1 Problems

Here I'm trying to give on a more detailed explanation of the problems I'm encountering with subject's data in this experiment. Let's pick apart what's going on.

First a terminological note: one reason why there are so many terms for local/Fourier/first-order/short-range/phi/.... motion versus "the other kind," and all the terms seem to be unsatisfactory, is because they all mix up stimuli with mechanisms(**?**). That is, people construct experiments with somewhat arbitrarily defined "first-order" or "second order" motion or "short-range" and "long-range," then attempt to infer from that properties of mechanisms that are imagined to be in correspondence with the stimuli. I believe there are multiple motion sensing mechanisms at play, but I would not like to pretend that my stimulus cleanly cleaves them (and in fact I know that it does not). Therefore I'd like to use terms that are unambiguously about the stimulus and not the mechanism. Since the stimuli are Gabor-like, I will talk about their *envelope* motion as opposed to their *carrier* motion.

The construction of my experiment, currently, is to measure the proportion of responses clockwise to a stimulus in which either the envelope or the carrier motion may be either clockwise or counterclockwise.

One problem is that many subjects, (as in three so far, which exceeds anyone's good-taste ability to dismiss "anomalies") do not actually show a reversal with this stimulus set. They look more like CF, GB or AS here.

Figure 1: insert figure of CF, AS or GB here.

On the other hand, if you show them the stimuli while chatting with them – the exact same stimuli – they readily agree that something in the percept fundamentally changes from "clockwise" to "counterclockwise" when the spacing between targets is reduced below a very reasonable critical spacing. It just doesn't show up when they answer forced-choice experiments. (Maybe the whole "experiment 3" exploration of the stimulus space would work a whole lot faster if I used a method of adjustment, instead of forced choice + fitting psychometric functions. Method of adjustment would allow subjects to select the obvious change in quality without being tricked into forced decisions.)

Taking a cue from more recent work on crowding, I set about to look at thresholds for the two types of motion in my stimulus. In this pilot experiment the feature that subjects are asked to discriminate is the envelope motion (really they are asked to discriminate the appearance of motion, but it is the ways in which the appearance of motion deviates from the envelope motion that are most interesting, so we will consider the data in terms of envelope motion.) I have also looked at some of this data in terms of the discriminating variable being the carrier motion, but it turns out to be pretty gnarly, as we may see later on.

So what I will do is to measure psychometric functions that have envelope motion, parameterized in terms of the *displacement* of the envelope between appearances. I collect a number of such functions, for various values of *direction contrast* and inter-element *spacing*.

Spacing is the distance between adjacent targets on the circle, while direction contrast is a parameterization of the amount of clockwise or counterclockwise carrier motion in the stimulus. Thus the "congruent", "counterphase" and "incongruent" motions in my existing experiment correspond to direction contrasts of 1, 0, and -1. For each combination of direction contrast and spacing, I run two interleaved staircases, one 2up-1down, the other 2down-1up, so that I can get a good idea of both the PSE and the slope of the psychometric function.

For reference, here (**??**) are all combinations of contrast and spacing for which I've collected a displacement staircase. Here I'm folding trials with clockwise and counterclockwise carrier motion together, so you only see non-negative values for contrast.

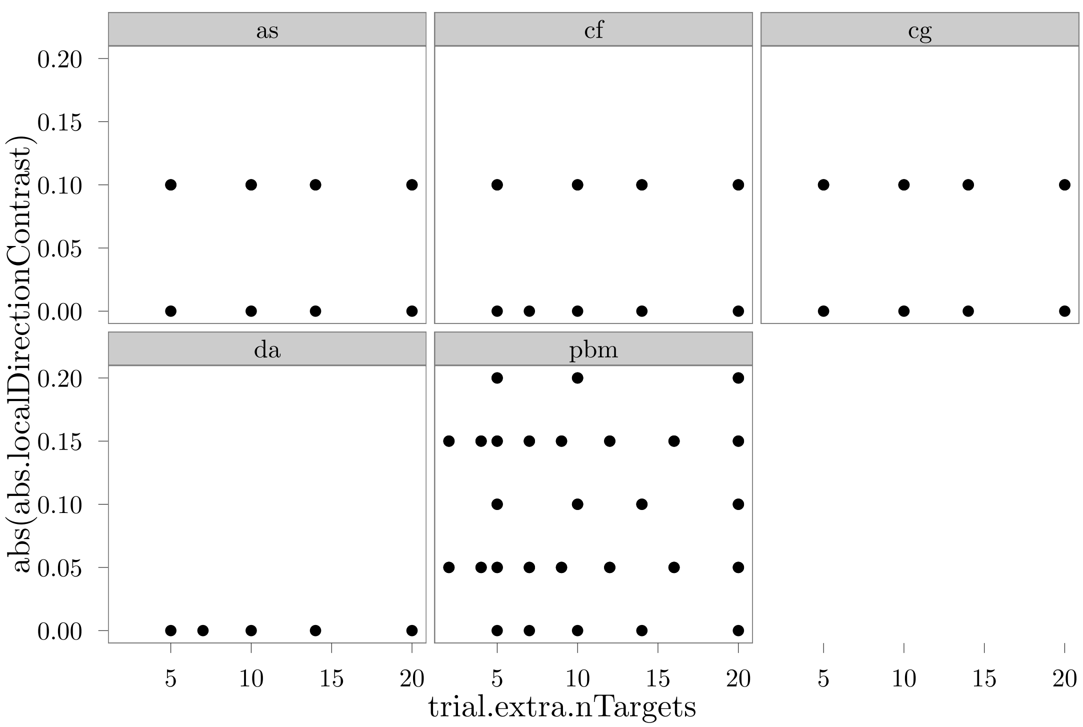

Figure 2: Current extent of pilot direction discrimination data. Dots indicate stimulus parameters for which a psychometric function was collected.

Now, as an illustrative example, I'll show my own data for a direction contrast of 15%.

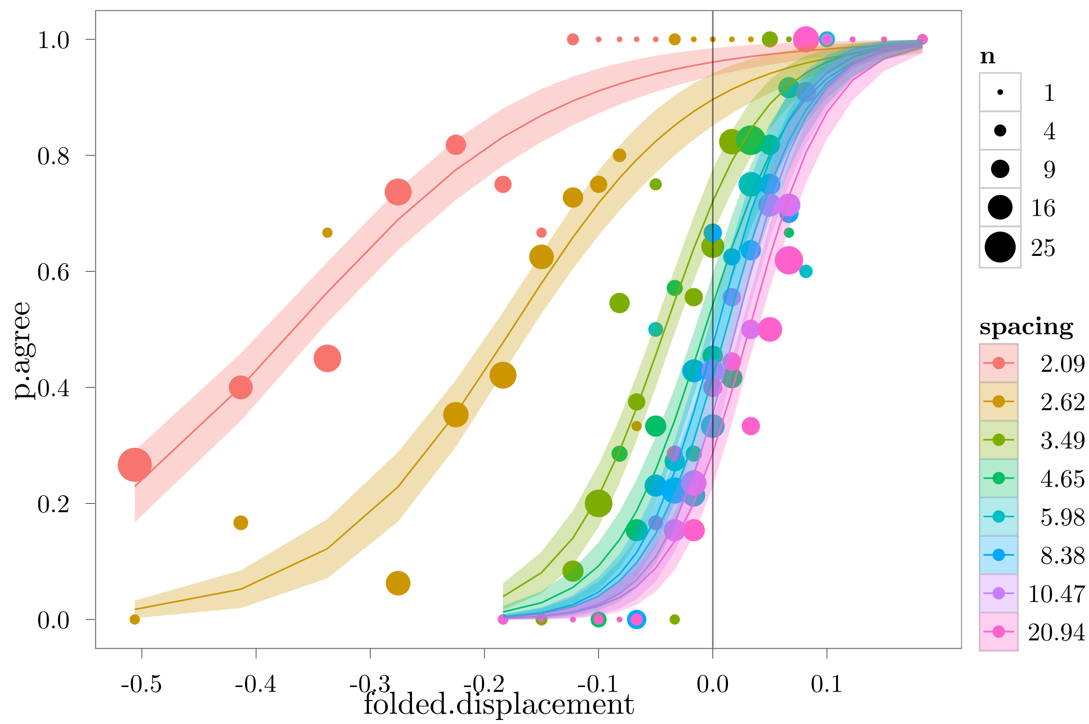

Figure 3: Example direction discrimination data for PBM at 15% carrier motion contrast.

Because it seems less confusing, I'll use the terms *left* and *right* with the understanding that the motions tested are more properly clockwise and counterclockwise, and moreover the data have been folded so that each data point represented as having "rightward" motion contrast reflects testing with counterclockwise carrier motion as well as clockwise.

In **??**, the vertical axis is the probability of answering "rightward" to a stimulus, as a function of the *envelope* displacement on the horizontal axis; positive values indicate rightward envelope displacements. Meanwhile, the *carrier* motion of all stimuli had a contrast of 15% rightward. A separate psychometric function is then fit to a wide range of inter-element *spacings*. At the largest two spacing values there were just two and four elements on the screen; at the smallest spacing there were 20.

All the functions have a positive slope, that is, more rightward displacement always makes you more likely to say "rightward", which makes sense. Now, looking at this there are a few things I would point to. One is that when spacing is on the wide end, the slopes of the psychometric functions are constant. That is, discriminating the direction of is just as easy when there are ten objects on screen as when there are two. This in itself is somewhat interesting; I'm unable to make use of more targets on the screen even if they are all giving me equally reliable information.

However the curves to not overlap; the *bias* is not constant. Look at where the curves intersect the vertical line where displacement=0; adding more targets (reducing spacing) shifts the intercept upwards. That is, in the uncrowded regime, adding more (rightward-carrier) targets on the screen makes me more likely to say that they are moving rightward (without affecting your precision in discriminating direction.) I'm not sure whether this (change in bias with target density in the uncrowded regime) is an effect of spacing, total number of objects, or aggregate motion energy as summed up by an optic flow detector, but my best guess is the last.

That addition of rightward carrier motion seems reasonable in this one example, but note something strange: We can already see one clue of this weirdness: for the widest spacings, at zero displacement, I actually tend to answer *left-ward,* for a stimulus whose carrier motion is to the right! Think about this for a second. Put two targets on the screen, and I answer "left." Show eight and I answer "right". The targets are completely identical and they are too far apart to be spatially interacting. What's going on?

Moreover the direction of carrier-motion-induced bias is *not* consistent between subjects, nor even *within* subjects (for example, bias changes non-monotonically with motion contrast, as I may show later, or you can read in)

These effects are a nuisance and the fact that they happen differently in different subjects is a further nuisance. What I need is some thoughtful advice on how to deal with these effects. I would suggest a detailed reading of (**?**)as well as**?**, since the nuisance effects I'm seeing all seem to be mirrored there.

Now, actual thing in my data which corresponds to "critical spacing," and the qualitative shift in the quality of motion, is seen by looking at the slopes of the psychometric functions. Something important happens when spacing reaches a critical value of about 3.5 degrees center-to-center (yellow-green line); the slopes start to decline, and they decline further with closer spacings. This is the true measure of spatial interactions: the spacing at which flanking carrier motions begin to interfere with the discrimination of envelope motion. The transition between non-critically spaced and critically spaced is quite clear.

How does this compare to my current experiment? My current experiment fixes a particular displacement value, and measures response rate as a function of spacing, with an aim to call the PSE, the point where the fitted curve crosses 50%, You can imagine drawing a vertical line though this graph at a displacement of -0.1 degree; you would find that as spacing changes from wide to narrow, my responses change from "left" to "right." But this displacement value is arbitrarily chosen; by selecting several different displacement values, I can extract several such curves, each having a different PSE (**??**).

So we see, the "PSE" that I have been measuring as a function of response rates is not a reliable measure of "critical spacing." It is affected by critical spacing, but also by the subject's individual pattern of bias in response to carrier

Figure 4: By choosing different values of displacement arbitrarily, I can obtain curves that have several different PSEs. (Imagine this figure, for now.)

motion contrast. It is true that there is a region of motion contrast and displacement that is somewhat stable – for myself and some other subjects, but this is not generally true for all subjects, and it is probably not true that changes in the "PSE" measured correspond to changes. What holds true across all subjects is the increase in threshold as a function of spacing. As (**?**) put the problem,

> Crowding has usually been characterized by just one number, "critical spacing", i.e., spacing threshold, the spacing required to achieve a criterion level of performance. That single number seems to be enough to characterize crowding when the flanker is similar to the target, but may not adequately describe the weaker crowding produced by dissimilar flankers. Disentangling the amplitude and extent of crowding demands a two-number description. The complete 'psychometric function', plotting proportion correct as a function of spacing, tells us little more than the critical spacing. Proportion correct has a small dynamic range bounded by the floor at chance, when spacing is below critical, and by the ceiling at 100%, when spacing is above critical. To get the whole story, we must replace proportion correct by a better dependent measure: threshold. To measure threshold, one varies a physical parameter of the stimulus to achieve a particular level of performance. Thus, threshold is measured on a physical scale with a wide dynamic range. For example, several studies have measured orientation discrimination thresholds as a function of spacing. These plots show that the weaker crowding produced by less-similar flankers has much less amplitude (maximum threshold elevation) but practically the same spatial extent. (**?**)

In the case of my stimulus I believe we do have "dissimilar flankers," despite the flankers being physically identical to the target; in crowded conditions it is the motion-energy from the flanker that interferes with a percept that is normally dominated by the envelope motion, whereas with a lone target the motion energy is easily captured by the envelope motion. That is so say, the relevant feature of the target (envelope motion) is dissimilar to the relevant feature of the flanker (carrier motion.) In some subjects this effect is indeed weaker than in other subjects. In fact, for stimuli that put first-order and higher-order motion systems in conflict, there appears to be a lot of individual variation, which can be made use of in a between-subjects design(**??**).

But in addition of this need to look at thresholds rather than response rates, this motion experiment turns out to be more complicated because the bias is also a function of spacing and motion contrast. So I need a procedure that extracts this critical spacing that is robust to the bias effects, and is efficient on top of that.

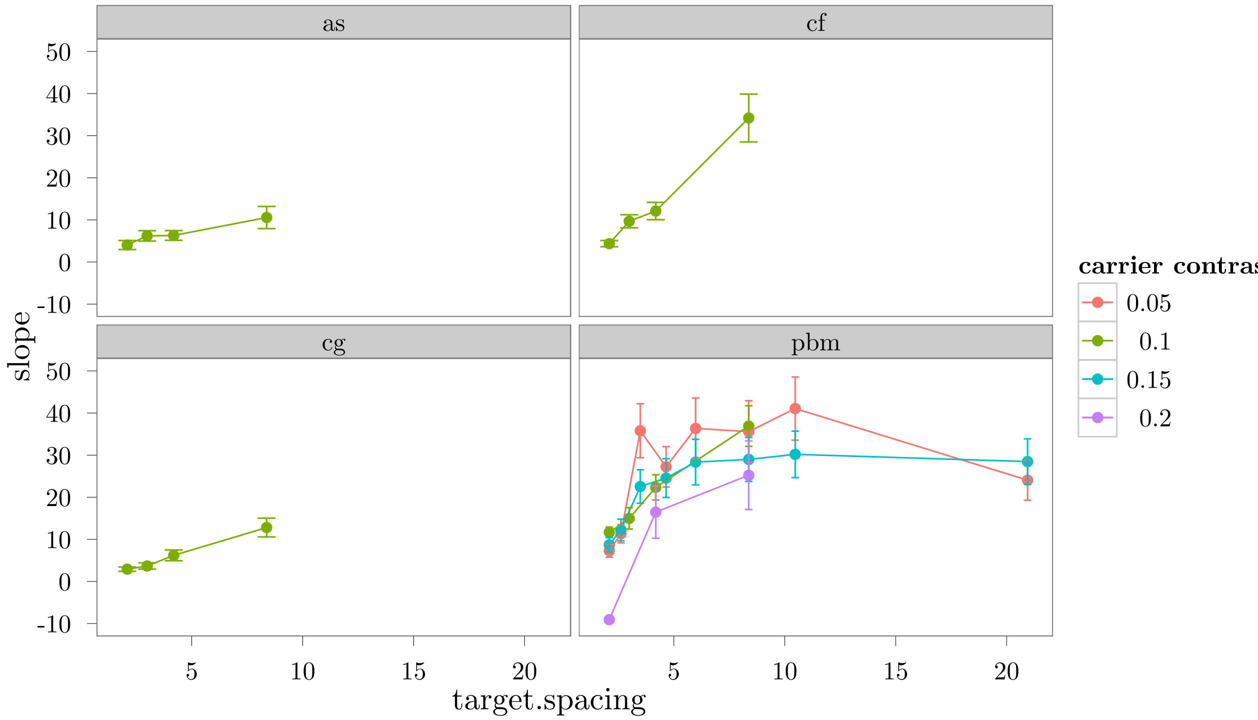

Figure 5: Measured slopes, as a function of target spacing, grouped by carier contrast.

Let's quantify each psychometric function in terms of slope and bias, and show the whole of the data.

In **??**I have left off the data from carrier motion contrast of 0, because there is an effect of motion energy that contaminates the data (see below.) So the data from other subjects appears limited here, but in inspecting the data from individual sessions (I can walk you through this), it seems likely that the pattern will hold.

Also interesting is the pattern of change in bias. In **??**we see that the bias increases with reduced target spacing. For my own data, this bias appears to increase further once below the critical spacing, perhaps reflecting "obligatory summation" which contributes to the motion reversal illusion – but the real driver of motion reversal is the way that the discriminability for displacement falls off a cliff.

[[Image Discarded Due To '/tikz/external/mode=list and make']]

Figure 6: Measured biases, as a function of target spacing, grouped by carrier contrast.

Figure 7: Apparently non-monotonic psychometric functions when carrier contrast is fixed at 0%.

1.0.1 Good news, bad news

- Bad news: Several subjects do not show a "motion reversal" when tested using the current procedure.
- Good news: You can rescue this with suitable adjustments to displacement and motion contrast used in testing.
- Bad news: such adjustments are arbitrary and lead to different PSEs for different values.
- Good news: Every subject does show a change in potion processing, corresponding to the change in subjective appearance to the stimulus that coincides with the idea of a "critical distance."
- Bad news: The current procedure (measuring response rates as a function of) does not extract this critical spacing even for subjects where it successfully extracts a "PSE."
- Good news: I know what the critical spacing means, in much better detail, and can measure it given enough time.
- Bad news: I don't know how to measure it efficiently. It's complicated by biases that vary with motion contrast, in a pattern that is individual to each subject.

What's below this line is much more incomplete than what's above.

It's worth looking at what happens when carrier motion contrast changes. Things get more complicated here, because our stimulus does not cleanly separate motion-energy from envelope motion. It turns out that small envelope displacements contaminate the motion energy of the stimulus with a couple of percentage points. That did not affect the above tests at 15% contrast, but here is some data at 0% contrast to look at. There's what looks like non-monotonic psychometric functions.

It turns out, when modeling the motion energy contained in each of these stimuli, that a small rightward displacement of a stimulus with 0% carrier motion contrast results in some leftward motion energy.

	Spatial frequency		
Eccentricity			
2.96			

Table 1: Bandwidth settings used in motion energy analysis These values were chosen by interpolating measurements performed by .

So how to disentangle this? Our measured “bias” values are a reflection of how much perceptual weight is given to motion energy in the stimulus So we do a multiple logistic regression on displacement and motion energy?

2 Motion Energy analysis

For each unique configuration of the stimulus that was tested, up to a rotation, we calculated the luminance values along the circle transecting the element centers, sampled at the monitor frame rate. To this data we applied a motion-energy model similar to (**?**), using space-time separable analysis filters. The spatial analysis filters were intended to approximate the bandpass properties of direction selective channels in human vision; to that extent we used a set of Cauchy filters (**?**) with the center frequencies matched to those of the stimuli. The spatial bandwidth of the filters was dependent on the center frequency and the eccentricity, consistent with the bandpass properties inferred from human psychophysics; in particular, at a given eccentricity, the bandwidths of the filters decrease somewhat with increasing spatial frequency (**???**). We interpolated the measurements of **?**. The bandwidths chosen for the analysis filters were thus a function of both eccentricity and center spatial frequency; we selected the spatial extent of each analysis filter by interpolating the measurements of . The parameters used at each eccentricity are given in **??**. On the other hand, the temporal component of each filter was identical at all cases, because temporal frequency sensitivity does not appear to vary with spatial frequency or eccentricity (**??**). The temporal component of each filter was the same as used in **?** which approximates something or other (movshon).