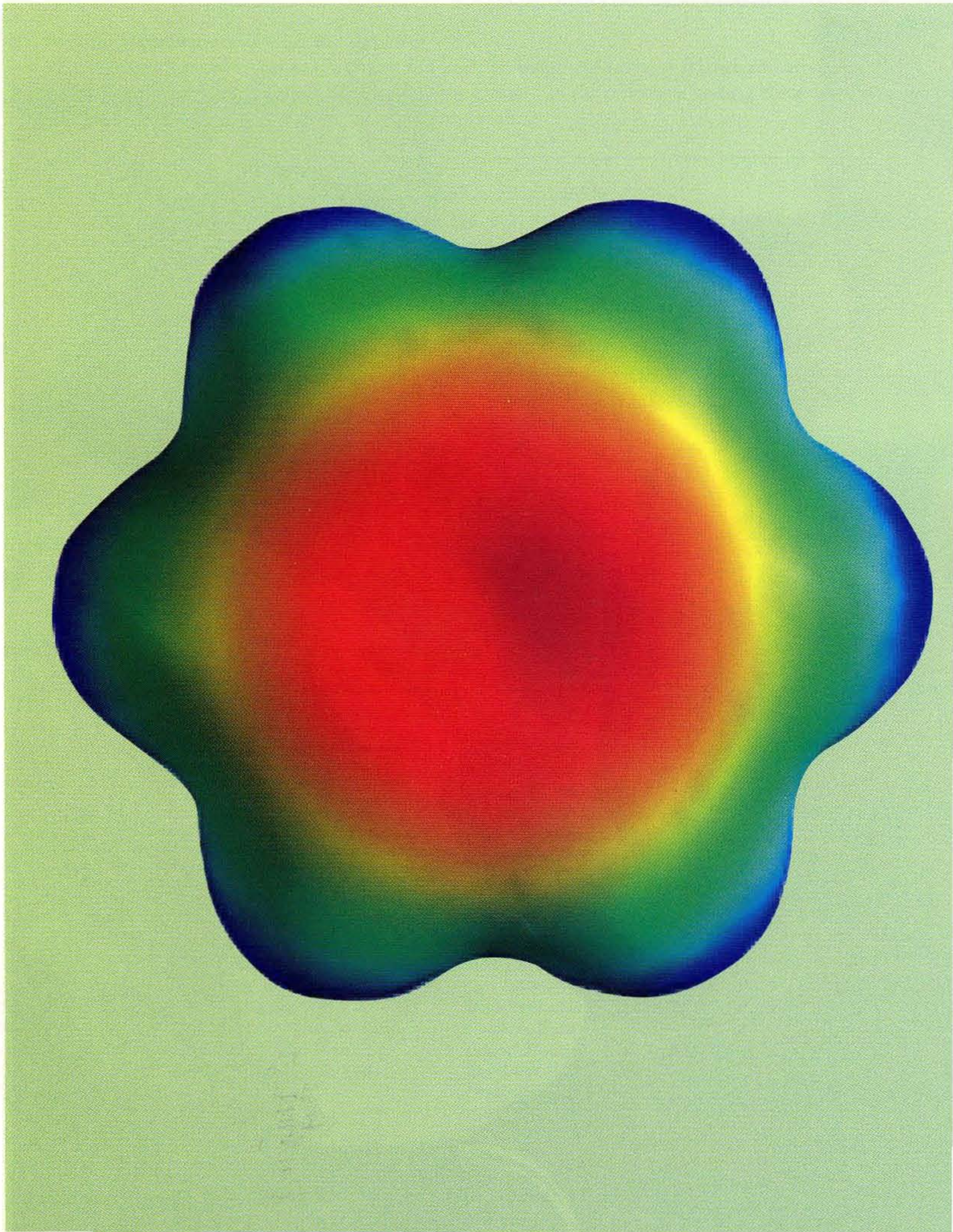
California Institute
of Technology

Engineering & Science

Fall 1994



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Cold Fusion Revisited

Greasy Brain Stuff

Tom Stoppard, who came to Caltech as the James Michelin Distinguished Lecturer, also spoke informally to Caltech's hard-core theater enthusiasts about his career as a playwright. His Michelin Lecture later that evening, "Playing with Science," is adapted in an article beginning on page 2. Faintly distinguishable (but by no means undistinguished) in the background is David Goodstein, author of "Whatever Happened to Cold Fusion?," which begins on page 14 and has nothing whatsoever to do with theater.

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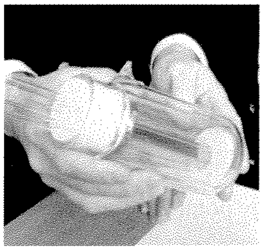

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On the cover: A map of the electrostatic potential—the degree of "static clinginess," if you will—of the flat face of a benzene molecule. The values plotted range from −20.0 (red) to +20.0 (blue) kilocalories per mole. Twenty kcals per mole will make your socks stick together when they come out of the dryer, let alone attract positively-charged ions. This behavior by an ostensibly nonpolar molecule has biological implications, as explained in the story starting on page 26.

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TZARA: Doing the things by which is meant Art is no longer considered the proper concern of the artist. In fact it is frowned upon. Nowadays, an artist is someone who makes art mean the things he does. A man may be an artist by exhibiting his hindquarters. He may be a poet by drawing words out of a hat. In fact some of my best poems have been drawn out of my hat which I afterwards exhibited to general acclaim at the Dada Gallery in the Bahnhofstrasse.

CARR: But that is simply to change the meaning of the word Art.

TZARA: I see I have made myself clear.

CARR: Then you are not actually *an artist* at all.

TZARA: On the contrary. I've just told you that I am.

CARR: But that does not make you an artist. An artist is someone who is gifted in some way that enables him to do something more or less well which can only be done badly or not at all by someone who is not thus gifted. If there is any point in using language at all it is that a word is taken to stand for a particular fact or idea and not for other facts or ideas. I might claim to be able to fly . . . Lo, I say, I am flying. But you're not propelling yourself about while suspended in the air, someone may point out. Ah no, I reply, that is no longer considered the proper concern of people who can fly. In fact, it is frowned upon. Nowadays a flyer never leaves the ground and wouldn't know how. I see, says my somewhat baffled interlocutor, so when you say you can *fly* you are using the word in a purely private sense. I see I have made myself clear, I say. Then, says this chap in some relief you cannot actually *fly*, after all. On the contrary, I say, I have just told you I can. Don't you see, my dear Tristan, you are simply asking me to accept that the word Art means whatever you wish it to mean; but I do not accept it.

TZARA: Why not? You do exactly the same thing with words like *patriotism, duty, love, freedom*, king and country, brave little Belgium, saucy little Serbia—

Playing with Science

There's an activity which we call art and an activity which we call science, and to some degree and in certain ways and in different places, they converge; elsewhere they diverge, and elsewhere they interact, and they also intersect.

by Tom Stoppard

Playwright Tom Stoppard came to Caltech as the third annual James Michelin Distinguished Lecturer on October 20. In the afternoon he met with Caltech's Theater Arts group, then rehearsing Julius Caesar, *and in the evening gave his lecture (which he later described in a* New York Times *article as "60 minutes of desperate free association") to a standing-room-only crowd at Beckman Auditorium. Per tradition, Vice Provost and Professor of Physics and Applied Physics David Goodstein introduced the speaker (see pages 14 and 40 for some of his other activities), and warmly thanked New York fashion designer Bonnie Cashin, whose gift established the lecture series in memory of her uncle. "Bonnie's uncle, James Michelin, was a geologist who always wanted to attend Caltech, but never did, and therefore never lost his affection for us," according to Goodstein. The purpose of the series is to promote a creative interaction between the arts and the sciences, and, said Goodstein, "Tom Stoppard is a living interaction between the arts and the sciences."*

Stoppard's play Hapgood *opened at New York's Lincoln Center in early December.* Arcadia, *currently playing at the Royal National Theatre in London, will come to New York in March.*

Left: Antony Sher plays the Englishman Carr in the 1993 revival of *Travesties* at the Royal Shakespeare Company in London.

I'm going to begin by showing you my first "slide." But now we've begun before we've begun—because I have no slides and yet my first sentence was true. It just happens to contain a metaphor. For a scientist, my first sentence would have been untrue or mistaken. For a playwright, the truth or untruth of a sentence is less rigid: I'm licensed to say "slide" as a metaphor for reading something to you. So we've already noticed, haven't we, that there's another way to use language, different from the one-to-one correspondence of a purely technical—or scientific—language. I wonder whether you think of the one-to-one correspondence of word-to-thing as a limitation to language or as a liberation from the dangers of ambiguity. We will return to the subject of the ambiguity of the very word "language," but in passing I would say that *purposeful* ambiguity, which I suppose has no place in scientific discourse, is an essential feature of what we'll call playful language.

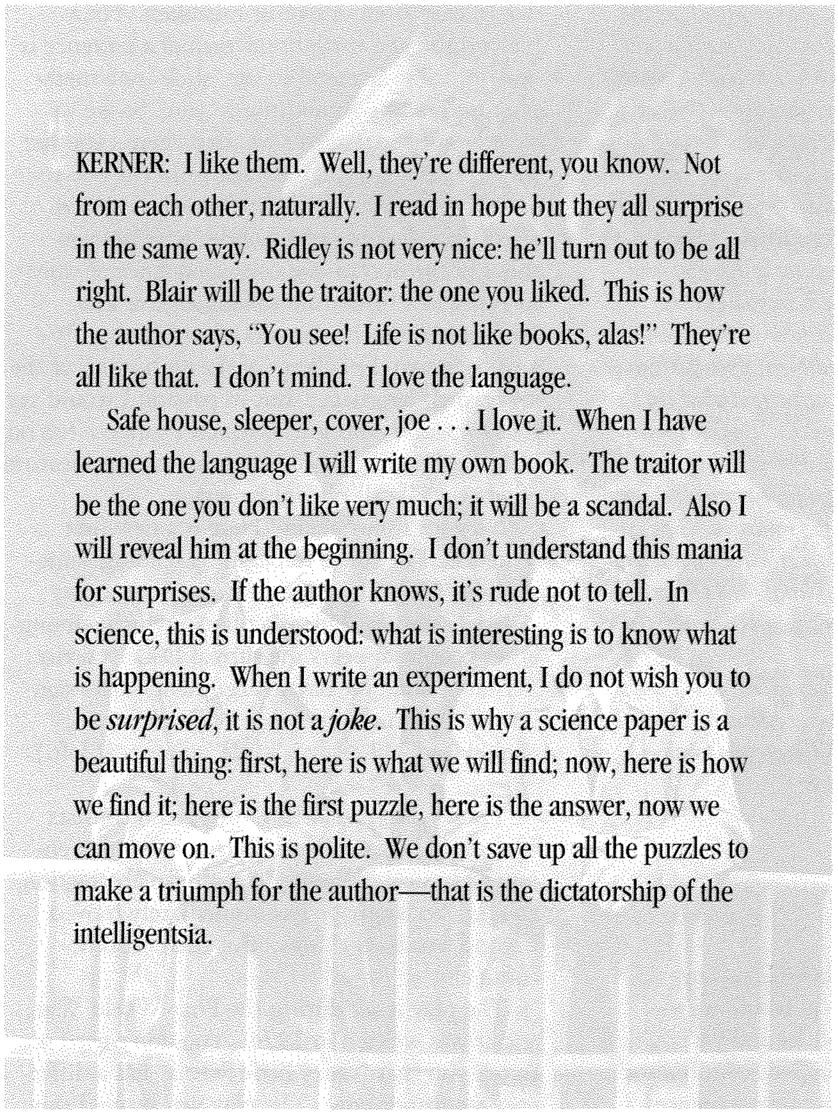
Now let's start again. Here is a new first sentence. I'm going to begin by reading something to you. The passage comes from a play called *Travesties*. This is a play in which, among other people, appears the surreal Dadaist artist Tristan Tzara. He has an argument with a conventional, conservative type of Englishman, named Carr. [The first "slide" appears at left.]

Tzara's list (patriotism, duty, etc.) consists of abstract nouns. Even "Belgium," which enjoys a physical existence, is really an abstraction, an *idea*. So language has immediately moved beyond words-as-things. But there is something else.

The play is set during the First World War, and it was written in 1974. You don't need me to tell you that "saucy little Serbia" has a difference resonance now. The play was revived this year, and when the actor said "saucy little Serbia" a ripple like wind across wheat went across the auditorium. It is as if *time* were a parameter of change in the "meaning" of the words "saucy little Serbia." Perhaps there is no such thing as "ordinary language," and scientific or logical language is not basic but, rather, a special case. Meaning and intent become functions of historical context (the newspaper headlines about Serbia being the context in this example).

Well, having got through all that and read all that, I think it's really time I showed you a slide. I'm not going to lay my work on you all night, but I do have about half a dozen of these extracts to read. They have to be mine because I don't know about any other writers. And I only speak for myself. I don't know how other playwrights think or work. I meet some occasionally but somehow we never ask each other about that. We say, "Whose round is it?" and that kind of thing. So here's the slide [at left]: the speaker is a Russian physicist named Kerner. He is in England and has learned English, and he absolutely loves spy novels and reads very little else.

Below: David Strathairn (right) as the Russian physicist Kerner and Josef Sommer as the senior intelligence official Blair in the Lincoln Center production of *Hapgood*, 1994.



KERNER: I like them. Well, they're different, you know. Not from each other, naturally. I read in hope but they all surprise in the same way. Ridley is not very nice: he'll turn out to be all right. Blair will be the traitor: the one you liked. This is how the author says, "You see! Life is not like books, alas!" They're all like that. I don't mind. I love the language.

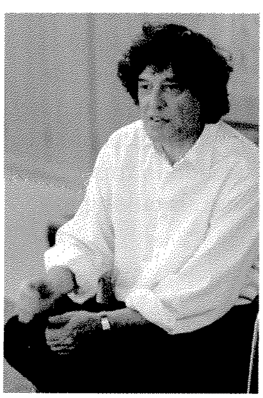
Safe house, sleeper, cover, joe . . . I love it. When I have learned the language I will write my own book. The traitor will be the one you don't like very much; it will be a scandal. Also I will reveal him at the beginning. I don't understand this mania for surprises. If the author knows, it's rude not to tell. In science, this is understood: what is interesting is to know what is happening. When I write an experiment, I do not wish you to be *surprised*, it is not a *joke*. This is why a science paper is a beautiful thing: first, here is what we will find; now, here is how we find it; here is the first puzzle, here is the answer, now we can move on. This is polite. We don't save up all the puzzles to make a triumph for the author—that is the dictatorship of the intelligentsia.

I have two reasons for reading you an extract from this particular play, *Hapgood*. The first reason entails a digression, but I want to tell you how I came to be here. This play was first performed in February 1988. We were in a theater just outside London. If my memory serves, we'd finished rehearsals. I'm not sure if we'd yet had our first audience; perhaps it was the day we were going to have our first audience. Backstage, I borrowed a newspaper from the stage doorman, and looking through it I saw an obituary of Richard Feynman. My grief was acute. I never knew him, had never met him. I have had no education in physics at all. I get interested in things, and I read some science—I call it science; *you* would call it airport paperbacks. I should say that my grief about Feynman was entirely selfish because I had an epigraph from Feynman in front of this play. In a sort of fan-club way, I had intended to send him my play—not really that he should read it; I just wanted this metaphysical connection between us. He'd get it in the mail, and he'd probably throw it on his desk, but somehow a bit of me would be in his house, and that meant something to me. I left it too late. The epigraph was from Feyman's *Lectures on Physics*:

We choose to examine a phenomenon which is impossible, *absolutely* impossible to explain in any classical way, and which has in it the heart of quantum physics. In reality it contains the *only*

Rather as a lover of Wordsworth might come to the Lake District, I came to Caltech, just to see where Feynman lived and worked.

mystery . . . Any other situation in quantum mechanics, it turns out, can always be explained by saying, 'You remember the case of the experiment with the two holes? It's the same thing.'



A year later, *Hapgood* was being done in Los Angeles. Rather as a lover of Wordsworth might come to the Lake District, I came to Caltech, just to see where Feynman lived and worked. My son, who was studying low-temperature physics, was with me. So I called up David Goodstein—a cold call. He was very sweet to us and showed us around. I looked around thinking, well, Feynman was here, and it's better than nothing, being here myself for a while. So I consider that you, collectively, were awfully kind to me. The final upshot of that meeting is that David asked me to give this lecture, and here I am.

My second reason for choosing this extract from *Hapgood* is that it implies a promise that I would also lay out my agenda, my wares. Then you would know what we were here to do and what we were trying to achieve. I suppose I can go some way towards doing that. We are here under the title of "Playing with Science." Somebody phoned me up and said, "We have to print this thing. Do you have a title?" And after a moment I said, "Playing with Science," which seemed reasonable because I felt I could say almost anything under that title. The agenda which I felt was appropriate is something like this: there's an activity which we call art and an activity which we call science, and to some degree and in certain ways and in different places, they converge; elsewhere they diverge, and elsewhere they interact, and they also intersect. We might consider what esthetics means in the context of science and art, and also the differences and similarities in the creative process between scientists and artists. And we might ask what exactly is reality, which is a favorite subject in theater.

What I'm *not* going to attempt (I hope you're as pleased as I am) is a historical survey of science in plays—*Galileo* and all that. I have no instinct towards learning these things or caring about them. I like individual plays. I don't really get interested in the abstractions and the generalities of what's happening in the history of theater. However, in the same breath I should say that on the occasions that I go to see a play or a film which purports to be about, for example, Turing, or the making of the atom bomb, I feel a sense of broken promise when I discover, as of course one invariably does discover, that there's simply no science in them at all, really. So, clearly, you have to take me with a pinch of salt when I disclaim that I'm a frustrated scientist, or a closet scientist. I feel I really am not, but there's something in me which often causes a reaction when I come across some science news. I had one term of physics when I was 13. I did no chemistry. We all did biology, but all I remember is cutting up dogfish; I remembered the smell for years. So I emerged from school with no science whatever. I think I'm here because I've written two plays which have some science in them, and apparently it does take two. One play may be thought an aberration, but two suggests purpose. Don't be misled, however. My next play is about India, and it includes some words on the miniature art of the Mogul empire in the 19th century in northern India. I'm confidently expecting an invitation to lecture at the Huntington Library next year.

What is a play? And what is theater? I'm going to do this *at* you, although you think you already know what a play is. Well now, suppose you were to go into the campus bookshop and say, "I want *Pride and Prejudice*, please, and Beethoven's Fifth, and I'd like Warhol's Marilyn Monroe print, and I would like *Death of a Salesman* by Arthur Miller." As the chap's putting this stuff together, he gives you a bound stack of pages between two covers, and he gives you a circular disk, a flat thing, and then he gives you a kind of flat rectangular plane which goes on the wall. And then he gives you another stack of pages. And you say, "No, no. The Arthur Miller one is a *play*." And he'd say, "Well, yeah, that's how they come." There's something odd about this. I suppose a play is a text, but theater is an event. Already we've shifted the idea nearer towards science in a general way. It is an event.

I might have said Shakespeare's *The Tempest* instead of *Death of a Salesman* because I want to describe to you a scene in a production of *The Tempest* which took place some years ago in Oxford College. The play was set out of doors, on the lawn which backed onto a lake. It began in natural light, and, as the play developed, it was time for Ariel, the sprite, to leave the action. Ariel said what he says, and he turned and ran across the grass. When he got to the edge of the lake, he kept running across the top of the water, because the producer had put a boardwalk just an inch below the water. Evening was coming on now, and you could just barely see him, and then you could hear him go "plish, plash" across the water. As he approached the other side of the lake, the evening swallowed him up, and as he disappeared, from the further shore a firework rocket was ignited, and it went—whoosh—into the sky. The rocket burst into sparks, and then all the sparks went out one by one, and he'd gone. When you look this up it says "Exit Ariel."

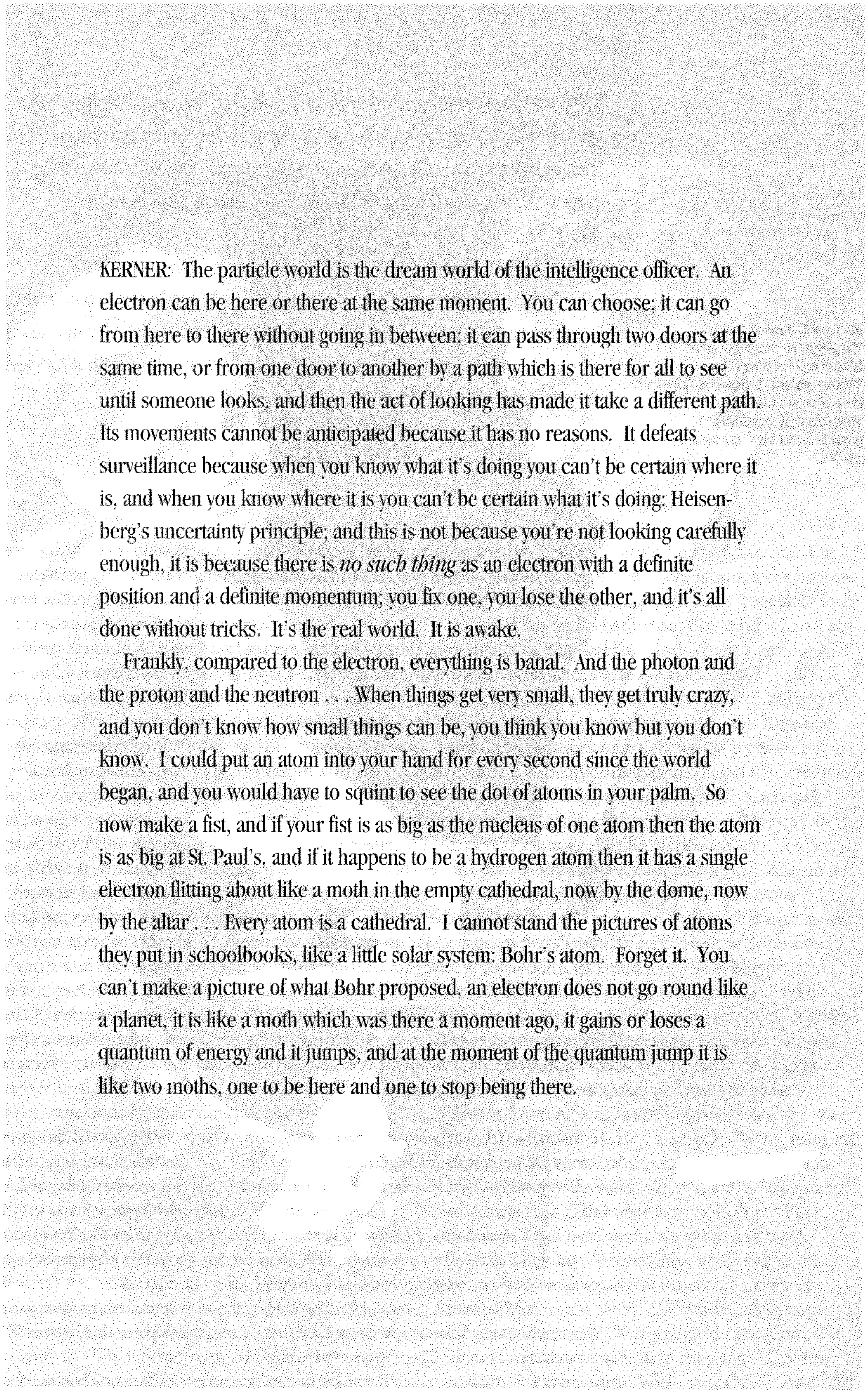
So we're talking about an event. It might be true, or at any rate provocative, to say that theater is an experiment which never repeats its results. By that I mean not that every production of The *Tempest* is different; I mean that the same production of *The Tempest* is different night to night. The equation which goes into the event is so complex that it cannot actually be repeated.

For a moment there we looked at art considered as a science. Let's now look at science as a subject matter of art. When I say *art,* I'm really talking only about the one I know about, which is the theater, under which I would also include film. I'm going to read you a speech from a screenplay made from a book called *Hopeful Monsters* by Nicholas Mosely. It has some physics in it, and a lot of other things too. And I'm going to follow that with two other slides: the first deals with quanta, and the last with entropy. But first—atoms. *Hopeful Monsters* takes place in the early twenties in England, and in this scene [left] Hans, a German physicist, is talking to a 12-year-old boy, Max.

MAX: But is it true, Hans?
HANS: See this.
MAX: Your ring?
HANS: Gold, pure gold. If I cut it in half, I still have gold, naturally. If I keep cutting it in half, do I have gold forever? Smaller and smaller pieces of gold? No. Finally I get to an atom of gold. And when I cut that in half, I don't have gold anymore; I just have little pieces of electricity.
MAX: Really?
HANS: Yes, really. The nucleus of an atom of gold is little pieces of electricity stuck together—what are called protons. The difference between gold and radium or any of the natural elements is just the difference in the number of protons which are stuck together like a sugar lump. Radium has 88.
MAX: But how did Professor Brainbox make a bomb?
HANS: Ah, that is his secret. But somehow it seems he broke up his radium sugar lumps and the force which keeps the sugar lumps together is suddently released. Of course, in each atom there's only a very little bit of force, but in a piece of radium as big as a pineapple, well, there are as many atoms as grains of sand on all the beaches you can ever think of. Boom.

The technique here is, as you can appreciate, bald. The atom explained for 12-year-olds (the mental age of film and theater audiences when confronted with science). I borrowed the idea of cutting an atom endlessly in half from C. P. Snow, the scientist-novelist. Give or take a sugar lump, this is an attempt at unadorned explication—always a troublesome business in my business.

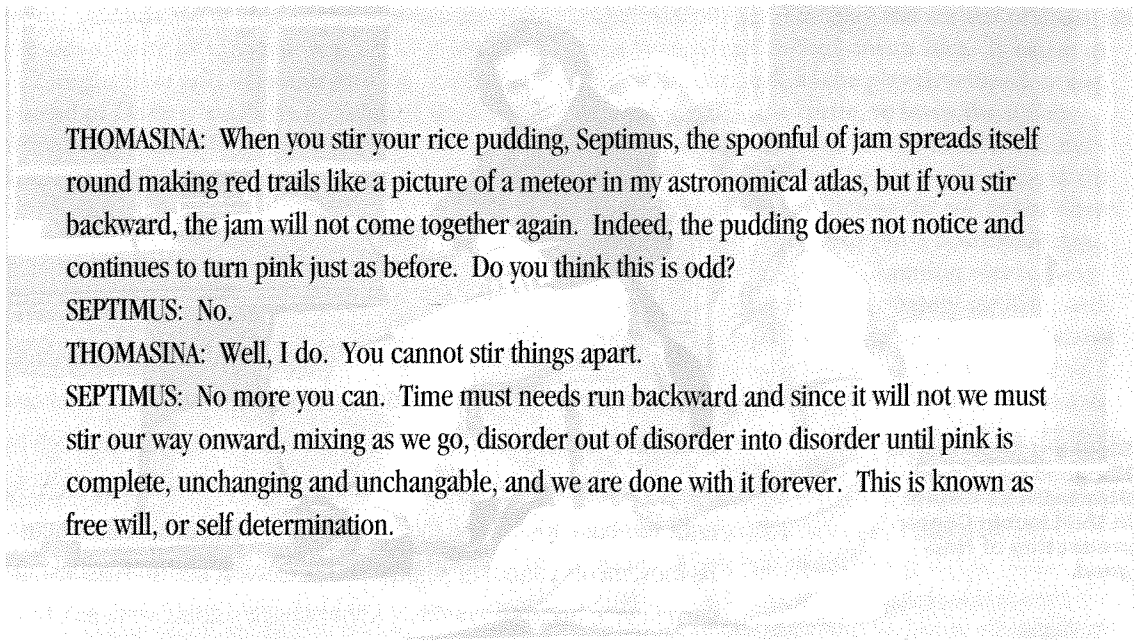
In *Hapgood* the subject is quantum physics. Kerner (the one who reads the spy novels) is coopted into the espionage world. You can see that to try to get the science into art, one has to try to transmute it in some way. He says: [right]



Stockard Channing as Hapgood and David Strathairn as Kerner in the Lincoln Center production of *Hap-good.*

KERNER: The particle world is the dream world of the intelligence officer. An electron can be here or there at the same moment. You can choose; it can go from here to there without going in between; it can pass through two doors at the same time, or from one door to another by a path which is there for all to see until someone looks, and then the act of looking has made it take a different path. Its movements cannot be anticipated because it has no reasons. It defeats surveillance because when you know what it's doing you can't be certain where it is, and when you know where it is you can't be certain what it's doing: Heisenberg's uncertainty principle; and this is not because you're not looking carefully enough, it is because there is *no such thing* as an electron with a definite position and a definite momentum; you fix one, you lose the other, and it's all done without tricks. It's the real world. It is awake.

Frankly, compared to the electron, everything is banal. And the photon and the proton and the neutron . . . When things get very small, they get truly crazy, and you don't know how small things can be, you think you know but you don't know. I could put an atom into your hand for every second since the world began, and you would have to squint to see the dot of atoms in your palm. So now make a fist, and if your fist is as big as the nucleus of one atom then the atom is as big at St. Paul's, and if it happens to be a hydrogen atom then it has a single electron flitting about like a moth in the empty cathedral, now by the dome, now by the altar . . . Every atom is a cathedral. I cannot stand the pictures of atoms they put in schoolbooks, like a little solar system: Bohr's atom. Forget it. You can't make a picture of what Bohr proposed, an electron does not go round like a planet, it is like a moth which was there a moment ago, it gains or loses a quantum of energy and it jumps, and at the moment of the quantum jump it is like two moths, one to be here and one to stop being there.



Rufus Sewell as Septimus Hodge and Emma Fielding as Thomasina Coverly in the Royal National Theatre (London) production of *Arcadia*, 1993.

THOMASINA: When you stir your rice pudding, Septimus, the spoonful of jam spreads itself round making red trails like a picture of a meteor in my astronomical atlas, but if you stir backward, the jam will not come together again. Indeed, the pudding does not notice and continues to turn pink just as before. Do you think this is odd?
SEPTIMUS: No.
THOMASINA: Well, I do. You cannot stir things apart.
SEPTIMUS: No more you can. Time must needs run backward and since it will not we must stir our way onward, mixing as we go, disorder out of disorder into disorder until pink is complete, unchanging and unchangable, and we are done with it forever. This is known as free will, or self determination.

The entropy passage [above] comes from a recent play, *Arcadia*. The scene involves another young person, a 13-year-old girl this time.

How gratifying that various passages, written years apart, should converge on my title, "Playing with Science." I captioned my three readings: atom, quantum, and entropy. But I turned out to be talking about sugar lumps, moths, and rice pudding. In the third passage, entropy didn't even get a mention, and perhaps on that score the third passage is the successful one. Pure metaphor. Metaphors may be apt (effective) or inapt. The response which makes that decision is a form of *esthetic* response.

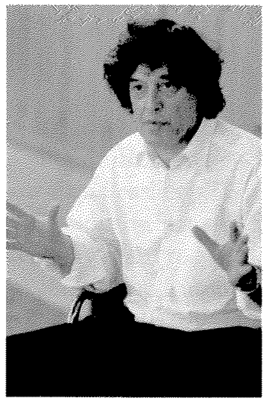
On the subject of esthetics, I'm happy to evoke Richard Feynman again. At an art-and-science meeting in London, I met Prof. Arthur I. Miller, not the author of *Death of a Salesman* but the head of the Department of the History, Philosophy and Communication of Science at University College London. The following is extracted from his paper published in *Languages of Design*.

> In his characteristically emphatic way, the American physicist Richard Feynman described his immediate reaction to a new theory he developed in 1958:
>
> *There was a moment when I knew how nature worked . . . It had elegance and beauty. The goddamn thing was gleaming.*
>
> —Richard Feynman, 1957 [8(338)]
>
> What notions of elegance and beauty did Feynman have in mind? The elegance came from a mathematical formalism which Feynman had been honing since his university days and had served as a basis for his 1948 theory of how electrons interact with light . . . The beauty of Feynman's theory can be seen only in the eye of a physicist. It is a beauty which concerns the theory's universality by which I mean the possibility of its use beyond the discipline to which it was intended.

Prof. Miller makes the point that one can talk about modern science in the way that one talks about modern art. Interestingly, what was happening to science towards the end of the 19th century and beginning of the 20th was happening to art at roughly the same time—cubism was trying to lead towards Picasso and beyond. Prof. Miller has also published a comparative study of Henri Poincaré and Albert Einstein. He uses some of the scientists' own introspections to try to figure out how their minds, their creative processes, worked. He also quotes from the notes of a psychologist named E. Toulouse, who conducted a series of interviews with Poincaré and Émile Zola:

> The one [Zola's] was an intelligence that was willful, conscious, methodical, and seemingly made for mathematical deduction: it gave birth entirely to a romantic world. The other [Poincaré's] was spontaneous, little conscious, more taken to dream than for the rational approach and seemingly throughout apt for works of pure imagination, without subordination to reality: it triumphed in mathematical research.

The convergence between art and science is



When we talk about "universality" having different meanings, we're saying that language works in different ways. It works by association and works through metaphor.

Science and art are nowadays beyond being like *each other. Sometimes they seem to* be *each other.*

not going to strike anybody here with novelty. I think we've tended to create and talk about a false dichotomy, and I think that we acknowledge that it is a false one most of the time we're talking about it. Science and art are nowadays beyond being *like* each other. Sometimes they seem to *be* each other. But while they converge, interact, and intersect, they diverge, too, and language sometimes throws light on this. When we (on the art side) hear about the beauty of Feynman's insight residing in its universality, we do recognize "universality." That's a word which crops up all the time in lit crit. But we mean something slightly different by it. In mathematics, perhaps, there's a correspondence between the *elegance* of a function and what it represents, say, the correspondence between the function $x^2 + y^2 + z^2$ and what the Greeks considered to be the perfection of a sphere. You then start to think, what exactly is it that we acknowledge as its beauty? A scientist might note that we can rotate it; we can look at it in a mirror; we can turn it upside down; and it preserves itself in all these variations and remains absolutely symmetrical and perfect. But that's a special way of talking about esthetics.

One day some time ago, I had the pleasure and honor of meeting Mandelbrot of "the set" at a sort of art exhibition. As you're probably aware, sections of Mandelbrot's set are now postcards, posters, and so on. I was quite keen on the whole thing and ended up buying about 40 postcards, 38 of which I never managed to think of anybody to send to. They never seemed right. I thought this was telling me something about the kind of art I was trying to palm off on my friends. On the whole I don't think there is much correspondence between what the computer generates from an equation and what artists do. And when I say artists here, I mean the kind which I am not—people who actually make pictures.

When we talk about "universality" having different meanings, we're saying that language works in different ways. It works by association and works through metaphor. This is where we came in, isn't it, with the Dadaist? Curiously enough, it was a mathematician in *Through the Looking Glass* who made somebody say "a word means whatever I choose it to mean." And in a way it does. Take as an example the word "cowboy." What's the first thing that comes into your head? Somebody will think of John Ford, and somebody will think of John Wayne, and somebody will think of a hat, and the cowboy icon, and also the sort of macho image of cowboys in our culture. I have always thought that was quite an interesting thing, because the job of looking after cows exists all over the place. Where I come from it tends to be done by a man in rubber boots, wearing a smock. Now, imagine that for one reason or another this Englishman had to change his work clothes; say he emigrated to America in 1880. He arrives in New York and says, "I'm a cowman. Is there any work here?" They say, "Here? No, you have to go West." So he gets on the train and shows up somewhere in the West. When he asks people for a job, they say, "Well, what do you do?" He says, "I'm a cowman." And they say, "Cow*boy*, surely." And he says, "Well, yes, OK." And they

Stoppard answers questions for members of TACIT (Theater Arts at Caltech).

At the age of forty-something I was exclaiming, "My gosh, this is amazing! How interesting!" about stuff which anybody who had stuck with physics through high school was wearily familiar with. . . . My interest in it, of course, was as metaphor.

say, "Fine. Sign here. You've noticed that the weather here is very hot; you need a rather wide-brimmed hat. And so that you don't burn your neck, you put this thing, this neckerchief, around your neck. The bushes here have gigantic prickles on them so we tend to put these leather things around our trousers, which themselves are made of very tough material because we ride horses, there being no roads here. And you need boots with a high heel because otherwise they'll fall out of the stirrups." The person has not changed. There *is* no person to change. I just invented him. But our response to the person may have changed. He has become a more romantic, macho kind of figure. But only one thing has really changed—the word which triggers the response: from "cowman" to "cowboy." It was all done by association. Creative language works associatively.

But we don't mean, do we, that language works by association word by word. In the two plays, which I've read bits of, with some science in them, what I was interested in was the metaphor. *Hapgood* is a play which derived from my belated recognition of the dual nature of light—particle and wave. As I said before, I've never done any physics. At the age of forty-something I was exclaiming, "My gosh, this is amazing! How interesting!" about stuff which anybody who had stuck with physics through high school was wearily familiar with. But I was thinking, "Gosh, I've found something out which I can use." My interest in it, of course, was as metaphor.

In a play called *The Fire Raisers* by Max Frisch, two arsonists are burning down a town. One day a very sinister man comes and knocks at the door of a bourgeois household, insinuates himself into the household, and in no time at all is in the attic as a lodger. Soon after that he introduces an equally sinister friend, and they share the attic. They leave the house and come back; then they leave the house and come back again, and it seems that when they've left the house and come back, another building has burned down. Then they start bringing cans of gasoline into the house and filling the attic with it. They'd take a few cans out and come home, and each time there would be a building burned down. Meanwhile, downstairs this bourgeois family is getting more and more concerned but they won't really talk about it. The father is there with his pipe and his newspaper, saying, "It's awful. When are they going to catch these arsonists?" And none of them can quite meet each other's eye. The moment finally comes when the larger and more sinister of these two people comes downstairs and asks the leader of the household, "Do you happen to have a box of matches?" After a rather long and thoughtful pause, the gentleman puts his hands in his pocket and hands over a box of matches. The sinister man says, "Thank you very much," and goes upstairs, whereupon the husband turns to his wife and says, "Now, look, if they were the arsonists, they'd have their own *matches*."

I saw this play when I was quite young, and I loved it, and I knew exactly what it was about. In fact, I went around telling people exactly what it was about: it was quite clearly about how the Nazis came to power in pre-war Germany. Some

What is real? Theater is not real. . . . I mean, we know that it's not actually a salesman coming home from failing to sell something and having a miserable evening. We know it's not really about some chap finding out that he's married his mother.

time later, I discovered that the author was under the impression that it was about how the Communists came to power in post-war Eastern Europe. The reason I mention it is that I wasn't wrong. In a certain sense the author can't say that I'm wrong. And I feel that I can't, and I never do, say that somebody is wrong in the way they interpret what I write. How can one refuse the existence of a response? It is its own validation.

We're talking now about language operating in a way which perhaps it doesn't in Kerner's scientific paper in the passage I read earlier. The subject matter in theater, in a more abstract sense than I've dealt with so far, has very often to do with what actually is real. A lot of people at Caltech might be said to be concerned with that single question. What is happening? What is real? Theater is not real. Now, again, we think we already knew that, don't we? I mean, we know that it's not actually a salesman coming home from failing to sell something and having a miserable evening. We know it's not really some chap finding out that he's married his mother. We know that that's not the kind of reality we're talking about.

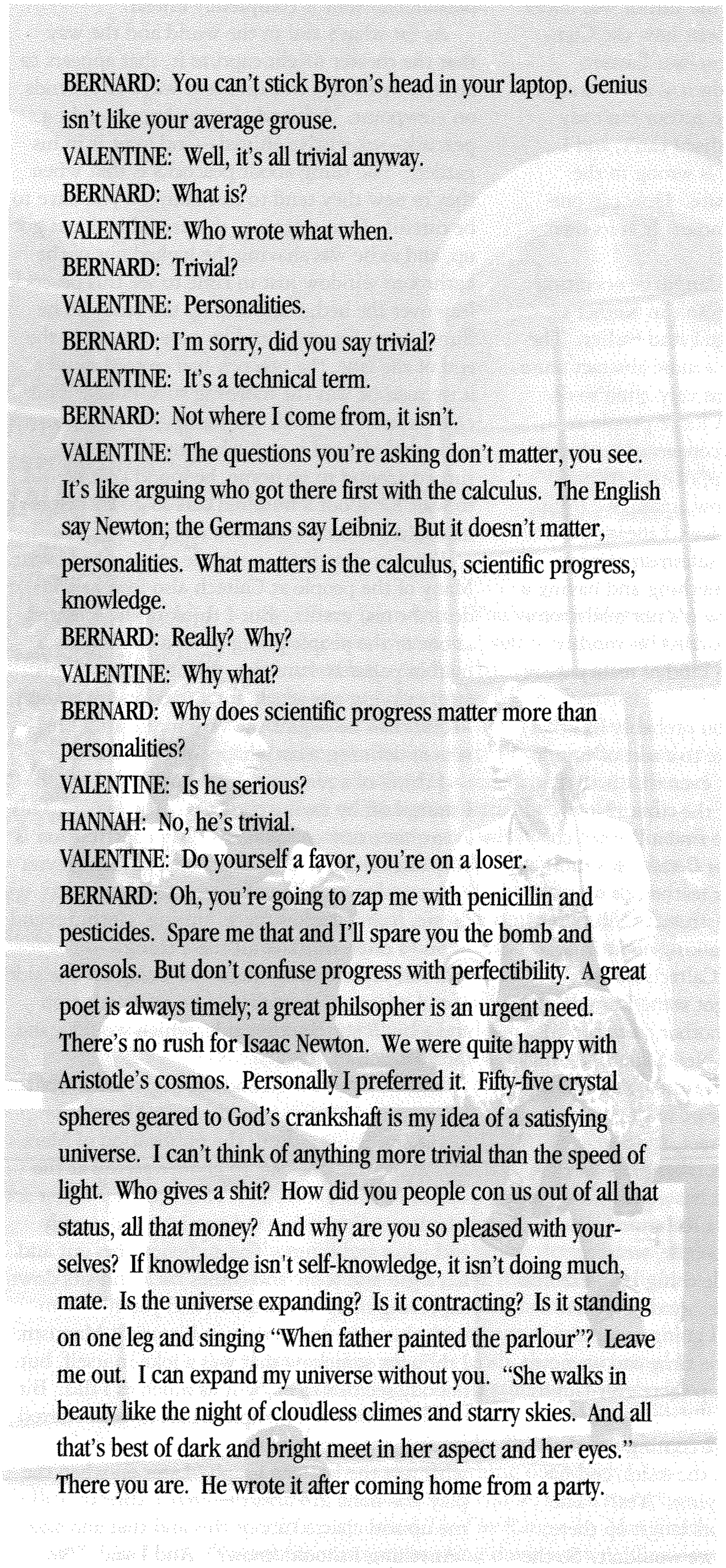
But on the other hand, you probably feel that certain kinds of theater aspire to a sort of simulation of reality. I don't think even that is true. Clearly, it's not true most of the time. Nowadays, one would be lucky to find a Roman column in a production of *Julius Caesar*. It's more likely to be chrome, or black leather, or whatever. This is fine; it can be very instructive, illuminating, and effective. I'm not talking about that. [Actually, the audiences for Caltech's recent production of *Julius Caesar* got some Roman columns.] At one time or another, possibly all of us have enjoyed a play by Neil Simon. At his best he's given me a lot of pleasure. You can tell by the design that there appears to be something real going on: it's never abstract, it's never symbolic. The action always takes place in a room, and tremendous effort has gone into making this room resemble a real room. People onstage are, as it were, real people, wearing proper clothes, and the whole thing is an exercise in re-creation of a slice of life. And yet, there's something completely weird going on up there on the stage. It's there all the time and we never notice it. It is that nobody up there ever laughs at any of those things we're all laughing at. These brilliant wisecracks are coming out three a minute and *we're* falling in the aisles, and up there, it's all these people saying, "Yes?" The convention is that if the actors laugh up there, they'd be doing it for us, so we wouldn't. So the behavioral event is completely unreal.

As for what's real in the world and the way that the theater might capture it, that appears to be equally elusive in a different way. It depends on viewpoint. A friend of mine once bought a peacock—expensive animal—and kept it in his garden. The thing about peacocks is that when they're new they tend to run away, so you have to be careful. One morning, this friend had just got up, and as he was shaving, he looked out of the bathroom window just in time to see this peacock leap over the hedge and run up the lane. So he flung down his razor, and he gave chase. At the end of the lane, the peacock had crossed quite a busy road (it was the morning rush-hour). This chap crossed the road, caught up with the peacock, and clasped it to his bosom. When he turned around to go home, he found that he had to wait for about a hundred cars to go by before he could get back across the road.

I've just described in simple terms a real event. Many of the people at Caltech also look at and describe real events. But I think of the scientist as one of the people going by in a car: he sees a man in pajamas, bare feet, shaving-foam on his face, carrying a peacock, for a fraction of a second—and then he begins the very interesting business of defining what's happened out there.

I think of a play as constituting an equation. I started off by saying the thing is an event. This event has many components. My contribution is only one of them. The experience acting on you is a complex equation of sense, sound, sight, music, light, shadow, pace, timing, clothing, and so on. I often think of all these things—or symbols representing them—as being on one side of the equation; then there'd be an equals sign and a big *S* on the other side, which would stand for Satisfaction.

In *Travesties*, the second act began with what I thought was quite a good idea: a 15-minute lecture on Lenin—from the publication of Marx's *Das Kapital* all the way to Lenin's arrival at the Finland Station in 1917. This was after a first act which was lots of fun and pastiche and parody and jokes and songs. The audience goes out and has a gin and tonic and comes back and sits down expecting more of the same, and you hit them with this very dry lecture on historical Marxism. I thought somehow that was a joke in itself, but nobody seemed to enjoy it as much as I did. Bit by bit (theater is an empirical art form) I started cutting away at this lecture, and we ended up with just the last paragraph. Later on when the play was done in Paris, the French director called me up and chatted about this and that and asked, "Anything I should know?" And I said, "No,"



Bill Nighy as Bernard Nightingale and Felicity Kendal as Hannah Jarvis in the Royal National Theatre production of *Arcadia*.

BERNARD: You can't stick Byron's head in your laptop. Genius isn't like your average grouse.
VALENTINE: Well, it's all trivial anyway.
BERNARD: What is?
VALENTINE: Who wrote what when.
BERNARD: Trivial?
VALENTINE: Personalities.
BERNARD: I'm sorry, did you say trivial?
VALENTINE: It's a technical term.
BERNARD: Not where I come from, it isn't.
VALENTINE: The questions you're asking don't matter, you see. It's like arguing who got there first with the calculus. The English say Newton; the Germans say Leibniz. But it doesn't matter, personalities. What matters is the calculus, scientific progress, knowledge.
BERNARD: Really? Why?
VALENTINE: Why what?
BERNARD: Why does scientific progress matter more than personalities?
VALENTINE: Is he serious?
HANNAH: No, he's trivial.
VALENTINE: Do yourself a favor, you're on a loser.
BERNARD: Oh, you're going to zap me with penicillin and pesticides. Spare me that and I'll spare you the bomb and aerosols. But don't confuse progress with perfectibility. A great poet is always timely; a great philsopher is an urgent need. There's no rush for Isaac Newton. We were quite happy with Aristotle's cosmos. Personally I preferred it. Fifty-five crystal spheres geared to God's crankshaft is my idea of a satisfying universe. I can't think of anything more trivial than the speed of light. Who gives a shit? How did you people con us out of all that status, all that money? And why are you so pleased with yourselves? If knowledge isn't self-knowledge, it isn't doing much, mate. Is the universe expanding? Is it contracting? Is it standing on one leg and singing "When father painted the parlour"? Leave me out. I can expand my universe without you. "She walks in beauty like the night of cloudless climes and starry skies. And all that's best of dark and bright meet in her aspect and her eyes." There you are. He wrote it after coming home from a party.

and then I said "Oh, yes, Cecily's lecture, top of Act II—don't feel you have to use all of it because we didn't. I thought I'd get away with it because it's a new character and she's young and pretty." He said, "*Mais, non.* We must have it all." And I said, "No, listen, I've been there; you don't really have to do this." And he said, "But it's *magnifique*!" So I said, "All right. Fine." Several months later this play happened and I called him up and said, "How are things?" "Wonderful," he said. And I said, "And Cecily's lecture?" "*Formidable*," he said. I thought, "Well, that's the kind of audience I deserve." So I go over to Paris to see the show. And he's right. She does the whole thing. The audience is rapt. You could hear a pin drop. The thing he hadn't told me was that she's doing it stark naked.

So, going back to our equation, the Cecily Lecture I was warning him against would look something like this: $n(t) = S - (co)$, where n is the scene, t is the 15 minutes, S is satisfaction, and (co) is the clothes-off factor. By adding clothes off to each side of the equation, the Parisian director achieved satisfaction. In London, we got S by doing $n(t - m)$, where m is most of the 15 minutes, but it wasn't as much fun.

There's a lot that might be said about where the artist and the scientist diverge, but all we really know about it is that there's some kind of attitude of the artist towards the scientist Here [left] is a literary man talking to a scientist (he studies grouse, birds) from *Arcadia*.

Well, of course, I load the dice. That's what I do for a living. But we do recognize something.

Well, of course, I load the dice. That's what I do for a living.

We recognize that it's like two kinds of animal meeting in the street. But elsewhere, Bernard, the literary Byron-lover of *Arcadia*, talks about the creative moment, and in trying to describe it he describes something which I believe is the same experience known to scientists in *their* most creative moments.

And because I want to end on a point where art and science intersect, I'll end with what Bernard says:

> BERNARD: I'll tell you your problem. No guts. By which I mean a visceral belief in yourself—gut instinct. The part of you which doesn't reason. The certainty for which there is no back-reference, because time is reversed. Tock tick goes the universe, and then recovers itself. But it was enough. You were in there and you bloody *know*. □

AUTHOR'S NOTE

This article is based on a transcript of a talk delivered from notes. I am grateful to the editors of *Engineering & Science* for giving me the opportunity to sweep up after myself. I have added some remarks, and rephrased others, while trying to retain the general order and sense of what was received by my—as the transcript makes clear to me—tolerant audience.

Tom Stoppard was born in Czechoslovakia, moved with his family to Singapore when he was two years old, and then escaped just ahead of the Japanese invasion to India. When World War II ended, his family settled in England, where he still resides. After graduating from school and beginning his career as a journalist, Stoppard turned to writing short stories and radio plays, and eventually stage plays. His first major dramatic success came with his 1966 comedy, Rosencrantz and Guildenstern Are Dead, *which immediately drew acclaim from both sides of the Atlantic for Stoppard's language virtuosity and wit, not to mention his knowledge of probability theory (a coin comes up heads 126 times in a row, provoking much discussion throughout the play).* Jumpers *followed in 1972; then came* Travesties *(1974), and, among others,* Every Good Boy Deserves Favor *(1978),* The Real Thing *(1984), and* Artist Descending a Staircase *(1988), all of which played in New York as well as in London. He has also written screen adaptations of* Rosencrantz and Guildenstern Are Dead, *J. G. Ballard's* Empire of the Sun, *and John Le Carré's* The Russia House, *and co-authored (with Terry Gilliam, formerly of Monty Python) the original screenplay of* Brazil.

*This year Stoppard had two plays running in London—*Arcadia *and a revival of* Travesties. *When* Hapgood, *which had originally played in London in 1988 and in Los Angeles in 1989, opened December 4 in New York, one critic described Stoppard as a "writer of uncommon cleverness, {who} has always laced his plays with antic wit and provocative ideas banging against other provocative ideas." But understanding this play, the critic complained, required "a nimble mind, an alert eye and graph paper."*

Whatever Happened to Cold Fusion?

According to everything we know about the behavior of matter and nuclei, cold fusion is impossible.

by David L. Goodstein

In the five and a half years since the Caltech Three burst the cold fusion bubble, they've done very little with the subject beyond keeping track of the literature and posing for this picture: from left, Nate Lewis, professor of chemistry; Steve Koonin, professor of theoretical physics; and Charles Barnes, professor of physics, emeritus.

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On December 6–9, 1993, the Fourth International Conference on Cold Fusion took place on the island of Maui, in Hawaii. It had all the trappings of a normal scientific meeting. Two hundred and fifty scientists took part, mostly from the United States and Japan (hence the site in Hawaii), but also a sprinkling from Italy, France, Russia, China, and other countries. More than 150 scientific papers were presented on subjects such as calorimetry, nuclear theory, materials, and so on. The founders of the field, Stanley Pons and Martin Fleischmann, were in attendance and were treated with the deference due their celebrity status. Pons and Fleischmann carry out their research today in a laboratory built for them in Nice, on the French Riviera, by TECHNOVA, a subsidiary of Toyota. At the meeting it was announced that the Japanese trade ministry, MITI, has committed $30 million over a period of four years to support research on what was delicately called "new hydrogen energy," including cold fusion.

Contrary to appearances, however, this was no normal scientific conference. Cold fusion is a pariah field, cast out by the scientific establishment. Between cold fusion and respectable science there is virtually no communication at all. Cold fusion papers are almost never published in refereed scientific journals, with the result that those works don't receive the normal critical scrutiny that science requires. On the other hand, because the cold-fusioners see themselves as a community under siege, there is little internal criticism. Experiments and theories tend to be accepted at face value, for fear of providing even more fuel for external critics, if anyone outside the group was bothering to listen. In these circumstances, crackpots flourish, making matters worse for those who believe that there is serious science going on here.

The origins of cold fusion have been loudly and widely documented in the press and popular literature. Pons and Fleischmann, fearing they were about to be scooped by a competitor named Steven Jones from nearby Brigham Young University, and with the encouragement of their own administration, held a press conference on March 23, 1989, at the University of Utah, to announce what seemed to be the scientific discovery of the century. Nuclear fusion, producing usable amounts of heat, could be induced to take place on a tabletop by electrolyzing heavy water, using electrodes made of palladium and platinum, two precious metals. If so, the world's energy problems were at an end, to say nothing of the fiscal difficulties of the University of Utah. What followed was a kind of feeding frenzy, science by press conference and e-mail, confirmations and disconfirmations, claims and retractions, ugly charges and obfuscation, science gone berserk. For all practical purposes, it ended a mere five weeks after it began, on May 1, 1989, at a dramatic session of the American Physical Society, in Baltimore. Although there were numerous presentations at this session, only two really counted. Steven Koonin and Nathan Lewis, speaking for himself and Charles Barnes, all three from Caltech, executed between them a perfect slam-dunk that cast cold fusion right out of the arena of mainstream science.

At a press conference on March 23, 1989 at the University of Utah, R. Stanley Pons (left) and Martin Fleischmann announced that they had created nuclear fusion at room temperature in the flask they display here. When other scientists got through debunking what was supposed to be the discovery of the century, cold fusion looked effectively dead.

The chemists had beaten the physicists, little science had beaten big science, cleverness had prevailed over brute force, two humble professors from Utah had won out over the aristocrats of bicoastal, non-Mormon America.

Before I go any further in telling this tale, I think I'd better come clean about my own prejudices. (Those of us concerned about the issue of conflicts of interest in academic life refer to this as "disclosure." It's supposed to help protect us from sin.) The Caltech protagonists, Steve Koonin, Nate Lewis, and Charlie Barnes, are not only my faculty colleagues, I count them all among my personal friends of many years. On the other hand, there is a player on the other side of this game who is also one of my oldest personal friends, and who is, besides, my longtime scientific collaborator. His story is one that, because it took place outside the United States, was largely off the radar screen of our journalists and popular authors. Nevertheless, the story is worth telling. It shows at the very least that the frenzy that began in Utah was not an isolated or unique phenomenon.

My friend, Professor Francesco (Franco) Scaramuzzi, is the head of a small low-temperature physics research group at a national laboratory in Frascati (a suburb of Rome), Italy, run by an agency called ENEA, roughly analogous to our Department of Energy. It is possible within this agency for a scientist like my friend Franco to be promoted to the rank of *Dirigente* (executive). The promotion would not change in any substantial way his assignment or responsibilities, but it would carry with it very substantial financial rewards and much prestige. Although Franco was certainly one of the laboratory's more distinguished scientists long before cold fusion appeared on the scene, he had not been awarded this promotion by 1989, when he was 61 years old. The reason is that, in the corrupt Italian system that has collapsed only recently, these promotions were based on political affiliation more than scientific accomplishment. For every two Christian Democrats promoted, there would also be a new Socialist, a Communist, and someone from one of the smaller parties among the ranks of the *Dirigenti*. Franco had not been promoted because he refused to join a political party in order to advance his professional career as a scientist. Franco is, in other words, a man of unflinching integrity.

On the morning of April 18, 1989, Franco called to warn me that I would find his picture in the *New York Times* the next day (I did). He had just come out of a press conference announcing the discovery of a new kind of cold fusion.

Like scientists everywhere, he had heard of the Utah announcement and decided to give it a try. He reasoned that electrolysis wasn't really necessary. It served only to get deuterium (the hydrogen isotope in heavy water) to insert itself into the atomic lattice of the palladium electrode. He also thought it necessary that the system not be in thermodynamic equilibrium. He and his handful of young scientists and technicians arranged to put some titanium shavings in a cell pressurized with deuterium gas (titanium is both cheaper and easier to get hold of than palladium, and, like palladium, it is a metal that absorbs large quantities of hydrogen or deuterium into its atomic crystal lattice). Then they used some liquid nitrogen (a refrigerant readily available in any low-temperature physics laboratory) to run the temperature of the cell up and down, thus

Meanwhile, back in Italy, physicists at the ENEA lab in Franscati continued experiments on *Fusione Fredda*. Here, two days after the April press conference announcing their observations created a national furor, members of the lab stay busy. Prof. Franco Scaramuzzi checks data at right; from left (foreground) are Prof. Marcello Martone, Dr. Salvatore Podda, Dr. Antonella De Ninno; (in back) Giuseppe Lollobattista, and Lorenzo Martinis.

creating thermodynamic disequilibrium. The crude apparatus was not suitable for the difficult measurement needed to tell whether any heat was being generated, but fusion should produce neutrons (that is what Steven Jones had claimed to detect at BYU). They got a colleague at the Frascati lab to set up a neutron detector near their apparatus. In the course of their experiments, they often detected nothing at all, but on a couple of occasions, their detector indicated very substantial bursts of neutrons.

When the second positive result was discovered on April 17, Franco decided he had to inform the head of his laboratory. In no time at all, he found himself in downtown Rome, talking about it to the head of the entire national agency.

The agency ENEA had been without funding for four months. The necessary legislation was stalled in Parliament. ENEA was borrowing money from banks to meet its payroll. All purchases were frozen. Research was paralyzed. To the politically astute agency head, Scaramuzzi's discovery was an opportunity not to be missed. Franco agreed to a press conference, but only if he could give a full technical seminar to his scientific peers first. The seminar, hastily organized for that same day, was crammed to the rafters with scientists from every laboratory in the Rome area, and was even covered by the evening television news programs. At the press conference the next morning, Franco, stunned to find himself flanked by two ministers of state, did his best to behave with the utmost scientific objectivity and reserve, but it made not the slightest bit of difference. The story made headlines all over Italy. Within days, Parliament had approved financing for ENEA and Franco had been promoted to *Dirigente*. The agency was solvent once more, and Franco's personal salary had increased overnight from one that would be meager for an American postdoc to one that would be generous for an American full professor.

He had also become the Italian Prometheus, stealing fire from the sun. My very reserved, correct, self-effacing friend was a media celebrity, suddenly the most famous scientist in Italy. When I came to visit just a few months later, in the summer of 1989, he handed me two books, each two or three inches thick, of photocopies of his press notices in Italy and abroad. Although it happened far offstage for most Americans, what happened in Italy had mirrored in many important ways the feeding frenzy in the United States.

For one thing, pecuniary motives had driven science out of the laboratory into the blinding glare of publicity. For another, the story instantly captured the public fancy. Not only were the gallant scientists about to rescue us from the grip of the greedy oil barons (the whole affair took place just shortly after the *Exxon Valdez* incident), the story was spiced with lots of delicious ironies. In America, mere chemists, spending money out of their own pockets, seemed to have succeeded where arrogant physicists spending hundreds of millions of dollars of public funds had conspicuously failed: they had produced controlled nuclear fusion. The chemists had beaten the physicists, little science had beaten big science, cleverness had prevailed over brute force, two humble professors from Utah had won out over the aristocrats of bicoastal, non-Mormon America. (True, the two Utah professors, Pons and Jones, were bitter rivals. Jones, the only Mormon of the bunch, was a physicist, not a chemist, and Pons's partner Fleischmann was not only an Englishman, but an Fellow of the Royal Society. These were mere footnotes, however.) Much the same was true in Italy. The dire straits of ENEA drove the story out of the lab and into the headlines. Not only had cold fusion been reproduced in Italy, the Italian version was of an entirely new kind: *Fusione Fredda*, or Cold Fusion Italian Style, was "dry fusion," that is, without electrolysis. True, Scaramuzzi was also a physicist, not a chemist, but he did small, clever, low-budget science in the Frascati lab, which is better known for its hot fusion and synchrotron-type big science. Suddenly, Italy had more to give the world than sunshine and pasta. An Italian scientific hero strode the world stage (or so it seemed from inside Italy).

The failure of cold fusion was due, above all, to the fact that it was an experiment whose result was contrary to prevailing theory.

The cold fusion story seemed to stand science on its head, not only because it was played out in the popular press without the ritual of peer-review, but also because both sides of the debate violated what are generally supposed to be the central canons of scientific logic. Science in the 20th century has been much influenced by the ideas of the Austrian philosopher Karl Popper. Popper argued that a scientific idea can never be proven true, because no matter how many observations seem to agree with it, it may still be wrong. On the other hand, a single contrary experiment can prove a theory forever false. Therefore, science advances only by demonstrating that theories are false, so that they must be replaced by better ones. The proponents of cold fusion took exactly the opposite view: many experiments, including their own, failed to yield the expected results. These were irrelevant, they argued, incompetently done, or lacking some crucial (perhaps unknown) ingredient needed to make the thing work. Instead, all positive results, the appearance of excess heat, or a few neutrons, proved the phenomenon was real. This anti-Popperian flavor of cold fusion played no small role in its downfall, since seasoned experimentalists like Lewis and Barnes refused to believe what they couldn't reproduce in their own laboratories. To them, negative results still mattered.

On the other hand, the anti-cold-fusion crowd was equally guilty, if you believe another of the solemn canons: it is said in all the high school textbooks that science must be firmly rooted in experiment or observation, unladen with theoretical preconceptions. On the contrary, however, the failure of cold fusion was due, above all, to the fact that it was an experiment whose result was contrary to prevailing theory.

All parties agreed that, if cold fusion occurred in the experiments of Pons and Fleischmann, Jones, Scaramuzzi, and many others, the primary event would have to have been the fusion of the two deuterium nuclei: deuterium nuclei repel one another because of the electric force between them, but if they get close enough together they fuse anyway because of what is called the "strong" (nuclear) force. The laws of quantum mechanics allow deuterium nuclei to fuse by accident every so often even if they are not initially close together, but the probability of that happening is very small. Suppose, for example, they are as far apart as the two deuterium nuclei normally are in a deuterium molecule. Then the probability of fusion is much too small to have produced the alleged effects claimed by the cold-fusioners. There are two ways to look at just how small the probability is. At the internuclear spacing in the deuterium molecule, the probability is too small by 40 or 50 orders of magnitude. Physicists love to throw around phrases like that one. An order of magnitude means a factor of 10. Too small by 40 or 50 orders of magnitude really means too small beyond discussion, beyond imagination, almost beyond meaning. On the other hand, that probability is insanely sensitive to how far apart the nuclei are to begin with. To increase the probability by the requisite 40 or 50 orders of magnitude requires getting the nuclei closer together by just one order of magnitude. It is extremely difficult to imagine how—given the well-known forces involved—they can be gotten closer together by a factor of 10 in an experiment on a tabletop. In fact, the whole purpose of the hundreds of millions of dollars spent on hot fusion is to produce exactly that result. Nevertheless, once we have been anesthetized by talking about 40 or 50 orders of magnitude, the idea that a one order of magnitude gap might somehow be overcome is not so hard to swallow.

Still the theoretical difficulties of cold fusion don't end with getting the nuclei somehow to fuse. When two deuterium nuclei fuse, they momentarily form the nucleus of the common isotope of helium, called helium-4. When that happens, however, there is so much excess energy in the reaction that the helium-4 almost always breaks up immediately into two smaller pieces. About half of the time, a neutron pops out, leaving a helium-3 nucleus. The other half of the time, a proton comes off, leaving a hydrogen-3, also known as tritium, nucleus. It also happens

The debate continued. Here at a meeting of the Electrochemical Society on May 8, 1989 in Los Angeles, Fleishmann responds heatedly to a statement by Caltech electrochemist Nate Lewis (out of picture). Pons, at right, might well be wishing he were elsewhere.

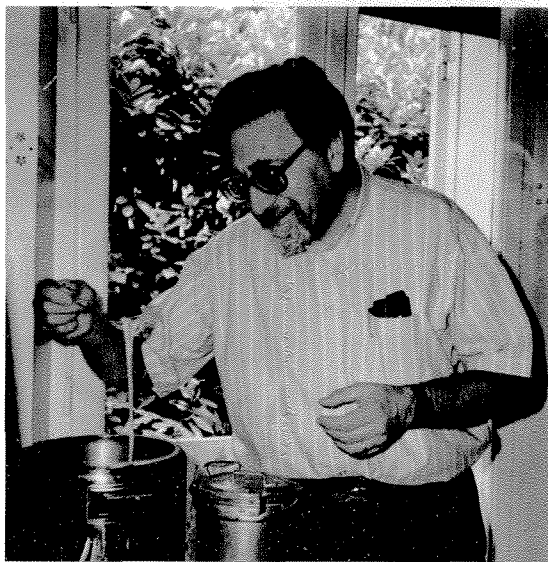
"If it were true, they'd both be dead."

that, one time in a million, the helium-4 doesn't break up at all. Instead, an intact helium-4 nucleus goes zooming off, while emitting a powerful gamma-ray photon. In all cases, the two pieces go off in opposite directions with lots of energy.

What you expect, then, is that about half the fusions will produce energetic neutrons, and the other half will leave behind tritium as evidence they occurred. In fact, as we have already seen, neutrons were detected by Jones, Scaramuzzi, and others, and offered as evidence for cold fusion, but there were always far too few of them to account for the amount of heat being claimed by Pons and Fleischmann (the heat would presumably be the end-product of the energy carried away by the nuclear fragments of the various reactions that could take place). In fact, on the evening of the original Pons and Fleischmann press conference, I ran into one of my buddies at Caltech, a battle-scarred veteran of experimental nuclear physics. "What do you think?" I asked (there was no need to be more specific). "It's bullshit," he said, slipping immediately into technical jargon. "If it were true, they'd both be dead." What he meant was that if enough fusions had taken place to produce the amount of heat claimed by Pons and Fleischmann, the flux of neutrons that resulted would have long since been enough to send them both to the happy hunting grounds.

To believe that Pons and Fleischmann, Jones and Scaramuzzi, and many others who claimed to observe either heat or neutrons or tritium were all observing the same phenomenon, one must believe that, when fusion occurs inside a piece of metal, such as palladium or titanium, the outcome is radically different from what is known to happen when fusion occurs in the sun, or in a hot fusion plasma, or an atomic bomb, or a nuclear accelerator. In other words, it is different from conventional nuclear physics. Let's call the three possible outcomes of fusion *a*, *b*, and *c*. We'll call *a* the one that emits neutrons, *b* the one that leaves tritium behind, and *c* the one where the helium-4 stays intact. In conventional nuclear physics, fusion results about half the time in *a*, half the time in *b*, and one millionth of the time in *c*. To account for the observations reported, with some consistency, by various researchers in cold fusion, fusion inside a metal would nearly always result in reaction *c* (without, however, emitting a gamma ray). One in every hundred-thousand or so reactions would result in *b*, and the probability of a reaction *a* would be smaller by yet another factor of a hundred thousand. These are the conditions needed to explain why cold fusion cells can generate power at the rate of watts, for periods of days or months, while, far short of killing Pons and Fleischmann, still yielding barely detectable traces of neutrons, and only tiny amounts of tritium.

Is it plausible that the nuclear reaction might be altered radically when it takes place among the atoms in a metal, rather than in a rarefied atmosphere? The answer, quite simply, is no. For one thing, the atomic nucleus is so small compared to the distances between atoms in a metal that for all practical purposes, the nucleus is *always* in a near vacuum. For another thing, events occur so quickly in the nuclear fusion



What does Italy's most famous scientist do in the privacy of his own home? Scaramuzzi cooks pasta.

In spite of all that, scientists are aware that they must be prepared, from time to time, to be surprised by a phenomenon they previously thought to be impossible.

reaction that the metal is simply unable to respond. If you like orders of magnitude, the fastest anything can happen in a metallic crystal is nine orders of magnitude slower than the typical time in which the nucleus created by fusing deuterium plays out its drama of fusion and breakup. In other words, when the nucleus is doing its thing, the atoms of the crystal are far away and frozen in time. Finally, the energy released in the nuclear reaction is so large that the crystal has no means to absorb it, unless it is spread out instantaneously, over vast distances, by some mechanism not now known (presumably, the same mechanism would have to account for why no gamma ray is emitted). In short, according to everything we know about the behavior of matter and nuclei, cold fusion is impossible. This is what I meant when I said that cold fusion is an experiment whose result is contrary to prevailing theory.

In spite of all that, scientists are aware that they must be prepared, from time to time, to be surprised by a phenomenon they previously thought to be impossible. There are two recent examples that seem relevant to the cold fusion problem. One is high temperature superconductivity, and the other is the Mössbauer effect.

In 1986, two Swiss physicists, J. Georg Bednorz and A. Karl Mueller, announced the discovery of a material that remained superconducting at temperatures as high as 30 kelvins. Superconductivity is itself a phenomenon that violates the trained intuition of physicists: at sufficiently low temperature, many metals can conduct electricity without any resistance at all, while simultaneously expelling completely any applied magnetic field. This behavior is so bizarre that it took nearly half a century after its discovery, in 1911, before an acceptable theoretical explanation was formulated. However, if nature was going to play such weird tricks on us, at least these tricks were confined to the privacy of the physics laboratory by the requirement of extreme low temperature. Before Bednorz and Mueller, it was well known that superconductivity could never exist at a temperature higher than 35 kelvins. After Bednorz and Mueller, it was only a matter of months before materials were discovered that remained superconducting up to 100 kelvins. That's still pretty cold—normal room temperature is about 300 kelvins—but the shocking impact of that discovery on the scientific community is hard to overestimate. The discovery of high temperature superconductivity in 1986 set the stage for the announcement—and at least temporary acceptance of the possibility—of cold fusion in 1989.

The Mössbauer effect, discovered 30 years earlier, was another completely unexpected phenomenon that seemed to have an even more direct bearing on cold fusion. As we've already seen, cold fusion is hard to swallow in part because it is so implausible to believe that a nuclear reaction might be altered in any meaningful way by taking place in a crystal. Yet the Mössbauer effect was an example in which precisely that does seem to occur.

When a nucleus has too much energy, it must find some means to get rid of the excess. For example, we've already seen that when two deuteriums fuse, the resulting nucleus, which has far too much energy, can actually break up in any of three ways. In all three cases, however, the result is two fragments that fly off in opposite directions. Mössbauer's discovery was that, in certain cases when a nucleus in a crystal gives up its excess energy by emitting a gamma-ray photon, instead of the photon going one way and the nucleus the other way as would normally be expected, there is a substantial probability that the photon will fly off and the nucleus will stand still. Instead of the nucleus recoiling (just as a rifle does when it fires a bullet) the recoil is taken up by the entire crystal, resulting in essentially no motion at all. The net result is that the gamma-ray photon emitted by a nucleus in a crystal can have slightly more energy than the gamma-ray photon the same nucleus would have emitted in a vacuum. Our carefully trained intuition—which says that nuclei are unaffected by being in a crystal because they exist in entirely separate realms of distance, time, and energy—

has been violated. If our intuition can be violated by the Mössbauer effect, then why not by cold fusion?

That's a good question, and there are very good answers. First, the Mössbauer effect can be observed only for a few special nuclear reactions in which the energy that must be disposed of is much smaller, and the time the nucleus takes to get rid of it much larger, than in the cold fusion reaction. In other words, it occurs precisely in those special cases where our argument that the nucleus and the crystal act on incompatible scales of time and energy no longer holds true. Second, even then, the Mössbauer effect does not change the intimate details of the nuclear reaction, such as the emission (or not) of a gamma-ray photon, or the probabilities of the various possible ways of giving up its excess energy. It is precisely these details that must be changed if cold fusion is real. Finally, the Mössbauer effect is in a sense the exact opposite of what is supposed to happen in cold fusion: instead of the nuclear recoil energy somehow turning into heat in the atomic lattice, the Mössbauer effect is interesting precisely because it's the special case in which no heat at all is produced.

Nevertheless, in spite of all the differences, many scientists instantly thought of the Mössbauer effect when they first heard of cold fusion. The discovery of the Mössbauer effect had been unexpected, but, once it happened, it was quickly and satisfactorily explained within the framework of conventional theory. It proved that there are still genuine surprises waiting for us that, once understood, don't violate conventional physical laws. And it also proved that there is at least some realm in which nuclear physics and solid state physics affect one another. Those are just the things you have to be willing to believe in order to be prepared to accept cold fusion, at least provisionally.

In any case, immediately after the press conference in Utah, most scientists were willing at least to suspend judgment for a while, to give cold fusion a chance. It was precisely during this crucial probationary period (so to speak) that cold fusion science went berserk. Many scientists tried their own hand at it. Those who succeeded, or seemed to succeed, held press conferences. Those who failed generally quietly let the matter drop and went on to other things. It would be difficult to devise a worse way of doing science. Among the exceptions to that behavior were Lewis, Barnes, and Koonin, of Caltech. They pursued every lead with relentless tenacity and Popperian rigor, repeating every experiment, calculating every effect, looking not merely for positive or negative results, but also for explanations of the false positive results that others were reporting—in other words, finding the mistakes of other scientists. These they found in abundance. Far from publicizing their work, they were so secretive that rumors started to circulate, and even appeared in the press, that they were protecting positive results. [For an in-depth account of Caltech's "Quest for Fusion," see *E&S* Summer 1989.] Finally, they were able, five weeks after the Utah press conference, to stand before their colleagues in Baltimore and, piece by piece, in vivid detail, demolish the case for cold fusion. Cold fusion had been given its chance, a suspension of disbelief no matter how unlikely it seemed, and it had failed to prove itself. Cold fusion was dead in the eyes of respectable science.

Cold fusion had been given its chance, a suspension of disbelief no matter how unlikely it seemed, and it had failed to prove itself.

Meanwhile, back in Frascati, Franco Scaramuzzi and his group of young researchers were not quite prepared to give up. Just as the drama in Italy was little noticed in America, events in Baltimore seem far away when you are in Rome. Franco himself had had, not just 15 minutes of fame, but a month of it, and it showed no signs of letting up. He was a hero, not only to the general public, but also to all his colleagues in the agency ENEA, and ENEA itself had suddenly shed its reputation for bumbling bureaucratic ineptitude. This was not a propitious moment to throw in his hand just because Lewis, Barnes, and Koonin didn't approve.

Besides, he had his own data, and he believed in them. Nothing convinces a scientist nearly as effectively as the experience of seeing data emerge from one's own experiment. In this case there

Left: Steve Koonin, Caltech professor of theoretical physics, delivers the *coup de grace* to cold fusion at a Baltimore meeting of the American Physical Association on May 1—a mere five weeks after discovery of the phenomenon was first announced.

Below left: Physicist Steven Jones from Brigham Young University, here presenting his data at Columbia University on March 31, 1989, claimed to detect not heat but neutrons, as did Scaramuzzi.

were, to be sure, many questions. It turns out that neutrons are not so easy to detect. The instruments used to detect them are sometimes tricky and undependable. In the aftermath of the Frascati announcements, experts from Italy and abroad (especially the United States) made brief visits to Scaramuzzi's lab and pronounced their verdicts on how the mistake had been made: the apparent bursts of neutrons were really artifacts due to changes in temperature, or humidity, or power surges on the (notoriously unstable) Frascati lab electric system, or other electronic problems. I remember during my visit that summer talking to one of Franco's young colleagues, Antonella De Ninno. "Do they think we're stupid?" she asked me angrily. "Of course we thought of all those possibilities and eliminated them!" Once the group was convinced they had seen the real thing, they weren't about to give up because someone had made a speech in Baltimore.

There was also a bit of wriggle-room available. At the Baltimore meeting, Pons and Fleischmann did not attend, but Jones did. He was the first speaker. He pointed out just how small was the effect he claimed to see compared to what Pons and Fleischmann were claiming (as we have seen, the number of neutrons that come out appears to be smaller than expected by about 10 orders of magnitude). Thus it seemed possible that even if cold fusion didn't produce heat (the Pons-Fleischmann claim) maybe something was going on at a much lower level, producing a few neutrons (as Jones and Scaramuzzi, among others, claimed). Of course, Barnes at Caltech had shown there were no neutrons just as effectively as Lewis had shown there was no heat (and Koonin had shown there was no theory), and furthermore, if cold fusion merely produced a few neutrons instead of a lot of heat it certainly wasn't going to solve the world's energy problems. Nevertheless, it seemed at the time that there just might be two kinds of cold fusion, the bad kind (heat) that Koonin and Lewis had put to rest, and the good kind (neutrons) that was still scientifically respectable. The Italian press made much of the fact that "Italian Cold Fusion" was of the good kind, not noticing that the good kind of cold fusion, if it existed, would be a scientific curiosity, not an epochal discovery.

In any case, after the furor died down, cold fusion research continued in a number of places. The key to continued research is financial; to paraphrase California politician Jesse Unruh, money is the mother's milk of scientific research. In the United States, the government funding agencies quickly fell into line with scientific orthodoxy and ceased funding anything that smacked of cold fusion. However, the industry-supported Electric Power Research Institute decided to put up some funds, just in case. In Japan, Toyota and MITI, apparently willing to accept some short-term risk in exchange for the possibility of a big payoff later, agreed to put up a few yen. In Italy, ENEA, with its budget and prestige resting on cold fusion, could hardly refuse to permit Scaramuzzi and his group to press on. In other places, where scientists were given modest financial support and some discretion in how to spend it, some chose to pursue cold fusion. In spite of the disapproval of the

In the continuing Italian experiments, Fabrizio Marini (left) and Dr. Antonio Frattolillo check the vacuum system for the high-resolution mass spectrometer (hanging off the top), used to detect helium-4, the end product of cold fusion.

worldwide scientific establishment, some cold fusion research kept right on going.

Scaramuzzi and his colleagues did not devote all of their attention to cold fusion. At the same time all this was going on, they also developed the world's best device for firing frozen pellets of solid deuterium into the plasma used to create hot fusion. If hot fusion were ever to produce useful energy, this is the means by which the reactor's deuterium fuel would be replenished. They were also responsible for the sophisticated cooling device that rendered it possible to make observations of infrared cosmic radiation in outer space, using relatively inexpensive long-range balloon flights instead of satellites to rise above most of the earth's atmosphere. In both of these tasks, they were doing successful high technology in the very center of the scientific mainstream.

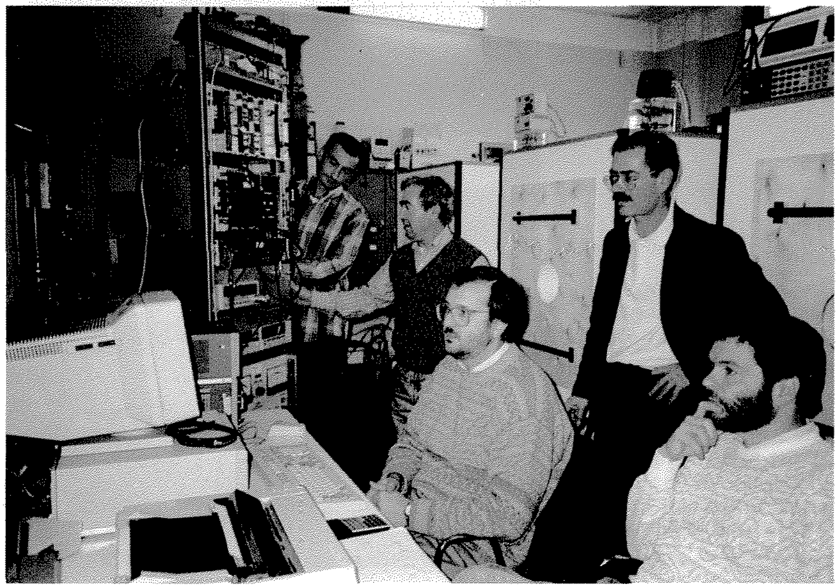
There was no dependable recipe for coaxing bursts of neutrons out of the cold fusion cell. As long as that was true the world of respectable science was not going to pay any attention even to the "good kind" of cold fusion.

But they also continued to pursue cold fusion. Reacting to criticism of the primitive technique they had used to detect neutrons, they purchased the best neutron-detection system in the world, essentially identical to the one used by Charlie Barnes at Caltech. Going one better, they installed it in physics laboratories that had been excavated under a mountain called the Gran Sasso, a two-hour drive from Rome. Anywhere on the surface of the earth, there are always some neutrons buzzing around due to cosmic radiation from outer space. This so-called "background" has to be subtracted from the neutrons produced by any other phenomenon such as cold fusion. In the galleries under the Gran Sasso, the shielding effect of the mountain reduces the cosmic-ray neutron background nearly to zero. That's why the laboratory was built there. An automated system was set up to monitor the neutron counter while running the temperature of a Scaramuzzi-type deuterium gas cell up and down. Every week or so, a member of the group would have to drive out to the Gran Sasso lab, check out the counters, replenish the supply of liquid nitrogen, and bring back the data. No one could accuse them any longer of being unsophisticated about neutron work. This experiment, however, like their own earlier work and many other experiments blossoming around the world, produced positive results, but only sporadically. There was no dependable recipe for coaxing bursts of neutrons out of the cold fusion cell. As long as that was true the world of respectable science was not going to pay any attention even to the "good kind" of cold fusion.

Then they decided to pursue the "bad kind" as well. They built a well-designed electrolysis cell, capable of detecting excess heat if any were produced, while obviating some of the shortcomings for which previous excess-heat experiments had been criticized. In 1992 and 1993, these experiments, too, gave positive results. The cell would produce very substantial amounts of heat (a few watts) for periods of tens of hours at a time. As in the neutron experiments, these episodes were sporadic, occurring seemingly at random, but at least they occurred only when the fluid in the cell was heavy water (containing deuterium), never when it was light water (containing ordinary hydrogen). The lack of this kind of control experiment had been one of the points of criticism of Pons and Fleischmann. By this time, however, the world of mainstream science was no longer listening.

I went to visit Franco in December 1993, when he returned from the Maui conference. While I was there, he summarized the results of the conference in a seminar presented to the Physics Faculty at the University of Rome ("*La Sapienza*," the first university of Rome; now there are two more). This was in itself an unusual event. The Physics Faculty of the University of Rome today is comparable to the physics department at a good American state university. For them, inviting Franco to speak about cold fusion was a daring excursion to the fringes of science. Feeling that this was a rare opportunity, Franco prepared his talk with meticulous care.

At the seminar, Franco's demeanor was subdued, and his presentation was, as always, reserved and correct. Nevertheless, his message was an optimistic one for cold fusion. In essence (although Franco didn't say it in these words), each of the criticisms that Nate Lewis had cor-



Waiting for fusion. (From left) Fabrizio Marini, Giuseppe Lollobattista, Dr. Aurelio La Barbera, Domenico Lecci, and Dr. Luciano Bertalot, look for heat (the "bad kind" of cold fusion) from measurements in progress in the calorimeters (inside the white boxes behind them).

Even more important, there was reason to believe that the magic missing factor, the secret ingredient of the recipe that accounted for why cold fusion experiments only sporadically gave positive results, might finally have been discovered.

rectly leveled at the experiments of Pons and Fleischmann had been successfully countered by new experiments reported at the conference. Even more important, there was reason to believe that the magic missing factor, the secret ingredient of the recipe that accounted for why cold fusion experiments only sporadically gave positive results, might finally have been discovered.

One of the criticisms that Nate had used with telling effect is that local hot-spots often develop in electrolysis experiments (Nate is himself an electrochemist, and a consummate experimentalist). By placing their thermometer at an accidental hot spot, and by neglecting the elementary precaution of stirring the bath in their cells, Pons and Fleischmann could easily have fooled themselves into thinking there was excess heat where none really existed. To counter this argument, Franco could point to the design of the cell used by his own Frascati group, which carefully averaged the temperature of the entire cell, rather than measuring it at a single point (many other groups had introduced mechanical stirrers into their cells). Another objection that had been raised was that, if heat was generated in these experiments, it was the result of some uninteresting chemical process rather than of nuclear fusion. Chemical processes that generate heat are not uncommon in electrolysis experiments. The strongest argument for nuclear fusion (given the near absence of the neutrons and tritium) was that the amount of heat generated was far too large to be due to any chemical process. That would be true, the critics replied, if the chemicals were being generated at the same time as the heat. However, all of these cold fusion cells had long, dormant periods during which energy was being pumped in and no excess heat was being produced. The heat finally liberated in the "cold fusion" episodes might just have been chemical energy stored up during the dormant periods. In other words, the cells were not producing more energy than was being put into them; they were just storing up energy and releasing it in bursts. Not only would that be much less exciting than a discovery of controlled nuclear fusion, it also wouldn't be of much help in our struggle against the oil barons. Now this argument could be countered as well: there were what appeared to be very careful experiments in which the total amount of energy consumed during the dormant periods was minuscule compared to the amount of heat liberated during the active periods.

Finally, one of the most damaging criticisms of Pons and Fleischmann was that they had failed to do control experiments. Nuclear fusion (if it occurred) should only have been possible (if it were possible) when electrolysis was done in heavy water, made of deuterium. It should not be possible using ordinary water, made of ordinary hydrogen. Now many groups, including Franco's, had done the necessary control experiments, and obtained the necessary confirming results (no heat in the controls). Unfortunately, other groups reported that they *did* observe excess heat in experiments done with ordinary light water. Franco dutifully reported these results at the Rome seminar, expressing only muted disapproval ("In my opinion, these results have not been consolidated," he said).

All of this was much less important than the fact that cold fusion experiments, if they gave positive results at all, gave them only sporadically and unpredictably. When Bednorz and Mueller announced the discovery of high temperature superconductivity in 1986, no one carped about control experiments, because, once the recipe was known, any competent scientist could make a sample and test it and it would work immediately. If, at their press conference, Pons and Fleischmann had given a dependable recipe for producing excess heat, they very likely would be Nobel Prize winners now (as Bednorz and Mueller are) rather than social outcasts from the community of scientists. The essential key to the return of cold fusion to scientific respectability is to find the missing ingredient that would make the recipe work every time.

If cold fusion ever gains back the scientific respectability that was squandered in March and April of 1989, it will be the result of a long, difficult battle that has barely begun.

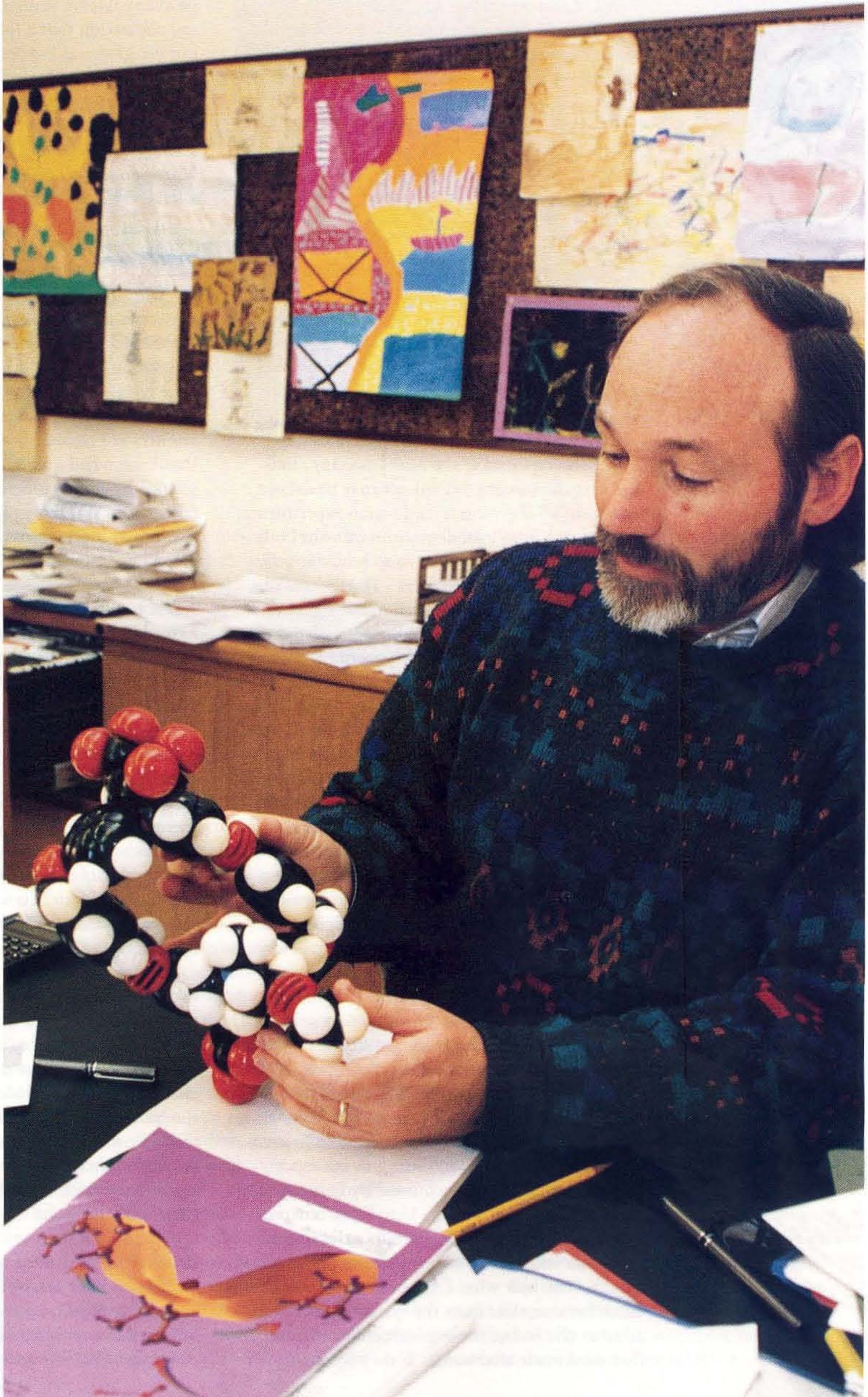
Experiments done in the United States and Japan, and reported at the Maui meeting, indicate that the missing ingredient may have been found. In all the various cold fusion experiments, the first step is to load deuterium into the body of metallic palladium. The issue is how much deuterium gets into the metal. The ratio of the number of atoms of deuterium in the metal to the number of atoms of palladium is called x. It turns out, by means of electrolysis, or by putting the metal in deuterium gas, that it is rather easy to get x up to the range of about 0.6 or 0.7. That is already a startlingly high figure. If there are almost as many deuterium atoms as palladium atoms in the material, the density of deuterium (a form of hydrogen) is essentially equal to that of liquid hydrogen rocket fuel, which can ordinarily exist only at extreme low temperatures. In other words, palladium (and certain other metals including titanium) soak up almost unbelievable amounts of hydrogen or deuterium if given the chance. This is far from a new discovery. However, according to the experiments reported at Maui, $x = 0.6$ or 0.7 is not enough to produce cold fusion. Both American and Japanese groups showed data indicating there is a sharp threshold at $x = 0.85$. Below that value (which can only be reached with great difficulty and under favorable circumstances) excess heat is never observed. But, once x gets above that value, excess heat is essentially always observed, according to the reports presented at Maui and recounted by Franco Scaramuzzi in his seminar at the University of Rome.

The audience at Rome, certainly the senior professors who were present, listened politely, but they did not hear what Franco was saying (that much became clear from the questions that were asked at the end of the seminar, and comments that were made afterward). If they went away with any lasting impression at all, it was just the sad realization that a fine scientist like Franco had not yet given up his obsession with cold fusion. They cannot be blamed. Any other audience of mainstream scientists would have reacted exactly the same way. If cold fusion ever gains back the scientific respectability that was squandered in March and April of 1989, it will be the result of a long, difficult battle that has barely begun.

Recently, I told this story in a philosophy course we teach at Caltech called "Ethics of Research." The first question, when I finished my tale, was, do I believe in cold fusion? The answer is no. Certainly, I believe quite firmly the theoretical arguments that say cold fusion is impossible. On the other hand, however, I believe equally firmly in the integrity and competence of Franco Scaramuzzi and his group of co-workers at Frascati. I was disturbed when I saw that Franco had gotten caught in the web of science-by-news-conference in April 1989 (although I was truly pleased that he finally got the long overdue recognition his agency ENEA owed him), and I was even more distressed when I learned that Franco and his group had observed excess heat (the "bad kind" of cold fusion). I have looked at their cells, however, and looked at their data, and it's all pretty impressive. The Japanese experiment showing that heat nearly always results when x is greater than 0.85 looks even more impressive on paper. It seems a particularly elegant, well-designed experiment, at least to the untutored eye of a physicist. (What do I know about electrochemistry?) What all these experiments really need is critical examination by accomplished rivals intent on proving them wrong. That is part of the normal functioning of science. Unfortunately, in this area, science is not functioning normally. There is nobody out there listening.

I suppose that, if nuclear fusion really does take place whenever x is greater than 0.85 in palladium, the world of conventional science will eventually be forced to take notice. If not, then the whole story I have told you is nothing but a curious footnote to a bizarre and ugly episode in the history of science. Either way, I think the story illuminates the inner dynamics of the scientific enterprise in a way that few other stories have done. For that reason alone, it may be worth telling. □

Although not an inside player in the cold fusion game, David Goodstein, vice provost and professor of physics and applied physics, is nevertheless a tireless and articulate observer of the scientific scene, an occupation from which E&S *has often benefited.*



Sing a Song of Benzene, A Pocket Full of π

Herein lies a paradox: Organic chemicals and oily guck are synonymous, as anyone who's ever taken an organic lab knows, yet water is the solvent of life.

by Douglas L. Smith

It is the very model of a major neurotransmitter. Professor of Chemistry Dennis Dougherty demonstrates how a molecule of acetylcholine, in his left hand, slips neatly into a molecule he designed. Acetylcholine is a common neurotransmitter—a chemical messenger that nerve cells use to communicate—and its correct fit into a receptor molecule is crucial to the transmission of its message.

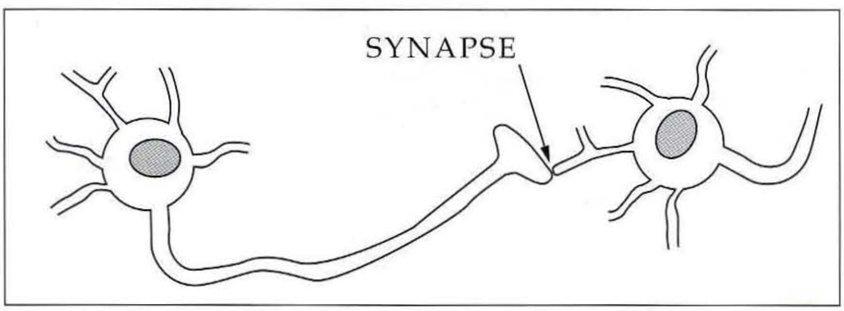
Greasy hair went out in the seventies, and greasy cooking in the eighties, but greasy amino acids are very much in vogue in the nineties, thanks to Caltech research showing that these gucky, oily residues are not just molecular filler, but a vital part of the protein in which they live.

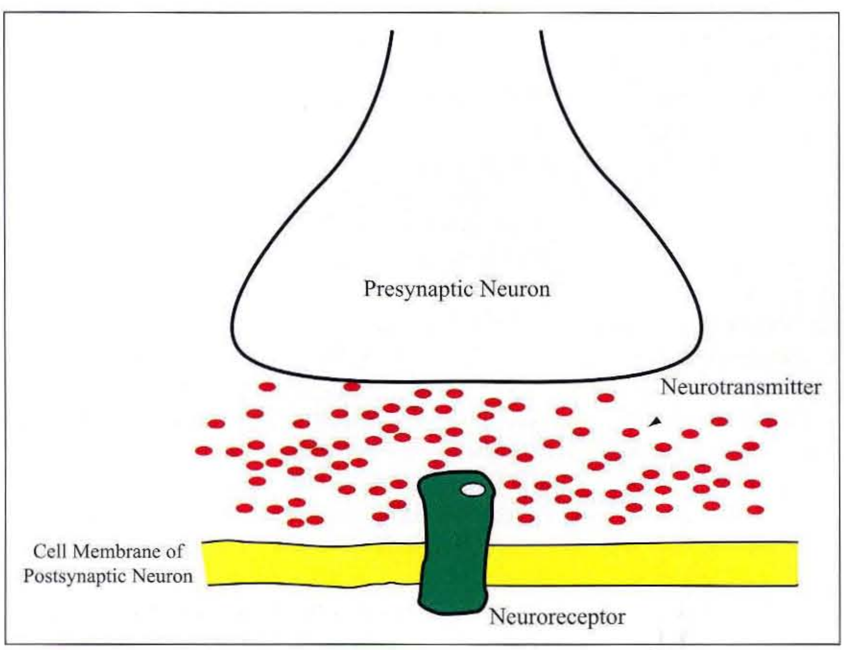
Proteins, of course, are the molecular machines that actually do the work of the cell. A protein is a chain of hundreds of small building blocks, called amino acids, strung together in a specific order that differs for every protein. The amino acids have various functions endowed by their physical and chemical properties. Some functions are structural—making a hairpin turn that folds the protein back on itself, or creating a sheetlike surface that might form the docking site for another molecule. Other amino acids actually *do* things—they have side chains that can participate in chemical reactions. Others form links between the protein strands, and hold everything in proper alignment. But, says Professor of Chemistry Dennis Dougherty, "the aromatic, or benzene-containing, amino acids (phenylalanine, tyrosine, and tryptophan) were primarily considered to just be greasy organic stuff—hydrophobic, repelled by water—and that was it." Now these molecular underachievers stand revealed as movers and shakers—the middlemen in communications between nerve cells.

Nerve cells make very few direct electrical connections with each other; instead they rely on a chemical process called molecular recognition. They secrete chemical messengers, called neurotransmitters, that leap across the gaps between cells. A message is delivered when the neurotransmitter finds its receptor—a protein embedded in the surface of the receiving cell—and binds to it, causing a change in the receptor that triggers an electrical response within the cellular addressee. It's a wonderfully flexible system—there are about 50 known neurotransmitters, and any one cell can have receptors for several of them, each of which conveys a different message. A signaling system based on molecular recognition needs two things. First, the receptor must be selective enough to pluck out its messenger—it needs a pocket that fits the neurotransmitter just right. Second, the pocket's bond must be the molecular equivalent of Velcro—strong enough to hold the neurotransmitter, yet weak enough to let go quickly once the message is delivered. The greasy amino acids, aptly enough, appear to do part of the dirty work—they're in the part of the protein that actually recognizes the messenger.

The late Linus Pauling (PhD '25) won the Nobel Prize in chemistry in 1954 for figuring out the nature of the chemical bond, but the chemistry of biology today is increasingly the chemistry of these Velcro bonds. They are much weaker than chemical bonds, and form and come undone without affecting the underlying chemical structures. Says Dougherty, "Pauling also recognized that these weak interactions were going to be the key to biology, because biological systems are dynamic—they're not etched in stone, locked in place; things are flexible. And the molecules are large, so biological systems can amass a very large number of weak interactions to produce a strong effect. The amazing chemistry of life, in the end, often involves a lot of very weak interactions

Below: Neurons, or nerve cells, don't generally make direct electrical connections with one another, but are separated by a gap called a synapse. Bottom: An outbound nerve impulse triggers the release of neurotransmitter molecules into the gap. The molecules jump across the synapse in a millionth of a second and bind to neuroreceptor molecules that protrude from the surface of the receiving cell.



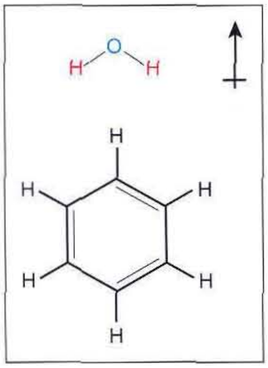


working in concert." (The receptor for a 25-atom neurotransmitter may contain some 30,000 atoms, for example. If the messenger were the size of your hand, the receptor would stand five feet tall, and you wouldn't be able to get your arms around its girth.)

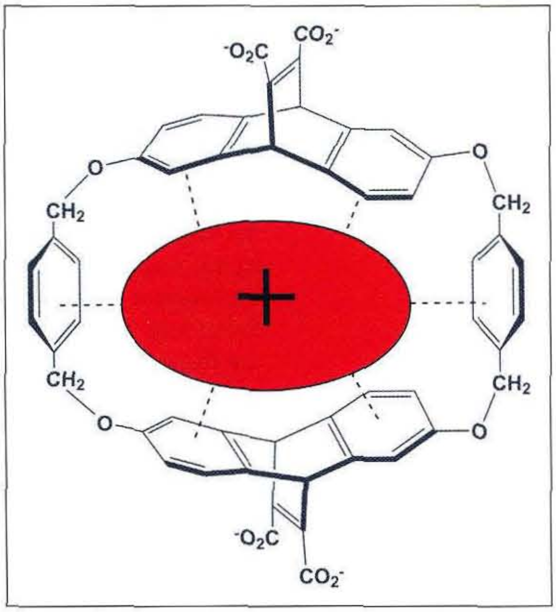
The best known Velcro bond is the hydrogen bond, not to be confused with the ordinary covalent chemical bond that hydrogen usually forms. In fact, a hydrogen atom can't form a hydrogen bond to something unless that hydrogen atom has already entered into a covalent bond with some other atom—usually oxygen—first. Covalent bonds are based on the principle of share and share alike, with each atom contributing one electron to the binding pair. But the oxygen atom likes electrons a lot more than the hydrogen atom does—a phenomenon called electronegativity, which Pauling also elucidated—and greedily draws the hydrogen atom's electron toward itself. The hydrogen atom acquires a slight positive charge; the oxygen atom an equal negative one. This charge distribution is called a dipole. Opposites attract, so the positive end of one dipole will seek out and snuggle up to the negative end of another one. This dipole attraction between a hydrogen atom and a negative charge elsewhere is the hydrogen bond. (Nitrogen, and, to a lesser extent, sulfur, affect hydrogen the same way, although they aren't quite as electronegative as oxygen.)

And herein lies a paradox: Organic chemicals and oily guck are synonymous, as anyone who's ever taken an organic lab knows, yet water is the solvent of life. Cells are about 70 percent water. Much of the common coinage of the cell—neurotransmitters, metabolic intermediates, regulatory molecules, and even pharmaceutical visitors from the outside world—carry positive charges in their biologically active forms. And water molecules have a huge dipole, with their two hydrogen atoms perched atop their oxygen atom like Mickey Mouse ears. The negative, or oxygen-atom, ends of these dipoles get right in there and nuzzle up to the positive charges on the active molecules as the water molecules cluster around them, dissolving them and making them available to the cell. So proteins make themselves soluble by wadding up in a way that exposes their dipolar, hydrogen-bond forming amino acids—handles for the water molecules to grab onto, so that they can drag the otherwise water-repellent proteins into solution.

Three of the water-averse amino acids are made slick by benzene rings within their structure. Like dissolves like, and benzene—perhaps best known to the layperson as a nasty carcino-

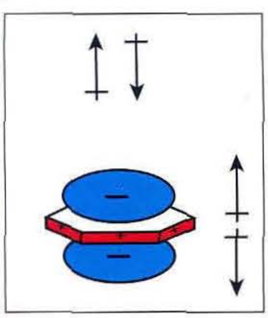


Above: A water molecule (top) has an asymmetric charge distribution, as shown by its red (for positive) and blue (for negative) atoms. This creates a dipole, symbolized as an arrow whose head points toward the negative charge and whose tail is a + sign. Benzene (bottom) is perfectly symmetrical and has no dipole.



Right: Dougherty's model system has six benzene rings that form Velcro bonds (dotted lines) to positive ions.

Below: A quadrupole is two dipoles pointing in opposite directions. They usually line up head to tail, as in the upper drawing, so that there's no net charge palpable to the outside world. But in benzene, they line up back to back and present substantial regions of negative charge, as shown at the bottom.



gen—is an organic chemist's best friend, because it dissolves all those greasy things that water won't. Benzene is oil to water's water—they don't mix. Unlike water, benzene has no electronegative atoms, and its molecule is perfectly symmetrical. It's a flat, hexagonal thing that looks like a Susan B. Anthony dollar—remember them?—but with fewer edges. "Benzene doesn't have a charge, like a cation [positive ion] does," says Dougherty, "and it doesn't even have a dipole. It avoids water, and so it tends to be buried in the *interiors* of proteins"—and thus was slighted as filler.

This is not to say that grease doesn't have its place. The cell membrane is a double layer of fatty molecules that separates the water outside the cell from the water within. Thus proteins spanning the cell membrane—and there are a lot of them—obviously need fat-soluble regions in order to reside in that neighborhood. Other suety organic molecules play vital roles in the cell, too; they bind to one another weakly through nonpolar interactions.

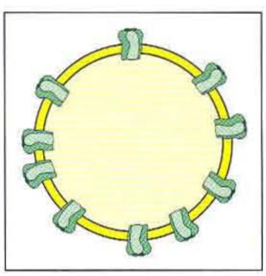
Low-level interactions—polar and otherwise—pose problems for theorists, says Dougherty. "The quantum mechanics of bonding that Linus and others worked on aren't easily applicable to these weak interactions. It's *much* more difficult to describe them rigorously. Proteins are gigantic, complicated molecules, so we organic chemists design and build model systems—smaller, more manageable systems that we hope exhibit the same basic physical properties, and that can be studied much more rigorously."

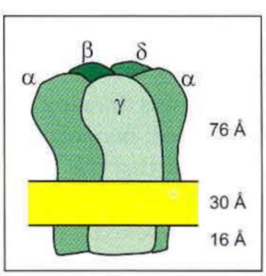
Dougherty's model system is a doughnut-shaped molecule—the "host"—whose interior is lined with the flat faces of six benzene rings. Actually, the molecule looks more like a sandwich-sliced Kaiser roll, with each half of the roll containing two benzenes linked edge-on. Two spacers, each containing another benzene ring, keep the halves of the roll a set distance from each other, thereby defining a slot into which slips the sandwich filling—a small "guest" molecule. Sprinkled like sesame seeds around the Kaiser roll's crust are carboxylate groups—negatively charged ions (anions) that make the entire sandwich, including the hydrophobic benzenes, water-soluble. The researchers use nuclear magnetic resonance, or NMR, a common analytical technique, to see what happens next. When a guest molecule enters the host's cavity, the NMR signal suddenly changes in a manner that allows the strength of the interaction—those Velcro bonds—to be calculated.

"Initially, we—Tim Shepodd, Mike Petti [both PhD '88], and I— emphasized neutral organic molecules as potential guests," Dougherty recalls. "Like others, we saw that the strength of the interaction with our host was directly proportional to how *in*soluble the guest was in water. That is, guests were going into our host cavity not because they liked the host, but because they were so unhappy in water." But mere solvent repulsion is not the same as molecular recognition, so Dougherty "decided to emphasize guests that were still organic, but had considerable water-solubility—structures that really would make a choice between our host and an environment (the water) in which they were not entirely unhappy. If this kind of guest chose to go into the host, that would signal a true attraction between host and guest (versus repulsion between guest and solvent) and that would be molecular recognition. The way to make an organic molecule water-soluble is to add charge, and there is a greater variety of structures for organic cations than for anions." Although Dougherty was looking for weak interactions between nonpolar molecules, "we ended up seeing, to our surprise, that the benzene rings kept binding cations."

But how could this be? What was drawing these ions to the ultimate uncharged, nonpolar, unwaterlike molecule? It turns out that the organic chemists, and thus the molecular biologists and the biochemists, had overlooked something that the physical chemists had known all along, but which wasn't of great relevance to them—benzene has a quadrupole. A quadrupole is not *one* dipole but *two*, arranged so that they point in opposite directions; as a result, there's no net dipole. But just as a dipole has a more com-

Top: The acetylcholine receptor (green) pierces the cell membrane (yellow), forming a channel into the cell through which ions can flow. Bottom: The receptor is actually made of five closely associated protein strands, called subunits, each of which has its own gene and is assembled independently before coming together to form the receptor. There are two identical α subunits, and one each of three others (β, γ, and δ), per receptor. The measurements at right are in Ångstroms, or ten-billionths of a meter. The acetylcholine molecule itself, in comparison, is about 10 Ångstroms long.





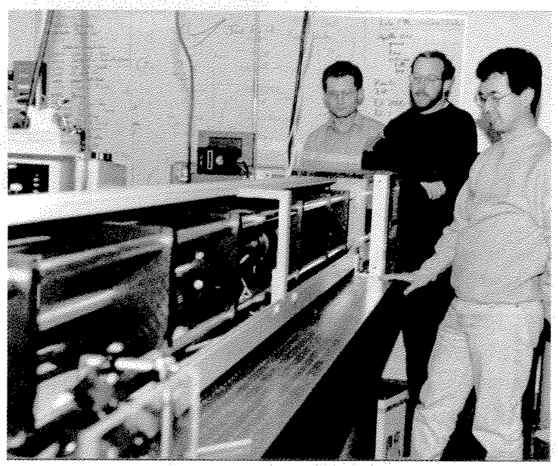
plex electric field than does a single charge, a quadrupole has a more complex field than a dipole. In benzene's case, a set of six electrons called π-electrons live above and below the plane of the hexagon, zipping around it like midget racers on a dirt track. This swirl of electrons creates a substantial negative charge on the hexagon's two flat faces, balanced by an equal-magnitude (but more diffuse) band of positive charge along the milled edge of the coin, as it were. And there you are—opposites attract, so it should be no surprise that the cations came a-flocking in what Dougherty has christened the cation-π interaction. Quadrupoles are not terribly exotic—electrical engineers and physicists use them in all sorts of ways, from focusing beams of charged particles to describing Jupiter's magnetic field.

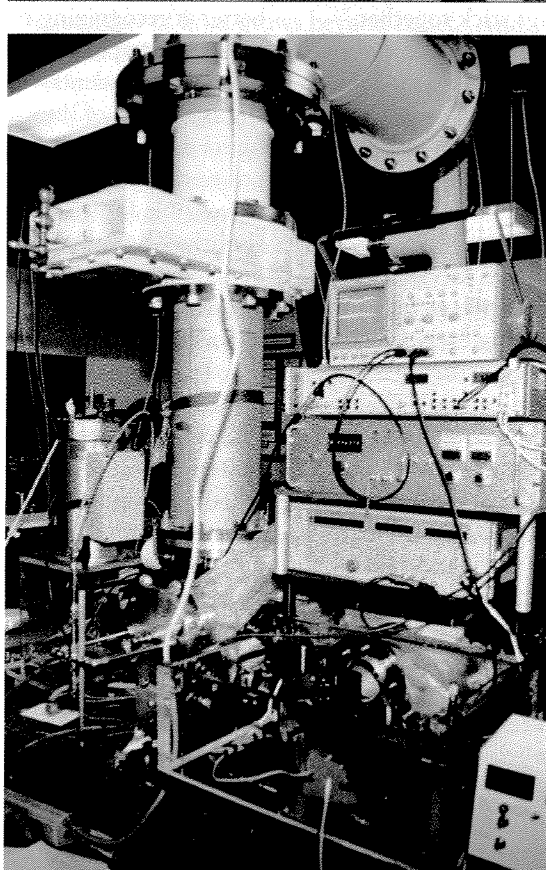
"The novel thing about benzene is that it is simultaneously hydrophobic yet polar. Most organic chemists think of that as a contradiction, but it's a fact. Fundamentally, everybody knew that benzene had a quadrupole; and everybody knew that in principle it could do something—the surprise was that the interaction was much bigger than any of us had anticipated. *So* big, that the quadrupole of benzene is able to compete with the dipole of water to bind cations. They will actually leave water, where they tend to be very happy, and race into an otherwise hydrophobic environment. This is totally backward from the way things normally operate." The cation-π interaction is roughly one-fifth the strength of a covalent bond and about five times stronger than a hydrogen bond—a middle ground that, happily, is just the right degree of stickiness to grab hold and let go readily at room temperature. And this is why benzene is ideal for forming Velcro bonds.

Further experiments showed that the cations the model system bound especially well looked a lot like acetylcholine, the first to be discovered of those 50-odd neurotransmitters mentioned earlier. The acetylcholine receptor is a ring of five large protein strands, bunched like a fistful of cigars, that penetrates the cell membrane. When the receptor binds to an acetylcholine molecule, a shiver runs down the length of the proteins, causing them to shift their bulk slightly away from each other and opening a channel into the cell's interior. A torrent of ions—an electric current—courses through the channel, galvanizing the cell into action. "So," recalls Dougherty, "we asked ourselves, 'Does nature bind acetylcholine the same way we do in our model?'" Dougherty posed this question to Henry Lester, professor of biology, and to Norman Davidson, the Chandler Professor of Chemical Biology, Emeritus, who've been jointly studying the acetylcholine receptor for many years.

This isn't an easy question to answer. Back in the early 1980s, Davidson's group had helped to find the sequence in which the protein's amino acids are strung together. But the biologically active protein bears as much resemblance to that sequence as a tangle of Christmas lights fished out of the bottom of the decoration box does to the lights when strung along the eaves. The usual way to figure out a protein's structure is to purify a sample of the protein, dissolve it in something from which it will slowly crystallize out, and determine the three-dimensional structure of the crystal by bombarding it with X-rays and analyzing how they're scattered. But most proteins that span the cell membrane have so far defeated attempts at crystallization. Separated from the membrane's embrace, the proteins lose their all-important shape, and the resulting crystal structure is meaningless. And left in the membrane, the doggone proteins just won't crystallize, because the membrane glop prevents the molecules from stacking neatly. "There are thousands of these incredibly important proteins," says Dougherty. "These are the molecules of thought. This *is* the brain, at the molecular level, and we don't have structures of them."

Chemical intuition comes to the fore in such situations, and Dougherty's told him that benzene's oleaginous mien might mask a clean-cut pillar of the molecular community. So David Stauffer (PhD '89) looked up all the known amino acid sequences of acetylcholine receptors, and discovered few of the anions one would normally expect to stick to cations, but scads of benzene rings. "So we went public with our prediction that acetylcholine binding sites would be rich in benzene rings. That was in 1990. And in 1991, the structure of the first acetylcholine-binding protein was solved. And to make a long story short, that structure validated our prediction. Spectacularly, in fact—14 benzene rings all over the place. And it's absolutely clear that the binding is due to benzene rings." (This molecule was actually an enzyme called acetylcholinesterase, which binds to used acetylcholine molecules and breaks them down into choline and acetic acid. The enzyme's business end drifts in the watery intercellular medium and is anchored to the cell—or, in some cases, the gel that fills the synapse—by a long, fat-soluble tail. The people who solved the enzyme's structure cut the anchor line and recrystallized only the water-soluble portion.)

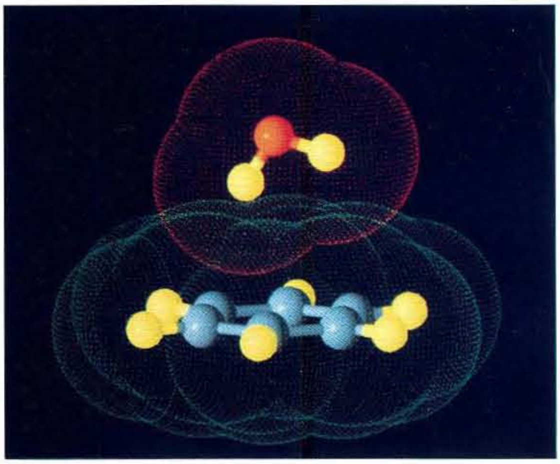




Top: (From left) Rodham, Blake, and Suzuki contemplate how much more room they'll have in their lab next year, once the carbon-dioxide laser before them is replaced by the shoebox-sized model. The CO_2 laser pumps a far-infrared laser (not shown).
Bottom: Since far-infrared lasers aren't tunable, the group makes light of the wavelength they need by mixing the laser beam with a microwave signal of adjustable wavelength. To prevent airborne moisture from absorbing the infrared light, the mixing apparatus and all of the infrared optical components live in the Plexiglas box. The bank of electronics above it is the microwave generator. The column to the left that resembles battleship plumbing is the exhaust system for the vacuum chamber, which is the horizontal cylinder to the left of the mixer.

Meanwhile, Linus Pauling turned 90. Since he was only a decade younger than the Institute itself, Caltech seized the occasion—February 1991—to throw him a birthday bash as part of the Centennial celebration. Among the speakers who gave papers on current work in fields Pauling had tilled over his long career was Nobel Laureate Max Perutz, who spoke on the significance of the hydrogen bond in physiology. In the audience that day was Associate Professor of Cosmochemistry Geoffrey Blake (PhD '86), who had been studying how clusters consisting of two or three small molecules form hydrogen bonds with one another. Such clusters are simple models for the water- and methanol-rich ices present in the interstellar medium and outer solar system—cosmic dust bunnies that slowly accrete into stars, comets, planets, and what have you. Recalls Blake, "Perutz gave a talk saying how unusual these benzene interactions were. And we went off and looked in the literature and almost no work on mixed benzene clusters had been done. There had been a lot of work on snowballs of pure benzene, but that was it. We were absolutely shocked! It's hard to think of a more important set of clusters to look at."

His interest piqued, Blake and grad students Sakae Suzuki and David Rodham and postdoc Peter Green (now a senior scientist in Caltech's Bank of America Environmental Analysis Center) started looking at clusters consisting of one benzene molecule and a molecule of either water or ammonia. The experimental method was quite simple—spray a benzene-water mist, carried by an inert gas, into a vacuum chamber and shine a laser through the cloud to look for spectroscopic



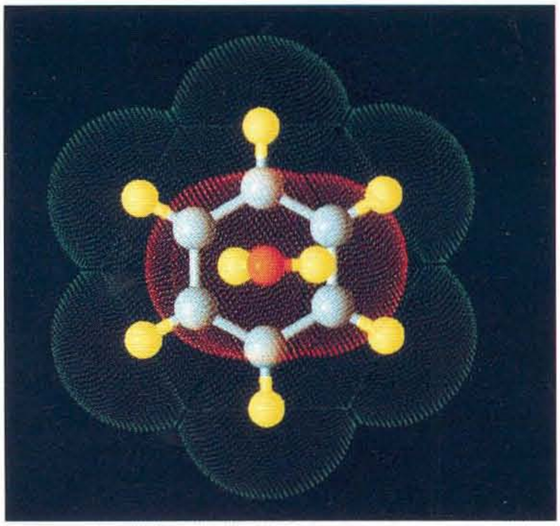
The water molecule spins freely around the hydrogen bond like a figure skater doing an arabesque—spinning on one hydrogen with the other one sticking straight out behind.

evidence of hydrogen bonding. Since all the molecules in the mist are traveling in the same direction at essentially the same speed, and in the molecular world speed equals temperature, "they think they're cold," Blake explains. "Their relative velocities are characterized by temperatures of only a few degrees Kelvin. So the collisions are very soft, and that's why things stick together." If the collisions had any more oomph, the molecules would rebound too hard for hydrogen bonds to form.

The Blake group had their first data in hand by April and, in collaboration with a theorist whose specialty is computer simulations, William Goddard III (PhD '65), the Ferkel Professor of Chemistry and Applied Physics, sat down to interpret the results. The art of spectroscopic interpretation consists of assigning every line in your spectra to a specific physical action by atoms in your sample—a certain bond bending or stretching, for example, or one part of the cluster rotating with respect to the rest of it. Blake's group would tell Siddharth Dasgupta, a member of the Beckman Institute in Goddard's group, what they thought the cluster's structure was. Dasgupta would then determine whether that structure was energetically favorable, predict exactly where all the atoms should be, and ascertain how hard it would be to rotate the water or pull it out of alignment with respect to the benzene. Armed with that knowledge, the cosmochemists would figure out where the spectral lines should fall. Then everyone would twiddle with the hypothetical structure—jinking the water molecule about, cocking its spin axis various ways, twirling it at different speeds—to try to make the lines generated by the proposed structure match the lines in the real spectra. An exact match indicated that the hypothesis accurately depicted the real molecules. "In theory," says Blake, "there are ways to do it with computers, but in practice the human mind is much better at recognizing incomplete patterns and making extrapolations, so the students and I—mostly the students—have spent long hours staring at lists of lines and plots and trying to figure out what the assignments are." A line's position depends on the masses of the atoms responsible for it, so varying one atom's mass slightly—by substituting deuterium for hydrogen, for example—causes its lines to shift, making them stand out against the fixed background of the other atoms' unmoving lines. So by repeating the experiment over and over again with minor variations in the masses of the atoms, Blake's group refined the calculations, predicting more precisely where the peaks should be, allowing the group to take better data, and so on.

Every line contains vital information because a hydrogen bond, unlike a covalent bond, is a dynamic beast even at low temperatures. It doesn't show up directly as spectral lines, so its presence must be deduced from a detailed analysis of the lines you *do* see. For example, the microwave frequencies tell how the cluster is tumbling, and seeing that the water molecule and the benzene molecule are spinning around a common axis could be a sign that a hydrogen bond between the two runs along that axis. This is the best place to start interpreting the spec-

In a discovery that gives a whole new meaning to the term "water ballet," it turns out that a water molecule (red) can pirouette gracefully atop a benzene molecule (green), as shown on the opposite page. The dot patterns represent the atoms' surfaces, while the ball-and-stick models within show the positions of the atomic nuclei. The water molecule is actually centered over the benzene ring, as shown at right. One hydrogen atom thrusts down into the center of the ring's π-electron cloud. The other lines up with the thickest part of the cloud, which lies directly over the carbon atoms.



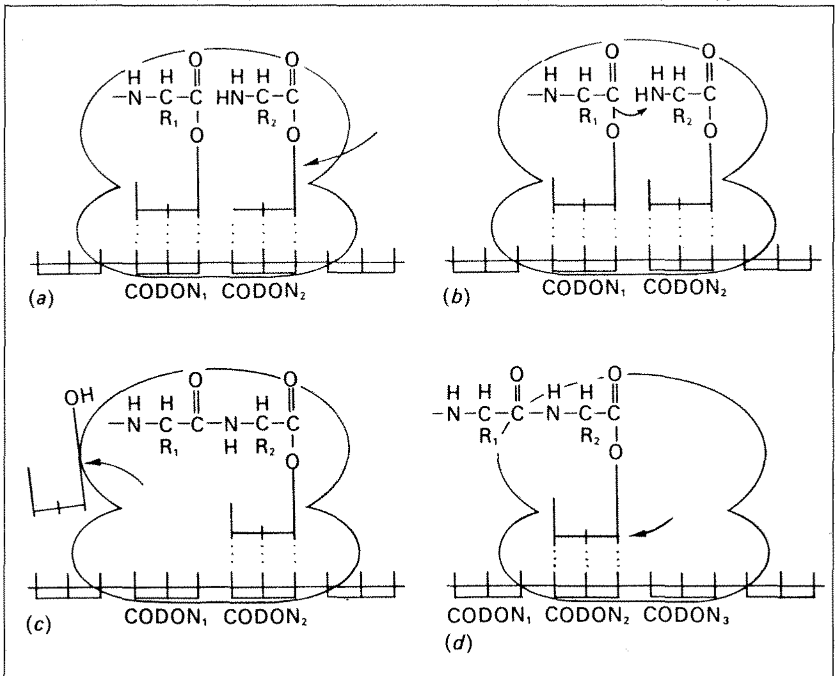
trum, says Blake, because "that's a positive, definite number—you can't have negative rotational frequencies. And you know roughly how heavy the molecule is, so you know within 10 percent or better where something's going to show up." Things get a lot hairier in the far infrared, where the vibrational frequencies lurk. The number of lines grows beyond belief. And worse, each line's precise location can wander greatly—the vibrational motions are strongly coupled to one another, like pendulums tied together by a spring, so that what happens to one vibrational mode affects the spectra of others.

"In the end," says Blake, "when we really assign a spectrum, all the lines fit to within a part in 10^6. There's no uncertainty. And that's the attractive thing about this kind of spectroscopy compared to, for example, protein studies, where you can have a few tenths of an Ångstrom's slop in the electron diffraction. But it also means that you'd better know something about what you expect to see going in. What saves these experiments is that no matter where you look, you see something. The challenge is to figure out exactly what it is."

What it was—proof positive that the flat face of a benzene molecule readily makes a strong hydrogen bond with a water molecule hovering over that face—made the cover of *Science* on August 14, 1992. (The water molecule spins freely around the hydrogen bond like a figure skater doing an arabesque—spinning on one hydrogen with the other one sticking straight out behind.) A companion paper with similar results for benzene and ammonia followed in *Nature* in early 1993. Both papers included a description of the deepest valley in what chemists call a potential-energy surface—a multi-dimensional description of the strength of the interaction between the molecules, depending on their separation and relative orientation. The lowest point on the potential-energy surface—the configuration in which the system has the least potential energy—is equivalent to the strongest interaction.

The group is now climbing out of that valley and exploring the hills around it. Says Blake, "We'd like to find out more details—for example how much energy does it cost to stretch that bond or to twist the water or the ammonia—and that requires moving up in energy." When a protein kinks up into its active shape, it will do its best to minimize its potential energy by squirming around like a restless traveler in an airplane seat until it's most comfortable, but there may be no way to bring two amino acids that want to form a hydrogen bond into the orientation corresponding to the deepest valley. Thus it's important to know what other valleys may be found at higher energies. This information will ultimately be rendered mathematically, in collaboration with the computational chemists, as force fields describing how amino acids attract or repel one another. The idea is that eventually one will be able to type the amino-acid sequence of a protein into a computer, and the computer will use the force fields to pull the protein into its natural shape.

Making the clusters is easy, compared to making laser light of the right wavelength. "That's the technical area where things are really changing," says Blake. "The experiment itself now lives on a five-by-twelve-foot optical table. We think there are some new techniques that will make it fit in a shoe box." And the big lasers have limited operating ranges, so you have to keep changing lasers as you scan across the spectrum. But the shoe box model will contain a single tunable laser. Just punch in a frequency, and—bingo!—there you are. This part of the project has brought Blake into collaboration with a lot of laser and detector gurus—Assistant Professor of Physics Jonas Zmuidzinas (BS '81), Associate Professor of Applied Physics Kerry Vahala (BS '80, MS '81, PhD '85), Associate Professor of Astrophysics Kenneth Libbrecht (BS '80), and Professor of Physics Jeff Kimble. "That's the thing about Caltech," remarks Blake. "It's small enough so that you get to meet people like Kerry and Jeff, whereas at a bigger university you might not." The mixers—which make the desired wavelength of light by combining two photons of other wavelengths—are being built



The protein-manufacturing process. The mRNA runs across the bottoms of the figures; the ribosome is the blob sitting on it.
(a) A tRNA-amino acid complex enters stage right, recognizes codon 2, and forms hydrogen bonds to it. This places the incoming amino acid next to the protein's growing end, which is Velcroed to the mRNA at codon 1.
(b) The chemical bond attaching the protein's terminal amino acid to its tRNA transmutes into a new chemical bond that attaches the new amino acid to the protein.
(c) The tRNA that formerly bound the growing protein to the mRNA departs in search of another amino acid.
(d) The ribosome creeps down the mRNA one codon, setting up codon 3 as the next recognition site.

at MIT's Lincoln Labs by Elliott Brown (MS '81, PhD '85, and a labmate of Blake's when they were both grad students), who will escape some Massachusetts cold by bringing them out to California in December. Blake hopes to spin this technology off into environmental studies in a few years by flying the shoe box on NASA's ER-2 spy plane or Perseus unmanned aircraft, where it could replace several instruments now used to track nitric acid, ozone, carbon monoxide, and various chlorine compounds. And the shoe box may also ride on a European Space Agency mission called FIRST (Far Infrared and Submillimeter Space Telescope), which will search the cosmos for various gases, plus those water- and methanol-containing ices that got Blake into this line of work in the first place.

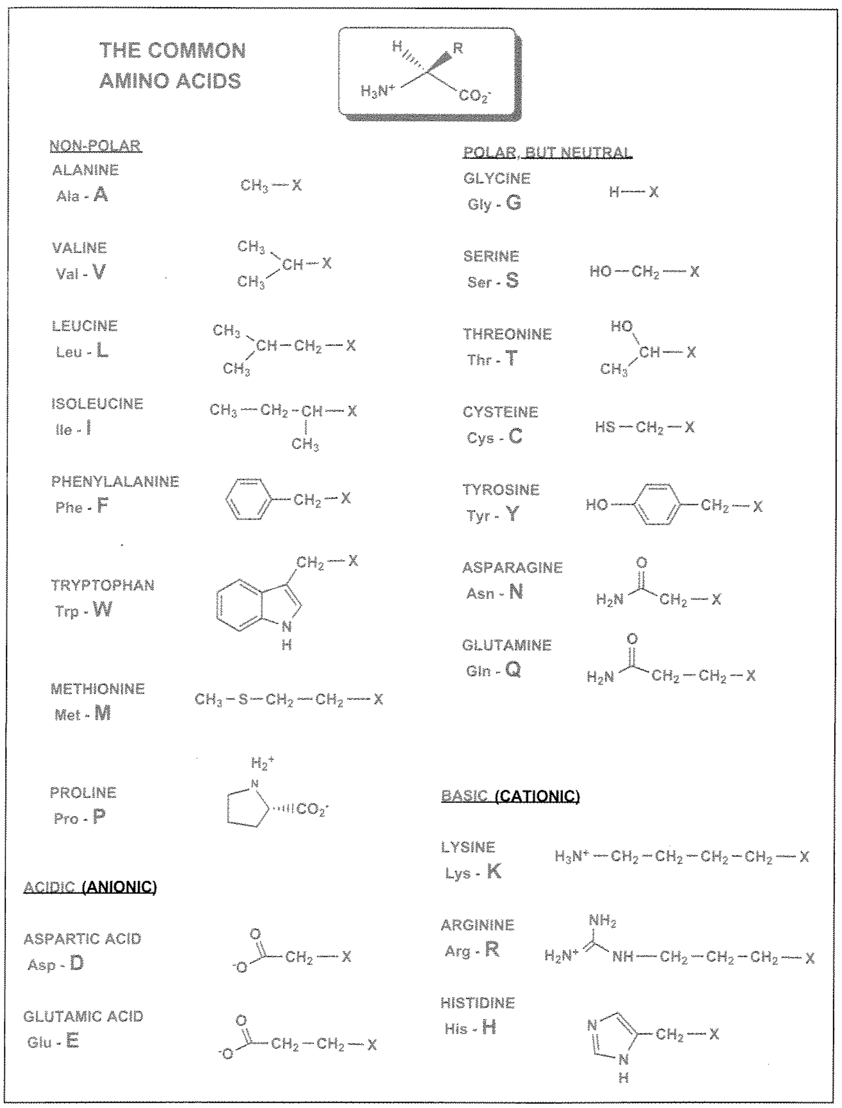
Getting back to the brain, or at least to neuroreceptors, we now have two independent lines of evidence showing that benzene can make Velcro bonds in two different ways. But having a crystal structure that puts benzene rings at the scene of the bind, and spectroscopic analyses that show an M.O.—that benzene's negatively charged face will indeed interact with even a partial positive charge—is not the same as an eyewitness account of a neurotransmitter being recognized by benzene rings in a living cell.

So Dougherty, Lester, Davidson, and John Abelson, the Beadle Professor of Biology, are hoping to become the star witnesses. Lester's group alters the gene that tells the cell how to make the protein, swapping out a benzene-containing amino acid for a different one. Then the researchers inject the modified gene into an unfertilized frog egg, which obligingly churns out the protein molecules and inserts them into the cell membrane. The group then assays the protein's function by a series of electrical measurements. If the modified protein behaves like the original one, then the change obviously wasn't important. But if the new protein behaves oddly or doesn't work at all, then the missing amino acid does something vital. And by replacing that vital amino acid with ones having a range of different properties, the researchers can sometimes infer what that something is. Dougherty notes, "You need such a broad range of disciplines for this project—organic chemistry, molecular biology, electrophysiology—that it would be very hard to do at larger places. But at Caltech, I talk to Henry, he calls John, and five minutes later we have a collaboration."

The 20 amino acids on nature's palette limit one's freedom to experiment with the structure. But in the late 1980s, Peter Schultz (BS '79, PhD '84), a professor of chemistry at UC Berkeley, figured out how to put an amino acid that nature had never designed into a protein. The trick was to suborn the molecules of tRNA that bustle about the cell looking for the right amino acids to feed to the protein-assembling machinery. The tRNAs get their instructions by "reading" a blueprint molecule called mRNA that encodes each amino acid as a sequence of three "letters" chosen from a four-letter alphabet—A (for adenine), C (cytosine), G (guanine), and U (uracil). Such a sequence is called a codon. And, as Watson and Crick discovered in DNA, G binds only with C, and A only with U, a pairing that—

The 20 naturally occurring amino acids. They share the structure in the box (symbolized by the "X" in the detailed structures), which is how they make links with their fellows—the CO_2^- of one amino acid reacts with the H_3N^+ of its neighbor. The "R" in the boxed structure represents the rest of the amino acid. Thus glycine (the top entry in the right column) is the boxed structure when "R" is replaced by a hydrogen atom. Note that, in this "traditional" classification, tyrosine is considered to be polar by virtue of its OH group, which can form hydrogen bonds.

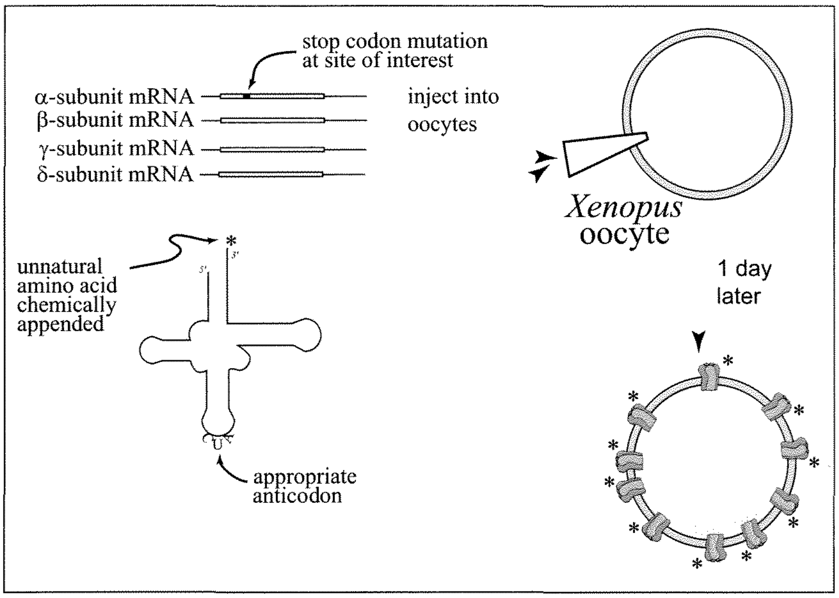
"Amber," in this case, has nothing to do with Jurassic Park. *The codon was named for a chap named Bernstein, who discovered it at Caltech some 30 years ago. "Amber," in German, is "Bernstein."*



wouldn't you know it!—involves hydrogen bonds. Each tRNA has two working ends. One end, called the anticodon, contains the three-letter sequence that recognizes the codon and pairs up with it. The other end binds only to a specific amino acid. The amino-acid-laden tRNAs and the mRNA are brought together by a marvelous device called a ribosome—part protein and part RNA—that helps rivet the amino acids together.

However, three codons—UAG, UAA, and UGA—don't have a corresponding tRNA. (Molecular geneticists of a previous generation dubbed these codons "amber," "ochre," and "opal." "Amber," in this case, has nothing to do with *Jurassic Park*. The codon was named for a chap named Bernstein, who discovered it at Caltech some 30 years ago. "Amber," in German, is "Bernstein.") When the ribosome reaches one of these codons, there's no amino acid to process, and the riveter lurches to a halt. These three sequences tell the assembly line that the protein is finished.

In 1986, Abelson and his group had learned how to change the anticodon of an otherwise normal tRNA so that it would pair with the amber codon. Then, using the standard tools of molecular biology, they could insert an amber codon into the gene at a location of their choosing, replacing the codon that normally occurred there. The amber-binding tRNA would then insert its habitual amino acid into the nascent protein at that point, and the ribosome would carry on assembling the altered protein. (But first, one has to make sure that none of the active



Below: Physics grad student Haiyun Zhang now injects frog eggs in Lester's lab—another discipline heard from!

Above: To grow a neuroreceptor, two solutions are injected into the *Xenopus* oocyte, or frog egg, simultaneously. (*Xenopus* is a genus of South African clawed frogs.) One solution contains the four mRNAs needed to make the subunits of the acetylcholine receptor. An amber codon has been introduced into the α subunit at the point where the unnatural amino acid is to be inserted. The other solution contains amber-binding tRNA molecules to which the unnatural amino acid (the purple asterisk) has been attached. (The egg already has a full complement of normal tRNAs and amino acids.) Then the cell gets to work building the receptors and dispatching them to the membrane.

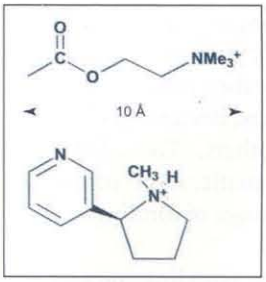
mRNAs in the cell have an amber stop codon. Any amber codons must be changed to ochre or opal in order for the protein to be assembled normally.) Schultz extended this idea by building tRNA molecules that had the amber-binding anticodon on one end, and his choice of doodad on the other. As long as the doodad was an amino acid—even if it wasn't found in nature—the ribosome would happily rivet it into the protein.

This proved to be a trickier proposition than it undoubtedly first appeared, but Schultz finally got it to work. However, it would only work in a test tube. This isn't a problem for many biologists—it might even save them the bother of having to extract the protein from the cell—but it's fatal for neurobiological work. It takes a living cell to put the protein in a lifelike pose in the membrane. Postdocs Mark Nowak and Patrick Kearney (who got his PhD in '94 from Dougherty) in Lester's group teamed up with senior research fellows Jeffrey Sampson and Margaret Saks in Abelson's group, says Dougherty, and worked "with Pete to modify the Schultz protocol in very clever ways—I can say that, because none of them were my ideas—so that we've gotten it to work in a living cell."

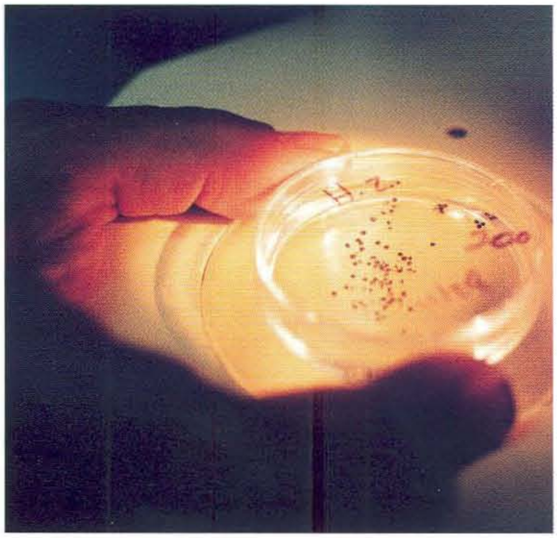
Once the protein has carpeted the cell surface, Lester's lab uses sensitive electrical instruments, including a device called a patch clamp (whose inventors, Erwin Neher and Bert Sakmann, received the Nobel Prize in 1991) to zoom in on a single channel—i.e., the receptor protein—and follow its behavior. The patch clamp is basically a glorified eyedropper—a piece of glass tubing drawn out to a blunt tip about one millionth of a meter in diameter. Fill the dropper with conducting liquid, place the point against a frog egg and apply a little gentle suction, and the cell seals against the dropper. Putting a wire in the conducting liquid creates an electrode that measures the current through the patch of cell membrane to which it's sealed. And if conditions are right, only one channel will open at a time in that piece of membranous real estate. "The patch clamp is an amazing tool," Dougherty exclaims. "You're getting a signal from one molecule in real time! Physicists get all excited when they see a signal from a single molecule, and biologists have been doing this for a decade."

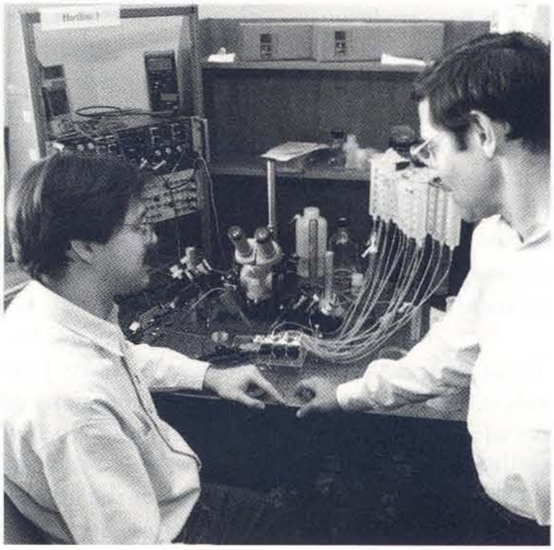
The frog eggs live in a Petri dish filled with liquid nutrients and awash with the neurotransmitter in question, which is supplied through a metering apparatus that looks like an IV drip. The receptor is put through its paces by varying the concentration of its neurotransmitter, making it open and close its channel as it binds and releases the messenger, like the machine that tests car doors by slamming them over and over and over again. The eggs are pretty durable, says Lester. "They're good for anywhere from a couple of days to a couple of weeks, depending on how carefully we handle them."

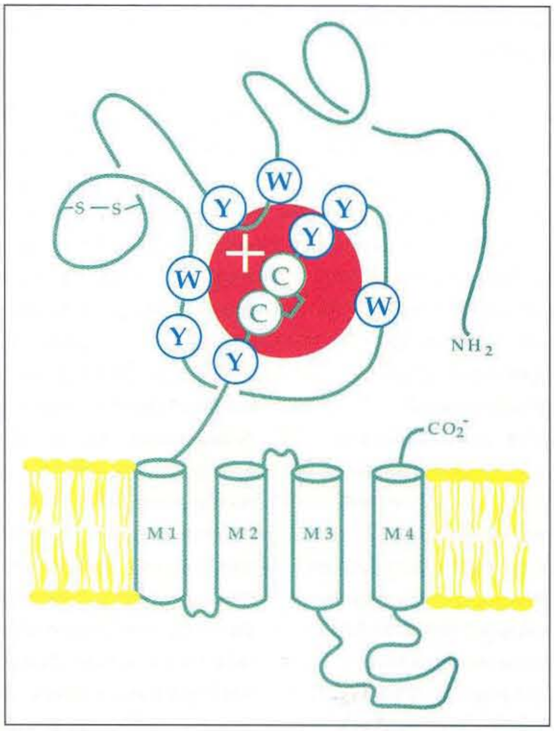
A typical experiment begins with the channel closed; the voltage through the patch clamp electrode is the baseline. Then the messenger binds to the receptor and the channel opens, changing the voltage. The electrode stays at this voltage until the receptor lets go, and the channel closes again. The trace from the electrode resembles a series of mesas on the Arizona desert floor—up



Above: Acetylcholine (top) and nicotine (bottom) may look different to us, but the receptor can't tell them apart. Both molecules are about the same size, and both have a positively-charged nitrogen atom on one end and an exposed electron pair (in either the C=O or the second nitrogen atom) on the other.
Far right: A serving of *Xenopus* caviar, injected and ready to go.
Right: Nowak (left) and Lester (right) at a patch-clamp setup. The dish full of oocytes goes under the microscope, where the patch clamp is held in place by adjustable screws. The drip lines at right deliver nutrients and neurotransmitters.





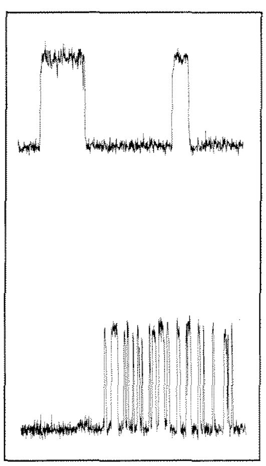


Right: The acetylcholine binding site flies above the cell membrane as part of the channel's α subunit (green). One might expect to find the anionic amino acids (aspartic acid and glutamic acid, abbreviated D and E, respectively) here. Instead, cysteine (C) is electrically neutral, and tryptophan (W) and tyrosine (Y) contain benzene rings. The cylinders labeled M1–M4 are the membrane-spanning regions that anchor the protein, and the yellow two-tailed polliwogs are the fatty molecules that make up the cell membrane.

for a while, down for a while, up again, then down, and so on. By measuring how the width and number of the mesas varies with the concentration of the neurotransmitter, one is actually measuring the strength of its attraction to the receptor.

Most of the experiments to date have been done with the nicotinic acetylcholine receptor, so called because nicotine binds to it as strongly as does its intended messenger molecule, acetylcholine. There are about half a dozen different types of nicotinic acetylcholine receptors. One type makes muscles contract, but all the rest are found in the brain. The interaction between nicotine and one or more of these receptors is the first step in nicotine addiction, says Lester, so it's hardly surprising that this molecule is of intense interest to a lot of people. The receptor's binding site includes several tyrosines, one of the benzene-containing amino acids, and it's known that these tyrosines somehow contribute to the binding. The Lester-Davidson-Dougherty-Abelson collaboration has substituted unnatural amino acids for three of those tyrosines. Each one of the three appears to make a different kind of contact with the messenger molecule, Lester explains, because when they are replaced in turn by the same set of unnatural amino acids, a variant that grips acetylcholine tightly when standing in for one tyrosine has only a weak effect, or none at all, in another tyrosine's spot. "Furthermore," he adds, "it looks as though one of these places is a good candidate for the cation-π interaction that Dougherty has been predicting." Another site appears to form a hydrogen bond.

An electrical recording from an individual acetylcholine receptor in the absence (top) and presence (bottom) of a blocker chemically related to the local anaesthetics. When the trace is up, the ion channel is open and flowing at a rate of about 10 million ions per second. In the top trace, the channel opens and closes normally. In the bottom trace, blocker molecules bind within the open channel, stopping the ions' flow. Each chasm in the trace corresponds to the binding of an individual blocker molecule, which then dislodges and the flow resumes. Each trace is about a tenth of a second long.



These concepts—and the benzene motif—apply to drug design as well. When something other than the intended messenger binds to the receptor, one of two things can happen. The drug may bind so well that it blocks the receptor permanently—spilling Super Glue on the Velcro, as it were. The channel never opens, and the trace becomes flatter than Iowa. Or the interfering molecule may fit well enough to bind, sort of, but not well enough to bind perfectly. The drug wobbles in and out of the receptor's grip, causing the channel to bounce open and closed like a screen door on a tight spring. Now the trace looks like the Badlands of South Dakota—all spires and chasms. Or, as Lester puts it, "they have a lot of flicker."

"Pharmacologists and chemists have traditionally had access to the enormous power of synthetic organic chemistry," Lester notes. "One can make a large number of organic chemicals and test how they affect the function of the neuron. So over the years, classical pharmacology has developed a large number of highly specific drugs, and also specific hypotheses about how these drugs interact with their receptors. But we have not had the structural tools to test these hypotheses."

So Lester, Davidson, and Dougherty are keen to install unnatural amino acids in many other proteins. Lester's group works with a broad spectrum of what he calls "excitability proteins"—the molecules that give a cell the ability to send and receive chemical and electrical messages. These include the neurotransmitter receptors and ion channels described above, plus the neurotransmitter transporters, which shepherd individual neurotransmitter molecules across the cell membrane rather than opening a floodgate as ion channels do. This is a very big field indeed—in fact, it's the entire grain belt. At the most fundamental level, these molecules regulate what gets into and out of nerve cells; on an intermediate level, they give the heart, the diaphragm, and every other muscle in the body their marching orders; and at the highest level, they are the molecules of thought. At every level, these molecules bind to drugs. Some are good—anti-epileptics such as Dilantin; antipsychotics, including Thorazine; the beta-blockers Atenolol and Inderol, which control high blood pressure; antidiabetics such as Glucotrol; and even the lowly "water pills," or diuretics, that go by names like Diuril and Clotride. Some aren't—cocaine, which binds to a dopamine transporter, and LSD, which does the same to a serotonin receptor, spring to mind. And some are a little of both—morphine and other opiate painkillers, including heroin; the benzodiazepines, of which Valium is the most notorious; the list goes on and on. Knowing more about how they bind is essential to improving existing drugs, designing new ones, and curbing the abuse of others. The collaboration's methods allow systematic, molecular-scale investigations of a broad range of binding phenomena.

"All of these insights into binding interactions are critical for the pharmaceutical industry," Dougherty remarks. "I've seen reports, for example, of a drug that worked OK and had an anion in it that was assumed to interact with a cation at the receptor. They then made a new drug where the anion was replaced by a benzene ring, and sure enough, it worked very well. And the cool thing about that is, you've taken an anion, which is very water-soluble, and replaced it with a benzene ring, which is very water-insoluble, so that you've massively changed the drug-distribution properties." This drug will really want to leap out of the bloodstream and into the cell's greasy membrane.

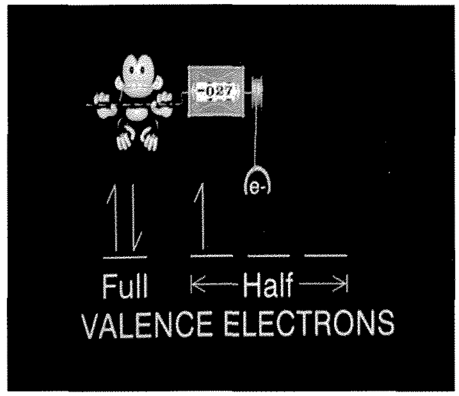
Benzene binding to cations certainly isn't the be-all and end-all of molecular recognition—there are other well-known factors at work, and probably many unknown factors as well—but the biologically active form of many, many important molecules contains a positive charge, and there's an awful lot of greasy stuff in the cell. Not just in nerve cells—molecular recognition recurs throughout the immune system, and in enzymatic processes generally. And nature is lazy—once it finds something that works, the same trick reappears over and over again in different guises in seemingly unrelated systems. (From this point of view, benzene's quadrupole providing the negative charge to which a hydrogen bond can form is simply another variation on the theme.) Dougherty waxes lyrical about the possibilities. "It has been proposed in, or documented in, a wide variety of protein structures. It's been proposed as a catalytic force in reactions that involve the creation or destruction of a positive charge. For example, Dave Stauffer and Alison McCurdy [PhD '94] have been looking at models for an extremely important class of gene-regulatory reactions that involve the transfer of a methyl group from a cofactor called SAM [S-adenosylmethionine] to DNA. SAM is a cation, and we believe cation-π interactions are involved in that mechanism. It has been proposed that the cation-π interaction is involved in cholesterol biosynthesis. The really neat thing here is that you have a way to recognize charge, but not with charge—with grease, in effect. It really opens up possibilities for molecular design." □

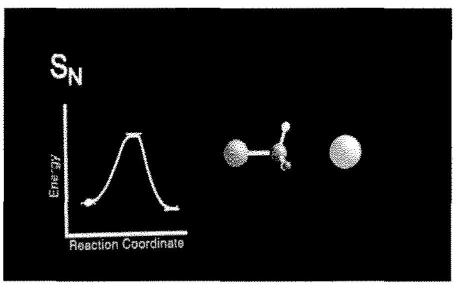
Caltech's Chemistry Animation Project, or CAP, is using computer animation to help high-school and college students visualize what can't be seen, by producing a set of videocassettes containing scientifically accurate, three-dimensional renderings of fundamental chemical concepts.

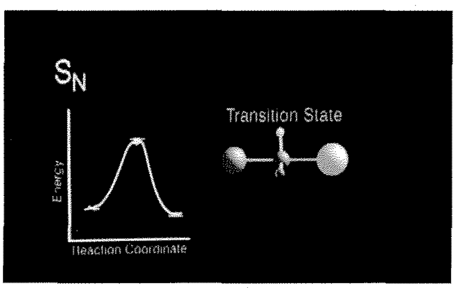
A CAPsule of Chemistry

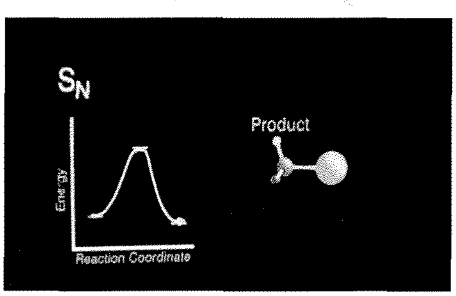
Sodium metal tossed into a pond becomes a dandy Roman candle, and sawdust blown through a Bunsen burner makes a very gratifying flamethrower. But what else do you remember from high-school chemistry? If you stayed awake between pyrotechnic demonstrations, you doubtless recall the instructor drawing a lot of three-dimensional figures on a two-dimensional blackboard. Chemistry teachers are not alone in this respect, of course, but chemistry is all about the spatial relationships between invisible, constantly moving objects. And badly drawn diagrams befuddle rather than elucidate, making the simple complex and the complex incomprehensible. Thus chemistry can appear a lot harder than it really is, and many students never give it a second chance as a result. Caltech's Chemistry Animation Project, or CAP, is using computer animation to help high-school and college students visualize what can't be seen, by producing a set of videocassettes containing scientifically accurate, three-dimensional renderings of fundamental chemical concepts.

CAP is the brainchild of Professor of Chemistry Nathan Lewis (BS, MS '77), who, by his own admission, "can hardly draw anything." Increasingly frustrated by this pedagogical impediment, he recalls that in 1991, "I asked my Chem 1 classes, 'Can someone please put atomic orbitals on the computer?'" No hands shot up. But surely, he thought, somebody somewhere must have done it. He called all around the country, but no dice. "There were pseudo-three-dimensional things, but if you didn't know what you were looking at, you couldn't figure out what they were." (Not unlike those computer manuals that only make sense if you already know how to do the procedure you're looking up!) So he recruited Andre Yew and J. Alan Low, two SURF (Summer Undergraduate Research Fellowship) students willing to work nights when certain equipment they needed was idle—namely, graphic workstations in the lab of Peter Dervan, the Bren Professor of Chemistry; and video recorders in the lab of William Goddard, the Ferkel Professor of Chemistry and Applied Physics. By summer's end, they had put together a 10-minute videotape of atomic orbitals rotating on a black background. "It was boring," Lewis says. "It was only good for me to use in my class. But I showed it around, and everyone thought it was great! Peo-









Right: The monkey cranks away at a carbon atom's electron (the e^-). The other electrons in the atom's bonding orbitals are drawn as arrows to show how their spins pair up. Below, top to bottom: An S_N2 reaction, in which an iodine atom approaches from the right and kicks out a bromine atom, which exits the molecule to the left. Meanwhile, the remaining atoms, which were originally cocked off to the right of the central carbon atom, flip over to the carbon atom's left, like an umbrella blown inside-out in a storm. The plots at left track the system's energy, which peaks as it goes through the transition state—midway between reactants and products.

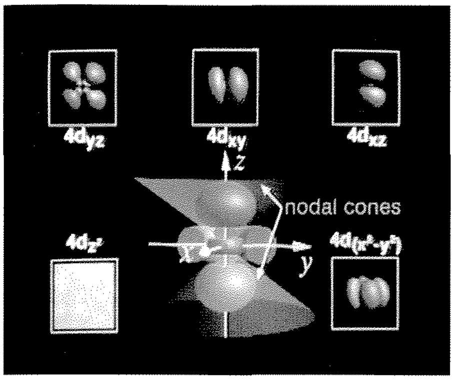
ple wanted to use it, but I didn't want to release it—it wasn't professional quality. So I decided there were two things to do—either abandon it, which is what I fully intended to do, or do it seriously. I'm doing it seriously now."

"Seriously," in this case, has grown to mean eight undergrads (at the moment) and about a quarter of a million dollars' worth of animation software donated by Wavefront, Inc.—the same stuff the networks use. The CAP project owns four Silicon Graphics workstations, overseen by a professional animator, who until recently was Juniko (June) Moody, whose TV credits include the head-butting beer bottles of the Bud Bowl. (Moody recently returned to Hollywood, and Lewis is in the process of hiring a replacement.) The videos include narration and music—light classics that are neither so soothing that the students fall asleep, nor so interesting that they ignore the pictures to listen—and are being dubbed by Complete Post, in Hollywood, to broadcast standards.

But serious doesn't mean stuffy. For example, a segment dealing with ionization energies—the energy it takes to pull an electron off an atom—features an organ-grinder's monkey. The monkey cranks the organ, which is actually a force meter, to tighten a rope attached to the electron. An odometer-like dial on the organ registers how much force the monkey has exerted. If he can't crank hard enough, he hyperventilates and sees stars when he stops. Other segments have flying calipers that measure atomic radii, and periodic tables whose squares turn colors and become bars whose heights are proportional to whatever property is being illustrated.

Of course, Caltech has a tradition of putting science on the small screen. *The Mechanical Universe*, starring Professor of Physics and Applied Physics David Goodstein, debuted on KCET, a Los Angeles PBS affiliate, in 1986, and eventually delivered a full year's worth of physics with calculus. And Project MATHEMATICS!, still in production under the guidance of Professor of Mathematics, Emeritus, Tom Apostol, is an ambitious series of half-hour videos that, among other things, brings the Pythagorean theorem to animated life. Lewis's aim is more modest. "Physicists have an agreement on how the subject should be taught," he explains. "Chemists don't. If I were presumptuous enough to make a series of 20 half-hour tapes that supposedly went through the chemistry curriculum, nobody would ever use it. So we make 10- or 15-minute modules—things the teachers can use in any order they want, and yet still help the students visualize the concept."

Visualization is the key word here. "When we do a simple nucleophilic sub-



Above: The $d_z{}^2$ orbital (center) as it really looks. The wave function is negative in the red regions, and positive in the green ones. The blue "nodal cones" are where the wave function changes sign. Below: These cheat sheets on the communal bulletin board help maintain consistency from video to video. The colors were chosen in accordance with longstanding conventions, modified to be visible on screen against one another.

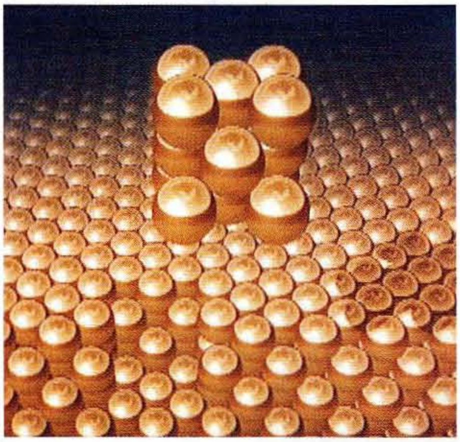
stitution reaction—an S_N2 reaction—it's fine to say the iodine comes in and the bromine leaves. Well, which path does it take? Where are all the other atoms? No chemist—except maybe now Ahmed Zewail [the Pauling Professor of Chemical Physics]—has ever measured this." But accurate depictions are crucial, because visual memories stick. "In most textbooks, people's images of even simple things like atomic orbitals are wrong, it turns out. The textbooks don't draw the whole wave function. When we integrated the $d_z{}^2$ orbital to the 90 percent probability level to find the electron, it didn't look like what was in any freshman chemistry textbook. Somebody even told me it was wrong! We checked and double-checked, and it's not wrong. So the image that you see, as opposed to the function somebody writes down, is the thing that's going to last in these kids' minds."

To get the images right, each video is plotted as meticulously as any stunt sequence Steven Spielberg ever filmed. The production teams—generally two SURF students per video—are survivors of Lewis's Chem 1 class and so know from experience where the ground turns treacherous, and can suggest how graphics in motion could have kept them from getting lost in the swamp. Lewis gives each team a set of concepts to be covered, and the students—"sometimes ignoring my ideas, sometimes using them"—brainstorm on how to present them. CAP's resident graphic artist, Teri Stachura-Seitz, turns these skull sessions into detailed storyboards that give a shot-by-shot, movement-by-movement outline of the video. Says Lewis, "Teri doesn't have a scientific background, by design. And if she can't understand what is being done in such a way that she can logically draw it, we have failed. We have to start over again. And we do that process many times." (Stachura-Seitz, who has a BFA from the Milwaukee Institute of Art and Design, also does the cover art for the video sleeves, and is working for Project MATHEMATICS! as well.)

Once the students have worked out how to depict the material, they use software donated by Biosym, Inc., to calculate the atoms' behavior based on quantum-mechanical first principles, followed by a set of Newtonian, billiard-ball-type calculations that imbue the atoms with "lifelike"—i.e., room-temperature—motions. The results are fed into the animation software, which generates the graphics. Marrying computational chemistry to an animation package designed for TV commercials requires certain adjustments. For example, Lewis remarks, "We have to format the chemistry codes for a program that's really meant to wrap the label around the soda can. But we can make carbon atoms look like strawberries, no problem! Just click on the strawberry texture."

The animation package gives the atoms pretty colors and nice, smooth motions, but there's a lot more to achieving a usable product, says Lewis. "It's not just putting the coordinates in. It's getting motion paths that look right, it's getting the lighting to look right, it's rendering to make things look good in three dimensions. And that's what our animator does. This will vary from buffing up an already good polish, to sanding and a rough polish and the whole thing, depending on what the students do." The production teams have to coordinate—"If we make carbon green here and yellow in the next video, teachers won't be able to lift frames from one to another." (Lewis hopes that teachers will edit the videos on their own VCRs, distilling his offerings into tapes that suit their own needs.) And visuals that are truly stunning on a high-resolution graphics-workstation monitor may look like mud on real-world AV equipment. So CAP checks out all their tapes on what Lewis calls "the world's worst TV"—a 13-inch portable whose color balance is shot, picked up at a garage sale for $7.00 by Todd Allendorf (BS '92. Allendorf went on to a stint at Magic Box, a Hollywood animation studio, and now designs 32-bit CD-ROM video games.)

The work is so labor-intensive, says Lewis, that "no team has ever finished a 10-minute video in a summer. Usually it's two summers. Sometimes it's a summer and an academic year. We make about 30 minutes of broadcast-quality animation a year—that's a *heroic*



Gold's face-centered cubic crystal structure is put on display in this unit cell of gold atoms, which levitates out of the background metal in "Crystals."

amount." Attracting students who are visually oriented as well as computer-literate helps, too. One, senior Scott Townsend, took a year off from Caltech to go to film school before returning to the project. Others are Hollywood-bound computer-science majors, who, following Allendorf's example, hope to parlay their CAP demo tapes into jobs.

(All this computer expertise comes in handy in other ways. The project has a home page on the World Wide Web—http://bond.caltech.edu/—where people will be able to find information about the videos. The page doubles as an on-line discussion group where teachers will be able to tell other teachers what worked for them.)

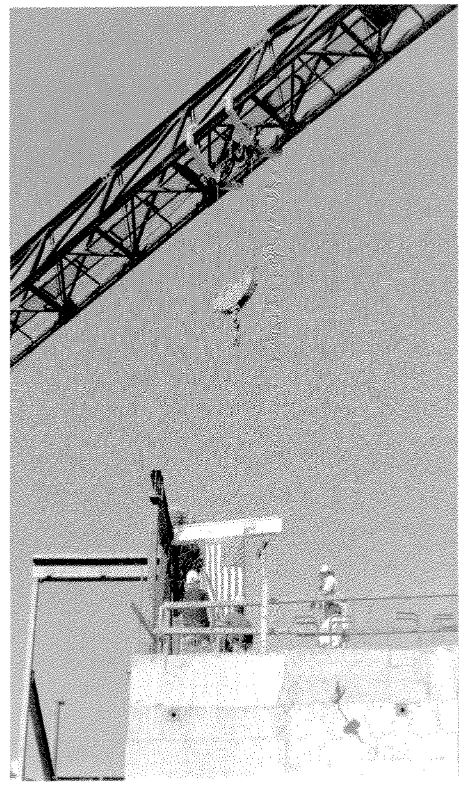
Now in its third year, CAP has hit its stride. The first two modules—Townsend and Yew's "Atomic Orbitals," and "VSEPR" (valence-shell electron-pair repulsion theory, which predicts the shapes of molecules by counting their electrons), by Mark Huber and Corinna Garcia, are slated to come out just in time for Christmas. (Three years from inception to release isn't bad. David Goodstein—who is also editing the CAP scripts—began working on *The Mechanical Universe* five years before its premiere.) Two more modules—Allendorf's "Crystals," and "Stereochemistry," by Michael Medaglia, Huy Lee, and David Zito—are too late for stocking stuffers, but should be available for Twelfth Night. Three more—"Molecular Orbitals," by Anthony Molinaro; "Diels-Alder Reactions," by Tim Uy and Anil Roopnarine; and "Nucleophilic Substitution," by Chris Bryant and Sean Upchurch—should be done by summer. Others—more crystals, trends in the periodic table, and a whole series on bonding—are in the pipeline. Lewis figures that another couple of years will suffice to cover most of the fundamentals.

Lewis wants to get the videos into the hands of as many teachers as possible. The distribution and pricing details have yet to be worked out, but he plans to sell them at Blockbuster Video prices—"you know, $19.95"—although probably not at Blockbuster Video stores. (That price includes permission for the teacher to make unlimited copies.) And while he doesn't expect to see them on the Movie of the Week, and probably not even on PBS, he's optimistic that teachers will actually use them, based on the reactions to the rough cuts he's been showing around. "Even the ones that we think don't look good and are boring, people say they would love to have."

"What's really neat about this is that students are doing it for other students," says Lewis. And it doesn't have to end with chemistry. "We've got a unique facility, because it's fairly user-friendly. With a little bit of training, an astronomer or a geologist could walk in and have students show earthquake strain propagation, or vector fields like the airflow over a wing, or the wind patterns that lead to weather changes. A lot of the other disciplines are excited about using visualization as a teaching tool." □ *—DS*

Random Walk

During the traditional topping-out ceremony on December 7,Board of Trustees Chair Gordon Moore and President Tom Everhart watch as the highest beam is set on the Moore Laboratory of Engineering. Gordon and Betty Moore gave $16.8 million for the building.



New NSF Center in Biomechanics to Collaborate with Industry

The newly established Center for Neuromorphic Systems Engineering, established at Caltech with a five-year, $11-million grant from the National Science Foundation, will promote the design and development of "biological machines," devices that possess human-like senses. The new center has been funded through the NSF's Engineering Research Center (ERC) program, which was established in 1985 to link engineering and scientific endeavors in areas where fundamental engineering advances would enhance U. S. competitiveness. Researchers at ERC centers work closely with their counterparts in business and industry to help turn basic research and technological advances into industrial applications. Caltech's center has also received $500,000 from the California Agency for Trade and Commerce.

Under the direction of Professor of Electrical Engineering Ron Goodman, the Center for Neuromorphic Systems Engineering will build on a unique multipdisciplinary research program in computation and neural systems that has been under way for nearly a decade. Researchers from biology, engineering, computer science, and several of the applied sciences have collaborated on developing such devices as a silicon retina and a silicon "ear," as well as "intelligent skin" that can monitor and react to the air flow over an airplane's wing. Close to 50 researchers, including as many as 30 graduate students, will work in association with the center, whose headquarters will be located in the new Moore Laboratory of Engineering (see above). Caltech will seek out five major industrial collaborators from automotive manufacturing, chemical processing, telecommunications, general manufacturing, and consumer electronics; some 30 to 40 other companies are also expected to sign on as partners.

Honors and Awards

The 1994 ASCIT (Associated Students of Caltech) Teaching Awards were presented to Cheryl Anderson, teaching assistant in chemical engineering; Erick Carreira, assistant professor of chemistry; John Elwood, teaching assistant in physics; Steven Frautschi, professor of theoretical physics; Melany Hunt, assistant professor of mechanical engineering; Julia Kornfield, assistant professor of chemical engineering; Tsutomu Ohshima, karate instructor; and P. P. Vaidyanathan, professor of electrical engineering.

The Graduate Student Council awarded its 1994 GSC Teaching Awards to Norman Brooks, the Irvine Professor of Environmental and Civil Engineering; Barbara Imperiali, assistant professor of chemistry; Gary Lorden, professor of mathematics and vice president for student affairs; Scott Page, assistant professor of economics; and Paul Sternberg, associate professor of biology.

Tom Ahrens, professor of geophysics, will receive the 1995 Shock Compression Science Award this August from the American Physical Society's Topical Group on Shock Compression of Condensed Matter.

John Brady, professor of chemical engineering, has been elected to fellowship in the American Physical Society.

John Carlstrom, associate professor of astronomy, has won a Fellowship in

A top-hatted Nate Lewis helps provide non-fusion-based motive power as random members of the Caltech community pull Dread Zeppelin, a band fronted by an Elvis impersonator in a camouflage-patterned jumpsuit and cape, through the streets of Old Pasadena in this year's Doo-Dah Parade, an annual spoof of the Rose Parade.

Science and Engineering from the David and Lucile Packard Foundation. The fellowship provides $100,000 per year for five years.

Robert Grubbs, the Atkins Professor of Chemistry, has been named a recipient of the 1995 American Chemical Society Award in Polymer Chemistry, an award sponsored by the Mobil Chemical Company.

Jeff Kimble, professor of physics, has been named a Distinguished Traveling Lecturer by the Laser Topical Group of the American Physical Society.

Mark Konishi, the Bing Professor of Behavioral Biology, has been honored by the Acoustical Society of America with its Science Writing Award for an article that appeared in the April 1993 *Scientific American*.

Fredric Raichlen, professor of civil engineering, has received the 1994 John G. Moffatt–Frank E. Nichol Harbor and Coastal Engineering Award at a meeting of the American Society of Civil Engineers.

Kip Thorne, the Feynman Professor of Theoretical Physics, won a Phi Beta Kappa Award in Science for outstanding contributions to the literature of science. Thorne's book, *Black Holes and Time Warps: Einstein's Outrageous Legacy*, which earned him the reward, was reviewed in the Summer 1994 issue of *E&S*.

James Westphal, professor of planetary science and director of Palomar Observatory, has been selected as the 1995 recipient of the Space Science Award, given by the American Institute of Aeronautics and Astronautics, for his leadership in the development of the Wide Field/Planetary Camera on the Hubble Telescope.

Caltech Makes Many Top-10 Lists, Academic and Financial

Caltech ranks high in the November/December issue of *Science Watch: Tracking Trends and Performance in Basic Research*, which lists the top 10 "highest impact" U.S. universities in the physical sciences. The publication of the Institute for Scientific Information based its rankings on citations-per-paper from 1981 to 1993. Caltech was ranked second in chemistry and materials science, third in geosciences, fifth in engineering, sixth in physics, eighth (with JPL) in astrophysics, and tenth in mathematics. Harvard took first place in four of the categories (physics, chemistry, geosciences, and astrophysics); Caltech outranked MIT in five categories (physics, chemistry, astrophysics, engineering, and materials science), but MIT was rated higher in geosciences, mathematics, and computer science.

In other rankings, *Money* magazine's "College Value Rankings" called Caltech the best buy of any scientific or technical school in the nation and the eighth-best buy in education overall. *U.S. News and World Report* recently placed Caltech seventh among its "Top 10 National Universities." And the Council for Aid to Education announced that Caltech had raised more donations in dollars per student in 1993 than any other institution of higher education in the country.

Caltech's Owens Valley Radio Observatory welcomed its latest arrival on October 14, when the Norris Planetary Origins Telescope—a 10-meter antenna sensitive to millimeter-wave radio signals—officially joined its five fellows, one of which can be seen in the background. At the dedication ceremony, held under the Norris Telescope's dish, President Everhart presented a portrait of the telescope to Kenneth Norris, Jr., chairman of the Board of Trustees of the Norris Foundation. The gas and dust from which stars and planets coalesce radiates strongly at millimeter wavelengths, and the combined array will be used to take a highly detailed look at sunlike stars in the early stages of their lives in hopes of discovering planetary systems a-borning. Since the data are taken by comparing the signals from all possible pairs of dishes, the sixth telescope increases the array's sensitivity by 50 percent.