Engineering & Science

California Institute of Technology | December 1980

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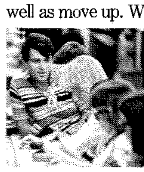

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SCIENCE/SCOPE

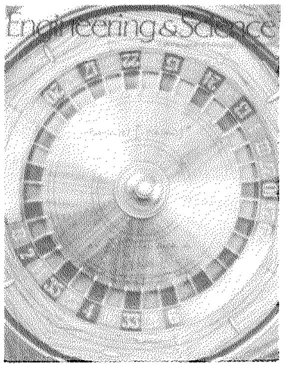
Water levels in cooling systems of nuclear reactors may be monitored more reliably, especially during an emergency core shutdown, by a metal-coated optical fiber developed by Hughes. The thin glass thread, some 1000 of which would be placed around a reactor's core, is tipped with a sapphire retro-reflector. Unlike plastic-coated fibers, it can withstand the harsh reactor environment of temperatures as high as 350°C and pressures up to 1800 pounds per square inch. Compared to resistive level sensors now in use, the fiber is a model of simplicity. The sapphire tip, when dry, reflects light transmitted through the fiber; when wet, it reflects no light. Prototype sensors were developed under Nuclear Regulatory Commission sponsorship.

Data rates of 4 billion bits per second -- a speed at which the Encyclopedia Britannica could be transmitted in just two seconds -- have been demonstrated by an experimental modulator at Hughes. The modulator, an important step toward ultra high-speed satellite communications, is a quadriphase shift-keyed arrangement of two field-effect transistor biphase modulators. Use of microwave FETs in the modulator driver circuit resulted in very low power consumption.

Doctors can look into the human body more clearly, quickly, and safely than ever before by using a new x-ray process called computed axial tomography. The process employs a sophisticated scanner to create pictures of one plane of the body. One benefit is that doctors have more detailed views of bones and organs. Also, because the scanner contains extremely sensitive photodetectors, patients receive much smaller doses of radiation. Hughes photodiodes are used in these instruments to detect scintillations produced by the x-rays.

If you're pursuing a Master's or Doctoral degree in Engineering (Electrical, Electronics, Systems, Mechanical), Computer Science, Applied Mathematics, or Physics, you might qualify for one of 100 fellowships Hughes will award this year. Recipients will have all academic expenses paid and will receive an educational stipend, summer employment, professional salary, and employee benefits. Already 4,000 engineers and scientists have used the program to achieve their advanced degrees. For more information, write: Hughes Aircraft Company, Corporate Fellowship Office, Bldg. 6/C122/SS, Culver City, CA 90230. Hughes is an equal opportunity employer.

For the first time a new microwave sensor will allow military weather satellites to "see" through clouds to monitor meterological conditions below. The instrument will detect rainfall, ice masses, ocean wind speeds, soil moisture content, and other conditions, and relay compiled data within minutes nearly anywhere in the world. Data will help commanders plan operations that depend on accurate weather forecasts. Hughes, under a U.S. Air Force contract, is to build one prototype and develop computer software for ground processing.

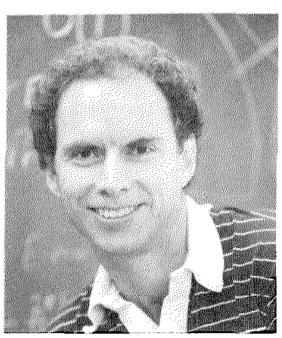


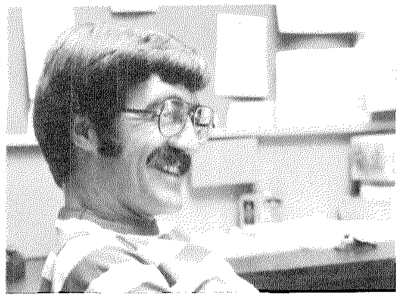
Casino Power

On the cover — mathematical formulas relevant to both statistics and gambling superimposed on a familiar instrument of the latter — a 70-year-old, hand-carved roulette wheel. Gary Lorden's research in sequential analysis, based on sampling data one point at a time, is applicable to both fields of endeavor — to dice as well as data. In his article, "Gambling with Statistics," on page 6, he explains this relationship and graciously volunteers a few mathematical tips on intelligent betting.

Lorden himself is not a threat to the Las Vegas casino circuit; he prefers two-dollar bets (which everyone knows won't break the bank). He's been risk averse ever since becoming a math major at Caltech (BS '62). With a PhD from Cornell in 1966, he returned to Caltech in 1968 and is now professor of mathematics as well as coach of one of the Institute's most successful teams — not the blackjack team, but the William Lowell Putnam Mathematical Competition team, which has won the national prize five times in the last nine years.

Gary Lorden





Morris Fiorina

The Party's Over

Election day may be past, but Morris Fiorina's involvement with elections didn't end then. As professor of political science, he maintains a year-round interest in government and politics, particularly in Congress and elections. Fiorina actually delivered his Watson Lecture on the weakening of political parties, "The Decline of Collective Responsibility in American Politics," before November 4, but the article adapted from it, beginning on page 12, is perhaps even more pertinent in hindsight. (The complete paper was first published in the Summer 1980 issue of *Daedalus*.) He has, however, written an analysis of the meaning of that election, which appears on page 32.

Fiorina came to Caltech in 1972 with a BA from Allegheny College and a PhD from the University of Rochester, and he's been instrumental in the development of the strong interdisciplinary research that characterizes the social sciences program at the Institute. He admits that being a specialist in politics at a place like Caltech "gets a bit lonely at times," but it must be somewhat less lonely at least every four years around this time. Fiorina is the author of three books, one of which, *Congress – Keystone of the Washington Establishment*, won the Washington Monthly annual political book award in 1977.

The View from Engineering

For several years *E&S* has been presenting excerpts from the Oral History of Caltech as gathered by the Institute Archives from interviews with more than 20 senior professors. Although each of those histories has covered roughly the same period of time, the recollections have been remarkably diverse — a mix of varied approaches to the same situations and genuinely different experiences.

In this issue Frederick C. Lindvall, professor of engineering emeritus, reviews his early days at the Institute as a graduate student and young instructor in electrical engineering. The reminiscences in this Oral History are particularly appropriate just now in connection with the symposium held November 6 and 7 in honor of the 70th anniversary of electrical engineering at Caltech as started by Professor Royal W. Sorensen. As one of the symposium speakers, Lindvall took a retrospective look at those beginnings. Many of his more personal recollections can be found in "Frederick C. Lindvall — How It Was," which begins on page 17.

Lindvall received his PhD in 1928, and then spent two years at General Electric. From 1930 to 1968, he was active in research, teaching, and administration at Caltech. The chief areas of his research were vacuum switching phenomena, aerodynamic effects on atmospheric glow discharges, and the dynamics of rail and road vehicles. He was also active in promoting engineering education both at Caltech and on a national level. For 24 years he was chairman of the engineering division. After his retirement, Lindvall spent three years as a vice president of Deere & Company in Moline, Illinois, and then returned to Pasadena, where he now lives with his wife, Janet, and serves as president of Lindvall, Richter, and Associates, an earthquake advisory firm.

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PICTURE CREDITS: Cover — Chris Tschoegl (roulette wheel, courtesy Big Al Bond)/2, 10, 23, 24, 26 — Chris Tschoegl/2, 11 — Floyd Clark.

Engineering & Science (ISSN 0013-7812) is published five times a year, October, December, February, April, and June, at the California Institute of Technology, 1201 East California Boulevard, Pasadena, California 91125. Annual subscription $5.00 domestic, $10.00 foreign, $15.00 foreign air mail, single copies $1.00. Controlled Circulation postage paid at Pasadena, California. Published by the California Institute of Technology and the Alumni Association. Postmaster: Send change of address to Caltech, 1-71, Pasadena, CA 91125.

Engineering & Science

December 1980/Volume XLIV/Number 2

Engineering&Science
DECEMBER 1980

Gambling with Statistics

by GARY A. LORDEN

The mathematical theory of probability originated with attempts to analyze games of chance involving cards and dice. Even modern textbooks on probability devote substantial space to examples and calculations related to gambling games. What is less well known is the extent to which certain methods of statistical inference are also intimately related to the study of gambling.

One such area is my own research specialty, sequential analysis, which is based upon the idea of sampling data "one point at a time," the total amount of sampling being determined by carefully specified rules. These rules call for larger samples when the data are inconclusive, and smaller samples when early results are dramatic. Methods of sequential analysis have been applied to a broad range of practical problems, and have been particularly successful in areas such as clinical trials, reliability testing, and quality control, where the taking of data is necessarily spread out over time or is very costly. A great many problems of statistical inference can be solved by sequential methods, usually with much greater efficiency than is possible using classical techniques that take a sample of fixed size.

Because of the one-at-a-time nature of sequential sampling, the performance of these statistical procedures depends on the same sort of "chance fluctuation" phenomena that are of interest to gamblers. It is therefore not surprising that many of the methods of probability theory that have proved useful in the theory of sequential analysis are also quite illuminating when applied to questions about gambling such as: What are the chances of winning a lot of money in a particular game? What makes some bets and betting schemes successful more often than others?

Let's analyze the following situation. Suppose you find yourself in a gambling casino with $100 in your pocket and feel a desperate need to win $2000. Which of the following bets would be the best way to start?

(A) Put $10 on "8 the hard way" at craps.
(B) Risk $40 on "red" at roulette.
(C) Wager $10 on the "pass line" at craps.

A typical Caltech student would check the odds and percentages:

A pays $90 profit, but the odds against it are 10 to 1, and the percentage in favor of the house is 9.1 percent.

B pays $40 with odds of 10 to 9 against, and the house percentage is 5.3 percent.

C pays $10 with unfavorable odds of 36 to 35 (approximately), and the house percentage is 1.4 percent.

To make the choice more definite, let's assume that, whichever you choose, you will continue with the same proposition and will always bet the same fraction of your money, no matter how much you have, until you win your bundle (if ever). For example, if you choose A and get up to $500, you will wager $50 on the next turn.

Are you ready for the answer? It appears at the end of the next paragraph, so to avoid prejudicial information you should make your decision now.

The most popular choice is the third, based on the low house percentage. Some prefer the second, because it in-

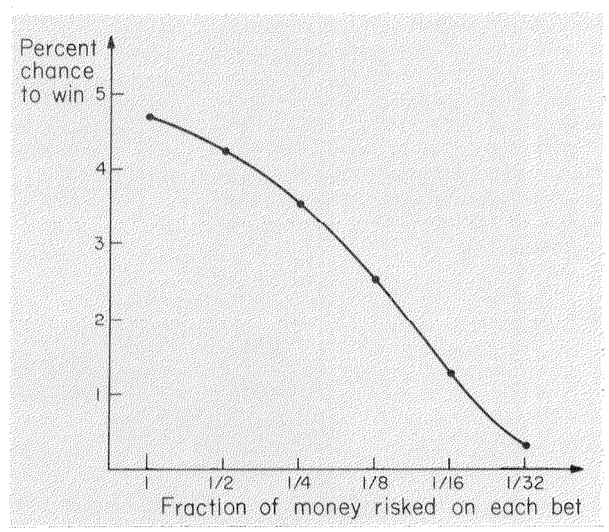


Figure 1. Probability of 20-fold win on the pass line.

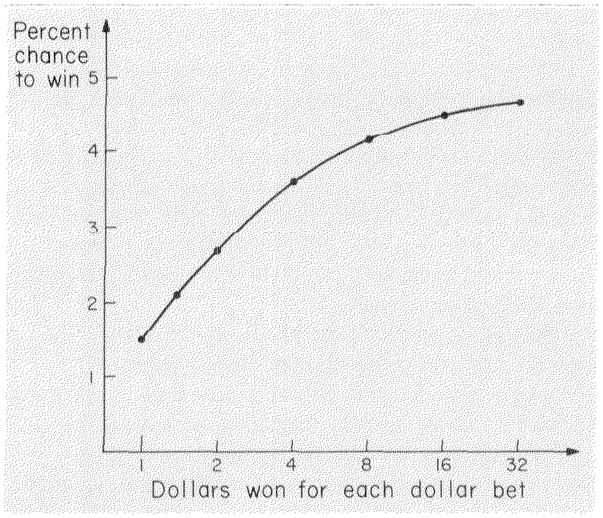


Figure 2. Probability of 20-fold win with various payoff ratios. All calculations assume 1) the fraction of money risked is 10 percent, and 2) the house advantage is 2 percent; that is, if R is the payoff ratio, then $.98/(1+R)$ is the probability of winning on each bet.

volves betting a larger fraction of your capital (which helps). Only an occasional optimist or devil's advocate picks the first, but that is actually the best bet of the three (and the third is the worst).

In fact, all three bets give the player roughly the same chance of success — small. Your probability of winning is 2.32 percent with A, 2.31 percent with B, and 2.14 percent with C. In a perfectly fair game (with no advantage for the casino) your chances of turning $100 into $2000 would be 1 in 20, or 5 percent. Why are the probabilities of A, B, and C so much lower? And how can it be that A is the best?

Figures 1 and 2 provide some insight into the answer. It is true that lower house percentages, like the one in C, help the player's chances. But there are two other important factors. One is the fraction of your money that you risk on each bet. The other is the payoff ratio — the number of dollars profit for each dollar bet when the bet wins. Figure 1 shows how dramatically the chances of a big win are reduced by risking small fractions of your money. Option B beats option C by virtue of risking 40 percent rather than 10 percent. Figure 2 illustrates the improvement in the player's chances resulting from a high payoff ratio. Bet A risks the same fraction as bet C and faces a much tougher house percentage, but its 9-to-1 payoff ratio more than compensates for this disadvantage.

Gambles like these can be analyzed easily by the methods of modern probability theory. One of these methods uses the notion of a martingale, the mathematical model of a fair game, which is used extensively in sequential analysis. Fair games have very nice mathematical properties. As mentioned before, the chances of turning $100 into $2000 are 1 in 20, and any other ratio works the same way. For example, the chances of turning $56.30 into $347.50 are 563 out of 3475.

A gambler using system A is playing an unfair game, of course, so his chances of winning can't be calculated that easily. But there is a trick we can use — a way of "keeping score" so that the game becomes fair. For example, in system A, if the player has $100, his score is $(100)^{1.256}$, the number 100 raised to the power 1.256 (which is easy to compute on a scientific calculator). If he gets to $2000, his score is $(2000)^{1.256}$, and so on. Why 1.256? Because this is the power (which I like to call the "casino power") that makes the scores behave like a fair game. Starting with $(100)^{1.256} = 325.1$, the chances of making it to $(2000)^{1.256} = 13{,}998.9$ are 3251 out of 139,989, which is the 2.32 percent mentioned earlier.

How is the casino power of 1.256 for bet A calculated? The player starting with $100 has one chance in 11 to win his $10 bet and jump up to $190, and 10 chances in 11 to lose and drop down to $90. If the power p is used for "scoring," then the player has a score of 100^p initially and after the first bet has either 190^p or 90^p. Taking account of the respective probabilities, his score after the first bet is, on the average,

$$\frac{1}{11} \times 190^p + \frac{10}{11} \times 90^p.$$

If this equals 100^p — his score *before* betting — then the game is fair. And this requires $p = 1.256$. Similar reasoning applied to bets B and C yields $p = 1.257$ and $p = 1.283$, respectively. Note that the *smaller* the casino power p, the *better* are your chances of winning, which are 100^p out of 2000^p, or $(.05)^p$. If $p = 1$, then the original bet is fair. Unfavorable bets will always have a casino power p bigger than 1. (The only exception is a bet with literally *no* chance to win, for which the casino power is undefined and, no doubt, of limited practical interest.) One of the nicest features of the notion of casino power is the fairly obvious principle that the chances of winning in a sequence of different types of bets are no greater than they would be if *all* of the bets had the same casino power as the best bet in the sequence. This simple principle makes it easy to refute a lot of the intricate betting schemes (particularly for craps) that are studied at length in the pseudo-scientific gambling literature.

The best casino power I know of for an unfavorable game is $p = 1.0081$ for craps in casinos that offer "double odds" side bets. This is obtainable by betting one-third of your current capital on the pass line and the remaining two-thirds behind the line. (For explanations of these terms, consult your local croupier.) Since casino chips don't come in fractions, you can only approximate this scheme in actual play, but casino powers of about 1.009 can be achieved in this way by players who are willing to risk everything at once. (Of course, the game of blackjack is favorable under proper conditions, and more limited bet sizes are advantageous in favorable games, where casino power is an irrelevant notion.)

What does all this have to do with statistics? Surprisingly enough, the same kind of calculations are needed to determine the chances of error in a statistical test. What's more, the most efficient methods for testing statistical hypotheses, those of sequential analysis, involve some of the same mathematical problems that a gambler is concerned with.

To understand these problems, consider the following hypothetical problem in statistics. You have a very large box containing millions of marbles, some of them white, the rest red. You want to pick at random a sample of a small number of marbles and infer which color the majority of marbles are. The classical methods of statistics lead to procedures like this: Determine in advance a sample size, take a sample of that size, and see which color most of the marbles in the sample are.

The chances of error in a test like this depend on the size of the sample and on the *actual* fraction of red marbles in the box. If 45 percent of the marbles are red, and we sample 121 (a convenient size for later comparison), then the probability of getting 61 or more red marbles in the sample is 13.5 percent, and this is the probability of error; if 48 percent of the marbles are red, the probability of error is much larger, namely, 33 percent. We can reduce the chances of error by taking a larger sample size. For example, sampling 241 marbles will yield error probabilities of 5.9 percent for 45 percent red and 26.7 percent for 48 percent red.

The main goal of statistical theory is to find methods of sampling and drawing inferences that are the most efficient — that is, have the smallest possible error probabilities for a particular sample size. Said another way, if we want specified error probabilities, then the most efficient tests are those that require the smallest possible sample size.

Going back to our problem with the marbles, it's hard to see how one could possibly improve upon the efficiency of the test that draws a sample and picks the color of the majority of that sample. In fact, there is a famous theorem in statistics that guarantees that no test taking a sample of 121 marbles can possibly improve upon the error probabilities of that test. Nevertheless, there *are* more efficient tests. These tests do not fix the sample size in advance, but instead sample marbles one at a time sequentially, following a previously specified rule for stopping the test and deciding which color predominates.

This approach to statistical inference, called sequential analysis, was developed in the mid-1940s by a mathematical statistician named Abraham Wald, whose book on the subject opened up new fields of research and applications. Wald's principal method, called the Sequential Probability Ratio Test (SPRT), gives recipes like the following for the marble problem: Sample one at a time until you have 11 more marbles of one color than the other. This could happen after 11 marbles, if all are the same color, or, perhaps, after 41 marbles are sampled, 26 white and 15 red. Whenever this situation occurs, the SPRT stops and decides the color that's ahead is in the majority in the box of marbles.

This process can be considered as a game in which a gambler bets even money that the next color drawn will be red. Think of the gambler starting with 11 chips and betting one at a time until he loses all 11 chips or wins 11 more chips. That corresponds exactly to the rule for carrying out an SPRT, but it is also an example of the famous "gambler's ruin problem." In fact, if the ratio of red to white marbles in the box is 9 to 10, this is just like betting on red in Las Vegas roulette.

What are the probabilities of error for the SPRT? They depend on the true percentages of the two colors in the box, just as they do for fixed sample size tests. If there are 45 percent red marbles, say, then an error is made if the gambler gets 11 ahead without previously getting 11 behind in a game where he has a 45 percent chance to win on each bet. To solve this "gambler's ruin problem," we can use the same idea of inventing a scoring system to make a fair game. Start off with a score of 1 and multiply the score by 55/45 every time he wins, and by 45/55 every time he loses. Since the chances of these are 45 out of 100 and 55 out of 100, respectively, *on the average* each bet multiplies his score by

$$\frac{45}{100}\times\frac{55}{45}+\frac{55}{100}\times\frac{45}{55}=\frac{55}{100}+\frac{45}{100}=1.$$

So on the average this score goes neither up nor down, and the game is fair. If he wins the 11 chips, his score at that time will be $(55/45)^{11}$, whereas if he loses, it will be $(45/55)^{11}$. If p and $1-p$ are his probabilities of winning and losing, then his average score when the game stops is

$$p\,(55/45)^{11} + (1-p)\,(45/55)^{11}.$$

Since the scoring system makes the game fair, this average score at the end must equal his score at the beginning, which was 1. That fact gives a simple equation to solve for p, and the answer is $p = .099$. Thus the probability of error is 9.9 percent for the SPRT when there are 45 percent red marbles in the box. For 48 percent red marbles, the same calculations with 45 and 55 replaced by 48 and 52 show that the error probability is 29.3 percent.

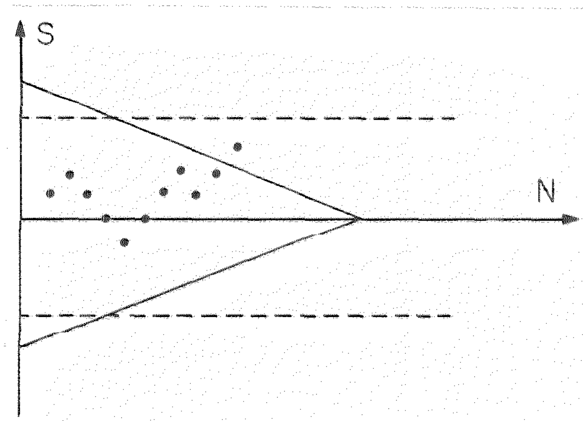
Why should a statistician want to use a test like the SPRT that imitates a gambling game? Because it is more efficient than fixed sample size tests. It turns out that the average number of marbles sampled by the SPRT depends on the proportions of red and white marbles in the box, but the worst case is the one where the two colors are equally numerous. In this case the sample size of the SPRT is 121, on the average — exactly the same as the fixed sample size test discussed earlier. So it makes sense to compare their error probabilities.

In the table below, which compares the error probabilities and average sample sizes of the SPRT with those of the fixed sample size test discussed earlier, notice that the SPRT yields improvements both in error probabilities and in average sample sizes. Of course, one can make other comparisons. For example, suppose we specify that the probability of error should be no more than 9.9 percent if 45 percent of the marbles in the box are of one color and 55 percent are of the other color. Then the smallest fixed sample size that accomplishes this is 164 — almost twice as large as the average sample size of the SPRT for these percentages of the two colors. In fact, Wald and Jacob Wolfowitz proved over 30 years ago that the SPRT is the most efficient possible test in a certain sense. For example, every other test needs a larger average sample size than 88.2 to obtain 9.9 percent error probabilities in the case of 45 percent or 55 percent red marbles.

	Probability of Test Error		Average Sample Size	
Fraction of red marbles in box	SPRT	Fixed sample size	SPRT	Fixed sample size
45%	9.9%	13.5%	88.2	121
48%	29.3%	33.0%	113.8	121
50%	—	—	121	121
52%	29.3%	33.0%	113.8	121
55%	9.9%	13.5%	88.2	121

Other efficiency criteria are important, however, and it turns out that the average sample size of 121 in the worst case can, in fact, be made smaller by choosing a different sequential test having the same 9.9 percent error probabilities. Finding tests that have the smallest possible average sample size in the worst case has been recognized as an important problem in statistical theory since it was first studied over 20 years ago by Jack Kiefer and Lionel Weiss. Among the difficulties associated with the problem is the fact that, except for situations like our marble problem that have a symmetrical character, it is not possible to identify the worst case in advance. It depends on the sequential test that is used. What we really want is the test whose worst performance is better than the worst performance of all other tests with the same error probabilities.

The problem of finding such tests, called "Kiefer-Weiss solutions," remains unsolved. However, work done in the last few years at Caltech has produced practical methods for constructing sequential tests that come close to solving it, and are, therefore, nearly as efficient as possible in this sense. The kind of sequential test that accomplishes this can be described most easily by a picture. Suppose that after we sample each marble, we plot a point on a graph of S versus N (above, right), where N is the number of marbles we've sampled so far, and S is the number of red marbles so far, minus the number of white marbles. Then an SPRT can be pictured by drawing a pair of parallel lines (dotted lines in the picture) that correspond to particular values of S, like +11 and −11. Whenever one of the plotted points reaches one of the dotted lines, the SPRT stops sampling. The solid lines in the picture, on the other hand, represent another sequential test, which stops sampling when one of the plotted points reaches or crosses over one of these lines.



My own research on the Kiefer-Weiss problem identified a class of sequential tests that look like the solid-line picture, which I called 2-SPRTs because they are constructed from pieces of two different SPRTs in a certain way. It turned out that one could prove that these sequential tests are very nearly the best possible in a sense closely related to the Kiefer-Weiss problem. My student, Michael Huffman, recently showed in his PhD thesis how to choose from the class of all 2-SPRTs those tests that will come the closest to solving the Kiefer-Weiss problem. Though all of this theory is what statisticians call "asymptotic" (that is, based on what would happen if the sample sizes were very large), it is possible to make very detailed calculations on a computer that show in particular situations just how close these methods come to solving the Kiefer-Weiss problem. In typical applications, the answer seems to be that one achieves about 97 or 98 percent of the best possible efficiency.

Current research in sequential analysis is expanding the scope of both theory and applications. Recent work has led to the development of useful sequential procedures in many classical areas of statistics, such as the analysis of variance, nonparametric statistics, and the design of experiments. A growing stockpile of techniques has taken the theory of sequential analysis far beyond the study of the Sequential Probability Ratio Test and its refinements. Some of the problems now being solved by sequential analysis methods, such as reactions to trends and changes in distribution, and assignment of confidence intervals with fixed precision regardless of the variance of the data, cannot be dealt with at all by fixed sample size techniques. Along with all of this usefulness and respectability comes the continuing realization that sequential analysis, perhaps more than any other branch of statistics, is firmly rooted in the phenomena that govern one of mankind's most cherished disreputable pastimes: gambling. □

A Change for Gates

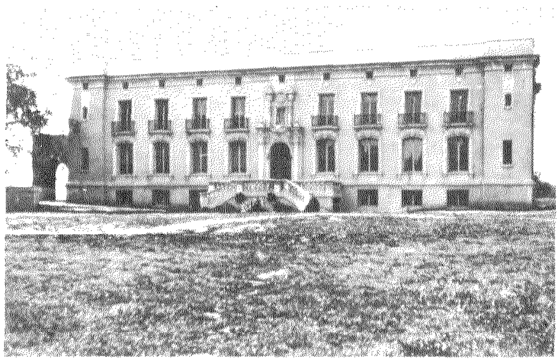
When the 1971 San Fernando earthquake exposed the structural weakness of Gates Laboratory, all the occupants and activities of the second oldest building on campus had to be relocated, and the beautiful old structure has stood empty and idle ever since. In fact, just to retain the shell required gutting the interior and reinforcing the walls with structural steel and gunite. But someday, it was decided, Gates would be renovated and used for the administrative offices displaced from Throop Hall, which was so badly damaged in the same earthquake that it had to be destroyed.

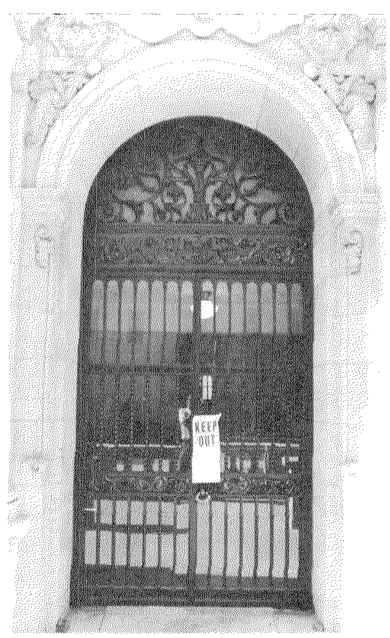
Now "someday" seems about to begin. The Ralph M. Parsons Foundation of Los Angeles has made a grant of $1,000,000 to convert the second floor of Gates — which will be renamed the Parsons Building — into offices for the president, provost, and vice presidents of Caltech, and to install the necessary mechanical and electrical systems, plumbing, furnishings, and an elevator. An additional $1,500,000 is needed to complete the renovation of the building's first, third, and basement floors. Achieving that will make it possible for most of the Institute's administrative staff to be housed in one building again — thus relieving the space crunch in the academic buildings into which these officers have been shoehorned since 1972.

The difference between $2,500,000 to convert a standing building into a usable structure and the $70,000 Gates originally cost is an impressive testimony to the changes in the economy since 1917 (when Gates was completed) and 1981 (when its rebuilding will begin). Going from serving chemistry to serving administration is no small change either — and it gives us an excuse for a nostalgic look at some photographs from the olden days.

The pictures in the lefthand column on the opposite page are, first, Gates as it looked in 1916. It was a shell then too, with the interior fittings still to be installed. Next is a 1921 photograph of the freshman chemistry laboratory on the first floor. Interestingly, the students all seem to be wearing identical shirts. At the bottom, a solemn moment in the 1925 commencement, one of many graduation ceremonies conducted from the porch on the east side of Gates.

The picture at the top right was taken in the 1930s and shows the ornate facade of Gates in detail. At the bottom is a closeup of the main door with the "Keep Out" sign that was hung there after the building was evacuated in 1972. One of these days, it's nice to know, a welcome mat will be laid down, and a gracious, elegant old building — shown above as it looks today — will be back in business. □

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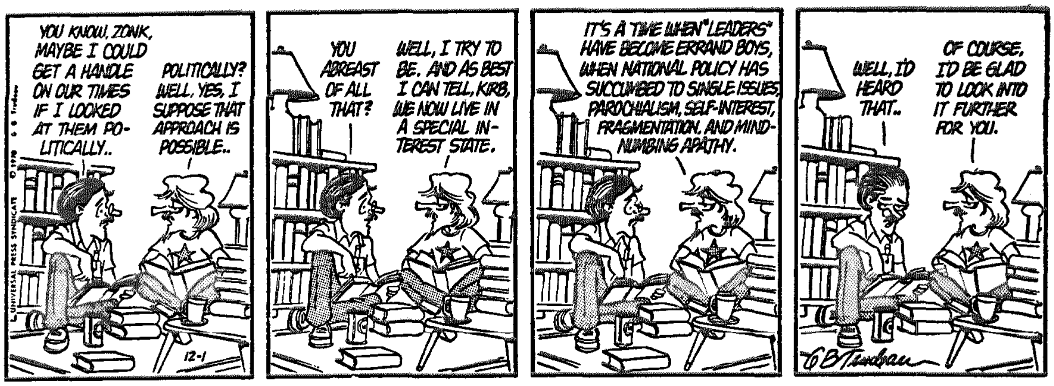


The Decline of Collective Responsibility in American Politics

by MORRIS P. FIORINA

As political parties have weakened, it has become easy for politicians to give us public relations rather than government.

*Though the Founding Fathers believed in the ne*cessity of establishing a genuinely national government, they took great pains to design one that could not lightly do things *to* its citizens; what government might do *for* its citizens was to be limited to the functions of what we know now as the "watchman state." Thus, the Founders constructed the constitutional system familiar to every schoolchild — federalism, separation of powers, and numerous points of check and balance. The institutional arrangements bequeathed to us hamper efforts to undertake major initiatives and favor maintenance of the status quo.

Given the historical record faced by the Founders, their emphasis on constraining government is understandable. But we face a later historical record, one that shows 200 years of increasing demands for government to act positively. Regrettably, however, the increasing irresponsibility of American politics makes it more difficult than ever to use government for positive purposes.

To say that some person or group is responsible for a state of affairs is to assert that he/they have the ability to take legitimate actions that have a major impact on that state of affairs. More colloquially, when someone is responsible we know whom to blame. Human beings have asymmetric attitudes toward responsibility, as captured by John Kennedy's comment that "success has a thousand fathers, but failure is an orphan." This general observation certainly applies to politicians, which creates a problem for democratic theory, because clear location of responsibility is vitally important to the operation of democratic governments. Without responsibility citizens can only guess at who deserves their support; the act of voting loses much of its meaning. Moreover, as the Founders clearly (and pessimistically) foresaw, only if elected representatives know that they will be held accountable for the results of their decisions (or nondecisions as the case may be) do they have a compelling personal incentive to govern in our interest.

In an autocracy, the location of power in a single individual locates responsibility in that individual as well. But individual responsibility is insufficient whenever more than one person shares governmental authority. We can hold a particular congressman individually responsible for a personal transgression such as bribe-taking. We can even hold a president individually responsible for military moves where he presents Congress and the citizenry with a *fait accompli*. But on most national issues individual responsibility is difficult to assess. If one were to go to Washington, randomly accost a Democratic congressman, and berate him about a 20 percent rate of inflation, imagine the response. More than likely it would run, "Don't blame me, if 'they' had done what I've advocated for ____ years, things would be fine today." And if one were to walk over to the White House and similarly confront Carter, he would respond as he already has, by blaming Arabs, free-spending congressmen, special interests, and of course, us.

American institutional structure makes this kind of buck-passing all too easy. In order to overcome it we must lay the credit or blame for national conditions on all those who had any hand in bringing them about: Some form of *collective responsibility* is essential.

The only way collective responsibility has ever existed and can exist, given American institutional arrangements, is through the agency of the political party. According to the textbook argument, a strong party can generate collective responsibility by creating incentives for leaders, followers, and popular supporters to think and act in collective terms. First, by providing party leaders with the capability (for example, control of institutional patronage and nominations) to discipline party members, genuine leadership becomes possible. Second, the subordination of individual officeholders to the party lessens their ability to separate themselves from party actions. Like it or not, their performance becomes identified with the performance of the collectivity to which they belong. Third, with individual candidate variation greatly reduced, voters have less incentive to support individuals and more to support or oppose the party as a whole. And fourth, party line voting in the electorate provides party leaders with the incentive to propose policies that will earn the support of a national majority, and party back-benchers with the personal incentive to cooperate with leaders in the attempt to compile a good record for the party as a whole.

There is considerable slippage between textbook and reality, but in the American context strong parties have traditionally unified politics by enabling citizens to direct their gratitude or ire to a clear target — the governing party. And when citizens assess responsibility on the party as a whole, party members have a personal stake in their collective performance. They have little to gain from gutting their president's program one day, then attacking him for lack of leadership the next, since they share in the president's fate when voters do not differentiate within the party.

Admittedly, party responsibility is a blunt instrument. The objection immediately arises that party responsibility condemns junior Democratic representatives to suffer electorally for an inflation that they could do little to affect. An unhappy situation, true, but unless we accept it, Congress as a whole escapes electoral retribution for an inflation they *could* have done something to affect. The choice is between a blunt instrument or none at all.

Though the United States has neither the institutions nor the traditions to support a British brand of party government, in the past we have experienced eras in which party was a much stronger force than today. And until recently — a generation roughly — parties have provided an "adequate" degree of collective responsibility. They have done so by connecting the electoral fates of party members, via presidential coattails, for example, and by transforming elections into referenda on party performance, as with congressional off-year elections.

To elaborate, in earlier times, when citizens voted for the party, not the person, parties had incentives to nominate good candidates because poor ones could have harmful fallout on the ticket as a whole. In particular, the existence of presidential coattails provided an inducement to avoid the nomination of narrowly based candidates, no matter how committed their supporters. And once in office, the existence of party voting in the electorate provided party members with the incentive to compile a good *party* record.

In the contemporary period, however, even the preceding tendencies toward collective responsibility have largely dissipated. As background for a discussion of this contemporary weakening of collective responsibility and its deleterious consequences, let us briefly review the evidence for the decline of party in America.

The Continuing Decline of Party in the United States

Party is a simple term that covers a multitude of complicated organizations and processes. Party manifests itself most concretely as the set of party organizations that exist principally at the state and local levels. Party manifests itself most elusively as a psychological presence in the mind of the citizen. Somewhere in between and partly a function of the first two is the manifestation of party as a force in government.

Party Organizations: In the United States party organization has traditionally meant state and local party organization. The national party generally has been a loose confederacy of subnational units that swings into action for a brief period every four years. Though such things are difficult to measure precisely, there is general agreement that the formal party organizations have undergone a secular decline since their peak at the end of the 19th century. The prototype of the old-style organization was the urban machine, a form approximated today only in Chicago.

Several long-term trends have served to undercut old-style party organizations. Briefly, the organizations' resources have withered in the face of continued attacks on the patronage system and on party control of nominations. The social welfare functions of the parties have passed to the government as the modern welfare state developed. And less concretely, the entire ethos of the old-style party organization has been increasingly at odds with modern ideas of government based on rational expertise.

In the 1970s two series of reforms further weakened the influence of organized parties in American national politics. The first was a series of legal changes deliberately intended to lessen organized party influence in the presidential nominating process. In the Democratic party "New Politics" activists captured the national party apparatus, and imposed a series of rules changes designed to "open up" the politics of presidential nominations. The Republican party — long more amateur and open than the Democratic — adopted weaker versions of many of the Democratic rules changes. Table 1 shows that the presidential nomination process has indeed been opened up. In little more than a decade after the disastrous 1968 Democratic conclave, the number of primary states has more than doubled and the number of delegates chosen in primaries has increased from little more than a third to three-quarters. Moreover, the remaining delegates emerge from caucuses far more open to mass citizen participation than previously, and the delegates themselves are more likely to be amateurs than previously. For example, in the four conventions from 1956 to 1968 more than 70 percent of the Democratic party's senators, 40 percent of its representatives, and 80 percent of its governors attended. In 1976 the figures were 18 percent, 15 percent, and 47 percent respectively.

A second series of 1970s reforms lessened the role of formal party organizations in the conduct of political campaigns. These are financing regulations growing out of the Federal Election Campaign Act of 1971 as amended in 1974 and 1976. In this case the reforms were aimed at cleaning up corruption in the financing of campaigns; their effects on the parties were a by-product, though a predictable one. Serious presidential candidates are now publicly financed. Though the law permits the national party to spend two cents per eligible voter on behalf of the nominee, it virtually requires the candidate to set up a finance committee separate from the national party.

Table 1: Recent Changes in Presidential Nomination Process

	Number of States Holding Primaries	Percentage of Delegates Selected in Primaries	
		Democratic	Republican
1968	17	38	34
1972	23	61	53
1976	30	73	68
1980	36	76	76

Source: 1968-1976, figures from Austin Ranney, "The Political Parties: Reform and Decline," in Anthony King (ed.), *The New American Political System* (Washington: American Enterprise Institute, 1978), Table 6—1. 1980 figures from *National Journal*, October 20, 1979, 1738-1739.

At the present time only presidential candidates enjoy public financing. But a series of new limits on contributions and expenditures affects other national races. Prior to the implementation of the new law, data on congressional campaign financing was highly unreliable, but even in the short time the law has been in effect, some disturbing trends have emerged. Party financing of congressional races has dropped from about one-sixth of the total in 1972 to about one-fifteenth of the total in 1978. Political action committees (PACs) and individually wealthy candidates have made up the difference. The limits in the new law restrict the House candidates to no more than $15,000 in funding from each of the national and relevant state parties (the average campaign expenditure of an incumbent in 1978 was about $121,000; of a challenger, about $54,000). A senator is permitted to receive a maximum of $17,500 from his senatorial campaign committee, plus two cents per eligible voter from the national committee and a like amount from the relevant state committee (21 senatorial candidates spent over a million dollars in 1978).

Yet there is less here than meets the eye. If the national party were to contribute $15,000 to each of its congressional candidates, and a flat $17,500 to each of its senatorial candidates, that would be more than $8 million. *All* levels of the parties contributed only $10.5 million of the $157 million spent in 1978 congressional races. Probably more constraining than limits on what the parties can contribute to the candidates are limits on what citizens and groups can contribute to the parties. Under current law individual contributors may give $1,000 per election to a candidate (primary, runoff, general election), $5,000 per year to a political action committee, and $20,000 per year to a party. From the standpoint of the law, each of the two great national parties is the equivalent of four political action committees.

The ultimate results of such reforms are easy to predict. A lesser party role in the nominating and financing of candidates encourages candidates to organize independent campaigns. And independent conduct of campaigns only further weakens the role of parties. Of course, party reform is not the entire story. Other modern-day changes contribute to the diminished party role in campaign politics. For one thing, party foot soldiers are no longer so important, given the existence of a large leisured middle class, which participates out of duty, enjoyment, or whatever, but which participates on behalf of candidates and issues rather than parties. Similarly, contemporary campaigns rely heavily on modern technology — survey research, the mass media, and modern advertising methods — which is provided by independent consultants outside the formal party apparatus. Although these developments are not directly related to the contemporary reforms, their effect is the same: The diminution of the role of parties in conducting political campaigns. And if parties do not grant nominations, fund their choices, and work for them, why should those choices feel any commitment to their party?

Party in the Electorate: In the citizenry at large, party takes the form of a psychological attachment. The typical American traditionally has been likely to identify with one or the other of the two major parties. Such identifications are transmitted across generations to some degree, and until recently they tended to be fairly stable. Prevailing party attachments, of course, are based on the dislocations of the Depression period and the New Deal attempts to alleviate them. Though only a small proportion of those who experienced the Depression directly are active voters today, the general outlines of citizen party identifications much resemble those established at that time.

The times are changing, however. In Table 2 we can see that, as the 1960s wore on, the heretofore stable distribution of citizen party identifications began to change in the general direction of weakened attachments to the parties. And as the strength and extent of citizen attachments to the parties declined, the influence of party on the voting decisions of the citizenry similarly declined. The percent of the voting-age population that reports consistent support of the same party's presidential candidate drops from more than two-thirds in 1952 to less than half in 1976. The percent of voters who report a congressional vote consistent with their party indentification has declined from over 80 percent in the late 1950s to under 70 percent today. And ticket splitting, both at the national and subnational levels, has doubled since the time of the first Eisenhower election.

Why has party in the electorate declined? To some extent the decline results from the organizational decline. Few party organizations any longer have the tangible incentives to turn out the faithful and assure their loyalty. Candidates run independent campaigns and deemphasize their partisan ties whenever they see any short-term gain in doing so.

Certain long-term sociological and technological trends also appear to work against party in the electorate. The population is younger, and younger citizens traditionally are less attached to the parties than their elders. The population is more highly educated; fewer voters need some means of simplifying the choices they face in the political arena, and party has been the principal means of simplification. The media revolution has vastly expanded the amount of information easily available to the citizenry. Candidates would have little incentive to operate campaigns independent of the parties if there were no means to apprise the citizenry of their independence. The media provide the means.

And finally, our present party system is an old one. For increasing numbers of citizens, party attachments based on the Great Depression seem lacking in relevance to the problems of the late 20th century. Beginning with the racial issue in the 1960s, proceeding to the social issue of the 1970s, and to the energy, environment, and inflation issues of today, the parties have been rent by internal dissension. Sometimes they failed to take stands, at other times they took the wrong ones from the standpoint of the rank and file, and at most times they have failed to solve the new problems in any genuine sense. Since 1965 the parties have done little or nothing to earn the loyalties of modern Americans.

Table 2: Subjective Party Identification, 1960-76

Party ID	1960	1964	1968	1972	1976
Strong Democrat	21%	27%	20%	15%	15%
Weak Democrat	25	25	25	26	25
Independent Democrat	8	8	9	10	12
Independent	8	8	11	13	14
Independent Republican	7	6	9	11	10
Weak Republican	13	13	14	13	14
Strong Republican	14	11	10	10	9

Source: National Election Studies made available by the InterUniversity Consortium for Political and Social Research, University of Michigan.

Party in Government: If the organizational capabilities of the parties have weakened, and their psychological ties to the voters have loosened, one would expect predictable consequences for the party in government. In particular, one would expect to see an increasing degree of split party control within and across the levels of American government. The evidence on this point is overwhelming.

At the state level 27 of the 50 governments were under divided party control after the 1978 election (compared to 16, 20 years ago). At the federal level the trend is similar. In 1953 only 12 states sent a U.S. senator of each party to Washington. The number increased to 16 by 1961, to 21 by 1972, and stands at 27 today. The tendency for congressional districts to support a congressman of one party and the presidential candidate of the other steadily increased from 3 percent in 1900 to 42 percent in 1972.

Seemingly unsatisfied with the increasing tendencies of the voters to engage in ticket-splitting, we have added to the split of party in government by changing electoral rules in a manner that lessens the impact of national forces. For example, in 1920, 35 states elected their legislators, governors and other state officials in presidential election years. In 1944, 32 states still did so. But in the past generations the trend has been toward isolation of state elections from national currents: As of 1970 only 20 states still held their elections concurrently with the national ones.

The increased fragmentation of the party in government makes it more difficult for government officeholders to work together than in times past (not that it has ever been terribly easy). Voters, in turn, have a more difficult time attributing responsibility for government performance, and this only further fragments party control. The result is lessened collective responsibility in the system.

In recent years it has become a commonplace to bemoan the decline of party in government. National commentators nostalgically contrast the Senate under Lyndon Johnson with that under Robert Byrd. They deplore the cowardice and paralysis of a House of Representatives supposedly controlled by a two-thirds Democratic majority under the most activist, partisan speaker since Sam Rayburn. And, of course there are the unfavorable comparisons of Jimmy Carter to previous presidents — not only FDR and LBJ, but even Kennedy. But it is not enough to call for more inspiring presidential leadership and to demand that the majority party in Congress show more readiness to bite the bullet. Our present national problems should be recognized as the outgrowths of the increasing separation of the presidential and congressional electoral arenas.

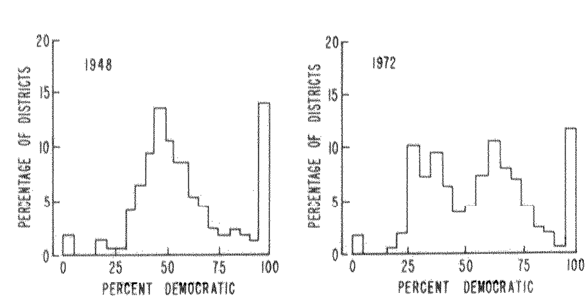
By now it is widely understood that senatorial races are in a class by themselves. The visibility of the office attracts the attention of the media as well as that of organized interest groups. Celebrities and plutocrats find the office attractive. And so, massive media campaigns and the politics of personality increasingly affect the senatorial voting. Senate elections now are most notable for their idiosyncrasy, and consequentially for their growing volatility; correspondingly, such general forces as the president and the party are less influential in the senatorial voting today than previously.

What is less often recognized is that House elections have grown increasingly idiosyncratic as well. I have already discussed the declining importance of party identification in House voting, and the increasing number of split results at the district level. These trends are both cause and consequence of incumbent efforts to insulate themselves from the electoral effects of national conditions.

Figure 1 shows the distribution of the vote garnered by the Democratic candidate in incumbent-contested districts in 1948 and 1972. Evidently a massive change took place in the past generation. Whereas in 1948 most congressional districts were clustered around the 50 percent mark (an even split between parties), most districts now are clustered away from the point of equal division. Two obvious questions arise: Does this change matter, and why has it occurred?

Taking the second question first, the figure suggests a bleak future for such electoral phenomena as presidential coattails and midterm referenda on presidential performance. In the world represented by the 1948 diagram, a swing of 5 percent in the congressional vote due to a particularly attractive or repulsive presidential candidate, or an especially poor performance by a president, has major consequences: It shifts a large proportion of districts across the 50 percent mark. The shift provides a new president with a "mandate" in an on-year election and constitutes a strong "message" to the president in an off-year election. In the world represented by the 1972 diagram, however, the hypothesized 5 percent shift has little effect: Few seats are close enough to the tipping point to shift parties under

Figure 1: Congressional Vote in Districts with Incumbents Running



the hypothesized swing. The president's victory is termed a "personal" victory by the media, or the midterm result is interpreted as a reflection of personal and local concerns rather than national ones.

Why has the distribution of the congressional voting results changed form over time? Recent research indicates that a variety of personal and local influences exert an increasingly important influence on citizen evaluations of their representatives. Along with the expansion of the federal presence in American life the traditional role of the congressman as an all-purpose ombudsman has greatly expanded. Tens of millions of citizens now are directly affected by federal decisions. Myriad programs provide opportunities to profit from government largesse, and myriad regulations impose costs and/or constraints on citizen activities. And whether seeking to gain profit or avoid costs, citizens seek the aid of their congressman. To many citizens the contribution of the congressman in the realm of district service appears considerably greater than the impact of his or her single vote on major national issues, and they respond rationally to this modern state of affairs by weighing nonprogrammatic constituency service heavily when casting their congressional votes. This emphasis on the part of constituents provides the means for incumbents to solidify their hold on the office. Even if elected by a narrow margin, diligent service activities enable a congressman to neutralize or even convert a portion of those who would otherwise oppose him on policy or ideological grounds. Emphasis on local, nonpartisan factors in congressional voting enables the modern congressman to withstand national swings whereas yesteryear's uninsulated congressmen were more dependent on preventing the occurrence of the swings.

Actually, the insulation of the modern congressman from national forces is even more complete than the preceding discussion suggests. Not only are few representatives so vulnerable that a reaction to a presidential candidate or his performance would turn them out of office, but such reactions themselves are less likely to find a reflection in the congressional voting. As congressmen increasingly build personal organizations and base their campaigns on local issues and their personal record of service

continued on page 30

Frederick C. Lindvall

—How It Was

Frederick C. Lindvall, professor of engineering emeritus, was interviewed by Ann Underleak Scheid for the Oral History Program of the Caltech Archives. E&S has made a shortened version of the original transcript and presents here Part One (of two parts).

Frederick Lindvall: I was born in Moline, Illinois, where my father had a drugstore. In those days a pharmacist had to do a good deal more in the way of compounding medicines and salves than happens today, and I learned a fair amount of chemistry from listening to my father talk and watching him work in the store. So when I got to high school chemistry, it was a breeze; I'd heard most of the words before.

All the time I was growing up, we had living with us my mother's sister, who was an elementary school teacher, and she took a great deal of interest in my education. She would know if I wasn't doing well in school and would help me. Also she was much interested in what I might do as a career ultimately, so she arranged through friends in the community to let me see various professionals in action — such as visiting a court where she knew one of the judges, visiting the Rock Island Railroad shops where she knew the superintendent, and visiting the surgery at a local hospital where one of the doctors let me stand around and watch. One way or another, I got a fair idea of what the different professions were up to.

My father used to take us to conventions of the American Pharmaceutical Association. At one of those I met Dr. Dohme, who later founded Sharp and Dohme, a pharmaceutical firm. He asked me what I was going to do when I grew up, and I said I thought I wanted to be an engineer. His answer to that was, "What do you want to do that for? You'll probably work for somebody else all your life. Why don't you get into a business such as mine, where you can be your own boss?" I didn't take his advice. I wanted engineering, perhaps because I'd always been interested in gadgets and what not. It was a source of concern to my parents when I would, on receiving a new toy, immediately start taking it apart. In those days, toys were put together with nuts and bolts rather than rivets and spot welding, so it was possible to take them apart and find out how things worked.

I went through high school in Moline, and before I finished, my father sold his drugstore. The next year, 1914, the whole family went on a trip to Europe. My father and mother thought it would be nice, for sentimental reasons, to spend their 25th wedding anniversary in Sweden as near my father's birthplace as they could, which they did. We were touring around Europe and were stuck in Switzerland when World War I broke out. It took us quite a while to get permission to cross France and get over to England, but we finally made it. At that time the Germans were making their big push toward Paris. I remember we stayed overnight in Dijon, and I could hear the big guns firing. It made quite an impression on me. We finally got passage on a ship back to the States. We made the trip with the portholes blocked with brown paper, and we took a northern route to escape submarines and landed at Boston instead of New York as scheduled.

In 1920 my parents decided that they'd lived long enough in Moline, considering my father's hay fever every year. So they came to Los Angeles, and I enrolled in what was then called the Southern Branch of the University of California, which was located in what was the old normal school on Vermont Avenue in L.A. The school offered the first two years of engineering, and I spent that time getting the basic chemistry, physics, math, surveying, and so on.

From there I transferred to the University of Illinois because I had made up my mind that I wanted to be in the railway business. So I enrolled in what they called railway electrical engineering, which was a promising subject at the time. After I finished the course, I received two or three railroad offers, but the man who was head of the department was honest enough to caution me. He said, "This railroad business is a tough game. If you live long enough, through the seniority system, you might get to be foreman of a roundhouse." That was a bit discouraging, so I wrote out to the Los Angeles Railway Company, which ran the yellow cars in L.A. at the time, to see if there was a chance of going to work for them. I received a rather curt letter back, saying they had no need for anyone with my training.

I came back out here anyway and went down to the railroad car shops — that is, the street railway car shops — and talked to the superintendent, and he gave me a job wiring streetcars. They were buying a whole group of new cars at the time, and they came only partly equipped. My job was to put in the electrical part of the equipment. That was good experience. I learned a lot about people — what the reactions of shop men were, for example. After a few months of that, I saw the chief engineer walking through the shop one day, and I said to him, "I think I've learned all I want to know about streetcar wiring; do you think you have anything closer to engineering that I could move into?" From that moment, the men in the shop didn't like me any more. They thought I was a spy. But I received an offer of a position uptown, what they called inspector. My job was to keep track of the system outside, where the underground cables for the return power from the trolley cars went back to the substations. They were having trouble with excavation machinery coming along and cutting through these cables. I stayed with that job another six or seven months, but in my summer vacation up in the high Sierra, which I suppose is a good place for introspection, I decided there was no future in the street railway business.

I had a couple of good friends from my early days at SBUC who thought Caltech was a wonderful place. One of them, Bill Holladay, later became president of the Caltech Alumni Association. Loys Griswold was another. So I came over here and talked to Professor Royal Sorensen about studying electrical engineering. He didn't know anything about me, so he was properly noncommittal, but after I sent in my transcripts, he said they would like to have me as a graduate student. I came in the fall of 1925 and worked for Professor Francis Maxstadt in the laboratory as a teaching assistant for the next three years, till I got my PhD in 1928. There was an interesting class that year — Arnold Beckman, Hugh Hamilton, and Charlie Richter. Clark Millikan and Morgan Ward were also here and were my good friends.

Ann Scheid: Was the public aware of Caltech at that time?

FL: Not very much. We heard about it at the time we were finishing over at the Southern Branch, because when you've had two years of engineering you look around for where you can get the rest of it. And Caltech was mentioned.

AS: What were the courses you took at Caltech that you particularly enjoyed?

FL: At that time, people who were starting graduate study in electrical engineering were encouraged by Professor Sorensen to get all the physics and math that they could. He had the philosophy that you shouldn't establish a great many highly specialized electrical engineering courses, so I was in the same physics and advanced math classes as people who were taking physics. In fact, at the time I got my PhD, the requirements for physics and engineering were such that my degree could have been labeled either one, and I chose the electrical engineering label.

I had some very interesting people as teachers. Dr. Smythe was a fellow who separated the men from the boys with the demands of his course in electricity and magnetism. I had lectures in thermodynamics from Paul Epstein — beautiful polished lectures that I enjoyed very much. But I was grateful, along with all the rest of the class, when just before final examination day, he posted a notice that he had concluded that the material in the course did not lend itself to an examination. Harry Bateman, an English-trained mathematician, offered a course in advanced vector analysis, which I took. It caused me a good deal of difficulty because it turned out to be pretty much a transformation-of-coordinates type of course. In other words, Dr. Bateman would give an hour lecture on whatever he happened to be working on at the time, and then he would usually say, "As an exercise, put this into vector analysis form." It made us work, but I learned a good deal from it.

I had an interesting course from Fritz Zwicky, who taught what was really sort of an introduction to thermodynamics of a broader sense than I had encountered as an engineering student. Engineers at that time were concerned more or less with the thermodynamic properties of steam and refrigeration vapors. Zwicky broadened it to include a lot of the basic things that are in physical chemistry and an overall energy approach to problems.

I also took a course in atomic physics from Richard Tolman. We were using a new book at the time — on atomic structure and spectral lines — and we had to sweat it through in the German language. Dr. Tolman was so frank and earnest that when some bright guy would ask a question, Tolman would say, "Don't rush me on that; that's in tomorrow's lesson." Tolman himself was a very stimulating person, and I also took from him an introduction to relativity course that involved a lot of his personal philosophy about science.

AS: It sounds as though the education at Caltech was very theoretical.

FL: Oh, it was. On the engineering side, we studied very practical matters of transmission-line design, high-voltage transmission of power. At that time, the mathematics for doing that was in the development stage, so we had to do a lot of our calculations the hard way. Convenient tables of functions simply didn't exist then.

During the years I was a graduate student here at Caltech, I lived in a bachelor shack with three friends — Harvey Cameron, Hallam (Dick) Mendenhall, and Arthur Warner. Cameron had just gotten his PhD, and he was Robert Millikan's first assistant in cosmic rays when cosmic rays were hardly an intellectually respectable subject of investigation. Dick Mendenhall got his degree a year before I did, and he later went on to Bell Labs. He owes his life to Professor Sorensen, literally, because in working with what we called the vacuum switch in the high-voltage laboratory, Mendenhall reached up to make an electrical connection and got hold of the high-voltage power service coming into the laboratory. His feet were terribly burned. Professor Sorensen used artificial resuscitation to get him breathing again by the time the ambulance came. He spent months afterward in the hospital, having skin grafts for his burns, but they never did heal properly. But he's made a career on crutches and canes since, and it never got his spirit down. And then there was Arthur Warner, who was my first physics instructor at SBUC. The four of us lived in the Chester Courts, which has since disappeared. It was there I first learned to do a little elementary cooking, and I've kept on with it and enjoyed it ever since.

One of the things Mendenhall and I missed was something of a humanistic-social science nature in the graduate work here at Caltech. So we spoke to Professor Judy, who was then chairman of the humanities division, and asked if it would be possible to have some kind of graduate seminar rather than just being welcome to sit in on an undergraduate class. So he started an informal seminar that used to meet at his house, which was a lovely place for meetings. He'd built the house around his books; he had a tremendous personal library. Among those who used to go to that seminar were Charlie Richter, Clark Millikan, Fritz Zwicky, and Graham Laing, who was an economics professor. We met once a week, and each of us in turn would present a paper on some nontechnical subject.

AS: What kinds of papers were presented?

FL: Well, Zwicky, for instance, gave a review of a then new book, *Decline of the West* by Oswald Spengler. None of us understood it very well, and I don't think he did either, but it stimulated a lot of discussion. Clark Millikan gave a paper on James Branch Cabell, and in a discussion of that Graham Laing came through with a number of his shady jokes. We all urged Laing to record or write down his limericks; he had a vast store of them in both

French and English. But he never did. I gave a paper on Professor Stewart Sherman, a professor at Illinois who had written a number of books of literary criticism. Charlie Richter gave a talk on Dante's *Inferno*. He confessed that he was studying Italian at that time, so he could read it in the original.

AS: You mentioned that you had a course with Tolman and the text was in German. You knew German then?

FL: Well, German was one of the languages we had to learn and pass an examination in. Fortunately, it was just a reading examination.

A lot of my work was on the vacuum switch, which was an idea of Professor Sorensen's. It was well known that a high vacuum was a good insulator, but could it be used as a medium in which electric current could be interrupted? There were about as many people who argued that it could as argued that it couldn't. Those who said it couldn't said, "An arc would form, and it would never go out. The metallic ions from the electrodes would keep coming out and maintain the arc." On the other side there were those who said, "With an alternating current, the current has to go through zero twice in each cycle. And at that point, if the metallic ions got out of the way, there would be a good vacuum again, so the arc would not re-strike." And indeed, that's the way it worked.

We were able to use the high-voltage laboratory as a source of power to do switching with small switches, but we built a larger single-pole switch that we took out to one of the Edison Company's substations and demonstrated. It was a pretty impressive demonstration because in those days the circuit breakers were oil filled and they would from time to time blow up, starting fires, and otherwise being messy. So there was a good deal of interest in anything that could replace them. With the cooperation of the Southern California Edison Company, we built a three-phase switch that — like many of Professor Sorensen's ideas — was really too far ahead of its time. Not enough was known about high-vacuum technique and how to handle it or how to get clean metals for the system. So it was never very successful, though it worked if we kept the pumps going vigorously and didn't allow any leakage to occur.

It wasn't until nearly 30 years later that one of the early Caltech students, James Cobine, at General Electric Company, brought the vacuum switch into use. By that time, vacuum techniques and clean metals and a better understanding of metallic arcs had been achieved, so it was possible to make a commercially acceptable switch. Incidentally, Sorensen's original vacuum switch is on display at the Smithsonian Institution in Washington.

Royal Sorensen with the 1923 model of his vacuum switch.

AS: What were some of Professor Sorensen's other ideas?

FL: First of all, he had the idea of a high-voltage testing laboratory. He got together with the Edison Company, which was really pioneering nationally in high-voltage transmission, and offered to design transformers if Edison would build a lab. He had done transformer design at General Electric before he came to Caltech. Anyway, at Caltech he designed these high-voltage transformers and the cascade interconnection so that you could get a million volts out with enough current to be realistic. General Electric was unsure of his design, although you might say he was one of their old alums, so Westinghouse built the transformers, though they wouldn't guarantee what they would do when interconnected. But after they were working successfully, Westinghouse put the "W" nameplate on them, as I remember the story.

In the high-voltage lab, Professor Sorensen and his graduate students also worked on some of the problems of high-voltage transmission. One of his concepts was that sooner or later the whole Pacific Coast would be tied together in one big high-voltage transmission system. I remember when that paper was presented at a technical meeting, it was almost laughed out of the meeting. But it wasn't very many years later that Edison and Pacific Gas & Electric were interchanging power, and then the large-scale power transmission grid was extended to the Pacific Northwest and down to San Diego.

Edison had a small research crew that used the lab about half of the time, and they studied some of the practical problems of insulators — the insulator strings that supported the transmission wires. One of these problems was the nature of flashovers. The laboratory crew would build up the voltage until the insulator strings would flash over, and then the Edison people would design guard rings and the like, so that the flash would not cascade over the porcelain insulators and crack them from the heat. They worked out a method of washing these insulators with a high-pressure water jet and found out that it could be done without shutting the power down. They did a lot of other little housekeeping details that were necessary before they could go up to 230,000 volts on the line that was built from Big Creek.

AS: How long had Edison been associated with Caltech in this cooperative effort?

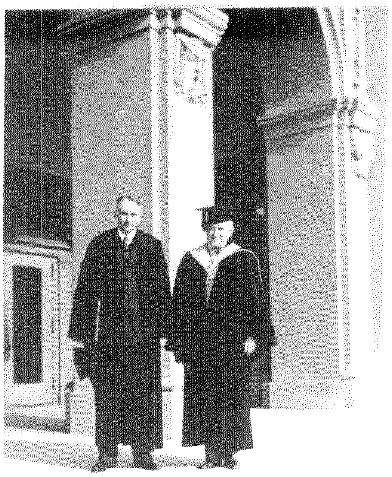
FL: That laboratory was finished, I think, in 1924. It was a going operation at the time I came in 1925. Sorensen had come to Caltech back in 1910. He was cagey enough to see with Millikan's arrival that if he hitched electrical engineering to physics, rather than leaving it tied to civil and mechanical engineering, he'd have a better chance of building graduate programs, which he foresaw as a necessity for the future of the engineering profession. And indeed that's the way it worked out.

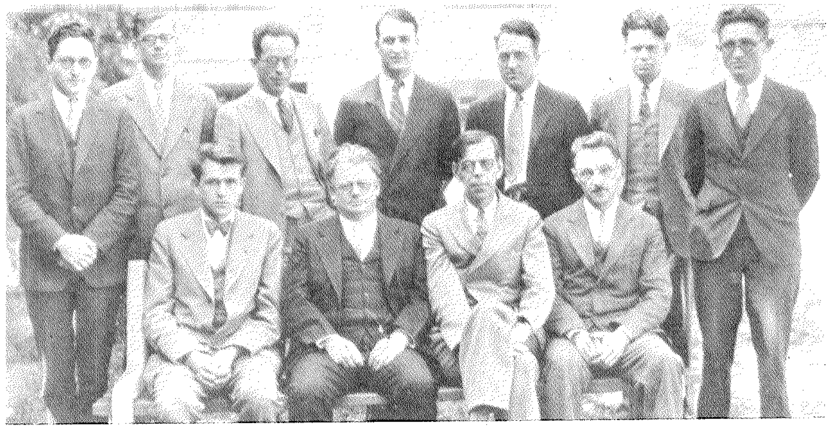
AS: Did Edison provide the money for the lab?

FL: They built the laboratory with their money, with the arrangement that they would have half-time use of it. For a while they used it about half of the time, and then their use gradually tapered off as the power industry developed more and more of the high-voltage know-how. So they had less and less need for it.

AS: Besides this high-voltage transmission, was Sorensen involved in any other major projects?

FL: He helped quite a bit with the Department of Water and Power in Los Angeles and their line to Boulder, and he consulted with the Metropolitan Water District. He gave them a sort of overview of the electrical equipment that went into their pumping stations.





At the left, Richard Tolman and Robert Millikan dressed for the 1935 commencement. Above, the faculty in electrical engineering in 1932. In the front row, left to right, are Frederick Lindvall, Royal Sorensen, Stuart Mackeown, and Francis Maxstadt.

AS: Were there other people in electrical engineering who were doing different kinds of work?

FL: Well, there was Professor Maxstadt. He taught a course in electrical traction; and later on after I came back here to teach, I took over that course until student interest dwindled to zero. Maxstadt was good at electrical machinery.

AS: Did you enjoy teaching?

FL: Yes. Naturally, I didn't know really enough about the psychology of students always to handle the class properly, but most of the time I did all right. I remember one day when the class was sort of unruly, and one of the fellows who was on the football team came to my rescue by saying, "Pipe down, you guys; I want to hear what he has to say."

AS: What about the larger scene in Pasadena. Did you participate in anything in Pasadena at that time?

FL: Not very much, except I was a devotee of the Pasadena Playhouse. I used to go there for practically every play they had. There were even a few Caltech faculty who used to participate in small parts down there. One year, they put on a play written by E. T. Bell, the mathematician, which was based on science fiction. I think Professor Zwicky worked on some of the sound and lighting effects to enhance the science fiction aspect.

Well, I finally got through with all my degree requirements and got the PhD in 1928. At that time we held commencement on the east side of Gates Laboratory. Richard Tolman was dean of graduate studies, and he loved to announce each PhD candidate: "And he is the author of a thesis entitled . . ." and he would reel off the whole technical phrase. If it was in German, he loved it even more.

AS: Did people write their theses in German?

FL: A couple of them did. We had two or three European students for whom that was an easier language than English.

AS: So Caltech had the kind of reputation by then that brought students from that far away?

FL: Yes. It got that reputation fairly early Millikan induced Paul Epstein to come from Germany. And we had a succession of important visitors from Europe, people like Bohr, Einstein, and Lorentz. Lorentz was a wonderful man, who gave beautiful lectures. He would finish the lecture, and turn to us with a little smile and say, "That is the result, if I have made no mistake." He was here a couple of times I think. People of that sort would go back and talk about this new institution. And of course, Noyes in chemistry was well known in this country and in Europe. Sommerfeld was here, and I heard him in seminars.

In those days the principal seminar, which we all attended, was the so-called physics seminar. Dr. Millikan used to go around to the various labs, and he'd see one of the graduate students and say, "Here's a new paper in your field. Report on it at the seminar." Of course, the faculty would take turns, and the visitors would participate. Sir James Jeans came, and he and Millikan had lots of arguments about cosmic rays. I remember one seminar where Millikan was theorizing about the origin of cosmic rays after listening to some of Jeans's cosmology. Zwicky was in the row in front of me, and he started rocking back and forth in his seat and muttering, "Jesus Christ, he's crazy."

Well, finally I graduated, and Professor Sorensen thought it would be highly desirable for me to have practical engineering experience, whether I wanted a teaching career eventually or not. Based on his own experience, he thought it was good to have some practical engineering in the background. So I went to the General Electric Company, into their general engineering department, which was a small department that handled oddball problems plus a certain amount of original investigation. I spent about three months in their test course and more time in their advanced course in engineering, some of which was fairly easy for me after having been here. But they also gave me some good tough engineering problems, more comprehensive than anything that had been offered here. Then I had various assignments working with different engineers in the company on machine design, and over in the research laboratory where I learned a good deal from Dr. Whitney, who was then the research director for G.E.

In the summer of 1930, Dr. Sorensen asked me if I would be interested in coming back to California, and I said I would even though it meant a cut in pay. In those days $500 was a lot of money. I won't say I jumped at the chance to come back, but I had just been married when I went to G.E., and my wife, Janet, and I didn't love the winters in Schenectady. Also our parents lived in California, so we were glad of the opportunity to return.

AS: What did you do when you came back to Caltech?

FL: I started teaching some of the established undergraduate courses. After a year or so, I was encouraged to start a graduate course that was called Engineering Problems. I got the idea while I was at G.E. in the advanced course in engineering. I was impressed with the educational value of comprehensive problems — taking a week to work a problem — and that was the general concept of the course that I started. Also the problems were not limited to any one discipline — not all electrical or mechanical engineering — but a mishmash that forced the students to dust off some of the things they thought they had left behind. It was well received by the students, though they hated the hard work that was involved. Many of the problems would have been a lot easier for them if they'd had the little calculators that are available today, because there was a certain amount of drudgery calculation involved in order to arrive at a numerical and definite answer.

AS: You had to go through the drudgery yourself, as well?

FL: Yes. And sometimes a student would approach a problem in a quite different way from what had been done before, and that was always refreshing. Si Ramo came in with a most novel solution to a problem in vibration of a generator system — a light plant in an imaginary small town. He wrote it up in the manner of a story from a *Saturday Evening Post* series called "Alexander Botts, the Tractor Salesman." He had little pictures he had cut out of magazines to illustrate this thing. It was a very, very ingenious thing.

AS: Was the solution as ingenious as the presentation?

FL: Well, the solution was absolutely correct, because, after all, the physical facts couldn't be denied.

AS: What kind of students did you get at this time at Caltech?

FL: Well, in those days — the middle thirties — we had some awfully fine students who were naturally afraid of the Depression job situation, and if they could possibly wangle a chance to go on and do graduate work, they wanted to do it that way. For instance, in one of the most outstanding classes — those who got their doctorates in 1936 in physics or electrical engineering — there's William Fowler, Dean Wooldridge, Simon Ramo, Bill Pickering, and John Pierce, just to name a few. They all stimulated each other. It was a real pleasure to work with students like that.

AS: How were they recruited to Caltech? Were they given assistantships too, as you had been?

FL: Some of them were; otherwise they couldn't have come. Of course, everything was very cheap in those days. My annual salary was $2500, and you could buy lots of food for that in the thirties, so we had no complaints. Granted, the college did have to cut corners here and there and hold back on hiring people and so on. At one faculty meeting Dr. Millikan suggested that it would be very nice if the faculty were to pass a resolution recommending to the trustees that their salaries be reduced by 10 percent. Dr. Epstein, with his very logical mind, said, "We do not vote to increase our salaries; why should we vote to decrease them?" Nevertheless, we did have a cut.

AS: Was there a shortage of students at that time or an increase?

FL: There was an increase in graduate students, and the number of undergraduates stayed about the same. Of course, their tuition costs were much lower then, and it was more of a commuting college than it is now. Students could get here with their cars or motorbikes, or if they came from Los Angeles, there was the Pacific Electric Railway, which ran up Lake Avenue.

AS: What were the job prospects for the students when they got out?

FL: Not very good. Toward the end of the thirties things began to improve, but many of them had to work for a time at much lower skilled jobs than they were trained for. There were a lot of things of a public works nature that made it easier for those who were in civil engineering to find jobs. The Metropolitan Water District built the aqueduct at that time, for instance. There was a lot of government aid going into all sorts of projects, but I don't remember any government money coming here for educational programs. Later on, when we got into the war, there were war training programs.

AS: What kind of work were you concentrating on in this period?

FL: I was exploring atmospheric glow discharges. I had the idea, from something I had read, that somebody had discovered that a glow discharge had microphonic properties, and I thought if it's microphonic, maybe it will work as an anemometer for wind tunnel purposes. I worked away at that for two or three years. Then I had a couple of graduate students who worked on different modifications of it; so I kept that going. I also got involved in some railroad equipment business through some consulting connections — railroad refrigerator and passenger car improvements. The passenger car business was ahead of its time, and we got three cars into regular passenger service just about the time Pearl Harbor was hit. So there was no more passenger car business until after the war was over.

AS: Were you consulting all during this period?

FL: Off and on. Of course, when you're just getting started as a faculty man, nobody's beating on the door for your services. It takes a while and help from older people who are willing to say, "Well, we've got a new man on the faculty who could do this for you very nicely," and that kind of thing. That's how you get started.

AS: Was Caltech getting any money from industry in this period?

FL: Not to any appreciable extent. Indirectly, yes. For example, the Metropolitan Water District built a hydrodynamics laboratory at Caltech to test models of pumps that the district needed for the aqueduct. The net result was that there were much better pump designs than the manufacturers had first proposed. They were more efficient, and consequently the pumping costs to the aqueduct system were substantially reduced. Later on the laboratory did some pump work for the Grand Coulee Dam project too, so the various manufacturers of pumps were in and out of that laboratory all the time. They supported it indirectly.

Things went along that way. I had graduate students whose theses I supervised. One of my stars was Rube Mettler, who's head of TRW now. My first graduate student was named Gibson Pleasants, now deceased. We worked together trying to understand cathode ray oscillographs to be used for high-voltage and lightning studies. At that time there were no sealed-off cathode ray systems. We built various forms of cathode ray things, and I know now they were very crude, but we were learning the principles. A couple of other students also worked on various problems of cathode ray oscillography, and eventually sealed tubes were beginning to appear on the market, so that ceased to be a fruitful field of study. We were not smart enough to see in our experiments with magnetic focusing the genesis of the electron microscope. □

Tech-nically, It's Music

Caltech now has two courses in which music can overlap comfortably and creatively with science and technology

Although a particular affinity of scientists for music may not be provable, the "fact" of a large incidence of musicians among scientists has been remarked so often that to some it seems almost a cliché.

The overlap, real or imagined, of music with physics and mathematics does have a long history. Among others, Pythagoras was intrigued by the numerical relationships in harmonies; Galileo (and the French mathematician Mersenne) demonstrated the relation between the number of vibrations per second and the musical pitch of a note; Kepler based the laws of planetary motion on musical notation; and the 18th-century mathematicians Leonhard Euler and Daniel Bernoulli used differential equations to determine a formula for the vibrating string. There also seem to be a great number of contemporary scientists who leave their labs behind for a bit of Bach on the piano or a string quartet with some colleagues.

By inference, one could conclude that many Caltech students might be musicians. But, although numerous extracurricular opportunities exist for student musicians to sing and to play in chamber orchestras and smaller groups, Caltech is not primarily in the business of music education. Until recently, with little incentive in the form of credit courses for musically talented students to continue their involvement, music instruction and performance has stopped for many Caltech students because other studies of higher priority demanded so much of their attention and time. That situation seems to be changing.

"Music for Piano Ensemble: History, Analysis, Performance" — Mu 18 — has been offered for the past three years. It is the only credit course for performance in the curriculum, but it's in no way just a course for "playing" around. In keeping with the Caltech spirit of mental rigor, it is an intellectually demanding introduction to a very specific area of musical literature. A large part of the orchestra repertoire does not include the piano, so pianists seldom get the training and discipline that come from ensemble playing. (Piano concerto is a specialized form of solo.)

Performing in at least one public concert is necessary for course credit, and students are required to spend a minimum of ten hours per week in preparation for three hours of class. Actually, the three hours of class usually stretch out to eight or nine when a performance is coming up, says Elma Schonbach, who originated the course and teaches it, using the two grand pianos in her own home near campus. The course is open to students of varying skill levels (last year's students had from 3 to 15 years piano experience) and all classes from sophomore through graduate. Students have invariably praised the course as "one of the few outlets for creative expression on campus" and "especially rewarding for those of us who have spent a fair amount of time during our lives in this endeavor."

Schonbach, who holds a degree from the Cincinnati Conservatory of Music and has been teaching at Caltech since 1976, became interested in piano ensemble as a teaching discipline about 20 years ago. It's also known as piano duet, but Schonbach generally avoids this designation because of the image it evokes of reluctant children plinking out elementary pieces for admiring relatives.

What Schonbach teaches in the ensemble course is a historical survey, through interpretation, of the literature written specifically for two players at one piano. (Two-piano music, much different in character, is also included in the course to a lesser extent.) Four-hand piano is considered chamber music and possesses the intimate style and mathematical complexity of that form. It demands a great amount of discipline and sensitivity. Playing ensemble piano is less subjective than solo playing; it does not allow the musicians arbitrarily to accommodate their own weaknesses (or emphasize their own strengths). The pianist is forced to become more aware of the composer's intentions and to try to work them out in balance with

Students in Mu 18 — Music for Piano Ensemble: History, Analysis, Performance — work together at two Steinways under the direction of their instructor, Elma Schonbach.

another musician as close as his, or her, own elbow.

Schonbach focuses the course on interpreting the stylistic trends of various periods. The 19th century was the most prolific period for four-hand piano literature; Schumann, Brahms, and, especially, Schubert created a great number of compositions for ensemble piano. Today this genre is enjoying a resurgence of recognition among composers as well as among concert and recording artists. Some original piano literature for eight hands also exists, and the Caltech class studies this form of ensemble piano also. Fortunately the different parts are often composed for varying levels of piano skill.

An eight-hands piece (one of Dvorak's Slavonic dances) was included in a short recital at the faculty dinner last May, at which students Carolyn Venger, Bruce Baskir, Don Berry, Ernest Cohen, Kurt Bachmann, and Vincent Powers performed. (Concerts were also given at the Westridge School and for the Pasadena Symphony Associates.) Combining the Athenaeum's dining rooms into an L-shaped space for large dinners does not provide ideal acoustics for music of any sort, and having to balance the sound produced by two pianists playing on a spinet with that of two others at a concert grand compounded the problem. But listening to each other and balancing their tone was something the students had learned in the course, and the faculty dinner entertainment was a hit.

Coping with surroundings not originally intended for perfect sound may be a fact of life for any Caltech music course. The room in the subbasement of Thomas where EE/Mu 107 meets bears remnants (pipes, faucets, sinks) of the kind of laboratory it used to be, but the odd swatches of shag rug on the floor and the acoustic mats hanging from the ceiling, as well as an assortment of speakers, amplifiers, and turntables, indicate that something new is going on here.

And indeed it is. "Projects in Music and Science," cross-listed in Humanities and Social Sciences and in Engineering and Applied Science (with credit in either division) is a course unique in the country, as far as anyone knows. It began last year with help from the President's Venture Fund and with the advice and cooperation of John R. Pierce, professor of engineering, now emeritus, who has long been interested in the strong link between music and technology. Pierce thinks Caltech is the ideal place to tackle such things as the meaning of musical sound.

Musical sound from a sophisticated scientific point of view (or rather, point of hearing) is the primary concern of the course and of its teacher, James Boyk, who came to Caltech as artist in residence in 1974. He has particularly fortunate qualifications to teach such a course — in addition to being a concert pianist with a Harvard degree in mathematics, he runs his own stereo consulting firm — Sound Decision — and record company — Performance Recordings. Boyk sees the course as an integration of the analytical and perceptual, the intellectual and emotional. It's not a matter of bringing science into music, he says; it's already there. Unfortunately, most engineers involved in audio design do not really know how to hear the sound of live music, he maintains, and most musicians ignore much of the content of performance training because it is scientific in language.

The first third of the course is an "acute listening experience"; students listen to and analyze live and reproduced sound. Last year they ranged far afield from their basement studio, visiting a well-known stereo manufacturer (the students were not impressed with the sound quality), a record-cutting laboratory known for the fidelity of its products, and a piano showroom, to compare the sounds of several "live" pianos. Then individual projects took up the rest of the year, after Boyk and his students had "hustled" $12,000 worth of equipment in gifts, loans, and rentals.

Seven official students last year (other students and faculty members often wan-

Hanging acoustic mats and "hustled" sound equipment help transform a former lab into a makeshift studio where Jim Boyk conducts EE/ Mu 107 — Projects in Music and Science.

dered in) chose projects expressing their interests and experience. The class included both undergraduate and graduate students, whose music backgrounds ranged from zero to substantial. Some projects focused more on the musical aspects of the course than on the engineering. One of these, the project of senior Eric Saund, investigated the neurological aspects of music perception — how the brain's sound-processing neural networks work, and why music is perceived as a "good" sound. His study included experiments on perception of critical band width (in which the ear cannot differentiate separate tones).

In another project, which dealt with music as a language of the emotions, Mike Kong tested a theory formulated by musicologist Deryck Cooke that associates particular emotions (joy, anguish, and so on) with each degree of the scale (major third, minor sixth, for example). He played on the piano some of 18 musical phrases for several groups of subjects, asking them to write down what they perceived to be the emotional content of a particular note and of the musical quotation it was a part of. Although the responses tended to support some of Cooke's ideas, the scope of the experiment was too limited for the results to be considered conclusive.

Other projects dealt more with audio hardware — the electrical engineering side. William Snyder and Bruce McArthur attempted to find out what matters in microphone placement. Although the best stereo sound is recorded with coincident mikes (at the same point in space but aimed in different directions) rather than with mikes at opposite sides of a stage or studio, they wondered how much difference small distances between the microphones would make in the sound. They build an apparatus to move two microphones from one and a half feet apart through coincidence and away again and used it to tape two fellow classmates playing Bach's double violin concerto. (The class correctly guessed which tape was made with the moving mikes and which with coincident, but decided that it was the motion that was disturbing rather than the placement.) For another project Snyder also designed and tested a turntable mounted on a three-point spring suspension to achieve maximum isolation from room noise.

Fan-Chia Tao worked with quadraphonic sound to try to reproduce the true "imaging" (the placement of apparent sound sources) of a piano being played in a concert hall, so that someone listening to the recording would hear a piano in front of him with the sound of the room around him. (Most quadraphonic systems give the listener the feeling of sitting on top of the piano, or in the middle of an orchestra, with music coming at him from all four speakers.)

Since specifications for audio equipment have little bearing on the actual sound, junior Bill Gross, whose project involved pulse testing of speakers, tried to correlate good sound quality with direct measurements. Gross manufactures and sells his own stereo speakers (Gross National Products — see *Caltech News*, January 1980).

Graduate student Pierre Schnoeller's study of room acoustics provided a valuable service for the class, since the room he studied (measuring the frequency range with a spectrum analyzer) happened to be the Thomas basement classroom. The end result of his experiments — hanging acoustical foam absorbers from the ceiling in particular patterns — made a big difference in the room's acoustics for a group that, by the end of the year, was extremely knowledgeable about and sensitive to sound.

Both courses are being given again this year. Caltech may never become a Juilliard, but, as Boyk and Schonbach agree, it is a place where many students possess an unusual blend of intellectual and musical talent. Boyk claims Caltech is the only place he has taught music where he can mention things like harmonic series and be sure people know what he's talking about. Certainly it is a place where music and science and technology can overlap comfortably and creatively. □

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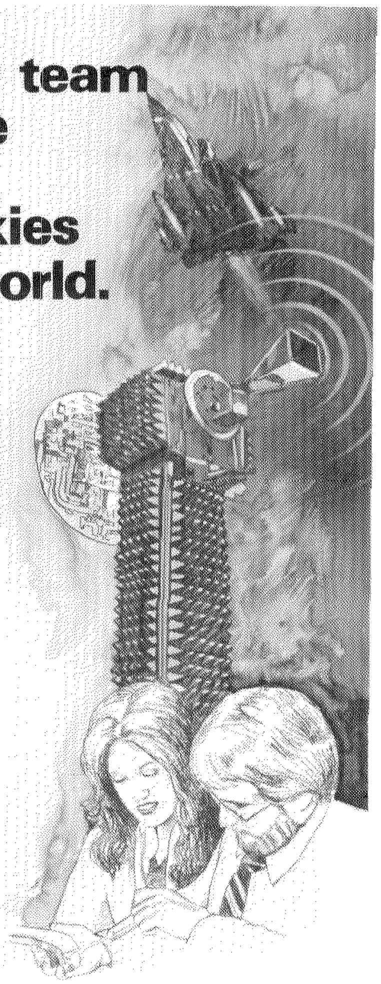

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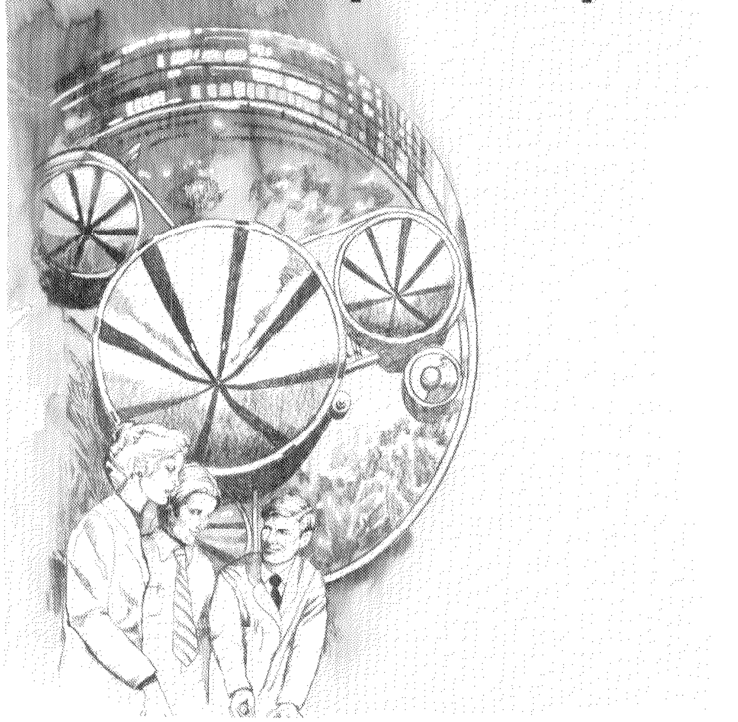

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In the combustion phase, tests were performed at over one hundred operating conditions of varied spark timing, spark plug location, engine speed and intake valve geometry. Detailed thermodynamic analyses were applied to the recorded cylinder pressures to calculate flame speeds throughout combustion. High-speed films were analyzed frame by frame to validate flame speeds and to characterize how gas motions influence the initial flame.

The researchers used these measured flame speeds, turbulence intensities, and the conditions under which they occurred to formulate a burning law for engine flames. They divided the combustion event into four stages. The initiation stage begins with ignition and ends as the flame grows to consume one percent of the fuel mass. In the second stage, the flame accelerates and thickens in response to the turbulent field. The third stage exhibits peak flame speed. In the final stage, the thick flame interacts increasingly with the chamber walls and decelerates.

OVER THE RANGE of turbulent intensities encountered in engines, the researchers were able to describe the turbulent burning velocity, S_T, during the critical third stage of combustion with the expression:

$$S_T = 2.0\, S_L + 1.2\, u'\, P_R^{0.82}\, \beta$$

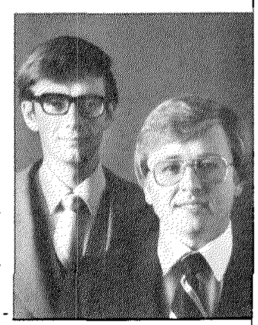
S_L, the laminar flame speed—a known function of pressure, temperature and mixture composition—is the flame speed that would exist without turbulence. The variable u' is the turbulence intensity. P_R represents a pressure ratio accounting for combustion-induced compression of the unburned mixture. The dimensionless factor β accounts for the effect of spark timing on geometric distortion of the flame which occurs during the first combustion stage and persists into the later stages.

The researchers also observed that the burning velocity in the second stage increases in proportion to flame radius, and that in predicting the energy release rate from the burning velocity equation, it is necessary to account for the finite flame-front thickness.

"The form of our burning equation," says Dr. Matekunas, "shows a satisfying resemblance to expressions for non-engine flames. This helps link complex engine combustion phenomena to the existing body of knowledge on turbulent flames."

"We see this extension," adds Dr. Groff, "as a significant step toward optimizing fuel economy in automotive engines."

THE MEN BEHIND THE WORK



Drs. Matekunas and Groff are senior engineers in the Engine Research Department at the General Motors Research Laboratories.

Both researchers hold undergraduate and graduate degrees in the field of mechanical engineering.

Dr. Matekunas (right) received his M.S. and Ph. D. from Purdue University, where he completed graduate work in advanced optics applications.

Dr. Groff (left) received an M.S. from California Institute of Technology and a Ph. D. from The Pennsylvania State University. His doctoral thesis explored the combustion of liquid metals.

General Motors welcomed Dr. Matekunas to its staff in 1973, and Dr. Groff in 1977.

Research in Progress

Rings around the Planets

Planetary rings are of interest because they probably represent an earlier stage of the evolutionary history of planets and their satellites. But because of a planet's tidal gravity, the particles that make up the rings haven't been able to accumulate into a satellite.

Rings around Saturn have been observed for centuries, but only in the last three years has it been discovered that two other planets — Jupiter and Uranus — share similar features. Voyager 1 detected Jupiter's rings in 1979, and rings around Uranus were found in 1977 quite by accident. Scientists from Cornell in the Kuiper Flying Observatory, studying Uranus when a particularly bright star passed behind it (a rare event), noted that the starlight also dimmed up to 90 percent several times before and after it was occulted by the planet.

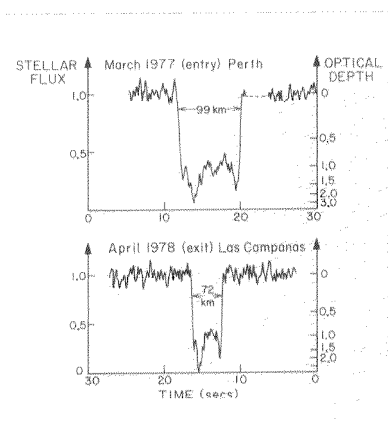
With this obvious evidence of rings, Peter Goldreich, professor of planetary science and astronomy, and others at Caltech interested in planetary rings, were too impatient to wait 20 years for another bright star (Uranus is 100 times brighter than a typical star) to be in the right place.

Fortunately, a breakthrough in observational technique shortened the wait. Former graduate student Jay Elias realized that, because the planet's atmosphere contains a large amount of methane, which absorbs light at longer wavelengths, Uranus would be extremely dark when observed at wavelengths of two microns; conveniently, many stars are red and appear brighter at long wavelengths. This realization has made it possible to gather occultation data from infrared observation of much fainter stars, which exist in relative abundance and find themselves behind Uranus a bit more often.

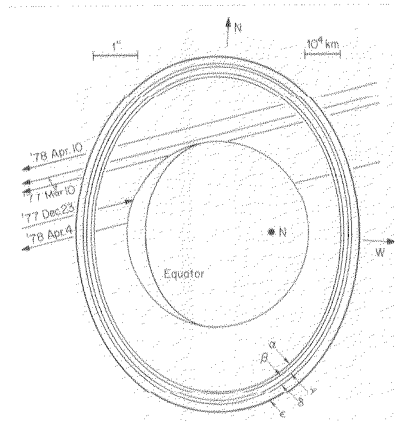
Once or twice a year, in fact. Caltech Scientist Keith Matthews has flown to Chile to use the 100-inch telescope at Las Campanas on these occasions, bringing back data that are yielding more and more detailed information about the rings. Some things are now known about the rings of Uranus, but there is plenty left for Goldreich, the theorist, to theorize about.

Nine rings have been observed so far. While Saturn's rings are bright, broad, and separated by narrow gaps, the rings of Uranus are dark, very narrow, and separated by wide spaces. The narrowest ring is only 2 or 3 kilometers wide, about 50,000 times greater in diameter than in width, and the widest varies from 20 to 100 kilometers. The widest ring, known as the epsilon ring, is also the outermost, and has the most eccentric orbit of the seven rings known to have elliptical orbits. The long axes of these ellipses precess 1 to 2 degrees per day, that is, the point of closest approach to the planet also orbits around it.

From the precession rate (as a function of distance) the gravitational flattening of Uranus can be derived. The mass of the rings can be calculated from the fact that they precess together, as a body; the



The epsilon ring is "seen" here before a star passes behind Uranus (top) and after (but different stars, different places, and different times) as the starlight is dimmed by the ring particles. Both figures show a bicuspid shape, but the varying width of the epsilon ring is evident. It is as narrow as 20 kilometers at another point.



Five of the nine narrow rings of Uranus are shown here (there are three more inside this set of rings and one in between), although the lines representing them are about 50 times too wide for correct scale. The paths of the occulted stars are indicated as well as the dates they were observed; initial discovery of the rings was on March 10, 1977.

calculated mass indicates that the rings are between 10 centimeters and 1 meter thick, probably only a single layer of particles, according to Goldreich. What the particles are made of is still unknown; they are as black as coal, unlike Saturn's ring particles, which are very bright and probably made of water ice.

What holds these narrow rings together? Goldreich thinks that there is a frictional interaction from pairs of satellites — one inside and one outside each ring. Because of the "perverse nature" of particles in orbit, the diffuse material of the ring gains angular momentum from the satellite closer to the planet; that is, it is pushed outward; the satellite on the outer boundary is balancing this force with an opposite push exerted in an inward direction.

Confirmation of the existence of such satellites will likely have to wait for a closer look. Voyager 1 is already providing clues from close-up views of Saturn's rings. If Voyager 1 performs all of its Saturn tasks up to par, Voyager 2 will be deflected to an orbit that is less optimal for Saturn but that will enable it to fly by Uranus in 1986. This should provide answers to some of the remaining questions about Uranus and its rings and either prove Goldreich's theories correct or send him searching for new ones. □

News of Neutrinos

Neutrinos and the question of whether they do or do not have mass have concerned physicists for years, the assumption being that they do not. No evidence of mass has been detected (until recently), but scientists have kept trying. The question suddenly erupted into public debate last spring with the announcement that research at the University of California at Irvine had shown that neutrinos oscillate; such oscillations are impossible if they are massless. Other scientists, including a group from Caltech, immediately challenged these results.

The Caltech researchers (Professor of Physics Felix Boehm, Research Associate Petr Vogel, Research Fellows Alan Hahn and Jean-Luc Vuilleumier, Senior Design Engineer Herbert Henrikson, and Heemin Kwon, whose PhD will be awarded in June of 1981), working with colleagues from the Technical University of Munich and the Institute for Nuclear Science in Grenoble, France, have also been trying to track the elusive neutrino through oscillations. Like the Irvine group, they have attempted to detect whether the electron neutrino oscillates to either of the other two forms — the muon neutrino or the tau neutrino.

Their detector system, consisting of alternate layers of two kinds of particle detectors — cells of "scintillation liquid" and a chamber full of an isotope of helium — took two years to build at Caltech and another year to test with the 57-megawatt reactor in Grenoble. The Grenoble experiment uses protons as targets to detect neutrinos (antineutrinos, actually), tracking the neutron and positron as reaction products of an antineutrino and a proton.

The Irvine experiment dealt with two reactions — a charged current reaction (antineutrino plus deuteron to two neutrons and a positron), which is sensitive to oscillations, and a neutral current reaction (antineutrino plus deuteron to neutron, proton, and antineutrino), which should not be affected by oscillations. Reportedly, the charged current reaction occurred only about 50 percent as often as the expected rate derived from the neutral current reaction, allowing the conclusion that the undetected neutrinos had oscillated into other forms.

The Grenoble experiment uses only one reaction, and in order to interpret it, the number of neutrinos that are really supposed to be emerging from the reactor must be known. If there are fewer than are expected, oscillations can be assumed. However, this expectation can only be predicted, and dependence on such predictions is a definite drawback of the Caltech effort. On the other hand, since the chosen reaction has an event rate more than 20 times greater than that of the deuteron reactions, it is easier to distinguish it from background events produced by cosmic rays. Also the information obtained is more complete because it is possible to measure not only the reaction event rate but also the positron energy. Changes in this energy spectrum would also indicate oscillations.

One set of calculations predicting the number of neutrinos emerging from the reactor was published last year by Brian Davis (PhD '80) along with Vogel and collaborators from the Hanford Engineering Development Laboratory. The Caltech-German-French experiments that began in Grenoble a year ago and have continued through the spring and summer have generated data indicating that the predicted number, according to Davis-Vogel, of neutrinos are emerging from the reactor core and that they also have the predicted energies. If the Davis-Vogel calculations are correct, the presence of oscillations has not been proved.

Another recently published theory, however, predicts a 30 percent higher spectrum of expected neutrinos; the difference probably is due to different nuclear model assumptions used to calculate the unknown short-lived beta decay. If this higher spectrum is correct, it might alter interpretation of experimental results.

But independent evidence obtained this past summer tends to support Davis-Vogel. An experiment at Grenoble measuring electrons (the same number of antineutrinos and electrons are generated from fission of uranium) produced a spectrum within 5 percent of the Davis-Vogel calculation. Similar results from Oak Ridge also seem to confirm this theory.

The final word on neutrinos is not yet in. The Caltech group's ultimate tests, independent of a calculated spectrum, are yet to be performed. After further testing of their detector at Grenoble, Boehm's group plans to move it to the 2,700-megawatt reactor at Gösgen, Switzerland. There they will measure the neutrinos emerging from the reactor core at several distances; if changes over distance are observed, they would have to be due to oscillations. The question of neutrinos' mass, and with it the key to a number of cosmological puzzles, may soon be answered definitively. □

The Decline of Collective Responsibility in American Politics . . . *continued from page 16*

to the district, national conditions and the performance of the party leader have less of an impact on House races.

The effects of the insulation of congressional incumbents have begun to show up in a systematic way in the governmental arena. Table 3 presents data on presidential success and presidential support in Congress for the first two years of the administrations of our last five elected presidents. Evidently, Carter was less successful than earlier presidents who enjoyed a Congress controlled by their own party; he was only as successful as Nixon who faced an opposition Congress. Moreover, in the House, Carter has done relatively poorly in gaining the support of his own party colleagues. It is noteworthy that Kennedy earned a significantly higher level of support from a congressional party that was nearly half Southern, whereas Carter enjoyed a majority in which the regional split was much less severe.

Of course, it is possible to discount the preceding argument as an unjustified generalization of a unique situation — a particularly inept president, a Congress full of prima donnas still flexing their post-Watergate muscles, etc. But I think not. The withering away of the party organizations and the weakening of party in the electorate have begun to show up as disarray in the party in government. As the electoral fates of congressmen and the president have diverged, their incentives to cooperate have diverged as well. Congressmen have little personal incentive to bear risks in their president's behalf since they no longer expect to gain much from his successes or suffer much from his failures. By holding only the president responsible for national conditions, the electorate enables officialdom as a whole to escape responsibility. This situation lies at the root of many of the problems that now plague American public life.

Table 3: Recent Trends in Congressional Support of the Executive

Congress	Year	Presidential Success	Presidential Support Within His Party	
			House	Senate
83rd	'53-54	83%	72%	72%
87th	'61-62	83	73	64
89th	'65-66	87	69	61
91st	'69-70	76	62	63
95th	'77-78	77	61	67

Source: Congressional Quarterly Almanacs

Some Consequences of the Decline of Collective Responsibility

The weakening of the parties has contributed directly to the severity of several of the important problems the nation faces. For some of these the connections are obvious; for others the links are more subtle.

Immobilism: As the electoral interdependence of the party in government declines, its capacity to act also declines. Consider the two critical problems facing the country today — energy and inflation. The failures of policymaking in these areas are easy to identify and explain. The problem lies in the future, while the solutions impose costs in the present. So politicians dismiss the solutions as infeasible and act as though the problem will go away. When it doesn't, popular concern increases. The president, in particular, feels compelled to act — he will be held responsible, both at election time and in the judgment of history. But congressmen expect to bear much less responsibility, and feel less compelled to act. At first, no policy will be adopted; later, as pressure builds, Congress adopts a weak and ineffectual policy for symbolic purposes. Then, as the problem continues to worsen, congressmen join with the press and the public and attack the president for failures of leadership. What makes this charade possible is the realization by members of Congress that national problems arising from inaction will have little political impact on them, and that the president's failures in dealing with those problems will have similarly little impact.

Political inability to take actions which entail short-run costs ordinarily will result in much higher costs in the long run; we cannot continually depend on the technological fix. So the present American immobilism should not be dismissed lightly. The sad thing is that the American people appear to understand the depth of our present problems and appear prepared to sacrifice in furtherance of the long-run good. But they will not have an opportunity to choose between two or more such long-term plans. For although both parties promise tough, equitable policies, in the present state of our politics neither can deliver.

Single Issue Politics: In recent years political analysts and politicians have decried the increased importance of single issue groups in American politics. But such groups are by no means a recent phenomenon. The gun lobby already was a classic example at the time of President Kennedy's assassination. And however impressive the anti-abortionists appear today, remember the Temperance movement, which succeeded in getting its constitutional amendment. American history contains numerous forerunners of today's groups, from anti-Masons to abolitionists to the Klan. Why then do we hear all the contemporary hoopla about single issue groups? Probably because politicians fear them now more than before, and a principal reason for their fears is that the parties are now too weak to protect their members and thus to contain single issue politics.

When a contemporary single issue group threatens to "get" an officeholder, the threat must be taken seriously. The group can go into his district, recruit a primary or general election challenger, or both, and bankroll that candidate. In earlier times single issue groups were under greater pressures to reach accommodations with the parties. After all, the parties nominated candidates, financed candidates, worked for candidates, and perhaps most importantly, party voting protected candidates. Only if a single issue group represented the dominant sentiment in a given area could it count on controlling the party organization itself, and thereby electoral politics in that area.

Not only did the party organization have greater ability to resist single issue pressures at the electoral level, but the party in government had greater ability to control the agenda and thereby contain single issue pressures at the policy-making level. Today we seem condemned to go through an annual agony over federal abortion funding. There is little doubt that politicians on both sides would prefer to reach some reasonable compromise at the committee level and settle the issue. But in today's decentralized Congress there is no way to put the lid on. In contrast, historians tell us that in the late 19th century a large portion of the Republican constituency was far less interested in the tariff and other questions of national economic development than in whether German immigrants should be permitted to teach their native language in their local schools, and whether Catholics and "liturgical Protestants" should be permitted to consume alcohol. Interestingly, however, the national agenda of the period is devoid of such issues. And when they do show up on the state level, the exceptions prove the rule: They produce party splits and striking defeats for the party that allowed them to surface. Of course, control of the agenda is a two-edged sword (a point we return to below), but present-day commentators on single issue groups clearly are concerned with too little control rather than too much.

A strong party that is held accountable for the government of a nation has both the ability and the incentive to contain particularistic pressures. It controls nominations, elections, and the agenda, and it collectively realizes that small minorities are small minorities no matter how intense they are. But as the parties decline, they lose control over nominations and campaigns, they lose the loyalty of the voters, and they lose control of the agenda. Party officeholders cease to be held collectively accountable for party performance, but they become individually exposed to the political pressure of myriad interest groups. The decline of party permits interest groups to wield greater influence, their success encourages the formation of still more interest groups, politics becomes increasingly fragmented and collective responsibility still more elusive.

Popular Alienation from Government: For at least a decade political analysts have pondered the significance of survey data indicative of a steady increase in the alienation of the American public from the political process. Table 4 presents some representative data. As seen, two-thirds of the American public feel that the government is run for the benefit of big interests rather than for the people as a whole, three-quarters believe that government officials waste a lot of tax money, and half flatly agree with the statement that government officials are basically incompetent. The American public is in a nasty mood, a cynical, distrusting, and resentful mood. The question is why.

Table 4: Recent Trends in Political Alienation and Distrust

	Government Run For Few Big Interests	Government Officials Waste "A Lot"	Government Officials Don't Know What They're Doing
1964	29%	46%	27%
1968	39	57	36
1972	45	56	34
1976	66	74	49
1978	68	77	50

Source: National Election Studies made available by the InterUniversity Consortium for Political and Social Research, University of Michigan.

Specific events and personalities clearly have some effect: We see pronounced "Watergate effects" between 1972 and 1976 in the table. But the trends clearly began much earlier. Indeed, the first academic studies analyzing the trends were based on data no later than 1972. Should we be at all surprised by the data? After all, if the same national problems not only persist but worsen while ever greater amounts of revenue are directed at them, why shouldn't the typical citizen conclude that most of the money must be wasted by incompetent officials? If narrowly based interest groups increasingly affect our politics, why shouldn't citizens increasingly conclude that the interests run the government? For 15 years the citizenry has listened to a steady stream of promises but has seen very little in the way of follow-through. An increasing proportion of the electorate does not believe that elections make a difference, a fact which largely explains the much-discussed post-1960 decline in voting turnout.

Continued public disillusionment with the political process poses several real dangers. For one thing, disillusionment begets further disillusionment. Leadership becomes more difficult if citizens do not trust their leaders and will not give them the benefit of a doubt. Policy failure becomes more likely if citizens expect the policy to fail. Waste increases and government competence decreases as citizen disrespect for politics encourages a lesser breed of person to make careers in government. And "government by a few big interests" becomes more than a cliché if citizens increasingly decide that the cliché is true, and cease participating for that reason.

Finally, there is the real danger that continued disappointment with particular government officials ultimately metamorphoses into disillusionment with government per se. Increasing numbers of citizens believe that government is not simply over-extended, but perhaps incapable of any further bettering of the world. Yes, government is over-extended, inefficiency is pervasive, and ineffectiveness is all too common. But government is one of the few instruments of collective action we have: Even those committed to large-scale reduction in government programs will find it necessary to use government to achieve their aims.

CONCLUSION

Recent American political thought has emphasized government *of* the people and *by* the people. Attempts have been made to insure that all preferences receive a hearing, especially through direct expression of those preferences, but if not, at least through faithful representation. Citizen *participation* is the reigning value, and arrangements that foster widespread participation are much in favor.

Of late, however, some political commentators have begun to wonder whether contemporary thought places sufficient emphasis on government *for* the people. In placing so much stress on participation, have we lost sight of *accountability?* Surely we should be as concerned with what government produces as with how many participate. What good is participation if citizens are unable to determine who merits their support?

Participation and responsibility are not logically incompatible, but there is a degree of tension between the two, and the quest for either may be carried to extremes. The attempt to maximize participation may lead to quotas and virtual representation schemes, while the attempt to maximize responsibility may result in a closed shop under boss rule. Moreover, both qualities can weaken the democracy they supposedly underpin. Unfettered participation produces Hyde Amendments and immobilism. Responsible parties can use agenda power to thwart democratic decision — for more than a century the Democratic party used what control it had to suppress the racial issue. Neither participation nor responsibility should be pursued at the expense of all other values, but that is what has happened with participation over the course of the past two decades, and we now reap the consequences in our politics.

The depressing thing is that no rays of light shine through the dark clouds. The trends that underlie the decline of parties continue unabated, and the kinds of structural reforms that might override those trends are too sweeping and/or outlandish to stand any chance of adoption. Through a complex mixture of accident and intention we have constructed for ourselves a system that articulates interests superbly but aggregates them poorly. We hold our politicians individually accountable for the proposals they advocate, but less so for the adoption of those proposals, and not at all for the implementation of those proposals and the evaluation of their results. By exalting political individuality and permitting, indeed encouraging, the decomposition of political parties, we have given ourselves officials who pander and posture rather than lead, officials who give us public relations rather than government. □

EPILOGUE

The results of the recent election might suggest to some that the preceding essay ends on an unduly pessimistic note. After all, over and above Mr. Reagan's handsome victory, the Republicans took control of the Senate and made seemingly impressive gains in the House. Did the American citizenry at long last impose responsibility on the governing party as a whole? Did national frustration lead to redress of the trends I have decried? Probably not. There is less to the recent elections than meets the eye of the beholder.

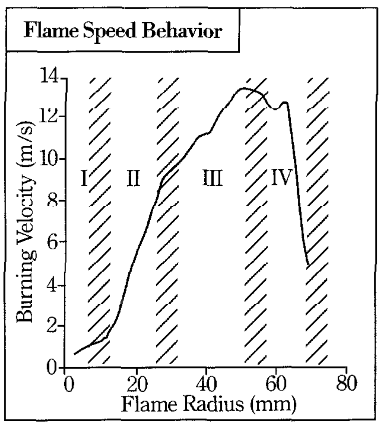
In the first place the loss of 12 Senate seats really provides scant evidence of massive rejection of the President, liberalism or Democrats. In three states (Alabama, Alaska, Florida), Democratic incumbents lost primaries; such internecine battles seldom leave a state party in good shape for the general election. One Senator (Talmadge of Georgia) was heavily touched with scandal. Another (Magnuson of Washington) is generally agreed to have suffered from age and health, not abortion, inflation, or liberalism. And what of the prominent liberals — Bayh, Church, Culver, Durkin, McGovern, Nelson — who were on various "hit lists"? Do not forget that the first five Senators listed received an average of 52 percent in their last election, and that was 1974, a very bad year for Republicans. It is arguable that all of the above, save Nelson, were serving on borrowed time; had 1974 been a more normal year, they probably would not have been around to lose in 1980. Their defeat this time might be due to popular rejection of the Democratic administration or popular choice of a Republican future, but, given, their electoral vulnerability, a small amount of such sentiment could have produced the notable electoral results. In short, while the extent of Democratic losses is unarguable, the extent to which those losses reflect any great degree of Democratic collective responsibility is highly problematic.

And what of the House? Thirty-three seats seem like a lot, but only by very recent standards. The bottom line is that 90 percent of all Democratic incumbents who ran, won (as compared to 98 percent of Republican incumbents), and this in a year when a number of them were under indictment, and when Republican efforts to unseat senior members were the most vigorous in a generation. We can perhaps hope that congressional Democrats will interpret the election returns as evidence that they will be held collectively accountable, but the reality underlying that interpretation is open to question.

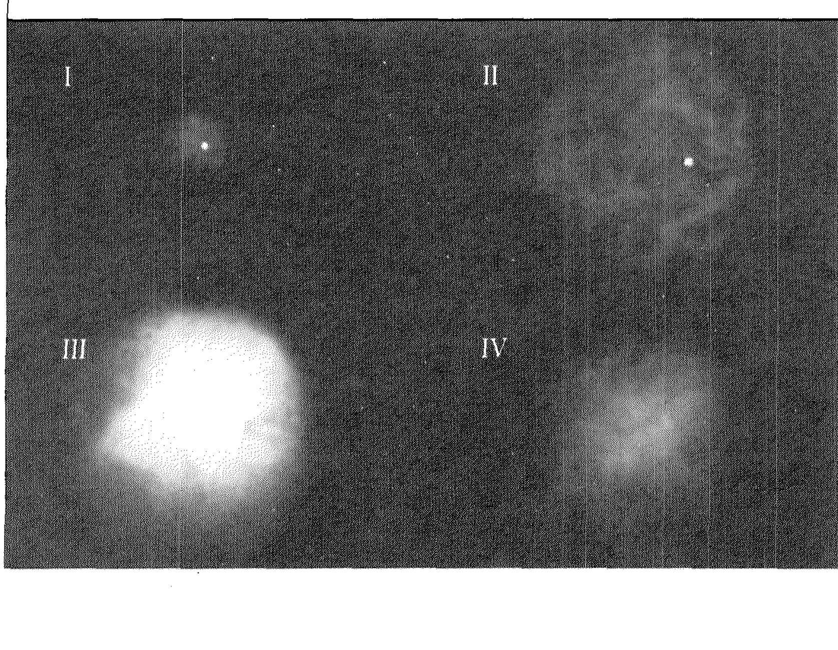
To their credit, Republicans emphasized common party membership in the recent campaign. But such group loyalty is easy when the scent of victory is in the air. One wonders whether Republican congressmen will be so party-minded if a 30 billion dollar tax cut and 20 billion dollar increase in defense spending leads to 20 percent inflation. Such a situation would provide a good test of whether the decline of collective responsibility really has halted. □

The Turbulence Parameter

Energy-efficient operation of the internal combustion engine requires the highly turbulent movement of fuel and air in the chamber. Recent advances at the General Motors Research Laboratories provide a new basis for determining what degree of turbulence will get the most work from each drop of fuel.



Burning velocity plotted as a function of flame radius. Combustion stages are indicated by roman numerals.



High-speed photographs showing flame evolution (lasting six milliseconds) through four stages: initiation (I); flame growth (II); full development (III); termination (IV).

WITHOUT TURBULENCE, the highly agitated motion of cylinder gases, combustion would take place too slowly for the gasoline engine to function. Predicting combustion behavior in order to design engines with greater fuel efficiency depends upon understanding the relationship between vital, turbulent gas motions and burning rate. The challenge is to quantify this relationship—a complex task made more difficult by the requirements of measuring a transient event occurring in a few milliseconds within a small, confined space.

New knowledge of how turbulence affects flame speed has been revealed in fundamental studies conducted at the General Motors Research Laboratories by Drs. Frederic Matekunas and Edward Groff. Their investigative results have been incorporated into a model that successfully predicts the effect of engine design and operating conditions on power and fuel economy.

The researchers separated their experiments into two phases. In the first phase, they measured turbulence in the engine cylinder; in the second phase, they determined flame speeds over a broad range of operating conditions. Testing took place in a specially designed, single-cylinder engine equipped with a transparent piston to permit high-speed filming of the combustion event.

Hot-wire anemometry was applied to measure the turbulent flows while the engine was operated without combustion. Instantaneous velocities were calculated from the anemometer signals and simultaneous measurements of gas temperature and pressure. More than 400,000 pieces of data were processed for each ten-second measurement period.

The significant measure of turbulence is its "intensity," defined as the fluctuating component of velocity. Because conditions in the cylinder are both transient within cycles and variant between cycles, separating the fluctuating and mean components of velocity is inherently difficult. The researchers overcame this problem by using a probe with two orthogonal wires properly aligned with the direction of the mean flow.