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How We Almost Solved the Problem of Why the 1987 Baseball Went Farther (If It Did) Than the 1986 Baseball

by Ronald F. Scott

Pedro Guerrero, then of the Los Angeles Dodgers, trots around the bases after a home run in 1987.

During the summer before last, everyone was talking and writing about the remarkable rate at which home runs were being batted in baseball's major leagues.

I saw a TV news feature one evening about the topic. Apparently, home runs were being hit at a rate 20 percent higher than a standard I didn't catch (last year? average rate?). Players, coaches, and fans were all ascribing the increase to a ball that was different in 1987. The manufacturer (there is only one, Rawlings, which makes balls for both major leagues) denied it, and two sectioned baseballs, that year's and the previous year's models, were produced to show that there was no evident difference.

What the TV broadcaster and I didn't know was that I would get involved, if you can call it that, in the controversy. At about this time, Caltech's public relations office had already been approached by a major West Coast newspaper, whose reporter wanted to know if anyone at Caltech might be interested in testing that year's and yesteryear's baseballs. The inquiry filtered down to me. In my life I have been more devoted to the flight of golf balls than baseballs, but the mechanics are the same (see Rabindra D. Mehta, "Aerodynamics of Sports Balls," *Annual Review of Fluid Mechanics*, 1985: 151-89), so I thought I'd take the question a stage further.

Hall Daily, Caltech's assistant director of public relations, filled me in on the story. This newspaper would 'support' some 'research' on the reason for the longer ball.

"When do they want the results?"

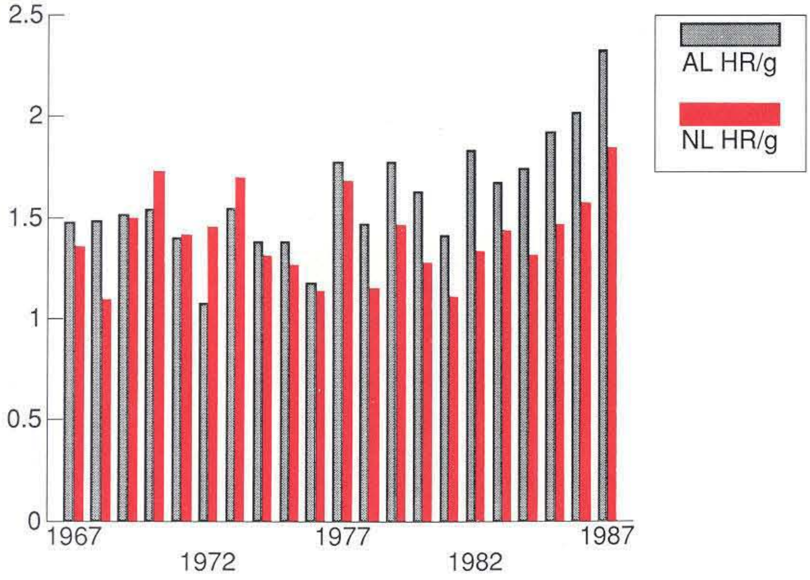
"Two weeks."

It was obviously not possible to do any reasonable amount of testing in such a short period, but I asked how many balls were available for testing.

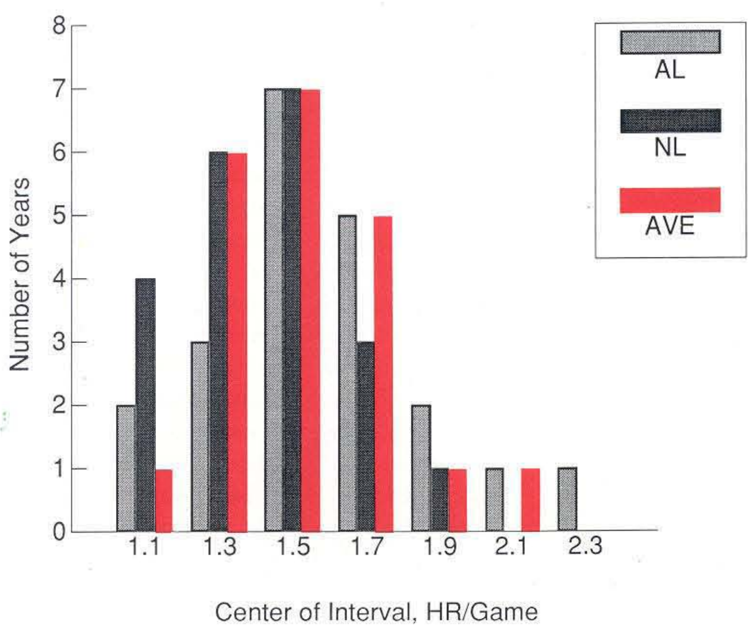
"Two: one 1986, and one 1987, but the reporter's looking for more."

It all seemed impossible at that stage (not enough balls), but I suggested some work the newspaper could do without using any baseballs at all. It was not clear, you see, whether the increased number of home runs was real or perceived; that is, whether 1987 was a particularly fruitful home run year, or 1986 had been one of especially low production. Since baseball is quintessentially a game of statistics (when our sons were younger, I had done several tours of duty at baseball parks, and had also listened to Vin Scully and his colleague, the numerically inclined Ross Porter), I concluded that it ought to be possible to make use of such data as the number of home runs hit per game, per league or both leagues, in the first two months of the season (this was about June 15) for, say, the last 20 seasons. Then we could establish a mean, standard deviation,* etc., and decide if 1987 was actually abnormal. I pointed this out to Daily. He thought it might be possible, but said the difficulty would lie in obtaining data on a

**I should point out that the manipulation of the statistics is not all that simple either. It could be assumed that the number of home runs, say, per year is a random sample, independent of time-classical statistics. Or we could postulate a correlation with some evolving factor, say, the weight of batters (like that of Rams linemen), in which case we should mess with time series analysis and Bayesian statistics.*



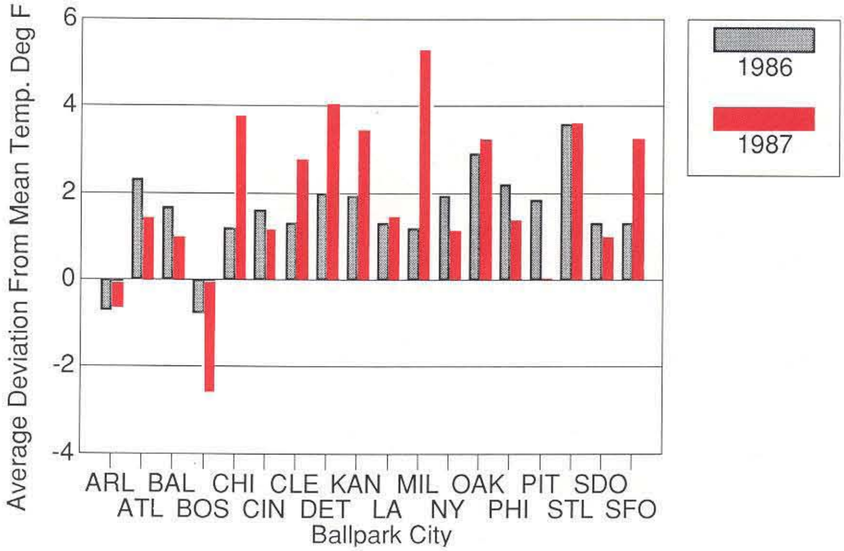
The top graph shows home run averages per game by year for 1967 to 1987; gray is the American League, red the National League. In the graph below the same data are arranged to display the number of years in each league (and the average of the two), in which the home runs per game shown on the horizontal scale were hit. The average is shown in red.



Baseball is quintessentially a game of statistics.

monthly basis. It did. Still, maybe this computation of annual home-run rates would solve the whole problem; 1987 could be within the standard deviation. Warming to the task, I then suggested weather statistics; balls will travel farther on a hot day. I said I'd settle for the average temperature at the major league parks over the two month period for the last 20 years. Daily indicated that park temperatures were not one of the usual statistics, so I compromised on the appropriate city temperatures for the period. That shouldn't be hard to get. A few questions came up—such as Candlestick Park, where we probably should use San Francisco airport temperature, not that of San Francisco itself; or correct for the Astrodome over the Houston city temperature. However, in general, city temperature was probably obtainable. Then the home-run fluctuation could be compared with the mean two-monthly city temperature over the period of interest. It might be an interesting correlation. Or it might not.

Hall arranged for lunch with the newspaper reporter. I talked to a graduate student who was interested in a possible research problem and could use the money, not necessarily in that order. I almost forgot the lunch, but was only 45 minutes late on the day. After introductions, we began serious negotiations. The lack of baseballs ("once is not enough") had been ameliorated. The reporter reported that a dozen 1986 balls had been located, and, of course, the 1987 balls were freely available. I asked where the 86s had come from, and he said an assistant coach had found a dozen in a box on the top of



This graph shows the average departures from the long-term mean monthly temperatures for April, May, and June for the major league baseball cities; the gray bars are 1986 and the red, 1987. The temperatures were measured at airports in or near the cities and the data obtained from the NOAA Climatological Data Annual Summary. (I strongly doubt the Boston figures, since they differ in trend from all other East Coast data.) Evidently 1987 was half a degree Fahrenheit warmer than 1986 for those three months, but I don't think this is significant for the home-run problem.

the lockers in the Dodgers locker room. I was curious enough to inquire how you could tell 1986 balls from 1987 balls. I thought they'd be dated. Not so.

"They have to be National League balls, otherwise we can't tell," he remarked.

He explained. The balls of both leagues have the names of the league and its president stamped on them. Charles Feeney, president of the National League for 13 years, had retired at the end of the 1986 season and was replaced by Bart Giamatti. So the former's name appears on the 1973-86 balls and the latter's on the 87s.

"Could I summarize the provenance of your dozen 1986 baseballs?" I asked. "I understand from you that your PR friend at the Dodgers got them from an assistant coach who found them in the locker room in a box."

"That's right," said the reporter.

"But how does the assistant coach know they are 1986s and not some previous year?"

"He *knows*. But it probably doesn't matter anyway, since it is the 87s that are different. Any demonstration that the 87s go farther than a previous year's ball would satisfy a lot of people."

I wasn't sure it would satisfy me. Hall had told him about my statistical notions. He cleared that issue up right away.

"What the fans want to find out is what's different about the *ball*; that's what everybody is asking. Just take it for granted that it's going farther."

"But if it really isn't going farther, as a *ball*, and it's just due to the warm weather this year, or the fact that there are no new pitchers this year (are there?) and everybody's got books on the old ones and practiced more, or there are more new young hitters this year than usual, or at several parks they've moved the fences in (have they?) since last year, what's the point of testing the baseballs?" I protested.

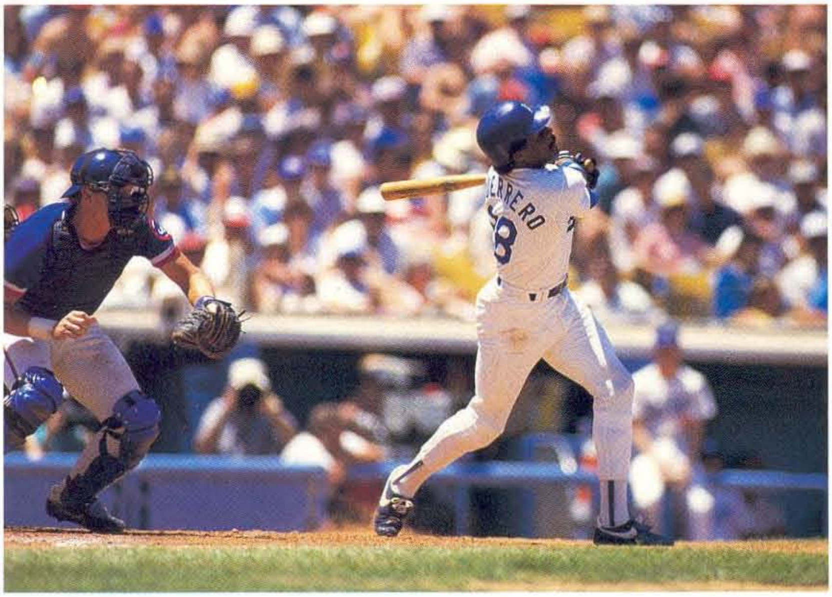
"It would still make a good story," the reporter said.

OK, back to the number of 1986 (?) balls.

"Couldn't you get more?"

"Well, that's a tricky point."

He didn't want people to know what he was working on, so he didn't want to call up all the major league teams and ask if they had any 86s left. Naturally, any communication with the manufacturer was out. Where did all the left-over balls go at the end of a season? I asked. Did Rawlings sell its surplus (we assumed it had one) to the minor leagues, Mexican leagues, Central American leagues? How were the balls dealt with anyway? Apparently they are delivered to each major league team, but umpires, who keep them in a locked room, are actually in charge of the balls and prior to each game they rub down a sufficient number to last through the game. Since a team wouldn't want to run out, they must have plenty left over; so what did they do with them at the end of each season? No one knew. Maybe they were kept for spring training. Did they use any 1987 balls in spring training? Obviously, all these questions could have been answered by a few calls to the Dodgers and the umpires, but we had to keep everything under wraps.



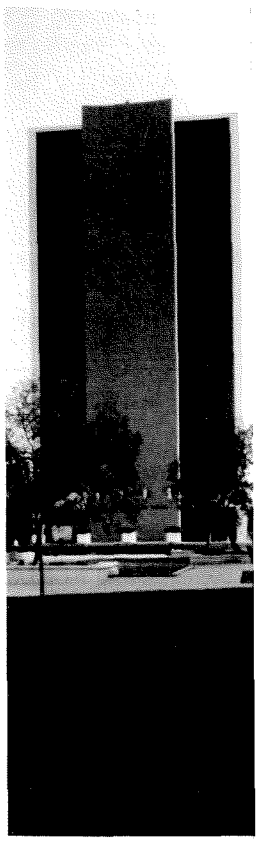
Then the fun began. Research is research. There's equipment, sensors, data acquisition, statistics, theory, and analysis. . .

Guerrero points out the destination of a home-run ball (June 1987).

Next, we discussed money. I needed support for a graduate student through the summer—a few thousand dollars if the newspaper paid him directly—and another couple of thousand to build some test equipment, since the reporter was serious about actually testing balls. It looked as though we'd have to build something to hit them with. He indicated that the total amount of money was acceptable to the newspaper. We agreed I'd put together a proposal after some more thinking and contact him in a couple of days.

Then the fun began. Research is research. There's equipment, sensors, data acquisition, statistics, theory, and analysis, whether you are bashing a baseball, running a rat, shaking a structure, or deifying DNA. I got together with a graduate student to run through a research plan. What could we do in the way of testing a baseball for impact and flight characteristics? Obviously, one or two 86 and 87 balls could be sliced open ("sacrificed" is what the biologists call it) and examined. There is a core (what's it made of?) wound with string (composition?) under some tension, and covered with a hand-stitched leather cover. The overall weight and diameter were regulated, but was some variation allowed in the two properties? If so, how much? Could the *average* weight or diameter of 10,000 1987 baseballs have differed slightly from that of the 86s? Presumably, we could tell if the core and string were made of different materials in each year, but it might take quite sophisticated equipment to tell whether, say, the core or string materials varied in chemical content if man-made, or whether their sources were different if the materials were natural. How could we tell if the string *tension* varied from one year to the next? I didn't see how we could *un*wind the string and measure the tension. We might unwind each ball and measure the length (under a small tension) and weight of the resulting pile of string. But we could only do this for one or two balls, unless another 1000 or so arrived from a miraculous donor. The results would likely be meaningless, for two balls. Needless to say, the dissected balls were of no value for further tests, say, impact tests, so our 1986/1987 stock would be depleted by one or two for each season.

Leaving the ball autopsy aside for the moment, we considered static compression tests. It would be easy to put a ball in a compression test machine and measure the amount of its compression at various force levels. We would have to see if there were any Edgerton pictures of a baseball at impact, showing how much it was deformed, so that we could estimate how much static compression of the ball would be representative of a home-run impact. This might turn up something, but it was a test that would have to be done on a *lot* of balls. However, we would have to assume that a massively crushed ball would not be a valid candidate for later impact tests, and our supply would thereby be diminished by another ball or two. There might be quite subtle reasons why one season's balls would fly farther than another's. A typical baseball in a ball game might be hit a few times, but probably not very many; a slight scuff



The first thought that came to mind was to drop a baseball from a high tower and measure the height of its rebound.

leads to its retirement. (Where does a retired baseball go? The umpire gives it to a ball boy, who normally adds it to the fungo bag for the next practice.) It was possible that each season's balls might have the same compressibility initially, but that one set might have a greater resilience after one or two hits, and thereby go a bit farther. It would be necessary to test each ball a few times.

After a lot of thought about *static* tests, it became clear that, no matter how they might be performed, the tests would be unlikely to resolve questions of the dynamic performance of a baseball. In addition, there was the consideration of the audience for the test results. We ought to perform fairly simple tests that the average fan could relate to, and whose results would be credible.

The first thought that came to mind was to drop a baseball from a high tower (say, Millikan Library), and measure the height of its rebound. That raised a number of questions. A brief calculation indicated that the terminal velocity of a baseball in free fall in air of about sea level density is about 130 feet per second; a second estimate showed that Millikan, at about 110 feet, was not high enough for the ball to reach terminal velocity. Now, at ball/bat impact in a baseball game, the relative velocity of baseball and bat appears to be about 200 feet per second, so we are faced with a problem. During contact, a baseball is certainly severely deformed; the impact process is what we call in mechanics "nonlinear." In simple terms that means that a ball arriving at a stationary bat at 200 feet per second does not leave the bat at twice the velocity of a ball striking the bat at 100 feet per second. If we want to find out, in as straightforward a fashion as possible, what happens when a baseball in a real-life ball game strikes a bat at 200 feet per second, then we have to have a baseball strike something resembling a bat at 200 feet per second.

That was not all with regard to drop tests, though. They are not all that easy to do. What should the ball hit? Presumably, it should be a piece of hardwood (rather than a steel plate, say), but the wood would have to be flat, as we could not guarantee an impact point to the required precision if we actually had a bat there. In fact, how much scatter (wind, etc.) would we get for a succession of balls dropped from the same place? How big would the piece of wood have to be?

The next consideration was the measurement to be made. The obvious thing was the height of bounce, which might be 50 feet plus. How would we record that accurately? We'd need to drop a few store-bought balls just to establish the general bounce height range, since we could not use up our precious supply of test balls. Then we could put up a marked (feet, inches) board at that elevation, and film each test with a movie camera, although where to locate the camera was not all that obvious either. A better method would be to record the velocity of the ball just before and after impact, but some thinking was needed to figure out how to do that, too, with an uncertain impact point each time. Could we drop the ball down a tube (it would have to be perforated, or consist of guides only, to let the air get out of the way) with photoelectric sensors at the bottom? Would it bounce straight up the tube? All in all, it didn't seem too good an idea after all to drop the balls off a building. More thought had to be given to another test.

What it boiled down to, eventually, was another dynamic test, preferably simulating as closely as possible the contact of bat with ball that occurs in the baseball field. In golf, there is a machine designed to perform such a test. It rotates a golf club, in a reasonable simulation of a real golf swing, to make contact with a golf ball on a tee, and drive it into a typical flight. The machine is called an "Iron Byron" because the motions are said to be a mechanical representation of the swing of Byron Nelson, formerly an eminent professional golfer. It is seldom used in golf ball commercials presumably because it could determine *accurately* whose ball went farther. The situation in golf is quite different

from that in baseball. There are many golf ball and club manufacturers, who must make the equipment in conformity with specifications required by the Professional Golfers Association (PGA). Many people, including professionals, play golf, and fairness requires that all use balls and clubs meeting the PGA specifications. Weight and diameter of the ball can be easily controlled, but the impact and flight characteristics demand a dynamic test for completeness.

In baseball, the ball is, of course, moving.

In baseball, there is essentially only one market for professionally used baseballs—the major leagues; and there is only one class of user—professional players. Here there is only one manufacturer. Batters of both teams in a game hit balls from the same batch; whatever ball is produced, everyone uses it. Perhaps it is for this reason that a dynamic baseball testing device does not appear to exist. (Or *is* there one, hidden in the hills of Haiti?) Anyway, we were not permitted to inquire. It appeared, therefore, that we would have to devise our own machine. Since we originally envisioned completing the research in a relatively short period, we decided to design this device first, so that the lengthy period of construction could take place while we undertook other tests.

Complicated though Iron Byron is, at least all the club-head has to do is hit a stationary ball. In baseball, the ball is, of course, moving. Maybe this is why we couldn't find any stroboscopic flash pictures of contact between a bat and a baseball; you don't know where precisely to aim the camera. After a short period of consideration, we decided we could not hit a moving ball either, and came to the conclusion that we would have to knock a ball off a tee, which meant that the bat would have to move a bit faster.

That brings us to the question of the bat. We should incorporate a real bat in the device, to which the bat would have to be clamped. That would be easier to do with a metal bat, although major league bats are required to be wooden. A metal bat, in fact, simplifies a number of problems (consistency, reliability, no break), so we considered using one. A baseball bat swing is, more or less, in a horizontal plane, so our first thoughts were to have a vertical axis and horizontal arm, arranged to clamp a baseball bat so that it would make contact precisely and repeatably with a ball on a tee at a radius of about four ft. from the axis (about the distance of the impact point on a held bat from the axis of the hitter). Initially, we considered that a spring (coil or torsional) mechanism would be appropriate, so that the bat arm could be rotated away from the ball—winding up the spring—and cocked. We never got as far as sketching out a trigger mechanism, obviously no trivial component. The whole system would have to be pretty rigid, not in the interest of realism but of repeatability. The spring would store a fair amount of energy, so safety was involved. We did not want to measure the distance a graduate student would travel.

After impact, the arm and bat would have to be stopped, which would require a shock-absorber mechanism, arranged so as not to break the bat or arm while stopping it. It would still

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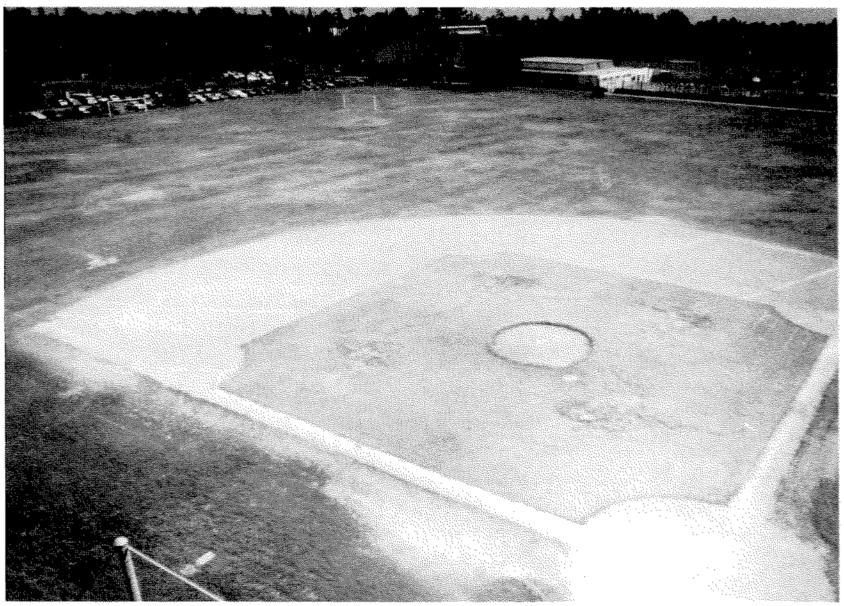
THE CAL TECH THEORETICAL BASEBALL TEAM PRACTICING FOR THE BIG GAME

be necessary to measure the initial velocity of the bat at impact, so that differences in the range of travel could be related to small variations in the impetus. The mechanism required to do all this was so violent that brief consideration was given to the design of a system in which the arm holding the bat was rotated by a motor, with rotation speed measured precisely, so that the impact velocity would be controlled. However, the rotation speed for an impact-point velocity of 200 ft. per second would have to be about 500 rpm, and the time interval between successive passages of the bat over the ball point would be about 1/8 second, in which time the ball would have to be introduced in such a way as to be stationary at impact. We also worried about the way the bat might be held. Ideally, the suspension should have the resilience or compliance and damping of a human bat handler, but how would you measure that, let alone reproduce it in a piece of machinery (apart from spinning up a volunteer)? I decided to leave that problem for later. This approach, therefore, seemed to possess the disadvantages of complexity. In addition, both these devices, spring-loaded and rotation, would be costly to manufacture and debug. These difficulties caused us to turn our attention to a different scheme, one in which an actual bat would not be used, but which might be simpler to construct and operate.

This device, which we never got so far as to design, was merely conceptual; it was to consist of a gun. An impacting piston, incorporating a piece of baseball bat, and energized by a spring or compressed air, would travel up a tube to make contact with a stationary baseball at about 200 ft. per second, propelling the baseball a horizontal distance of, say, 400 ft., roughly typical of a home run's travel. Contact would be arranged to impart some back spin to the ball about a horizontal axis, since this spin modifies the lift and drag to which the ball is subjected in flight, and has a substantial effect on the range achieved. The device could be relatively safe, with suitable arming and triggering precautions, and could be precisely aimed and locked at suitable azimuth and elevation angles. A certain amount of practice with store-bought balls should produce the initial calibration data we would require.

As part of the calibration process, we would have to measure the emitted ball's velocity each time (and spin rate, too) since we could not hope to make all impacts identical, and we would have to allow for variations in range resulting from slight changes in the initial conditions. It didn't seem that this would be too hard to implement.

This brought up the question of the actual tests, however: Where and how were they to be performed? The ball would travel in a typical home run trajectory, horizontally about 400 ft.



The deserted Caltech baseball field at night might have been the site of the baseball tests—but wasn't.

and perhaps to 150 ft. in height at a maximum. Since we would be striking 20 or 30 balls about ten or more times each, with each shot requiring some initial preparation and subsequent measurement, several minutes would be needed for each test, and the whole process would consume hours. Consequently, wind and temperature would be factors. Ideally, the tests should be done indoors, in a large air-conditioned building; a closed baseball stadium (Astrodome, King Dome) would be ideal, but obviously we could not conduct tests in such a partial (as opposed to impartial) location. How about a large aircraft or airship hangar? What size are the largest ones? I didn't know. The easiest thing to do was to conduct the tests outdoors in a suitable playing field, for example, at Caltech. For obvious reasons, we would want to work in private. It would not be suitable to have a number of people milling about, possibly prone to beaning by the batted ball, and harassing the distance measurement team. There are significant extra hazards to performing outdoor experiments at Caltech, too—audience suggestions, for example: "Why don't you make the launch tube of neodymium?" "Is gravity the same here as in Kansas City?" —and the usual $\ddot{x}$, F_{ij}^{*} quantum electrodynamics stuff. From all these considerations (time, temperature, throng) the only reasonable way of testing *al fresco* would be to work from, say, 2 to 5 a.m. There would be some other problems associated with working in the dark, of course, but with a relatively constant trajectory providing a landing ellipse not greater than 20 ft. in major axis length and less in the minor, plus radio-communications between mission control and target personnel, flashlights, hard hats, and maybe body armor, we should be able to get by.

The landing area was another puzzle. The best solution seemed to be to use a large shallow box, filled with sand of just the right density to bring the terminal baseball to a stop without its bouncing, burial, or damage—the ideal sand trap of the other sport, or of the long jump. Box edge markings and strings would facilitate the measurement of distance. To eliminate possible bias among the experimenters it would be necessary for the shooters to be ignorant of which ball (1986 or 1987) they were projecting, but on the other hand, the balls would have to be marked in code so we could keep track of the number of times each was struck. The code would ensure that the terminal team didn't know what the ball was either, when they measured its range. Then, before a test, the marked balls would be scrambled in a box and picked out blind for loading.

This process, imperfect as it was, took a few hours of thought; then we were ready to communicate with the reporter. I told Hall Daily where we were in the experiment design and he said he'd call the newspaper. Word came back that they weren't interested anymore. I presumed that the whole thing had been a figment of the reporter's imagination, and that when he told his editor about the project, the latter had told him he was deranged if he thought the newspaper would finance such a dubious research project.

As everyone now knows, home-run production fell off markedly in the second half of the season; no one surpassed Babe Ruth's record or Roger Maris's asterisked total, and discussion of a spiked baseball fell to zero. And in the 1988 season, home-run production dropped to a pretty low rate.

All the groundwork has been done, however, if the American League or National League wants to dig into the dynamics of a bonked baseball in the future. But count me out. □

Ronald Scott, the Dotty and Dick Hayman Professor of Engineering, has been a member of the Caltech faculty since 1958. He grew up in Scotland (his BSc is from Glasgow University) where he played cricket, not baseball. He continued playing cricket as a graduate student at M.I.T. (with a West Indian team) and even played a few times on a Caltech team. He has always played golf.