be more impressive. So far as we know, data of this kind have not yet been reported.

Tyler's statement concerning poor spatial resolution in the chromatic channels does not seem to add much to our understanding of these effects, especially given his comment that "large receptive fields could also occur in the achromatic channels." He is correct in predicting achromatic aftereffects. As yet unpublished data from several laboratories indicate that results commensurate with our color aftereffects can be obtained with achromatic stimuli. Threshold elevations, obtained under conditions of successive adaptation (*2*), simultaneous contrast (*3*), and interocular suppression (*4*), occur whenever the major Fourier components of checkerboards and gratings have the same orientation. Using achromatic contrast thresholds and checkerboards with Fourier frequencies as low as two cycles per degree, the aftereffect occurs on the diagonals (*2*). Thus, it seems clear that poor spatial resolution occurs with achromatic aftereffects as well as with chromatic aftereffects.

Fourier analysis has made a considerable contribution to the study of pattern perception. Without this powerful conceptualization the argument discussed here would not have occurred. The contribution of electrophysiological data to the understanding of perception has also been great; however, as this argument indicates, the exact relationships between single-unit responses and psychophysical judgments remain to be specified. Fourier components are essentially a feature of the stimulus; our experiment asked what features were salient in this situation rather than directly addressing the question of the type of mechanisms involved.

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Simple Solutions: Concentrations in the Surface Region

In an otherwise excellent article on the colligative properties of simple solutions (*1*), Andrews makes some incorrect statements about concentrations of molecules in the surface region between a liquid solution and its equilibrium vapor phase. He states that in an ideal (Raoult's law) solution "solute molecules occupy the same fraction of the surface as their mole fraction" and that in a Henry's law solution "these fractions are not necessarily the same but they are proportional" (*1*, p. 569). Neither of these statements is true in general, although one can imagine particular binary liquid solutions in which (relative) surface concentrations are the same as bulk concentrations.

Numerous adsorption studies have shown that surface concentrations of solutes (in regions where the solute obeys Henry's law and the solvent follows Raoult's law) are decidedly nonlinear in bulk solute concentration. Typically, the concentration of a surface-active solute in the interfacial region will increase to a saturation value (often corresponding nearly to monolayer coverage) at low bulk concentrations and will change very little at much larger solute concentrations or activities. To be sure, the rate of evaporation of a solvent can be considerably reduced by the presence of a monomolecular layer of a slightly soluble compound (*2*); however, the rate of return of solvent molecules to the bulk solution will be simultaneously reduced by (nearly) the same fraction.

The major point is that, although the kinetics of evaporation and condensation are quite complicated, the arguments developed by Andrews can be made (and in fact are made throughout the rest of Andrews's article) solely in terms of the reduction of solvent chemical potential (μ_1) caused by the presence of a dissolved solute. Stated somewhat differently, the kinetics of evaporation and condensation must be consistent with the thermodynamics, but the actual mechanism of these processes and the changes that occur in the molecular composition of the surface region are of little interest in relation to the thermodynamic treatment of colligative properties.

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2. See, for example, V. K. La Mer, Ed., *Retardation of Evaporation* (Academic Press, New York, 1962).

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I thank Christian and Tucker for calling attention to the molecular model I chose to illustrate ideal vapor pressures in one paragraph of my article. I did not make clear the fact that the model I used was simply an example, chosen because it was easy to understand. Their point is well taken that in many cases more complicated models are required to explain the mechanism of these phenomena, although the overriding thermodynamics is thoroughly understood.

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