Review of “On the sensitivity of food webs to multiple stressors”, by Beauchesne et al.

This manuscript tackles the important issue of how the structure of food webs can influence the impact of stressors on population abundances, and in particular how multiple stressors might act at the same time, often with surprising effects. The authors propose a general protocol for addressing this issue. They ask (p. 6) three basic management questions: should species interactions be considered in environmental impact assessments? should stressors be considered in combination, or separately? and, can one identify which species are most sensitive to stressors? Enroute to trying to answer these questions, they introduce some new jargon – unitary pathways of effect, g; integrative pathways of effect, G; species motif census; trophic sensitivity, A; trophic amplification, A; trophic variance, V – along with some metrics for the latter three. They then use simulation studies of a Lotka-Volterra model (where parameters are chosen randomly from probability distributions) to examine these metrics for a number of community modules (tritrophic food chains, etc.), abstracted from the St. Lawrence food web. After that, they turn briefly to the full web. The bottom line is that species interactions can be crucial to consider in assessing stressor impacts, often magnifying or buffering such impacts; impacts of multiple stressors are not just additive across stressors; and, species at different trophic positions can strongly differ in their response to stressors.

These are all sensible conclusions, and I am broadly sympathetic to what the authors are trying to do. The basic idea that interspecific interactions can modulate responses to stressors is not new (indeed, Volterra was motivated to get into ecology because of phenomena related to this noticed by his son-in-law in World War I). This is an important topic.

However, I found this a somewhat frustrating and indeed opaque manuscript. I have read it through several times, and felt the same, each time. These are largely I think matters of exposition. I’ll put down a few things that struck me.

This first paragraph refers to ‘interactions between stressors’, mentioning for instance that “susceptibility of corals to … [thermal] bleaching increases with nutrient enrichment). So for instance, mortality rates of corals might be a joint function of the levels of temperature and nutrient loading. However, what the authors mean by different stressors (as best as I can discern) is that different stressors affect different growth or interaction parameters. This is sort of a ‘switch-and-bait’.

I think the paper would benefit enormously from say a Box with a thoroughly worked example of say a single motif for three species, that says what the various g’s and G are, and derives the various metrics the authors explore. They do this with an example in Fig. 1, they should do it more in depth. Given that the basic model if a Lotka-Volterra model, so per capita growth rates are linear, one can solve for equilibrial abundances of each species (see Rossberg’s book for solutions of the matrix equations), and examine the sensitivity of each abundance to small changes in parameter values. The sensitivities of changes in different parameters will be intertwined (the non-additivity noted by the authors). One could also simply pick two stressors (i.e., parameters), and plot out equlibrial abundances as a function of the values of those parameters; deviations from a plane would be a mark of nonlineary. This would add a bit of analytic heft to the authors’ simulation studies.

It was not clear to me where the authors drew the basic parameters that they use to define the pre-stressor state of their system. Is this drawn from published information about the St. Lawrence? The authors should also note that a realistic model of this system would be surely more complex than their

LV model (e.g., saturating functional responses for consumers). They seem at my reading to be drawn from a hat.

One crucial aspect of multispecies models is specifying the strength and trophic position of direct density dependence. If this is strong enough, it vitiates more complex food web effects. This issue could confound comparisons among the different motifs, which appear to differ in how many species are assumed to have direct density dependence.

The example in Fig 1 C seems to show very large changes in species abundances (often > 50%). Most of the examples shown in Figures 2 and 3 seem to reveal much smaller changes in abundance (a few percent), which in real-world systems would almost surely be obscured by measurement error and noise. I wonder if this might in part reflect the protocol described on p. 16, lines 8-14, where the only parameter combinations considered were those where perturbations from stressors were small enough that no species went extinct? This protocol might provide a misleading sense of what the impact of multiple stressors might be, when inflicted on a community that was initially coexisting just fine, but where some extinctions ensure due to the stress.