

# The search for optimal visual stimuli

Andrew B. Watson \*

*NASA Ames Research Center, Moffett Field, CA 94035-1000, USA*

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In 1993, Watson, Barlow and Robson published a brief report in which they explored the relative visibility of targets that varied in size, shape, spatial frequency, speed and duration [1] (referred to subsequently here as WBR). A novel aspect of that study was that visibility was quantified in terms of threshold contrast energy, rather than contrast. As they noted, this provides a more direct measure of the efficiency with which various patterns are detected and which may be more edifying as to the underlying detection machinery. For example, under certain simple assumptions, the waveform of the most efficiently detected signal is an estimate of the receptive field of the visual system's most efficient detector. Thus one goal of their experiment was to search for the stimulus that 'the eye sees best'. Parenthetically, the search for optimal stimuli may be seen as the most general and sophisticated variant of the traditional 'sub-threshold summation' experiment, in which one measures the effect upon visibility of small probes combined with a base stimulus.

Since the initial report, several authors have continued the search for optimal stimuli [2,3] and the use of contrast energy as an informative measure of signal strength has also become more widespread [4–6]. Recently, Kathleen Turano and I published a study in which we searched for the optimal stimulus when the observer was required to judge the direction of motion [7] (subsequently referred to here as WT). Our purpose was to find the most efficient sensor in the motion system, as opposed to the more general contrast detection system. We found that our optimal stimulus was a moving Gabor function (a sinusoidal grating drifting behind a Gaussian spatiotemporal window) with a spatial frequency of about 3 cycles/degree, a temporal frequency of about 5 Hz, a duration of about 133 ms and a size of about 1.3 cycles.

Garcia-Perez and Sierra-Vazquez have offered a critique of our report [8] (subsequently referred to here as GPSV). I appreciate the attention that the authors have paid to our work and the attention of others that they may draw to our work. While their note

does not ultimately support any of the criticism they level against our study, it does provide some useful observations.

In the abstract and throughout their note, they assert that our study contained 'conflicting results.' After considerable effort I am unable to discover in their note any specific identification of these 'conflicting results.' The closest I am able to come to such an identification is their allusion, rather late in their note, to 'threshold variations of up to 0.1 log units over the entire set of conditions' (separate variations of width and height about a tentative optimum). To describe such variations as 'conflicting results' is baffling for at least three reasons. First, the variations were across conditions; no statement was made that any one condition varied by that much. Second, even if the variations were within a single condition, those who do psychophysics for a living will ruefully appreciate that 0.1 log unit is well within the range of variability of human observers. Third and most ironically, our actual sentence was that the variations were 'less than 0.1 log unit' and our point was that there was so little variation in threshold with aspect ratio.

Their second complaint is that we report 'disagreement with earlier results.' In WT, we found an optimum with respect to spatial frequency of about 2–4 cycles/degree and optimal size of about 1.3 cycles, while in WBR, the optimum frequency was at about 6–8 cycles/degree and the optimal size at about 2.7 cycles. The difference in optimal sizes is easily disposed of, since a cursory examination of the WT results (Fig. 8) shows that a size of 2.6 cycles produces almost equal efficiency. As to the difference in optimal spatial frequency, the term 'disagreement' is usually reserved for cases in which two reports give different results for the same experiment. However, these two experiments differed in many ways, most notably, in WBR a two-alternative forced-choice detection threshold was measured, while in WT a direction-discrimination threshold was measured. In addition, the mean luminance varied by a factor of 8.5 and of course, different observers were used. In short, different results were obtained under different conditions; no 'disagreement' was reported.

It is nevertheless of scientific interest to know why these different conditions yield different spatial frequency optima. We offered one possible explanation:

\* Corresponding author. E-mail: abwatson@mail.arc.nasa.gov.

that the direction discrimination criterion used in WT favoured a lower optimal frequency. Let us elaborate this explanation a little. Consider the two curves that describe contrast energy sensitivity as a function of spatial frequency for the two tasks of detection and direction discrimination. In general the direction curve will lie below, or be coincident with, the detection curve. And from prior results we know that the curves should agree at low spatial frequencies [9] (referred to subsequently here as WTMN). But if the curves begin to separate somewhere between 2 and 8 cycles/degree, so that the direction curve begins to decline while the detection curve continues to ascend, then the optimum for direction discrimination would be found at a lower spatial frequency than for simple detection. Also, it is precisely between 2 and 8 cycles/degree that direction and detection thresholds do separate [9]. Since these data were cited in this context in our study, it is puzzling why GPSV chose to ignore them and state that the different optima 'cannot be attributed to different tasks'. They cite other authors to the effect that detection and discrimination thresholds are nearly identical [10] (referred to subsequently here as AB) [11] (referred to subsequently here as AH), but those measurements were made at a much higher temporal frequency, mean luminance and duration than those of WT (Table 1). A high temporal frequency, in particular, is known to ensure agreement of detection and direction thresholds (WTMN).

Furthermore, contrary to the claim of GPSV, there are many conditions in which thresholds for detection and direction-discrimination differ substantially. Some examples come from the AB and AH studies themselves and others are documented in WTMN. But the main point is that no-one has documented the identity of these two sorts of thresholds under the conditions of either WT or WBR. Thus this factor remains a possible basis for the modest difference in peak spatial frequency in the two experiments. As usual, more data would be more edifying than more words.

The third main complaint of GPSV concerns probability summation. In WT and WBR we alluded to the possible complicating effects of probability summation; in particular, in WT we suggested that probability summation might flatten the surface describing sensitivity as a function of width and height, thus reducing the accuracy with which the optimum could be located. GPSV conduct simulations of a model that completely confirms this predicted flattening. But in a breathtaking lapse of statistical common sense, they then assert that 'the flattening of the sensitivity surface' is 'of no consequence for the determination of the optimal stimulus'. Under fairly general condi-

tions, the error in locating the optimum will be directly related, even proportional, to the flatness of the surface.

The fourth main complaint of GPSV is that we did not use an optimal search strategy to traverse the multidimensional stimulus space. There is, of course, no such thing as an optimal strategy in multidimensional search. Even the iterative procedure they recommend can be quite inefficient. Under careful scrutiny their argument reduces to the complaint that we did not collect more data. This is a legitimate complaint (though not a 'flaw') and one that could equally well be levelled at GPSV. They provide no empirical evidence, on human observers, that additional iterations through the space would produce a different answer. They do not even provide simulations to this effect, since their Fig. 3 is only a simulation of a simulation.

We are indebted to GPSV for the simulations they provide which clarify the role of probability summation in determination of the shape of contrast energy sensitivity surfaces. In the case of varying spatial frequency, probability summation has negligible effect on the shape (their Fig. 1), while in variation of width and height, probability summation produces a pronounced flattening of the surface. This latter result reinforces the caution, expressed in both WT and WBR, that probability summation may flatten the surface and complicate its interpretation. Also, while the generality of both of these results is limited by the particulars of the model, the information provided is a valuable contribution. In more general terms, their simulations support the idea that while probability summation may reduce the accuracy with which optima are located, it will not, at least in the dimensions they consider, produce a large bias in the optimum location. This is a reassuring result and confirms the value of the search for optimal stimuli as a useful and powerful method in vision science.

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