines outlining a visual composition, the *primo pensieri*, were what were most highly prized (Fig. 2).[12] These sketches, sufficiently evolved, served as the template for say, an altarpiece, and even had legal force. It was the Renaissance version of copyrighting an idea.

This has a parallel in science also. "Imagination is more important than knowledge," Einstein said. One can argue that a lot of the creativity in science resides in picking problems and that the cleaning up afterward rests mostly on technique. There are always more people who are good at technique than people who have the great initial ideas. Or, put in another way, posing the question is as important as, or even more important than, the answer.

More often than not a "new idea" is a combination of several existing ideas. This is a common occurrence in science. In fact, it is how science works. But a visual arts analogy might help and a work by Picasso serves as an apt example. "Baboon and Young," a bronze sculpture by Picasso, consists of an assembly of manufactured objects. The head of the baboon consists of two toy cars, the ears were ceramic pitcher handles, and the fat belly was a large pot.[13] The central point is that objects, once combined, cannot be seen independent of the whole. A theory may be the combination of several ideas generating a new whole.

The central point of the above discussion is that knowing about context helps. We normally are taught sanitized narratives of creative outputs, a distilled and cleaned-up view leading smoothly to an end point. Knowing how something was put together helps in our appreciation and understanding and, maybe, lifts and challenges our creative spirits. The reality is that trajectories leading to what is seen as a creative end point are messy. I believe, and this is the central point of my remarks, that peering into the creative process, knowing some of the history behind creation and discoveries, may increase creativity rather than diminishing it. What we normally get, and what students get as a result of standard education, are distilled views: perfect buildings with no hints of the scaffold.

Scientists, philosophers, and mathematicians commented on this, what I call the "deception of final pictures." Ludwig Wittgenstein's metaphor of "kicking the ladder after climbing up" is an apt one.[14] Many of the highest exponents of creative thought leave no trace of the ladder. Karl Friedrich Gauss (1777–1855) and Bernhard Riemann (1826–1866) left no trace of the scaffold that lead to their final results. "I did not succeed in compacting the proof as to make it publishable," Riemann said, and simply stated four properties of the "Riemann" function. It took Jacques Hadamard (1865–1963) 30 years to prove the first three conjectures.

Final results look solidly logical but hide what may have been a myriad of tentative steps. Hermann von Helmholtz, in a well remembered talk, described the two roads to research: (1) *the shaky ladder* every researcher has to climb and (2) *the smooth royal path* on which the results are presented to an audience.[15] "The secret of creativity is knowing how to conceal your sources" has been said. Helmholtz's shaky ladder has bifurcation points, roads not taken and dead ends, which make Kant and Cohen's inevitability issue a rather shaky proposition.

III. HOW CREATIVITY OCCURS

Creative ideas do not happen in a vacuum. Creativity is not a private enterprise but involves the interaction of three things: (i) a person or a group, (ii) a domain, say, any of the branches of mathematics or fluid dynamics, and (iii) the "field," the set of people who are the gatekeepers to the domain.[16] This is so because in order for an idea to be judged as creative it has to affect a domain and become a permanent part of that domain. Altering the content of a domain in transformative ways is very difficult. People see things through the lens of their times. In order to accept something new we may have to expand or, more crucially, alter our knowledge base. This is not easy.

The character of a domain limits what people within it claim they can do. In some instances, for example mathematics or physics, the domain trumps the field. We cannot violate the second law of thermodynamics, for example; in mathematics a theorem is either proved or not. But even within these tightly organized domains there is variability.

At the other end of the spectrum, in visual arts, for example, the field trumps the domain. Gatekeepers "collectively decide"—this has rings of emergence in complex systems—what works of art are worthy of inclusion in the history or art; but judgments are not final. There are ups and downs in artistic reputations as a function of time. When we say that van Gogh was unrecognized in his lifetime or that William-Adolphe Bouguereau was over-rated we are in essence over-riding the judgment of the field at the time. In general the rules of acceptance by a field are clearest in math and in physical sciences. There are ways to check and verify results; the biggest discovery of science is science itself, it has been said. There is a methodology for moving forward. But even in these fields, things are not always that clean cut. Style matters. There may be two different styles of doing something and this is true even in mathematics. As an example consider the titanic battle at the 1900 Paris International Congress of Mathematicians between Poincaré and Hilbert about the roles of intuition and proof in mathematics. For example, pictures or no pictures?

IV. FLUID DYNAMICS, PICTURES, AND DYNAMICAL SYSTEMS

All of us are working in fluid dynamics due to some choice. Why did I gravitate to fluids? Part of this was documented elsewhere,[17,18] but the main attraction was the purity of the concepts and the aesthetics. Fluid dynamics, which I learned in the context of transport phenomena, was for me like an island of stability in what was a sea of chaos in Argentina in the 1970s. On this my views may have been influenced by the fact that I was exposed early to both science and art: my father did biology and my mother was trained as an artist. I grew up in an academic environment. I was fascinated by books, especially old books. But I had never seen the collected works of Maxwell, Rayleigh, Euler, Reynolds, or Taylor until I got to the U.S. Nothing beats the experience of holding a volume of collected works, seeing the actual expanse in the shelves, an entire life trajectory, what a first-rate mind was capable of.

I got into fluid mixing in my Ph.D. thesis. My initial inclination was to do something practical; to understand reactive mixing and investigate how the products of chemical reactions are affected by flow.[19] I started by looking at reactive mixing and rather quickly I was sketching pictures of lamellar structures and one-dimensional (1D) diffusion and reaction. I realized that mixing was crying for a geometrical viewpoint and tried to find the right pictures to guide my thinking and possible experiments. After a year I had abandoned practicality and specificity. A lamellar model and stretching became the central point and a Lagrangian viewpoint became the central part of the picture.[20] The Lagrangian picture and stretching became part of something which we called warped time.[21] For infinitely fast reactions all the fluid mechanical history could be incorporated in this time. Explaining the thinning of striations was easy, but how had the striations been created in the first place? It seems obvious now but if one wants to know how something gets mixed, it makes sense to try to follow what happens to a blob injected in a fluid as function of time. This viewpoint was hardly popular. And there were dead ends. Now with the hindsight of time, I can see how a good paper by a great person can even preclude progress. I spent a lot of time reading and interpreting a paper by Danckwerts;[22] a good paper, but one whose ideas could not be extended.

A. Lagrangian dreams

A Lagrangian viewpoint seemed ideal, following material bodies, watching where things go. Part of the mathematical machinery was in continuum mechanics. I became fascinated by the timeliness of kinematics and the beauty of its results. It helped that I was taking a course in fluid mechanics with James Serrin and continuum mechanics with Roger Fosdick. In looking back it is remarkable the number of people who can shape our thoughts in a brief period of time. (They are so many that listing them here would disrupt the flow. I list them in the acknowledgment of the paper.) The Lagrangian viewpoint became an obsession, and I looked for pictures connected to mixing to guide my thoughts.

I learned at that point that, what came to be known as a Lagrangian viewpoint, material coordinates and the like had been shaped by a large number of people (Truesdell's *The Kinematics of Vorticity*[23] became a sort of a historical guide). We tend to believe that things are older than they are. What we now call π was not baptized as pi until the 1800s; it was called "c" before and many other names. Learning about where things came from became part of my education.

B. In search of visual inspiration

I started looking for pictures to inspire me and to my surprise I found remarkably little. *The Classical Field Theories* by Truesdell and Toupin[24] runs for 567 pages and has 47 figures; Truesdell's *The Kinematics of Vorticity*,[23] a monograph with awe-inspiring historical footnotes, runs for 232 pages and has only five figures. It seemed as if people had been guided by Lagrange, and probably they were: "The reader will find no figures in this work. The methods which I set forth do not require either constructions or geometrical or mechanical reasonings: but only algebraic operations, subject to a regular and uniform rule of procedure," Lagrange stated in his preface to *Mécanique Analytique*.[25] This did not seem to mesh with another often-cited Lagrange quote: "As long as algebra and geometry have been separated, their progress has been slow and their uses limited; but when these two sciences have been united, they have lent each mutual forces, and have marched together towards perfection."

Be that as it may, in the historical journey I encountered the names of d'Alembert, Euler, Rankine, Maxwell, and Hadamard. Many of them had dabbled in kinematics. However, in the texts of the times, from Horace Lamb's *Hydrodynamics* onwards, there was little on the Lagrangian viewpoint, streaklines, pathlines, and so on. There were a few lines in Batchelor,[26] almost nothing in Lamb,[27] and even less in Landau and Lifschitz[28] and Milne-Thompson.[29] A curious exception was Prandtl and Tietjens[30] and a gem of a book by Rutherford Aris.[31] But possibly the best guidance I got from the unpublished notes of Stanley Corrsin's fluid dynamics at Johns Hopkins (Lecture Notes on Introductory Fluid Mechanics, Baltimore, 1966). I looked for guidance in turbulence but little was there as well. Notwithstanding isolated attempts the prevalent picture of turbulence during the 1950s, 1960s, and 1970s, was one, as aptly described in a quote attributed to Theodrore Theodorsen, of "a perfectly random motion of particles [where] no basic pattern should or could exist." How could one imagine pictures where "no basic pattern should or could exist"? That is why I could not find pictures of flows. There were very few pictures in fluid mechanics in *Proceedings of the Royal Society* and in *Philosophical Transactions of the Royal Society* prior to 1970. Meanwhile I was looking for inspiration in contemporary papers and along the way I found a few, including an older paper by Welander.[32] The absence of pictures in papers is somewhat paradoxical. The Fluid Mechanics Films project, 1962–1969, is remarkable for the use of visual arguments. There are for example two films that I watched over and over, both by John Lumley–one on Eulerian and Lagrangian views, the other on deformation. At the same time Lumley wrote a book on Turbulence (with Tennekes) with few figures and without a single experimental photograph. A turning point, in the mid 1970s, was Anatol Roshko's work on coherent structures.

While this was going on, I received a crucial phone call. At the end of 1982, well before he published his 1984 pioneering paper in *Journal of Fluid Mechanics*, I was contacted by Hassan Aref who shared his result on of the blinking vortex.[33] The visit to Brown was eye opening.

Almost at the same time, while at UMass Amherst, I "discovered" via Hawley Rising the Smale horseshoe map.[34] This was the "aha" moment for me. I was surprised to see that it was almost contemporary math. And I quickly imagined that I could have a fluid do this. With this came a realization that dynamical systems provided the right framework for this problem and set out to find examples where math, as opposed to just outright computation, was possible. Thus, there were two ways to see mixing: one as perturbation from an integrable case, breaking homoclinic or heteroclinic orbits, a view at that point grounded on Hamiltonian chaos, and another using intuition and trying to construct horseshoe maps. I set up to increase my knowledge of dynamical systems. Guckenheimer and Holmes[35] and Lichtenberg and Lieberman[36] became the indispensable guides. I devoted time looking at Arnold's papers. In the end Hamiltonian chaos was less important than I thought, and I ended up pursuing the horseshoe route.

We set out to calculate analytically bifurcations and to play with geometrical constructions leading to the formation of horseshoe maps.[37] In parallel we set up experiments.[38,39] We realized that successfully persuading the fluid dynamics community that low Reynolds number flows could produce chaos was far from trivial. (Everybody seemed to know of Taylor and his mixing-unmixing experiment in a Couette flow. This was not helpful however; unmixing in a Couette flow is the exception to the rule insofar as mixing is concerned.) Only one other group, at Columbia, embarked on experiments.[40] We realized that sustained effort was needed for the ideas to catch on and that this would require a marriage of theory, analysis, and experiments.

Papers were written and one ended up on the cover of *Nature*. But by then the idea of a book had taken shape and, in what was a lucky accident, I found myself at Caltech, ready to write *The Kinematics of Mixing*.[41] Caltech was the right place to be. There I connected with the work of Anatol Roshko, Paul Dimotakis, and, during one of his visits, Katepalli Sreenivasan, and became familiar with the work of Brian Cantwell, who, in many regards, had come close to some of the ideas that had to do with chaos.[42] And in what was a stroke of luck I developed a long-term and fruitful collaboration with Steve Wiggins who was just lecturing on what would become one of his first books. Howard Stone was finishing his Ph.D. I realized that if the ideas were to catch on I needed to reach the level of the top work in fluid mechanics–the bar set by Andy Acrivos, Gary Leal, John Brady (then just arrived at Caltech), and others. It was also

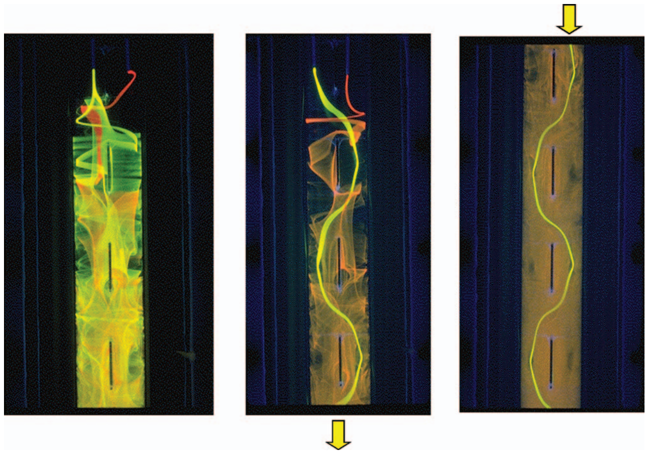

FIG. 3. (Color) Coexistence of chaos and regular behavior in duct flow. Kusch and Ottino, Experiments on mixing in continuous chaotic flows, J. Fluid Mech., **236**, 319–348 (1992). Reproduced with permission from the Journal of Fluid Mechanics, Cambridge University Press.

important to spread the ideas to other fields, such as geophysics. Caltech provided me with a footprint in this area as well. As the book was in press, another paper, giving a bit of the history up to that point, ended up on the cover of *Scientific American*.[43] The ideas took off from there.

In retrospect, the ideas were an augmentation of the kinematics of flows. Who could have predicted that there was so much hidden in composing simple motions? Retrospectively, it is easy to assemble the pieces and then put them in the right order. But along the way many things that now seem inconsequential preoccupied us. For example, can a streakline cross itself?

Once the initial building blocks are in place the rest is (almost) cleaning up loose ends. This may sound extreme but work is driven by questions posed by different areas and different viewpoints giving rise to entire branches. What are the limits of computations?[44] "The examples are too idealized," we were told. Can one add more realism, get rid of corners and singularities, for example? Does the shape of the forcing matter? Do discontinuities matter? How can one quantify mixing?[45] Can one imagine examples of continuous flows that will move us closer to applications? (see Fig. 3). Are there 3D examples?[46] How can one describe agreement between computations and experiments?[47] What is the nature of errors, including round-off discretization errors in the computation of the velocity field? What is the role of inertia? What is the role of rheology? And then there were questions about uses of the theory. Are there practical consequences?[48] Can new devices be invented? And finally, when mixing by itself may not be important but its consequences are, how does mixing aid other processes, i.e., how are chemical reactions affected by mixing?[49] How can mixing be used to aggregate—or the opposite, how can chaotic mixing can be used to unmix or disperse?[50] This leads to coagulation and cluster formation on one side and the breakup of droplets and other microstructures on the other. And along the way, experimental results help build the arguments. By the time I spent a year in Stanford in 1991 the ideas had penetrated the community, though by no means they had become mainstream. I was lucky to connect with Parviz Moin and the Center for Turbulence Research and to learn about the frontiers of turbulence and managed to join forces with Charles Menevau in connecting multifractals and mixing. Eventually the work affected other fields including earth sciences and oceanography. In retrospect we made some logistical mistakes in placing a few papers, including one originated in Stanford,[51] that eventually reached oceanography though in a convoluted path.[52] One thing I did not foresee, an area that would become one of the biggest consumers of the chaotic mixing ideas: the microfluidics revolution.[53]

There is no question that the path could have been significantly more efficient. It would have been nice to have had more of the math needed in place, perfectly ordered at $t=0$. This was not the case. I had come across the Kolmogorov–Arnold–Moser theorem, ca. 1954, and the Smale Horseshoe, ca. 1967, in the early 1980s. I learned about symmetries in the mid-1980s and applied them by the early 1990s.[54] But I missed older things that could have been valuable much earlier. I became aware of the Brouwer theorem only after I wrote my book, and learned the theory of Linked Twist Maps,[55–57] via Stephen Wiggins, much later. This we thought was important enough that we should put it in a book in conjunction with Rob Sturman.[58] Linked Twist Maps (LTMs) constitute one class of systems where mathematical prediction of chaos on a set of full measure, i.e., positive area, is possible.[59] Horseshoes are measure zero, and the effect of this measure zero set on a full neighborhood of trajectories has to be determined with the help of numerics. LTMs are much more powerful. Moreover, they are the first practical application of ergodic theory methods in fluid mixing in a way where one can verify strong chaotic properties ahead of time without simulating massive amounts of trajectories.

V. THE "IN THE AIR" HYPOTHESIS

This is an appropriate point to revisit the claim that discoveries in science do not have the uniqueness of those in art. True, it is sometimes hard to attribute scientific ideas to a sole individual—the list of people who contribute to a scientific idea may be surprisingly long. My own example, above, includes at least two dozen people, maybe more, who in one way or another shaped my thoughts, some in major ways, and some in small but still surprisingly critical ways. This confluence of ideas leads often to wrong attributions. As mentioned earlier, if one looks at the historical record, the "Lagrangian description" is not due to Lagrange (Truesdell).[23,24] In a *tour de force* historical footnote, Truesdell shows the collection of names involved in this attribution, and it is a surprisingly long and distinguished list of mathematicians, including Dirichlet (who set the path, wrongly, it turns out, for Lagrange being attributed the idea). At other times, an idea is so prevalent that it cannot be attributed to an individual. Thus for example, I have never been able to trace the history of "the baker's transformation"—who actually used it for the first time? Or the newfound emphasis on Lagrangian coherent structures, an idea that has been around since at least the early 1980s if not earlier. Historical investigations show that discoveries often happen almost simultaneously, something that goes by the name of *simultaneous discoveries*. The first study of this hypothesis seems to have been done in 1922 by William Ogburn and Dorothy Thomas.[60] There are many examples that fit the multiple pattern hypothesis. For example, the law (or principle) of conservation of energy was put forward, almost simultaneously in 1847 by Joule, Thomson, Colding, and Helmholtz. And all four seem to have been anticipated by Robert Mayer in 1842. This led to a revision of the romantic notion of the lonely scientific genius, not as a unique source of insight but more as an efficient channel of insight. Examples abound. A Columbia sociologist, Robert K. Merton,[61] and collaborators, examined 400 of Lord Kelvin's 661 scientific communications and addresses and found that at least 32 qualified as multiple discoveries. The codiscoverers were an illustrious set; they included Stokes, Green, Helmholtz, Cavendish, Clausius, Poincaré, Rayleigh, all names associated with significant scientific achievement in their own right. But the list also included distinguished scientists such as Hankel, Varley, Pfaff, and Lamé, arguably a notch below on the prestige scale. But this does not diminish Kelvin's greatness. It indicates that it required a considerable number of others of top scientists just to duplicate only a subset of the discoveries that Kelvin made.

But the argument has been taken farther. Popular author Malcolm Galdwell in an article in *The New Yorker*,[10] argued that if one puts people like Hankel, Pfaff, Varley, and Lamé in a room together one could get a large subset of Kelvin's discoveries, without ever having Kelvin in the room. The point of the argument is that there are plenty of people like Hankel, Pfaff, Varley, and Lamé but there are very few Kelvins. Gladwell also argued that this does not apply to artistic geniuses. "You can't pool the talents of a dozen Salieris and get Mozart's Requiem. You can't put together a committee of really talented art students and get Matisse's '*La Danse*'." "A work of artistic genius is singular," Gladwell said. There is undoubtedly some truth to this (though Mozart's Requiem example is unfortunate since Mozart died before completing it and it was finished by Franz Süssmayr).

Science often involves logical connections. But sometimes there are leaps, things that are or look nearly discontinuous. Newton, Galois, Gibbs, and Einstein come to mind. There is also scientific style. And style, the way an argument or theory is put together in science, is crucial to the idea's acceptance. This aesthetic appeal is as important in science as it is in art. The flip side is that it is crucial to recognize that art does not live in a vacuum. This is clear in science but not often clear in art. Context matters. There are very few unattributed famous paintings in the world from the 1500s onwards and virtually none by an artist who produced a single masterpiece in his or her life. Context and style matter in both science and art. Finally the "in the air argument" brings up a "critical mass" issue. How many people are needed before an argument has critical backing? Here I could look at my own work. Could someone have done what I did say 50 or even 100 years ago? The answer is yes. Could the idea have been able to connect to the canonical knowledge of the times? Maybe not.

VI. CHAOS AND MIXING: PRECEDENTS

What would have happened if someone had put a drop of ink in a driven fluid and had connected with someone more mathematically minded? The drop of ink/mixing analogy was used by Willard Gibbs in discussing the thermodynamical arrow of time.[62] Gibbs was a contemporary of Reynolds and Poincaré. I do not know if Gibbs knew of Reynolds. He must have known of Poincaré though. We know he spent a year each at Paris, Berlin, and Heidelberg, and that he was influenced by Kirchhoff and Helmholtz.

In 1894 Osborne Reynolds gave a lecture at the Royal Institution in London on what he called "the method of colored bands."[63] In this lecture Reynolds put forward the central idea of mixing as stretching and folding. "In fluids [...] this attenuation is only the first step in the process of mixing—all involve the second process, that of folding, piling, or wrapping, by which the attenuated layers are brought together." This idea did not go far. Just a year later, probably unknowingly, Reynolds came up with a competing concept,[64] which marked the beginning of the statistical theory of turbulence and gave birth to eddy diffusivities and the like. It is apparent that the analysis of mixing succumbed to the structureless viewpoint. One idea took root—because the math was ready—the other did not; iteration of maps, especially two-dimensional (2D) maps, had not reached math yet. Physical ideas and math have to go hand-in-hand for a theory to flourish.

Ironically, some of the math was being developed at the same time and in his paper Reynolds unknowingly mentions the crucial discovery that gave origin to this new math. In his 1894 paper Reynolds compares unraveling the internal motion of fluids with the effort behind the discovery of Neptune, possibly the most significant achievement of its day. The connection with Poincaré and the new math comes via Neptune. (I have commented on this earlier.)[65]

Neptune's discovery was the result of unbounded faith in Newtonian mechanics and determinism. Uranus was not behaving according to Newtonian predictions and the idea of a trans-Uranian planet, one of the several hypotheses that could account for the deviations, took shape. The laborious task of discovering the trans-Uranian planet—solutions involved lengthy calculations—was completed almost simultaneously by Jean Leverrier in France and John Couch Adams in England, and produced a new planet, baptized as Neptune. This was going to be the crowning triumph of determinism, and, in some sense, it would be its last. In 1884 the editor of *Acta Mathematica*, Mittag-Leffler, in order to bring attention to his journal, persuaded King Oscar II of Sweden to fund a prize intended to encourage discoveries in higher mathematics. It was then decided that the first prize was to be awarded to the first closed-form analytical solution for the three-body problem. The discovery of Neptune notwithstanding, the solution to the three-body problem was still an open problem. The prize was won by Poincaré—the story is quite involved and only recently uncovered[66,67]—but just as the printing of Poincaré's winning paper was being completed it was pointed out that his proof contained a mistake. In the process of fixing the proof Poincaré discovered what we now call homoclinic intersections, the fingerprint of chaos, the mechanism being exactly equivalent to stretching and folding in phase space. This was precisely the same idea advocated by Reynolds to explain fluid mixing. Both men died the same year, five months apart. The obituaries for both of them appear in *Nature*. As far as I know Reynolds and Poincaré never met.

There was no contemporary link connecting the work of Reynolds and Poincaré. Someone was needed to advance Reynolds's method of colored bands and produce a convincing experiment. And the experiment needed to be interpreted in terms of the right math.

In some sense the idea had been there before, close to me, way before I came across Reynolds's paper. Bill Ranz mentioned taffy pulling as an example of mixing in the abstract of the paper that I read the most number of times of any during my thesis.[21] But I did not see how taffy pulling could be imagined as a baker's transformation. In retrospective the connection and some of the consequences are almost obvious. It could simply have been assigned as a homework problem.

VII. SPATIAL IMAGINATION

Not all problems can be mapped into the visual domain. But in some cases this is essential, especially in coming up with the first sketch of a novel idea. We know relatively little about the thinking processes of notable scientists and mathematicians. Poincaré was in fact one of the few who documented his own thinking. Another example is the French mathematician Jacques Hadamard. In his book *Psychology of Invention in the Mathematical Field*,[4] he described his own mathematical thinking and surveyed 100 of the leading mathematicians and physicists of the day—Hermann von Helmholtz, Henri Poincaré, and others—asking them how they did their work and reviewing what was known about earlier examples, e.g., Carl Friedrich Gauss (who died ten years before Hadamard was born). Another notable example is work of Gerald Holton, a historian of science at Harvard.[68] Holton, in particular, considered the roles of various types of "scientific imagination," including visual imagination. Holton exemplified visual imagination by juxtaposing the astronomical investigations of Thomas Harriot (1560–1621) (representing the no-visual imagination side) and Galileo Galilei (1564–1642) (representing the visual-imagination side). He finds examples of the visual imagination side in Albert Einstein (1879–1955) and Richard Feynman (1918–1988). To this list we may add David Hilbert (1862–1943). Jacques Hadamard, commenting on one of Hilbert's books said "diagrams appear in every other page." Werner Heisenberg (1901–1976), on the other hand, represents the opposite no-picture side ("The progress of quantum mechanics has to free itself first from all these intuitive pictures.") and, most definitely, Joseph Louis de Lagrange (1736–1813), who we quoted earlier, and Karl Weierstrass (1815–1897). "You may leaf through all his books without finding a figure," Poincaré said of him (as quoted by Hadamard). Fluid mechanics in the 1950–1970 period was on the nonvisual side, at least in the way results were presented after removing the scaffold. Pictures were moderately accepted when I started my Ph.D. Now they are everywhere. So much that I felt that they are being overused.[69]

VIII. CONTEXT: FITTING WITH THE CANONICAL KNOWLEDGE OF THE TIMES

My last example has to with connecting an idea to the canonical knowledge of the times. It is not an example in the league of Reynolds and Poincaré, but it is one that shows how an important observation may remain unconnected to large parts of science (it is interesting to note that Reynolds plays a role here as well).[70]

In 1939, a Japanese researcher, Yositisi Oyama, wrote a paper[71] that dealt with mixing of two granular materials in a rotating cylinder. The goal of his work was mixing, but Oyama noted that the materials, when looking from the end of his container, segregated in alternating bands, something that now is referred to as axial segregation. [Oyama's work is usually listed as I.P.C.R. 18, 600 (1939), "in Japanese." Attribution to this work as in Japanese in virtually all papers citing it is baffling as the paper is translated, though far from flawlessly, into English.]

Oyama's observations were not entirely new. The formation of segregated rings was a well-known problem in the cement industry. In fact, in 1904 Edison patented a device to destroy molten clinker rings that form in rotary cement kilns.[72] But this issue remained wholly in the technological domain.

Oyama's paper was eventually noticed. The paper was cited in 1959 by Weidenbaum.[73] Bridgwater in 1976 (Ref. 74) cites the paper and attributes the reference to Weidenbaum. Then there is a long lag and the next influential reference–since focused on the banding itself—is in a short note in 1991 by Das Gupta *et al.*[75]

There are many players in the granular field but their strands of thinking remained unconnected. One strand in the 1960s to 1980s was work being done by engineers who were trying to solve practical problems in what was essentially a case-by-case approach. Some work that systematically investigated phenomena such as axial segregation was done by Donald and Roseman.[76] Other efforts connected mostly with solid mechanics; fluidization was a more glamorous area and this subfield was much more developed than granular materials as a whole. And, until recently, most of the body of work alluded to above was largely undiscovered by physicists. This was to be a fertile area and in the late 1980s and early 1990s two things happened: physicists acknowledged that the physics of granular materials was largely unexplored[77,78] and engineers argued emphatically that a better understanding of granular matter would have a tremendous benefit to industry.[79] The field got quick respectability in the physics community when several high profile researchers, e.g., Pierre-Gilles de Gennes in France[80] and Sam Edwards in England became interested in the topic. It quickly became a hot area, attracting substantial attention in the physics community. It was only then that Oyama's ideas could be connected to a larger context. Axial segregation

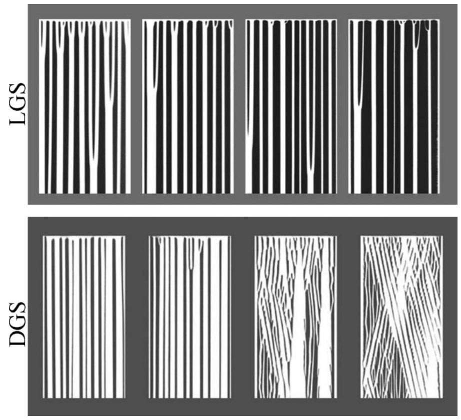

FIG. 4. Consequences of Oyama's initial ideas. Segregation and coarsening (Ref. 88). Top row, slurries, bottom row, dry systems. Speed of rotation increases from left to right; some systems coarsen; others do not and lead to complex temporal patterns. Reproduced with permission from Physical Review Letters, American Physical Society.

attracted interest in the physics community and is now a topic that has generated hundred of papers.

Our incursion in this area was largely under nearly complete ignorance of all the above. We started by looking at what we thought was the simplest example: mixing of two materials in a quasi two-dimensional container. And within this example we looked at slowly rotated containers, those involving a succession of clearly separated avalanches over a rigid bed, that is, one avalanche was finished before the next one starts. We imagined that the system could be described in pure geometrical terms: an avalanching wedge goes into another wedge, and within the wedge we imagined the mixing as being random.[81] The picture worked; it captured experimental results and we saw that the model could be easily translated to other geometries. Later on we saw results that indicated that segregation[82] may take place during avalanching. However, our view at that point was highly idealized and did not get considered (or derailed?) by what are undoubtedly many important effects. Eventually we moved on to the case where avalanches run into each other, the so-called continuous flow regime (see, for example, Ref. 83). From there on, it was a succession of "clean up" questions. Is the assumption of quasi-2D realistic? How do walls influence the results? Is the assumption of an immovable bed realistic?[84] Do material properties matter?[85] What is the role of cohesion? Later, these questions moved on to the effects of coarsening (Fig. 4),[86] time-periodic forcing, starting with a mixed system and studying it as it unmixes,[87] mixing and unmixing in 3D,[88] long cylinders, and so on, a succession of questions that even reached to the effects of gravity on the supposedly rigid bed.[89] That is how we reached the long cylinder case.

The marriage of pure mathematics and a physical picture is rare, especially when the math is an almost contemporary with the physical picture. None of the examples mentioned above required new math. The avalanche model, for instance, requires nothing more than geometrical insight and some simple programming. But the extension of ideas around 3D granular flows provides an example connecting mathematics and a physical picture. Consider a half-filled spherical container with granular material that is iteratively rotated using a two-axis mixing protocol—say successive rotations of 90° alternating from one axis to the orthogonal axis. Upon rotation, granular particles flow in a thin surface layer after reaching the angle of repose such that the flowing layer is flat and the flow is continuous. In the limit of a vanishingly thin layer the mixing mechanism is cutting and shuffling. The physical mechanism of cutting and shuffling can be put on a theoretical foundation using an emerging area of dynamical systems theory called piecewise isometries.[90,91] An isometry is a map that preserves distances (for example, a rigid rotation). Piecewise means that two (different) isometries are joined along a curve separating the domains of the two isometries. Two arbitrarily close points can become separated (cutting) when they are on different sides of the curve, and they are "shuffled" when they move under the action of different isometries. A single isometry cannot be chaotic in the sense of having exponential separation of points as the map is iterated, since the distance between points remains constant during iteration.[92] However, when two or more isometries are combined the resulting dynamics can exhibit great complexity.

IX. LESSONS

This is a suitable place to look back and distill a few lessons. They are based on my own experiences and observations, looking at other people's careers. These lessons—admonitions—should be interpreted in context, for if taken literally and in isolation, they could appear to contradict each other.

Learn something about history

History serves to illuminate the present. Science and technology changed dramatically over the last 100 years; people, however, have not. What ultimately dictates the acceptance of an idea is how a field reacts to it. Aesthetic considerations, from how a theory is put together to how compact a result is and how it illuminates other issues all play a role in the acceptance of a new idea. Knowing history also shows the tortuous paths that ideas have until, finally, when the scaffold is removed, we have what may look like effortless perfection. But this perfection is seen mostly in retrospect. Seeing how far people go into seeking support for an idea is deeply revealing. A good example is James Clerk Maxwell. When seeking support for what became the kinetic theory of gases and ultimately gave rise to statistical physics, his examples about constancies of averages were taken from the nascent science of social science, including for example, the constancy of incorrectly addressed letters (called dead letters) in the British Postal Service. A good exposition can be found in the book "Critical Mass."[93]

Start with a solid grounding

Without solid grounding we learn trivia. But having technique—unless one is an absolute virtuoso—is simply not enough. Technique aids in solving existing problems but not in uncovering new problems. In the worst cases, when technique dominates the picture, it forces viewing through a single lens. Over-reliance on technique can have a paralyzing effect. A variation on this is the Italian saying: "Impara l'arte, e mettila da parte." Learn the craft and then set it aside. That is, learn the basics, but know when to deviate from them.

Take time to reflect
Moving and doing does not imply progress. It is healthy to step back and assess the entire picture. But this does not mean that one should wait for divine inspiration to strike. In science one has a filter; when is an idea good enough to publish. This is a way of controlling the quality of one's output. Art is different and it is also revealing for in visual arts everything done becomes part of the preserved output. Picasso did not paint thinking that everyday he would come up with a masterpiece; he painted a lot. Anybody who has been to a retrospective sees that all great painters painted a lot. The same attitude applies to physical sciences. But once in a while it is important to reflect.

Do not wait for divine inspiration
One should know when one has waited long enough. If ideas do not come one should follow Jasper Johns dictum: "… do something, then do something else to it…" Or to quote another painter, Robert Motherwell, "If you cannot find your inspiration by walking around the block one time, go around two blocks—but never three." Do not wait for "the idea."

Learn how to adapt
Edison put it this way: "… an idea has to be original only in its adaptation to the problem at hand…." Picasso put it more strongly. Bad artists copy, great artists steal. Stealing means picking an idea and making it part of a much larger whole.

Do not converge too quickly
One may solve a problem correctly, but solve what may turn out to be the wrong problem. Masterpieces may appear effortless, but may involve numerous unseen sketches. The time spent in the sketches and turning things around may be much more than the actual time of execution of the final piece.

Step back and look at the entire picture
It is a mistake to fall in love with the "final" product; one should be willing to completely rethink and modify things at the end. Picasso provides a great example. The story is that William E. Hartmann, who was a senior partner at the architectural firm of Skidmore, Owens and Merrill of Sears Tower fame, was visiting Picasso in Mougens, France, and presented Picasso with a catalog of a 1968 Chicago exhibit where Picasso's "Mother and Child" appeared. "The painting was originally different" Picasso reportedly said: "there was a bearded man holding a fish over the baby's head," and proceeded to give Mr. Hartmann the fragment that he had cut at the last moment from the left side of the painting. Both the fragment and the painting are now at the Art Institute of Chicago. Can we take drastic action when things appeared to be finished?

Be conscious of repetition
Success has a drawback: The need to repeat success by going deeper and deeper in an area or problem and not knowing when to quit and move to something else. There is a style of doing science, in the same way that style is what allows a trained person to attribute a painting, that he or she may have not seen before, to a specific artist. In the final instance, style is what makes a scientific theory or a clever experiment recognizable and unique. There is an unavoidable all-too-human tendency to stick with a style, a way of doing things, way past its useful limit. At that point style becomes a caricature.

Decide when to attack a problem
What constitutes the optimal amount of knowledge to tackle a problem? This is a balance of knowing enough, knowing the right things, but often of not knowing too much. This may sound surprising, but it has two clear implications. One is to not second guess our ideas: "Well, if things were that simple someone would have done this before." The second aspect has to do with being fearless. My own take on this came from the avalanche work in granular mixing. Since we did not know much about cohesion we did not factor it into the problem. We were not distracted by roughness effects either. And not being able to use particle dynamics (we still had not learnt it) was, in retrospect, also a plus. We saw the problem though the lens of mappings. Candidly, we focused on the simplest possible problem mostly because we did not know how to deal with the more complex cases. But this was the right thing to do. There are more illustrious examples and Maxwell, again, is a useful case study. The kinetic theory of gases developed by Maxwell predicted that the viscosity was independent of the density, and that the specific heats were constant. Maxwell was troubled by this, and wrote to Stokes (1819–1903) and learned from him that there was (only) one experiment made in 1892 by a scientist of the name Edward Sabine that suggested that the viscosity of air does vary with the density. This disagreement was mentioned by Maxwell in his kinetic theory paper.[94] The case of the specific heats was more problematic—it was clear that the kinetic theory could not account for the variation with temperature. Maxwell made this point clear at the 30th Meeting of the British Association for the Advancement of Science: "[the theory] being at variance with experiment… overturns the whole hypothesis [the molecular hypothesis], however satisfactory the other results may be."[95] The issue of viscosities is an interesting one for another reason. Maxwell conducted experiments and found that the viscosity was nearly constant over a wide range of densities; this, in fact, became one of the stringiest arguments in favor of the kinetic theory (Sabine's experimental results had assumed that the viscosity would vanish at low densities). Had it not been for Maxwell's theory this (rather "natural") assumption would have remained uncontested for a long time. Fortunately, in spite of these two conflicts, constant viscosity and constant specific heats, Maxwell decided to push ahead developing the kinetic theory of gases and inspired others to follow him. In retrospect it would have been hard to develop in one stroke a theory accounting for all these facts. The essential merit of Maxwell's theory is given by the fact that it is still part of the standard curriculum in physical chemistry courses.

Be prepared to prepare the ground

To add new ideas to the scientific cannon is to innovate. And innovation is never easy. Innovation, that is, having an idea being accepted by a field, depends on the new idea connecting with the canonical knowledge of the times. The innovator has to prepare the ground; diminishing the shock of the new, connecting the idea to previous ideas, and increasing the value and reach of the previous ideas. This was said best by Wordsworth in the context of literature: "Never forget what I believe was observed to you by Coleridge, that every great and original writer, in proportion as he is great and original, must himself create the taste by which he is relished."[96]

ACKNOWLEDGMENTS

The number of people who influenced my thinking is large; many are probably unaware that they did so. Thanks to many of them that were given, as things were happening, in the acknowledgment of *The Kinematics of Mixing* and also in my article in *Scientific American*. Another account, much more personal about early influences leading up to my Ph.D. work, appears in Edwards.[18] The point of view of this presentation is broader, and it some sense encompasses both pre- and post-Ph.D. As to ramifications of the work, given the stated objective of the talk, I have kept these to a minimum. There are many people I should thank. If there is a lesson in what I have been able to do it is on the benefits of having a diverse circle of people to influence our ideas, and many I have mentioned in the course of this paper. But that leaves many others. I should start by recognizing my many students. And of these, one who deserves a special mention is Devang Khakhar, a collaborator of many years. At Minnesota I should thank the late Bill Ranz. Bill passed away while I was writing this article, and Chris Macosko—my first paper was written with both of them—but also Roger Fosdick and Dan Joseph, Skip Scriven and Gus Aris, for shaping my early thinking. I was lucky to spend hours talking to Bill Ranz, one of the persons with the best physical intuition I have encountered. On the outside I owe a debt of gratitude to George Batchelor, Stanley Corrsin, and Clifford Truesdell for encouragement of support, and also Jerry Gollub and Harry Swinney, not because I may have used their work directly, but for demonstrating so elegantly the wealth of possibilities at the intersection of fluid dynamics and physics. On topics more directly connected to my research I want to acknowledge Hassan Aref and specially Stephen Wiggins with whom I have interacted since 1986 and collaborated in a very substantial manner over the last few years. I have been blessed by having Steve Davis nearby to provide an anchor in what is fluid dynamics and, on the outside, Andy Acrivos and Bill Schowalter, who have influenced me in more ways than they know. I am thankful to both for comments on a draft of this paper. But in Northwestern my main debt goes to Rich Lueptow, my closest collaborator over the last eight years.

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