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Book Review

The Psychophysics of Detection

N. V. S. GRAHAM. **Visual Pattern Analyzers.** Oxford: Oxford University Press, 1989. Pp. xv + 646, $95.00

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A principal tool of psychophysics is the measurement of the threshold of detectability of a stimulus. The application of detection experiments to the analysis of spatial vision has been extensively reviewed in a recent book by Norma Graham (1989), focusing on sinusoidal patches as a basis set from which all other patterns can be composed. She identifies no less than 92 distinct assumptions involved in detection studies, many of which are tacit in the original publications and are made explicit in her extensive review of the field. The book is much more than a review,

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it is a exceptionally thoughtful overview of the many nuances of the psychophysical decision-making process.

In a sense, Graham's book is about the mind-body problem: how to connect subjective sensations (the mind) to the activity and interactions of underlying mechanisms (the body). Except for Part 1 (Introduction) and Part 5 (Identification), the subjective sensation in question is simply "Is anything visible?" The observer's task is detection, involving a discrimination between a blank screen and a screen with a faint pattern. No book does a better job than Graham's in showing how much "gold" can be mined from the simple detection task. As is made amply clear in Parts 3 and 4, however, the connection between the detection sensation and the underlying mechanisms requires many assumptions. It is Graham's goal to present a carefully thought out, logical organization that gives the experimenter a blueprint for how to determine which of the assumptions are valid. For anyone interested in how introspection (psychophysics) can be used to learn about neural processing, her book should be required reading.

It was a good idea to focus the book on detection and ignore the further complications of suprathreshold psychophysics. Detection experiments are complicated enough, as will be seen in our overview of the 13 chapter which Graham organized into six parts. After reviewing each of the six parts, we will have a section titled: *"What would we add?"* This section allows us to expand upon small details in Graham's book that we believe will be of interest to readers of this journal and we hope of interest to Graham for future editions of her book. Our goal in examining these issues (see especially our critique of present methods of probability summation) is to point out several fundamental problems facing mathematical psychology. Our intention is to be provocative to encourage further research in these areas.

1. THE INTRODUCTION

Two chapters comprise the Introduction. The first is a fine overview of the array of psychophysical techniques that makes it possible for the subjective appearance of cleverly chosen stimuli to provide information on the properties of the underlying physiological mechanisms. Very quickly Graham's strengths become apparent. Her careful definitions (e.g., distinguishing analyzer, neuron, mechanism, channel, and element) remind us how easily sloppy thinking can take over when the words are not clearly defined.

The second chapter is titled "Some Mathematics." This chapter sets the tone and level of the book. Although Graham discusses topics that are deeply mathematical throughout the book, she made a decision to keep it simple. The mathematics is mainly about linear filters, convolutions, Fourier transforms and such. The minimal mathematics will greatly please readers with scant fondness for integral equations and elegant formalisms. Although patrons of this journal may wish for more mathematics to make the formalism explicit, this is not a big problem since the many references allow one to go to the original sources. Graham's thought-provoking overviews of a wide range of topics do not require detailed derivations. This book is solidly in the tradition of mathematical psychology. The formalism connecting the assumptions and the data is well laid out.

What Would We Add to Part 1?

Graham discusses only the Gabor basis functions, but not their problems nor alternative basis functions. The receptive field shape of a Gabor patch is given by (Graham, Eq. (2.9))

$$G(x) = \exp(-x^2/2\sigma^2)\cos(f_0 x + \theta), \tag{1}$$

where 9 is the relative phase between the carrier frequency $/_0$ and the peak of the Gaussian envelope. The Fourier transform of the Gabor function is

$$F_G(f) = \exp(-i\theta)\exp\{-(f - f_0)^2\sigma^2/2\} + \exp(i\theta)\exp\{-(f + f_0)^2\sigma^2/2\}. \tag{2}$$

In Graham's version of this formula (Eq. (2.39)) there is a sign error in the argument of one of the phase terms.

Since many vision researchers use Gabor functions both as stimuli and for modeling, it is useful to point out their problems. One bothersome property of Gabor functions is that the magnitude of the Fourier transform depends on the phase, *6:*

$$\begin{aligned} |F_G(f)|^2 &= \exp\{-(f - f_0)^2\sigma^2\} + \exp\{-(f + f_0)^2\sigma^2\} \\ &\quad + 2\cos(2\theta)\exp\{-(f^2 + f_0^2)\sigma\} && (3) \\ &= 2\exp\{-(f^2 + f_0^2)\sigma\}\{\cosh(2ff_0\sigma^2) + \cos(2\theta)\}. && (4) \end{aligned}$$

The phase dependence makes it risky to use Gabor functions in modeling calculations since they can give nonzero response to uniform fields. This DC response plays havoc with any attempt to normalize the sensitivity of a mechanism in terms of the contrast sensitivity function.

One solution to the above problem is to use Cauchy functions instead of Gabor functions for stimuli and modeling. The Cauchy function receptive field (derivatives of the Cauchy pole $(x + i\sigma)^{-1}$) given by Klein and Levi (1985) is

$$C_n(x) = \cos^n(\varphi)\cos(n\varphi + \theta), \tag{5}$$

where $\varphi = \tan^{-1}(x/\sigma)$. The two factors in Eq. (5) are the envelope and carrier, similar to the two factors in Eq. (1). One of the advantages of the Cauchy function is that the magnitude of its Fourier transform is independent of the phase θ:

$$|F_{Cn}(f)| = f^{n-1}\exp(-f\sigma). \tag{6}$$

Equation 6 is much more appealing to work with than the Gabor function in Eq. (4). For narrow bandwidths (n>10 for Cauchy functions, and $f_0\sigma > 1$ for

Gabor functions) there is minimal difference between the functions; however, for medium and broad bandwidths the Gabor functions should be used very cautiously. Graham's (and most other vision researchers') general acceptance of Gabor functions should be reconsidered.

The Gabor model has also been evaluated negatively as a description of empirical data. Hawken and Parker (1987) found that the poor fit of the Gabor model to data from monkey striate cortex could be improved by use of a difference-of-offset-Gaussian model. Stork and Wilson (1990) obtained a similarly poor fit of the Gabor model to human psychophysical masking data, which were adequately described by a difference-of-Gaussian model. One of the reasons that the Gabor function does not provide a good description of visual receptive fields is that for the Gabor function the inhibitory side lobes have the same width as the central excitatory lobe. However, both psychophysical and neurophysiological evidence point to side lobes that are broader than the central zone. Difference-of-Gaussian, derivative-of-Gaussian and Cauchy functions all have larger side lobes and are thus better candidates for visual receptive fields.

There is an interesting recent controversy about which basis functions are most local in both space and spatial frequency. Daugman (1975) suggested that complex Gabor functions minimized the "Heisenberg" product of the spatial variance times the spatial frequency variance. For real-valued functions. Gabor (1946) and Stork and Wilson (1990), using a variational principle, claimed that Hermite polynomials times a Gaussian have minimum joint space-spatial frequency uncertainty. Gabor and Stork and Wilson made an amusing mistake. They did find the real-valued functions that produce an extremum of an uncertainty relation. Unfortunately, as shown by Klein and Beutter (1991) they found a maximum! That is, the Hermite functions are the *worst* functions to use if one wants to minimize the joint uncertainty in space and spatial frequency for the class of functions that are a polynomial of a fixed order times a Gaussian.

2. ADAPTATION

Adaptation experiments provide an excellent beginning to the empirical part of Graham's book since these experiments have a special place in the recent history of spatial vision. The Blakemore and Campbell (1969) experiment on selective adaptation was the turning point in spatial vision research because of the connection made between psychophysics and physiology. The title of their paper says a lot: "On the Existence of Neurones in the Human Visual System Selectively Sensitive to the Orientation and Size of Retinal Images." They used psychophysics to measure properties of "neurons," and found that the spatial frequency and orientation bandwidths obtained using selective adaptation agreed with the bandwidths of visual neurons. Graham was wise to put this section at the beginning to provide an aura of solidity for her enterprise.

Nevertheless, Graham makes it very clear that many assumptions are still needed to connect psychophysics to the properties of underlying mechanisms. Her honesty in laying out these assumptions is sobering. Twenty-three assumptions are to be found in the adaptation chapter. These are assumptions about receptive field shapes, about how information from the multiple mechanisms is pooled and about the nonlinearities underlying adaptation. She then shows the predicted consequences of the different assumptions and compares the predictions to data. Unfortunately there are conflicting data on the cause of adaptation (is it fatigue or sustained inhibition?), which has not been resolved even though it has been more than 20 years since the Blakemore and Campbell paper appeared. The issues are important enough to make us want to stop writing this review and get back to the lab to try to resolve some of the conflicting data. This scenario occurs throughout her book. It should be inspiring reading to many graduate students interested in clearing up the confusion associated with connecting perceptions to mechanisms.

What Would We Add to Part 2?

(1) The focus of Graham's book is on detection, but discrimination may be of greater interest. As she says on p. 127, "Discrimination among suprathreshold patterns is also affected by adaptation and is perhaps a better place to look for clues as to the function of adaptation." Adaptation produces many aftereffects on *suprathreshold* perception. The detectability of a contrast increment is progressively reduced on a suprathreshold pedestal. The perceived spatial frequency of a grating shifts after adaptation. So does the ability to discriminate spatial frequencies. The spirit of the spatial frequency channel story is that one should be able to predict the tuning of the spatial frequency shift and of spatial frequency discrimination tasks based on the tuning of the channels as revealed in detection studies.

In a remarkable paper, Wilson and Regan (1984) made a "blind" prediction about spatial frequency discrimination after adaptation. The way it worked is that Regan told Wilson all the experimental details and Wilson made his prediction before seeing the data. Now that's the way science is supposed to work. The agreement between theory and experimental data was quite good, which is impressive since the results are unusual in showing that the peak threshold elevation for the discrimination task is shifted upward in spatial frequency by as much as an octave. This shift would not have been predicted by a naive theory. This is the type of research that makes science exciting and helps one believe in the filter model of spatial vision. Closer examination of the model fit to the data shows that there is still some room for improvement, since the actual discrimination threshold elevation extends to higher spatial frequencies than does the prediction. The same discrepancy was found by Klein, Stromeyer and Ganz (1974) for the spatial frequency shift. The spatial frequency shift extends for more than two octaves away from the adapting frequency, whereas the Blakemore-Campbell adaptation extends for only about one octave. It is easy to postulate that the perception of spatial frequency occurs at a later stage with a broader bandwidth. This postulate would, however, undermine the original dream that the spatial frequency channels of Blakemore and Campbell could be used to account for all spatial perception.

(2) One of the fascinating results discussed by Graham (there are eight citations in the index to this topic) is the finding by DeValois (1977) that, after adapting to a sinusoid, thresholds are *facilitated,* not elevated, at frequencies about two octaves from the adapting pattern. This result causes difficulties for simple models that explain adaptation as the fatigue of the responding mechanisms. More complicated models involving inhibition between spatial frequency channels might explain the results. Graham makes the comment that all the authors who report a facilitation effect after adaptation used the method of adjustment. If Graham were writing her book today, she would undoubtedly include the result of a new study of the facilitation effect by Greenlee (1990) using signal detection methodology. He found no facilitation! If Greenlee's result is correct, then Graham's chapter could be shortened, to the delight of many people who would like to keep the models simple.

(3) Klein and Stromeyer (1980) found surprising results in their study using a first-plus-third harmonic adapting pattern. They found, as had others previously, that the presence of the first harmonic suppressed the adapting power of the third harmonic. The new feature of their study is that, when the relative phases of the first and third harmonic were decoupled either by relative motion or by relative random jitter, the full adapting power of the third harmonic came back. Two explanations were offered: (a) phase inhibition between mechanisms tuned to different phases or (b) a principle that "what you see is what you adapt." When the first and third harmonic were phase locked the third harmonic does not "look" like a third harmonic. The relative motion made the third harmonic look like a separate grating, so its adapting power returned. This would also explain why the adapting power of a fifth harmonic was not suppressed (Klein & Stromeyer, 1980), since it always "looks" as though it is present. It would have been useful for Graham to have mentioned these notions to stimulate others to follow them up.

(4) What happens to the shape of the psychometric function after adaptation? Williams and Wilson (1983) claim that the psychometric function gets steeper after adaptation. The psychometric functions shown by Klein and Stromeyer (1980), however, seem to keep their slope of 2 (plotting log d' vs log contrast). Further experiments are needed to clarify this question. The shape of the psychometric function provides a sensitive clue to the nature of the adaptation process, so it would have been nice if Graham's book had a section to inspire further experiments to clarify this area.

3 AND 4. SUMMATION AND UNCERTAINTY

These two sections are closely related, so they will be treated together. Summation and uncertainty must be dear to Graham's heart since her own research is centered in these areas. The Graham and Nachmias (1971) study on subthreshold summation was one of the early papers that provided the foundation for the spatial frequency paradigm that was to dominate the decade of the 1970s. They showed in that paper that the brain combined a first and third harmonic according to probability summation rather than linearly. Since that paper 20 years ago, Graham has been concerned with probability summation. The work that she did with her students Beth Davis and Pat Kramer showed that the effects of uncertainty cannot be separated from the effects of summation. Graham's book provides by far the best review of this tricky area.

As was stated at the beginning of this review, the big problem with psychophysical modeling is that many assumptions are needed to make the connection between the subjective sensation that the screen is or is not blank and the activity of the underlying mechanisms. Parts 3 and 4 of the book provide a clear view of Graham's approach to untangling the multiplicity of assumptions. There are assumptions about: (1) the shape of the mechanisms in space or spatial frequency, (2) the shape of the psychometric function (transfer function), (3) the decision rule (signal detection vs high threshold), (4) the role of uncertainty, (5) the role of attention, (6) the role of subjective bias, (7) the effect of nonlinearities, (8) the effect of correlations in the activity of the mechanisms, (9) the shape of the noise distribution, and (10) the stage at which the noise enters the detection channel. Unfortunately, these assumptions are not independent of one another. A change in any one of these factors can have an effect on other factors.

Suppose that one is mainly interested in the first item of the above list, since the mechanism shape is the item most easily connected to physiology. Graham provides ample evidence that one's conclusions about mechanism shape depend *strongly* on all the other factors. This is very different from the view of most reseachers who believe that most of the above factors play only a minor role. Consider, for example, the finding of Sachs, Nachmias and Robson (1971) that a pair of gratings of similar spatial frequency do not summate well. If spatial pooling (items 2 and 3 of the preceding paragraph) were ignored then one would conclude that the underlying mechanisms must have very narrow spatial frequency tuning. However, Stromeyer and Klein (1975) showed how the appropriate transducer exponent could explain the Sachs *et al.* data with pooling of medium or broad bandwidth mechanisms.

Gorea and Tyler (1986) have done similar calculations in the temporal domain to show how one's estimate of the temporal impulse response is strongly dependent on assumptions that are made about how to do probability summation and about the span of attention. They analyzed data showing that temporal integration for pulse stimuli continues for at least 500 ms at high spatial frequencies, but is complete by 100 ms at low spatial frequencies. These differences could largely be explained by the effect of probability summation on the type of impulse response (monophasic versus biphasic), rather than a fivefold difference in impulse response duration or attentional duration.

The book contains discussions of the detailed predictions by Kramer, Graham and Yager (1985) on the effects of the mechanism shape, the decision rule (maximum output, sum of outputs), type of attention, and noise. As Graham shows, by controlling the amount of uncertainty (blocked vs unblocked designs) and the number of stimulus components, it is possible (but difficult) to disentangle these interwoven issues. It would have been useful to see the data that test the many assumptions. We fully agree with the importance that Graham places on these issues. There are, however, a few points on which we differ, as will now be discussed.

What Would We Add to Parts 3 *and* 4?

Questioning the precise shape of the psychometric function or the exact amount of summation may seem of minor importance, like "dotting the *i* and crossing the *t*". However, in connecting the output of individual neurons to the behavior of the whole organism, the precise nature of summation takes on central importance. The question of how to carry out probability summation calculations must be answered before one can connect psychophysics to physiology. Graham's book lays out many of the assumptions that are involved, but more work is still needed before one can have confidence that the calculations are being done properly. We have eight suggestions to make.

(1) We would have emphasized that there are two quite different connections that can be established between the shape of the psychometric function and the amount of summation. Throughout Parts 3 and 4 probability summation is calculated by using the formula proposed by Frank Quick (1974):

$$S=\left(\sum S_i^q\right)^{1/q},$$

where S, is the stimulation of the z'th mechanism (it is the stimulus contrast times the mechanism sensitivity), q is the Quick pooling exponent, and S is the total stimulation (threshold is defined to be at $S = 1$). Equation 7 can be derived by two very different methods:

Method 1 is based on the Weibull form of the psychometric function

$$\text{probability correct} = 1-2^{-S^\beta}. \tag{8}$$

Quick (1974) showed that a "high threshold" theory connects the pooling exponent, q, to β by

$$q=\beta \qquad \text{(based on high threshold theory).} \tag{9}$$

Method 2, proposed by Stromeyer and Klein (1975) and Gorea and Tyler (1986), is based on signal detection theory where the assumption of independent signals in noise leads to the summation rule given by

$$d'^2_{\text{tot}}=\sum d_i'^2.$$

If the transducer function (which is what the psychometric function is called when d' rather than probability is used to represent percentage correct) is a power function of stimulus strength, i.e.,

$$d' = S', \tag{11}$$

then the connection between the summation exponent, q, of Eq. (7) and the power, t, is

$$q = 2t. \tag{12}$$

Pelli (1985, 1987) showed that to a good approximation t 0.8β. With this relationship, the connection between q and the psychometric function is

$$q = \quad 1.6\beta \quad \text{(based on signal detection theory).} \tag{13}$$

A comparison of Eq. (9) with Eq. (13) shows that different assumptions about the underlying detection model produce a different relationship between the slope of the psychometric function and the amount of summation.

The problem with using Quick's approach is that it is based on the high threshold assumption. Graham shows that it can also be derived from a maximum-output model. There is evidence, some reviewed by Graham, that these models are both wrong. It is therefore worrisome to see that she built the huge edifice of how to do summation over multiple mechanisms on flimsy assumptions. Graham waits until almost the end of Part 4 to mention the connection between the transducer exponent and the pooling exponent (Eqs. (10)-(13)).

The connection between the psychometric function and the amount of pooling has been discussed in the past. One of the points made by Stromeyer and Klein (1975) was that the pooling exponent implied by summation experiments (q) seemed to be lower (more pooling) than the exponent predicted by the psychometric function (β or t). This result would argue against the signal detection approach which, as shown in Eq. (13), produces *higher* rather than lower values of q. Robson and Graham (1981) claimed that the pooling exponent, q, and the Weibull exponent, β, were both approximately 3.5, an apparent vindication of high threshold probability summation. The value of $\beta = 3.5$, however, seems larger than is often found. If we use the signal detection approach for the Robson and Graham data rather than the problematic high threshold theory or maximum-output model, then based on pooling, the transducer exponent (see Eq. (12)) is $t = q/2 = 1.75$, while based on the shape of the psychometric function the transducer exponent is $t = 0.8\beta = 2.8$ (see Pelli, 1985 and 1987 for a derivation and discussion of the 0.8 factor). The difference between 1.75 and 2.8 indicates that there is more summation than is expected from pure probability summation according to signal detection theory. Given the discussion on p. 299 of her book, Graham is aware of this problem. It would have been appropriate for her to include this point in her list of discrepant findings on p. 176. It is tempting to conclude that the agreement between

$q = 3.5$ and $\beta = 3.5$ vindicates the high threshold approach. However, we believe that there is enough evidence against the high threshold theory and the maximum-output rule that one should be cautious. Furthermore, $\beta = 3.5$ corresponds to / = $0.8\beta = 2.8$, which is about 50% larger than the power exponents that we typically find in detection studies. It is worth remembering that an ideal observer should exhibit more summation than what would be expected from probability summation.

(2) Graham (1989) and Pelli (1987) have discussed the equivalence between the transducer and the Weibull form of the psychometric function, but to our knowledge the two forms have not been compared graphically. Fig. 1 provides such

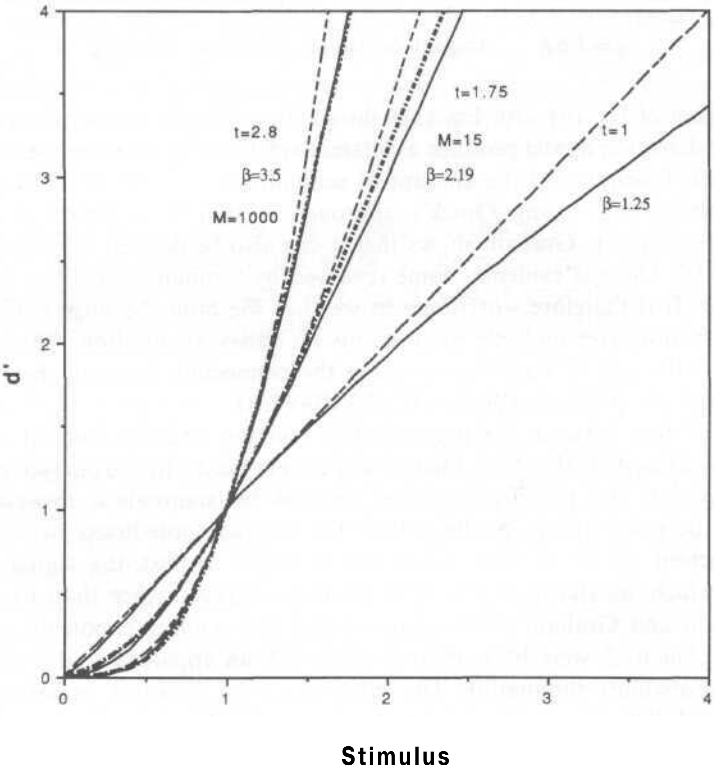

FIG. 1. Weibull vs Power vs Uncertainty. Three forms of transducer functions (psychometric functions) showing d' as a function of stimulus strength, S. The dashed lines are given by the power function $d' = S^t$ for $t = 1$, 1.75, and 2.8 as discussed in the text. The solid lines are the Weibull functions given by $p_c = 1 - 2^{-S^\beta}$, with p_c converted to a d' according to the 2AFC formula $d' = \ 2z_c$, where z_c is the z-score corresponding to p_c. The values of β are 1.25, 2.19, and 3.5 obtained from Pelli's relationschip $\beta = 1.25t$ for the above choices of t. The dot-dashed curves are the psychometric function produced from uncertainty assumptions as analyzed by Pelli (1985), with M=15 and M=1000 attended channels, one of which has a signal, in a 2AFC paradigm. The $t = 2.8$, $\beta = 3.5$, and M=1000 curves are quite close to each other as are the $t = 1.75$, $\beta = 2.19$, and $M = 15$ curves.

a plot for Eqs. (8) and (11). The dashed curves are power functions given by Eq. (11) for transducer exponents of $t=1$, 1.75, and 2.8. The latter two are the exponents discussed in the preceding paragraph. The solid curves are the matching Weibull functions (according to Pelli, 1985) with $\beta = 1.25t$, corresponding to values of $\beta = 1.25$, 2.19, and 3.5. The dot-dashed curves are based on an uncertainty model and will be discussed later. All the curves have been normalized to have $d' = 1$ at threshold ($S = 1$). The slope of the Weibull curve matches the slope of the power curve at $d' = 1$. However, at higher and lower d', the Weibull function lies below the matching power function. As Pelli showed, the two curves are so close that it will be difficult experimentally to distinguish between the Weibull and the closest matching power function. It should be possible, however, to distinguish befween power exponents of 1.75 and 2.8 that arise from the connection between summation and the slope of the psychometric function as discussed earlier.

(3) There is another method that should allow subtle psychometric-function shape differences to be measured. Pelli (1987) pointed out that it might be possible to discriminate between similar shapes by comparing the slopes before and after summation. Assessing the slope (first derivative at threshold) has greater statistical reliability than assessing the shape difference (higher derivatives) between two functions whose slopes at threshold are the same. If the slope is unchanged then the Weibull form is implicated by assuming high threshold summation, or a power form is implicated by assuming signal detection summation. If the psychometric function changes shape, however, then a different psychometric function or summation rule is operating. If the transducer function is not a power function but rather has a decreasing effective exponent in the region of interest (as is expected to the right of the "dipper" regime), then we might expect to find that at threshold the effective exponent is larger for detecting a large grating patch compared to a small grating patch. Data on the change of exponent with stimulus size are not clear. The data of Mayer and Tyler (1986) were consistent with the hypothesis that the exponent is independent of stimulus size. However, almost all their data show an increased exponent as the stimulus increases from 2 to 8 bars. Cohn (personal communication), however, found that the exponent decreased with increasing stimulus area or duration, albeit in a situation with presumably large uncertainty (increasing the stimulus extent decreases uncertainty and thus decreases β). If the exponent changes with stimulus extent, then the probability summation calculations of Robson and Graham (1981) are suspect. They used the exponent measured for a small grating patch. It would have been more correct to use the exponent for the full patch since the summation is actually occurring in the low contrast regime. We have gone into these details concerning the connections between summation and the psychometric function to inspire others to look closely at summation data and summation theory so that these concepts can be placed on a more solid footing.

(4) Graham's Part 3 and 4 are about summation and uncertainty. It would have been nice if, after reading these sections, one would know how to do summation calculations from within an uncertainty-based theory. Graham could have analyzed

Pelli's (1985) discussion of the connection between uncertainty and summation. Pelli suggests that Weibull functions and power functions are quite similar to the psychometric functions predicted by uncertainty, so to a good approximation one could use the summation formula, Eq. (7) with $q = \beta$ or $q = 2t$, as discussed in the preceding section. The missing ingredient is a picture showing the similarity of the three psychometric functions. This missing ingredient is supplied in Fig. 1, where we plot three types of psychometric functions in terms of d' as a function of stimulus strength: the Weibull function (Eq. (8)), the power function (Eq. (11)), and the uncertainty function (Eq. (4.9) of Pelli, 1985). The uncertainty functions that are plotted are for $M = 15$ and $M = 1000$, where M is the number of channels that are attended. These two values were chosen to provide fairly good matches to the Weibull functions shown in Fig. 1. Our plots are in good agreement with Table 1 of Pelli (1985). One item is still missing, however. A calculation is still needed for the effect of uncertainty on summation using the exact uncertainty formulation (Eq. (4.9) of Pelli, 1985) to see whether it agrees with the summation formula (Eq. (7)) and, so, if whether q is determined by β or by t as we have discussed! We suspect that the uncertainty formulation should show that the effective β is reduced with summation because the number of "irrelevant" channels should be reduced. The value of β should be reduced as uncertainty is reduced by having more of the channels being "relevant." Thus with summation, as one moves to a lower point on the psychometric function, one might expect a lower effective slope. By plotting Fig. 1 on log-log coordinates we found that the Weibull psychometric function has the opposite behavior, the slope gets steeper at lower stimulus strengths. Thus we conjecture that summation with uncertainty will be closer to power summation than to Weibull summation.

(5) We were surprised that Graham did not provide details on how to do spatial summation in both space and spatial frequency. It is an interesting question since there is often confusion about how to do summation in both domains. One might think that the spatial representation and the spatial frequency representation are mutually exclusive. However, Stromeyer and Klein (1975) introduced the "Stimulation Map" (later called a *viewprint* by Klein & Levi, 1985) in which position is plotted on the abscissa and spatial frequency on the ordinate. The magnitude of a particular point on the Stimulation Map specifies the amount of stimulation of a mechanism with a given peak spatial frequency, centered at that position and whose phase is optimally matched to the stimulus. This stimulation is equal to the Pythagorean sum of the responses of the even and odd symmetric mechanisms centered at that point. The reason for introducing the Stimulation Map was to carry out probability summation across both space and spatial frequency, in order to place constraints on the bandwidth of the underlying mechanisms. The goal was to rule out narrow bandwidth mechanisms.

Stromeyer and Klein (1975) measured the detectability of frequency-modulated gratings and found that their visibility was slightly less (about 95%) than that of the unmodulated grating. The FM grating had three dominant components spaced far enough apart that a narrow bandwidth mechanism could not linearly summate two components. Probability summation across the three components was also insufficient to make the FM grating visible. Thus, in order to see the grating, the bandwidth would have to be large enough to include at least two of the components. Graham and Rogowitz (1976) used the Stimulation Maps to conclude that bandwidths could be a bit narrower than claimed by Stromeyer and Klein. It would have been of interest to see some of those issues brought up in her book since it would be a reminder to the innocent about how tricky it is to measure properties of underlying channels using psychophysics. The point to be made is that, although probability summation and uncertainty make the calculations more difficult, by cleverly choosing one's stimulus one can produce a multiplicity of constraints that finally do place limits on the tuning characteristics of the underlying mechanisms. The other point of interest is the usefulness of Stimulation Maps for understanding how the visual system "sees" a stimulus through an array of filters.

(6) In order to explain the effects of uncertainty, and also to show how summation and uncertainty interact, we would certainly have introduced a figure depicting a two-channel model. Figure 9.7 in Graham's chapter on identification should have been placed in Chapter 4 on Summation, and her Fig. 9.6 (corresponding to Fig. 7 of Klein, 1985) should have been placed in Chapter 7 on uncertainty. The abscissa and ordinate of this latter figure represent the activity of two independent noisy mechanisms. The response of the mechanisms to a stimulus is represented by a circle. If only the first mechanism responded to the stimulus the circle would be shifted rightward by a small amount. In a summation experiment both mechanisms respond to the stimulus and the circle would be shifted along a 45° angle. Stimulus uncertainty would be represented by the observer not knowing to which mechanism to attend, so the observer's criterion lines would have to be symmetric between both mechanisms even if only the first mechanism is stimulated. This nonoptimal placement of criteria due to channel uncertainty produces a degradation in performance as discussed by Nolte and Jaarsma (1967), Pelli (1985), and Klein (1985). Many of the words used by Graham (eg., Maximum-Output Rule, Sum of Outputs Rule) would be greatly clarified by using this two-dimensional picture. For example, the lower right-hand panel of Fig. 9.6 shows how uncertainty predicts a shallower transducer at low contrast than at higher contrasts. At low contrasts the relevant criterion lines (corresponding to the Sum of Outputs Rule) are nonoptimally tilted at 45° so that $d' = s/\ 2$. At high contrast the relevant criteria (close to the Maximum-Output Rule) become closer to optimal, and $d' = s$, where s is the signal strength in noise units. This simple example would have shed light on the complex issue of how uncertainty affects the shape of the psychometric function.

(7) We would have liked to see a discussion of the meaning of different values of psychometric steepness, β. The parameter, β, is often treated as though it has roughly the same value under all conditions where forced-choice methods are employed. In fact, both theoretical and empirical considerations suggest that differences may be obtained. Pelli (1985) shows that the psychometric exponent

increases markedly with increases in the degree of uncertainty about the position or form of the stimulus. Mayer and Tyler (1986) reported large but stable individual differences between different adult observers. Brown (1986) showed that infants have much shallower psychometric functions, with an exponent that approximates 1 at low luminances. This is a particularly serious case, since by high threshold theory, it implies that probability summation is indistinguishable from full summation, which can produce wild distortions in estimates of channel properties based on peak-detection assumptions. More research is needed to clarify the different factors that contribute to the psychometric function slope and which of these factors also contribute to the amount of summation.

(8) There is one more item that we would add to any discussion of the assumptions underlying detection. During the past two decades there has been a steady improvement in the methodology used by vision researchers. Whereas 20 years ago the method of adjustment was probably the most common psychophysical technique, present researchers are more likely to use a method that controls for criterion shifts such as a rating-scale signal detection or a forced-choice methodology. We would like to assert strongly that the forced-choice method is a poor choice with respect to any attempt to shed understanding on the issues brought up in Graham's Part 3 and 4 on summation and uncertainty. As noted by Graham, Nachmias (1981) points out that it is not known how to relate the psychometric function to summation in a forced-choice procedure when the high threshold assumption is abandoned. In a two-alternative forced-choice task the ideal observer must memorize and compare the activity of multiple mechanisms in each interval. The comparison of two sets of random variables creates difficulties in predicting summation and uncertainty results. In addition, because of the memory load, the real observer will have trouble being ideal. The rating-scale signal detection methodology has additional advantages over forced-choice methods. With a rating-scale procedure one obtains information about ROC slopes that can be useful in identifying the detection process used by the observer. It is also faster (even in a *spatial* forced-choice paradigm) since a single judgement takes less time than multiple judgements. Reseachers should think twice before using the two alternative forced-choice method.

5. IDENTIFICATION

The focus of Parts 2-4 was on detection. In Part 5 the observer's task is to identify *which* of several near-threshold stimuli was presented. There are several advantages to keeping the stimuli near threshold: (1) adaptation and masking nonlinearities are minimized, (2) the topics of hyperacuity, resolution, and general pattern recognition can be ignored, (3) multidimensional signal detection techniques can be used to analyze the data. Graham shows that conjoint detection-identification experiments can provide a powerful tool for learning about the strategies and biases of the observer and about the interactions among the multiple mechanisms. This section is like a window being opened into a large and beautiful garden. There are undoubtedly many fruits to be picked in this garden once one has learned how to avoid the complexities discussed by Graham. We have the highest regard for Part 5 (not just because a good portion of it is based on Klein, 1985) and think it will be of great interest to the Mathematical Psychology community.

What Would We Add to Part 5?

We would underscore the importance of following up on the Hirsch, Hylton, and Graham (1982) and the Olzak (1981, 1985, 1986) experiments that showed inhibition between threshold-strength sinusoids of far-apart spatial frequencies. It is of great importance to discriminate between physiological interactions and response biases. If the inhibitory effects found by Hirsch and Olzak are caused by real interactions rather than by attentional factors, that means that a subthreshold stimulus can have a strong effect on a far-away channel. Such a finding would undermine the entire linear filter story. This direction of research should obviously be pursued.

6. MULTIPLE DIMENSIONS

This section is a cleverly organized overview of a wide variety of detection experiments. Graham examines the following 13 dimensions: spatial frequency, orientation, spatial position, spatial extent, spatial phase, temporal frequency, temporal position, temporal extent, temporal phase, direction of motion, contrast, mean luminance, and eye of origin. On each of these dimensions she asks whether there are multiple analyzers, what are the bandwidths of the analyzers and are they labeled? She then goes on to ask about whether there is independence and whether there is inhibition or facilitation among the analyzers along different dimensions. Finally she includes an extensive reference list organized by the above-listed dimensions as primary dimensions and subdivided by secondary dimensions which are the 13 listed above and an additional 7: viewing distance/accommodation, psychophysical procedure, eye movements, age, species, pathology of observer, and color. Although, as Graham points out, the list is not exhaustive (and several of these additional dimensions are themselves multidimensional), it provides a unique way for newcomers to a particular field to quickly find a good number of significant references.

What Would We Add to Part 6?

The reference list of Part 6 and the fuller list in the reference section would have been even more useful if each entry in the reference section would have a citation back to the page number where it was discussed. That would provide a useful secondary index and allow the book to act as a source reference for the array of cited references. It is quite frustrating to be unable to find Graham's assessment of a particular study in the field.

In summary, we believe that this book is unique. No other text with which we are familiar explores the underpinnings and mechanisms of the detection process to nearly the extent that Graham has done. Although the excellent new book by Macmillan and Creelman (1991) goes over many of the same topics, it is not focussed on vision. For anyone interested in visual detection, Graham's book is essential reading.

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