and the comprehension of such content by children in the normal intelligence range is truly surprising, particularly in the light of our current expectations of children.

I have written at greater length elsewhere on this theme [see my article in *The Science Teacher* (March 1960)], and I expect to spend the next several years assessing the feasibility of elementary-school science programs based on content selection by professional scientists.

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Weather Forecasting

In his recent article "The atmosphere in motion" [*Science* **131**, 1287 (1960)], Robert R. Long has presented an interesting summary of his well-known work on the channel flow of stratified fluids, and the comparisons between theory and experiment which he presents are impressive. I think most dynamic meteorologists would certainly agree with him in stressing the need for a great deal more basic hydrodynamical research in order to strengthen the foundations of dynamical weather prediction. I also feel that he would find few who would quarrel with the statement that forecasting accuracy has improved little in the past 40 years or so, although more variables are now predicted over greater regions of the atmosphere. Long's introductory remarks on the role of numerical weather forecasting in the past decade, however, may be misleading to the general scientific reader and deserve some comment.

The numerical (or dynamic) forecasts now used subjectively by the forecasting meteorologist differ from his other sources of information in at least two important and fundamental respects. In the first place, the numerical forecasts represent the result of a systematic application of dynamical equations to the problem of large-scale atmospheric flow and are in this sense objective and reproducible. Secondly, the numerical forecasts may be (and have been) systematically improved by the introduction of more realistic models and previously neglected physical effects, as well as by improvement of the numerical procedures employed in the solutions. From a practical viewpoint the test of a forecast is, of course, its accuracy, and in this respect the present numerical predictions are disappointing in some ways. The low-level forecasts issued, for example, by the Joint Numerical Weather Prediction Unit in Suitland, Md., are not superior to those produced by the usual synoptic means; the higher-level (500 millibar) numerical forecasts, on the other hand, are now more accurate for periods up to 3 days than other comparable forecasts. This recent improvement has resulted from the systematic error reduction noted above. In view of the many physical and mathematical approximations incorporated in present operational models, I feel that their performance is more surprising than disappointing; relatively simple dynamical methods are here effectively competing with all of the synoptic calculations and intuitive skill of the forecaster.

From a broader viewpoint, the numerical integrations represent an attempt to verify the same set of basic dynamical equations with which Long is concerned, although for larger-scale phenomena, in which different physical effects are important. While the comparison of theory and observation is here poorer than in the more restricted experiments of Long, I feel there is good reason to entertain more optimism than he suggests is in order. The small but systematic improvement in the prediction of the large-scale flow is here, I believe, a significant improvement. As this scale of motion is progressively better understood, the results of research on small-scale phenomena—of which Long's studies of tornado-like circulations is an excellent example—may then be incorporated into the overall dynamical picture and should result in further systematic forecast improvement, especially for the smaller-scale motions which are closely associated with our subjective impressions of "weather."

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Binocular Fusion of Colors

In an article entitled "Colors of all hues from binocular mixing of two colors" that appeared in *Science* [**131**, 608 (1960)], the following statement was made by Geschwind and Segal. "The problem of binocular fusion of colors has interested investigators since Hecht's demonstration in 1928 that presenting red to one eye and green to the other led to a subjective sensation of yellow. . . . Hurvich and Jameson . . . confirmed these results; it is today generally accepted that such fusion is readily obtainable in most subjects."

A major finding of the article by Hurvich and Jameson cited by Geschwind and Segal was the following: "The fact that does clearly emerge from these results is that, unless there is a yellow sensation and corresponding to it some form of yellow excitatory process (whether a single or dual event) in each monocular system (and by that phrase we understand the combined receptor-neural processes), there is no mysterious, synthetic central emergence of the quality yellow. . . . When pure red and pure green stimuli are mixed, the resulting sensation is a neutral at an appropriate mixture ratio" [*Science* **114**, 199 (1951)]. Thus, our experiment did not confirm Hecht's conclusions on the binocular synthesis of yellow.

A historical bibliography of earlier papers on binocular contrast effects of the sort reported by Geschwind and Segal can be found in Parson's text on color vision [*An Introduction to the Study of Colour Vision* (Cambridge Univ. Press, Cambridge, England, 1924), p. 142].

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We regret that in pointing out some aspects of the elegant work of Hurvich and Jameson we inadvertently gave the impression that they supported Hecht's theory of yellow. The point we wished to stress was, of course, that Hurvich and Jameson had confirmed that binocular fusion of colors was possible.

We are pleased that Hurvich has had the opportunity to point out that the laws of binocular fusion of single colors do not obey the theory drawn up for them by Hecht.

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Science Reporting

I am writing with reference to the excellent article "Science reporting—today and tomorrow" [*Science* **131**, 1193 (1960)]. I thoroughly commend what has been said therein. Since the author looks into the future, let's try to improve as the future rolls around.

Why can't science reporters be trained to appreciate that there is more than one science, or better, more than one kind of science? One picks up the morning paper and reads such headings as, "A scientist discovers. . . ." or "Science finds that. . . ." There are species, genera, orders, and so forth among scientists. Is it beyond the scope of the reporters to differentiate among biologists, geologists, chemists, physicists, and so forth?

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Meetings

Free Radicals in Biological Systems

A symposium on free radicals in biological systems was held at Stanford University from 21 to 23 March under the joint sponsorship of the Biophysics Laboratory of Stanford University and the Biophysics and Biophysical Chemistry Study Section. Support was derived from the study section's special programing funds (grant RG-5048) from the National Institutes of Health, U.S. Public Health Service. The symposium was attended by 75 invited participants and included 30 papers summarizing much of the current research in this rapidly developing field.

Britton Chance (University of Pennsylvania) gave the opening paper, on free radicals in enzyme substrate compounds, and reviewed the prolific work of Leonor Michaelis, which has been responsible for much of the current interest in free radicals as naturally occurring biochemical intermediates.

Recent research on biological free radicals by other than magnetic resonance methods was discussed by H. Beinert (Wisconsin), who reported on studies of semiquinone formation by flavins and flavoproteins conducted by optical absorption spectroscopy, and by A. S. Brill (Cornell), who discussed the detection of free radical intermediates by use of a magnetic susceptibility balance incorporating a flow system. V. Massey (Sheffield) spoke on intermediates in the lipoyl dehydrogenase-substrate system as observed by optical spectroscopy.

George Pake (Stanford) discussed the general problem of applying electron paramagnetic resonance methods to the study of biological and biochemical systems and described specific approaches (sample-holder design, increase of modulation frequency, and so on) to the problems inherent in observing free radicals in aqueous, conducting systems. Electron paramagnetic resonance instrumentation was further discussed by B. Chance (Pennsylvania), who reported on a rapid-flow apparatus for spin resonance measurements, and by A. Müller (Radio-biology Institute, Karlsruhe, Germany), who described a double-cavity method for precision measurements of free radical concentration.

M. S. Blois (Stanford) reported on a series of precise g-value measurements of free radicals in solution and discussed the interpretation of g values in terms of free radical structure. M. W. Hanna (California Institute of Technology) described recent work done on the paramagnetic resonance of long polyene radicals. H. Beinert reported on paramagnetic resonance observations of semiquinone formation by flavins and flavoproteins—observations made with R. Sands (Michigan) as a parallel study to his optical measurements. L. Augustine (Atomic Energy Commission) discussed the thermoluminescence of irradiated biochemicals.

An attempted demonstration of free radical intermediates in reactions catalyzed by pyridinoproteins was discussed by H. Mahler (Indiana), and T. Nakamura (Pennsylvania) described the results of his paramagnetic resonance observations on free radicals in enzymatic oxidations. The free radical intermediates which appear during the autooxidation of dihydroxyphenylalanine were discussed by J. E. Wertz (Minnesota); L. H. Piette (Varian Associates, Palo Alto) and I. Yamazaki (Oregon) spoke on the identification of free radical intermediates during the course of the peroxidase-substrate reaction. T. Vänngard (Uppsala) described his research on the free radicals and metal valency changes occurring in the xanthine oxidose-substrate systems.

B. T. Allen (North Staffordshire) reported on some of the recent work in Ingram's laboratory and discussed the production of unpaired electrons in large molecules by ultraviolet irradiation. D. H. Whiffen (National Physical Laboratory, Teddington) spoke on the paramagnetic resonance spectra of the free radicals in irradiated single crystals of glycine and glycollic acid, and the spectra of several irradiated peptides were described by H. D. Box (Roswell Park Memorial Institute). M. L. Randolph (Oak Ridge National Laboratory) reported on his studies of the decay rates of radiation-induced free spins in crystalline amino acids, and Walter Gordy (Duke) discussed his paramagnetic resonance studies of cytochrome and hemoglobin. A paper by T. Henriksen (Norsk Hydro, Oslo) on the free radicals of irradiated thiols and disulfides was read by Tor Brustad, currently at Berkeley. Radicals and radiation damage in irradiated choline chloride were discussed by R. O. Lindblom (Dow Chemical Co., Pittsburg, Calif.)

Free radicals in systems of greater complexity were next considered. D. E. Smith (Argonne National Laboratory) spoke on free radicals in photodynamic systems, and P. B. Sogo described the paramagnetic resonance studies of photosynthetic materials carried out in Calvin's laboratory. Bernard Smaller (Argonne) discussed his findings on photo-induced free spin species in plant pigments. A. Müller read a paper describing the work in Zimmers' laboratory (Radiobiological Institute) on radiation-produced free radicals in biological systems, and Anders Ehrenberg (Caroline Institute) continued with