trinal stand it cannot be in error. This is because the Church believes it was founded by God and guaranteed by him against error in "faith and morals." Over a period of 20 centuries the Church has never made an essential change in any of its doctrines.

Two other points should be mentioned in connection with the review.

1) Davis believes that the method of birth control does not determine the morality of marital intercourse as long as the married couple "intends in good faith to have children and does have them." The Catholic Church, on the other hand, believes that the end does not justify the means, and that the use of bad means for a good end makes the act morally bad. While the end, limitation of the number of children, may be good in some cases, the means, artificial birth control, are always bad.

2) Davis does not make a proper distinction between the Church's philosophical and theological position and the *tactics* that may be employed by Catholics in certain instances. The *laws* against contraceptives were placed on the statute books of Massachusetts and Connecticut by Protestants in the latter part of the 19th century. It is only natural that, in opposing Margaret Sanger and her coworkers of 1914, Catholics should make use of existing laws. The "first line of defense" against a fire is an existing firehose.

Finally, I hope that in the future, when books of this type are reviewed, the editors of *Science* will insist on the same objectivity in presentation of the position of the Catholic Church that they would on any strictly scientific matter.

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Moon Illusion and Age

Leibowitz and Hartman in their report "Magnitude of the moon illusion as a function of the age of the observer" [*Science* **130**, 569 (1959)] interpret the moon illusion "as resulting from a normal developmental process, namely the dependence of the magnitude of the size constancy correction on experience." This conclusion is not in conflict with the result of their outdoor experiment, where presumably there were objects of common experience in the horizontal plane which could provide the subjects with size standards and with landmarks for parallax distance determination. However, it is difficult on the basis of this "experience" theory to see how the same results could be obtained in a darkened theater. If the theater is darkened to the extent that

no common objects remain in view, then accommodation and convergence will provide the only cues to the distance of the disks. (I assume this was the reason for conducting the experiment in the dark.) In the darkened theater, then, there remain but two variables: the position of the head relative to the body, and the angular aspect of the otolithic organs. While it seems reasonable that these might be related to perceived size, it is not clear why the postural experience of children and adults should lead to the results obtained.

Furthermore, I would like to point out one assumption underlying the mathematical sign of the correction. The authors state that, for near objects, "for a given retinal image size, perceived size is proportional to distance. However, as the observation distance is increased, this correction is no longer complete, and especially so the younger the subject." This implies that the correction is a magnification which increases with increasing distance. I note that, just as logically, the correction might be a diminution which increases in absolute value with decreasing distance. This distinction bears on the following point. That the illusion is stronger for the children than the adults may be explained in two ways. Either the children misjudge the distance of the disk when it is overhead (believing it to be closer than the adults believe it to be) and "correct" just as an adult would for an object at that distance, or the children judge the distance of the overhead moon as an adult would but apply a correction different from that an adult would apply. (I take the first quotation in this letter to indicate that the authors favor the latter explanation.) Now here the sign of the correction assumes importance. If the correction is a magnification, in the above sense, then the children are not correcting enough, and the correction will increase with age. But, on the other hand, if the correction is a diminution, then the children are overcorrecting, and the correction will decrease with age.

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We agree with Howland with respect to the possible importance of postural and vestibular cues as factors influencing perceived size. It seems reasonable to assume, on the basis of developmental studies, that children may be more dependent on stimulation from proprioceptors than adults. Whether this is also true for perceived size is currently being investigated in this laboratory.

While it is logically correct to consider that the size constancy "correc-

tion" might be a diminution which increases in absolute value with decreasing distance, such an assumption would not be preferred, in view of the experimental data relating size matches to the age of the observer. Children and adults produce the same matches—that is, make a correction for distance of the same magnitude—for close observation distances. It is only by increasing distance that differences are obtained, as manifested by the more rapid falling off of the tendency toward constancy for the younger as compared to the older subjects. We have interpreted these data to imply the following relationship: the younger the subject and the greater the distance of observation, the smaller the magnitude of the correction. We suggested also that experience plays a role in this correction, although we are not certain whether it is mediated by visual or proprioceptive cues. The data obtained in the dark room, which presented some visual cues due to scattered light from the projector beams, do not permit us to make a decision at this time.

The suggestion that one investigate the role of perceived distance, with perceived size considered as a secondary effect, is both logical and tempting. Experimentally, however, it has proved to be relatively easy to obtain reliable measures of perceived size but extremely difficult to measure perceived distance, especially with children. For this reason, we prefer to avoid speculating as to whether perceived size or perceived distance is the more basic variable, and to emphasize the dependent variable of matched size, which can be assessed experimentally. We know for certain that as physical distance is increased, size matches become less veridical, but we are not able, due to methodological limitations, to determine whether these data result from a failure of perceived size or of perceived distance.

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Correlation

The recent Stetten and Hearon report, "Intellectual level measured by Army Classification Battery and serum uric acid concentration" [*Science* **129**, 1737 (1959)], gives a coefficient of correlation of +.0759, which the authors in effect say is statistically significant. From this result the authors conclude that "a low level of positive correlation . . . does indeed exist between the score attained in the ACB test and the level of uric acid in blood serum in the population studied."

Leaving aside the unusual device of reporting an estimate like this to four decimal places, we have here a coefficient of correlation of about .08. Even though the result is statistically significant, it is very doubtful that the conclusion follows. This is evidently a case of confusing statistical significance with practical or scientific significance. If the coefficient is squared, the result yields an estimate of the percentage of the common factor variance shared by the two variables. Squaring it, we get .0064, or about 0.6 percent. Thus, to be generous, it can be said that the variables of this study share about 1 percent of their variance in common! (Incidentally, the assumption of normality is not necessary for computing r, as the authors imply [see M. D. Nefzger and J. Drasgow, *Am. Psychologist* **12**, 623 (1957)]).

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In our earlier publication we found and reported a small but statistically significant correlation between score achieved in the Army Classification Battery and serum uric acid concentration in the population studied. Kerlinger apparently agrees with these findings but questions our conclusion, which is merely a restatement of these findings, that the correlation coefficient obtained is positive, small, and significantly different from zero. There is no confusion in our report between "statistical" and "practical" significance, since we have used the term *significant* only in its statistical sense. The correlation was originally examined for what appeared to us to be adequate reasons, stated in our earlier communication, and the value of r, though small, was considered worthy of publication, since it answered a question raised by Haldane [J. B. S. Haldane, *Nature* **176**, 169 (1955)].

The interpretation of the answer is left to the reader. The improvement of the estimation of one variable from the knowledge of its correlation with the other is small by any test, including the statistic r^2 employed by Kerlinger, and it was never our intention to suggest replacement of intelligence testing by serum urate analyses. Although not helpful in the prediction of one variable from knowledge of the other, the correlation might provoke inquiry into a possible biological basis. Incidentally, it was neither stated nor implied that the assumption of normality is necessary for computing r. The assumption is necessary, however, as we have clearly stated, for the test of significance which we employed.

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