20 April 1979 • Vol. 204 • No. 4390 $1.50

SCIENCE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

ISSN 0036-8075
20 April 1979
Volume 204, No. 4390

SCIENCE

SCIENCE is published weekly on Friday, except the last week in December, by the American Association for the Advancement of Science, 1515 Massachusetts Avenue, NW, Washington, D.C. 20005. Second-class postage (publication No. 484460) paid at Washington, D.C., and at an additional entry. Now combined with **The Scientific Monthly®**. Domestic individual membership and subscription (51 issues): $34. Domestic institutional subscription (51 issues): $70. Foreign postage extra: Canada $12, other (surface mail) $15, air-surface via Amsterdam $40. First class, airmail, school-year, and student rates on request. Single copies $1.50 ($2 by mail); back issues $2.50 ($3 by mail); classroom rates on request. **Change of address:** allow 6 weeks, giving old and new addresses and seven-digit account number. **Postmaster:** Send Form 3579 to *Science*, 1515 Massachusetts Avenue, NW, Washington, D.C. 20005. *Science* is indexed in the *Reader's Guide to Periodical Literature* and in several specialized indexes.

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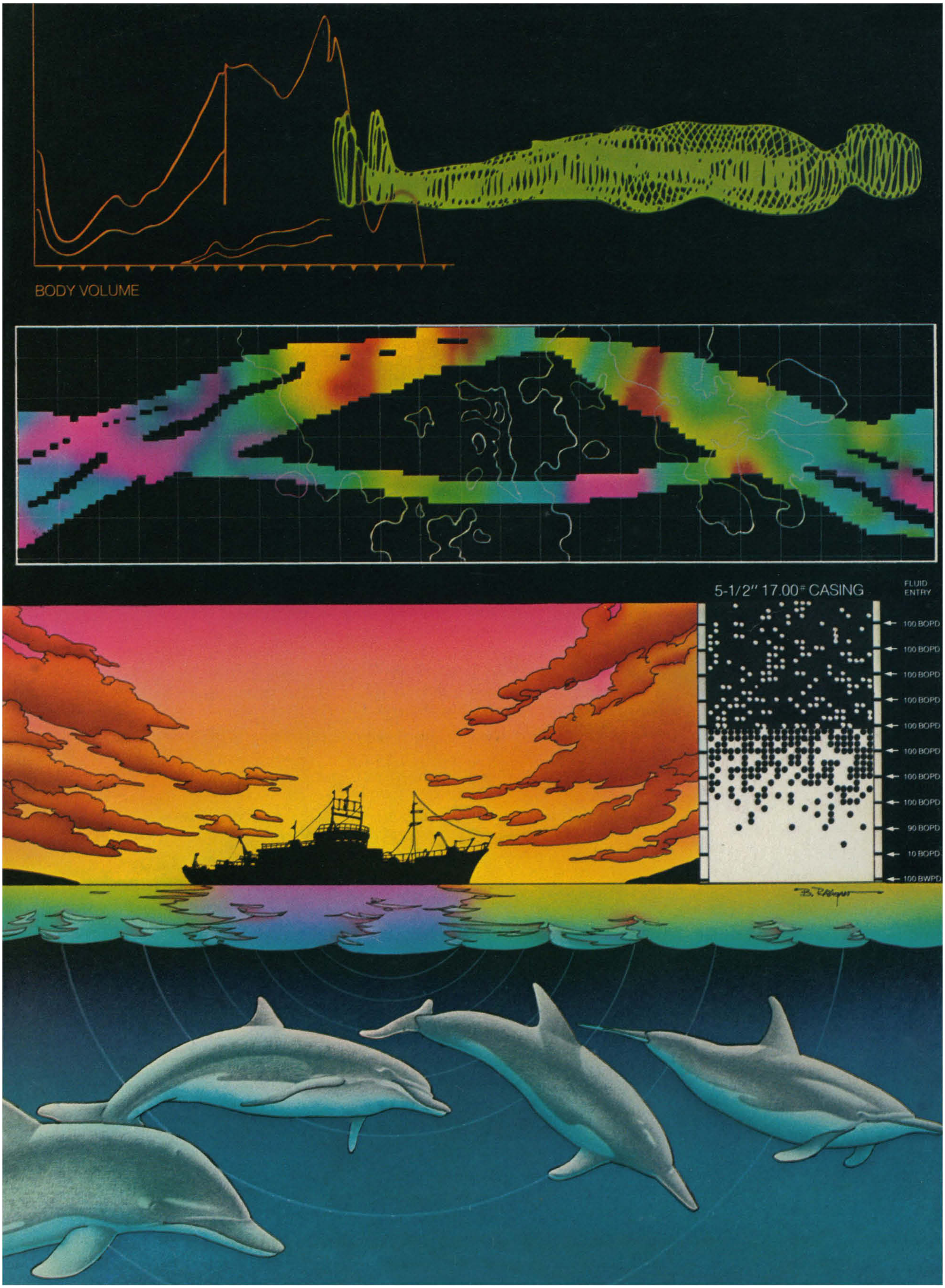
REPORTS

COVER

St. Mark's Square, Venice, Italy. Salinity and tide heights are used to describe the exchange of material in a simulated portion of the Venetian lagoon and to predict changes that occur after entrance size reduction. See page 261. [Lesley A. Marvel, *Science*]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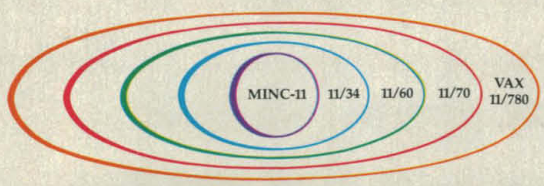

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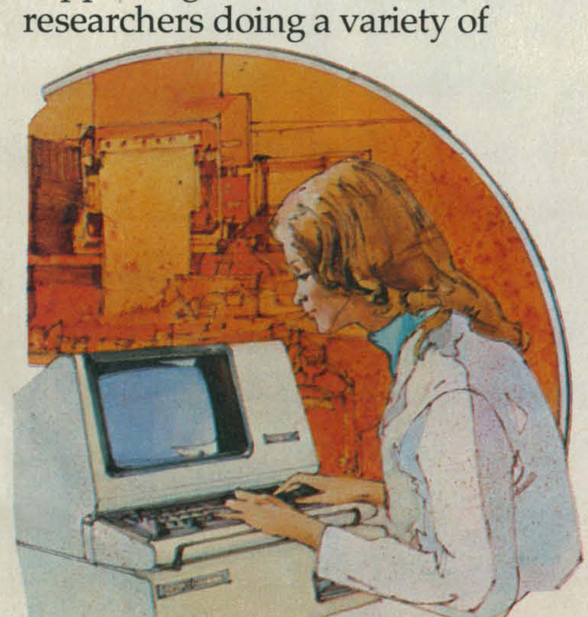

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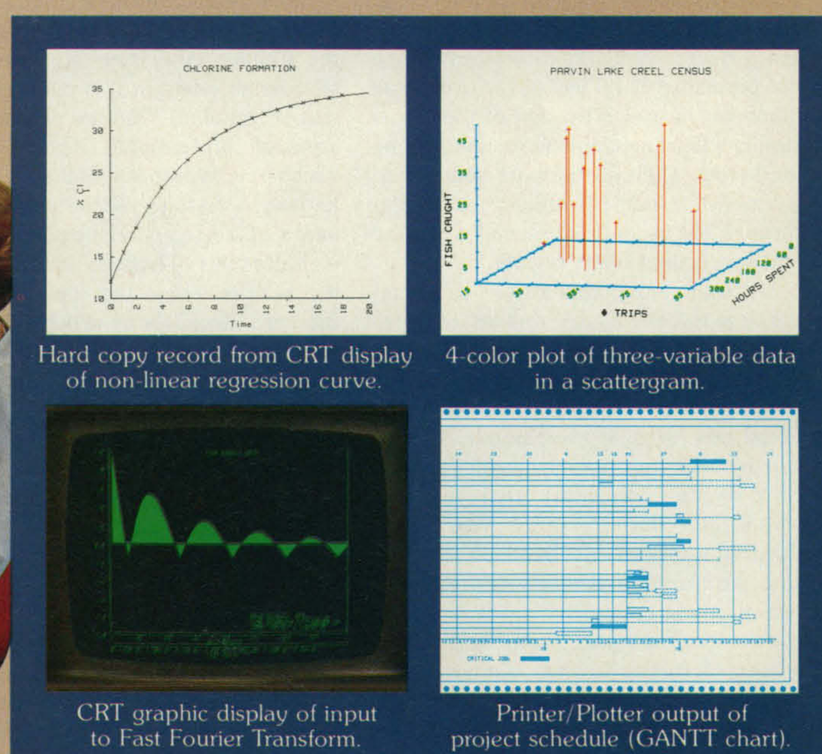

Hard copy record from CRT display of non-linear regression curve.

4-color plot of three-variable data in a scattergram.

CRT graphic display of input to Fast Fourier Transform.

Printer/Plotter output of project schedule (GANTT chart).

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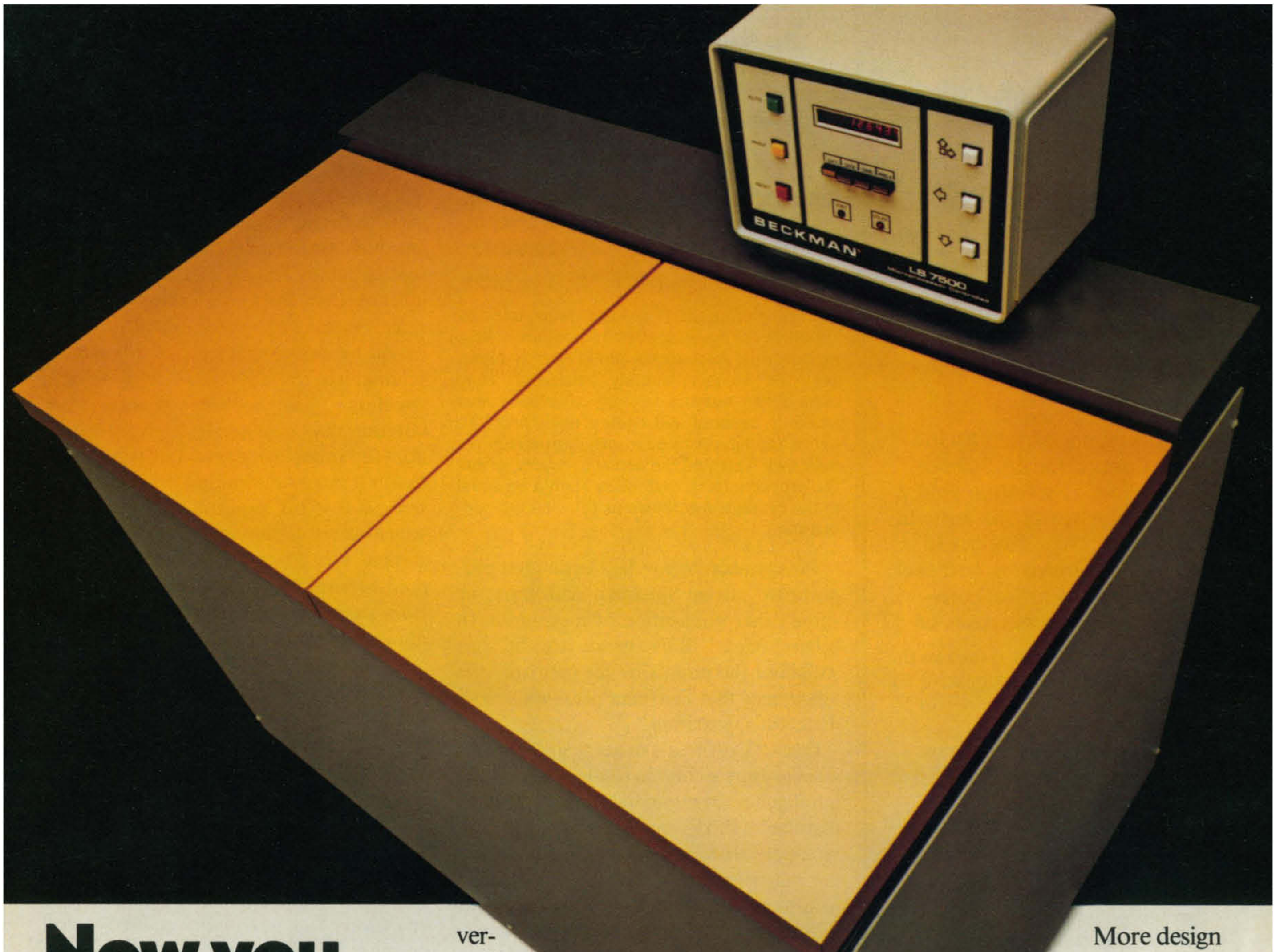

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the row totals (the proportions in each class) agree perfectly with 1926 data of Spielman and Burt, saying "the coincidence is bizarre indeed." Dorfman's contention that Burt described his row totals as "simply totals per mille" is simply wrong, and his conclusion that the agreement is "bizarre" is uncalled for. What Burt in fact wrote was (*1*, p. 10):

> In constructing the tables the frequencies inserted in the various rows and columns were proportional frequencies and in no way represent the number actually examined: from class I the number actually examined was nearer a hundred and twenty than three. To obtain the figures to be inserted (numbers per mille) we weighted the actual numbers so that the proportions in each class should be equal to the estimated proportions for the total population.

Presumably he got the "estimated proportions" from Spielman and Burt, or some other publication of these data. In other words, Burt is *saying* he has weighted the counts to get precisely the agreement that Dorfman presents as evidence of fabrication.

One of Dorfman's major arguments relates to Burt's column totals, his grouped intelligence assessments for all classes together. Dorfman demonstrates convincingly that Burt's column totals fit a normal distribution exactly, if rounding is allowed for. What does Burt say about that? Immediately after the above-quoted passage, Burt wrote (*1*, p. 10):

> Finally, for purposes of the present analysis we have rescaled our assessments of intelligence so that the mean of the whole group is 100 and the standard deviation 15. This is done because the results of so many intelligence tests nowadays are expressed in terms of conventional I.Q.'s conforming to these requirements.

The question is, what did Burt mean by "rescaled"? For if he meant that he followed the by no means unknown practice of "curving" the scores to fit a normal curve (with mean 100 and standard deviation 15), either by transforming his pooled (over classes) IQ's individually or by reweighting his columns (as he did his rows) to fit "estimated proportions," then Dorfman's case collapses. It is clear that for his purposes Burt would need the father scores and son scores to be comparable. It seems plausible that if the raw data (which were gathered over a half century under widely varying conditions) were in fact "crude" (and thus possibly skew or otherwise markedly nonnormal) and the assessments of adult intelligence "less thorough and reliable" (*1*, p. 9) (and hence possibly more variable), then Burt would rescale the marginal totals to agree with one another so that he could make direct comparisons of within-row variability. Contrary to Dorfman's implications, Burt did believe IQ's were approximately (though not exactly) normal (*2*), and in a "pilot study" he might well choose to rescale using a normal distribution (*3*). In fact, Dorfman's table 7 could be interpreted as showing just how Burt might have proceeded in scaling tables III and IV. It may be significant that Burt used the word "rescale" instead of "standardized," which would be more suggestive of a linear rescaling. (In psychometric terminology, "scaling" has a more general connotation than a simple linear transformation, although linear rescaling is a special case.) It must be admitted that Burt's description of his procedure is extremely vague—one cannot even determine his sample size from his description. Dorfman reports that the consensus is that Burt's sample size was 40,000 pairs. This number must be an error (although not one that originated with Dorfman), based on Burt's statement that for class I the number examined was "nearer a hundred and twenty than three," a 40 to 1 ratio. But he reweighted different classes with different weights and probably chose an extreme ratio to emphasize disparity. Burt made no further statement about the actual number of pairs, but it may have even been less than 1000. One can easily see how many readers could be misled into believing the counts were frequencies, but Dorfman does Burt and the readers of *Science* a great disservice by not even mentioning a reasonable alternative explanation that does not involve either fabrication or fraud.

Another of Dorfman's major errors involves his calculation of regression coefficients. In table 3 he calculates $\bar{X}_c/(\bar{X}_f + 100)$ for each class, where $\bar{X}_c$ is the mean IQ for children and $\bar{X}_f$ is the mean IQ for fathers, and seems surprised at the coincidence that all answers are (to two decimal places) 0.50. He has used the wrong formula. (I thank David L. Wallace for pointing this out to me.) If we really wish to estimate the regression coefficients based on these limited data, we should presumably calculate $(\bar{X}_c - 100)/(\bar{X}_f - 100)$, which gives (to two decimal places) 0.52, 0.48, 0.49, 0.56, 0.50, and 0.49. Dorfman's formula is nonsensical; it adds 100 to the numerator and 200 to the denominator, which biases the results toward 0.50, regardless of the actual data.

As a final point, I note that Dorfman's logic is seriously at fault in his "conclusions." He writes, "These findings show, beyond any reasonable doubt, that Burt fixed the row and column totals of the tables in his highly acclaimed 1961 study. Since the totals are completely determined by the cell entries, Burt determined the cell entries." I have argued that Burt announced that he had weighted the rows to get predetermined row totals and hinted that he had done the same for the columns. But this does not at all imply that he fabricated the individual entries. If a two-way table is reweighted along rows and columns as Burt appears to have done, the individual entries will still be estimates of rates per 1000 for the corresponding cells of the bivariate frequency distribution for the entire population, and it is no abuse of statistical terminology to still refer to the table as "data," as Burt did. The entries will no longer be frequency counts, but, as Burt announced, the cell entries "in no way represent the number actually examined" (*1*, p. 10). In fact, if Burt really did fabricate his data, he chose an extremely difficult way to do it. He would have had to *first* invent a two-way table and *then* rescale his original table to get the predetermined marginal totals. It would be far easier to merely invent a table and skip the rescaling, or at least not bother with a precise rescaling. Contrary to Dorfman's implication, it is not a simple matter to fill in either a 6 × 10 or a 6 × 6 table with predetermined marginal totals and get a plausible correlational pattern. A careful inspection of Burt's tables will reassure the reader that they cannot be perfectly fitted by a bivariate normal distribution. For example, in row VI of table I, 45 percent of the scores fall between 80 and 90, suggesting a standard deviation of less than 8.5. Since the mean score in this class is given as 84.9, no more than 0.2 percent of the scores should exceed 110, whereas we are told that 9/261 or 3.4 percent did so.

I do not wish to be interpreted as endorsing either Burt's statistical procedure or his unclear explanation of what he did (and his refusal to present the raw data), but given the standards of Burt's time (*4*) and his repeated disclaimers (it was "merely a pilot inquiry," "data are too crude and limited") (*1*, p. 9), the charges of fabrication or fraud seem, at least in this instance, to be without foundation; the evidence presented is irrelevant to the case.

STEPHEN M. STIGLER
Center for Advanced Study in the Behavioral Sciences, Stanford, California 94305

References and Notes

1. C. Burt, *Br. J. Stat. Psychol.* **14**, 3 (1961).
2. ——, *ibid.* **13**, 98 (1943); *Mental and Scholastic Tests* (King, London, 1922), p. 162.
3. See, for example, J. P. Guilford, *Psychometric Methods* (McGraw-Hill, New York, ed. 2, 1954), pp. 345–346.
4. Burt's paper was published in 1961, but his statistical training was rooted in the 1920's and 1930's, when the normal distribution was considered to be a more "normal" (≡ usual) state than is now the case and was used much more freely as a basis for test scaling. The only statistical reference in Burt's 1961 paper is the 1934 edition of R. A. Fisher's *Statistical Methods for Research Workers*, an excellent work but one that lays considerable stress upon procedures based upon the normal distribution.

I am distressed by the application of statistics in "The Cyril Burt question: new findings" by D. D. Dorfman. Although I have no intention of defending Burt, I think that Dorfman's analyses provide no evidence for his claim that "the eminent Briton is shown, beyond reasonable doubt, to have fabricated data on IQ and social class". . . .

Fixed margins of a table do not "determine" cell entries as Dorfman supposes, even in a 2 × 2 table. Furthermore, fixing margins is a useful and accepted tool in many statistical problems (*1*). . . .

It is likely that Burt (a) transformed the IQ data to follow a normal distribution with mean 100 and standard deviation 15 for both parents and children, (b) fixed the class margins in his tables I to IV at census data, but (c) did not otherwise alter the data. Burt's descriptions of his tables (*2*, pp. 10, 12, 15) imply (a) and (c) and clearly state (b). Also, (a), (b), and (c) are consistent with (i) the excellent but imperfect fit to normality displayed in the IQ margins of tables I to IV, (ii) the slight differences between parents' IQ margins and children's IQ margins in the tables, and (iii) the slight differences between the IQ margin and the class margin in table III as well as in table IV. The hypothesis that Burt fixed both row and column margins at population proportions is inconsistent with (i), (ii), (iii), and his description of the tables (*2*, pp. 12, 15).

Although Dorfman's statistics do not provide any evidence that Burt fabricated data, there may be such evidence in Burt's tables. Assuming that Burt first normed IQ and then fixed the class margins, his tables I and II present the same data as his tables III and IV but with different boundaries for the IQ categories. By combining the information from tables I and III (parents' data) and from tables II and IV (children's data), it is possible to calculate frequencies in narrower IQ categories. (For example, the frequency 86 for parents having IQ's between 100 and 103 is found by subtracting the sum of frequencies in IQ categories 50–100 in Burt's table I from the sum in IQ categories 50–103 in his table III.) The results are shown here in Table I.

Burt's ambiguous labeling of categories makes it difficult to compare pre-

tional classes it is in my view desirable to examine, not only (as is usually done) the class-means, but the entire frequency distributions. Accordingly, in Tables I and II, I give *frequencies* [my italics] both for adults and for children." Thus Burt did not call those numbers "weighted" counts. He called them "frequencies," and he called the distributions of numbers "frequency distributions." In fact, Burt never called those computed numbers "weighted" or adjusted counts anywhere in his paper. Even in the only passage Stigler selected to support his interpretation, Burt referred to those numbers as "proportional frequencies" (*12*, p. 10), not "weighted" or adjusted counts.

Moreover, contrary to Stigler's statement, Burt's use of Spielman and Burt's percentages as the row totals of his tables is quite bizarre. Spielman and Burt characterized their percentages as "nothing more than the roughest approximation" (*13*, p. 15). They were "based mainly upon Charles Booth's survey" (*14*, p. 349) of London in the late 19th century (*15*) combined in an unstated way with some unspecified pre-1926 London census figures for employed male adults aged 14 plus, with or without children and married or unmarried. Burt's purported sample for the 1961 paper was described as a sample of father-son pairs having an average age "difference of 28.4 years" (*12*, p. 16) from an anonymous "London borough selected as typical of the whole county" (*12*, p. 9) for "nearly fifty years, namely, from 1913 onwards" (*12*, p. 4). Thus the Spielman and Burt percentages have a nonsensical relation to Burt's sample and population of interest. Yet Stigler sees nothing bizarre in Burt's using row totals whose source is never given, whose quality is never described, and whose relevance to the problem is never discussed.

Stigler next tries to justify the essentially perfect fit of Burt's column totals to a normal curve. In introducing his tables I and II, Burt says only that "we have rescaled our assessments of intelligence so that the mean of the whole group is 100 and the standard deviation 15" (*12*, p. 10). Stigler argues that Burt "hinted" that he had rescaled to a normal curve with mean 100 and standard deviation 15, but doesn't explain why Burt didn't simply tell the reader that that was what he had done. To support his conjecture, Stigler says that the normal distribution was then "used much more freely as a basis for test scaling." His statement is incorrect for IQ test scores. Indeed, Burt would have strongly disagreed with Stigler's unsupported assertion. In 1957 Burt wrote a paper specifically devoted to the distribution of intelligence, entitled "The distribution of intelligence" (*16*). In that article Burt states (*16*, p. 174), "Except for those intended solely for a single age-group, most [intelligence] tests are scaled by methods independent of any pre-supposition about the resulting distribution: e.g. (i) by mental age, (ii) by least perceptible or equal-appearing intervals, or (iii) by the number of unit processes performed." Since the scaling did *not* determine the distribution, Burt then argued that the observed frequency distributions of IQ test scores agreed with his predictions from the genetic theory. He concluded his discussion with the following: "With each of these procedures [mental age, equal-appearing intervals, number of unit processes performed] the frequency curves obtained yield, in nearly every case, statistical constants implying a definite though slight degree of leptokurtosis and negative asymmetry. Thus, the results are fully consistent with the twofold hypothesis suggested by genetic arguments and entirely incompatible with the other three hypotheses" (*16*, p. 174). As further evidence that Burt did not view scaling IQ's to a normal curve as a common device, Burt actually raised suspicions about the frequency distributions published by E. L. Thorndike, an eminent leader in educational measurement. Burt expressed his suspicions in 1963 in the *British Journal of Statistical Psychology*, 2 years after he published his almost perfect normal curves in that journal. Burt declared (*17*, p. 176), "On applying the chi-squared test to his [Thorndike's] own measurements he reaches values for *P* ranging from 0.99 to 0.999,999. Now we used to be warned that 'a value of *P* very near to unity should lead the investigator to suspect his hypothesis quite as much as very small values: such very close correspondences are too good to be true' " (*18*). If Burt had considered scaling to a normal curve to be a common device for IQ scores, he surely would not have questioned the close correspondence of Thorndike's data to the normal curve.

Furthermore, Stigler limits his discussion to the question "what did Burt mean by 'rescaled'?" but neglects to mention that Burt did not use the term "rescale" in his introduction to tables III and IV. In introducing table III, Burt said (*12*, p. 12): "If we now *reclassify* [my italics] the actual data for adults according to these new borderlines, we obtain the distribution set out in Table III."

In introducing table IV, Burt said (*12*, p. 15): "Table IV shows the distribution of the children with the scale for intelligence *subdivided* afresh [my italics]." The terms "reclassify" and "subdivide afresh" do not mean "rescale to a normal curve," whereas they are completely consistent with my representation of those tables.

There is another problem with Stigler's explanation of the almost perfect fit of the column totals to the normal curve. He proposes that Burt constructed his tables in one of two ways. He either (i) transformed "his pooled (over [occupational] classes) IQ's individually" to a normal curve and then weighted by the Spielman and Burt proportions or (ii) weighted "his columns (as he did his rows) to fit 'estimated proportions.'" Rubin discusses these two possible explanations, and gives excellent arguments against the second one, which is the one that Stigler prefers. Rubin points out that "the hypothesis that Burt fixed both row and column margins at population proportions is inconsistent with [Rubin's properties] (i), (ii), (iii)." It is also quite inconsistent with the fact that the mean IQ's for the occupational classes given in the last column of Burt's tables I and II disagree in eight of nine possible comparisons with the arithmetic means computed from those tables using the midpoints of the intervals. If alternative two is true, they should be identical in every case. Rubin also points out that alternative two is inconsistent with Burt's "description of the tables." Rubin cites the pages where Burt used "reclassify" and "subdivide afresh," which do not mean "reweight along rows and columns." On the basis of Rubin's excellent arguments, we may conclude that Stigler's alternative two would be a significant misrepresentation of Burt, and Stigler would surely not want to misrepresent Burt. Hence, we only need to evaluate Stigler's alternative one, which is also Rubin's proposal.

I will now show that alternative one won't work. Let us hypothetically construct Burt's four tables using alternative one. We begin with a sample of individual IQ's of fathers and their sons, and then transform each set of scores separately to fit a normal curve (*19*). After this transformation, we must construct a table of sample frequencies for each of Burt's four tables of published numbers. To weight along rows by the Spielman and Burt proportions, we first need to convert the hypothetical tables of sample frequencies to tables of relative frequency distributions by dividing each cell frequency f_{ij} (i^{th} row, j^{th} column) by $N_{i\cdot}$, the sample size for the i^{th} row. To fix the row proportions to the Spielman and Burt proportions ($P^*_{i\cdot}$, $1 \le i \le 6$), we now multiply each $f_{ij}/N_{i\cdot}$ by $P^*_{i\cdot}$. The row totals after this weighting are

$$\sum_{j=1}^{n} (f_{ij}/N_{i\cdot})\, P^*_{i\cdot} = P^*_{i\cdot} \qquad 1 \le i \le 6$$

where n is the number of columns. Burt's published numbers are in per mille, so that $N^*_{i\cdot} = 1000\, P^*_{i\cdot}$, where the $N^*_{i\cdot}$ $(1 \le i \le 6)$ are Burt's published row totals for the table of interest. Alternative one assumes that column totals are not changed by the weighting along rows, so that

$$\sum_{i=1}^{6} \frac{f_{ij}}{N_{i\cdot}} P^*_{i\cdot} = \sum_{i=1}^{6} \frac{f_{ij}}{N_i} \frac{N_{i\cdot}}{N} \qquad 1 \le j \le n \qquad (1)$$

or equivalently that

$$\sum_{i=1}^{6} \frac{f_{ij}}{N_{i\cdot}} \left(P^*_{i\cdot} - \frac{N_{i\cdot}}{N} \right) = 0 \qquad i \le j \le n \qquad (2)$$

But the matrix $\{(f_{ij}/N_{i\cdot})N^*_{i\cdot},\ 1 \le i \le 6,\ 1 \le j \le n\}$ is Burt's published table by hypothesis. Moreover, each of Burt's published tables can be shown to have rank equal to the number of rows, so that the matrix $\{f_{ij}/N_{i\cdot},\ 1 \le i \le 6,\ 1 \le j \le n\}$ has rank equal to the number of rows. Hence the system of Eqs. 2 is only satisfied by the trivial solution, or equivalently

$$P^*_{i\cdot} = \frac{N_{i\cdot}}{N} \qquad 1 \le i \le 6$$

Thus, weighting the rows by the Spielman and Burt proportions would preserve the column totals if and only if Burt's actual sample proportions in each occupational class equal the nonsensical Spielman and Burt proportions. In brief, if Burt had rescaled the individual IQ's to fit a particular normal curve, he would have lost that normal curve by weighting the rows with proportions different from his actual sample proportions. One can also show that accidental fit to the normal curve would be highly unlikely by trying random vectors of weights in the neighborhood of $P^*_{i\cdot}$ $(1 \le i \le 6)$. In conclusion, Stigler's alternatives do not explain Burt's column totals.

Stigler next expresses concern about the sample size. He apparently believes that he is better able to interpret Burt's descriptions than Dobzhansky, who said "some 40,000" (*5*, p. 19); than Eysenck, a former student and colleague of Burt's, who said "some 40,000" (*7*, p. 62); than Gottesman, who said "some 40,000" (*8*, p. 36); than Herrnstein, who said "40,000" (*9*, p. 436); and Willerman, who said "40,000" (*11*, p. 11). According to Stigler, these authorities "must be" in error, since his second alternative—which Rubin and I have shown is inconsistent with Burt's descriptions and tables—implies otherwise. Stigler evidently sees nothing suspicious in Burt's failure to give the size of his purported sample. My own position is that Burt did not explicitly report the sample size because there was no sample.

Stigler and several others who wrote letters to *Science* are distressed with my calculation of $\bar{X}_{ic}/(\bar{X}_{if} + 100)$ for the occupational classes. They think that I incorrectly derived that formula from a statistical regression equation. In fact, I derived that formula directly from an equation that I believe is part of Burt's fabrication device. I was suggesting that Burt's fabrication gave the following simple relation between the means of the children and the means of the fathers:

$$\bar{X}_{ic} = \frac{1}{2}(\bar{X}_{if} + 100) \qquad (3)$$

where $\bar{X}_{ic}$ and $\bar{X}_{if}$ are Burt's published means of the children and the fathers respectively. I inferred this equation "from Conway's discussion" (*20*, p. 1179) in her (Burt's ?) article on social class published in 1959 (*21*). The equation predicts a regression of the children's means toward 100, and therefore I called the coefficient of 1/2 a "regression coefficient." Perhaps I should have called it a "fabrication coefficient." Rearrangement of Eq. 3 gives

$$\frac{\bar{X}_{ic}}{\bar{X}_{if} + 100} = \frac{1}{2}$$

which was verified for every class. It should be emphasized that I never called Eq. 3 a "statistical regression equation" anywhere in the *Science* article and I used the symbol α so that my proposed fabrication constant would not be confused with "β," the standard statistical regression coefficient.

If we subtract 100 from both sides of Eq. 3, we obtain

$$\bar{X}_{ic} - 100 = \frac{1}{2}(\bar{X}_{if} - 100) \qquad (4)$$

Rearrangement of Eq. 4 gives

$$\frac{\bar{X}_{ic} - 100}{\bar{X}_{if} - 100} = \frac{1}{2}$$

which is what Stigler tested. Equation 4 is fine as a statistical regression equation, but it would be a nonsensical proposal for a fabrication equation, since

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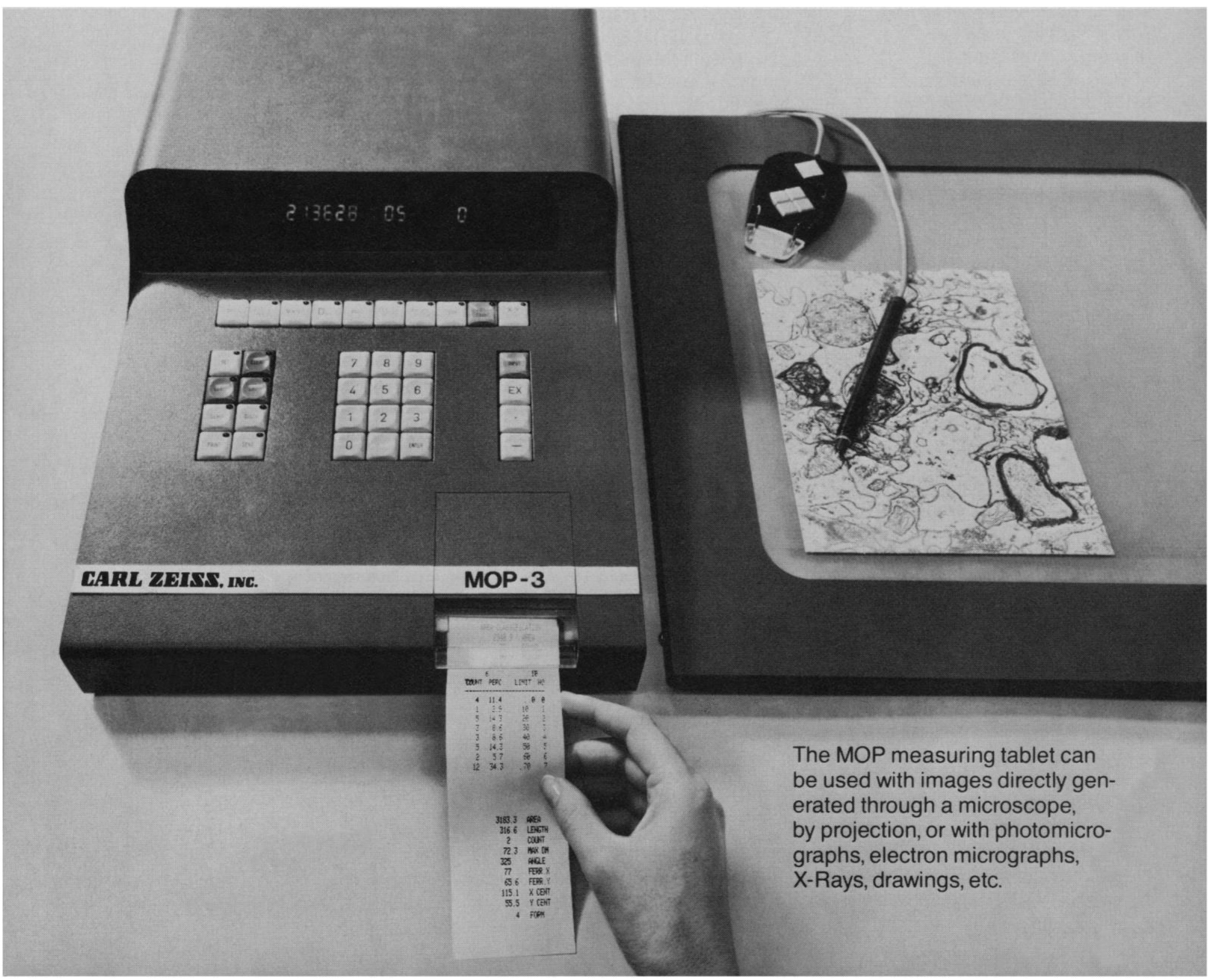

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20 April 1979, Volume 204, Number 4390

SCIENCE

Science Policy in Washington

The week of 27 March was an instructive one for observers of American policy-making in the fields of science and technology. It began well, heralded by a special presidential message to Congress which conveyed a strong and inclusive commitment to the advancement of science and spelled out the details of Mr. Carter's State of the Union posture on the importance of science and innovation in the national agenda.

Almost simultaneously, the House of Representatives was voting to undo the National Science Foundation's programs in support of research in the social and behavioral sciences. This exercise in fiscal decapitation occurred in the context of an otherwise remarkably sensible House debate on the NSF's authorization bill. But it is a year in which the cuts must match the increases, and as usual the social and behavioral sciences furnished the sacrifice. To the NSF's contention that "we do not know the dynamics underlying our society and its institutions" came the rebuttal that "NSF may not know this but philosophers and thinkers have been contemplating such concepts for centuries without NSF support and have been able to reach far-ranging conclusions without wasting tax dollars." In the resulting colloquy it was established that the social science research budget would be halved.

In still another quarter, the Senate Budget Committee was readying itself for the markup of the first concurrent Budget Resolution, and one of the options up for decision will be a cut of $200 million from basic research in NSF, the National Aeronautics and Space Administration, and the Department of Energy. The House Budget Committee is rumored to be preparing cuts of the same magnitude.

To further enliven the week, a spectacular assemblage of industrial, academic, and university leaders met at New York University to identify and debate critical issues in science and technology policy. Judging from the outputs, the United States faces a mounting list of dilemmas along with an enfeebled capacity for their policy resolution. An indigestible feast of problems is not in itself so worrying; the source of deep concern lies in the time constants associated with the central problems of choice. As lead time shrinks while improvisation and misfiring mark the behavior of national policy, the time constants will tend to preempt the outcomes.

On the whole, it was a week to be remembered by science watchers. The presidential message on science and technology, for all of its important substance and policy signals, was scarcely noticed by the media. If it is likewise passed over by the scientific and technological professions, a rare opportunity for feedback will be lost. There is not much incentive for a President to put his views on the line if hardly anybody is listening. If we did things properly, such a message would precipitate baseline hearings in Congress and in the annual meetings of the scientific and professional societies, and the policy intentions expressed in the statement would be examined and argued. Although the budget numbers for science always become major news, perhaps because they are indicators of the short-term research and development market, they are not nearly as important to the prospects of lively science and innovation as is disclosure of the government's policy intentions. How long will it be before this sinks in?

If anything resembling a consensus national policy system for science and technology is to emerge, it should not be the sole province of the executive and legislative organs of government to define it. Industrial and academic science and technology should have a great deal to say about its properties, assumptions, and directions. Happily, this is the mind-set of the House Committee on Science and Technology, which will begin comprehensive hearings in April on the national investment in research and development. Although it was not planned that way, the first week in April could put to rights the unsettling last week of March.—WILLIAM D. CAREY

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