Table 1. Analyses of gases collected from Sulfur Bank fumarole.

Sample tube	Constituents (vol %)					
	CO_2	CO	H_2	SO_2	O_2	N_2 (residue)
During an eruptive stage of Kilauea Volcano, July 1952						
1	97.4	1.0	0.4	1.5	0	0
2	97.6	1.4	0	1.8	0	0
3*	96.2	1.7	0.4	1.6	0	0
During a quiet stage of Kilauea Volcano, June 1953						
1*a*	10.9	3.2	0	0	13.6	71.5
1*b*†	10.6	2.2	0	0	15.1	70.7
2	10.2	2.9	0	0	13.2	71.8

* No. 3 was a vacuum-bottle collection made at a later date than collections No. 1 and No. 2.
† Poor analysis.

Table 2. Stable carbon isotopic ratio in the carbon dioxide present in volcanic and Sulfur Bank fumarolic gases.

Gas sample	C^{12}/C^{13}
Sulfur Bank 1949, Mauna Loa in eruption	89.0
Sulfur Bank 1952, Kilauea in eruption	89.0
Sulfur Bank 1953, both volcanoes quiet	89.0
Gas collected from 1950 Mauna Loa lava flow	91.2
CO_2 extracted from Olivine Basalt of 1950 Mauna Loa lava flow	90.7

carbon dioxide from a Jurassic limestone that has been used as a primary standard by other workers, through the kindness of A. O. Nier (*5*). Isotopic ratio determinations of the carbon dioxide samples were made on the Consolidated-Nier type of mass spectrometer.

The results for the gas analyses are listed in Table 1. The great difference in the composition of the gas between times of eruption and quiescence of the nearby volcano is noteworthy. During the quiet period, there is strong indication of air contamination from the presence of nitrogen and oxygen in the gas. The possibility of using a systematic gas-analysis routine to detect changes in the proportions of the gaseous components with time and to use this as a predictive tool in volcanology immediately arises and, in fact, has been suggested previously.

The results for the determination of the carbon isotopic ratios are listed in Table 2. The significant points to be noted are (i) the constant value of the isotopic ratio of the carbon dioxide obtained from the Sulfur Bank fumarole despite the eruption or dormancy of the adjacent volcanoes and (ii) the "heaviness" of the fumarolic carbon dioxide when compared with the gas extracted from the lava or from above the active lava flow.

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References and Notes

1. S. S. Ballard and J. H. Payne, *The Volcano Letter,* No. 469, July–Sept. 1940.
2. J. H. Payne and S. S. Ballard, *Science* **92**, 218 (1940).
3. We are deeply indebted to A. O. Nier of the Physics Department, University of Minnesota, for running some of the isotopic determinations, and to Earl Ingerson and Gordon A. Macdonald, U.S. Geological Survey, for aid and advice in the collection of the samples. Some of the early work on the preparation of the samples was done at the Frick Chemical Laboratory, Princeton University, and most of the work was aided by the Office of Naval Research, under contract Nonr-981(00), project NR 081 185.
4. S. Dushman, *The Scientific Foundations of Vacuum Technique* (Wiley, New York, 1949), p. 649.
5. A. O. Nier, *Phys. Rev.* **77**, 789 (1950).

13 September 1954.

The Visiting Research Professor

Ten years ago Carl E. Seashore of the State University of Iowa, Emeritus Professor of Psychology, but called back to serve as Dean of the Graduate College, proposed [*Science* **100**, 218 (1944)] the appointment of retired persons who desire to continue their researches as *visiting research professors* at a neighboring university. Supporting his view he appointed two visiting research professors in 1944. As one of those fortunate persons, I can testify to the enormous benefits that have accrued to me. A stipend was granted sufficient to enable the appointee to spend 3 months in residence at the university or to defray the expenses of making frequent visits for longer or shorter periods. Most important has been the fellowship of the resident staff, and the incentive to keep on doing those things that one has been doing and hoping to continue to do. I commend the visiting research professorship to university administrators and to retired professors. It is immediately available. It meets the needs of elderly persons and increases the national scholarly output. And it requires no outlay for additional buildings, libraries, or laboratories.

Henry S. Conard
Emeritus Professor of Botany, Grinnell College

7 September 1954

Psi and Probability Theory

The occurrence of significant deviations from mean expectancy in experiments in which guesses, cards, drawings, die faces, and so forth, are matched with targets has been attributed not only to psi (*1*) but also to error in the production, recording, selection, or analysis of the data. That these counterhypotheses to psi have been adequately refuted, either by ratiocination or by the performance of experiments in which the counterhypotheses were precluded, is testified to by the subsequent silence of their proponents. Two explanations for the results of these matching experiments have remained, namely (i) reality of psi and (ii) fallacy of probability theory. While there are comparatively few who have accepted the first explanation, there have been practically none, until recently, who have voiced their acceptance of the second explanation. As unacceptable as psi theory may be, it has so far proved generally less unacceptable than the abandonment of probability theory.

Recently, however, an experiment has been reported by Brown (*2*), the results of which have caused him to question the validity of probability theory and the evidence for psi that rests upon it. In this experiment he matched "randomly selected columns of random digits" and obtained results that differ from the mean expectation by more than three standard deviations. If forthcoming, a detailed publication of the procedure and data of Brown's experiment may indicate that the results were producible by error in the production, selection, or analysis of the data. Otherwise (unless the improbable conclusion is accepted that this deviation with a p of less than .001 is the result of chance), there are two possible explanations for Brown's results: (i) they are the result of fallacy of probability theory, and (ii) they are the result of psi. While Brown recognized only the first explanation, the following considerations indicate that the second explanation is also a possibility.

Sections of a random series vary in degree of similarity and must be randomly selected so that their matching will constitute a valid test of probability theory. While Brown does not state the method by which the columns he matched were "randomly" selected, the method he used is not important since, if psi is real, any method of selection may be influenced by it. For example, if the columns were selected by cutting a book, the result may have been influenced by extrasensory perception; if they were selected by rolling a die, the result may have been influenced by psychokinesis (*3*); and if they were selected by the next day's temperature, the result may have been influenced by precognition. Even if the matched sections are chosen systematically, the choice of the system (of choosing the matched sections) may be influenced by psi. For example, if the chosen system is to match the first two columns, there existed the choice of matching two contiguous sections at the beginning of the series or some other pair, the choice of the length of the sections that were matched, and the choice of the table of random digits that was used. Since it is possible to select two similar sections from a sensorily perceived random series, there is no valid reason to doubt that, if psi is real, two similar sections could be selected from an extrasensorily perceived random series. This is supported empirically by the significant results obtained with the "ESP shuffle" technique which consists of matching two series of cards whose symbols are not sensorily perceived (*4*).

The results of Brown's experiment add little to the evidence for accepting psi or abandoning probability theory that is not already provided by the results of other matching experiments. However, Brown's results may be differently received. Here, in an experiment designed to test the concept of randomness and which, therefore, may not be disregarded by those whose work is based on the validity of probability theory, are results that cannot be explained, unless they are attributed to chance, except by the alternatives of the reality of psi or the fallacy of probability theory. The results of Brown's and other matching experiments place statistical theorists, and those whose work is based on probability theory, in the unpalatable position of having to assert that psi is real in order to uphold the validity of probability theory. To make it doubly distasteful, if they accept psi they must admit its possible effect on the selection of "random" samples.

Perhaps the dilemma of accepting psi or abandoning probability theory will be completely resolved only if and when there is adequate experimental evidence for psi other than that based on probability theory. Until then the only logically defensible position is affirmation of psi or denial of probability theory.

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References and Notes

1. Personal factors of processes in nature that transcend accepted laws.
2. G. S. Brown, *Nature* **172**, 154 (1953).
3. The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.
4. J. B. Rhine *et al.*, *Extra-sensory Perception after Sixty Years* (Holt, New York, 1940).

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A scientific hypothesis must live dangerously or die of inanition. Science thrives on daring generalizations. There is nothing particularly scientific about excessive caution. Cautious explorers do not cross the Atlantic of truth.—Lancelot Hogben, Science for the Citizen *(1938).*