Chapter 1

IN THE BEGINNING WERE THE NUMBERS

Cigarette smoking is a health hazard of sufficient importance in the United States to warrant appropriate remedial action.

— Surgeon General's Report[1]

"You can prove anything by statistics" is a common gibe. Its contrary is more nearly true—you can never prove anything by statistics.

— G. V. Yule[2]

THIS IS HOW "Tobacco Timeline" on the Internet tells the story:

> On a Saturday morning, January 11, 1964, at 9 A.M., 200 reporters were physically locked into the State Department's auditorium to hear a two-hour briefing by surgeon general Dr. Luther L. Terry and a panel of experts. The top-secret measures were felt necessary because of the bold and closely-guarded conclusion reached in the brown paperback book they received titled *Smoking and Health*. When the press conference was over, the reporters ran to the telephones "like flushing ducks off a pond." In 1964, in a country where over 50% of adult males smoked, a multi-billion-dollar industry seemed to hang by the book's astounding verdict: smoking causes cancer.[3]

The Office on Smoking and Health of the Centers for Disease Control and Prevention adds a few more details:

> The circumstances surrounding the release of the first report in 1964 are worth remembering. The date chosen was a Saturday morning to guard against a precipitous reaction on Wall Street. An auditorium in the State Department was selected because its security could be assured—it had been the site for press conferences

> of the late President John F. Kennedy, whose assassination had occurred fewer than two months earlier. The first two copies of the 387-page, brown-covered report were hand delivered to the West Wing of the White House at 7:30 on that Saturday morning. At 9:00, accredited press representatives were admitted to the auditorium and "locked in," without access to telephones. Surgeon General Terry and his Advisory Committee took their seats on the platform. The report was distributed and reporters were allowed 90 minutes to read it. Questions were answered by Dr. Terry and his committee members. Finally, the doors were opened and the news was spread.[4]

As Terry himself recalled 20 years later, "We released [the] report in an explosive manner on a Saturday morning, and it was shocking to much of the public."[5]

There's something in the two accounts above that puzzles me, the first of four curious and surprising things about the famous 1964 report of the surgeon general I encountered when I began researching for this book.

As they record, January 11, 1964 was a Saturday, that day having been selected for the official unveiling of the report because the stock market was closed, thus precluding a "precipitate" selling frenzy by tobacco stock investors. Saturday was also chosen in order to maximize coverage in Sunday's newspapers. But the editorial I wrote about it, which I quoted from in the Preface, is dated January 10, 1964—Friday, the day before that fateful Saturday. How could this be?

Back in the days of "hot type," Newspaper Enterprise Association mailed its clients printed pages of all the various features in that day's package, along with papier-mâché mats from which they cast in lead their own type and engravings. I still have my editorial, which I cut out of the editorial page printout. George DeLong, the colleague who marked up my copy for the pressroom, had given it the title "Puffing in Peril" and the date *printed* on it is "1-10-64." (I always brought my editorials home for my wife to read, then stuck them in large manila envelopes, then, as the number of envelopes grew, put them in a cardboard box, eventually stashing the box in the attic in Ohio, then in an attic in Virginia and finally an attic in Georgia, never dreaming I would ever have reason to dig up this particular editorial again.)

Boyd Lewis, president of the company and a nonsmoker, was in town that week from NEA's New York headquarters on one of his

periodic visits to the hinterlands. I distinctly remember encountering him that Friday morning on the stairs leading to the second-floor business offices. He asked me if I'd heard about the surgeon general's report. I said yes I had, and nodded gravely. He knew I was a smoker and looked at me equally gravely and nodded back. It wasn't necessary for him to say that he expected me to write an editorial on the subject, with my usual trenchant comments.

(I was not only NEA's "chief editorial writer," I was its only editorial writer, and one difficulty I labored under was that about half our client editors used my daily musings as their own unsigned editorials while the other half ran them as op-ed columns accompanied by my name and picture. Thus I always had to tread a fine line between being too personally opinionated on the one hand and too bland on the other. However, my editorial on the surgeon general's report expressed my true feelings on the matter. Based on the knowledge I possessed at the time, I wouldn't have written it any other way.)

There also was an editorial staff meeting that afternoon at three o'clock. I'd completely forgotten about it but discovered the minutes of it while rummaging around in another box in the attic. I must have written my editorial about the surgeon general's report that morning because my lead one on the Panama crisis was favorably mentioned. The only reference to the surgeon general's report had to do with an editorial cartoon about smoking, which I don't remember at all.

In the same box I found copies of two letters. The first had been addressed to me by the editor of a newspaper in Redlands, California. Unfortunately, it was made on a chemical copier and most of it had faded away, but I could make out the gist of it. He mentioned "Puffing in Peril" and thanked me for saving him from having to spend a couple hours on a Saturday afternoon writing editorials, and for "writing much better than I would have myself." The date at the bottom was still legible—January 11, 1964. This meant that he had received our Friday mailing on Saturday, the very day the surgeon general's report was supposedly revealed to the public for the first time.

The second letter had been sent to Boyd Lewis by the editor of *The Daily Sun* in Yuma, Arizona, and passed on to me. This copy was in good condition.The editor said he liked the brevity of my editorial on the SG's report but found it "far too mild for the nature of the danger involved." This one was dated January 13, 1964. But if our package

had reached California in only one day, on Saturday the 11th, it must have got to Arizona the same day.

It was only when I read that second letter for the second time, after a lapse of more than three decades, that I fully appreciated how immediately and completely the surgeon general's report had been accepted even by supposedly hard-boiled, cynical newspapermen—or at least by one of them, who thought I had not editorialized strongly enough about "the nature of the danger involved." And it was then, reading between the lines of my own editorial, that I realized that my present reservations about the SG's report actually dated back to the very beginning.

The only source I could have had upon which to base my editorial had to have been the United Press teletype machine, which clattered 24 hours a day a few feet from my desk. If 200 reporters had been locked into an auditorium to prevent premature leaks about the surgeon general's findings, how could UP (later United Press International) have sent out its story a day before the official release (or possibly even Thursday evening, *two days* before it, since I had written my editorial on Friday morning)? Obviously, the closely guarded report had not really been all that closely guarded.

Thirty-three years later I looked up *The New York Times* for January 10, 11 and 12, 1964 on microfilm at the main Cobb County, Georgia, library in Marietta. There was nothing at all about the report on Friday or Saturday but, sure enough, the paper did its usual complete "cleanout," as we used to say, of the event on Sunday, January 12. Jumping from a lead-in article on top of page one were two full pages, 64 and 65, on the story, continuing on page 66 and taking up three-quarters of that page, and another half a page on page 67, all in the *Times*'s customary small type and narrow columns.

There was also an editorial which fully endorsed (or parroted) the report and hit all the bases that were to be hammered on in coming years: "It is now official . . . smoking is harmful to health—and remedial action is necessary . . . [T]he Surgeon General promises that the Public Health Service will recommend further action. The Congress and other concerned Federal agencies cannot evade their responsibilities here . . . The immediate and main thrust of an antismoking campaign should be directed toward the nation's youth."

That last statement put the *Times* well ahead of even the surgeon

general. It was to be at least a couple decades before the antismokers realized that despite their best efforts their crusade for the total eradication of smoking was being stymied by a stubborn minority of Americans who persisted in the habit. It was then that they enlisted "the children" to carry the crusade to a smoke-free Jerusalem.

The editorial also stated that the surgeon general's 10-man panel had come to its conclusion after an objective and meticulous study of "many thousands of scientific reports." This was a bit of an overstatement, as we shall see later in this chapter.

(Way down at the lower right-hand corner of page 65 was an item from the Associated Press quoting one scientist who was not convinced there was an association between smoking and lung cancer. "The government has only statistics," said Dr. Harry S. Greene, chairman of Yale University's department of pathology, "and a statistical association has to be interpreted. It might show cause and effect or it might show happenstance. But the results must be subjected to a laboratory test. They've been doing that for 15 years and have come up with absolutely nothing."

(I don't know what kind of laboratory test Dr. Greene had in mind or what previous tests he was referring to, but the tiny size of the item—two and a half inches—and its placement was a sign of how much exposure *The New York Times*, and everybody else in the years to follow, was going to give to dissenting voices. However, the newspaper had done its journalistic duty and reported "both sides" of the issue.)

It is interesting, if only to me, to compare *The New York Time*'s editorial with the one I wrote:

The *Times* called upon both the legislative branch and agencies of the executive branch of the government of the United States of America to lend all assistance to some kind of as yet unspecified "remedial" action against smoking, and for an antismoking campaign especially directed at youth. As for me, the surgeon general's statement about the need for "remedial" action did not even register. As I quoted from myself in the Preface, I wrote simply that people who wanted to smoke would continue to do so despite the report, although they were now officially on notice that they did so at their own risk.

In short, I had actually thought: "That's it for this story; what'll I write about next week?"

In other words, the august *New York Times* called for concerted

intervention against smoking on a national scale; I naïvely believed that the decision to smoke or not to smoke was one for each individual to make, guided by the newly revealed facts (or what I then accepted were the facts) about the potential dangers. It was of course the *Times*'s scenario the nation was to embark upon and to enlarge upon and to persist in for the remainder of the century (and will continue into the next), a scenario that has given rise to a myriad of unforeseen and pernicious consequences on American society.

I also checked the almost-as-august *Washington Post* for Saturday, January 11 and Sunday, January 12, 1964 to confirm that it also did not report on the report until the day after its official release. As I expected, its coverage of the story (on Sunday the 12th) was nearly as extensive as that of the *Times* but it was its editorial I was most interested in. It was surprisingly temperate: "The Committee *appears* to have established beyond reasonable challenge that the cigarette is a grave menace to health." [Emphasis added.]

As for the surgeon general's clarion call for "remedial action" to deal with the menace, "What that action should be is not yet clear," said the *Post* in its concluding paragraph, "but it is clear that the report should be read and pondered by every smoker in the land. A national habit which the Committee attributes largely to psychological and social drives entails an appalling drain on our national health and vigor. It can no longer be ignored as a harmless addiction."

I'm sure that a search of the archives of every newspaper in the land (other than a few hundred NEA clients who received our mailing the next day) would further confirm that Saturday, January 11, 1964 was the day the surgeon general's findings were revealed to the public. So how I could have written about it on Friday the 10th remains a mystery.

Unfortunately, Newspaper Enterprise Association did not wield the prestige of a *New York Times* or a *Washington Post*, nor did any of its subscribers, most of which were small-to-medium newspapers in small-to-medium cities. So anything written by me about the surgeon general's report counted for very little in the scope of things.

THE SECOND CURIOUS and surprising thing I learned about the report was that it seemed to have virtually disappeared into some sort of void. At least, it wasn't available anywhere the ordinary person could readily gain access to it.

Even before I began this writing, I knew that I had to obtain a copy of the all-important *Smoking and Health*. After all, it was what started everything this book is about. How could I write about the profound societal changes of the past three decades that this historic report set into motion on January 11, 1964 without seeing with my own eyes what the SG's advisory committee had actually said? How could I (fond thought) even contemplate making an analysis of the committee's conclusions without knowing something about how they had been arrived at? To my knowledge, no one had ever done such an analysis, certainly no layman like me. Like some kind of scripture, the surgeon general's landmark report was still venerated by believers three decades after it was handed down from the mountain but nobody consulted it any more, if anybody ever had.

But *Smoking and Health* was not in the Cobb County, Georgia, library system or in the main Fulton County library in downtown Atlanta. It didn't matter, however, because I had just acquired a computer with the capability of accessing the Internet and World Wide Web. On one of the few prosmoking—or, more accurately, anti-antismoking—sites on the Web I came across the name of one Lauren A. Colby, Esq., an attorney in Frederick, Maryland, who had written and self-published a small book called *In Defense of Smokers*, one chapter of which deals with the report. At that time he would send the book free to anybody who asked for it. (It is now available only by downloading from the Internet[6] but still at no charge.) I sent for a copy, read it, then wrote a letter to Mr. Colby. I told him of my inability to obtain the famous report. I asked him if he knew if it was available from the U.S. Government Printing Office and if I should write them. He said that he had had the same problem and that I would be wasting the postage.

> When I was trying to get the 1964 Report [he e-mailed me], I happened to be traveling a great deal, in connection with some hearings. Every time I visited a different city, I'd drop into the local library and asked whether they had the report. The answer was always "no." Plus, I made dozens of phone calls to libraries all over the U.S., with negative results.
>
> There is an office within the office of the Surgeon General, called "The Office on Smoking *and* Health," which works closely with a semiprivate organization called the "Office on Smoking *or* Health" (I'm not exactly sure how the two are related, but they are as "thick as thieves." I placed a call to the "and" office in D.C., and

> was told they couldn't help me. However, the lady suggested that I call a number in Atlanta (I'm not sure whether Atlanta was "and" or "or").*
>
> I called Atlanta, but got voice mail. Left a message. Weeks later, a lady called me and said that her organization had just one dog-eared copy of the report. She didn't know of any other place that I could get a copy, but she *did* know about a place in Alexandria [Virginia] which had the microfiche. That's how I got my copy.

(The Library of Congress has the report, of course, as I found out later by browsing the library's Internet catalog. It is listed under Call Number RA1242.T6U5, with the notation that it is for sale by the U.S. Government Printing Office. It may have been once but it isn't anymore. The LOC catalog also listed an edition published by D. Van Nostrand. In early 1998, still curious, I put in a belated request through my local library to the Interlibrary Loan System. They located a copy of the D. Van Nostrand hardcover edition in the library of Kennesaw Junior College. The elusive report had been available only a few miles away all the time! It is still strange, however, that such an important work should have required so much effort to find.)

Because he's a nice guy, and willing to help anybody who's trying to expose the pious falsehoods that have been spread, and continue to be spread, about smoking, Colby graciously offered to make a photocopy of his photocopy for me. (I'm also indebted to him for providing me with a copy of an obscure review of *Smoking and Health* by a professor of statistics named K. A. Brownlee, which I discuss below.)

One spring day in 1996 the heavy package arrived from Frederick. At long last I held the actual sacred text itself, or a reasonable facsimile thereof, in my trembling hands, all 387 pages of it.

I quickly discovered a third curious thing: there was no date on the title page. Only on the very last page, at the bottom in small type, was "U.S. Government Printing Office: 1964 O—714-422." I remarked

*Colby has since agreed with me that he was probably thinking of the Coalition on Smoking OR Health (it capitalizes the "OR"), composed of the American Heart Association, The American Lung Association and the American Cancer Society. The Office on Smoking and Health is part of the Centers for Disease Control and Prevention (CDC) in Atlanta, Georgia. There is also a private organization called Action on Smoking and Health (ASH), about which much, much more later in this book.

on this to Larry (we were now electronically corresponding regularly).

"Yeah, this is definitely Orwellian!" he replied. "However, the fact that there was no date on the Report kinda shows that it was never intended as a serious scientific document. Maybe that's why medical and other libraries didn't keep it."

You've got to be kidding, Larry. Whether or not it was truly scientific, the report was certainly taken seriously (what an understatement!), both by the medical community and the community at large, as few such documents in history have been. Still, the absence of a date on its title page and its nonexistence in public libraries remain as curious puzzles, again if only to me. Just to prove something to myself, I did write to the Government Printing Office to order a copy of the report. Larry was right: it was a waste of postage. All I got was a list of other publications by the Public Health Service. Not a word about *The Report*.

Back to January 11, 1964 and those 200 reporters locked in the State Department auditorium and holding the thick report in their hands, and no doubt itching to rush to the telephones or to their typewriters.

Even if they were given an hour and a half to read it, I doubt if many of them did more than flip through its 387 pages. Or if some of them did try to examine it closely, I doubt if they understood much of what they were reading. Indeed, I doubt if all the members of the advisory committee itself understood everything; only one of them was an epidemiologst and only one a statistician, and the report is full of tables in tiny type, molecular diagrams, logarithmic graphs and arcane and highly technical passages.

One of the best examples is Appendix II to Chapter 8 of Part II, which I've reproduced on the following three pages to illustrate what I'm talking about. (Totaling 182 pages, Chapters 8 and 9 of Part II, which bears the title, "Evidence of the Relationship of Smoking to Health," constitute both the bulk and the core of the entire report.)

APPENDIX II

Stability of Mortality Ratios

In computing the mortality ratio of a group of smokers to a group of non-smokers, each group is subdivided into age-classes (usually 5-year). For the ith age-class let y_i denote the number of smoker deaths and x_i the number of non-smoker deaths. The "expected" number of smoker deaths in the ith class (expected on the assumption that smokers have the same age-specific death rates as non-smokers) is

$$\frac{\text{(Person-years for smokers in class } i\text{)}}{\text{(Person-years for non-smokers in class } i\text{)}}\, x_i = \lambda_i x_i \qquad (1a)$$

The estimated mortality ratio $\hat{R}$ is defined as

$$\hat{R} = \frac{\Sigma y_i}{\Sigma \lambda_i x_i} \qquad (1)$$

summed over the age-classes.

In the interpretation of the values of $\hat{R}$ found in the seven studies, much weight has been given to the consistency of the values from one study to another, on the grounds that if the values of $\hat{R}$ for a particular cause of death are high in all seven studies, this evidence is more impressive than $\hat{R}$ values that are high in say, three studies but show no elevation in the remaining four studies. As a consequence, the question whether the value of $\hat{R}$ in an individual study is significantly above unity, in the technical sense of this term, becomes less important. Nevertheless, an answer to this question is occasionally useful in the analysis. Moreover, for some causes of death the total numbers of deaths, even when all seven studies are combined, are small enough so that a measure of the stability of the combined $\hat{R}$ is needed.

Assumptions

In attempting to get some idea of the stability of $\hat{R}$ without too much complexity, the following assumptions will be made.

1. The numbers of deaths y_i and x_i are distributed as Poisson variables. As Chiang (3) has shown, a more accurate assumption is to regard y_i and x_i as binomial numbers of successes. But with causes of death for which the probability of dying in a 5-year age span is very small the Poisson assumption, which is slightly conservative, is reasonable.

2. The quantities λ_i can be regarded as known constants. This is not quite correct. Initially, the λ_i are the ratios of the numbers of smokers to non-smokers in the age-classes, which can reasonably be regarded as given. In subsequent years, however, the numbers are depleted by deaths, and the number of deaths is a random variable. When death rates are small, however, this assumption should introduce little error.

3. The variates y_i and y_j are uncorrelated. An error in the age assigned to a death, putting it in the wrong age-class, induces a negative correlation between y_i and y_j. The existence of such errors should have no effect on

the variance ascribed to Σy_i on the assumption of independence. The same remarks apply to the assumption that x_i and x_j are uncorrelated.

4. The variates x_i and y_i are uncorrelated. An error in assigning a death to the correct smoking category would induce a negative correlation between x_i and y_i. Such errors should of course not be allowed to happen, since they vitiate the comparison of the death rates that is the main point of the study, but occasional errors of this type may have occurred.

With these assumptions the numerator Σy_i of $\hat{R}$ follows a Poisson distribution. The denominator $\Sigma \lambda_i x_i$ is a linear function of independent Poisson variates, and numerator and denominator are independent of one another. The exact distribution of a ratio of this type has not been worked out. Two approximate methods of obtaining confidence limits for the true mortality ratio $\hat{R}$ will be given. Confidence limits are presented rather than the standard error of $\hat{R}$ because the distribution of $\hat{R}$ is skew when the numbers of deaths are moderate or small, so that the standard error is harder to interpret.

The Binomial Approximation

If the λ_i can be regarded as approximately constant ($=\lambda$, say) then $\hat{R}$ becomes of the form $y/\lambda x$, where y and x are independent Poisson variates. Since λx then represents the expected number of deaths of the smokers, the quantity λ is estimated as the ratio of the expected number of smoker deaths to the number of non-smoker deaths.

By a well-known result it follows that $x/(y+x)$, the ratio of non-smoker deaths to smoker plus non-smoker deaths, is distributed as a binomial proportion with

n = number of trials = $y+x$
p = probability of success = $1/(1+\lambda R)$

where R is the true mortality ratio. Confidence limits for R are found from those for p.

Example. For the study of men in 25 States, the figures for lung cancer for cigar and pipe smokers are as follows:

	Non-smokers	Smokers	
	Observed	Observed	Expected
Number of deaths	16(x)	15(y)	9.71(λx)

Hence, $\lambda = 9.71/16 = 0.607$ and the binomial ratio is $16/31 = 0.516$. Hald's (9) table of the 95 percent two-tailed confidence limits of the binomial distribution gives 0.331 and 0.698 as the confidence limits for p. Those for R are given by the relation

$$R = (1-p)/\lambda p$$

This yields 0.7 and 3.3 as the 95 percent limits for R. Since the lower limit, 0.7, is less than unity, the estimated $\hat{R}$, 1.5, is not significantly above unity.

Unfortunately the assumption that λ_i is constant is not true in these studies. For instance, in the study of men in 25 States λ_i has the value 3.88 for cigarette smokers aged 45–49 and declines steadily with increasing age to a value of 0.96 for men aged 75–79. For cigar and pipe smokers the fluctuation in λ_i with age is less drastic but is still noticeable.

The Normal Approximation

This approach avoids the assumption that the λ_i are constant, but makes other assumptions that are shaky with small numbers of deaths. If R is the true mortality ratio, the quantity

$$y - Re$$

where $e = \Sigma\lambda_i x_i$ is the expected number of smoker deaths, will follow a distribution that has mean zero. If μ_i, m_i denote the true means of y_i and x_i, respectively, the variance of $(y - Re)$ is

$$\Sigma(\mu_i + R^2\lambda_i^2 m_i)$$

The basis of this approximation is to regard the quantity

$$\frac{y - Re}{\sqrt{\Sigma(\mu_i + R^2\lambda_i^2 m_i)}} \qquad (2)$$

as normally distributed with zero mean, since y_i and x_i are regarded, as previously, as independent Poisson variates. The 95 percent confidence limits for R are then obtained, by a standard device, by setting the absolute value of this quantity equal to 1.96 and solving the resulting quadratic equation for R.

Since the μ_i and the m_i are unknown, a further approximation is to substitute y as an estimate of $\Sigma\mu_i$ and $\Sigma\lambda_i^2 x_i$ as an estimate of $\Sigma\lambda_i^2 m_i$.

Example. For the example previously discussed the data are as follows:

$$y = 15;\ e = 9.71;\ \Sigma\lambda_i^2 x_i = 6.669$$

On squaring (2), the quadratic equation becomes

$$(15 - 9.71R)^2 = 3.84(15 + 6.669R^2)$$

The roots are found to be 0.7 and 3.4, in good agreement with the limits 0.7 and 3.2 given by the binomial approximation. This agreement is better than will usually be found with small numbers of deaths.

The following are 4 comparisons of the confidence limits for cigarette smokers in the same study.

Cause of death	Number of deaths			Mortality ratio	95 percent limits	
	Non-smokers observed	Cigarette smokers			Binomial	Normal
		Observed	Expected			
Cancer of lung	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

And I thought I was going to "analyze" the advisory committee's conclusions! I'm still wondering what the difference is between "R" with the ^ on top and plain old "R" or between "y_1" and "y_j". Are we talking about real people here?

And why go into such detail about a study of pipe and cigar smokers and not cigarette smokers when it was cigarette smoking the committee was to indict while finding pipe and cigar smoking more or less harmless?

Fortunately for those 200 reporters, they didn't have to read or understand the report. Everything was laid out by the Public Health Service in a release that was distributed at the January 11 press conference.

TO DIGRESS FOR A moment, a couple of interesting historical notes are contained in a story by Marjorie Hunter, headed "Smoking banned at news parley," in the January 12, 1964 *New York Times*:

> There was not a whiff of tobacco smoke in the State Department auditorium today when the report on smoking and health was released.
>
> Nine "no smoking" signs were fastened to the walls.
>
> But in the lobby and corridors outside, a few newsmen and Government and tobacco industry spokesmen puffed self-consciously on cigarettes.
>
> Some looked sheepish. Some slipped into vacant rooms along the corridors. Some puffed determinately, as if trying to convince themselves that they were not a bit worried about the report's conclusion that cigarette smoking enhances the death rate.

Already, the "guilt factor," without which the coming antismoking crusade would never have gotten off the ground, was in full operation among smokers.

Ms. Hunter further wrote that:

> One of the heaviest smokers this morning was J. Stewart Hunter, the Surgeon General's assistant for information. "I must have smoked 15 cigarettes," he confided. "I was nervous. We were scared we'd be stormed by 5,000 folks, all clamoring to get in here." Instead, only 200 persons showed up. Most were newsmen. Others represented the tobacco industry and various health organizations and departments of Government. Far more people used to show up for President Kennedy's news conferences in the same auditorium.

So only "most" of the 200 attendees were reporters. While the surgeon general's report proved to be, indeed, a watershed event, the hardbitten, heavy-smoking practitioners of the journalistic profession at the time were not universally in attendance at its birth and were perhaps not all that concerned with the subject. Antismokers were going to have to do a heck of a lot more to rally the national media behind the call for "remedial action"—and their successful doing of it is, of course, the reason for this book.

I HATE TO THINK what the statistical gobbledygook I reproduced on pages 40-42 would have been like if the "Assumptions" in Appendix II had not avoided "too much complexity"! There is a lot of such padding in the report. I do not use the word in any derogatory sense. Such elaborate detail was not "padding" by the advisory committee but the result of its determination to examine the question of smoking and health from all possible angles and approaches and provide the completest possible documentation for its conclusions. Overall, the report is an impressive collection of just about everything that was known or suspected or alleged about tobacco and its uses and effects at the time it was written.

Ignoring the mathematical symbols, the table for the study of men in 25 states in the "Binomial Approximation" does seem on its surface to be pretty straightforward. The figures indicate that there was one more death from lung cancer among nonsmokers than among pipe and cigar smokers. But could any meaningful conclusion be derived from such a small number of deaths—a total of only 31? It could hardly have meant that nonsmokers ought to take up pipe or cigar smoking! Appendix II in fact states that the "Normal Approximation" involves "shaky" assumptions. Or do the numbers 15 and 16 mean something other than the actual, counted number of deaths? As we will see below, the 25-state study involved no fewer than 448,000 men and lasted for more than three years. Were there really only 15 deaths among pipe and cigar smokers? Also of interest, even though the number of smoker deaths exceeded the "expected" number, so did nonsmoker deaths.

A statistician could explain it all and would probably consider it ridiculously elementary. In fact, by dint of plowing through a couple beginner texts on statistics, I can appreciate now that the formulas in Appendix II really are fairly basic, especially compared to some

statistical exercises that require the use of special computer software programs to do the calculations. The "Poisson distribution" is a standard formula in statistics. But again I doubt that questions like those posed above were running through the minds of those restless reporters (how many, if any, where versed in statistics?) and I have reproduced Appendix II only to illustrate the difficulties facing any nonscientist who would have attempted to "analyze" the report. Anyway, in his briefing, the surgeon general may have noted in passing that lung cancer among pipe and cigar smokers was not found to be of any great danger, so the reporters didn't have to try to understand the convoluted mathematics of those tables. The main conclusion of the report—the one the SG certainly concentrated on, the one the reporters had their ears tuned for, the one that flushed them like "ducks off a pond" to the telephones—was the connection between lung cancer and *cigarette* smoking. And that connection was clear and unmistakable, as shown by another table from the same 25-state study at the bottom of page 119. To summarize it:

- For cancer of the lung, 16 deaths were again observed among nonsmokers, but 399 deaths were observed among cigarette smokers.
- For emphysema, the observed number of nonsmokers deaths was 7, but was 115 among cigarette smokers.
- For cancer of the rectum, the observed number of nonsmoker deaths was again 16, but was 64 among cigarette smokers.
- For influenza and pneumonia, the observed number of nonsmoker deaths was 29, and of cigarette smokers it was 97.

This was a total of 68 nonsmoker deaths and 675 smoker deaths from these diseases, or approximately 10 smoker deaths for every nonsmoker death. This is very impressive on the face of it. Yet not only does a total of 743 deaths out of nearly half a million subjects over three years seem rather few on which to base a sweeping verdict but, if my arithmetic is correct, those 675 smoker deaths amounted to only about 1.5 thousandths of that half million. Moreover, as I will explain in the next chapter, when you compare smoker vs. nonsmoker deaths on the basis of their death rates per 100,000 people, you find—believe it or not—that a smoker in this study had 99.9 percent of the chance that a nonsmoker had of *not* contracting these diseases!

As for the "confidence limits" (more usually expressed as the "confidence interval") from which the surgeon general derived the "mortality ratios" in this table (more usually expressed as the "relative risk"), in the course of writing this book I have of necessity learned a little bit about epidemiology. (Just enough to be dangerous, some might say, but the same could also be said of many of the researchers who conduct smoking studies.) A 95-percent confidence interval, which is considered to be the gold standard in epidemiology, means that the authors of a particular study are 95 percent confident that their findings are not due to pure chance or to certain "variables" or "confounding" factors not taken into consideration—diet, alcohol use, family health history, etc.

To put it another way, there is only a five percent possibility that the "relative risk" is due to chance. Relative risk, or RR, is the estimated excess risk that people who do something, like smoking, or are exposed to something, like radon or asbestos, have of acquiring a particular disease or illness compared to people who don't do that something or aren't exposed to that something.

To be even more specific, a 95-percent confidence interval means that there is a two and one-half percent chance that the risk is actually somewhat higher and a two and one-half chance that it is actually somewhat lower than the figure they hand out to the newspapers. The essential thing is that the RR must be at least above unity, or 1.0, and preferably above 2.0, for the RR to be considered "statistically significant." *

But even if a risk found in an epidemiological study is statistically significant, and especially if it is just barely so, it does not mean it has anything to do with real life in the sense that it enables anyone to predict that any given human being will or will not acquire the disease being studied.

Since the lower limit of the confidence intervals the surgeon general reported for cancer of the rectum and for influenza and pneu-

*"In epidemiology, relative risks of less than 2 are considered small and are usually difficult to interpret. Such increases may be due to chance, statistical bias, or effects of confounding factors that are sometimes not evident . . . Lynn Rosenberg, Sc.D., Boston University School of Medicine, points out that a 'difference in risk of 50 percent (relative risk of 1.5) is small in epidemiological terms, and severely challenges our ability to distinguish whether it reflects cause and effect or whether it simply reflects bias.'"[7]

monia are either at unity (1.0) or just above, the risk to cigarette smokers of contracting these diseases because they smoke cigarettes really amounted to no risk at all.

Ah, but for lung cancer and emphysema, the confidence intervals, which range in the first case from 5.0 to 21.4 and, in the second case, from 3.5 to 40.0, are significant indeed, at least epidemiologically speaking. The only question is: how valid were the studies themselves in terms of accounting for and eliminating all possible confounding factors before they arrived at these figures? We'll see below how well they did on that score.

THE 25-STATE STUDY was one of a mere seven studies that the advisory committee used as the basis for its conclusion regarding cigarette smoking and lung cancer. But before looking at those studies, a little background may be in order. (Much of it cribbed from Gene Borio's "Tobacco Timeline" [see Note 3 for this chapter] and "A Capsule History of Tobacco" on the Internet. Borio's "Tobacco BBS" or bulletin board at www.tobacco.org is also an excellent source of current news and commentary in the never-ending crusade against smoking.)

Tobacco has been hated and preached against ever since the weed was introduced into Europe from the New World in the 16th century. Its use nevertheless spread rapidly to every part of the globe, despite such severe penalties as cutting off the noses of smokers in Russia and even executing them in Turkey. But this hatred was based on little but pure prejudice. This inexplicable prejudice still operates today and seems to be so fundamental to the worldview of a certain portion of the human race that it would continue to exist even were it somehow proved that tobacco promotes health and long life.

And although antitobacco "authorities" frequently claimed that smoking was a cause of any number of diseases or disabilities, especially after the advent of the cheap, convenient and ubiquitous cigarette in the late 19th century, it was not until near the middle of the 20th century that doctors began reporting a worrisome increase in the incidence of lung cancer, up until then a rare disease, with the victims overwhelmingly found to be cigarette smokers. Credible scientific support for the suspected smoking-lung cancer connection soon began to appear.

In 1950, for example, Dr. Morton Levin published the results of a

10-year epidemiological survey of Buffalo, New York, patients in the prestigious *Journal of the American Medical Association* (JAMA). His startling finding was that smokers were statistically twice as likely as nonsmokers to develop lung cancer. (Startling, that is, until you remember that "statistically twice as likely" meant that the relative risk he found must have been around 2.0, or only marginally significant.) But this was the heyday of cigarette smoking and few people outside or even inside the medical establishment paid much attention.

In 1952, however, *Reader's Digest*, which was a pioneer in and still is a leading fount of antismoking propaganda for the millions, published an article titled "Cancer by the Carton," based on the findings of Levin and others. This helped set off a "tar derby" among the companies and the introduction of cigarettes with lowered tar and nicotine levels and improved filters.* The *Digest* later published another article comparing filtered cigarettes in which it gave Kents high marks. I vaguely remember the article. Whether or not I read it or only heard about it, it may have been a factor that prompted me to start smoking Kents with their "revolutionary" new Micronite filter that was later claimed to have been so deadly because it contained asbestos. The "tar derby" continued in pace with the public's increasing awareness of the smoking-and-health issue. In point of fact, by the early 1960s, even before the surgeon general's advisory committee set about its work, the number of cigarette brands had expanded from the original "big five"—Camel, Lucky Strike, Chesterfield, Old Gold and Philip Morris—to no less than 17 major brands, with all of the newcomers filtered.

(Now that I think back to the 1950s, I can recall a few people, maybe parents of a friend of mine or acquaintances of my own parents, who had once been cigarette smokers and had simply quit—entirely unaware that they had kicked a habit we are now told is as bad as shooting heroin and even more addictive. Whether they did so out of health concerns or other personal reasons, I don't know, any any more than I know why my father switched from cigarettes to pipes and cigars in the 1930s.

*According to C. Harcourt Kitchin, the chief reason the companies introduced filtered brands, at least in Britain, was not for health reasons but because only the tobacco content of cigarettes was taxed. I would guess also that since filtereds used less tobacco than unfiltereds, they probably could get more of them out of a given quantity of tobacco.[8]

(It was also around this time that I read an article somewhere by a well-known writer of that day, Quentin Reynolds, whose doctor advised him to stop smoking because his throat "looked like a piece of raw meat." What should it look like? I thought. Cooked meat? Of course, the doctor meant that the throat was inflamed and, naturally, the inflammation could *only* have been caused by smoking. I seem to remember a certain tone of bitterness in Reynolds's account as he suddenly recognized the power he had allowed cigarettes to attain over him, and a sense of having been "betrayed" by those little white cylinders he had once enjoyed setting fire to. Or maybe I am projecting this thought into the past because I have heard that kind of feeling expressed by contemporary ex-smokers.)

One of the most notable studies was published in JAMA in 1953. Drs. Graham and Wynder reported that tobacco tar condensates from tobacco smoke painted on the skins of mice caused tumors which, by logical extension, implicated tobacco smoke with lung cancer in humans. This study was alarming enough to the industry that it hired the public relations firm of Hill & Knowlton* to deal with the smoking "health scare" and to form the Tobacco Industry Research Committee (later the Tobacco Research Council), with the purported purpose of encouraging research on smoking and health. In charge would be a scientist "of unimpeachable integrity and national repute" overseeing an Advisory Board composed of "distinguished men from medicine, science, and education." In January 1954 The TIRC ran a full-page "A Frank Statement to Smokers" in more than 400 newspapers:

> Recent reports on experiments with mice have given wide publicity to a theory that cigarette smoking is in some way linked with lung cancer in human beings.

*This is something else I didn't know until I started researching for this book. In 1964, when I lived in New York, a Hill & Knowlton account executive named Ben Schechter took me out to lunch at least once a month, during which he would pitch a "hook" upon which to hang a column mentioning one of his clients. Not once was it suggested I write something about the smoking controversy, even though Ben and other H&K executives I met knew that I smoked and would be favorably inclined to the industry. In fact, in 21 years of writing editorials after 1964, not once was I approached by *anybody* to write about smoking. It makes me feel kind of slighted.

> Although conducted by doctors of professional standing, these experiments are not regarded as conclusive in the field of cancer research. However, we do not believe that any serious medical research, even though its results are inconclusive, should be disregarded or lightly dismissed. At the same time we feel it is in the public interest to call attention to the fact that eminent doctors and research scientists have publicly questioned the claimed significance of these experiments.
>
> . . . We accept an interest in people's health as a basic responsibility, paramount to every other consideration in our business. We believe the products we make are not injurious to health. We always have and always will cooperate with those whose task it is to safeguard the public health . . .

This was damage control, or what today we call "spin-doctoring." But even a cynic would agree that one sentence in the industry's statement was sincere and came straight from the heart:

> [T]he fact that cigarette smoking today should even be suspected as a cause of a serious disease is a matter of deep concern to us.

Regarding the mice experiment, it is a fact that just about anything causes cancer in laboratory mice or rats. Indeed, they're specifically bred for susceptibility to carcinogenic substances. They also have to be subjected to massive doses of a substance, vastly exceeding anything people encounter in the real world, in order to achieve a result within their short lifetimes. It is another fact that people don't paint the inside of their lungs with concentrated tobacco "tar" (smoke condensates) mixed with acetone, where it is left to fester for weeks, as was done on the skins of the mice in Wynder's and Graham's "bioassay" and later on rabbits' ears in a second experiment by Graham in 1956. It is a further fact that no one has ever induced *lung* cancer from tobacco smoke itself in an animal.

This in no way proves that tobacco smoke does not cause lung cancer in humans. But researchers tend to extrapolate findings from animal experiments to humans when it supports whatever they are trying to prove. When the experiments don't result in anything except a lot of dissected animals, they tend to keep quiet about it or, more usually, call for "more studies," which translates into "more money."

In any case, by the end of the 1950s, a fierce debate was raging within the medical community over the dangers of smoking. Or that

was the impression the TIRC labored to create—that physicians were divided over the issue. Indeed, some were; in editorials in its *Journal*, none other than the American Medical Association downplayed the significance of the mounting evidence against smoking. But that, it was charged, was because the docs were more worried about the specter of socialized medicine than about smoking. According to one source, the AMA later struck a deal with southern tobacco state congressmen. In return for the AMA's issuing a statement contradicting the surgeon general's 1964 report, the congressmen threw their votes against Medicare, which eventually came anyway.[9]

With or without the wholehearted support of the AMA, however, a tidal movement against smoking was building. From the section "Historical Notes and Chronology" in Chapter I of the 1964 *Smoking and Health*, pages 6 and 7:

> The U.S. Public Health Service first became officially engaged in an appraisal of the available data on smoking and health in June, 1956, when under the instigation of the Surgeon General, a scientific Study Group on the subject was established by the National Cancer Institute, the National Heart Institute, the American Cancer Society, and the American Heart Association. After appraising 16 independent studies carried on in five countries over a period of 18 years, this group concluded that there is a causal relationship between excessive smoking of cigarettes and lung cancer.
>
> Impressed by the report of the Study Committee and by other new evidence, Surgeon General Leroy E. Burney issued a statement on July 12, 1957, reviewing the matter and declaring that: "The Public Health Service feels the weight of the evidence is increasingly pointing in one direction; that excessive smoking is one of the causative factors in lung cancer."
>
> Again, in a special article titled "Smoking and Lung Cancer—A Statement of the Public Health Service," published in the Journal of the American Medical Association on November 28, 1959, Surgeon General Burney referred to his statement issued in 1957 and reiterated the belief of the Public Health Service that: "The weight of evidence at present implicates smoking as the principal factor in the increased incidence of lung cancer," and that: "Cigarette smoking particularly is associated with an increased chance of developing lung cancer."

Somewhere between 1956 and 1959, it seems that the "weight of

evidence" was no longer indicting just "excessive" smoking of cigarettes but any kind of cigarette smoking.

Even so, it took increasing pressure from the old-line public health organizations over the next two years to motivate the government to "do something." In the summer of 1961 the American Cancer Society, the American Heart Association, the National Tuberculosis Association and the American Public Health Association petitioned President Kennedy to appoint a national commission to study "the widespread implications of the tobacco problem"[10] by investigating the accumulating studies (by this time more than 7,000[11]) implicating smoking with lung cancer as well as other diseases, and, it was hoped, come up with a definitive and authoritative conclusion.

The new president had more pressing matters on his mind and it was nearly another year before Surgeon General Luther L. Terry, now under pressure from some members of Congress, formally proposed to Secretary of Health, Education and Welfare Abraham Ribicoff the formation of an advisory committee composed of "outstanding experts who would assess available knowledge in this area (smoking vs. health) and make appropriate recommendations."[12]

In July 1962 the SG met with representatives from the American Cancer Society, the American College of Chest Surgeons, the American Heart Association, the American Medical Association, the Food and Drug Administration, the National Tuberculosis Association, the Federal Trade Commission and the President's Office of Science and Technology, plus the industry's six-year-old Tobacco Institute, to compile a list of more than 150 scientists and physicians "working in the fields of biology and medicine, with interests and competence in the broad range of medical sciences and with capacity to evaluate the elements and factors in the complex relationship between smoking and health."[13] Any of the organizations, including the Tobacco Institute, had right of veto over any name, no questions asked. From that list Terry appointed a 10-member advisory committee.

Wrote Elizabeth Drew later in *The Atlantic*, "The cigarette industry, it was widely assumed, had been boxed in."[14]

The report's "Historical Notes and Chronology" cites seven "significant developments" since Burney's statement in 1959 on the position of the Public Health Service as the reasons for the advisory committee's undertaking:

1. New studies indicating that smoking has major adverse health effects.

2. Representations from national voluntary health agencies for action on the part of the Service.

3. The recent study and report of the Royal College of Physicians and Surgeons of London (which had reported early in 1962 that cigarette smoking was a cause of lung cancer and bronchitis, probably contributed to the development of coronary heart disease and various less common diseases, and delayed healing of gastric and duodenal ulcers).

4. Action of the Italian government to forbid cigarette and tobacco advertising; the voluntary curtailing of cigarette advertising by Britain's major tobacco companies on TV, and a similar decision on the part of the Danish tobacco industry.

5. A proposal by Senator Maurine Neuberger that Congress create a commission to investigate the health effects of smoking.

6. A request for technical guidance by the Public Health Service from the Federal Trade Commission on labeling and advertising of tobacco products.

7. Evidence that medical opinion has shifted significantly against smoking.

Needless to say, all of this history, from the first reports of smoking-caused lung cancer to the appointment of an official government commission, passed completely over my head as I smoked blithely and happily away, preoccupied with living my so-called life.

In November 1962, the 10 members of the advisory panel and their staff assistants convened for the first time in the basement of the National Library of Medicine at the National Institutes of Health in Bethesda, Maryland, and, assisted by some 189 outside "contributors," set about the gargantuan task of sifting through more than 6,000 articles from 1,200 journals up to 1959 (evidently the origin of *The New York Time*'s "many thousands"), supplemented by an additional 1,100 titles provided by the National Library of Medicine. Over the next 13 months their work continued, enveloped in an aura of secrecy reminiscent of that in which Enrico Fermi and his team of nuclear scientists had achieved the first atomic chain reaction under Stagg Field at the University of Chicago 22 years before.

The analogy is not inapt. The surgeon general's 1964 report was to set off a chain reaction in American society—and indeed around the

world—as significant and far-reaching in its way as the one in Chicago in 1942. But while the resulting atomic bomb changed the world forever in an instant, we have yet to witness the final consequences of the bomb the Advisory Committee to the Surgeon General of the Public Health Service exploded on January 11, 1964.

Looking back now—considering the centuries-old prejudice against tobacco (even if it may not have been shared by every or even any member of the surgeon general's advisory committee or staff, about half of whom were smokers); considering the thousands of studies condemning it; considering Surgeon General Burney's prior public statements on behalf of the Public Health Service; considering the secrecy in which the advisory committee labored (they were fully aware that they were in the process of fashioning a document that would have profound social ramifications)—considering all this, it is clear to me that the entire investigation was really an exercise in validating what many medical authorities already believed. In short, the historic verdict on page 232 of the report that "Cigarette smoking is causally related to lung cancer in men" and "The data for women, though less extensive, point in the same direction" was nothing less than foreordained.

AS NOTED ABOVE, the advisory committee relied chiefly upon seven studies. It wasn't until Chapter 8 and page 81 of the report—after explaining how the investigation was conducted and what criteria* were used for judgment and after presenting a summary of its conclusions, followed by tables on the consumption of cigarettes in the United States and diagrams of the chemical and physical characteristics of tobacco and tobacco smoke and the pharmacology and toxicology of

*"Statistical methods *cannot establish proof of a causal relationship* in an association. The causal significance of an association *is a matter of judgment* which goes beyond any statement of statistical probability," said the report [emphases added]. To guide them in making the leap from a statistical association to the judgment of a causal relationship between smoking and disease, the advisory committee relied on five criteria: the consistency of the association, the strength of the association, the specificity of the association, the temporal relationship of the association and the coherence of the association.[15]

As for nicotine, the committee held that it was a habit-forming but not a physically addictive drug. That verdict was not to be long tolerated by the antismoking community (see Chapter 4).

nicotine—that the report got around to the actual studies on which it based its conclusions. (How many of those reporters in the State Department auditorium read that far?)

These were seven large "prospective" studies of American and Canadian males. The text explains that in a prospective study, information about current and past smoking habits is first obtained from the members of the group to be studied, along with supplementary information such as age. "Provisions are also made to obtain death certificates for all members of the group who die in subsequent years. From these data, overall death rate and death rates by cause are computed for the different types of smokers, usually in five-year classes." In brief, the job is to follow a number of people over a number of months or years and record who dies and what from, and whether they were smokers or nonsmokers.

The earliest study was begun in October 1951, the latest in October 1959. They were (with the names of the researchers and duration of the study in parentheses):

(1) 34,000 British doctors responding to a questionnaire sent to all members of the medical profession in the United Kingdom. (Doll and Hill, from October 1951 to 1956.)

(2) 188,000 white American men in nine states between the ages of 50 and 69 enrolled by American Cancer society volunteers. (Hammond and Horn, from January-March 1952 to 1958.)

(3) 248,000 holders of U.S. Government Life Insurance policies, available to veterans who served in the armed forces between 1917 and 1940. (Dorn, from January 1954 and January 1957 to 1958.)

(4) 67,000 men aged 35-64 in nine occupations in California who were suspected of being subject to higher than usual occupational risk of developing lung cancer. (Dunn, Linden and Breslow, November 1953 and May 1957 to 1960.)

(5) 60,000 California members of the American Legion and their wives. (Dunn, Buell and Breslow, from May-November 1957 to end date not given.)

(6) 78,000 pensioners of the Canadian Department of Veterans Affairs who served in World Wars I and II and the Korean War. (Best, Josie and Walker, from September 1955 and July 1956 to 1961.)

(7) 448,000 men in 25 states enrolled by American Cancer Society volunteers. (Hammond, October 1959 and February 1960 to 1963.)

Because these seven studies were culled from some 7,100 other studies or articles or reports on smoking from at least 1,200 journals, it is reasonable to assume that they must have been the best-conducted and most probative the advisory committee could find in the whole bunch. (The first three had already been used in the report of the Royal College of Physicians and Surgeons two years before. The surgeon general's 1964 report not only adopted the same title as the British report—*Smoking and Health*—but borrowed heavily from it.)

But before looking at their findings I'll quote from the surgeon general's report some of the advisory committee's own caveats regarding them, lest I be accused later of having the temerity to try to second-guess these experts. (I don't know if these reservations about the reliability of the studies were included in the Public Health Service's press release. Even if they were, I doubt very much that they were dwelled on during the presentation to reporters on January 11, 1964.)

From pages 84 and 85 (all emphases in what follows are mine):

> Smokers and nonsmokers may differ with regard to variables other than age that are known or suspected to influence death rates, such as economic level, residence, hereditary factors, exposure to occupational hazards, weight, marital status, and eating and drinking habits. [The "confounding" factors—D.O.] In the summary results to be presented in subsequent sections, as in most results previously published, the death rates of smokers and non-smokers *have not been adjusted so as to equalize the effects of these disturbing variables* . . .
>
> A further complexity in interpreting the results comes from interrelationships among the variables that describe the habit of smoking. As will be seen, the death rates of a group of cigarette smokers vary with the amount smoked, the age at which smoking was started, the duration of smoking, and the amount of inhalation. In trying to measure the "net" effect of these variables, such as the number of cigarettes smoked per day, we should make adjustments so that the different groups of smokers being compared are equalized on all other relevant aspects of the practice. *This can be done at best only partially. Most studies measured only some of the variables on which adjustment is desirable.* When the data are subclassified in order to make the adjustments, the number of deaths per subclass are small, with the consequence that *the adjusted death rates are somewhat unstable.*
>
> Consequently, like previous reporters on these studies, we have

used our judgment as to the amount of subclassification and adjustment to present. *The possibility that part of the differences in death rates may be associated with smoking variables other than the one under discussion cannot be excluded.*

Pages 94 and 95:

None of the [seven study] populations was designed, in particular, to be representative of the U.S. male population. Any answer to the question "to what general population of men can the results be applied?", *must involve an element of unverifiable judgment.* However . . . taken as a whole, the seven populations offer a substantial breadth of sampling of the type of men and environmental exposures to be found in North America and Britain . . .

The seven studies differ considerably in size. They vary also in the extent to which they are free from methodological weakness. The studies of men in nine states [*sic*] and men in 25 States [*sic*], for instance, suffer from the difficulties that the populations studied are hard to define, that the smokers and nonsmokers were recruited by a large number of volunteer workers, and that completeness in the reporting of deaths was hard to achieve, since this depends on reports from the volunteers. On the other hand these studies have the advantage of being large [they were the two largest—D.O.] and of having a broad geographic representation of the U.S. male population . . .

Pages 95-98:

In all studies the death rates for non-smokers are *markedly below* those of U.S. white males in 1960. Even the smokers of one pack of cigarettes or more daily have death rates that average slightly *below* the U.S. white male figure. To some extent this is to be expected, since hospitalized and other seriously ill persons are not recruited in such studies. The sizes of the differences appear, however, surprising for the studies with United States populations . . .

It is clear that the seven prospective studies involve populations which are healthier than U.S. males as a whole. Secondly, the low death rates for non-smokers suggest the possibility that the studies recruited *unusually healthy groups of non-smokers* . . .

In all five studies that had a clearly defined target population, *sizeable proportions of the population were omitted.* The major reason was failure to answer the questionnaire . . . The possible effects of these amounts of non-response on the mortality ratios *have received little discussion* . . .

But even after adjusting for possible overestimation of mortality ratios because of nonresponse, the committee stated that:

> [U]nder *assumptions that are rather extreme*, although consistent with the available data, the mortality ratio of cigarette smokers would still remain substantially higher than unity [1.0] for these amounts of over-estimation . . . On balance we are inclined to agree with the opinion expressed by the authors of several of the studies to the effect that the general result of errors in reporting smoking history is to depress the mortality ratio of smokers relative to non-smokers, so that reported ratios will tend to be underestimates so far as this source of error is concerned.

In other words, both the study authors and the advisory committee were making informed guesses here. Or when in doubt, assume the worst.

In the British doctors study, for example, 32 percent of the doctors the questionnaire was sent to didn't send it back. Nevertheless, Doll reported that the death rate of the nonrespondents was higher than that of the respondents and that there were relatively more smokers among the nonrespondents than among the respondents. How he ascertained this if the nonrespondents didn't respond was not explained.

> In the 25-state study, *over 20 other variables* that may be associated with death rates were recorded. (Page 99)

> [I]t is not unreasonable to speculate that the kind of men who become regular cigarette smokers are, to a moderate degree, less inherently able to survive to a ripe old age than non-smokers. *We know of no way* to make a quantitative estimate of the difference in death rates that might be attributable to such constitutional and genetic factors. (Page 104)

(In the last chapter in the report, Chapter 15, the committee also considered the possibility that there are morphological differences between smokers and nonsmokers. In 1963, C. C. Seltzer had published in the *Journal of the American Medical Association* the results of a study of 922 Harvard graduates. He found that cigarette smokers were larger than nonsmokers, averaging 4.37 pounds heavier, pipe smokers larger still and cigar smokers the largest. The committee decided, however, that the data were "too meager to permit a conclusion." I think they were right. In my experience, smokers come in all shapes and sizes.)

> [P]art of the difference [between smokers and nonsmokers] may represent *a general debilitating effect of cigarette smoking* in addition to marked effects on a few diseases . . . though there are difficulties in making this hypothesis precise enough to be subject to medical investigation. (Page 105)

This was not even informed guesswork. On page 113 the report again suggests "the possibility that cigarette smoking has some general debilitating effect, although no medical evidence that clearly supports this hypothesis can be cited." My uninformed guess is that they decided, "Doesn't matter. We'll throw it in anyway."

With the above reservations in mind, what *did* those seven studies say?

1. For males who smoked cigarettes only, the overall death rate, expressed as the "mortality ratio," was higher than that for nonsmokers in all the studies. The increases ranged from 44 percent for the British doctors (Doll and Hill) to 83 percent for men in 25 states (Hammond).

2. For smokers of other forms of tobacco as well as cigarettes, the increases in death rates were in all cases lower than for smokers of cigarettes alone.

3. For smokers of cigars or of pipes only, three of the studies showed small increases in overall death rates, ranging from 5 to 11 percent. However, the British study and the study of men in 25 states found slight *decreases* in death rates for pipe and cigar smokers compared to nonsmokers!

It's not clear to me what the difference, if any, is between a "morality ratio" and a "relative risk." The "relative risk" is what researchers have usually reported in the multitudes of smoking studies that have been conducted, and continue to be conducted, since 1964. So I am assuming that these terms are interchangeable.

The only place in the 387 pages of the report where I found the term "relative risk" used was in the heading of a section in Chapter 9 on page 160: "Relative Risk Ratios From Retrospective Studies." There it was stated (with bracketed words added) that:

> Retrospective studies are usually designed to establish the probability of an association of an attribute A [say smoking] with disease X [say lung cancer]. Procedurally, one compares a supposedly representative group of patients with disease X , with another group as controls, in regard to the percentages of individuals with and without the attribute A.

Evidently "the probability of an association" is also another term for "relative risk."

As for the difference between a prospective study and a retrospective study, in the first kind the researchers wait to see how many of those who have attribute A (say smoking) develop and/or die from disease X (say lung cancer) over a period of months or years, while in the second kind the researchers look at those who already have disease X, or have died from it, and try to determine whether they also have or had attribute A (they are or were smokers).

The advisory committee did in fact examine 29 retrospective studies, and in each of those that investigated male lung cancer, the degree of association with smoking increased as the amount of smoking increased. However, the committee did not rely on them in reaching its historical conclusion because of well-known problems with retrospective studies, especially "recall bias." In such studies researchers depend upon the personal testimony of people as to how much they smoke or, if they no longer smoke, their recollection of how much they used to smoke. If the subjects are deceased, next best (or worst) are the recollections of their survivors or someone who knew them. While death certificates are used when available, these are also notoriously unreliable.

It was because of such deficiencies in retrospective studies, the committee noted on page 160, that several "courageous" investigators were led to undertake the protracted, expensive and difficult prospective approach. Among the first to do so were Doll and Hill, whose pioneer study of British physicians was initiated in October 1951.

(We have come full circle. In the field of smoking, the courageous investigators today are those who resist the temptation to apply for some of the abundant antismoking grant money that is available—much of it extorted from smokers—and to make a name for themselves by conducting yet another study into some aspect of smoking and health or, quite frequently, simply publish a rehash of previous studies.)

Other than in this section heading, the report doesn't use the term "relative risk" again. Thus one must assume that in a table on page 164 of ratios for lung cancer by smoking status, even though they are called "mortality ratios," the ratios given are relative risks. And for the seven studies they range from a ratio of 6.0 in the Dorn study to 25.2 in the Best, Josie and Walker study, for all smokers.

Recalling what Dr. Rosenberg was quoted as saying in the footnote on page 46, a relative risk of 1.5 is a 50-percent risk. That would mean that a relative risk of 2.0 would be a 100-percent risk, a relative risk of 3.0 a 200-percent risk, and so on up to that 25.2 relative risk in the Dorn study, which would be—what?—a *2,420-percent* risk?!

It is here that my headlong plunge into epidemiology began to arouse in me a profound skepticism. In ordinary, everyday language, 100 percent means a sure thing. If the weatherman predicts a 100-percent chance of rain, it means he's absolutely sure it's going to rain—*he couldn't be any more sure*. If someone jumps off a 10-story building, we can also be pretty darn close to 100-percent sure that he will kill himself (although there have been miraculous survivals from a fall that far or even farther). If he jumps off a 20-story building and falls twice as far, the risk of death would still be 100 percent, not 200 percent. A dead (no pun intended) certainty can't be greater than 100 percent, can it?

It is in fact because of this commonsensical understanding that 100 percent of anything is as high as you can go that antismoking researchers like to present their findings in terms of percentages—as, for example, a study I discuss in the next chapter that claimed that nonsmoking nurses married to smoking spouses had a 91-percent greater chance of lung cancer than nurses married to nonsmoking spouses.

Wow—91 percent! That's awfully close to the 100-percent-sure death-inviting risk of jumping off a tall building. And that was precisely the purpose of it—to convey to the ordinary person the idea that not only do almost *all* nurses married to smokers get lung cancer but so does nearly *everybody* married to a smoker. They can get away with it because not many people know that in epidemiology, a "91-percent risk" is a relative risk of 1.91, which is so small, so close to what epidemiologists call the "noise" or background level, that it is meaningless. Nevertheless, the American Heart Association had no compunctions about issuing a press release announcing this finding. It was heralded on an Atlanta TV news program as "new evidence of the danger of secondhand smoke."

Another source of skepticism for me was reading about "the latest study" showing that people who smoke are X number of times "as likely" or "more likely" to develop this or that disease than nonsmokers. For lung cancer, it has been put at from 10 to 20 times "as likely" for a smoker.

I wrote to Marilyn vos Savant's "Ask Marilyn" column in *Parade* magazine and asked her to explain this "times as likely" business, but the question was never used. Either it would have required too long an answer or it was simply too stupid to consider.

Eventually it dawned on me that my problem was that these "times as likely" figures are seldom related to any kind of baseline. That is, how many people do they actually refer to? For example, in Chapter 10 I cite a study which claimed that teenagers who smoke are "18 times as likely" to attempt suicide as teenagers who don't smoke. Does that mean that for every nonsmoking teen who attempts suicide, 18 smoking teens do? But *how many* teens attempt suicide? For that matter, how many succeed, and how are they broken down between smokers and nonsmokers?

You have to have a very significant relative risk to come up with a "times as likely" number that high, and it usually only happens with lung cancer. That is why, for most "smoking-related" diseases or behaviors, researchers like to present the risk as a percentage; it sounds so much more impressive that way.

To sum up my amateur's "analysis" of the surgeon general's report, I believe that there probably is a dose-response or dose-effect factor involved with smoking because it makes sense to me that anyone who chain-smokes two, three or four packs of cigarettes a day (though not necessarily how many years he does so) is courting injury to his health in some way. Too much of anything, including pure drinking water, is not good for you. It is an old axiom of medicine that "the dose makes the poison." That many cigarettes a day could overwhelm the body's constantly operating recuperative powers, which is why, contrary to medical authority, I think the number per day is more important than the number of years.

(Incidentally, as for true chain-smoking—lighting each cigarette from the butt of the proceeding one—my "personal best" is 18 consecutive cigarettes. But that was an exception and a long time ago and they were only king-size cigarettes, not the 100s I smoke today. My normal consumption has always been around a pack a day.)

Otherwise, however—as a layman, as an ordinary shnook like those 200, or how many there were, reporters locked in the State Department auditorium on January 11, 1964, like the editors of their newspapers and magazines and wire services, and especially like the millions of

readers and viewers of and listeners to the news media—I am forced in the end to accept on faith alone the conclusion of the surgeon general's advisory committee that smoking *may* cause or otherwise be implicated in lung cancer, at least in some cases.

But that smoking *will* cause lung cancer or some other dire disease—inevitably and universally, as most people have been taught to believe—or even that it may only *probably* cause lung cancer or some other disease, a lifetime's observation of people who smoke and people who don't smoke, plus my own half-century of smoking in good health, plus what I have learned about epidemiology, plus critiques by knowledgeable people of smoking studies conducted subsequent to the surgeon general's 1964 report, keep telling me: *it just ain't so*.

As the report itself states, epidemiology does not, and indeed cannot, show causation between attribute A and disease X but only a statistical association or correlation. The association between smoking and lung cancer is, and always will be, at its basis, statistical. This is why, to buttress its case against smoking, the advisory committee was forced to go beyond the raw numbers into the realm of subjective judgment. (Was the association consistent among the studies? Was the association strong? Was it coherent? That is, did it make sense?)

That "it's only statistical" is of course the frail reed that the cigarette makers have always clung to, or did cling to from 1964 to 1997, until sheer brute, extortionate force exerted by 40 *mafiosi*, a.k.a. the attorneys general of that many states (see Chapter 12), forced them to confess otherwise. (One wonders: if the "merchants of death" are nothing but a bunch of liars, what value is their confession? Are they saying under their breaths, in paraphrase of Galileo before the Inquisition, "Nevertheless, it's still only statistical."?)

Medical science is still searching for causation; i.e., how *exactly* does smoking cause lung cancer? One of the possible culprits the SG's 1964 report looked at was benzo(a)pyrene, which it called the most carcinogenic of all the many suspected carcinogenic substances in tobacco smoke. It's still being looked at. In 1996 some researchers in Texas claimed to have discovered *exactly* how the benzo(a)pyrene in cigarette smoke initiates lung cancer (see Chapter 2).

This "discovery" was, of course, widely reported in the media and has become part of "common knowledge," even though it was pure malarkey. Unfortunately for the researchers, this compound is present

everywhere in the environment and in greater amounts than in tobacco smoke.*

But what about those statistics in the 1964 report? At least one statistician at the time was less than impressed: K. Alexander Brownlee, associate professor of statistics at the University of Chicago and a member of the Royal Statistical Society of London, whose review of *Smoking and Health* was published in the *Journal of the American Statistical Association.*[18] In the second paragraph of his review, Brownlee wrote:

> I have observed amongst some statisticians a wistfulness that statistics has not so far played a larger part in science generally. Since this association between smoking and lung cancer, interpreted by many as one of causation, is of prime importance, one might have expected it to be greeted with enthusiasm and loud admiration. On the contrary, the comments and reactions of the statistical profession have been very restrained. By and large, in fact, the silence has been deafening.

(By and large, in fact, in the course of researching for this book I have noticed that the scientists who are usually most skeptical of studies purporting to show statistical associations between smoking and disease are . . . *statisticians*.)

As I had, Brownlee noted the absence of a publication date to the report, as well as the lack of an index, which made it difficult to look up subjects. He also observed that the list of organizations that met with the surgeon general on July 24, 1962 to suggest candidates for an advisory committee "appears to be heavily weighted towards government agencies and organizations large, general, and active in public relations, and to have low representation of societies with specifically scientific outlooks." No statistical society was represented, he commented.

To cut to the chase, in Brownlee's opinion a key factor in determining whether or not one accepted the report's conclusions was

*Benzo[a]pyrene (B[a]P) is one of the polycyclic aromatic hydrocarbon PAH compounds. Because it is formed when gasoline, garbage, or any animal or plan material burns, it is usually found in soot. The chemical combines with dust particles in the air and is carried into water and soil and onto crops . . . People may be exposed to B[a]P from environmental sources such as air, water, and soil and from cigarette smoke and cooked food . . ."[16] According to Joe Dawson, "A ten-pound bag of charcoal produces as much smoke (and harmful chemicals) as 160 packs of cigarettes."[17]

how one interpreted Table 26 on pages 109-110. This was a compilation of the smoker-nonsmoker mortality ratios for 24 diseases and one other category (accidents, suicides, violence) from the seven studies, listed in order of mortality ratios, from highest to lowest. They were:

Cancer of lung; bronchitis, emphysema; cancer of larynx; cancer of oral cavity; cancer of esophagus; stomach and duodenal ulcers; other circulatory diseases; cirrhosis of liver; cancer of bladder; coronary artery disease; other heart diseases; hypertensive heart disease; general arteriosclerosis; cancer of kidney; all other cancer; cancer of stomach; influenza, pneumonia; all other causes; cerebral vascular lesions; cancer of prostate; accidents, suicides, violence; nephritis; rheumatic heart disease; cancer of rectum; cancer of intestines.

The mortality ratios (relative risks?) ranged from 10.8 for cancer of the lung down to 0.9 for cancer of the intestines, with all but eight below 2.0.

> If one believes [Brownlee wrote] that the observed association between smoking and lung cancer is substantially real, and not an artefact of biased sampling, then one would seem required also to accept the observed association between smoking and almost all causes of death as substantially real.
>
> If one believes that the observed association between cigarette smoking and virtually all causes of death is substantially real, then one must take one of the following positions:
>
> (a) All the associations are due to causation.
>
> (b) Some of the associations are due to causation and others to correlations, concealed or otherwise.
>
> (c) All of the associations are due to correlations, concealed or otherwise . . .
>
> If one adopts position (a), then one is under some obligation to provide hypotheses as to possible mechanisms, or to hold out hope that future research will provide these hypotheses. The difficulty with the smoking hypothesis is that it has not really got to first base on even lung cancer, let alone the other 24 causes of death, even though the matter has been under intensive investigation for ten years or more.

(And, as noted above, more than three decades after the report was published to the world, the search for a possible "mechanism" by which smoking causes lung cancer continues.)

But if one adopted position (b), averred Brownlee, one was then placed in the unenviable position of admitting that concealed correla-

tions account for some diseases but not for others. And once one admits that concealed correlations account for a substantial number of the observed associations, "then one has to work very hard to disprove the hypothesis that they may account for all the observed correlations [position (c)]."

For lung cancer, one correlation emphasized in the report was that between the observed rise in lung cancer incidence in the 20th century and the per capita increase in cigarette consumption during the same period. But, said Brownlee:

> All statisticians know that the presence of a positive, zero, or negative correlation between two variables observed over time has been the basis for more ludicrous nonsense than any other statistical procedure. For example, the incidence of cancer of the stomach has been declining for many years . . . but only a madman would infer that the increased smoking has caused the decreased stomach cancer.

Actually, it's not such a mad idea today. Some modern studies have "suggested" (a cautious word researchers always use) that smoking may have a protective effect against stomach and colorectal cancer (see Chapter 3). In fact, on page 229 of the report the advisory committee stated that "the mean gastric cancer ratio for cigarette smokers [given as 1.4 in Table 26] is below the mean total mortality ratio" and concluded that *"No relationship has been established between tobacco use and stomach cancer."* [Emphasis mine.]

Yet it placed stomach cancer as 16th in the list of 25 smoking-related causes of death in Table 26. One asks why, if there was no relationship between this disease and tobacco use.

In my personal opinion, Brownlee's position (b) is the only logical one. It would go against all common sense to believe that smoking "causes" accidents, suicides and violence, which are listed as number 21 among the 25 alleged causes of deaths among smokers. There must be a multitude of other factors involved. But if there are hidden correlations in this category, then, as Brownlee said, you have the job of showing that there are no hidden correlations involved with all the other categories.

Table 26 really proved too much, and thus proved nothing. By associating smoking with so many named diseases and "other" or "all other" diseases and causes and happenstances, the advisory committee

not only violated but made rather a mockery of one of its own criteria: "the specificity of the association."

As for the correlation between the historical increase in lung cancer and the increase in cigarette smoking, Brownlee suggested a possible explanation:

> The generation born in 1880 reached the age of 20 in 1900, and a substantial fraction, 24 percent, had died by this time, presumably largely due to the traditional infectious diseases of childhood. The generation born in 1900 reached the age of 20 in 1920, and a lesser fraction of this generation had died by this age, namely 15 percent. Therefore, the 1900 cohort at the age of 50 represents quite a different stratum from the 1880 cohort at the same age of 50, since the former includes the "weaklings" who were eliminated from the latter.
>
> The relatively large number of cases of lung cancer observed in the 1900 cohort can merely be largely those who would not have survived to run the risk of lung cancer if they had been born 20 years earlier. On this model the alleged historical increase in rate of lung cancer can be readily accounted for.

It was an interesting theory at the time. But now almost all the people born in 1900 are gone, and because the case against cigarette smoking is settled as far as everybody except a few people like me is concerned, no one is going to waste time speculating about possible significant differences between that generation and the one that preceded it, even though—and I think this is important—it was chiefly individuals who were born in the early years of the century who were the subjects of the seven studies cited in the surgeon general's report. It is also possible that another of Brownlee's suggestions is still valid: that in the early days many cases of lung cancer may have been misdiagnosed as tuberculosis.

Brownlee also discussed benzo(a)pyrene and noted that cigar smoke has almost four times as much and pipe smoke about 10 times as much as cigarette smoke, "yet pipes and cigars are pretty well innocent of the charge of association with lung cancer."

(The advisory committee did conclude, however, that pipe smoking was causally related to cancer of the lip. But how pipe smoking could cause cancer of the lip when the tobacco doesn't touch the pipe smoker's lips, the committee didn't say. I would think that cigars, which do touch the lips, would be more likely to cause cancer of that site, but apparently they don't. Maybe pipe stems are the culprit.)

The difference between cigarettes and cigars and pipes "is a puzzling feature of the indictment of tobacco," Brownleee wrote, and then speculated:

> It could be, of course, that the variety of tobacco used differs significantly, that cigars and pipes do not use cigarette paper, that possibly cigars and pipes burn at lower temperatures, or that many cigarette smokers inhale whereas few pipe or cigar smokers do. But if inhalation is the crucial item, then it should show up strongly when cigarette smokers are analyzed into inhalers and non-inhalers, and as reported above, the present evidence is not clear.

Brownlee was here referring to page 188 of the report where "contradictory information" about inhalation is discussed. Several retrospective studies in which inhalation and the amount of smoking were considered came up with "the provocative finding that with increase in daily amounts of cigarettes smoked the differences in risks between inhalers and noninhalers *diminished* [emphasis added]. There is no immediate explanation for this apparent discrepancy."

It was more than provocative. It was astonishing. In one of their first studies, a retrospective study, Doll and Hill had asked lung cancer patients if they inhaled. When the results came back showing that fewer lung cancer patients inhaled than did smokers without lung cancer, they were at such a loss for an explanation that they simply stopped asking subjects if they inhaled.[19]

This also goes against common sense—that the more you inhale, the less risk you take compared to a smoker who doesn't inhale. Nor does it square with the dose-response relationship the seven prospective and 29 retrospective studies found: that the more you smoke and the more years you smoke, the greater the chance of lung cancer. My explanation is, well, there are studies and then there are studies and one is always free to pick and choose among them, depending upon what one wishes to prove. Also, the medical research community had barely begun marshaling its forces against smoking back then. Today those retrospective studies that found an inverse relationship between the amount of cigarette smoke inhaled and the risk of disease would either be laughed out of court or never published at all.

In any case, cigar smokers were to be allowed pretty much of a free ride over the ensuing years while the tobacco haters concentrated on cigarettes. That is changing, thanks to the recent upsurge in the

popularity of cigars. For sure, we will sooner or later be seeing all kinds of "latest studies" reporting that cigar smoking is every bit as dangerous as cigarette smoking and demands that the Food and Drug Administration regulate the nicotine content in cigars as well as in cigarettes, and eventually eliminate it entirely in both.

That raises an interesting question in my mind. Since the typical cigar smoker doesn't inhale but derives his pleasure from the taste of the tobacco, the feel of the cigar in his mouth, the holding of it in his hand, the sight of the growing ash on the end and the blue smoke he blows into the air, would it make any difference to him if it were nicotine-free? If it didn't, that would drive the antismokers nuts. It isn't nicotine they hate, it's smoking.

I am acquainted with only one cigar smoker, Lauren Colby, who does inhale the smoke and has been doing it for more than 40 years at the rate of six or seven cigars a day, plus a few pipes. He writes in *In Defense of Smokers*:

> The fact remains that inheritance seems to play a major role in cancer. Pancreatic cancer is very rare, but former President Jimmy Carter has seen it in at least four members of his family: his two sisters, his brother and his father. His mother died from breast cancer which metastasized to her pancreas.*
>
> Diabetes is the scourge of my family. Three of my four grandparents died from the disease. All were obese and consumed a diet rich in starches and sugars. As a young man, I was obese and ate a lot of starches and sugars. I chose to go on a life-long diet, in which I refrained from eating starches and sugars. Simply avoiding starches and sugars is enough to control my weight (I weighed 240 lbs. when I first went on the diet at the age of 38; now, I weigh 162 lbs.) I consider this a sensible precaution. If I had a history of cancer in my family, especially lung cancer, I might choose not to smoke. However, I have no such history, so I puff away.[20]

Speaking of cigars has made me realize that even a veteran ciga-

*Another curiosity: on the "Regis and Cathy" television show on December 26, 1996, Mr. Carter said that the reason he never started smoking was because he saw his brother and sister die of lung cancer from cigarettes. Since even the medical profession agrees that it takes a number of decades for smoking to cause lung cancer, this means that he waited around for at least 20 or 30 years to see how smoking affected his relations before making the decision not to smoke. *Come on, Jimmy* . . .

rette smoker like myself, as anti-antismoking as I can be, has been subtly influenced by decades of antismoking propaganda. When I see a picture of Arnold Schwarzenegger smoking a cigar it seems like a perfectly normal thing to do. The same with the handsome young officer who used to smoke cigars on the TV series "JAG" (until antismoking propaganda or something got to him and he gave them up.) The officer even shared one with his lovely sidekick. But if he had lit up a cigarette I would have been shocked. If she had smoked a cigarette, I would have been even more shocked (but pleased).

There's something about cigars—I don't know what: an impression of masculinity (though they look good on women too), of success, of self-confidence and satisfaction with one's status in life. They're also a prop or an emblem one can flourish. Cigarette smoking, which is now déclassé thanks to the antis, seems almost a furtive and sneaky and aberrant behavior.

Again we've come full circle. Tobacco men in the later part of the 19th century were quite as contemptuous of the new fad of cigarettes as any modern antismoker, as illustrated by a passage from the 1948 novel, *Bright Leaf*. In an early scene, the president of a tobacco company, Major James Singleton, has been approached by the inventor of a revolutionary cigarette-making machine capable of cranking out tens of thousands more cigarettes a day than the hand-rolling method then used. The major explains why he is not interested:

> "Believe me, I'm sorry, Mr. Barton. No hard feelings to you and your machine, understand . . . My family has been in tobacco, one way or another, for the last hundred years or so. We started raising it in Virginia way back before the Revolution . . . All that time we have been talking, eating, thinking, chewing, smoking, admiring tobacco to a man—why, it's been the very breath of our lives! That kind of association with and feeling for a thing makes for a pretty steady tradition, Mr. Barton. This tradition is built on everything that has to do with tobacco: its cultivation, its marketing, its proper function in the society of man. It's the sort of tradition that is respected in this part of the country and any place where tobacco is understood or admired . . .
>
> "And—again without meaning any offense, sir!—I simply can't agree with you about cigarettes . . . I've never been able to see them myself. In fact, I have always regarded their entrance into the business as a distinct step downward toward degeneracy and degradation in the use of a noble plant. A little paper wrapped around

> some low-grade sweepings and stuck into the mouths of grown men! It turns my stomach every time I have occasion to witness it. So you can see, Mr. Barton, I could not in all honesty encourage something that might help promote a condition that I not only deplore, but which is entirely opposed to my way of thinking about tobacco."[21]

Today, alas, the thousands of tobacco farmers for whom the cultivation of the "noble plant" is, as it was for the major, a proud tradition going back generations, are the indirect casualties of the crusade against smoking. Many make a living growing tobacco on small plots that are not suitable for anything else. The antis have shed crocodile tears over what will happen to them when, as they intend, the use of tobacco in all its forms disappears.

I don't know whether Brownlee was a smoker whose criticisms of the surgeon general's report were partially motivated, perhaps unconsciously, by a wish to defend the habit, or whether he was an entirely disinterested observer whose sole concern was to uphold the standards of the statistics profession. I believe it was the latter. In any event, there is no point in quoting further from his paper, nor even in further examination of the report itself. The questions Brownlee raised were either ignored or were soon forgotten in the epidemiological frenzy against smoking that followed after 1964. Indeed, the seven studies on which the report was based are no longer of any interest to anyone. Few scientists today, including epidemiologists, could even tell you what they were.

THIS IS ALL VERY well, you may say. But even if the original seven studies weren't absolutely conclusive, there have been gazillions of new studies since then that have confirmed their findings. Haven't there?

That's what I used to think. Now we come to the fourth surprising thing I learned about the 1964 report, perhaps the most significant thing of all—a major shortcoming in the evidence on which the advisory committee based its conclusions about cigarette smoking, a shortcoming which most people have never heard about. It may in fact lend additional validity to Brownlee's theory about the generation born in 1900, a cohort that grew up smoking strong, unfiltered cigarettes. For this I quote from the encyclopedic and much-acclaimed history of the

tobacco industry, *Ashes to Ashes*, by Richard Kluger, who is by no means a partisan of smoking:

> [T]he careful, temperate, and comprehensive report of the Surgeon General's panel had but one glaring fault—an omission that was never admitted or subsequently corrected . . . The elevated mortality rates of smokers, so neatly corresponding to dose-response measurements, were entirely or largely based upon the use of pre-filter cigarettes. During the five years prior to the report, the industry had reduced the tar and nicotine yields of the filter brands by an average of 40 percent; half the smoking population, moreover, was now using filter brands . . . Cigarettes with a filter that was more than cosmetic simply had not been on the market long enough for epidemiologists to conduct a meaningful population study—and the better part of another generation would be required before the mortality rates of those who smoked high-filtration brands exclusively could be calculated.

And here is the kicker (the emphasis is mine):

> *In fact, such a study has never been made in the three generations* [sic] *since the original report to the Surgeon General, doubtless because the public-health community has long since considered the case against cigarettes conclusively proven.*[22]

That statement is puzzling to me, for there have been plenty of studies since 1964 and they must have involved filtered cigarettes because that is the kind most smokers have been smoking for the past three decades. (One such study is cited in Chapter 3.) Kluger's central point remains valid, however: the surgeon general's report was based on outdated studies involving a product no longer universally used. Combine this "glaring fault" with the advisory committee's own reservations that I listed above about those very studies and there would seem to be very good grounds for stating that the case against cigarettes was anything but "conclusively proven" in 1964.

Why didn't the tobacco industry jump all over this "glaring fault"? Well, it did protest meekly, Kluger records, but "a more vigorous denunciation would likely have been taken as the companies' tacit acceptance of the [advisory committee's] findings with regards to unfiltered cigarettes, as well as an undemonstrable health claim for the filter brands that might have invited the Federal Trade Commission's wrath."

I doubt if the cigarette companies were particularly worried about

the FTC. After all, they'd been making thinly disguised health claims about their product for years, even using models dressed like doctors in their advertisements, and had often been slapped for it.

Kluger is right about the first point, though. What really made the cigarette manufacturers hold their tongues was that the slightest hint of an admission that there just might possibly be health hazards associated with smoking cigarettes, even if only with pre-1960s unfiltered ones, would have laid them open to even more lawsuits than they were to be faced with in the coming years. The same with any acknowledgment that they were working on developing less harmful cigarettes. They may also have simply hoped that the surgeon general's report would fade from the public's consciousness in time. They could not have foreseen, any more than anybody else could have foreseen in 1964, the antismoking firestorm to come.

In any event, for all the vaunted power and influence of the tobacco industry, its behavior in the years following 1964 was to be characterized by high ineptitude and low courage. The consequence is that nothing the industry says in its own defense, even if true, is believed. The consequence is that anything anybody else says in defense of smoking is taken as proof that he is "in the pay of Big Tobacco."

Yet how else could it have been? Had the industry been open and aboveboard in 1964 and conceded that the surgeon general's report had validity, the ever-hungry vultures among us—the product-liability lawyers—would have had a field day. To paraphrase the wisecrack, no good deed by the tobacco industry would have gone unpunished.

Even if the surgeon general had held out a hand to the industry, inviting its cooperation and participation in further investigations into the harmful effects of smoking and how they might be ameliorated—which he did not do—and suggesting that the companies be offered some protection from retaliation for that cooperation—which of course he would have had no authority to do—a litigious society would not have countenanced it for an instant.

But there was to be no cooperation with the merchants of disease and death, much less any kind of "compromise." The surgeon general was not interested in promoting healthy, or even less dangerous, smoking. War had been declared, and in the coming years each retreat by the industry would only enourage demands by the emergent antismoking movement for ever more forceful onslaughts against smoking.

That the industry was indeed aware of possible health hazards associated with smoking—was in fact even *more* aware than was the surgeon general's advisory committee when the committee began its investigation—seems pretty well demonstrated in *The Cigarette Papers*, published by Stanton Glantz and colleagues at the University of California, a compilation of documents stolen from the files of the Brown & Williamson Tobacco Company and its parent, the British American Tobacco Company (BAT).[23]

However, far from proving the reprehensibility of the industry for allegedly hiding what it knew because it didn't give a damn what its products might do to people, these documents show that B&W at least was deeply concerned about these hazards and that it expended considerable efforts in trying to come up with a cigarette that minimized them. Unfortunately, such efforts were unsuccessful. (To my mind, the most amazing "revelation" in *The Cigarette Papers* is how impressed the company was by those essentially meaningless experiments in which tobacco "tar" was painted on the skins of mice and how much it worried about them.)

But even if the tobacco companies did suppress (or, if you will, lie about) what they knew about smoking and its health dangers, as a legion of industry whistleblowers has claimed, for people to be angry about that is to suggest that they believed the companies all these years and did *not* believe the surgeon general. That is patently not true. (Except, of course, for those people who were to file liability suits based on the premise that they didn't know that smoking was bad for their health because the cigarette makers never told them it was and thus, by implication, told them it wasn't.)

For people to be angry that the cigarette companies lied to them is further to suggest that even though Americans have known the "official" truth about smoking since 1964, they *couldn't know for absolutely sure* because the industry had never confirmed it. That was part of the motivation behind the disgusting inquisition of seven tobacco CEOs by Rep. Henry Waxman in 1994 (see Chapter 12)—to put them under oath in hopes of forcing them to admit what everyone had known since 1964, and when they refused to admit it, to accuse them of perjury. That is the reason for the antismokers' feeling of smug satisfaction when a plaintiffs' attorney in the 1997 flight attendants' suit in Florida cleverly led one industry executive to concede that sec-

ondhand smoke "might have" caused thousands of lung cancer deaths (see Chapter 3).

Americans really must place great trust in and have great respect for the scientific knowledgeability of the tobacco industry after all.

AT THAT HISTORIC meeting on July 24, 1962 between Surgeon General Terry and the representatives of the various private health organizations and government agencies, it was agreed upon that the investigation into smoking and health would be undertaken in two consecutive phases:

"Phase I—An objective assessment of the nature and magnitude of the health hazard, to be made by an expert scientific advisory committee which would review critically all available data but *would not conduct original research* [emphasis added]. This committee would produce and submit to the Surgeon General a technical report containing evaluations and conclusions."

(Phase I resulted in, of course, *The Report.*)

"Phase II—Recommendations for actions were not to be a part of the Phase I committee's responsibility. No decisions on how Phase II would be conducted were to be made until the Phase I report was available. It was recognized that different competencies would be needed in the second phase and that many possible recommendations for action would extend beyond the health field and into the purview and competence of other Federal agencies."

What this tells me is that even before Phase I (the investigation) was initiated and even longer before its conclusion (the report) was available, the surgeon general already envisioned the necessity of "action" against smoking. This further fortifies my assertion that the condemnation of smoking was foreordained.

That conclusion, the most significant and oft-quoted statement in the entire report, was:

"Cigarette smoking is a health hazard of sufficient importance in the United States to warrant appropriate remedial action."

The Phase II strictures notwithstanding, what else was this but a "recommendation for action" against smoking of the most sweeping nature? And what "action," and "actions," this simple statement has inspired, actions that have indeed gone "beyond the health field and

into the purview and competence of other Federal agencies"! Americans have been "remediated" up the wazoo.

I wonder . . .

Had the members of the advisory committee been able to foresee what was to flow from their work—the ostracizing of smokers from respectable society; the loss of or denial of employment to smokers (Chapter 8); the general hysteria over secondhand smoke with its consequent bans on smoking in workplaces and other public places (Chapters 6, 7 and 8); an antismoking lobby feeding on money extorted from smokers in the form of higher and higher taxes and fomenting discord, acrimony and fear among Americans; the denial of child custodial rights to smokers (Chapter 7), the corruption of science and the manufacture of statistics in the cause of a "smoke-free" society (almost all the chapters but especially Chapter 6); in short, the whole sorry mess—I wonder, had they been able to foresee all this, would they have approved of it? Or might they have been appalled and said to themselves, *"My God! What are we starting here?"* Might they then have recommended something different? Might they have said something like:

"We believe that we have established with reasonable certainty the existence of a number of possible health risks associated with smoking, that of lung cancer and cigarette smoking in particular. Although further confirmatory studies in this area are needed and encouraged, we believe that cigarette smoking is an overall health hazard of sufficient magnitude in the United States that the public should be made aware of it. Having accomplished that, our task is finished. Further actions, if any, lie within the realm of the individual responsibility of the ordinary citizens of this country. Those who smoke, as well as those who may be thinking of taking up smoking, must now decide for themselves whether the perceived benefits they derive from the use of tobacco outweigh what, in our best scientific judgment, are risks to their well-being and their very lives. Those decisions, however, are each individual's, and his alone, to make."

But they couldn't foresee, and of course weren't about to suggest that the product of their 13 months' labor was something people should merely think about, much less admit that Terry's "explosive" report was, in epidemiological terms, actually more like a firecracker in a barrel than a real bomb. So they called for "appropriate remedial action"—by "different competencies . . . and other Federal agencies."

That ever-so-innocent-sounding phrase, "appropriate remedial action," echoes through American society today with all the sinister overtones of some kind of "final solution."

THE SURGEON GENERAL'S 1964 report taught us more than about smoking and health. In fact, in retrospect, that is probably the least important thing it taught us. The most important thing it taught us is that our bodies do not belong to ourselves but to society at large and thus that the healthy maintenance of those bodies falls under the rightful purview of the governing authority of that society—the state and its "competency" (i.e., power). If we don't have enough sense to do the right thing healthwise for ourselves after we have been thoroughly warned about the consequences of our personal behavior, the state will take whatever "appropriate" measures it deems necessary to do it for us. After all, it's for the general welfare as well as our own. (Ask not what smoking does to you. Ask what it does to your country.)

This is a disturbing echo of the concept of *Pflicht zur Gesundheit* — "the duty to be healthy"—of Hitler and his National Socialists.[24]

The surgeon general's 1964 report also created lifetime employment opportunities for many people. As famed epidemiologist Ernst Wynder, whose work was cited 21 times in Chapter 9 alone in the report, recently wrote: "[I]f, in the late 1940s, my idea had been to study the relevance of electromagnetic fields to brain cancer rather than whether cigarette smoking caused lung cancer, my career might well have taken a very different turn."[24]

And if, in the mid-1960s, the surgeon general's report had not been written, American society also might well have take a very different and, I believe, a far happier, turn in the years that followed.

Truly can it be said that seldom in the annals of scientific endeavor has a single field of research been so rewarding to so many for so long as that devoted to finding out more and more bad things about tobacco and smoking.

And as another legacy of the surgeon general's 1964 report, sadly can it be said that because of the present atmosphere of political correctness in which this field of science operates, the few dissident scientists who dare to question some of those findings do so at risk, at best, of being accused of shilling for the tobacco industry and, at worst, of placing their professional reputations, if not their careers, in jeopardy.

In the chapters that follow I will attempt to give some small voice to the conscientious objectors in the crusade against smoking and will refer again to the 1964 Report of the Advisory Committee to the Surgeon General of the Public Health Service, which, as if it needed to be repeated, started the whole sorry mess.

Notes

1. *Smoking and Health*. Report of the Advisory Committee to the Surgeon General of the Public Health Service, U.S. Department of Health, Education and Welfare, Public Health Service Publication No. 1103, p. 33.

2. G. V. Yule, "The function of the scientific method in scientific investigation." Industrial Fatigue Research Board Report, 1924. Quoted in J. O. Irwin, "The place of mathematics in medical and biological statistics." *Journal of the Royal Statistical Society*, Series A, 1963. Requoted in Brownlee, p. 733 (see Note 18 infra). Yule was an early 20th-century pioneer in the science of statistics.

3. Gene Borio, "Tobacco Timeline." At www.tobacco.org/History/Tobacco_History.html#aal.

4. "History of the 1964 Surgeon General's Report." Office on Smoking and Health, National Center for Chronic Disease Prevention and Health Promotion, Centers for Disease Control and Prevention (CDC). Posted on the CDC's Tobacco Information & Prevention Sourcepage at www.cdc.gov/needphp/osh/30yrsgen.htm.

5. Quoted by CNN correspondent Dan Ronan in "Tobacco war smoldered for decades." Posted on the World Wide Web June 20, 1997.

6. At www.lcolby.com/.

7. "Abortion and Possible Risk for Breast Cancer: Analysis and Inconsistencies," National Cancer Institute CancerNet. From University of Pennsylvania Internet page at www.oncolink.upenn.edu/pdq600342.html.

8. C. Harcourt Kitchen, *You* MAY *Smoke* (New York: Award Books, 1966), p. 23.

9. Q&A in "Walter Scott's Personality Parade." *Parade*, October 27, 1996, p. 2.

10. *Smoking and Health*, p. 7.

11. "History of the 1964 Surgeon General's Report."

12. *Smoking and Health*, p. 7.

13. Loc. cit.

14. Elizabeth Brenner Drew, "The Quiet Victory of the Cigarette Lobby: How It Found the Best Filter Yet—Congress." *The Atlantic*, September 1965. From "The Atlantic Unbound" at www.theatlantic.com.

15. *Smoking and Health*, p. 17.

16. ATSDR (Agency for Toxic Substances and Disease Registry**)** of the Centers for Disease Control and Prevention, Public Health Statement, May 1990. At www.atsdr.cdc.gov:8080/ToxProfiles/phs8805.html.

17. Joe Dawson, "Essays on the Anti-Smoking Movement." At www.tezcat. com/smokers/issues1.html.

18. K. A. Brownlee, "A Review of 'Smoking and Health.'" *Journal of the American Statistical Association*, 60, 1964, pp. 722-739.

19. Kitchin, p. 61.

20. Lauren A. Colby, *In Defense of Smokers*. Self-published 1995, p. 42.

21. Foster Fitz-Simons, *Bright Leaf* (New York: Rinehart & Co., 1948).

22. Richard Kluger, *Ashes to Ashes*: *America's Hundred-Year Cigarette War, the Public Health, and the Unabashed Triumph of Philip Morris* (New York: Alfred A. Knopf, 1996), pp. 260-261.

23. Stanton A. Glantz et al., *The Cigarette Papers* (Berkely: University of California Press, 1996).

24. Robert Proctor, *Racial Hygiene: Medicine Under the Nazis* (Harvard University Press, 1998), p. 248. Cited in Sean Gabb, "Smoking and Its Enemies: A Short History of 500 Years of the Use and Prohibition of Tobacco." From FORCES Canada at www.forces-cdn.com/forest01.htm.

25. Ernst Wynder, *American Journal of Epidemiol*ogy 1996; 143;747-749. Quoted by Steven Milloy on his "Junk Science" page at www.junkscience. com/news/wynder.html.