We thank the reviewers for their helpful and incisive feedback. We have made changes addressing their concerns about accessibility to the broad readership of *eLife*.

Main comments: We combined comments from all reviewers into a single bullet point if they concerned the same issue. There were many overlaps among reviewers.

1. R1: Response dynamics: I was quite confused about the importance of differences in response dynamics of different ORNs. In places the text appears to state that differences in dynamics are small (e.g. intro, right column of page 1), and in others that they are important (page 3, left column). Some of this may originate from responses of a single cell to multiple odors vs responses of different cells. Nonetheless, the present version of the paper is confusing in this regard.

We were cavalier about this distinction. We clarified it in the *Results/Model of ORN sensing repertoire* section in the paragraph beginning “We verified …”. Though ORN response filters are same for ORNs, the response dynamics are distinct because these filters act on receptor activity , which depends on the receptor-dependent binding constants and adapted . In other words, the filters are the same for ORNs, but the inputs may differ, so the temporal dynamics can be ORN-dependent.

1. R1: The embedding process used in the analysis illustrated in Figure 2 is not explained in any detail - meaning that I could not interpret Figure 2. Later in the Discussion (page 6, right column) this figure is referred to with respect to response dynamics - this was particularly unclear. This figure is critical to the paper, so must be explained in more detail.

R3: What does the 2-dimensional embedded representation mean from a biological point of view? Why 2D? If it is just to demonstrate clustering, maybe is worth assessing a metric for the clustering (such as intra-cluster vs extra-cluster distances).

We apologize for the lack of details of the t-SNE projections. We added discussion of why we use t-SNE and what exactly it is projecting to a low-D space, in the *Results/Concentration-invariant…* section. We also note in the text that the responses are immediate responses following or preceding adaptation. We also provided a schematic in Figure 2 to illustrate what is being projected.

The later discussion in terms of response dynamics was intended to draw parallels between our clustering results in t-SNE and previous results in which time traces of spiking activity were projected to 3D. In both cases, responses cluster by odor identity. There, they used the time trace, while we consider the response at a single time. We apologize for the confusion and amended the text in the Discussion to clarify this.

We explained in the Results section what the projection means, and why 2D (just for ease of visualization). We added a metric for clustering – the silhouette score – in the added section in the Results, in which we discuss relaxing Weber’s Law.

1. R1: The use of compressed sensing in the decoding analysis in this figure is unclear. Related to this point, it's not clear how an appropriate tolerance is chosen (page 4, top of right column). The approach to decoding needs to be described in considerable more detail.

R2: The discussion of compressed sensing is highly...compressed. If the authors could describe this in an intuitive or graphical way in the main Results it would help readers understand what this is and how it works.

Indeed, our discussion of CS was too terse. The schematic in Fig. 3 was also mysterious. We added a few sentences in the *Results/front-end adaptation enhances odor decoding in complex environments* section explaining what this decoding scheme aims to do, and how it can be enacted using constrained linear optimization. We didn’t add many mathematical details, rather just the main point (CS permits the estimation of a high-dimensional stimulus vector from a low-dimensional vector of responses, when the signal is sparse). We removed the unnecessary equations from the figure; replacing them with a graphic simply illustrating that CS is a linear optimization over the odorant concentrations, and its output is an estimate of the original odor signal vector. We had included the metric for assessing decoding accuracy in the Figure caption, but instead put it in the *Methods* to explain why it was chosen as such.

1. R1: The origin of Eq. 1 could get explained in more detail. The form of feedback in Eq. 2, and particularly its relation to Weber Law adaptation, should get explained more.

R2: The manuscript is written for a highly quantitative audience and assumes a background familiar with the various models (receptor model, compressed sensing, t-SNE) they employ. I think the paper could be made more accessible by unpacking some of the mathematical formulae in the main text. For example, it would be helpful to show a plot of the activation function as a function of odor concentration (Eq. 1) for some of their sample model neurons, in both the unadapted and adapted state.

We agree. We added some details in the first section of the Results. First, the definition of receptor active fraction is presented, and then we discuss in words how one can get the final closed-form expression for active fraction (formerly Eq. 1). The explicit Botlzmann factors that go into this formula are listed in the SI, rather than adding these technicalities to the main text. Next, we discuss what are the required ingredients in the feedback dynamics to permit Weber’s Law. We also draw and discuss the general shape of the active fraction for neurons in different states (e.g. unadapted, adapted). We hope this will help intuit how affects the activate fraction curves for elevated signal concentrations.

1. R1: It is not clear here why the background should be represented as static. I would have thought it would be subject to many of the same properties that make the signal dynamic. The role/importance of short term memory is also unclear.

We are concerned with the detection of novel foreground odors amid background odors, thereby assuming that backgrounds odors have persisted for some time beforehand. Indeed both odors are carried by the same fluid flow. Of course, if backgrounds fluctuate on the same timescale as the foreground, adaptation does not increase coding fidelity. This is essentially because the distinction between foreground and background is lost. We chose to simplify the presentation so that one of these odors is on a much slower timescale, effectively static. This may be conceivable if the foreground and background arise from spatially separated sources, which can affect the intermittency timescale.

The role of short term memory is to limit the amount of information utilized from the past. We now discuss this briefly in the pertinent section in *Results/Front-end adaptation enhances odor decoding in complex environments*. Mathematically, the activation energies integrate in time, so they could be shaped by information from long ago. This would require an artificially large amount of neuronal capacity. The main point is simply to see how far back background odors should be retained. We find that a memory timescale on the order of the adaptation time 250 ms seems sufficient.

1. R1: It would be interesting to see how important ORN-specific adaptation is for the results presented, as compared to a mechanism that acted universally across all ORN responses.

R2: One possible interpretation of the results in Figs. 2 and 3 is that in the non-adaptive system, high background odor concentrations cause the receptors to saturate, preventing them from encoding anything about the target odor, or at least massively compressing their dynamic range. This would mean that sensitivity adaptation is important (the activation curve needs to shift with increasing odor concentration), but not the precise form of the adaptation. Could the authors perform additional simulations to address this? For example: (1) What is the state of the receptors (distribution of activation levels) in the adapted versus un-adapted system in high background odor (prior to target odor presentation) vs background+target? (2) How do the results in figures 2 and 3 differ if the adaptation is not exact? That is, what if there is some factor ß in front of Aa(t) in equation 2? How precise does the adaptation have to be for this to work?

Both of these comments suggest a need to investigate how much we can break Weber scaling and still maintain combinatorial codes. This is an excellent point and was absolutely missing in the previous draft. We first note that a multiplicative factor on could be absorbed into redefinitions of and . This would still preserve Weber’s Law scaling (which follows from the integral feedback onto and the constancy of ), just to a different background level at possibly a different rate. The critical element in maintaining Weber’s law here is the constancy of , which we have now noted in the first Results section. Thus, we broke Weber Law scaling by allowing to be receptor-dependent, and to scale as ~log . We investigate the ramifications of this relaxation of Weber’s Law, and show the distribution of activity levels and t-SNE clustering in added panels in Fig. 2. We also added a new section that discusses this in detail, and addresses the implications of breaking the scaling. Indeed, we attribute the effect of Weber’s Law, in part, to a prevention of receptor saturation as suggested by Reviewer 2.

1. R2: Using a KC-inspired model to decode odor identity will probably be the most intuitive decoding scheme for many biologists. Here this decoding scheme is presented last but perhaps it might go earlier in the manuscript.

We were also somewhat on the fence in the ordering of the results. We opted for this presentation mainly because primacy coding and compressed sensing decoding are more easily interpretable and far more tractable computationally without the added machinery of the AL and MB connectivity. Further, primacy coding has been shown in projection neurons, one step away from ORNs, so we presented it before we discuss the AL-MB connectivity. We do note in the CS section that we will later investigate the implications of circuit mechanisms in later sections. For these reasons, we chose to keep the ordering as is.

Minor comments:

Reviewer 1:

1. Page 3, left column: Variability in ORN responses appears to originate from the distribution of lower bounds to the free energy differences and from the distribution of dissociated constants. It is not clear why both are needed, and how much each contributes to the diversity of ORN responses.

Why do you assume changes in free energy are bounded by an upper and lower limit?

The lower bounds on free energies allow the adaptation to enable for different receptors at different signal magnitudes, as has been observed experimentally. If this did not occur, for all stimuli levels, the steady state ORN firing rate would jump, discreetly, from 0 Hz to 30 Hz once the odor is strong enough. In fact, for sufficiently low firing rates, for many ORNs, the steady state firing rises with signal intensity, until at certain point where it remains fixed at 30 Hz.

The upper bound is metabolic -- without it, the neuron could adapt to A0 irrespective of signal intensity, which would require more and more energy as the signal increases. We included this upper bound to remove this possibility, although it is not critical for our results.

We noted the metabolic origin of this in the text.

Meanwhile, the distribution of Kd is based on experimental results in Si, Kanwal, et al Neuron 2019, which measures these in larvae.

Both may play a role in the observed diversity, but both are predicated on prior experimental results.

1. Abstract, last sentence: This is pretty technical, and I think could be stated more simply.

Yes, that’s true. Amended.

Reviewer 2:

1. p. 4, 1st paragraph "When de-convolved from stimulus dynamics, the shapes of the temporal kernels of Drosophila ORNS that express orco are largely receptor- and odor-independent." I am not sure I entirely buy this although I don't think it is critical to the conclusions of the paper.

This is the main conclusion of Martelli et al (2011), whose conclusions we accept here as is. However, It is not critical to the results we present here.

1. p. 4, 2nd paragraph: can you clarify the relationship between free energy and Kd?

We have rewritten Eq. 2 (formerly Eq. 1) to explicitly show where *K* and *K*\* enter the free energy in the definition of *A*(t).

1. Fig. 5b: I am not sure I understand why the divisive normalization model is contributing so little to the classification of odor identity. This seems at odds with the results of simulations in Olsen et al. 2010 and also Zhu...Friedrich 2013. Is this because the decoding model is different? Can the authors provide any insight into why the normalization contributes very little in this case?

There may be a few reasons for the discrepancy. First, we decode the KC responses, not glomeruli, so it is possible that synaptic divergences may render divisive normalization less effective. Another distinction is that we use odor mixtures, containing several components, rather than single odorants. We noted this in the text.

1. Discussion, 1st paragraph: "odorant-odorant antagonism" is this implicitly included in your model because there is only one binding site on the model receptor? (I am thinking of the competitive binding model in Singh et al. (<https://www.biorxiv.org/content/10.1101/311514v3>).

That effect is distinct. Odorant-odorant antagonism arises from the interplay of binding affinity and activation energy – ORNs may bind odorants readily but then activate weakly, or vice versa. This then would imply that some odorants in mixtures would occupy binding sites without leading to ORN activity, reducing the effect of other odorants in the mixture that bind less readily, but are more effective in leading to firing events. The model presented in that paper is not allosteric – receptor activation is a second kinetic step after binding. Our model treats binding and activation as distinct and independent, not sequential. For that reason, activation is independent of odorant identity. So odorants within mixtures do not mutually antagonize – more of either odorant in the concentration will increase firing rates. The independence of activation energy on odorant identity is a critical feature of our model that enables activity-dependent Weber Law adaptation, irrespective of odorant identity. The two models are not inconsistent, since that work refers to mammalian olfaction, not insect olfaction.

1. p. 7, 1st paragraph: is it worth mentioning here the findings from Cao 2016 suggesting that Orco-mediated adaptation relies on intracellular calcium (Fig. 6c)? A similar Ca-mediated adaptation is observed in vertebrate olfactory receptors (e.g. Leinder-Zufall et a. 1999) and of course in photoreceptors (e.g. Fain et al. 1989), which might say something about the generality of this mechanism and form of adaptation.

TODO

Reviewer 3:

1. What does the 2-dimensional embedded representation mean from a biological point of view? Why 2D? If it is just to demonstrate clustering, maybe is worth assessing a metric for the clustering (such as intra-cluster vs extra-cluster distances).

Addressed above in main comments.

1. Adaptation of specific receptors enables maximum sensitivity of the system of given odors (well described in the article). It would be a good idea to describe an issue of a potential overlap in the receptors stimulated by the background and foreground odor?

TODO