Ridge, are characterized by a large-amplitude positive anomaly over the center.

The argument (*1*) that "the residual negative free-air anomaly indicates an isostatic imbalance that should tend in the long run to raise the crust rather than bend it down" is invalid. The section shown in Fig. 1 is in isostatic equilibrium. The principle of isostasy states that there is a surface within the earth on which the pressure due to overlying structure is equal. Part of the pressure may be due to the mass of the section, but part may also be due to bending stresses in the lithosphere (*8*). Thus, large gravity anomalies may exist even though a region is in isostatic equilibrium.

We have made no attempt in Fig. 1 to match the computed gravity effect of the deformation model to the contours of the free-air anomaly map of Case *et al.* We consider their contours largely invalid. Short-wavelength free-air gravity anomalies in oceanic regions generally correlate most closely with changes in topography (*9*). In spite of this, the map in Case *et al.* shows a steady gentle decrease in free-air anomalies between Marchena and San Salvador, ignoring the gravity effect which would arise from a channel 1800 m deep between these islands (Fig. 1).

We are not attempting to prove that a hot spot or mantle plume does not underlie the Galápagos Islands, or that Fig. 1 necessarily represents the actual crustal structure beneath the islands. We have used a simple deformation model, which has been applied to other volcanic islands, to explain the observed data in a quantitative manner. Thus, it is not valid to interpret gravity data in terms of a hot spot or mantle plume beneath volcanic islands until the gravity effect of the topography and the manner in which it is supported is quantitatively accounted for.

A. B. WATTS
Lamont-Doherty Geological Observatory of Columbia University, Palisades, New York 10964
J. R. COCHRAN
Lamont-Doherty Geological Observatory and *Department of Geology, Columbia University, New York 10027*

References and Notes

1. J. E. Case, S. L. Ryland, T. Simkin, K. A. Howard, *Science* **181**, 1040 (1973).
2. F. A. Vening Meinesz, *Proc. Kon. Ned. Akad. Wetensch.* **42**, 1 (1941); R. Gunn, *J. Franklin Inst.* **236**, 373 (1943).
3. R. I. Walcott, *Tectonophysics* **9**, 435 (1970); A. B. Watts and J. R. Cochran, *Trans. Am. Geophys. Union* **54**, 331 (1973).
4. R. I. Walcott, *Can. J. Earth Sci.* **7**, 716 (1970).
5. ———, *Geol. Soc. Am. Bull.* **83**, 1845 (1972); J. R. Cochran, *ibid.* **84**, 3249 (1973).
6. T. E. Chase, *U.S. Dep. Inter. Circ. No. 291* (1968).
7. G. G. Shor, H. W. Menard, R. W. Raitt, in *The Sea*, A. E. Maxwell, Ed. (Wiley-Interscience, New York, 1970), vol. 4, pt. 2, pp. 3–28.
8. R. Gunn, *J. Geol.* **57**, 263 (1949).
9. M. Talwani, X. Le Pichon, M. Ewing, *J. Geophys. Res.* **70**, 341 (1965); M. Talwani, H. R. Poppe, P. D. Rabinowitz, *NOAA Tech. Rep. ERL-228-AOML7-2* (National Oceanic and Atmospheric Administration, Rockville, Md., 1972).
10. Lamont-Doherty Geological Observatory Contribution No. 2045. Supported by NSF grant GA-27281 and ONR contract N00014-67-A-0108-004. The manuscript was critically reviewed by M. Talwani and E. Herron.

21 December 1973

Watts and Cochran have reiterated the main point of our report on the Galápagos gravity anomaly—namely, that a low-density mass underlies the Galápagos platform.

As outlined in our report, at least three causes of the anomaly may be geologically plausible: (i) A block of low-density continental crust may underlie the platform. (ii) Thermal expansion related to a plume or hot spot may lower the density of the crust and upper mantle. (iii) Weight of the volcanic pile may have caused crustal downwarping. None of these possibilities can be excluded on the basis of the gravity anomaly field, and the possible causes may overlap. Each of these three possibilities has been modeled (*1*), and the observed gravity anomalies can be fitted by any of the three. Because of the scanty data, we did not publish the models in our report.

Model (iii) is the one discussed by Watts and Cochran, and they maintain that the site of the crustal downwarp is in isostatic equilibrium. We agree that this model may well be correct, but, because gravity potential fields have nonunique solutions, independent data are required for confirmation. The critical test is in the lithologic (velocity) structure of the crust and upper mantle. If seismic refraction data indicate that the M-discontinuity dips beneath the archipelago to form a "root" of approximately 6 km, as suggested by their model, then crustal loading or a block of continental crust may be suspected. It then remains to explain the origin of the volcanic material that is loading the crust to form the downwarp or to explain the origin of the block of continental crust.

Watts and Cochran stated that they believe our data in no way support the conclusion that "the gravity data can be most readily interpreted in terms of a low-density region related to a hot spot or plume" beneath the islands. They failed to note our geologic reason for the interpretation, namely: "This preference is based on the direct evidence of the widespread active Holocene volcanism of the islands themselves and the topographic expression of past volcanism leading away from the islands along the Cocos and Carnegie ridges."

We are pleased that Watts and Cochran pointed out errors in our report and that they have focused attention on the Galápagos problem. We hope this discussion will stimulate the refraction studies that are crucial to solution of the problem.

J. E. CASE
U.S. Geological Survey, P.O. Box 6732, Corpus Christi, Texas 78411
S. L. RYLAND
California Institute of Technology, Pasadena 91109
TOM SIMKIN
Smithsonian Institution, Washington, D.C. 20560
K. A. HOWARD
U.S. Geological Survey, Menlo Park, California 94025

References

1. S. L. Ryland, thesis, University of Missouri, Columbia (1971).

21 March 1974

Conceptual Deficits in Women

Thomas *et al.* (*1*) report that many college women, unlike men, do not know the principle that the surface of still water is invariably horizontal and that, as a result, they perform poorly when required to estimate the surface angle by adjusting an artificial water level. Second, they claim to demonstrate that ". . . college women who do not know the principle do not readily learn it in tasks designed to optimize self-discovery of the concept." Both conclusions seem unwarranted by their data.

The study has two potentially serious experimental flaws. First, their proce-

dure indicates that experimental and control groups were handled differently. Experimental subjects were obtained by giving a group of college women a pretest in which they had to match the angle of a pretend water line (seen through an empty bottle) with the actual surface angle of red water contained in a second bottle. Only those women who did not meet predetermined skill levels were retained. They were then asked two questions pertaining to the principle and tested further with the apparatus. In contrast, control subjects were obtained by asking the two questions first, retaining the women who knew the principle, and then testing them with the apparatus. (The male control group was "unselected" but like the female control group was first questioned and then tested with the apparatus.) The results from experimental and control groups are not comparable. The difference in selection procedure amounts to informing the control group that the test concerned the determination of water level angle and not informing the experimental group of the fact. The control group received cues, in the form of leading questions, about the purpose of the test before they could generate their own hypotheses and make any errors with the apparatus. In contrast, the experimental subjects were not asked leading questions and so received no cues about the purpose of the test. For all they knew, it could have related to conforming behavior, manual dexterity, or the properties of liquids. We feel that this difference is not insignificant when one is testing the effect of preknowledge on test performance.

Our second criticism relates to the teaching procedure and to the apparatus as a teaching device. First, it appears from figure 1 in their report that an adult must look down and to the left to view the water. The water surface will then be seen as an ellipse, seemingly tilted to the horizontal at an angle that depends on and changes with one's position. Neither the long nor the short axis of the ellipse will appear to be parallel to the horizon. Moreover, the side walls of the bottle make different angles with the surface and axes of the ellipse. A trial with a beaker of water will make it evident that this array of three-dimensional surfaces and angles does not resemble the pretend water line that the subjects must adjust (*2*). (The latter is the one-dimensional boundary line between red and white halves of a cardboard disk seen through the empty test bottle.) A rounded fish bowl might have been a more appropriate vessel. Second, maintaining a fixed angle to the horizontal in the face of conflicting visual cues is notably difficult for some individuals whom Witkin *et al.* (*3*) have termed "field dependent" subjects. They (*3*) found that a higher proportion of women are field dependent than men. Moreover, performing the test correctly requires no understanding of physical principles, since the answer is always the same (that is, horizontal). The failure of the subjects to learn this in 48 trials suggests that the apparatus presents optical or perceptual problems. Unfortunately, the authors do not present the direction of the errors of the angular settings of the red-white disk for individual subjects, so that it is impossible to determine what effects, if any, these problems may have had on the subjects' performance.

In addition, the testing procedure may well discourage learning rather than optimize it. The women were tested for the ability to learn physics using an unfamiliar device without previous instructions, and only those women who failed seven times in a row were retained for studies 1 and 2. In all probability, at least some of the subjects themselves believed that they were mechanically inept or that they lacked the intellectual ability to understand physics, both being widely held views about women in our culture. For both reasons, these subjects were probably in less than an optimal state for learning.

Furthermore, there are several reasons for believing that experimental bias entered the study (*4*). First, had no naive males been found, the report could justifiably have focused on women alone. However, the statement in the opening paragraph that ". . . by 12 years of age, boys understand the principle . . ." is misleading, since it suggests that naive male subjects were not available. In fact, 13 percent (8 out of 62) of the college males were classified as naive [reference 4 in (*1*)]. Since naive male subjects form one of the appropriate control groups for naive female subjects, we question why the authors did not test them in a way comparable to the naive women. Furthermore, data from the naive males are pooled with data from sophisticated males in table 1 in (*1*). The results should be presented separately. Second, as Orne (*5*) has pointed out, experimental subjects often have an uncanny sensitivity to the "demand characteristics" (the experimenter's expectations) of an experiment. It is therefore disturbing to read terms like "subjects . . . who were expected to do poorly" and "nonlearning subjects," even though the authors attempt to define them in a nonpejorative sense.

Consideration of the effects of apparatus complexity, field dependence, subject morale, and experimental demand characteristics calls into question the report's primary conclusion that women who do not know the principle "do not readily learn it." The procedure may simply be a poor way to teach it (*2*). Comparison of the performance of naive and sophisticated subjects has little meaning since they were treated differently. The topic is interesting and merits further study, but we urge the authors to pay stricter attention to details of experimental design and to consider alternative explanations for their results. Like other studies touching on "innate" intelligence, these results have social implications and can be easily abused. After a hasty reading, someone convinced that women lack conceptual understanding or are incapable of learning physics might believe that his or her prejudices had been scientifically proved (*6*).

TIMOTHY PERPER
JULIA CHASE

Department of Biology,
Livingston College,
Rutgers University,
New Brunswick, New Jersey 08903

References and Notes

1. H. Thomas, W. Jamison, D. D. Hummel, *Science* **181**, 173 (1973).
2. Informal trials with both women and men who know the principle suggest to us that it is extremely difficult to estimate the surface angle of colored water when one must look down and to the side to see the water. When asked to estimate the perceived angle of the water, as opposed to the actual angle, these individuals made systematic errors.
3. H. A. Witkin, H. B. Lewis, M. Hertzman, K. Machover, P. B. Meissner, S. Wapner, *Personality through Perception* (Harper, New York, 1954).
4. R. Rosenthal, *Experimenter Effects in Behavioral Research* (Appleton-Century-Crofts, New York, 1966).
5. M. T. Orne, in *Nebraska Symposium on Motivation*, W. J. Arnold and M. M. Page, Eds. (Univ. of Nebraska Press, Lincoln, pp. 187–265.
6. We thank J. Burger, M. MacDonnell, and L. Karen for helpful comments, and one of the *Science* referees for pointing out the relevance of Orne's work to this study.

11 October 1973; revised 26 December 1973

Although Perper and Chase (*1*) chide us about our experimental design (*2*), each of their major criticisms misrepresents our method. They state that "subjects were obtained by giving a group of college women a pretest in which they had to match the angle of a pretend water line . . . with the actual surface angle of red water contained in a second bottle." Our subjects were required to predict, not match, the position of the real water level. We clearly stated that "a cover was placed on the bottle and the subject was asked to adjust the pretend waterline. . . ." Later in that same paragraph we define the pretest as consisting of eight prediction trials. We retained for training those subjects who made fewer than seven out of eight correct responses on the pretest. Our criterion was not "seven times in a row," as Perper and Chase report.

Perper and Chase state that our criterion women "were obtained by asking the two questions first, retaining the women who knew the principle, and then testing them with the apparatus." This was not our procedure. We tested 72 college women and 62 college men; each subject made a series of prediction adjustments and afterward responded to the two interview questions. Thus neither the criterion women nor unselected men received any "leading questions."

Perper and Chase are concerned that our subjects viewed the real water surface from some oblique position and thus have been misled. Subjects typically adjusted their position so that they had a direct view of the water surface, and they viewed the water surface as almost a line. In addition, our data show that Perper and Chase's claim is implausible. We stated that "when the real water was visible the pretend waterline adjustments in both studies 1 and 2 were always accurate (median error 0°)." Now we ask how naive subjects who somehow do not see or judge the real water as horizontal can persistently adjust the pretend waterline to horizontal when the real water is visible? We believe that the naive subjects do see the water as horizontal; but they apparently have not abstracted the principle and cannot apply it in the prediction situation.

Perper and Chase suggest that our findings are another manifestation of Witkin's field-dependent response style. We note three factors that distinguish our research from that of Witkin *et al.* (*3*). (i) On the rod and frame task all ordinary visual cues are absent; subjects are tested in the dark. Our testing was done in an ordinary room with prominent visual cues. (ii) Our naive subjects errorlessly adjust the pretend waterline to the horizontal position (when real water is not in view) if specifically instructed to do so. But the essence of the rod and frame task is the subject's inability to adjust the rod to a vertical position. (iii) Witkin assumes the subjects correctly understand the principle of verticality; the predictive task performance and verbal responses of the subjects we define as naive convince us that they lack knowledge of the physical principle.

Perper and Chase's description of our testing situation leaves the impression that we are engaged in the teaching of physics. They suggest that we have capitalized on women's stereotypic feelings of inferiority with regard to things mechanical and scientific. However, the context of our task was hardly that of a physics lesson, and the apparatus was designed for and first used successfully with nursery school children. Perper and Chase worry that the experimenter's expectations may have affected our subjects' performance and as evidence cite statements that we used to describe subjects' performance long after they left the laboratory. The statements "expected to do well" and "expected to do poorly" were part of the instructions we gave judges who sorted subjects based on their verbal responses.

Perper and Chase wonder why we did not attempt to train naive men. By our estimate of the proportion of naive men on this campus, it would have been necessary to sample about 500 men to recruit 63 naive men to replicate studies 1 and 2. However, we know of no useful purpose this effort would achieve. And Perper and Chase offer none. We have no reason to believe that naive men would behave differently than naive women. Incidentally, we wonder whether Perper and Chase would want us to test women if all our experimental subjects had been men.

One remaining issue that we did not raise in our report, but which Perper and Chase mentioned, concerns some implications of our findings. We offered no explanation for our findings, but Perper and Chase saw them as "touching on 'innate' intelligence." We are not alarmed by this possibility. In fact, we believe that our findings may be related to sex-linked factors and have tested a sex-linked model by using the proportion of sophisticated subjects of each sex in a college sample (*4*). The model was ultimately rejected. At the moment we tend to think that sex-linked genetic factors create predispositions within individuals which affect the ease with which the principle is acquired through experience. There is good evidence from other sources that sex-linked factors are involved in behavior processes (*5*). We maintain that the genetic hypothesis is reasonable to consider, especially given the differences in the proportion of sophisticated subjects within each sex. It is this difference in proportion that any alternative explanation of the phenomenon will need to explain. We have heard of no psychological explanations which will account for both the within and between sex differences that we have reported.

We have refuted the methodological criticisms of Perper and Chase. None of their remaining comments justifies altering our conclusion that a great many college students do not know still water is horizontal and may have difficulty learning it.

HOBEN THOMAS
WESLEY JAMISON
Department of Psychology,
Pennsylvania State University,
University Park 16802
DONNA D. HUMMEL
Department of Psychology,
Bryn Mawr College,
Bryn Mawr, Pennsylvania 19010

References

1. T. Perper and J. Chase, *Science* **184**, 809 (1974).
2. H. Thomas, W. Jamison, D. D. Hummel, *ibid.* **181**, 173 (1973).
3. H. A. Witkin, H. B. Lewis, M. Hertzman, K. Machover, P. B. Meissner, S. Wapner, *Personality Through Perception* (Harper, New York, 1954).
4. H. Thomas and W. Jamison, *Merrill-Palmer Q.*, in press.
5. R. D. Bock and D. Kolakowski, *Am. J. Hum. Genet.* **25**, 1 (1973).

1 March 1974