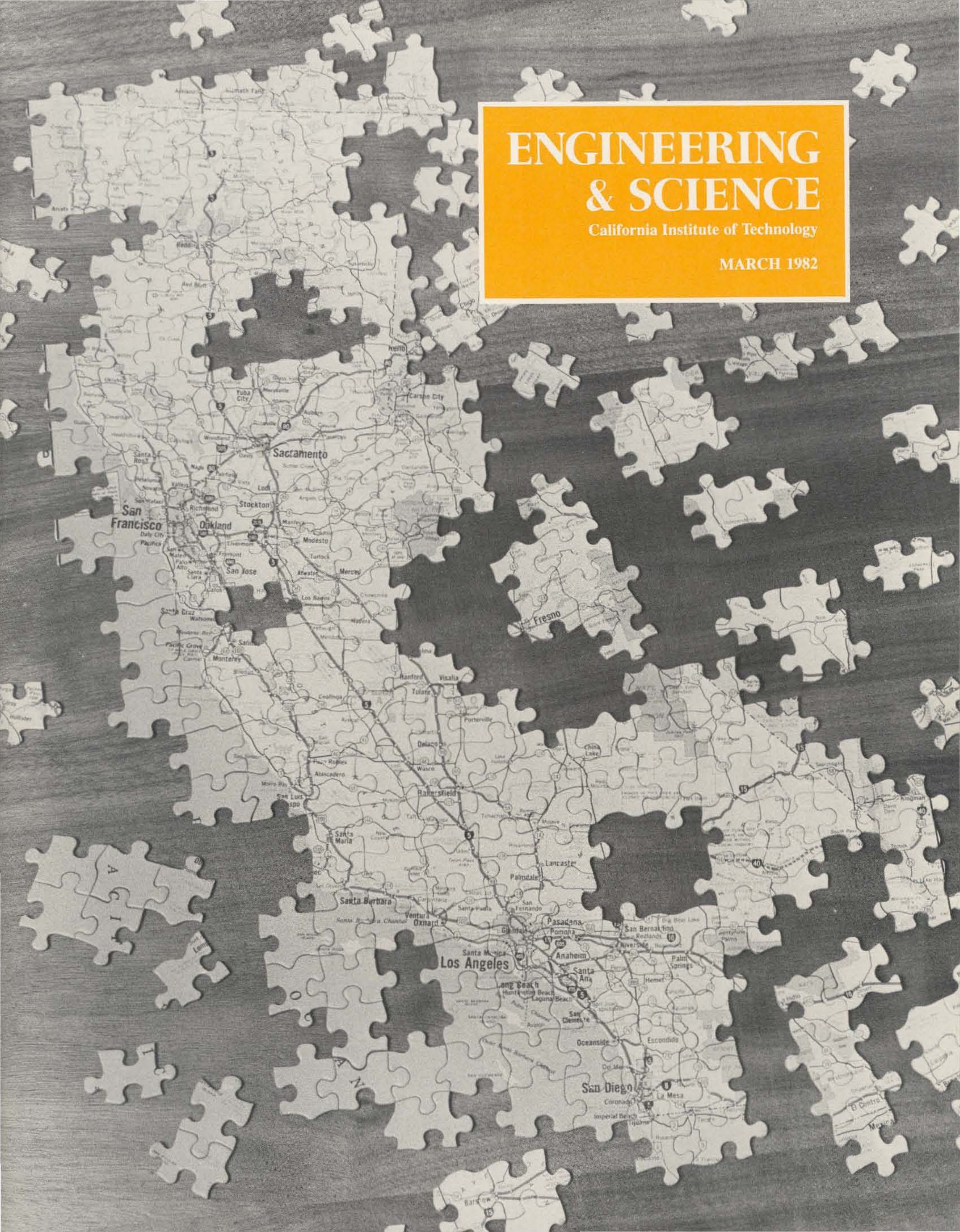
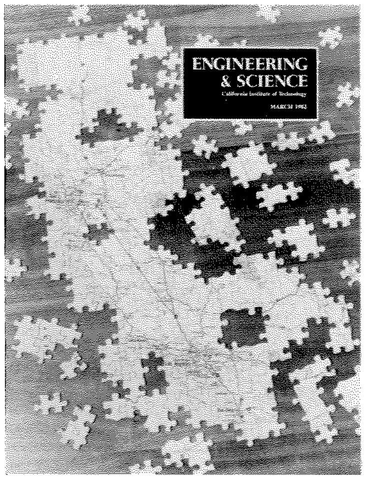
ENGINEERING & SCIENCE

California Institute of Technology

MARCH 1982



In This Issue



Where to Draw the Line
On the cover — an unfinished jigsaw puzzle of the state of California presents one kind of challenge. Taking the state apart and putting it back together again in districts for legislative seats so as to reflect changes in population distribution and insure equal representation offers quite another. Last year, as

chief consultant for technical operations for the California State Assembly Elections and Reapportionment Committee, Bruce Cain had a rare, real-world opportunity, for a political scientist. He participated in what has been called the "most political" (and "most selfish") duty of legislators — redividing the state into districts for legislative seats.

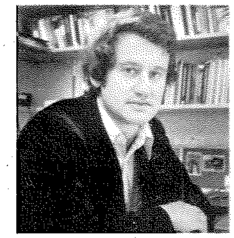
It was an educational experience, to say the least, and Cain has not been spared the political flak that comes from the "inevitable differences of opinion." So far, those differences have impeded approval in Sacramento of the reapportionment plan that emerged from Cain's labors, although the California Supreme Court ruled recently that it will at least be used in this year's elections. Final acceptance hangs on a voter referendum in June.

Cain directed the reapportionment work on a year's leave of absence from his position as assistant professor of political science at Caltech, which he has held since 1976. He earned his BA from Bowdoin College in 1970, then, as a Rhodes Scholar, studied politics at Oxford, where he received a B.Phil. degree in 1972. His PhD is from Harvard (1976).

Cain's major fields of interest are voting behavior and political parties in Great Britain and the United States, and he has extensive background in applying quantitative methods to election procedures. He is currently writing a book, a part of which is adapted on page 4 as "The Reapportionment Puzzle," explaining some of the complex interests and issues that a reapportioner must balance.

Good Vibes
Ever since the Charlie Lauritsen days of the early 1930s, being associated with Kellogg Radiation Laboratory has involved having a particular state of mind, characterized by loyalty to Kellogg and its people, to doing good science, and to enjoying it. It was appropriate, then, that last November Kellogg's 50th birthday was celebrated joyfully and by people who have been a part of Kellogg in one way or another over the years. The technical papers presented were models of good science — past, present, and future — and everything was liberally laced with auld lang syne. One especially happy coincidence was that the 70th birthday of Kellogg's senior member, Willy Fowler, could be celebrated at the same time. On page 15, "Yesterday, Today, and Tomorrow" is a review of the conference, and on page 18, "Phyphty Years of Phun and Physics in Kellogg" is a condensed version of Phowler's talk at the San Francisco meeting of the American Physical Society and the American Association of Physics Teachers in January of this year.

Getting on the Nerves



A very good case for the relevance of basic research was made by Jeremy Brockes in his Watson lecture, "Nerve, Myelin, and Multiple Sclerosis," and in the article adapted from it beginning on page 9. His work using antibodies to label and isolate specific classes of cells within the nervous system is beginning to provide insights into how these cells manufacture the myelin sheath that surrounds and insulates the nerve axon. The neurological disease multiple sclerosis results from the loss of myelin in the central nervous system, and an understanding of the process of myelination, which is best studied under the defined conditions of cell culture, could contribute to its eventual cure.

Brockes, associate professor of biology, has been at Caltech since 1978. Born in Haselmere in Surrey, England (reportedly a quaint, picturesque village), he did his undergraduate work at St. John's College, Cambridge University, and graduate work in molecular biology at Edinburgh University. It was only after completing his PhD in 1972 that he became interested in neurobiology and did his initial postdoctoral work in that field at Harvard Medical School. After returning to work in the Medical Research Council Neuroimmunology Group at University College London, he crossed the Atlantic again, this time to California. Brockes was recently one of 14 scientists nationwide to receive a $100,000 McKnight Foundation Neuroscience Development Award for advanced research.

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ENGINEERING & SCIENCE

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Luis Castellanos mines copper with software.

Most copper is found deep underground. But the Bell System's 995 million miles of copper cable have tons of it above and below ground. That copper provides vital circuit paths to transmit customer voice, data and video signals for today's Information Age needs.

And Luis Castellanos, seven years out of undergraduate school, supervises one of the groups that helps Bell System companies "mine" all that copper. He works with one of the largest computer hardware and software systems in the world—the Trunks Integrated Record Keeping System (TIRKS). Every day it "mines" the vast Bell network for available circuits and equipment. As a result of efficient use of network facilities, the Bell System saves millions by eliminating the need for certain capital expenditures.

Plus, there's more to TIRKS than "mining copper." It also configures circuits and assigns components needed for each circuit path. That allows Bell companies to respond faster to customer requests for complex services like video and data transmission. Employees are more productive too, because TIRKS helps them set up circuits and forecast facility needs.

Before TIRKS was available, keeping track of communications circuits and facilities required enormous amounts of paperwork and manual calculation. Every day, the average Bell System company handles orders involving 1500 circuits and up to 7500 individual components associated with them. Each detail has to be specified and accounted for.

Now, thanks to people like Luis, TIRKS keeps track of all that information instantaneously using computers. Information is up-to-date. It's instantly available. And it's more accurate.

According to computer scientists like Luis, the benefits from TIRKS are just beginning. He believes that, as more computer hardware and software systems like TIRKS interact, new benefits for customers may be possible, as well as additional productivity increases for employees.

Luis joined Bell Labs with a B.S. in computer science from Pratt Institute. Under a company-sponsored graduate study program, he attended Stevens Institute of Technology for his M.S. in computer science. At the same time, he worked part-time assuming responsibility for a large piece of TIRKS software. Working with design teams, he gained valuable insight from experienced members. Now, his technical performance has earned him a promotion to supervisor.

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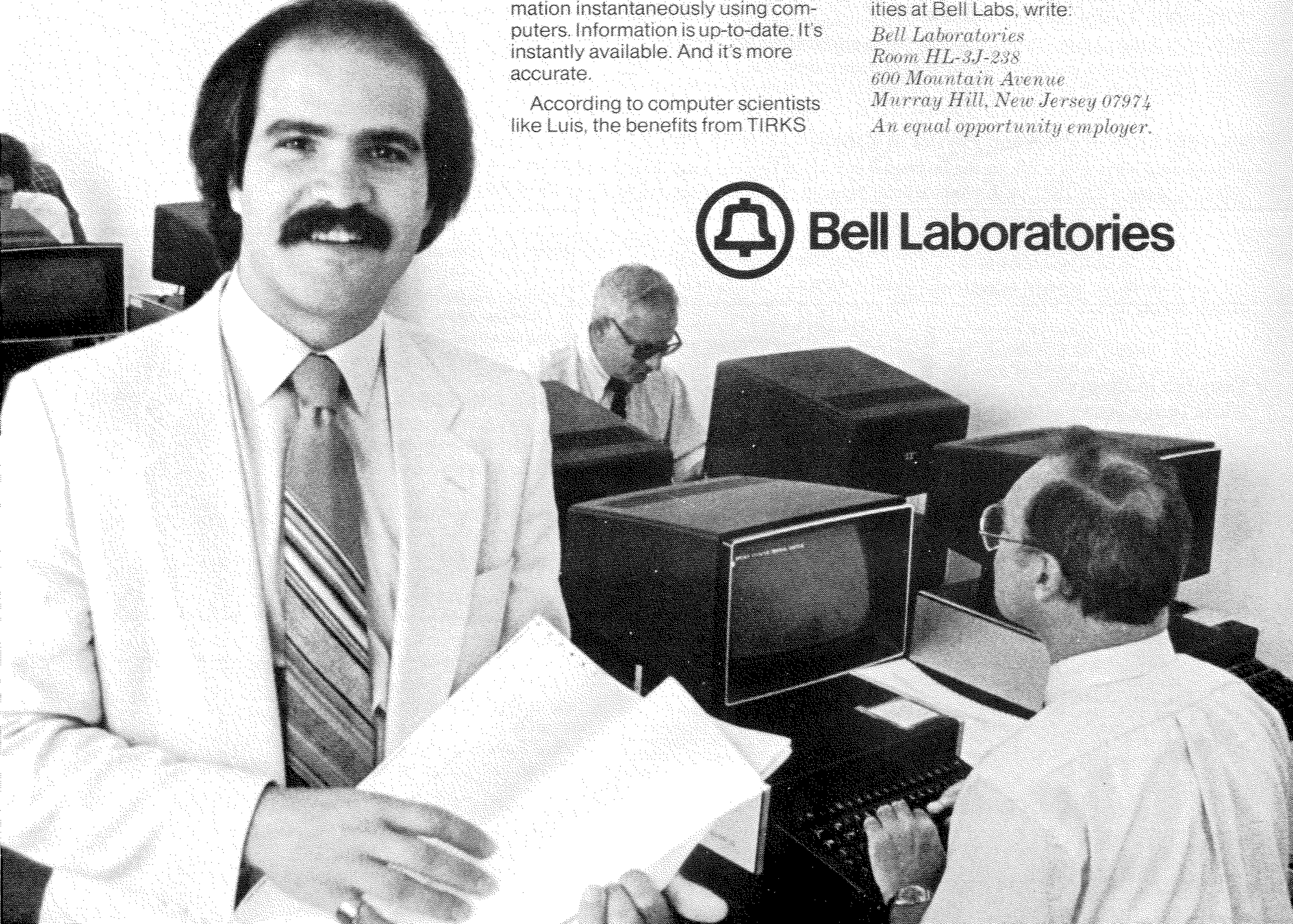
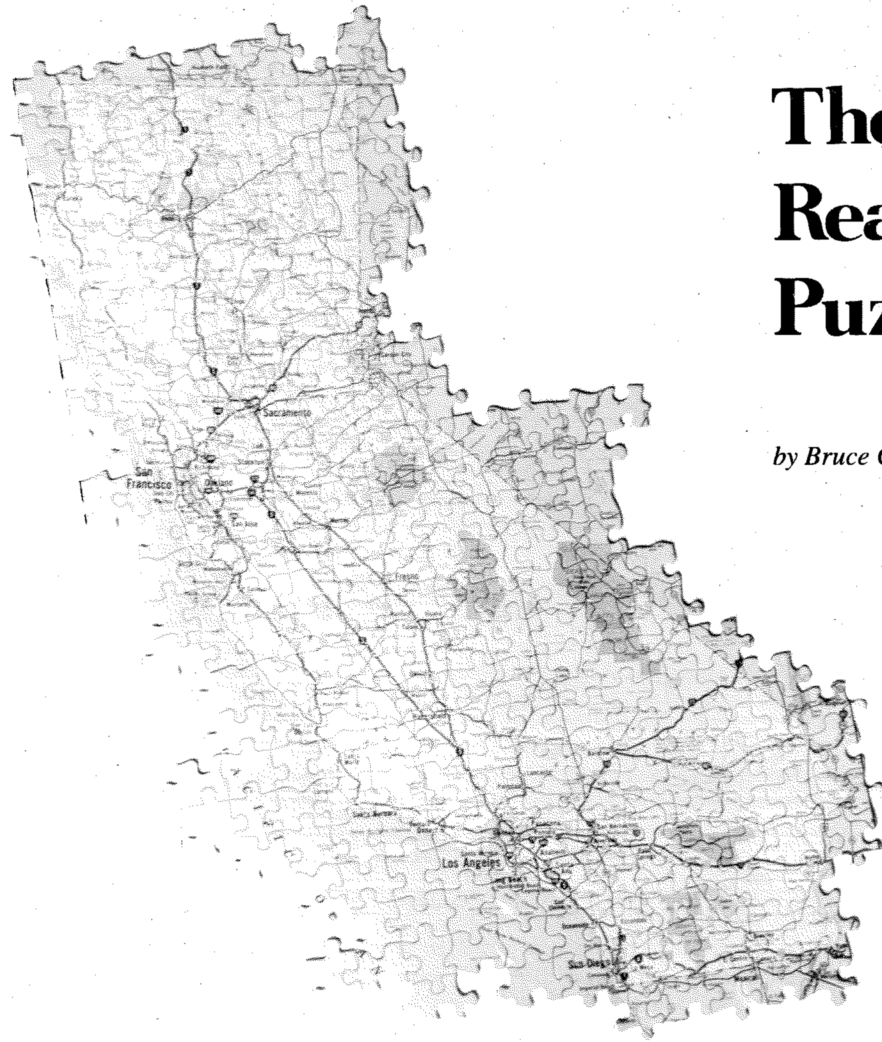




The Reapportionment Puzzle

by Bruce Cain

Every few years the population of the various legislative districts in this country changes sufficiently that they need to be reapportioned. On the face of it, that task seems a simple matter. All that needs to be done is to draw a specified number of districts so that each has the same number of people, or as close to the same number as is practical. The ideal electoral district population is the total state population divided by the number of seats in the legislature. So defined, the problem is technical, not political, since determining the ideal population is an arithmetic exercise and figuring the size of the districts requires knowledge of and skill in the use of census data. One would think that a problem of this sort should not provoke a great deal of controversy; either the districts have equal populations or they do not.

By contrast, the practice of redistricting is quite complicated. A great deal of time and money is spent on drawing and analyzing plans. Reapportionment staffs collect immense amounts of data and build or purchase sophisticated computer systems to aid them in their tasks. The legislators themselves sit through numerous meetings, arguing about various proposals and bargaining for a better seat. The legislative leadership too must devote time to putting together the votes for a bill, time which some would say could be better spent on more pressing policy matters. Even after a bill is passed, the reapportionment struggle continues. Aggrieved parties bring suit against the legislature to invalidate the plan, with the consequence that reapportionment can be fought in the courts for years to follow. In the end, both the participants and the public grow weary of this struggle, and quite naturally, people begin to question whether all of the bother was necessary. Is it not possible to reapportion a legislative body with less expense and trouble? My experience is that both the problem and the solution are more complex than we are likely to imagine.

During 1981, I was on leave from Caltech to head the technical staff for the California State Assembly. I came to the job somewhat fortuitously. At the end of November 1980, I had been suggested to the newly appointed chairman of the Elections and Reapportionment Committee, Richard Alatorre, as someone who could direct the Assembly's technical work. Since graduate school, I had concentrated my research on elections and parties and had had considerable experience in the application of computers and statistics to the study of elections. When the idea of becoming involved in the Assembly reapportionment was raised, it seemed to me that my background in quantitative approaches would be valuable to the Assembly, and that a year's exposure to what the ex-Speaker of the Assembly, Bob Moretti, has called "the most political, the most crass, the most selfish act that any legislator ever engages in" would be educational for me. What better way for a political scientist to get a taste of politics than to participate in this "most political" legislative duty?

I was able to hire a number of Caltech students over the summer and on a part-time basis to assist me in the technical work. In brief, there were three tasks. One was to build a data set that could be used to analyze the consequences of various proposals. This meant merging census and political data into a large computerized file. Since there is no easy conversion between the two sorts of information, it was a time-consuming and laborious job requiring several months of intensive work. The second task was to construct a graphic display that would show the outlines of proposed districts and update tabular information associated with them. The third task was to put together a plan reflecting the preferences of the legislators that would meet all the requisite constitutional and technical standards. Needless to say, this became the most illuminating part of my job.

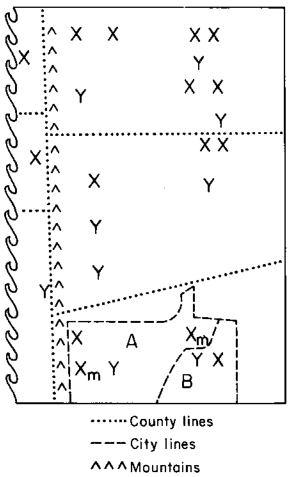
Perhaps the most valuable lesson that I learned from this experience is that the reason reapportionment has proven to be so controversial over the years is that the problem itself is political: that is, it is one that vitally affects the interests of the parties and various interest groups and for which there is no uncontroversial solution. The best way to see this is to examine a simple approach to reapportionment and discover what kinds of problems arise consequently. Then, we will look at some actual problems with drawing district lines in California and return to the issue of reform.

The simplest approach to reapportionment would be to start in some corner of the state and draw square-like districts with the required number of people in them. Many people believe that compactness is the key to fairness. If asked, they would say that they can tell a gerrymander when they see one. The term gerrymander itself derives from the salamander that a painter drew on the map of a contorted district in 1812. Fingers, slivers, jagged edges, noncontiguous census tracts, and abstract forms of all sorts are the images associated with unfairness. Compact forms such as circles and squares are associated with good government. Consequently, the press and the public tend to measure the worth of a reapportionment plan by its shape: A plan with compact forms is assumed to be in the public interest, and one with noncompact forms is assumed to be in the self-interest of the majority party or of incumbents in general.

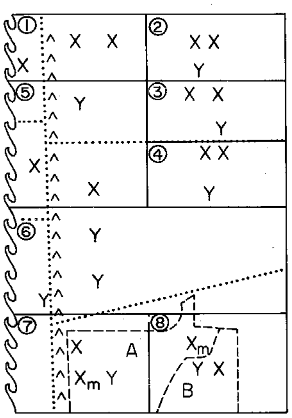
The popular concern for compactness has several sources. One is the legacy of earlier periods in history when communication and transportation were more difficult. Compactness guaranteed that representatives could meet their constituents with relative ease and, vice versa, that constituents could visit their representatives. With the improvement of modern communication and transportation, however, travel over large and sprawling areas is no longer a formidable task. Moreover, the inconvenience of representing a large area can be lessened in fairly simple ways. The representative can have several district offices, or can take cases in a mobile van, or can delegate much of the day-to-day dealings with constituents to district staff. Furthermore, various studies have shown that a great deal of contact between representatives and their constituents occurs over the phone and by mail. People do not have to visit the district office to get what they want.

Thus, the historical reason for compact districts — to lessen transportation and communication costs — is less applicable in the modern era. The more common argument for compactness is for its indirect, rather than direct value. By indirect I mean the value that compactness has because it facilitates the observance of other good-government criteria. By direct, I mean the intrinsic value of compactness per se.

Compactness is commonly linked to other good-government criteria in several ways. It is said that compactness helps preserve communities of interest. Sprawling districts can tie together disparate communities for the sake of partisan advantage. Beach and desert, urban and rural, mountain and valley interests are mingled for political purposes. By requiring districts to be compact, you make it harder for reapportioners to reach across communities for whatever purposes they might have in mind. Compactness for the same reason serves as a preventive against political gerrymandering. Observing compact lines, it is sometimes alleged, ensures greater political fairness because it makes contortions for political advantage more difficult. Compactness also saves cities and counties from being split for political purposes and protects minorities from racial gerrymandering. In all these ways, then, compactness allegedly encourages compliance with other good-government criteria. But does it? Is there any logical connection between compactness and other criteria of fairness, and if not, is there at least some empirical connection between the two in California?

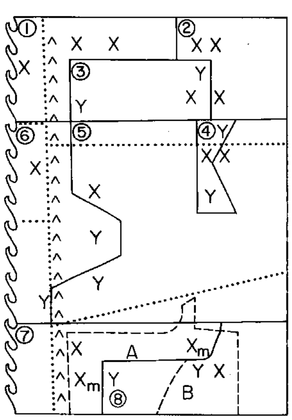


One way to explore the question of whether there is any logical connection between nice looking districts and good-government values is to examine a hypothetical example. The figure at the left is a hypothetical state with 6 counties, 2 cities (A and B), and a population of 24 people, of whom we assume that the X individuals belong to one party and that the Y individuals belong to the other. The Xm individuals are minorities. The state has regional variations: The ^ represent mountains, and we will assume that the left-hand edge of the figure is coastal. The dotted lines are county lines, the dashed lines are city lines, and — in the rest of the figures — the solid lines will be the district boundaries. There are eight seats in the legislature, and we will assume that districts must be equally populated with no deviations.



Using the tabula rasa (clean-slate) approach at the left, we will start in the upper left-hand corner and draw a series of compact districts from top to bottom. Each of these eight districts will be either square or rectangular with no jagged edges, slivers, or curvy forms. The compactness of the districts is of course facilitated in this example by the symmetry of the state shape — drawn as a rectangle — whereas in the real world, states themselves can be oddly shaped. Each of our eight model districts has three people in it so there is no population deviation.

The symmetry of the shapes in our model plan masks some disturbing features. To begin with, although the Y individuals constitute over one-third of the population (that is, 9 out of 24), they have only one seat. In short, their ratio of seats to population is highly skewed. In addition, the minorities are split so that they cannot control a seat although they have enough people to do so. The city and county lines are in several places violated where they are noncompact. Finally, the beach areas are linked with the valley and urban areas in several places, making it very hard for them to lobby effectively for their environmental concerns.



The Remedy for Partisan Skew. The first problem is how to redress the imbalance between the population of Y individuals and the number of seats they control. Given the dispersion of the Y population, compact districts do not accurately reflect their numbers. It is well known that the type of electoral system we use in this country is not as fair to dispersed minority parties as is a proportional representation system, common in European countries, which assigns seats to parties based on their proportion of the vote. It is always possible for a minority party to be so dispersed throughout the polity that it comes close to winning several seats but loses them all. In fact, some see this as a desirable feature of single-member, first-past-the-post systems. By exaggerating the strength of the majority party, the system ensures a large enough legislative majority to get bills passed. It is a hedge against legislative immobilism.

While acknowledging that our electoral system is inherently unfair to minorities when they are geographically dispersed, we also see that the way the lines are drawn can aid the bias in favor of the majority considerably. The compact option in our simple example exaggerates the strength of the majority X individuals and the weakness of the minority Y individuals. Such is the importance of the way lines are drawn that the ability of the minority party to achieve representation hinges crucially on which option you pursue.

For example, you can easily adjust the shapes of the districts to increase the strength of the minority; adjusting the shape can compensate, in effect, for the initial dispersion of the minority party population. The reason the Y individuals have so few seats in the first districting is that the Ys in seats 2 and 3 are cut off from each other, as are those in 4 and 5 and those in 7 and 8. In the one seat they hold — seat 6 — they are concentrated so that they have more than the simple majority needed to win the seat. To give the Y population control of four seats, you need to do the following (in the margin bottom left):

1. Put the Y from seat 3 with the Y from seat 4.
2. Put the Y from seat 2 with the Y from seat 5.
3. Put the X from seat 5 with the Ys from seat 6.
4. Put the Y from seat 7 with the Y from seat 8.

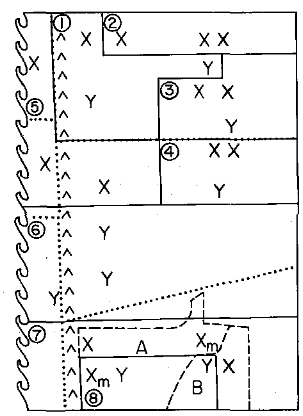
The shapes that result are by most definitions noncompact, or what is known in the trade as "ugly." We have gone from a situation of one seat for the Y individuals to a situation of four seats by making our districts as dispersed as the Y population. This indicates dramatically the potential effect of line drawing upon the partisan distribution of a state — it can change the Y population from a minority position to one of political equality, but not without some attendant costs.

First we observe that the lines still cross county and city lines. Second, they violate communities of interest by linking the coastal and noncoastal areas in the new seat 6. The urban areas of cities A and B are linked with nonurban areas in seats 7 and 8. Most important, however, allowing the Ys to have a fourth seat gives them more seats than they deserve. They have only 9 out of 24 individuals in the entire population, but the new plan gives them 50 percent of the seats. In short, the remedy was excessive.

A more moderate proposal for partisan distribution (although still ignoring the other criteria) would be the following:

1. Put the Y from seat 2 with the Y from seat 5 and with an X from seat 1 rather than the X from seat 3 as before.
2. Put the remaining Xs from seat 2 with one X from seat 1, and put the remaining X from seat 1 with the Xs from seat 5.
3. Put the Ys from seats 7 and 8 together with an X from one of those seats.

As shown in the margin at top right, this gives the Y individuals three seats out of eight which is exactly proportionate to their population distribution. The ugliness of the lines is lessened somewhat although the new lines are not as compact as our original set.

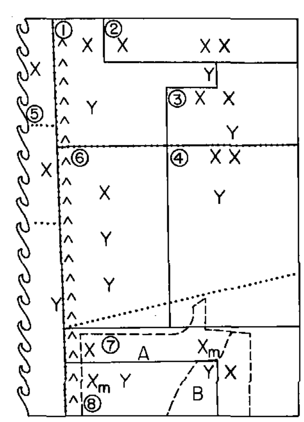


A remedy for communities of interest. Our first observation about shapes, then, is that compact forms are not necessarily more fair in a partisan sense than noncompact forms. We must not overlook, however, a second characteristic of our electoral system, which is that our legislative districts are geographically based. In a proportional representation system, representatives are elected at large or in big multi-member districts. Typically, voters choose from alternative party lists. The number of specific candidates that are chosen in some order from those lists is commensurate with the party's share of the vote. The representative in such a system does not have sole responsibility for representing a particular geographic area, whereas the representative in the single-member system does, giving specific geographic areas — agricultural, urban, coastal, mountains and desert — a representative who can articulate and defend their interests. Geographically defined seats are thus a crucial component in the pluralist process, that is, a government in which decisions are made by coalitions of groups. The mandate to represent geographic interests is clearer when the districts are more homogeneous; for example, when beach communities are not thrown together with inland industrial areas, when agricultural interests are separated from urban, and so forth. This is the genesis of the idea of preserving communities of interest in reapportionment. While the Supreme Court has not accorded the principle of respect for communities of interest the same standing that it has given to the principle of equal population, the logic of our electoral system makes an argument for striving to preserve these communities of interest wherever possible.

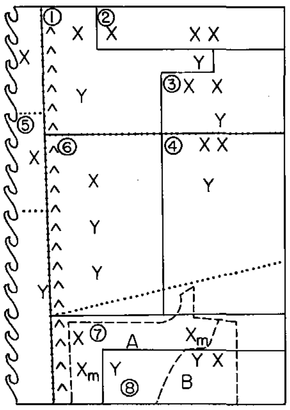
Here too an unhealthy passion for compactness can be an impediment. Consider the example of our hypothetical state. The coastal area in it has been very narrowly defined. Based on the experience of California, it is entirely conceivable that you would not have to travel very far inland before you encountered attitudes on issues such as the environment that were very different from those held by the beach people. In our example, a seat that was purely coastal — or even mostly coastal — would be very long and narrow in shape. Seats that cut across the mountains to take in coastal areas would be more compact but would dilute the voice of the coastal interests.

A second community-of-interest problem in our example is the urban/rural division. Our example has two cities (A and B) at the bottom of the state that were violated as urban districts both in the first plan and in the proposal that would have given the Y population four seats. Mixing urban and rural interests can create a situation in which the more populous urban areas swamp out the voices of the less populous rural areas. Preserving urban interests in a manner compatible with the compactness requirement is somewhat easier than preserving rural interests. This is because the very fact that urban areas are more densely populated means that they will need less area to achieve their required populations than will rural districts. Compact and homogeneous rural seats are harder to construct since by definition there will be fewer people per acre of area. To maximize compactness, the reapportioner will be sorely tempted to combine urban areas with rural areas since this will lessen the total area needed to construct a seat, but maximizing compactness in this sense diminishes homogeneity.

A plan that observes the communities of interest in our model state is shown at the right. It would do the following:



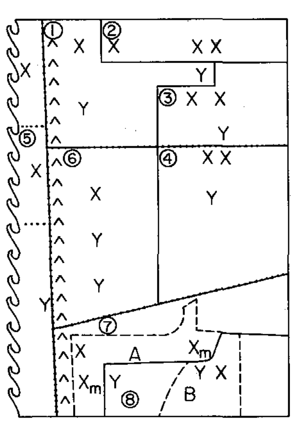
1. Unite the coastal Xs from seat 5 with the coastal Y from seat 6, and put the remaining X from seat 5 with the two noncoastal Ys from seat 6.
2. Keep the two urban seats wholly contained so that they are not tied in with the coastal population.



The remedy for minority dilution. The third flaw in the compact district plan we drew earlier is that it divided the minority community in a manner that deprived it of a seat. The principle of minority group strength under first-past-the-post electoral systems is identical to that of minority party strength. To the extent that support is efficiently concentrated, the minority group will not suffer under-representation, but to the extent that the minority group is dispersed, or overly concentrated, it will suffer under-representation.

The division of the minority population can be remedied as shown at the left by putting the Xm from seat 8 with the Xm from seat 7. In order to preserve our earlier move to give the Y party proportionate strength in the legislature, we would add this Xm population to the X individual in seat 7, thereby allowing the Ys to control one of the urban seats.

The remedy for city and county splits. Finally, the quest for compactness runs into yet another hurdle; city and county boundaries might not be compact themselves. Several states have adopted constitutional amendments that require reapportionment plans to respect city and county lines to the extent possible. One justification for these provisions is a version of the community-of-interest argument. Cities and counties are communities with special concerns, and dividing them makes it harder to articulate those concerns. Instead of having voting strength n/p in the seat where n is the number of voters in the city or county and p is the total number of voters in the district, the voting strength of the split city is n′/p where n′ is the share of the city or county that remains in the seat. Some have also argued that neatly interlocking local, state, and congressional lines lessen confusion in the minds of the voters and facilitates cooperation between officials at all levels.



Whatever the merits of constitutional provisions mandating respect for city and county lines, the relevant point is that they will sometimes cause districts to be noncompact. Recall that the dotted lines on our hypothetical state represent county lines and that the dashed lines represent city lines. Respecting county lines dictates that the new seats 4 and 6 be wholly contained in one county. The new form (left) is less compact but more consistent with city- and county-line criteria. The counties at the top are larger than one district in size, so it is necessary to divide them both to create the surplus seats. And it is possible to preserve the city and county lines in our urban area by drawing two seats that are wholly contained in the county. It is common, however, to find that cities annex in very peculiar — and often politically shady — ways, and this is reflected in the nonpopulated appendage of our hypothetical city line. If there were projected growth in that area, it is quite possible that the city would insist that we respect its border even though no one lives in the area at present.

A comparison of the new lines, with all the changes we have made so far, and the original set of lines is stark. The new lines are much less compact, and yet they better satisfy the other good-government criteria. As before, we still have eight seats with three voters in each, but the new lines have given the Y individuals control of three out of the eight seats, which is exactly proportional to their population. The minority group Xm also has gained control of a seat, and the new plan conforms better to county and city lines. Finally, the new lines preserve the beach, urban, and rural communities of interest to a greater extent than did the old.

The hypothetical case we have just examined demonstrates that it is fairly easy to construct a plausible example in which compactness conflicts with other good government norms. There is no necessary logical relation between compactness and other criteria. But it is possible that even though there is no logical relation between the two, there is nonetheless an empirical connection; there could be a happy coincidence between compact lines and proportionate outcomes for minority groups and parties, respect for city and county lines, and the preservation of communities of interest. From the point of view of salvaging the indirect value of compactness as defined earlier, it would not matter much whether the connection was logical or empirical. The relevant consideration is simply that it happens.

In order for this happy coincidence to occur, the following conditions would have to hold:

1. An efficient distribution of partisan support for both parties would have to be compact.
2. City and county lines would have to be compact.
3. Minority communities could not be dispersed.
4. Communities of interest would have to be compact or divisible into wholly contained compact forms.

The first proposition simply reiterates the point that single-member, simple-plurality systems will produce especially disproportionate results if the minority party's support is not efficiently distributed. No doubt, there is a great deal of variation

continued on page 29

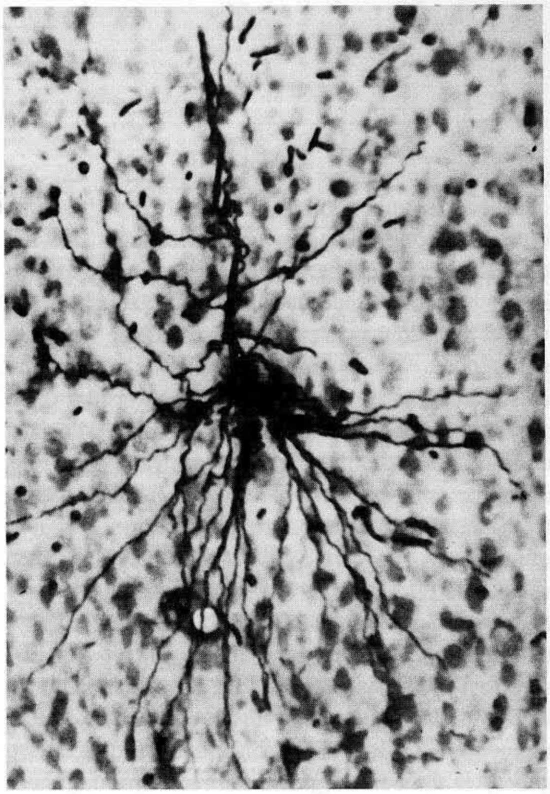
Nerve, Myelin, and Multiple Sclerosis

by Jeremy P. Brockes

Those who investigate the nervous system may do so at a variety of different levels. The student of behavior may spend much time in the observation of behaving animals. The neurophysiologist who is interested in the mechanisms underlying our senses, such as hearing or vision, concentrates on those areas of the brain that are specialized for these particular functions. In this account I would like to focus on a different level of organization — the cellular one. Just like our other tissues and organs, our brains are made up of cells, and I shall be concerned with various interactions between the different cell types.

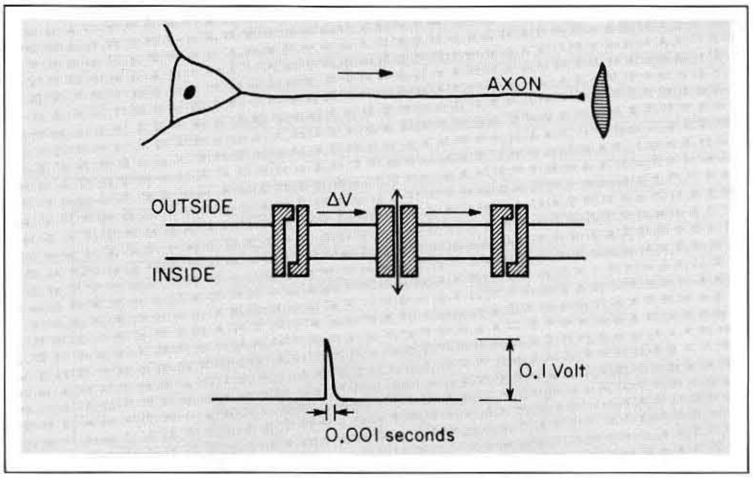
Nerve cells are the principal cell type in the nervous system. They are responsible for conducting and processing electrical and chemical signals, and their performance underlies all of those conscious and unconscious activities that we associate with our nervous system. Over half of the bulk of the brain is made up of another cell type called glial cells. Glial cells are unable to conduct and process chemical and electrical signals, but they perform a variety of important functions that support the activities of nerve cells. In our central nervous system (the brain and spinal cord), the principal glial cells are the oligodendrocyte and the astrocyte. In our peripheral nervous system (those parts of our nervous system that lie outside the brain and spinal cord — for example, the nerves passing from the spinal cord to skeletal muscles), the principal glial cell is called the Schwann cell. It is named after a great German anatomist who was active in the first half of the 19th century and was in fact one of those who were first responsible for proposing that biological tissue is made up of cells.

There are many different types of nerve cell in our nervous system. In fact it is a significant point of uncertainty that we do not know how many. Everyone would probably agree that there are at least 100 categories of nerve cell in the brain and spinal cord. Others would argue that there are likely to be thousands, or still more if you looked with methods of adequate discrimination. Since there are well over a million million nerve cells in our nervous system, there is considerable scope for uncertainty on this question.



This nerve cell is seen in a slice or section that has been cut through a region of the brain specialized for vision. The section has been stained in a particular way so that only one of the nerve cells present in the tissue is actually stained. If all of the nerve cells in the section were stained, the whole picture would just look black. (Original photograph by Charles Gilbert and Torsten N. Wiesel.)

Nerve cells have a thickened central portion called the cell body. The cell body contains within it the nucleus, which is responsible for directing the construction of all of the different components that make up the nerve cell. Radiating out from the cell body are a number of very fine processes, and it is these processes which are responsible for making connections with other nerve cells in the brain. The fundamental mechanism of communication over long distances in the nervous system is electrical. Nerve cell bodies and their processes serve as conducting elements for tiny electric currents which flow between areas of high and low voltage.

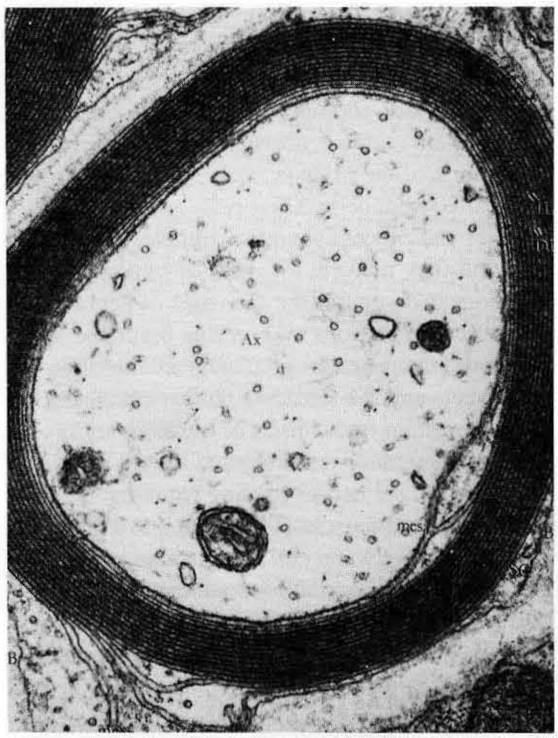


The top diagram depicts a nerve cell (left) with its axon contacting a muscle fiber (right), while in the center is a schematic drawing of the axon membrane with its voltage-sensitive gates. In its normal resting state (left) the gate is closed, but a change in voltage opens it (center) and permits charged substances to cross the membrane, which changes the voltage and opens the next gate and so on. The general profile of this nerve impulse is shown below.

The diagram above shows a nerve cell whose cell body is embedded in the spinal cord. It sends out a very long process which makes contact with a skeletal muscle fiber. If this were a muscle fiber in our toes, the process — called an axon — would have to go several meters distance from the spinal cord. When we want to activate our muscles, electrical signals arise from the vicinity of the cell body and travel along the axon leading to contraction of the muscle. The electrical signaling in the nervous system differs in an important way from that which we are familiar with in telecommunications. When we pick up the telephone and speak into it, a train of electrical signals is set up and carried away by cables. The telephone company exerts considerable ingenuity in trying to ensure that the signals are not degraded or attenuated in their long passage through the cables. But the nervous system uses a different principle. It generates the basic signal continuously along the length of the conducting processes. In that respect it can be likened to lighting a fuse of gunpowder.

The axon is a long cylinder covered on its outer surface by a very thin membrane, which serves to separate the outside from the inside. Stuck in and all over the membrane is a very remarkable component which acts like a channel or gate. In its normal resting state this gate prevents the passage of substances across the membrane. The remarkable feature of the gate is that it is sensitive to voltage. When the voltage across the membrane changes, then the gate opens, allowing certain charged substances to cross the membrane, and thereby further changing the voltage across it. The gate soon closes, however, and returns to its initial state. If we look at a little patch of membrane during this process and measure the voltage across it, there is a deflection as the gate opens, and then the signal comes down again as the gate closes. The size of this deflection is about 0.1 of a volt, and it is over in about one millisecond. It is a very remarkable fuse of gunpowder that is capable of being relit hundreds of times a second, since when the signal returns to baseline the axon is able to "fire" many more of these signals. Now it is very important for many of our axons to conduct these signals at high velocities of a hundred or more meters per second. It is usual at this point to remark upon the practical problems that giraffes face in communicating from their spinal cord to their feet. In fact there are many locations in our brain and spinal cord, as well as in our peripheral nervous system, where it is important to have rapid conduction of the nerve impulse. In order to do this, the axons have to be insulated to prevent dissipation of the currents.

The electron micrograph below shows a section that is cut across one of the axons that are specialized for very rapid conduction of the nerve impulse. Because the axon is a cylinder, the section has a circular profile, and it is enormously magnified since its diameter is only 0.01 millimeters. There is a central circular area that is surrounded by about 20 closely packed concentric black lines. The innermost black line is the membrane that we have just been considering; it is the structure which carries the voltage-sensitive components that generate the nerve impulse. Surrounding the cylinder are more of these black lines, and this packed sheath of membranes is the insulating



In this electron micrograph of a cross section of a myelinated axon, the innermost black line is the membrane in which the nerve impulse is generated. The surrounding lines are the insulating myelin sheath.

material used in both the central and peripheral nervous system. This material is called myelin, and in the brain and spinal cord it is made by the oligodendrocyte, one of the glial cells mentioned earlier. In the peripheral nervous system, myelin is laid down by the Schwann cell.

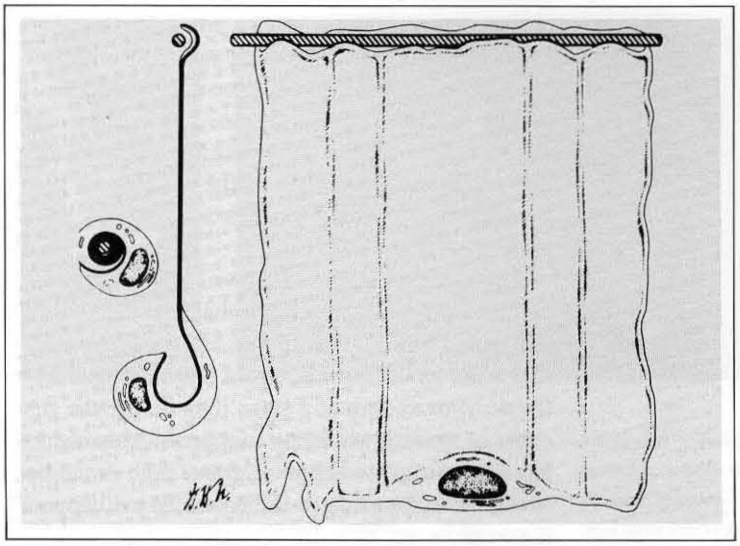
An enormous amount of our brain is given over to myelin, and there is a fundamental distinction between "white matter," containing myelinated processes, and "grey matter," containing the nerve cell bodies. The myelin present in our nervous system testifies to the importance of rapid channels of communication. For a long time it was unclear how myelin was laid down around a nerve, but eventually it was shown that this concentric arrangement is a closely packed spiral, which unwinds as an enormous sheet of membrane.

The questions about how the oligodendrocyte and the Schwann cell interact with the nerve cell have many fascinating aspects to them, but I have chosen to emphasize here the great clinical importance of myelin. There are a number of neurological disorders that result from the breakdown of myelin either in the central nervous system or the peripheral nervous system, leading to failure of conduction of the nerve impulse. The most important and best known of these diseases is multiple sclerosis, or MS, which in many countries is the most prevalent neurological disorder among young adults. In MS the myelin in the central nervous system is lost in certain areas. If we were to look at a section of the spinal cord that had been stained for the presence of myelin, there would be rather discrete patches that did not stain, and where the myelin had been destroyed. These areas are called plaques — a term introduced by the great French neurologist Charcot, who first systematically described the disease over a hundred years ago at the Hospital of the Sâlpetrière in Paris. In the vicinity of the plaque the oligodendrocytes are destroyed and the myelin is lost. Interestingly, multiple sclerosis only affects myelin in the central nervous system. The myelin in the peripheral nervous system, which is the product of the Schwann cell, is not affected. In the vicinity of the plaque the astrocyte, which is the other principal glial cell in the central nervous system, increases in number and takes over the location of the oligodendrocyte. The astrocyte, however, is not able to make myelin so it does not remedy the defect in insulation. One of the remarkable things about the MS plaque is that the nerve axons remain intact despite this great cellular destruction and reorganization.

MS is in many respects a bewildering disease, but most people would agree about its basic qualities. It is not an inherited disease in the strict sense like, for example, hemophilia or Duchenne muscular dystrophy. Certain aspects of susceptibility, however, may well be inherited. The most common age of onset of the symptoms of MS is in the early 30s, but it may be considerably later. The symptoms are highly variable, presumably reflecting in part the location of the demyelinated plaques, though they may affect a variety of neurological functions. But one of the curious features of MS is familiar to almost all patients — the episodes of remission and relapse. A period of remission is one where the initial symptoms either disappear completely or abate for quite a long period, only to be followed by a subsequent period of relapse when the symptoms return.

How common is MS? When we are talking about short-lived diseases, like influenza, it makes sense to talk about incidence — the number of cases that occur in a given year. But when we are considering a long-lived disease like MS, the more relevant figure is the prevalence, which is usually expressed as the number of cases on a given day per hundred thousand of the population. One of the interesting features of MS is the rather marked variation in prevalence with geographical latitude. Although there are some exceptions, it is a rare disease in the tropics. In northwest Europe, the northern United States, in southern Australia and New Zealand, the prevalence may well be at the level of 40 cases per 100,000 or even higher. In the Orkney and Shetland Islands off the north coast of Scotland, the prevalence is a staggering 200 or 300 cases per 100,000. These numbers tell us that many individuals in society suffer from the disease, but it is the celebrities with MS who most effectively draw attention to it. One of the hostages in Iran

Reprinted from an article by H. deF. Webster in the Journal of Cell Biology 48 (1971).



At the far left is a schematic diagram of a profile of a myelinated axon in its normal wound configuration, similar to the micrograph on the previous page. Next to it on the right are two views of the axon and glial cell after the myelin sheath has been unwound.

was allowed to return home early because he was afflicted with MS. So far as I am aware, however, the most famous patient with MS is the virtuoso English cellist Jacqueline du Pré, whose career was ended by the disease a few years ago, but who has continued to speak out very rationally and courageously for the need for more public awareness of the disease and for increased support of research.

The cause of MS is unknown, and there is no certain cure. One suggestion that I would like to consider a little further is that MS occurs as a result of an environmental agent that impinges on the nervous system at a stage long before the first onset of symptoms. Some support for this idea has come from studies of migrants who move between different areas of the world. What happens if you grow up in an area of characteristically high frequency, like northern Europe or the northern United States, and then move to an area that has a characteristically low frequency, such as South Africa or Hawaii? The general trend in these data is quite interesting. If you move at an early enough age from your residence of high MS frequency, then you can move to your new location and take up its characteristic low frequency. If, however, you move past a certain age from your original residence of high frequency, then it is too late, and you express the frequency of your original residence even though you now live in one of low frequency. The age at which this changeover occurs has not been established precisely, but many people consider that it is around 15. For example, there have been studies of migrants from continental United States to Hawaii, which is a characteristically low frequency area. If they migrate below the age of 15, they express a prevalence that is five times lower than those who move over the age of 15. There is also a considerable amount of data on people who moved from northern Europe to South Africa that in general supports this picture.

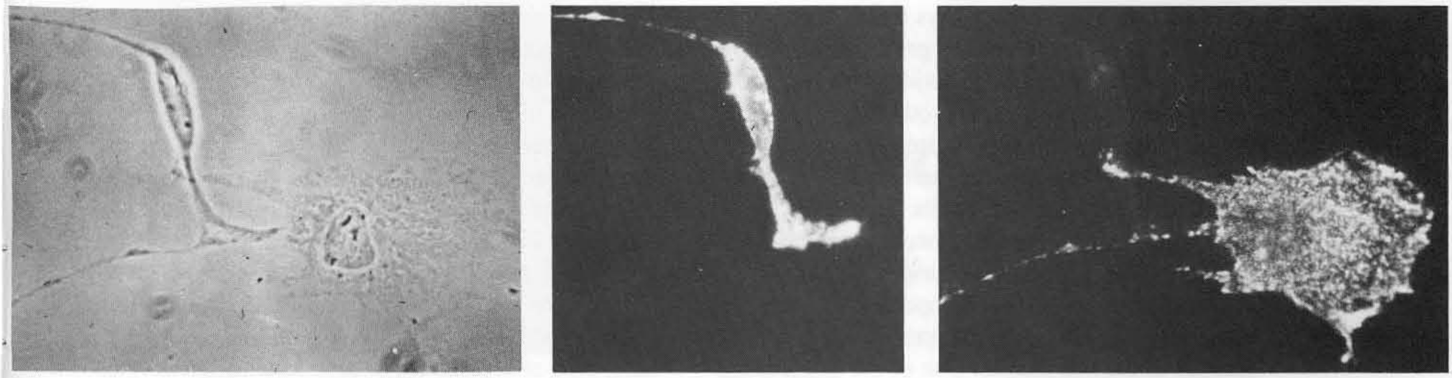
So what does this mean? We do not understand in detail, but some have suggested that at a critical early stage in adolescence an infectious agent, for example a virus, may enter the nervous system. The infectious agent may lie low in the nervous system for ten years or more and only express itself at a much later stage when the first symptoms are evident. The problem with this is that the only decisive proof of such a hypothesis would be to actually isolate and characterize the agent that is being invoked. Unfortunately, all efforts so far to isolate such agents have either been unsuccessful or have not been reproduced in other labs.

There is a second very important aspect of understanding MS, and that is the role of the immune system. When we inject a mouse (the immunologists' favorite animal) with a foreign substance, the mouse's immune system recognizes it as foreign. The immune system responds in a characteristic way, so that if a week later we take a small sample of the blood from this mouse, we can detect components called antibodies. The antibodies bind tightly to the substance, the antigen, that was injected into the mouse. If we look in the white cells of the blood, we can detect cells which again specifically recognize the original antigen. The immune system is a very important aspect of our defense mechanisms to infection, but it is a prerequisite of a system of this sort that it should not recognize the mouse's own components as foreign. If it did so then the mouse's immune system would attack its own tissues with disastrous consequences. There is a set of diseases, called autoimmune diseases, where this happens; that is, our immune system recognizes our own components as foreign and attacks them, leading to their destruction. The circumstances surrounding the removal of the myelin sheath and the oligodendrocyte in MS strongly suggest some autoimmune involvement. It seems that the patient's own immune system participates in the destruction of myelin in the central nervous system.

How would one put this idea, which invokes a role for the immune system, together with the previous suggestion about an infectious agent? If an infectious agent were to enter the nervous system around adolescence, to lie low for about ten years or more, and then put its head above water by expressing itself in the oligodendrocyte, then the immune system might attack the oligodendrocyte and the myelin sheath because it recognizes them as foreign. This is a hypothesis that quite a number of people find attractive, but it is not yet established in any crucial respects. In particular, the identity of the infectious agent that is postulated in such theories is as yet unknown.

It is often very difficult in medicine to achieve a complete chain of causation, that is, to completely understand what gives rise to a particular disease state. In fact, there are several examples where diseases have been cured without knowing such a complete chain of causation. One example is diabetes, which has been effectively treated by modern medicine without understanding the fundamental defect in the pancreas. In the case of MS, the presence of the intact axons in the vicinity of the MS plaque encourages us to think that it would be good to try to promote remyelination in order to reinsulate the axons. Alternatively, we might try to find circumstances that would enable

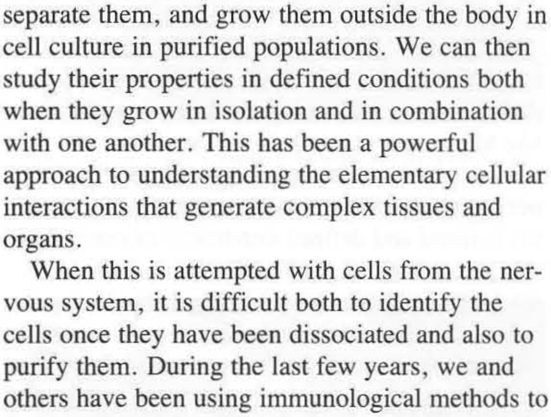
Reprinted from an article by J.P. Brockes, K.L. Fields, M.C. Raff in Nature, vol. 266 (1977).



These two cells from a rat's peripheral nerve are shown growing in tissue culture. The cells have been reacted with fluorescent antibodies and are seen at left under visible light. Under ultraviolet light the fluorescent antibodies can be distinguished, and the spindle-shaped Schwann cell (center) would appear red, while the other cell (right), which is not derived from the nervous system, would appear green.

the axons to recover the conduction of the nerve impulse. One problem with such aspirations is our ignorance of the basic cell biology of the elements that make up the MS plaque — the oligodendrocyte, the astrocyte, and the nerve cell. The complex architecture of the brain makes it very difficult to understand the elementary interactions between cells which give rise to that architecture. A variety of experience has told us that in such situations where we are faced with a complex biological organization, the most powerful analytical approach is to dissociate the tissue into its component cells, and to take these cells, separate them, and grow them outside the body in cell culture in purified populations. We can then study their properties in defined conditions both when they grow in isolation and in combination with one another. This has been a powerful approach to understanding the elementary cellular interactions that generate complex tissues and organs.

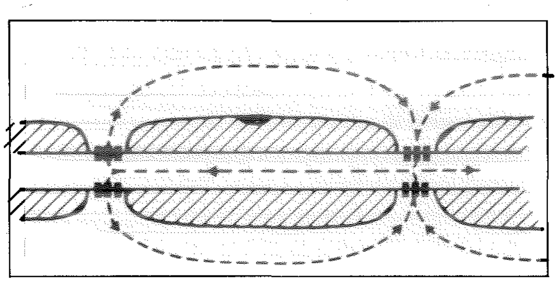
When this is attempted with cells from the nervous system, it is difficult both to identify the cells once they have been dissociated and also to purify them. During the last few years, we and others have been using immunological methods to achieve this end. Although the immune system is a problem in the context of autoimmune disease, it can also be considered in a more beneficent mould — that of giving us substances that we can use to identify and purify cells. The principle of this is very simple. The picture below shows two cells that are different, and one of the ways in which they differ is that they are made of different components shown here by a triangle and a semicircle. We are particularly interested in components like these that lie on the surface of the cells. What we can do is to inject these cells into animals, and the animals' immune system will recognize the triangle or the semicircle as foreign and will make antibodies that bind specifically to them. The antibodies are represented as binding to the two components. We can label these antibodies with different dyes so that they can be distinguished when they are irradiated with ultraviolet light. So what we are doing, in essence, is to use the great discrimination of the immune system to make the distinction between different cell types.



Given that we are able to use antibodies in this way to identify and purify cells from the nervous system, what can we do with the populations that result? The cell type with which we have had most success is the Schwann cell, and the availability of pure populations of these cells has provided a number of new opportunities. One focus of our efforts has been to investigate the process of myelination in a more analytical way. We are now able to separate the two cells that give rise to the myelin sheath — the nerve and the glial cell. They can be studied in isolation and together in culture. One of the insights from this work is that the interaction between the two cells is more complicated than we might have thought. The Schwann cell is not only induced by the nerve to spiral around the axon; it also does not make the characteristic components of myelin unless instructed to do so by the nerve. We are trying to identify the signals that pass from the nerve cell to the glial cell and induce it to do this. The ability to separate the two cells and put them back together again under controlled circumstances may allow us to progress with this difficult problem.

Control of cell division is another aspect of our work. Cells increase in number by doubling certain of their critical constituents and then dividing in two. Cell division is obviously very important, and there are a number of ways that it is controlled in the body. When we grow the purified Schwann cells, they do not divide even under conditions which allow many other cell types to do so. There is a component present in the brain and in the pituitary gland at the base of the brain which makes the Schwann cells divide. We have been able to purify this component from the pituitary gland and to grow purified Schwann cells in its presence. This component also acts on astrocytes, the glial cells that proliferate in the vicinity of the MS plaque. One of the interesting things about it is that it seems to be a product of nerve cells. Nerve cells are distinguished by the fact that they do not divide, which is one reason why neurodegenerative diseases are so serious. Yet we have found that nerve cells apparently make a component which is able to stimulate other cells to divide. There is considerable indirect evidence that nerve cells play an important role in regeneration in a variety of contexts. The fact that we have been able to get hold of a defined component of this sort allows us for the first time to investigate some of these issues directly.

Another focus of our research is the voltage-sensitive component mentioned earlier that is located in the nerve cell membrane and is responsible for generating the nerve impulse. In the diagram of a myelinated nerve process below, the myelin is represented by cross-hatching, and one segment is laid down by each glial cell. Before we reach the next segment there is an area of bare nerve membrane, which is called the node. It has been found in the last few years that essentially all of the voltage-sensitive components that generate the impulse are located in this bare membrane between adjacent myelinating glial cells. None of them are found in the nerve membrane underlying the glial cell. This is something that makes very good sense for rapid conduction in a myelinated axon. When the axon is demyelinated, however, the distribution is inappropriate, and this is the reason why conduction fails. It would be much better if the distribution were uniform after the myelin was removed. We have very little understanding of the factors that control the presence of this component in the nodal membrane. In the last few years, our colleagues in the chemistry division at Caltech have made considerable progress in removing this component from the membrane and purifying it. During the last 18 months we have joined with them and have been able for the first time to make antibodies that recognize this component. These antibodies should allow us to investigate those factors responsible for localizing the channels in the node. We plan to explore this issue by using the defined culture systems that I discussed earlier. One obvious question that we cannot answer at the moment is whether the glial cell plays a role in segregating the channels to the node or whether the nerve cell is able to put them there in an autonomous way.



In this diagram of successive segments of myelin laid down around a nerve axon, the myelin is crosshatched and the nucleus of the glial cell is shown as a black semicircle. The voltage-sensitive gates represented by black rectangles are localized at the node between successive segments. The dotted lines and arrow indicate the flow of current through the gates and into the axon.

All of these issues are relevant to the environment of the MS plaque. We would like to understand how to induce cells to remyelinate, and we would therefore like to identify those signals which the nerve cell normally passes to the glial cell to instruct it to make myelin. We would like to understand the factors controlling cell division in the vicinity of the plaque, and we would also like to promote the redistribution of the voltage-sensitive component that is responsible for the nerve impulse. These problems are best studied in the isolated and defined conditions of cell culture. The availability of purified Schwann cells also raises the long-term possibility of introducing them into the central nervous system to see if they are capable of remyelinating axons that have lost their myelin.

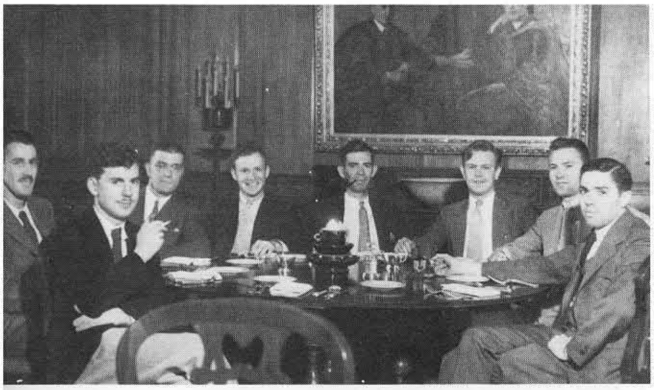
It would be irresponsible to minimize the difficulties associated with diseases like MS or to underestimate in any way the value of direct clinical studies. Nonetheless, at a time when support of basic research is rapidly dwindling, it would be equally irresponsible not to point out that such research may be directly relevant, and often in an unpredictable way, to the clinical problems that are under consideration. □

Acknowledgments

The research topics from my own laboratory that are discussed here have greatly benefited from the collaboration of Martin Raff, Kay Fields and Rhona Mirsky when I was in the MRC Neuroimmunology Project at University College London, of Lawrence Fritz, Karl Fryxell, Greg Lemke and Katherine Stygall in the biology division at Caltech, and of Hsiao-Ping Moore and Michael Raftery in the chemistry division.

Yesterday, Today, and Tomorrow

Physicists gather from far and near to celebrate Kellogg Radiation Laboratory's 50th birthday—and a good time is had by all.



Most of the Kellogg Round Table gang lunching at the Athenaeum in 1934. From the left they are Norton Moore, John Read, Wolfgang von Finklenburg, Henry DeVore, Richard Crane, William Fowler, Lucas Alden, and Walter Jordan. The group's insignia was the candlestick on the table, which was made from an insulator for high-voltage tubes.

Together for the 50th birthday of Kellogg last November, the authors of 1957's classic paper on the synthesis of elements in stars – Margaret Burbidge, Fowler, Sir Fred Hoyle, and Geoffrey Burbidge.

The new tandem accelerator, around which many of the future research efforts at Kellogg will be built.

Special quantized numbers have played an important role in 20th-century physics, and nowhere more than at Caltech's Kellogg Radiation Laboratory where a recent conference centered on the importance of 1981 — and 50, 70, and 0. The year 1981 is important because it marked the 50th anniversary of the founding of the laboratory in 1931 by Charles Lauritsen. The significance of 70 is that 1981 also marked the 70th birthday of William A. Fowler who is now Institute Professor of Physics but who began as a Kellogg graduate student with Lauritsen in 1933. Since then, Fowler has carved out a distinguished career, both for himself and for the laboratory, in nuclear physics and astrophysics. And the meeting focused on 0 because of the dedication of a new tandem accelerator facility, around which many of the future research efforts of the lab will be built.

When the Kellogg gang get together, it's a party, and this two-day-long fete consisted of one historical session that covered Kellogg's past, two reviews of the present status of nuclear physics and astrophysics, and — looking toward the future — the accelerator dedication. The meeting wound up with a half-session of presentations by members of the current Kellogg staff sketching the directions of coming research.

Robert Bacher, professor of physics emeritus, was chairman of the historical session, which opened with a talk by Charles Holbrow, professor

Edwin E. Salpeter (right) represents the National Science Foundation at the dedication of the new accelerator. At the far right, Crane passes the Kellogg gang's long-treasured candle holder on to Fowler in honor of Fowler's 70th birthday.

of physics at Colgate and a frequent Kellogg visitor. He described the events leading to the founding of the laboratory by Lauritsen and R. A. Millikan, and the negotiations with W. K. Kellogg that made possible its construction and initial operation. (A very readable account of this early period has been published by Holbrow in the July 1981 issue of *Physics Today*.) Life and research in the lab in the prewar and postwar eras were covered in talks by H. Richard Crane, now professor of physics emeritus at the University of Michigan, and by Fay Ajzenberg-Selove, professor of physics at the University of Pennsylvania, who has often visited Kellogg and who was a close collaborator with Thomas Lauritsen.

The speakers in the sessions on the present state of nuclear physics and astrophysics are distinguished leaders in their fields and also "associates" of Kellogg in the sense of having been graduate students, postdoctoral fellows, or visitors. All of the speakers who dealt with nuclear physics were, in fact, once Kellogg graduate students: R. G. Stokstad of the Lawrence Berkeley Laboratory, E. G. Adelberger of the University of Washington, A. D. Bacher of Indiana University, and P. D. Parker of Yale University. Chairman of this session was Rochus Vogt, who is also current chairman of Caltech's Division of Physics, Mathematics and Astronomy.

In the period immediately following World War II, the two Lauritsens, Fowler and Robert Christy recognized that the low-energy Kellogg accelerators were ideally suited to provide the basic nuclear physics data required to understand the processes of nuclear energy generation by stars and the synthesis of the chemical elements. The importance and successes of these efforts were quickly recognized, and they led to a steady stream of postdoctoral fellows and visitors who were eager to participate in this exciting research area. The first era of this research led to the classic summary paper, "Synthesis of the Elements in Stars," by Burbidge, Burbidge, Fowler, and Hoyle in 1957, based largely on the experimental measurements of Charles Barnes, Ralph Kavanagh, and Ward Whaling. Known both reverently and irreverently ever since as B^2FH, this paper has been the basis for what came to be called nuclear astrophysics, and the great majority of its practitioners have been Kellogg visitors at one time or another. This was certainly true of the participants in the conference session entitled "Nuclei, Stars, and Cosmology," which featured R.V. Wagoner of Stanford, Fred Hoyle of the University of Manchester in England, Stanford Woosley of UC Santa Cruz, E. E. Salpeter of Cornell, and — as chairman — J. N. Bahcall of Princeton.

As illustrated by nuclear astrophysics, the Kellogg Laboratory has been instrumental in initiating new directions in physics, the prime example being the beginning of nuclear physics research by Lauritsen and his students in 1932. In 1948 C. C. Lauritsen suggested the construction of an electron synchrotron at Caltech, a suggestion that was considerably augmented by R. F. Bacher and R. L. Walker and that led to the present particle physics efforts at Caltech. In 1963 Fowler and Hoyle began research on the consequences of the theory of general relativity for stars — research that eventually led to the widely recognized relativistic astrophysics research group at the Institute. In 1968 Fowler supported development of a new generation of high-precision, high-sensitivity mass spectrometers by Gerald Wasserburg, professor of geology and geophysics, and Dimitri Papanastassiou, senior research associate in geochemistry. By means of isotopic analyses on meteoritic materials, these mass spectrometers have permitted fundamental insights into the origin of the solar system and the materials from which our solar system was made. During the

1970s Thomas Tombrello led the group into such new directions in applied physics as the design of superconducting accelerators, the interaction of ion beams with solids, lunar and planetary science, and the monitoring of radon emanation from rocks as a possible means of earthquake prediction.

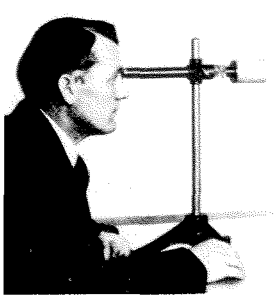
At present Kellogg is an exciting blend of the old and the new, as well as the pure and the applied, but it has no intention of resting on its laurels. This was demonstrated by the dedication of the new accelerator and the final conference session entitled ''The Beginning of the Next 50 Years.'' The chairman of this session was C. A. Barnes, Caltech professor of physics, under whose direction the accelerator was purchased and installed. The senior scientists speaking were Wasserburg and Tombrello, but some of the younger members of the Kellogg staff were also featured: Robert McKeown, who is assistant professor of physics; Steven Koonin, professor of theoretical physics; and Barbara Cooper, research fellow. Cooper described one of the first experiments planned for the new accelerator, a search for fractionally charged particles, which is a problem of fundamental importance to contemporary physics.

The highlight of the accelerator dedication, whose chairman was R. F. Christy, professor of theoretical physics and sometime Kellogg collaborator, was a christening of the instrument by Fowler. Thanks to an uncooperative champagne bottle, this ceremony required not one but two mighty blows. Speeches, of course, always go before action, and this occasion was no exception. Prefatory remarks were made by President Marvin Goldberger speaking for the Institute, President Emeritus Lee DuBridge in behalf of the Trustees, H. H. Barschall from the National Academy of Sciences, Rochus Vogt for the phyiscs division, and C. A. Barnes as the principal investigator for the accelerator project.The accelerator was purchased with financial support from the National Science Foundation, which was represented by E. E. Salpeter, and — appropriately — the W. K. Kellogg Foundation, represented by Robert E. Kinsinger. A special spot on the program was reserved for Admiral J. T. Hayward, who is now retired from the U.S. Navy. Admiral Hayward was experimental officer at the Naval Ordnance Test Station at China Lake, California, and worked with the Kellogg Rocket Project during World War II, and he was instrumental in obtaining support for the laboratory from the Office of Naval Research in the period 1946-65.

There was no danger that all work and no play would make the Kellogg birthday participants dull boys – or girls. At the informal party on the last night of the conference Fowler and Fay Ajzenburg-Selove demonstrated how to cut a nonexistent rug. A reconstituted band played the music, and everything wound up with group singing led by Fowler.

So much for physics. Charles Lauritsen believed, however, that, just as physics should be fun, physicists should have fun. Both precepts have been honored at Kellogg. A gala evening birthday banquet was held for Fowler, with Wasserburg in his true (though perhaps previously unsuspected) element as toastmaster. He was more or less aided by the perceptive remarks of Margaret Burbidge, Andy Bacher, Peter Parker, Ardiane Fowler, Fred Hoyle, and Charlie Barnes. Altogether fitting was the presentation to Willy by Dick Crane of a candle holder made from an insulator from one of the original Lauritsen high-voltage tubes. This prized antique had been used as an Athenaeum table centerpiece by Crane and Fowler and friends during their graduate student days.

Banquets are somewhat formal, so a second evening was given over to an old-fashioned Kellogg party in the informal tradition of C. C. Lauritsen. It featured dancing to a hastily reassembled, but highly proficient, Kellogg band with Barnes at the piano, Stokstad on the guitar, Cary Davids of Argonne National Laboratories on the trumpet, and Vic Ehrgott, emeritus member of the Kellogg staff, on the sax. Both evenings closed with singing led by, who else but, Fowler himself, and all the traditional Kellogg songs were included. □



Phyphty Years of Phun and Physics in Kellogg

by Willy Phowler

The man who began it all – Charles Lauritsen – eyes the title of this condensed version of a recent talk given by his one-time graduate student, who is now the Institute Professor of Physics and who is also known as William A. Fowler.

This story is based on personal reminiscences. After all, I have been in Kellogg for 48 of its 50 years, so please forgive me if I appear on the scene from time to time. The hero of my story is Charles Christian Lauritsen (1892-1968), who was lured to Caltech by the siren call of Robert Andrews Millikan in 1926. Charlie worked with Millikan on the temperature independence of the cold-emission of electrons from metals and received his PhD in 1929.

Charlie, Ralph Bennett, and Benedict Cassen, with Richard Crane joining Charlie as a graduate student in 1931, then went on to develop a high-potential X-ray tube that could operate at the one million volts provided by the AC transformers in the old High Voltage Laboratory — now the Sloan Laboratory — at Caltech. The transformers had been installed by the Southern California Edison Company to test insulators, circuit breakers, and other equipment on long distance power lines such as the one from Hoover Dam to Los Angeles.

Springing from the successes in the High Voltage Lab, the Will Keith Kellogg Radiation Lab was built for the use of Charlie's high-potential tubes in cancer therapy and for study of the physics of high-energy X rays. One of the consequences of the latter activity was Charlie's demonstration with John Read that the energy dependence of the scattering of photons by the Compton effect was described accurately by the Klein-Nishina formula, which incorporated electron spin, rather than by the Dirac-Gordon formula, which did not.

Then came 1932 — the golden year of classical nuclear physics. Urey discovered deuterium, Chadwick discovered the neutron, Anderson discovered the positron, and Cockcroft and Walton, using an accelerated proton beam, succeeded in disintegrating nuclei below the Coulomb barrier as predicted by George Gamow in 1928. With one million volts of alternating potential available, Charlie and Dick Crane began — and completed before the end of 1932 — an ion beam tube in the High Voltage Lab. They used two large porcelain insulator bushings left over from the power-line-testing days.

Another of Charlie's great inventions — the Lauritsen electroscope — consisted of a fine quartz-fiber about five millimeters long with a one millimeter cross hair on the end that could be viewed against a marked scale with a low-power eyepiece. We all carried a fountain-pen version for measuring personal exposure to radiation. In its most frequently used form, it was mounted inside a thin aluminum cylindrical chamber two inches in diameter and three inches long, and it was charged electrostatically via friction by turning a small knob on the side a few times.

The electroscopes were calibrated using standard gamma-ray sources, even then available from the National Bureau of Standards. When measurements started with the million-volt ion tube, these electroscopes were all that was available for detection in the laboratory, and they were superb for running excitation curves against beam energy. With a thin lead wall either inside or outside the aluminum, they were excellent detectors of the Compton electrons and pairs produced by the several-million-volt gamma rays produced in nuclear disintegrations. Lined with a thin layer of paraffin, they were an ideal neutron detector. Crane, Lauritsen, and A. Soltan (a Rockefeller Fellow from Warsaw) put helium into their ion source, bombarded a beryllium target, and were the first to produce neutrons in an accelerator using the same reaction that Chadwick had used. Thanks to the fact that Soltan was a French-speaking (and writing) Polish count, they got quick publication for their first paper in *Comptes Rendus*, thus managing to beat their competitors, Ernie Lawrence and company at Berkeley and Tuve and Hafstad at, of all places, the Department of Terrestrial Magnetism in Washington.

I joined the team as Charlie Lauritsen's graduate student in 1933 at the same time as Lewis

Delsasso. To all of us he was "Del," and it is my recollection that he was the one who pinned "Willy" on me. He was an "older man," as graduate students go, and was already married with a family of three. Charlie put the two of us to work building a cloud chamber, actuated by a sylphon bellows, and supplied with a uniform magnetic field by a pair of Helmholtz coils. We both did our theses with this somewhat Rube Goldberg-like device that had cam-operated switches for turning the illuminating light on and off, expanding the chamber, opening and closing the camera shutter, and recompressing the chamber — all on a 30-second cycle. We eventually took 100,000 pictures with that darn thing and reprojected and studied every one of them. The early result was a series of about ten papers by Crane, Delsasso, Fowler, and Lauritsen that cluttered up the *Physical Review* from 1933 to 1935.

Del was an exquisite technician and superb experimentalist; his contributions at Aberdeen Proving Grounds and White Sands during and after World War II capped an all-too-short career. But he had trouble with the theoretical courses all the graduate students had to pass in those days. He had already flunked Fritz Zwicky's course in Analytical Mechanics several times, but he took the course once again when I took it for the first time. Fritz believed in the Socratic method of teaching. He lectured infrequently but most often sat in the back of the class and sent one of us to the board to answer questions that literally came out of the blue. We were allowed to take the textbook with us, and we could search through it for help. The textbook was Webster's *Dynamics*, and Del's copy was well worn and tattered. When Del was asked in turn to go to the board, he took this old book with him, and would try to leaf through it with pages falling on the floor. We all cringed. Del never answered a question all term, but, lo and behold, at the end of the term he passed. Those of us who knew of his great ability in the lab were pleased and decided to call on Zwicky to find out what principles of teaching we could glean for our future use. We put the question to Fritz. He glared at us, waved his arms, and replied, "Vell, Gottdammit — I had to pass him; his book wore out."

My thesis, published with Del and Charlie, was an experimental one but concluded with a fairly significant theoretical conclusion. There is a story behind that. We measured the curvature in the magnetic field and thus the momentum and energy of the positrons from ^{11}C, ^{13}N, ^{15}O, and ^{17}F. Franz Kurie was doing the same thing at Berkeley, and he developed the famous Kurie plots. We noticed that the energy end-points of these radioactive nuclei progressively increased. When we called this to the attention of Robert Oppenheimer and Robert Serber, they pointed out that this progression was the effect of Coulomb energy increase along the series and that our results showed the existence of mirror nuclei and the charge symmetry or equality of proton-proton and neutron-neutron forces. That went into my thesis, thanks to the two Roberts.

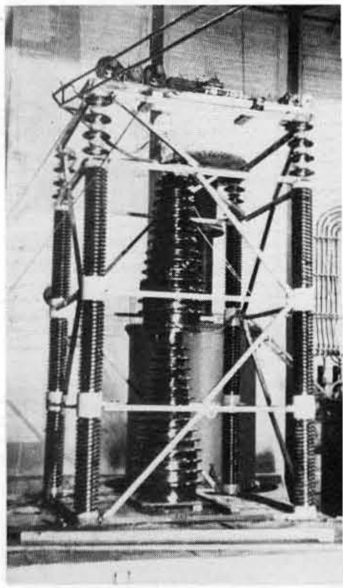
Robert Oppenheimer — "Oppy" — was the great man in theoretical nuclear physics in those days. He had a joint appointment at Berkeley and Caltech and came to Pasadena every year for the spring quarter. No other theorist at Caltech except Richard Tolman gave a hoot about what we were doing. Richard was a benign observer of what went on in Charlie's lab, and he and Charlie and Oppy were close friends. Many times they sat after lunch in some old weatherbeaten wicker chairs in the sun outside the High Voltage Lab discussing the great happenings of the day in physics.

Charlie had three other graduate students in those early days — Wilson Brubaker, Walter Jordan, and Louis Ridenour. Before the start of World War II he graduated nine more — Kamal Djanab, William Stephens, Thomas Lauritsen, William McLean, Frank Oppenheimer, Robert Becker, John Streib, Charles Sheppard, and Everett Tomlinson.

Charlie did more than guide our graduate careers. He taught us how to use a lathe, how to bring the mercury back down in the stem of a Macleod gauge by gently tapping without breaking it, how to outgas the vacuum tube after repairing a leak by painting it with shellac, and a million and one other practical things in the nuclear lab of those days. But he taught me much more than that. When I complained that the hours were too long, day after day, he said "Stop complaining; what if you had to work for a living?" When I thought some of the problems we worked on were just too hard, he said, "If it were easy, someone else would already have done it."

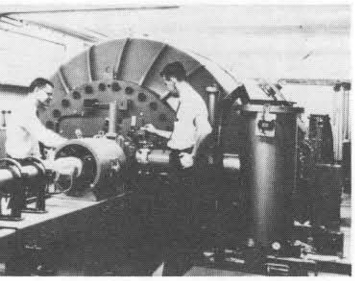
Actually doing experiments in nuclear physics in those halcyon days in the mid-thirties was much easier than it is now. We didn't have to do much planning or thinking about the future. When one experiment was done, we picked a new target, decided what to bombard it with, measured the activity produced as a function of peak bombarding energy with the electroscope, or ran the cloud chamber for a few days or a few weeks, measured the tracks, and wrote a paper. It all changed after the war.

Charlie helped us learn about X-ray and nuclear physics as well as about plumbing. He held



An ion beam tube (above) was built in the High Voltage Lab by C. C. Lauritsen and Dick Crane in 1932. What is now called the "experimental area" was located in the concrete block house beneath the tube. Count Soltan (below) is in this area peering at a Lauritsen electroscope next to the target, which is behind lead shielding.

Tommy Lauritsen teaching a class sometime in the 1960s.



Tommy Lauritsen with Bob Christy and Kellogg's first tandem accelerator.

a weekly seminar every Friday night. Dick Crane remembers the graduate students plowing through Siegbahn's book on X-ray spectroscopy. When I came in 1933, we started on Rutherford, Chadwick, and Ellis, and each graduate student led the weekly discussion, chapter by chapter. This was interspersed frequently by journal club reviews of exciting new papers as they were published. But there was time left over for relaxation. After the seminars, we repaired to Charlie's home, where he served drinks and Sigrid, his wife and a practicing doctor of radiology in her own right, served refreshments. Young Tommy Lauritsen played the piano, Charlie played the fiddle, and we sang sentimental songs.

I'll never forget when I first came to realize what it really meant to work in Kellogg, which was named for the Corn Flakes King. Millikan had inaugurated dinners for The Associates at the Athenaeum, and faculty and even postdoctoral fellows were invited. On one occasion I was placed next to a little old lady from Pasadena. (Little old ladies in tennis shoes was the usual description, but Millikan invited only those whose tennis shoes were studded with diamonds.) In good time she turned to me and asked, "Young man, what do you do at Caltech?" "Madam, we bombard lithium with protons and produce alpha-particles in nuclear disintegrations." The look on her face told me that she may have heard of lithium, had probably never heard of protons, and certainly had never heard of alpha-particles. I tried again with, "Madam, what we are doing is popularly known as atom busting." "And where do you do this, young man?" "In the Kellogg Laboratory," I replied. "Oh," she said, "now I know what you do — you puff rice."

Tommy Lauritsen became a graduate student under his father in Kellogg in 1936 and received his PhD in 1939. We came to realize in 1936 that the days of an alternating potential tube were numbered, so we had to do some planning and building for the future. Thus Tommy's doctoral work was mainly the construction in collaboration with his father and me of a 1.5 MV electrostatic accelerator housed in a pressure vessel. The new accelerator was put into use early in 1938. It operated continuously, except during World War II, until 1980.

With this new accelerator we were able to produce high-resolution, precise excitation curves for a number of targets primarily bombarded by protons. Charlie and I were the only faculty members before the war, joined later by Tommy. However, when I say "we" from then to the start of World War II, I refer to the graduate students and visiting associates, the earliest being Hans Staub and Tom Bonner. That was it. Before the war, fewer than 20 people were involved in research in nuclear physics in Kellogg. Kellogg was one of the first team operations in physics, but the team was small, especially compared to Berkeley.

Early in 1939 Hans Bethe described the operation of the CN-cycle in heavier main-sequence stars in a letter and a full paper in the *Physical Review*. For those of us in Kellogg this was a dramatic event in our lives. What we were doing in the lab had something to do with the stars. There we were in one of the great astronomical centers of the world with the 100″ telescope in operation on Mt. Wilson and the 200″ Palomar mirror already under way. There was no way to go but up — up to the stars. There was a long delay, however. World War II came along. By August 1941 we were developing and producing rocket ordnance, primarily for the U.S. Navy, and creating an organization of several thousand people, most of whom were transferred at the end of the war to the Naval Ordnance Test Station (now the Naval Weapons Center) at China Lake.

The war finally came to a nuclear end in August of 1945. Charlie spent some time in Washington assisting Admiral Robert Conrad and others in establishing the Office of Research and Inventions, which soon became the Office of Naval Research. He decided under some counterpressure not to involve Kellogg or Caltech in further work in rocket ordnance, and by mid-1946 we had transferred all war work to NOTS, China Lake. We were virtually alone in Kellogg until a swarm of new graduate students returned in the fall of 1946.

Charlie made some deliberate decisions. First, Kellogg was to remain part of the Division of Physics, Mathematics and Astronomy and not become a separate lab with a director and any administrative bureaucracy. All permanent Kellogg staff were to be faculty members with tenure.

Second, we resolved to stay in low-energy, light-element classical nuclear physics using electrostatic accelerators equipped with high-resolution electrostatic and magnetic analyzers, with double focusing magnetic spectrometers, and provided with electronic detectors, photo-multipliers, and all the other marvelous gadgets that came out of war research. Eventually an 0.7 MV high-current accelerator and a 3 MV accelerator were built in-house, and a 6.5 MV EN-tandem accelerator was provided by the ONR. We resolved to concentrate on those nuclear reactions thought to occur in stars, not only in the CN-cycle but also in the proton-proton chain of Bethe and Critchfield. In this we were greatly encouraged by Ira Bowen, who had been our colleague before the war, directed all our high-speed rocket photography during the war, and became director of Mt. Wilson soon after the war.

Third, with Oppy's departure to the Institute of Advanced Study, we realized that we urgently needed a theoretical nuclear physicist in Kellogg. Oppy recommended Robert Christy, and Bob came to Kellogg in 1946. He brought a keen appreciation of the relation between experimental and theoretical physics and contributed in a most significant way to the development of postwar Kellogg.

Finally, we needed some money, and so on June 19, 1946, we submitted a three-page proposal to ORI (even before it became ONR) for $94 thousand to fund research in nuclear physics and astrophysics for one year for four faculty members, one senior research fellow, eight to ten graduate students (we hoped), and one engineering draftsman, one electronic technician, one instrument maker, one machinist, and one secretary. Small in some ways, but lots of people to be supported on 94 kilobucks.

That proposal concluded with an interesting commentary on the perceived problems of those times. The final paragraph was entitled "Publication of Results and Nature of Research." Here it is in full:

> It is essential that all results of these studies be in the "unclassified" category and that publication in scientific journals and announcements to scientific societies be permitted at all times. It is also essential that it be clearly understood that these studies are primarily fundamental in nature and that the possibility of applications is of secondary importance. Finally a clause must be inserted in any contractual agreement guaranteeing the Institute the right to purchase from the Government all equipment provided under the contract.

Kellogg entered a new era. The faculty grew to include Ward Whaling, Charles Barnes, Ralph Kavanagh, Tom Tombrello, Steve Koonin, Peter Haff, and Robert McKeown. They and their students and postdoctoral fellows deserve all the credit for Kellogg today. Closely associated with Kellogg have been Jim Mayer and Jim Mercereau, and still closely associated are Don Burnett, Kip Thorne, and Jerry Wasserburg, all of Caltech. It has been hard to match their accomplishments.

There is no room to discuss what has transpired since 1946, but it was all given in a wonderful talk presented by Fay Ajzenberg-Selove at the Kellogg 50th birthday celebration. Her talk and one given by Dick Crane are available in the Caltech Archives, and I am indebted to both of them for some of the things I have been able to put down here. Let me give you, for example, a few facts from Fay's talk about Kellogg activities. She pointed out that prior to 1942, 16 PhDs graduated from Kellogg, and since 1947, 147 individuals have received Kellogg PhDs from Caltech. Before 1950 a total of 8 postdoctoral fellows or visiting faculty were given appointments at Kellogg for varying lengths of time. Since 1950, almost 400 such appointments have been made.

Fay checked up on our publication record since 1946 too and came up with a total of slightly more than 2000 papers, including all publications whether published in scientific journals, in books, in conference proceedings, or in popular magazines. And she included publications written by members of the extended Kellogg groups while on leave at other institutions and the publications written by visitors, research fellows, and visiting associates while they were at Caltech.

That's just about the end of the line. We continued to work hard, sometimes under difficult and complicated circumstances, but we continued to have fun trying and sometimes succeeding. We have high hopes for the future in using our newly dedicated high-current, high-resolution 3-MV tandem accelerator. It was lowered into place in September and, because of its paint job, was promptly christened the Yellow Submarine.

Good laboratory facilities lead to good physics. Charlie Lauritsen believed that all his life. The Kellogg gang has grown considerably since Charlie's time; it still has fun doing good physics. □

***A** recent picture of most of the Kellogg gang.*

Research in Progress

Animal Magnetism

Joseph Kirschvink is interested in the birds and the bees and does research on animal magnetism — but in its most literal sense. An assistant professor of geobiology, Kirschvink has discovered tiny amounts of magnetite (Fe_3O_4), also known as lodestone, in the tissues of honeybees, pigeons, tuna, and sea turtles, among others, adding to a growing list of animals that apparently manufacture the mineral biochemically. The discoveries have lent weight to the hypothesis that the orientation behavior of many of these animals, which is known to be geomagnetically influenced, is due to the presence of an internal compass. (Human beings are another story — see Random Walk in this issue.)

Recently magnetic material has shown up in another surprising context. Kirschvink, working with Frank L. Tabrah and Stanley Batkin from the Cancer Center of the University of Hawaii School of Medicine, found anomalously high concentrations of what they think is magnetite in mouse tumors. They worked with two types of tumors — YC-8 lymphoma and Lewis lung tumor, freezing sample tissues to immobilize the magnetite and inducing magnetism in them by briefly exposing them to a strong magnetic field. They then measured the magnetism in the samples with a highly sensitive superconducting magnetometer. Kirschvink, Tabrah, and Batkin found the equivalent of one to five crystals (about one-tenth of a micron in size) per cancer cell in the mouse tumors. Normal distribution of magnetite crystals, which have been detected in the tissues of monkey brains and human adrenal glands, is one per 100 to 1000 cells.

The scientists also exposed the cultured mouse tumor cells to varying magnetic fields to determine whether their growth rates would be affected. While the Lewis lung tumor cells showed no growth response to the fields, the YC-8 cells did. Exposing these cells to a 2000 Hertz oscillating magnetic field accelerated the cell growth, while exposure to a 60 Hertz rotating field produced by a spinning permanent magnet retarded growth significantly. Further research is needed to understand this phenomenon, but Kirschvink theorizes that the rotating magnetic field produces damage to the cells either by mechanically torquing the magnetite or by inducing damaging currents.

Whether this might have any bearing on cancer treatment is still unclear. The scientists also studied three types of human carcinomas and did not find unusually high concentrations of magnetite. These tumors do not grow as quickly, however, and since there are also more than 100 different kinds of human tumors, much research remains to be done to determine the possible significance of magnetite in cancer.

Also still unknown is the precise location in the cells and the biological function, if any, of the magnetic crystals. They might simply represent some form of iron storage, but many tumors have high concentrations of the iron storage protein ferritin and would not need magnetite for that purpose.

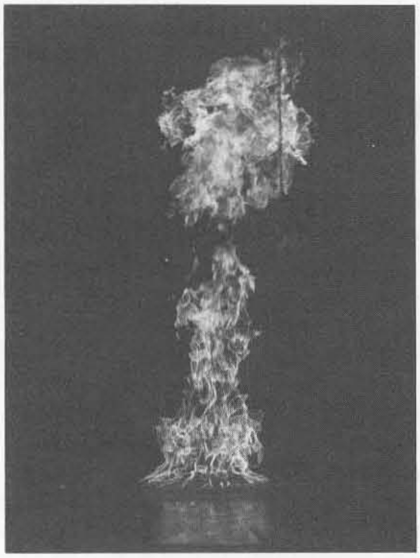
Caltech will soon have its own clean laboratory for research on ferromagnetic material in animal tissues. Since Kirschvink is dealing with nanogram (one-billionth of a gram) quantities of material, background contamination is a big concern; even a few specks of dust could be more magnetic than some tissues. Located in the subbasement of Arms Laboratory of the Geological Sciences, it will be sheathed with 4000 pounds of soft transformer steel, magnetized to cancel the earth's magnetic field. □ — JD

Where There's Smoke

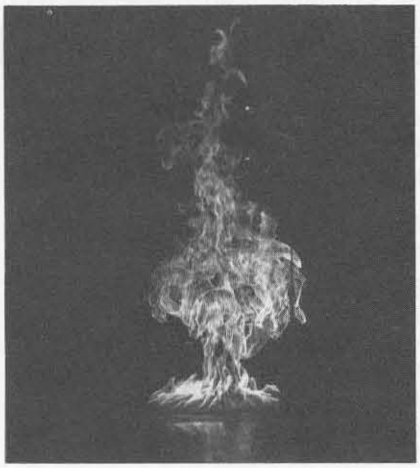
A fire starts in one room of a house. Hot gas produced by the flame mixes with fresh air as it rises and forms a distinct layer under the ceiling. As the layer becomes deeper, the hot gas also starts to flow out under the door lintel into the next room. The combustible materials in the first room, meanwhile, are heated by radiation from the flame, and they begin to decompose into gaseous fuel. Sometimes this fuel is ignited almost simultaneously in all parts of the room and causes the entire room to burst into flame — flashover.

For several years Edward Zukoski, professor of jet propulsion and mechanical engineering, and Toshi Kubota, professor of aeronautics, have been studying the fluid dynamics of the early stages of the fire-spread process in order to be able to predict the motion of combustion products before the flashover point. An understanding of the many interacting physical processes occurring in the fire has become particularly urgent because the increasing use of plastics as building and furnishing materials has introduced a host of fire safety problems, which present building codes and safety regulations don't handle very well. These problems arise in part because of differences between the combustion of natural materials, such as wood, and the new plastics. While burning wood forms a char layer that retards decomposition, heat causes plastics to disintegrate (or pyrolize) at low temperatures. Thus large masses of gaseous fuel can be added to a fire in a relatively short period of time. Since almost all of the combustion actually takes place in the gas phase, this rapid pyrolysis can cause a fast buildup of a fire.

In the early stages of a room fire, when the hot gases are gathering under the ceiling, the properties of this well-stirred smoke layer can be considered roughly homogeneous. This fact has formed the basis of a two-layer model for the description of fire spread. For example, Zukoski's group, which includes graduate students Baki Cetegen and William Sargent, has divided a room into two zones or homogeneous blocks of space — a hot layer near the ceiling and a cooler zone



When a vortex-like puff that has risen to the top of the flame burns off (left), the top of the flame drops to the next puff.



near the floor. Equations for the conservation of energy and mass can then be used to follow the temperature and vertical extent of both zones as a function of time.

Their computer model of the behavior of these two zones is based on observations and measurements of room fires in the laboratory. They are not, of course, tracking billowing smoke through the halls of Karman Laboratory but use an experimental "room," 4′ x 4′ x 8′, with fireproof walls and a large 8′ x 8′ hood to remove products of combustion from the laboratory. They set their fires with natural gas — a mixture of hydrocarbons that is predominantly methane.

In their experimental setup for studying the fire plume, the researchers control the level of the interface of the two zones with the large hood placed above the fire. The hood fills with hot gas from the plume to form the upper layer, and hot gas is drawn out from the upper zone through the side of the hood. When these flows are in balance, a stabilized interface height is maintained, and measurement of the flow rates between zones is possible. The rate at which the products of combustion diluted with air enter the hot zone is critical to the growth and spread of fire.

Mass transfer between the layers takes place at three points: at the interface of the layers, where the fire plume plunges into the upper layer, and at the fire plume itself. The latter is the most significant of these processes, say Zukoski and his colleagues; the fire acts as a pump, pulling in the cooler air, mixing and heating it, and carrying it to the upper zone. The rate of production of hot gas and the temperature of the gas depend heavily on the rate at which this entrainment happens.

Besides measuring this entrainment rate, the investigators have studied the flame itself and the heat transfer processes produced by the plume. The flame geometry has been recorded on videotape (30 frames-per-second), in still photographs, and with a shadowgraph system. A clear pattern of three different zones within the fire plume emerged from the pictures. At the base, a column of flame the size of the perimeter of the burner could be seen, and above the top of the flame, a typical bouyant plume of hot gas. In between, the pictures showed the presence of periodic pulsations, or puffs, in the flame — vortex-like structures that rose to the top of the flame and disappeared, presumably because combustion was complete. Then the next lowest puff became the top of the flame. Zukoski and Kubota believe that these complicated turbulent puffs play an important role in the entrainment of cool air from the room into the fire.

The computer model of the entrainment process — a simple numerical program that can be applied when the heat release rate is known — is virtually completed. The model is being modified to include a calculation of the rate of convective heat transfer produced by the plume. Zukoski and Kubota view their model as one element in a more ambitious calculation that will lead to the prediction of the spread of fire and combustion products through a complex building. They believe it will be useful in assessing accurately the safety of architectural design and new materials, and in the future the model may be used as part of a fire code. The research is funded by the Center for Fire Research of the National Bureau of Standards. ☐ – *JD*

Charles F. Richter – How It Was

Charles F. Richter, professor of seismology emeritus since 1970, has been considered a master interpreter of earthquakes for most of the last 50 years. This may seem somewhat unusual because he graduated from Caltech with a PhD in physics. It was the opening up of a research assistant's job at the newly established Seismological Laboratory in 1927 that deflected him, and one result, in 1935, was his first public enunciation of a way to measure the magnitude of earthquakes. Richter's name has since become a household word in seismology – and grist for the mill of the Oral History program of the Caltech Archives, for which he was interviewed by Ann Underleak Scheid. E&S *presents here an excerpt from the transcript of those interviews that describes his background and some of what led to the development and worldwide use of the "Richter Scale."*

Ann Scheid: Let's start with some of your background, your childhood and early life.

Charles Richter: To begin with, the name Richter is actually my mother's maiden name, which she resumed after a divorce, and I have never been known by any other name. It is, of course, the name of my maternal grandfather, to whom I owe practically everything I am in terms of support and education. My great-grandfather Richter was a brewer in Germany, Baden-Baden. He became involved in the political disturbances of 1848 and had to leave Germany in a hurry, bringing his small son who was then about four years old — my grandfather, Charles Otto Richter. The family was at first in New York, and not long before the Civil War they moved to Richmond, Virginia. In later years my grandfather was with a large firm manufacturing stationary engines at Hamilton, Ohio. He owned a farm and house about seven miles from Hamilton, and that is where I was born. The family, which then included only my grandfather, mother, and myself with an older sister, moved to California in 1909.

AS: So you went to school in Los Angeles?

CR: Yes, a short time in the public schools and then at the age of 12 I entered the preparatory school for USC, which was at first Southern California Academy and later University High School. Still later it was discontinued. I owe it a considerable debt for a very solid foundation in elementary mathematics, in which, it turned out, I had some ability, and consequently it more or less affected my subsequent education and career.

I should explain that my first scientific interest was in astronomy, and for many years I had the idea that I would eventually be going into that. It only came about later that there was a shift, and I went through a progression of chemistry to physics, theoretical physics, and, of course, the entry into seismology was more or less of an accident.

AS: You went to college?

CR: My first year of college was as a freshman at USC, but from there I transferred and went to Stanford. There, as I mentioned, I took a chemistry course first, and that didn't seem to be satisfactory. Gradually I got into physics, which was more congenial. I think one of the deciding factors was merely that at that time I was quite nervous and tended not to be neat, particularly with my hands, and this is fatal in a chemistry laboratory. After some unfortunate experiences, I felt it wasn't for me.

AS: You finished Stanford at quite an early age. How old were you?

CR: Twenty.

AS: And then you came back to Los Angeles?

CR: Well, I did finish my AB in physics, and then I found other things to do, and in particular employment. My first job was as a messenger boy at the Los Angeles County Museum. After that, I was for a couple of years working in a warehouse for the California Hardware Company in Los Angeles. That will account for the years about 1920 to 1923. By 1923 the former Throop Institute of Pasadena was reorganized as Caltech, and Dr. Millikan came to take charge and also to lecture. Of course, with my interest in physics, I couldn't miss the opportunity to hear his lectures. The result was that very soon I gave up my employment and entered Caltech as a graduate student. Eventually I became Paul Epstein's student, and I owe a very great deal to him.

AS: Would you describe Epstein a little bit, as a person, as a lecturer, as a teacher?

CR: He was a very beautiful lecturer in that his lectures were always carefully planned and organized. He had a number of odd mannerisms, some of which were Germanic and some of which were individual. I remember he was something of a pacer, and there was one particular lecture room which had a loose board or something at one end of the lecture platform, and he almost invariably hit that with a plunk. I'm not sure whether it was completely an accident. He was very much absorbed in his subject anyhow.

His standards were those of sound scientific work of the sort we regard as characteristically German, and he expected himself and others to keep up to those levels of care and precision. This was no special difficulty for me because I approved of it heartily, even though it was not always easy. Nevertheless, it was not that he had to push me to try to do things right; I had to push myself to get them right.

AS: When you taught, did you attempt to emulate that?

CR: Well, hardly. I pass over my brief experience as a teaching assistant trying to teach mathematics to freshmen. The Institute quite wisely got me out of that pretty promptly. Later on, when I came to give a course in elementary seismology, things were better. The principles of organization and presentation were to the best of my ability the sort of thing that I had learned from Epstein — and others. Paul Epstein was by no means the only member of the Institute staff who was capable of maintaining high standards.

Also the quantum mechanics was developing very rapidly, and one of its features, which was a controlling element and was difficult for some observers to adjust to, was the idea of approaching every definition and discussion in terms of known and observable quantities and to leave out as much as possible of theoretical or, still worse, philosophical implications. This stuck with me and was responsible for a feature of the magnitude scale, namely, that the magnitude is very carefully defined in terms of what can be measured on the seismograms. Frequently there have been suggestions that the scale should be defined in terms of energy, but to do that would have involved continuous revisions, both numerical and theoretical. I have always insisted that the magnitude scale represents what we observe, and this may or may not be interpretable in terms of energy.

AS: You did your thesis under Epstein?

CR: Yes, though actually the topic came about through Dr. Millikan. Millikan had received a letter from Paul Ehrenfest, which was in German (which Millikan could read perfectly well), describing the results that G. E. Uhlenbeck and S. A. Goudsmit working under Ehrenfest had obtained by bringing in the hypothesis of a spinning electron, which made sense of a lot of apparently contradictory items that had been coming up in atomic theory just at that time. Millikan asked me to look this over, and I checked on it and found that indeed it promised to be at least a partial theoretical answer to some of the matters that were troubling him. Finally this developed into matter for a thesis.

AS: What was your thesis topic?

CR: It was on the hydrogen atom with a spinning electron. Actually it developed into two theses, because I had taken up the investigation first on the basis of the classical mechanics and found that it would give results similar to those already obtained applying classical mechanics to atomic problems. Then just at this time along came Max Born, Werner Heisenberg, and Erwin Schrödinger with quantum mechanics, and there was almost a second thesis dealing with the same subject from that point of view.

AS: Schrödinger came to Caltech at about that time. Do you remember him at all?

CR: Quite vividly. He was a decidedly good lecturer, and he was speaking on a very fresh and new subject. I owe to him one very priceless general remark that I found opportunity to squeeze into my textbook many years later. He was dealing with the generalization of the mechanical treatment, which had originally been set up on a non-relativistic basis. The problem was how to generalize it so that it would take into account the special theory of relativity. He was about to outline the procedure he favored at that time; then he stopped, saying, "Now, of course, the generalization is not unique." He stopped again and then said, "Of course not, otherwise it would not be a generalization." I always enjoy repeating that because it is a very profound observation.

Another story I like to tell is about E. T. Bell, the mathematician. He was a highly original and imaginative person, and naturally this was expressed in his work. Also, he had facility in writing which was evident in both his scientific contributions and in his fiction. He was a well-known author of science fiction under the name of John Taine, and he had several worthwhile books dealing with mathematics. My story is illustrative of both the man and the subject. I had come upon a rather general proposition on factorability of expressions that I thought might be interesting to put forward, as can be done in mathematical publications, simply as an outstanding question. I wondered if someone of better ability than mine could make sense of it. So I told Bell about this and said, "The only trouble about this is that you have to state it in such a way as to exclude trivial cases." And he said, "Well, that's easy. You just start out by saying, 'Excluding trivial cases, . . .'"

AS: In graduate school were you doing physics just for the love of it, or were you thinking about what you would do afterwards?

CR: Not very specifically. This was before the Depression, and I was not yet married, so I was pretty well free. What was in the back of my mind, of course, was that if I stuck around, probably something would be found for me. When a position as research assistant at the Seismological Laboratory became available, Dr. Millikan recommended me.

AS: Were you sorry to leave what you had been doing in quantum mechanics?

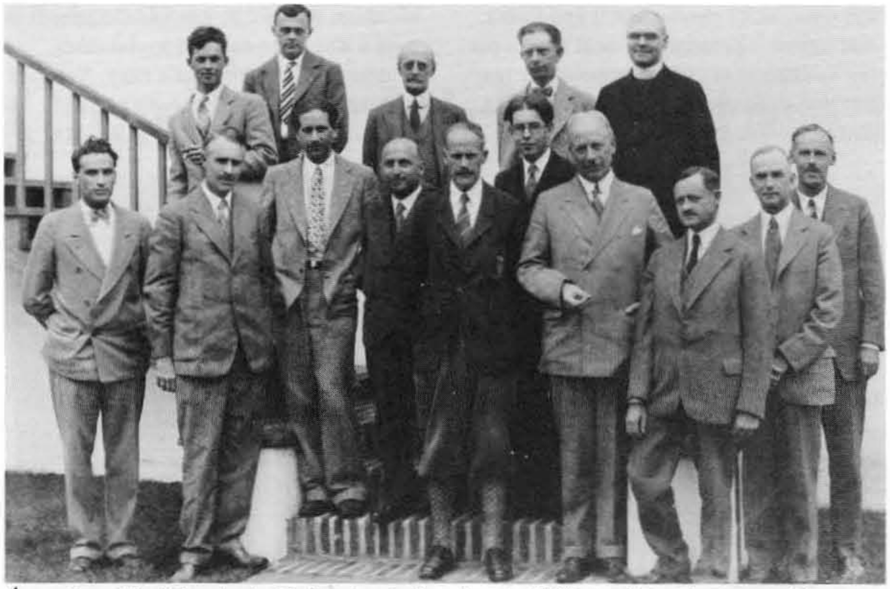
CR: I didn't feel that I was leaving anything, because so long as I could stay in or near Pasadena, I could keep in touch, which I did to a certain extent. The opportunity to work at the Seismological Laboratory provided me with the means to stay around here. And gradually I settled into the seismological work as my main occupation.

AS: You joined the lab at the very beginning. They had a building, I assume, that they were renovating or something.

CR: It was the "new" laboratory building then, and it's the "old" one now. It had been completed in 1926 and was occupied by staff in January of 1927. I made its acquaintance in the fall.

AS: There was already a staff there?

CR: Yes. Harry Wood was in charge, and it is largely due to his personal persistence and initiative that the Seismological Laboratory was established. He was a petrologist with an appointment at UC Berkeley at the time of the 1906 San Francisco earthquake, and he was a member of the commission that investigated that event and published on it. People still refer to that paper on occasion for details. From Berkeley, Wood went to Hawaii where he was at the Volcano Observatory for a number of years. After the First World War, he returned to this country and exercised himself in getting the Carnegie Institution to implement a proposal for a seismological network and installation in southern California. The program was officially set up in 1921 with Wood as research associate in charge.



A meeting at the Seismological Laboratory in Pasadena in 1929 brought together many of the world's leading authorities in earthquake research. It also led to an invitation to Beno Gutenberg to come to Caltech. In the front row (left to right) are Archie King, L. H. Adams, Hugo Benioff, Beno Gutenberg, Harold Jeffreys, Charles Richter, Arthur L. Day, Harry Wood, Ralph Arnold, and John Buwalda. At the back are Alden C. White, Perry Byerly, Harry Reid, John Anderson, and Father J. P. Macelwane.

Hugo Benioff was on the staff when I came to the lab, but I didn't see much of him because he was incapacitated by a chronic illness. Later on he returned, and about 1931 or 1932 he did some of his very best work in connection with the laboratory. Archie King was the technical assistant, and Halley Wolfe was a young fellow who acted partly as secretary and partly as photographic assistant. John Anderson, who was on the Mt. Wilson Observatory staff, was a very good personal friend of Harry Wood, and they worked together on the development of the torsion seismometer, which became known as the Wood-Anderson instrument.

AS: Millikan was getting money from a lot of southern Californians for Caltech. Did he ever get any for seismology, or was that totally supported by the Carnegie Institution?

CR: The Institute had contributed certain funds to the program and in particular it was due to the generosity of one of the Caltech trustees, Arthur Fleming, that the laboratory building was established and constructed. Thereafter, the Institute took over the maintenance of the building and grounds.

AS: Since the area was seismologically active, there must have been people who would have been positive toward seismology in the belief that to learn more about it would be of an advantage.

CR: We always had a certain amount of support. It was not always financial but in other indirect ways from the insurance interests because they had taken a bad beating at the time of the Santa Barbara earthquake. I wish I could give you a little more detail on that because it has been rather regularized, formalized, since then, and I would hate not to give adequate credit to the insurance associations who maintained a considerable interest in the programs. They also sent out some members of their own staffs into the field to prepare reports that are valuable.

This is an ongoing cooperation that rather improved with time. Actually, it was beginning to get under way before the 1925 Santa Barbara earthquake, but that event enormously accelerated it. Prior to that, earthquake insurance had been written very extensively in southern California with little regard to the actuarial soundness, and that event was a great shock with some companies suffering losses in claims that were disproportionate for a comparatively moderate event.

AS: Did insurance companies have scientific personnel and instruments?

CR: I think on the instrumental side their contribution has always been in the direction of funding or otherwise supporting operations that were ongoing. For one thing, their organizations took corporate memberships in the Seismological Society, which made funds available for investigation of earthquakes. On the whole it has been a pretty satisfactory setup, and I think the insurance industry has contributed significantly to the progress of seismology, sometimes to the alarm of public figures, because the insurance people have a financial interest in sound earthquake-resistant construction.

AS: And perhaps in prediction as well.

CR: Yes. If there really were a very solid and established basis for prediction, you would find the insurance people backing it wholeheartedly. I think their attitude toward the efforts we're making at present is favorable, but naturally they have to see some definite promise before they can justify anything on a large scale. The people who have done investigation field work specifically for the insurance people have been engineers, but many of them have been out in the field and observed the geological effects so frequently that they are better by far probably by this time on that subject than I am.

Quite a number of important reports on the effects of a given earthquake have been published from the engineering side, and it is due to the studies published by engineers, and particularly those connected with the insurance organization, that I first came to realize the enormous effect of type and soundness of construction on the damage and consequently the apparent effects of a given earthquake. That was one of the motivations in setting up the magnitude scale, because it was very clear that we were getting earthquakes in some parts of the world that were alarming in the amount of damage and even loss of life, and yet they simply weren't writing large records on the seismographs. In fact, that very circumstance over many years led to what really was over-estimation of the degree of seismic activity in the Mediterranean and the Near East — because of the prevalence in that part of the world of traditional types of construction that were far from earthquake resistant. One of the horrifying examples was the catastrophe in Morocco in 1960 with a loss of 12,000 lives in an earthquake with a magnitude of only something like 5.75.

So the insurance people and the better building organizations were on our side, and between them they produced the first versions of the uniform building code, which did contain some auxiliary provisions for safe construction against earthquakes. Those were rather carefully detached from the main body of the code. One direct and productive result of the 1933 Long Beach earthquake was the enactment by the State Legislature of the Field Act, but that was only effective for schools and public buildings and only for those of new construction. So that did not solve the problem of old and unsafe structures.

AS: What was the attitude in those days toward giving out information?

CR: No problem from the Institute side, provided that nothing was said that might be needlessly sensational. We felt a certain responsibility to keep the public informed, particularly as misinformation was often seized upon and twisted in a way that was contrary to the public interest. We were very much in favor of earthquake-resistant construction and normal safety measures, and a consciousness in the general population as to the possibility of earthquakes and earthquake risk. This was from the very first. It was accentuated by the circumstances of the Long Beach earthquake, and by some of the wild, panicky rumors that got out at that time. We felt that it was the responsibility of Caltech toward the public to give out correct information on any matter of public concern. Earthquakes were a particularly critical area because they are subject to a great deal of honest misunderstanding as well as misrepresentation.

AS: I wonder if publication of fault lines and fault maps was a sensitive issue at any time.

CR: I don't recall that we ever had any very serious public relations problems, although occasionally some individual or group would take offense, or some uninformed public figure would sound off in the press — or somebody would write a stupid editorial. But we were never seriously inconvenienced in that way as far as I know. We were much more inconvenienced by the circumstance that we were still in the process of getting over the Depression, so we were not able to really start any expansion of the seismological program, which was urgently needed.

AS: You said that expansion was urgently needed. You mean setting up more stations to gather more data?

CR: We had had plans for more stations, more instruments. One thing I was particularly interested in was the development in use of portable installations. That went on with various accidents and mistakes, but it did progress gradually until finally we always had at least one portable unit that we could use in an emergency. The original idea was to take a portable unit out, particularly after a considerable event, and record the aftershocks so that we could trace down the geographical area from which they were originating. That was first done right after the Long Beach earthquake. We were just barely able to put the unit into operation, but it did work and did make a few useful recordings that contributed to our understanding of the event.

AS: The Long Beach earthquake was kind of a watershed in seismology in southern California it seems.

CR: At least it settled some matters forever because we had had individuals ready to claim in public and even in print that there was no real earthquake danger in the Los Angeles area, that it was all San Francisco. The Long Beach disaster put an end to that. And also, as I mentioned, it produced the first permanent action on the part of the state, the Field Act. The provisions of the Field Act were good, and the later school buildings constructed under them performed properly and conspicuously better in comparison with those of earlier construction. So there is no doubt that it was a good and effective measure. It simply didn't go far enough.

AS: Something I haven't asked you about yet is the coming of Beno Gutenberg. In 1930, wasn't it?

CR: Yes, on appointment. In 1929 the Carnegie Institution called a conference at Pasadena to evaluate progress to that point. Two very important visitors from abroad were there, Harold Jeffreys and Beno Gutenberg. It was commonly understood among the whole Pasadena group that in all probability one of our distinguished foreign visitors would be invited to come to us, either on a temporary or a permanent basis. There was some back and forth, and finally it was decided to offer the opportunity to Gutenberg. He accepted and arrived with his family the next year. He had a professorship at the Institute, but he did not become a member of the laboratory staff as such, except as a courtesy. He was given office space at the laboratory and spent a good deal of his time working there and familiarizing himself with what was going on, contributing to the program, and even doing some research work on the records that were then available.

AS: Gutenberg was very eminent at that time? Do you know why he decided to come to Pasadena? Was there no opportunity for him in Germany?

CR: It was certainly a better position. In Germany, he had the position in Frankfurt of Professor Extraordinarius, which I think had a small stipend only. He was consequently depending for his living and the support of his family on the operation of the family soap factory. You didn't have to be independently wealthy to be an academician there, but the position he had was more an honorary one than a remunerative one. In addition, he was doing a lot of publishing and editing, which brought in some income. But, in general, the offer was attractive to him from an economic point of view, so he was coming to a better position both in terms of compensation and actual influence. And he had already some indication of the trouble which was then developing in Germany.

AS: He felt that he didn't have much of a future there?

CR: Well, after all, he was Jewish, and there were already indications of trouble. After he was over here, he went to considerable trouble and expense to help a number of other people to get out of Germany before the storm broke.

At Gutenberg's invitation, I picked up a certain amount of work in collaboration with him, and we wrote one book together. I owe a very great deal to him and came to regard him with almost filial affection.

AS: These were the years when you were assembling the data that eventually became organized in the scale that is named for you?

CR: The first work was done on a group of earthquakes that occurred in January of 1932. And that was sufficient to arrive at and set up the general picture. I considered the technique as more or less under test for several years afterwards, although by the end of the year we were putting out bulletins with numbers on them from the scale. But the details were not published in full discussion until 1935.

AS: The purpose of the scale actually seems to have been more of a public one than anything else.

CR: We needed something which would not be subject to misinterpretation in terms of the size and importance of the events. And also in the process of working with the scale it developed (which we had already suspected) that the statistics of earthquakes in general were in a very bad way because they had been too much influenced by accidental circumstances of local intensity. It seemed desirable to have some objective and instrumentally founded means of comparing earthquakes with each other. Even within a limited region such as California it had advantages, and when it developed that it could be expanded to cover the entire world, the value of the scale was greatly increased.

AS: How was it that your name was attached to it? You were instrumental in doing it, but there were other people involved.

CR: Well, the scale as such originated under my hands quite unexpectedly. I had been working with Wood trying out various tentative means of comparing our California earthquakes, and we weren't getting anywhere with it. Then I got hold of a paper by a Japanese seismologist, Professor Wadati, and that gave me the idea of plotting up our data in a particular way. It worked out much better than I had expected and produced this definite numerical scale that practically fell out of the data. I showed this to Gutenberg and Wood separately, and they both liked it, and I went on systematizing it. Wood put a brief mention of it in his annual report to the Carnegie Institution.

AS: Is that where the term "Richter Scale" was first used?

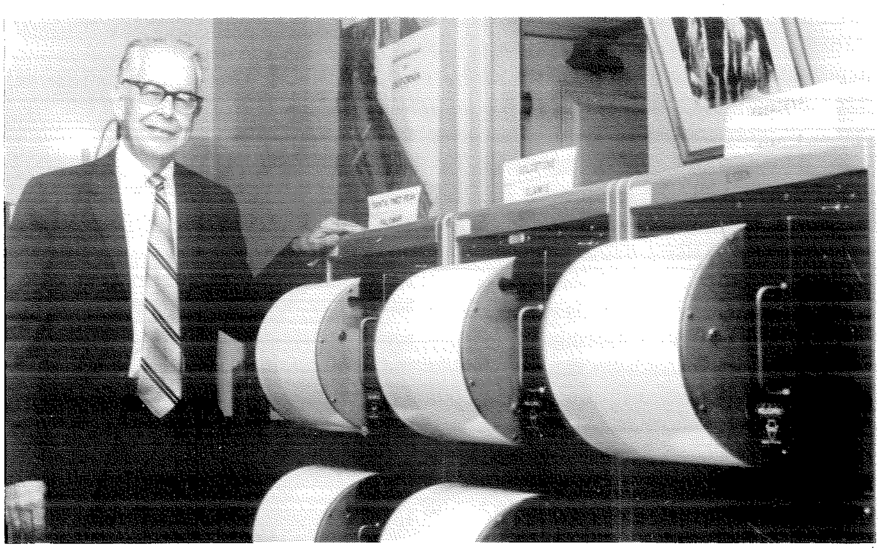
CR: I called it the magnitude scale, and I refrained from attaching my personal name to it for a number of years. I think it was Professor Perry Byerly of UC Berkeley who started referring to it as the Richter Scale in public. This name somewhat underrates Gutenberg's part in developing it for further use, because after all he knew a tremendous amount about seismographs and seismograph recordings, and his knowledge could be applied to the interpretation of records written all over the world in a way that was coherent with the scale I had set up in California.

AS: Was he the one who suggested using a logarithmic scale?

CR: Yes. The common practice in engineering and physics is to use a vertical logarithmic scale that compresses the data. And Gutenberg had undoubtedly encountered that procedure in some of his own practice. When I went to him and pointed out my problem with the numerical values in the data, he said, "Try plotting the data on a logarithmic scale." I did, and it then became evident that it could be used in a manner to set up a definite scale.

The logarithmic scale is rather a natural procedure wherever you have to deal with numbers that extend over a very wide range, and the range proved to be rather astonishingly wide in the case of earthquakes. In fact, if there was anything you could call an actual discovery that came out of that scale, it was that the biggest earthquakes were enormously bigger than the little ones.

AS: Would you expand on that a little? I suspect there is still some misunderstanding of exactly what the scale is and what it measures.



Charles Richter stands by the Seismological Lab's recording drums, on which the records of incidence, location, and magnitude of earthquakes are read.

CR: First, let me point out that the scale is not an instrument but a series of charts and tables, and it measures magnitude, not intensity. An intensity scale has arbitrary grades, say from I to XII, that are applied by experienced investigators to describe or rate the shaking produced by an earthquake at a given point. On the Modified Mercalli scale, for example, Roman numeral I indicates that the earthquake was in general not felt at a reported place, IV that it was strong enough to rattle windows, and XII (a degree of shaking that is rarely observed) that it was sufficient to cause total destruction to buildings.

The magnitude scale, on the other hand, represents measurements (expressed in ordinary numerals and decimals) of the deflection indicated on a seismogram during an earthquake. It compares earthquakes in terms of the amount they disturb the ground at a fixed distance from the earthquake's epicenter. If we compare local intensity on the Mercalli scale to the signal strength on a radio receiver at a given locality, magnitude is comparable to the power output in kilowatts of a broadcasting station.

Now there is no upper limit to the possible magnitude of an earthquake; that is, earthquake magnitudes are not measured on a fixed scale of, say, one to ten. The highest magnitudes assigned so far to actual earthquakes are about 9, but that is an observed fact, not a ceiling — a limitation in the earth, not in the scale. The scale is, as we said, logarithmic, so a step up of one unit in magnitude implies a tenfold increase in ground motion. An earthquake of magnitude 8, which is a great earthquake, causes ten times as much ground motion as one of magnitude 7, and 100 times as much as one of magnitude 6. If we assign the number 1 as the deflection for a magnitude 3 earthquake, the San Francisco earthquake of 1906, which was of magnitude 8.3, showed a deflection 100,000 times as large. That is what I meant when I said that the biggest earthquakes are enormously bigger than the little ones.

Let me add that this concept of the earthquake magnitude scale is not a final one. Paul Jennings [professor of civil engineering and applied mechanics] and Hiroo Kanamori [professor of geophysics] are particularly active in research here at Caltech in revising its formulations and applications. So the last word on the Richter Scale has by no means been said. □

The Reapportionment Puzzle

. . . continued from page 8

across states in this condition, but it seems unlikely that the minority parties in most states will not have wasted strength, that is, population that is too dispersed or too highly concentrated. In California, for instance, both the Democrats and the Republicans have areas where their strength is dispersed and other areas where their strength is overly concentrated. The coincidence of an equitable division of seats and compactness requires efficient levels of concentration — in our hypothetical example, clusters of two. California Democrats are too highly clustered in urban areas and too dispersed in rural and suburban areas. Conversely, Republicans are too highly concentrated in suburban areas and too dispersed in urban and certain rural areas. True efficiency for the Democratic party would require spoke-like appendages from inner city seats out into the suburbs, and even that would be next to impossible for some of the seats right in the middle of Los Angeles and San Francisco. Similarly, you would have to annex inner city areas to the suburban Republican seats to make them more efficient, and reaching the seats in Orange and San Diego counties would require some truly "creative cartography." Quite simply, there is no happy coincidence between efficient partisan strength and compactness in California.

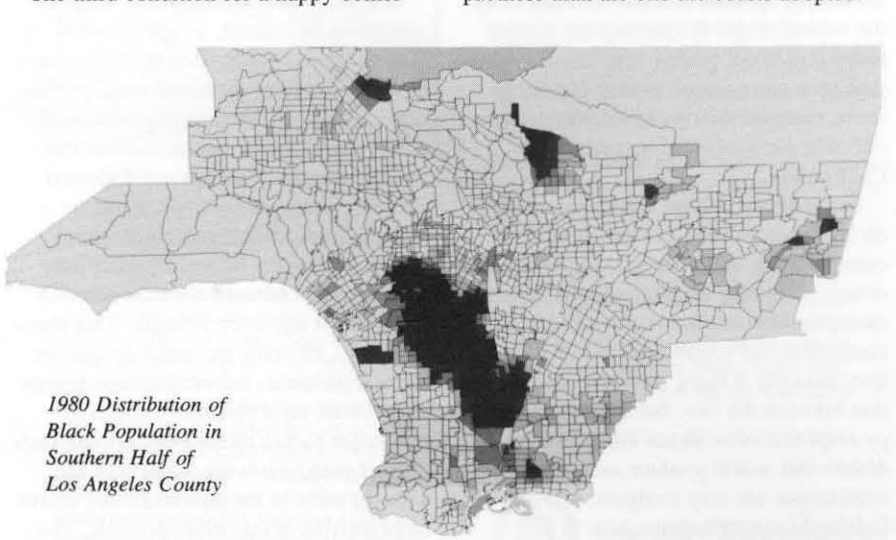
The second condition is that city and county lines would themselves have to be compact. While this may be true in some states, it is certainly not the case in others such as California. Many California cities and counties have noncompact lines. Recently incorporated California cities are particularly good examples of this problem. Gary Miller (formerly assistant professor of political science at Caltech, now at Michigan State), in his study of municipal incorporation of Los Angeles, found that city lines were determined by a variety of political motives. The City of Industry, for example, incorporated an industrial area so that it would not be annexed to nearby cities attempting to increase their tax bases. The effect is that Industry does not need to provide any services since it has practically no residents. Nearby cities — several of which have sizable poor populations and high service needs — are deprived of a potential industrial tax base. Miller concluded that this pattern of incorporation by rich communities to avoid annexation with — and hence taxation by — poor communities is quite prevalent in Los Angeles County.

These politically motivated incorporations have not been compact or symmetrical. The City of Industry looks like the hull of a boat. The city of Monrovia has a narrow appendage with fewer than 100 people in it that is connected to the main body of the city by a drainage ditch. The city of Los Angeles itself is connected to its port area in San Pedro by a narrow corridor that skirts the cities of Carson and Torrance. Pasadena has a stovepipe extension to the north that protrudes up through a reservoir area into unincorporated county land. Commerce, like the City of Industry, is a largely unpopulated industrial area with many jagged sides. The city of Riverside is a mosaic that rivals the most creative efforts of gerrymanderers over the years. The list of similar such examples is quite long.

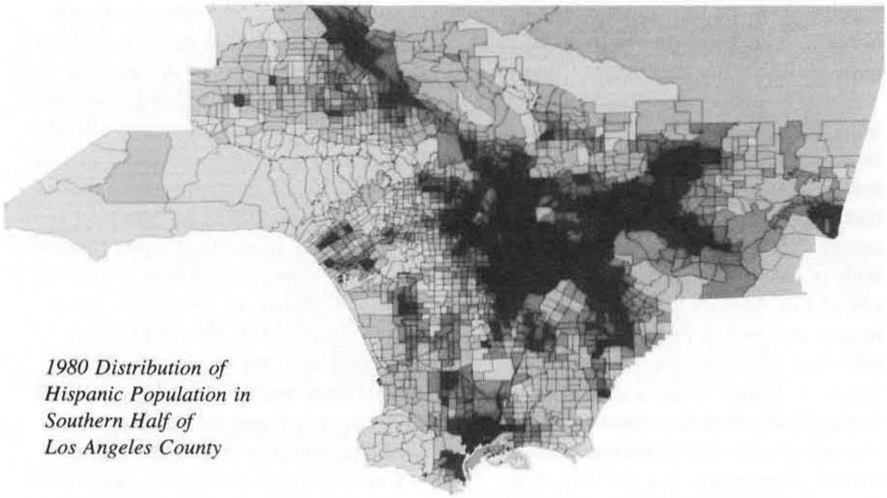
If cities and counties are not always, or even usually, compact, it will be harder to make districts look compact. The reapportioner will be forced to choose between straightening out lines at the expense of splitting parts of cities and counties and preserving the city and county lines at the expense of compactness. If the members of a particular polity decided that compactness was essential, it would probably necessitate a re-evaluation of what constitutes a city or county split. If the separation of even small amounts of territory from a governmental unit is defined as a split, it will be extremely difficult to improve districts aesthetically. A more tolerant definition of a split — more than a certain percentage of population or area — would make the reapportioner's task much simpler.

The third condition for a happy coincidence of aesthetic and other good-government criteria is that minority communities cannot be dispersed. As we have seen several times already, minority communities like minority parties cannot afford to be inefficiently distributed under our electoral system. Of the two sorts of maldistribution, a minority is far better off from the standpoint of political representation if it is overly concentrated than if it is overly dispersed. Hence, as long as the minorities in a given state are concentrated geographically, the representation bias against them will not be too great. If the state has very dispersed minority communities, that bias will be substantial.

California is an interesting case in this regard since it has both dispersed and concentrated minority communities. The Black community in California is concentrated in a few areas: south central LA, Pasadena, parts of San Francisco, Oakland, and Richmond. The Hispanic community, by contrast, is dispersed both within the urban areas and over the rural areas. The Los Angeles Hispanic community is centered in East LA but spills into a number of communities in the East San Gabriel Valley, downtown LA, and the San Fernando Valley. There are also large concentrations of Latinos in San Diego, parts of Orange County, the Imperial Valley, the Salinas Valley, San Jose, the central valley, and Ventura. Dealing with Black representation under a strict compactness constraint is not nearly the problem that dealing with Hispanic representation is. Not surprisingly, the court was able to remedy the underrepresentation of Blacks in 1973, but was less able to please the Hispanics. Affirmative gerrymandering for Hispanics would require a more lenient interpretation of compactness than the one the courts adopted.



1980 Distribution of Black Population in Southern Half of Los Angeles County



1980 Distribution of Hispanic Population in Southern Half of Los Angeles County

The last of the conditions that would have to hold is that communities of interest would have to be compact or at least divisible into compact seats. Obviously, the reapportioners are constrained by the shape of the terrain they have to work with. Valleys, coastal areas, deserts, and urban areas are going to be compact and symmetrically shaped only by fortuity. If the community of interest is sufficiently large, it may be possible to divide it into regular forms, but even so, a purely rural seat will always tend to be dispersed in area because of the low ratio of population to territory. Defining coastal areas will always be problematic, because it is difficult to say where coastal interests begin and end.

As to the plausibility of our example, those familiar with the geography of California will immediately see the resemblance. The desert and mountainous areas of California are so sparsely settled that any seats that contain only these areas will be very large. The coastal areas run down the side of the state and are hemmed in by the coastal range. Respecting the coastal range makes the coastal seats more narrow than pure compactness would dictate. In short, many of the considerations raised in our example apply to the situation in California.

Summing up, I have argued that there is no necessary relation between aesthetic considerations and other good-government criteria. It is easy to construct plausible examples of how the two are sometimes compatible and sometimes not. In addition, not only is there no logical connection between the two, but there is no happy empirical coincidence either. The conditions that would produce such a happy coincidence are very stringent, and the California example shows how in one major state, they certainly did not pertain. The conclusion one would have to draw is that there is not a great deal to be said for the indirect value of compactness; there is no reason to expect that it is a useful facilitator of other good-government criteria. If there is any reason to retain compactness as a reapportionment guideline, it would have to be its direct or intrinsic value.

Does compactness per se have any intrinsic value? We have already considered one possibility — that compact districts lessen transportation and communication costs — and concluded that this was more important in previous periods of history. It might seem that there are no others. Surely, no one would argue that compact districts produce more conscientious, thoughtful representatives than noncompact districts. There is one feature of compactness, however, that is absolutely central to the working of Anglo-American electoral systems: It contributes to their stability.

One of the strongest arguments for a geographically based, simple-plurality system such as we have in the US is that it prevents the proliferation of small parties and exaggerates the strength of the winning party. In other words, the fact that the rules discriminate against dispersed minority parties and groups, it can be argued, is an advantage. Our electoral rules restrict entry by small parties into the legislature because the rules discriminate against dispersed strength. This keeps right- and left-wing extremist groups out of the legislature. It forces interest groups to articulate their demands through the two major parties rather than forming their own. It exaggerates the strength of the winning party in the legislature and makes large legislative majorities possible. Proportional representation systems, by contrast, give each group above a certain threshold size its share of seats. This tends to cause the number of parties in the political system to proliferate and to give extreme groups a public forum. Governments in proportional representation systems tend to be coalitional because no one party has enough seats to form a legislative majority by itself.

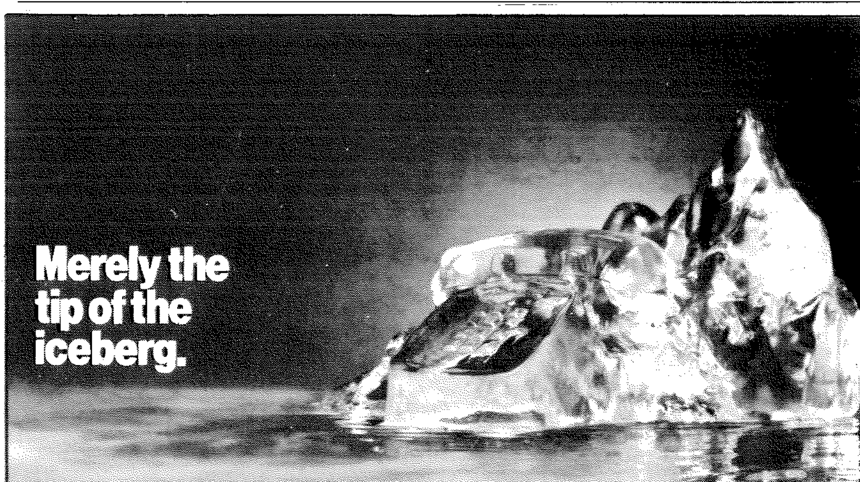
Of course, electoral rules are not the only factor that explains two-party stability, but they are a major contributing factor. The effect of making districts intentionally noncompact is to undermine the bias in the rules against dispersed minorities and by so doing weaken the stabilizing feature of single-member, simple-plurality systems (SMSP). Districts that are intentionally noncompact will concentrate a minority group or party when their residential patterns are electorally inefficient. By reaching out and uniting individuals of the same party, ethnic, or racial group, you attempt to give them representation in the legislature commensurate with their population. This would happen naturally in a proportional representation (PR) system, or in an SMSP system where the minority happened to reside in moderately concentrated areas. Without PR rules or a fortuitous geographical distribution, commensurability between voting strength and seats can only occur by some willful effort to make minority strength efficient. We are torn between the demands for representational equity and the nature of the electoral system.

By requiring compactness, you preserve the nonintentionality between shape and the efficiency of minority strength. If it so happens that a minority party or group is efficiently distributed, then they will not be discriminated against. If that group or party is not so fortunate, then the rules will be biased against them. Since most minority groups and parties are dispersed inefficiently in at least some part of the state, the stabilizing feature of the SMSP system is preserved. The key, however, is that the districts must be compact, and where they are not compact, they must be randomly noncompact. If districts are weirdly shaped to help a minority party or group, then they will weaken the system's bias.

What this means is that there may be a fundamental tension between aesthetic criteria and two good-government goals: fair representation for the minority party and fair representation for minority groups. Those who argue for the importance of compactness must be willing to accept limitations on the achievement of equity for minorities. This may be less of a problem for minority parties than for minority groups. It seems reasonable that minority parties should pay the price of a bias against them in return for two-party stability. The bias against minority groups is more troublesome, however, in the light of recent court efforts to ensure that minority communities are not carved up. From the perspective of the white, median voter in this country, compactness is desirable since it enhances the strength of the majority. From the perspective of the nonwhite population, compactness deprives them of equitable representation for the same reason. For the reapportioner, it presents the first of many conflicts between supposedly consistent good-government goals.

We have seen how conflicts can arise between supposedly consistent criteria. What this means is that reasonable people holding different values can disagree about the way districts are designed. Moreover, it is impossible to draw district lines without affecting the parties in some way, and it will matter little to them whether the effects were intentional or nonintentional. A public interest approach to reform, as advocated by Common Cause, would hand the task of reapportionment over to an apolitical commission with the mandate to draw lines that conform as closely as possible to formal criteria. The problem with this is that there is no simple uncontroversial solution to reapportionment, and the commission must ultimately make choices between competing claims. As the history of commission approaches shows, they too quickly become entangled in the politics of redistricting.

An alternative approach, which is more consistent with the way we normally handle political issues, is to assume there will be inevitable differences of opinion and try to set up an institution that would encourage the most amicable resolution of disagreements possible. This would mean either a politically constituted commission or legislative reapportionment with stricter guidelines about public disclosure. A pluralist approach cannot create unanimity where there is none to begin with. It cannot even build the comforting facade of agreement that a public interest approach offers. Rather, it promises a tolerant, open way for a polity to resolve its disagreements, which, as history demonstrates, is a considerable achievement in itself. □



Random Walk

Scientific Recognition

The prestigious New York Academy of Science recently held its 164th annual meeting, and one item on the agenda was presentation of its Presidential Medal to Marvin Goldberger, president of Caltech. A certificate of citation and a cash award of $1,500 in recognition of outstanding accomplishments in the cause of science went along with the medal.

Goldberger is a theoretical physicist. He is best known for his work on the theory and application of dispersion relations to problems of weak and strong interactions. He is the author of some 150 papers on such subjects as nuclear physics, plasma physics, theory in intensity correlations, reactions among elementary particles at high energies, S-matrix theory, and quantum field theory.

Research Detour

If you think that some problems in science are too far-out to be addressed by a dedicated Caltech alumnus, you don't know James Gould (BS '70), who is an associate professor of biology at Princeton. Not long ago he made a slight deviation from his normal research patterns, and a report of the outcome of the detour is reproduced at the left from a recent issue of the *Princeton Alumni Weekly*.

Still to Come

Three of the 1982 winter/spring series of Watson Lectures are still ahead. On March 31, Thomas Ahrens, professor of geophysics, will speak on the "Death of Dinosaurs." William Corcoran, Institute Professor of Chemical Engineering, talks on April 14. His title is "Heart to Heart — Design and Performance of Artificial Heart Valves." The series winds up on April 28 with Leroy Hood, the Ethel Wilson Bowles and Robert Bowles Professor of Biology, speaking on "Genetic Engineering and Medicine of the Future."

Stamp Act

The Caltech community has long recognized the distinction of Robert A. Millikan — as a Nobel prizewinning scientist, an administrator, and a diligent laborer in behalf of many national and civic causes. And his secretary for the last three years of his life, Helen Holloway, has been pointing these facts out to the Stamp Advisory Committee of the U.S. Government Printing Office ever since 1954. Her persistence finally paid off, and on January 26 at a ceremony in Beckman Auditorium the newest stamp in the Great American Series was officially presented to the American public. For 37 cents you can now buy a blue stamp adorned with an engraving of Millikan's face, and, at least for a while, you may use it to send two ounces of first class mail.

Participants in the First Day of Issue Ceremony in honor of the Robert Millikan stamp included Millikan's grandson Dr. Michael Millikan. On either side are F. X. Biglin, senior assistant postmaster general, and Kathryn Wilson, Pasadena postmaster.

Homing Nonsense

A REPORT by British zoologist Robin Baker that people, like pigeons, have homing instincts prompted some American scientists to test his theory. Princeton biology professor James Gould, well-known for his discovery of magnetite in bees and pigeons, which could account for their ability to home, duplicated Baker's experiment using tighter controls.

Students were blindfolded, put on buses with foil-covered windows (to prevent them from sensing the direction of the sun), and taken for rides into the countryside. At different stops they were asked to indicate the direction of home. The results did not support Baker's findings. "If humans have the ability to tell where they are," remarked Gould, "it's certainly not the case with Princeton students." Nor was it the case with Cornell students or those at the State University of New York at Albany who participated in similar experiments.

Baker was puzzled and offered to come to the States to help out with the experiment. So last spring at Princeton a homing showdown was held. Taking part were experts on pigeon navigation, the discoverer of bat sonar, the originator of statistical methods to analyze orientation research, and the Amazing Randi, a magician who is both an expert and skeptic on the subject of extrasensory perception.

Baker joined the field of 20 test subjects. Having once observed that though Princeton students may know where they are going, they seem to have no idea where they are, Gould decided to include some recruits from the Lawrenceville School. The volunteers were draped in black velour hoods, herded onto the specially prepared buses, and driven away. The results did not change. If the students did poorly, so did the bus drivers, who managed to get lost in spite of having detailed maps and compasses.

Though Baker remains undaunted in his conviction that humans have a magnetic homing sense, most of the participants would agree with Randi, who suggested they might do better by relying on Chinese fortune cookies.