latable by contemporary psychophysical procedures such as payoff changes.

The striking similarity in the shape of the RT and TO functions was surprising to us. It strongly suggests that the motor component in the RT task adds little variance relative to variability in receptor system latency. Since any variance in the motor component is doubled in the difference distribution, the predicted psychometric function should appear shallower than the obtained. As this is not the case, it is likely that motor component variability is low. An alternative view would require that the contribution of motor variance in the prediction be just offset by an additional source of variability in the TO performance.

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3. This may be seen by defining a new density, $f^*_2(-t_2) = f_2(t_2)$, over the negative real line. Then $F^*_2(-t_2) = 1-F_2(t_2)$, and substitution in Eq. 1 produces the convolution integral for t_1-t_2.
4. D. M. Green and J. A. Swets, *Signal Detection Theory and Psychophysics* (Wiley, New York, 1966), pp. 43–49.
5. A minimum of 5 (condition III) and a maximum of 18 (condition I) sessions were run under each luminance combination, with the first session for each condition not included in the data analysis. The obtained TO functions were adjusted for intersession variability by shifting the functions from individual sessions so that 50 percent points were superimposed upon the average of the 50 percent points for all sessions within a given condition, and the resulting data were pooled. The RT functions were adjusted in a similar manner by shifting all RT distributions from individual sessions so that their means were superimposed upon the grand mean of all sessions within a given condition. The resulting pooled RT distributions for fovea and periphery were then used to calculate the RT difference distribution from the discrete analog of Eq. 1

$$P(k) = \begin{cases} \sum_{i=1}^{N} P_1(i+k) \sum_{j=i}^{N} P_2(j), & k \leqslant 0 \\ \sum_{i=1}^{N} P_1(i) \sum_{j=i+k}^{N} P_2(j), & k > 0 \end{cases}$$

where k is the τ category, N is the τ category for the largest τ, and $P_s(i)$, $s = 1, 2$, is the probability that a latency from receptor s falls in category i.
6. W. J. McGill, in *Handbook of Mathematical Psychology*, R. D. Luce, R. R. Bush, E. Galanter, Eds. (Wiley, New York, 1963), pp. 309–360; W. J. McGill and J. Gibbon, *J. Math. Psychol.* **2**, 1 (1965).
7. The goodness-of-fit test was adapted from D. J. Finney, *Probit Analysis* (Cambridge Univ. Press, London, ed. 2, 1952). The criterion statistic is the squared normalized binomial variate with expected values calculated by interpolation from the predicted functions. The reliability levels reported in the text are for χ^2 computed with the extreme upper and lower points of the obtained functions omitted. The number of trials run at these τ values was considerably lower than for intermediate points (an average of 25.6 per point for the extremes versus 132.1 per point for intermediate points). Consequently the middle ranges of the obtained functions are more reliable than the extremes. When the extremes are included in the χ^2 computations, all function pairs differ at well below the .001 level.
8. The predicted functions were shifted by an amount equal to a weighted sum of the difference between interpolated medians for the two functions. The weights used were the proportion of the total N in each condition. The resulting values were −19.07 msec for subject JT, conditions I, II, and III; +8.40 msec for subject BM, conditions I and II; and −5.25 msec for subject BM, condition III. Adding these constants to τ in the predicted $F(\tau)$'s, resulted in χ^2 values with associated probabilities above .25 with the extremes of the obtained functions omitted. When the extreme values were included, these probabilities fell to between .1 and .05 for subject JT, and between .05 and .025 for subject BM. Iterative procedures would undoubtedly improve these estimates, however the difference between medians appeared to provide a reasonable first approximation to the data. Moreover, it is difficult to decide on an appropriate test size when a discrimination is attempted between a model and no model. Goodness-of-fit levels carry more weight against sharp alternatives.
9. Supported in part by NINDB grant NB 05221, PHS grant MHO 3616, and NIH grants MH 07279 and GRS 5-SO-1-FRO 5650. Dr. J. Kerr wrote the computer programs used for data analysis.

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Coevolution

I challenge Muller's interpretation (*1*) of the coevolution of plant and animal interactions in that: (i) He has cited no direct evidence (and neither is any evidence available for the vast majority of defensive compounds) that the secondary substances are "primarily metabolic wastes." It is hard to understand how many compounds (for example, alkaloids, free amino acids, saponins, glycosides, and so forth) virtually unique to plant metabolic systems can be considered waste products, when animal metabolic systems do quite well with very few kinds of waste products (except for sessile marine animals which are known to contain many of the same compounds Muller regards as plant waste products). (ii) The need to void, sequester, or otherwise render a toxic compound unavailable to the producing organism is not evidence that the toxic compound is a waste product. By Muller's line of reasoning, the defensive compounds of animals must also be waste products. (iii) Natural selection serves as a mechanism by which a population of herbivores may "call forth *de novo*" the evolution of a biosynthetic pathway producing compounds toxic to the herbivore. Obviously, initial stages in such a pathway may arise through mutation, or other genetic changes, just as do initial stages of any other biosynthetic pathway. For selection for the production of a toxic compound, all that is required is that the new form of compound in the mutant plant strain be slightly toxic, deterrent, hallucinogenic, distasteful, sleep inducing, and so forth, to the herbivore. (iv) The failure "to regard such [secondary compounds] as primarily animal [and other plant] toxins renders impossible the explanation of how these products came to be." What other selective force in the environment besides herbivores (*sensu latu*) and competing plants has the diversity of quality, yet specific persistence, of environmental challenge to lead to essentially a unique combination and array of secondary compounds for each species of plant? (v) Acetylcholine, bile, trypsin, and vitamin A are toxic to animals in large doses. Muller's reasoning would lead to the conclusion that these are metabolic waste products, because they would intoxicate the system if not eliminated or used. That a complex compound is a potential intoxicant of the system producing it can hardly be taken as the definition of a waste product.

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2 May 1969

The debate initiated by Ehrlich (*1*), and now joined by Janzen, began with objection to my interpretation of the nature of toxic compounds released by plants and effective against other plants. The implication of both critics that there exists no difference between those toxins effective in plant-plant and those effective in plant-animal interactions is too simplistic to fit the facts and is unduly emphatic in its unswerving zoocentricism. This stance is, of course, necessary to their thesis that animals somehow cause plants to initiate novel metabolic pathways (where I have ascribed to animals the role of selecting between existing pathways by means

of herbivore pressure). Janzen chooses to disregard this latter statement of mine (*2*) reiterating it as his own. His objections are here answered. (i) I have never said that the compounds he cites are all "waste products." But some glycosides (for instance, arbutin and amygdalin) are harmless compounds of deadly toxic moieties which eventually harm the species that too freely deposits them in its environment, as in the peach "replant problem" described by Patrick (*3*). (ii) The production of immense quantities of a voided toxic compound widely dissipated (as are the volatile terpenes) and the heavy concentration of many nonvolatile toxins in senescent leaves or other deciduous organs are indeed indicative of waste products. Animals avoid autointoxication by similar means, even if some species do hide behind their excrement for protection (*4*). (iii) Janzen's understanding of to "call forth *de novo*" is quite the opposite of mine. I had always assumed that biological characteristics originated ("*de novo*") as mutations, as I have consistently said, and that herbivores then applied selective pressure. (iv) I do not see how anyone could read the last paragraph of my challenged paper and fail to understand that I have said the same thing. I regret that the distinction between the qualities in common of "mutation," "*de novo*," and "primary" on one hand and those of "selection pressure" and "secondary" on the other hand is so difficult to explain. (v) In the condition "if not eliminated or used" lies the crux of the problem. Any chemical compound produced by an organism which is autotoxic if not eliminated is a metabolic waste. Any compound that is metabolically used, whether toxic or otherwise, is not, to the extent that it is used, a waste. *Qua re haec de lana caprina?*

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13 June 1969

X-ray Integrated Reflection Coefficient of Lithium Fluoride

Meekins *et al.* (*1*) have measured the x-ray spectrum of solar flares using Bragg crystal spectrometers aboard the orbiting solar observatory OSO-4 spacecraft. In order to reduce the data to photon flux units, it was necessary to determine the integrated reflection coefficient R for LiF crystal.

In figure 2 of (*1*) a function designated "integrated reflectivity of LiF" is plotted against wavelength. Examination of their references [see (*2*)] suggests that this is the quantity usually called the integrated reflection coefficient and is identical to the function R_m below. However, their evaluation appears to be in error and cannot be calculated from diffraction theory, including their references (*2*). This error should be corrected, because LiF is a useful crystal for x-ray diffraction and will be used more widely in applications where knowledge of the variation of R with wavelength λ is necessary. Furthermore, diffracted intensity from actual LiF cannot be predicted from theory and must be experimentally determined with the actual crystal used. Examples for a typical crystal are shown and are ¼ to ⅙ the values cited in (*1*).

Diffraction theory (*2*) predicts the limiting cases of x-ray diffraction from a perfect crystal and from a mosaic crystal. Equations for the integrated reflection coefficient are

$$R_p = \frac{4}{3\pi} N\lambda^2 |F| r_e \left\{\frac{1 + |\cos 2\theta|}{\sin 2\theta}\right\}$$

$$R_m = \frac{N^2\lambda^3}{4\mu} |F|^2 r_e^2 \left\{\frac{1 + |\cos^2 2\theta|}{\sin 2\theta}\right\}$$

where θ is the Bragg angle; λ is the wavelength; N is the number of scattering units, for LiF N is 1.53×10^{22} cm^{-3}; F is the structure factor, and for the (200) reflection from LiF at 22°C F is 29.5; r_e is the classical electron radius, 2.82×10^{-13} cm; and μ is the mass absorption coefficient (*3*). These functions are plotted as solid heavy lines in Fig. 1. The dotted line is from figure 2 in (*1*) and is about 1.5 times too large. The point 0 on the R_m curve at 1.54 Å is due to a calculation by Renninger (*4*). Diffraction measurements from LiF do not agree with either case and fall somewhere in between, varying with each crystal and with surface treatment. Thin lines in Fig. 1 plot data for a typical crystal 1 cm thick with a fresh cleavage face on one side and an abraded surface on the other prepared by polishing on graded paper to a final 600-grit polish, which is a typical treatment for commercial spectrometer crystals. The data was obtained by integrating $(1, -1)$ rocking curves at many points in the indicated wavelength range and correcting to account for the partial polarization of the x-ray beam by the first crystal. The full width at half maximum of the rocking curve was 1 to 2 minutes of arc and 5 to 6 minutes of arc for the cleaved and abraded crystals, respectively (*5*).

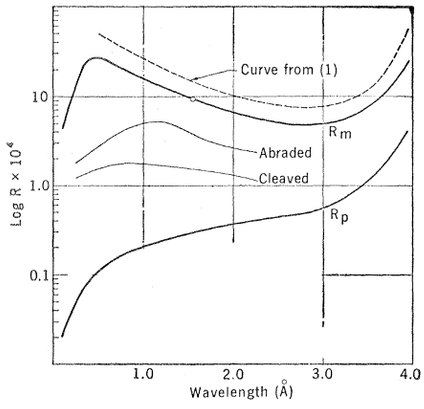

Fig. 1. Integrated reflection coefficient with unpolarized incident x-radiation for LiF (200) calculated for a mosaic crystal R_m and for a perfect crystal R_p. Thin solid lines are experimental data for cleaved and abraded crystals. The dotted curve from (*1*) is questioned.

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5. The value of 20 minutes of arc in (*1*) appears to be much too large for LiF.
6. I thank D. E. Sayers and R. G. Bingham for assistance in the experimental work and valuable suggestions.

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