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Circle No. 19 on Readers' Service Card
The 1957 Hasselblad.
It's enjoying a revival. But not by Hasselblad.

A curious thing is happening in the camera business. Other people are just beginning to build what we discarded in 1957. A 2¼" camera with a focal plane shutter.

At the root of the problem is the sudden recognition of the 2¼" picture size as one that couples large format quality with miniature camera versatility (something that Victor Hasselblad recognized 23 years ago). The trouble is, with everyone rushing in to make 2¼" cameras, quality isn't always a big consideration.

Which is why you should know—before you buy any 2¼" camera—why Hasselblad changed from a focal plane to a leaf shutter back in 1957.

The focal plane shutter has to move across the entire film area, exposing the negative piece by piece, through a travelling slit. When the subject is moving parallel to the film plane, like a moving car, the position of the subject has changed by the time the last slit is exposed. This time lag can create distortion—an elongated or compressed car.

The same thing happens when taking pictures from a moving car or plane. The landscape tends to appear elongated.

In 35mm photography, focal plane shutter distortion is minimized by the short distance the shutter has to travel. But with 2¼" cameras, where the shutter has to travel farther, the distortion becomes more noticeable.

The leaf shutter, on the other hand (which we call a Synchro-Compur shutter in the Hasselblad) exposes the entire negative area all at once. Which makes it a much more accurate and desirable shutter.

The focal plane shutter has another considerable disadvantage. It can only be synchronized with electronic flash at very low shutter speeds. Which makes it all but useless with strobe for action and sports photography. (At higher shutter speeds, only a strip of the film would get exposed. At slower speeds you end up with ghost images.)

The problems are overcome by the leaf shutter which can be synchronized with all kinds of flash at all speeds and apertures (giving complete control over depth of field and background brightness). Making the leaf shutter far more versatile and useful to virtually every photographer.

So in 1957, Hasselblad carefully weighed the pros and cons of both shutter systems and decided to change over from the focal plane to the leaf shutter. We had to increase the camera price to do so, because the leaf shutter is a more complex, sophisticated mechanism. But the objective was to build the best camera possible, without being forced to compromise through economic necessity.

We then developed a full line of ten interchangeable lenses, each with its own leaf shutter mounted between the lens elements next to the diaphragm, in the most optically ideal position.

We increased the number of interchangeable film magazines to a total of six, providing a wide variety of different capacities and formats.

We added many accessories, including a microscope shutter and adapter, a gunstock telephoto lens mount, and a prism viewfinder with exposure meter. Giving greater flexibility to what was already the most flexible camera system. Leading NASA to choose Hasselblad as the space camera, using it aboard Mercury and Gemini flights, taking it to the moon on the Apollo flights, and for use on Skylab orbiting laboratories.

Many features of the 1957 Hasselblad were well worth copying. In fact we've copied many of them ourselves. But we also knew what not to copy. With the result that most 2¼" cameras now employ the shutter system we abandoned 14 years ago.

Of course, if price is a consideration, you'll have to select a camera with a less costly focal plane shutter. But before you buy a new imitation of the old 1957 Hasselblad, look into the used camera ads. You can probably pick up the real thing for less.
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the project at M.I.T. and is not a report on the output of the project. The book was an unspersonal effort. The project sponsored by the Club of Rome and the Volkswagen Foundation has been for the purpose of extending the model, modifying and documenting the assumptions, and discussing the inputs as well as the consequences by working with scientific research groups in other institutions who have relevant information and opinions. The published results of the project will begin to appear soon.

JAY W. FORRESTER
Alfred P. Sloan School of Management, Massachusetts Institute of Technology, Cambridge 02139

Unlike many of my colleagues who are extremely skeptical about large-scale simulation, data banks, and the organized use of politico-socioeconomic models, I believe (and am on record as believing) that an effort of the magnitude of the United States space program should be mounted to do this work. Where Forrester and I differ is that, although we may both see the limitations of econometrics and the importance of model building in areas where measures are hard to obtain, we have different views of what is feasible and useful. In my reading of his Industrial Dynamics and World Dynamics and glancing at Urban Dynamics, I have not seen anything to suggest that the models presented are other than extremely simplistic, given the current state of knowledge in the social sciences. I fear that if Forrester, with his drive, energy, and intelligence, were to obtain large funding for his work at the national level, he might easily be the source of a fad for producing grossly oversimplified models, which after a few years would bring about a reaction that would seriously set back progress in simulation, data bank creation, and large-scale social-scientific investigation.

In reply to the specific points in Gabor's letter, I should say first that it appears to me the burden of proof of the relative worth of Forrester's work lies somewhat with its proponents. I have not seen this proof forthcoming. No evidence is given in Forrester's books that his results are insensible to the details of the assumptions. Furthermore, contrary to Gabor's assumption, such insensitivity is not always a merit. Those who have worked with industrial, economic, social, or psychological simulations (Orcutt, Simon, Coleman, Abelson, Adelman, Crecine, Clarkson, to name a few) are well aware of the difficulties of obtaining a good first-approximation model. If it is too insensitive to parametric sensitivity analysis, the model is probably concentrating on the wrong variables. If it is highly sensitive, then data sufficiently accurate for the purposes at hand are probably impossible to obtain.

The application of careful dimensional analysis and the specification of good measures in the right dimensions are all difficult and critical. For instance, what is a measure of "welfare"? Even given a measure, what is its operational significance? Answers to questions such as these call for an intimate knowledge of subjects such as economics, sociology, psychology, and political science. Why are so few social scientists referred to by Forrester? Are there none whose knowledge is worth considering when building models of social, political, economic processes? In fact, there are many who have both deep substantive knowledge and the ability to use computers and who are convinced that the computer and computer models are critical for good social scientific exploration. Consider, for example, the Brookings SSRC model, Tinbergen's work on planning, Orcutt's simulation, or Klein's econometric models. Forrester in his writings does not tell us why such other workers' applications, their sensitivity analysis methods and model building, are not as good as his.

I believe that a proper scientific evaluation of the work on planning and forecasting models in the social sciences is of great importance. This, of course, should include Forrester's work. I propose to Gabor, to the Club of Rome, to the AAAS, to NSF, and others that a project be sponsored or at least a conference be held to initiate such an evaluation. It may turn out that Forrester's work is better than, as good as, or worse than that of Balderton and Hoggatt, Clarkson, Kalman Cohen, Coleman, Crecine, Cyert and March, Gordon, Klein, Leontief, Manne, Markowitz, Meyer, Naylor, or Orcutt, to name a few. An examination of Forrester's contribution to this large body of work in such a context could be of benefit to all. It might provide him with the opportunity to join with members of the growing body of social scientists working on social systems. I should be happy to supply him with a list of eminently qualified judges of work of this nature, in the fields of artificial intelligence, computer science, communications, eco-
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Test Bias

The letter (24 Dec. 1971, p. 1278) by Kenneth Clark and Lawrence Plotkin was meant to correct what they describe as "three egregious misstatements" in Stanley's article "Predicting college success of the educationally disadvantaged" (19 Feb. 1971, p. 640) where he discussed, among many other studies, one by Cleary (1). Clark and Plotkin took out of context a single sentence in the 7½-page article: "Cleary tried to replicate the findings of Clark and Plotkin [2] with a better controlled design, but failed." This conclusion referred to Stanley's prior quotation from the Clark and Plotkin study: "... Clark and Plotkin... had reported results of a study based on 'alumni' classes of the National Scholarship Service and Fund for Negro Students in which they concluded that: ...

... scholastic aptitude test scores are not clearly associated with college grades. It is suggested that college admissions officers weigh test scores less, since they do not predict the college success of Negro students in the same way they do for whites. This study indicates that motivational factors are probably more important than test scores in the demonstrated superiority of Negro students in completing college." Stanley was not questioning their conclusion that an able, highly motivated group of black students persisted well to graduation in a variety of interracial colleges during the 1950's. (For example, see note 19 in Stanley's article.) He did, however, cite much evidence—including Cleary's study—that Scholastic Aptitude Test scores and high school records tend to predict the college grades of blacks at least as well as they do those of nonblacks. In their letter Clark and Plotkin disregard this other evidence and thereby imply that Stanley's whole case rests on the Cleary study alone, which they seem to perceive as part of an Educational Testing Service plot against them ("Accustomed as we have become for our study to be the launching pad for ETS papers... "). Although one need not defend the professional integrity of researchers at ETS (3), we note that neither Stanley's article nor most of the reports he cites were done at ETS. Some of the strongest evidence came from investigations by black non-ETS researchers such as S. O. Roberts, Joseph P. McKelpin, and Charles Leo Thomas and from studies in Illinois, Maryland, Georgia, and the Seven Sisters colleges. Even a glance at the bibliography in the Stanley article would indicate the broad data on which he based his conclusions. (Indeed, only three lines of his paper were devoted to the Cleary article.)

Since Clark and Plotkin have chosen to criticize the Cleary article, let us consider the points they make. Clark and Plotkin say that their data were better than Cleary's because they "had data on over 1200 Negro subjects (with questionnaire response from over 500) drawn from all sections of the country and distributed in hundreds of diverse colleges." The quality of data is, of course, always relative to the purpose they are to serve. For the conclusion of Clark and Plotkin that an admissions officer (who necessarily works within a single institution) should weigh test scores less, distribution of the sample among "hundreds of diverse colleges" is a disadvantage rather than an advantage. For the admissions officer, Cleary's within-college analysis is clearly more relevant. Since Cleary was able to analyze data in only three colleges, she limited her conclusions: "The schools used in this study do not represent the full spectrum of colleges in the United States, so general conclusions cannot be reached." But there are many studies other than Cleary's in which the within-college analysis has been performed, and most of these were reviewed in the Stanley article. With each new analysis, the Clark and Plotkin conclusion becomes less credible.

In their criticism of the Cleary article, Clark and Plotkin persist in their emphasis of correlation coefficients to the exclusion of regression lines. It is well known that the size of a correlation coefficient is a function of the variability of the group: when the range of scores is restricted, the correlations are attenuated. For this reason and others, the comparison of regression lines is a more appropriate analysis. Clark and Plotkin computed no correlation coefficients or regression equations, nor did they even work within colleges. Instead, they pooled college grades from 187 different colleges and

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universities (2, pp. 58–59), thereby treating grades at highly selective institutions as equivalent to grades at unselective ones. (We recognize that they had no choice in this matter; at the time of their study, institutions of higher education were so severely segregated that a useful within-college analysis in integrated colleges was impossible.)

Rather than focus on correlations, Cleary proposed a definition of test bias that was to be examined in her study (1, p. 115):

A test is biased for members of a subgroup of the population if, in the prediction of criterion for which the test was designed, consistent nonzero errors of prediction are made for members of the subgroup. In other words, the test is biased if the criterion score predicted from the common regression line is consistently too high or too low for members of the subgroup. With this definition of bias, there may be a connotation of "unfair," particularly if the use of the test produces a prediction that is too low. If the test is used for selection, members of a subgroup may be rejected when they were capable of adequate performance.

She was able to conclude that, in these colleges, the SAT was as appropriate for the prediction of college grades of blacks as of whites (1, p. 123):

In the two eastern schools, there were not significant differences in the regression lines for Negro and white students. In the one college in the southwest, the regression lines for Negro and white students were significantly different: the Negro students' scores were overpredicted by the use of the white or common regression lines. When high school grades or rank-in-class are used in addition to the SAT as predictors, the degree of positive bias for the Negro students increases.

In their letter Clark and Plotkin state that one has to examine Cleary's tables to find the differences in correlations for blacks and whites, because she fails to mention them in her text. In fact, in both the earlier Research Bulletin (4) which they cite and the journal article (1) cited by Stanley, three paragraphs are devoted to correlation coefficients; the differences are pointed out and explanations are proposed.

Clark and Plotkin are correct that there may have been some misidentification of race in the Cleary study: in only one school were records of race available from the college; in the other two colleges identification was made from photographs, with corroboration provided from an NAACP list in one of the colleges. Gross errors in classification would be required, however, to change the results markedly.

Testing and Fair Employment (5) is cited by Clark and Plotkin to indicate that differential racial validity is a problem in industrial settings. This is really not relevant to the question of the SAT in educational prediction. Even so, several important points may be noted: (i) the book routinely examines correlation coefficients, rather than regression lines; (ii) all but one of the studies has very small samples of Negroes, usually about 31, and yet large numbers of correlation coefficients are computed and compared; (iii) in the only study with a reasonably large sample (98 Negro and 437 white), differential validities were not found; and (iv) in the one educational study that they say contradicts Cleary's results, the Negro and white students were in different schools, although they took the same criterion tests (State Examinations in Nursing).

In two recently published studies of educational predictions, Temp (6) and Davis and Temp (7) argue strongly for validity studies in all institutions that are using the SAT (or any other admission tests or predictors) for the selection of students. We agree with this recommendation. The College Board provides, through the Educational Testing Service, free validity studies to any institution that uses the SAT in its admissions process. Users of the SAT are encouraged to study the separate regression lines for Negro and white students (as well as other groups such as males and females) so that they can make reasoned judgments about the utility of the test for groups of students in their institution.

T. ANNE CLEARY
College Entrance Examination Board, 888 Seventh Avenue, New York 10019

JULIAN C. STANLEY
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References and Notes

3. For a rejoinder to one of Clark and Plotkin's complaints about ETS, see R. O. Forman, Science 175, 706 (1972).
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**Psychology and Health Care**

John Walsh (News and Comment, 3 Dec. 1971, p. 1003) reports on "Health manpower training: Funding levels at issue" and describes certain inherent problems of fiscal, political, and manpower balance. Both Walsh's comment and the legislation that was enacted omit realistic consideration of a major health profession—psychology.

In terms of hospital beds, patients affected, community services provided, professionals involved, or funds expended—to say nothing of social cost—mental disorders constitute a very major and substantial health problem. Yet the current national-level proposals for health insurance and the broad range of existing prepaid health coverage largely exclude or seriously limit coverage for mental disorder. These proposals and existing coverage commonly exclude the services of psychologists or fail to recognize psychologists as the independent health practitioners that they are—licensed in 44 states and the District of Columbia and represented by a national organization of over 33,000 members, the American Psychological Association.

If there is any doubt that psychology has major substantive relevance and capacity to deliver in this arena, or that mental disorder is not a major health problem, I would recommend a review of the ten reports (published as separate texts by Basic Books, New York) of the Joint Commission on Mental Illness and Health appointed by President Kennedy. Six of these texts were authored, and one coauthored, by psychologists, perhaps evidence both of the preeminent scope of knowledge which my profession has to contribute to this field and its documented willingness to participate in the resolution of health problems at the national level.

Hermann Dörken

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**Research Management**

In the editorial (1 Oct., p. 11) that was excerpted from the 1945-1946 Carnegie Institution yearbook (1), Vannevar Bush seems to imply that fundamental research cannot be managed, but only supported, with the management of a program left to the scientist conducting the research. Quite the contrary! Experience in industry has repeatedly shown that the scientist conducting the research is frequently the poorest choice as manager due to his lack of objectivity, or removal from personal involvement. It has been far more successful for the manager to be a separate person from the researcher, although also a scientist.

The "managed" approach has also more successfully kept the final objective in clear focus. The alternative course, as advocated by Bush, frequently results in a diffuse approach, which either never comes to grips with the central problem, or takes a considerably longer time to do so.

The managed approach is not without its disadvantages or risks. But even Bush's "eminent" scientists must make judgments and take the chance of making a wrong choice. The central issue is to make sure that the managers or management groups are properly staffed. If the scientists are anywhere as eminent as Bush contends, they should have no difficulty convincing their managers of the wisdom of their choices. None of us likes to be called to account for our work and forced to justify the direction we have chosen. But in the balance, this is preferable in the case of any national commitments we might choose to make, now or in the future.

F. LOUIS FLOYD

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Reference


**The College Professor**

Because the AAAS has recently made efforts to reform its governance, I decided to read the list (18 Feb., p. 804) of AAAS officers, committees, and representatives. An intriguing pattern began to emerge; of the scores of academic men and women listed, only two are affiliated with an undergraduate college (if we regard Dartmouth, the City College of New York, and Queens College as entities greater in scope and size than a "college"). Of these two, Rhoda Dorsey, who is on the Committee on Minorities in Science, is apparently a representative not only of the majority sex in America but also of a significant minority in academia, the college professor. The other, Donald Aitkin, is on the Youth Council.

Why does the AAAS have active
participation from all components of its membership save from those colleges that do not grant doctorates? These institutions represent an important segment of our educational system, and prepare many of our future teachers, doctors, and (hopefully) scientifically aware citizens. I hope that the AAAS officers will recognize that science is not the province only of the university, the corporation, and the government.

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Machine Translation

H. Wallace Sinaiko comes to substantially the same conclusion in his letter (17 Dec., p. 1182), "Translation by computer," as that of a National Academy of Sciences—National Research Council committee in 1966. The members of that committee (chairman John R. Pierce, John B. Carroll, Eric P. Hamp, David G. Hays, Charles F. Hockett, Anthony G. Oettinger, Alan Perlis) are gratified at this confirmation.

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Reference

Lunar Basalts

We agree with most of Allen L. Hammond's review (Research News, 25 Feb., p. 868) of the history and structure of the moon. However, there is one important point that should be clarified. He says, "Within this period, two major phases of thermal evolution have been identified: (i) widespread melting that apparently occurred about the time of the moon's formation; and (ii) partial melting beginning as early as 4.1 billion years ago to form basalts enriched in potassium, rare earth elements, and phosphorus (KREEP basalts), and somewhat later, flooding of preexisting basins with lava to form the lunar maria between 3.1 and 3.7 billion years ago." He makes other remarks that are not clear about the times at which the basalts were formed.
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We conclude that the maria basalts acquired their gross composition 4.5 to 4.7 billion years ago, and that this composition was only slightly modified in the second melting about 3.1 to 4.0 billion years ago. This point was discussed at the Second Lunar Science Conference by Urey et al. (1), and it has been clearly established by Wasserburg et al. (2) with an isochrone plot of $^{87}$Rb against $^{87}$Sr in the average soils of Apollos 11, 12, 14, 15, and Luna 16. We must conclude that these basalts were formed about 4.6 billion years ago, and that a closed-system melting process occurred later. Our explanation for the closed-system melting is as follows: When melting (of undifferentiated terrestrial material) occurs in a strong gravitational field, for example, that of the earth, the liquid separates and forms an extensive pool which, after considerable accumulation, bursts through to the surface. This liquid has the composition of basalt, and the unmelted material below has another composition. The rubidium-strontium composition differs in the two fractions. Because the moon has a weak gravitational field, liquid and solid separate less completely or not at all; when a previously formed basalt melts, solid and liquid flow out together, and the mixture has the same composition as that of the original pack of basaltic rock. The relative distribution of rubidium and strontium in different minerals was changed in the remelting process, although the overall ratio of Rb to Sr was not changed; hence, two isochrones are secured, one when the data from individual crystals of mare basalts are plotted, and the second when the gross compositions of the different mare basalts are plotted. This is the observation of Wasserburg et al. (1). Our arguments are somewhat different and involve the uranium and thorium-lead dates as well, but we arrive at the same conclusion. We also give a suggestion about the thermal history of the moon, but what we wish to emphasize is that the basaltic composition of the lunar-ash flows must have been acquired 4.5 to 4.7 billion years ago, in the primitive melting and crystallization process, and remained approximately unchanged in the second melting process.

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References and Notes
3. We acknowledge support from NASA contract NGR 05-009-150.

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