1.      Please include a graphical abstract and Highlights section. See <http://www.elsevier.com/journals/food-webs/2352-2496/guide-for-authors#40010> for details.  
2.      Use "and", not "&" for references, e.g. Pimm and Lawton 1977 (lines 17-18).  
3.      Groups of references should be listed first alphabetically, then chronologically. Line 18, for example, should be (Pimm 1982; Yodzis 1981). **Done (but double check this**  
4.      Please expand the abstract. It is currently relatively short and I believe a few additional sentences will aid in clarifying the scope/scale of the research undertaken.  
5.      Please replace 1, 2, 3, 5 (lines 19, 20, 21, 26) with one, two, three and five.  **Done**  
6.      Last sentence of the first paragraph of the introduction should be deleted and this should be discussed later in the paper (Discussion). **Deleted** There are numerous other places in the introduction with text out of place (Reviewer #1 brings this up). It would help the readability of the introduction if this section of the paper did not move between results, interpreting your results. Please read through carefully and tease apart the sentences that are not necessary and either remove them from the manuscript or move them to the discussion and expand if/when necessary.  
7.      Delete "; 253" on lines 35-36. It is not necessary. Reviewer #1 wants this part deleted and I can see their point. In order to keep it in the paper, further integration is necessary. I would alos benefit the paper by expanding the introduction in places, as suggested by reviewers. **Removed it**  
8.      Line 41, add "a" between "at" and "trophic".  **Done**  
9.      Figure 1b is not mentioned until the results (line 201). I would therefore request this be a separate figure inserted at the appropriate place in the manuscript.  
10.     Delete comma at end of line 57. **Done**  
11.     Please have one section for Methods (2. Methods). Then 2.1 Model Selection will be a sub-heading, 2.2 Simulation models, 2.2.1 Sensitivity analysis.  
12.     Not necessary to refer to specific page numbers of papers (e.g. lines 141, 143). **Deleted**  
13.     Unless I missed it, there is no reference to Figure A2 in the text. Please carefully go through the text and make sure all figures, appendices etc, are referred to correctly and in the appropriate order.  **It is there, will be made a full figure anyways**  
14.     Please follow Journal format for references. For example: "Van der Geer, J., Hanraads, J.A.J., Lupton, R.A., 2010. The art of writing a scientific article. J. Sci. Commun. 163, 51-59."  
15.     Please do not capitalize all letters in figure axes labels.  
16.     Please add reference to Appendix A in the legend of Figure 1 to direct readers to the literature used to form this figure.

1. One issue I have with this paper is that the author's don't really clarify what they mean exactly by a "food chain". They seem to use this term interchangeably with the number of trophic levels in a food web, but is this what they meant? For example, couldn't a food chain have a series of horizontal steps within the same trophic level, such as an herbivore feeding on a plant, a spider feeding on the herbivore, a bigger spider feeding on the first spider, a bigger spider feeding on the second spider, etc (with each spider considered a member of the third trophic level). This is a crude example but would the horizontal steps in the third trophic level (i.e., the predators) mean there are five links in the food chain but only three trophic levels. I think the authors need to better define their terminology up front and distinguish the relationships they want to imply between food chain length and trophic levels in a food web (are these the same, different, etc?)
   1. **In the given example this would constitute a food chain with 5 trophic levels based on the definitions given on line 19. I clarified the definitions used in this manuscript in the first paragraph of the introduction.**
2. The introduction of this paper is extremely poorly written. There often seems to be little connecting one paragraph to the next, and the authors discuss their results in seemingly every paragraph without even describing what their study is going to do. I had a very hard time following the line of thought as it just seemed like a jumble of different ideas, without any linear flow that made sense. **Deleted some out of place sentences and added some connecting thoughts.**
3. The model is not explained in suitable fashion. The first sentence of the model selection paragraph describes a sign matrix generated for each food web, without even talking about what types of food webs were generated. Shouldn't this be the first thing that the authors discuss? Moreover, assumptions are not clearly defined. What is the predator-dependent functional response and how is it modeled? Similarly, terms like prey-dependence and ratio-dependence will not be clear to all readers and need a definition. I honestly read to the end of line 156 and have absolutely no idea what the authors actually did with this model.
4. The results of the simulations seem to contradict the empirical data, which was a flaw pointed out by the authors in the introduction. They mention that stability declines significantly from 3 to 4 trophic levels (L202-203). But, looking at Figure 1A food webs with this many trophic levels appear to be about equally likely in nature. Can they explain this discrepancy? I would expect (based on the empirical data) a big drop off from 2 to 3 and a similarly large drop off from 4 to 5.
5. The authors point on L245-246 is a key one. In Nature food chains are embedded in complex webs. However, their simulation model only assumed ten species. The results would be more convincing if the food webs they simulated were more representative of real-world food webs (i.e., potentially hundreds of interacting species). I realize this may be computationally unfeasible (perhaps) but a 10-species food web is far too simplistic to draw any real conclusions about what might operate in nature.
6. Other issues/comments:  
     
   L18-20: This is a bit misleading in my opinion. Looking at the figure 1A it seems there is relatively no difference between 3 and 4, thus this sentence should say that most species have a trophic position from 2 to 4. This is a problem because the trend does not line up with the simulation results. **Changed to quantitative descriptions of trophic levels.**   
     
   L28-29: This seems like an odd ending to the first paragraph of the introduction. Such sentence, with evidence for how results compare to the perspectives paper, belong in the discussion not here. **Deleted.**   
     
   L30-31: This needs to be developed much more fully in my opinion. What are the predictions from each of (1) energetics; (2) foraging theory; and (3) dynamic constraints that suggest food chains should be constrained? Without explanation it's not clear what the strengths and weaknesses of each hypotheses are (which should also be explained, L31-32). It seems like the authors begin to develop these ideas in the third paragraph, such that the Macarthur quote seems completely out of place.  **These alternative hypotheses have been thoroughly discussed elsewhere.**   
     
   L35-38: This quote has nothing to do with developing a broad introduction and should be deleted. Similarly, L38-39 is discussion not introduction. **Deleted**  
     
   L61-62: Again, the authors should not be ending introduction paragraphs with brief segments describing the implications of their results. It's hard to process the results of this paper before the authors even describe what they plan to do. **Deleted**  
     
   L93-94: The authors use a lot of jargon, but I point it out here. What is a sign-stable system? What is a trophic loop? These terms need to be defined.  
     
   L122-123: I think the authors have ignored a major aspect of many food-webs, intra-guild predation. This can occur if predators eat other predators, or if herbivores eat other herbivores (which can occur passively if one herbivore eats plant tissue containing eggs of another). I find this omission problematic in that these interactions have shown to strongly affect community structure and ecosystem functioning. Not considering horizontal interactions within a trophic level is a major flaw in the reasoning of this paper.  
     
   L124: Is the meaning that a predacious mammal could never eat an herbivore and a plant? This is obviously not the case, what about a bear for example (that would eat both a hare and plants the hare feeds on).  
     
   L142-143: What does donor-controlled mean?  
     
   L158-159: Is this a method or a result?  
     
   L160: Never use "as well as" unless it is a comparison (John could not play ball as well as Steve). Otherwise "and" is grammatically correct. **Switched it to “and”**
7. The authors do not mention another kind of explanation for limitation on food chain length that has been imposed, which has to do with colonization-extinction dynamics; basically, there is an inherent asymmetry in any food webs that involve specializastion (and all do, to some extent), in that say a herbivore cannot colonize unless its required plant is present, but the reverse is not true.  Moreover, if predators tend to drive their prey extinct, they can still nevertheless persist in a spatial context.  This has been known since the classic work of Huffaker in the 1950's; he experimentally showed that unstable predator-prey interactions leading to local extinction could persist in a spatial array of patches if dispersal is limited.   This involves another kind of dynamic constraint on food webs, having to do with spatial dynamics, in effect overriding local instability.   The spatial hypothesis of Post et al. (see also papers by Holt, like book chapter in  
   Losos-Ricklefs) is itself for the most part a 'dynamical constraints' hypothesis.
8. I found the arrangement and formatting of figures and legends very annoying.  Please put figures like A1 and A2 in the main text, and put each figure directly with its legend.  It is hard to fully understand what the text says without inspecting these figures, so put them in the main text.  And, the code can all be compiled and annotated separately.  For sure put that (the code) in the appendix.  
   The figures verge on being unreadable, at least in my copy.
9. The flow of thoughts in the paper in describing and justifying the model is a little unclear, and should be clarified and strengthened.   The authors on lines 12-129 state that they will deal with a model with "universal omnivory". So if one ranks-order species, the top-ranked species is eaten by no one, but it eats all other species; likewise, a species of rank n eats all species with lower rank.  Is this correct?  If so, say it.  They should explicitly note how this limiting case differs from (or is similar to) models such as Cohen's cascade model, and subsequent niche models of Martinez, Williams, and others.   It really is not obvious to me that universal omnivory is more "realistic" than assuming linear unbranched chains.  If one takes into account the actual heterogeneity of weightings of flows and interactions in a matrix, and tossed quantitatively negligible links, there would be much less omnivory than assumed here.  
   The whole protocol of the paper is to assess the local stability of equilibria in randomly assembled matrices with specified constraints.  The authors need to say something about what happens when an equilibrium is unstable.  Do you expect species to always go extinct, without oscillations?  The simplest example of this is the 2-species Lotka-Volterra model, where with stronger inter than intraspecific competition, an equilibrium with both species present exists, but the slightest deviation leads to one or the other winning out (the priority effect case).  Or instead, are there sustained oscillations around the equilibrium. I realize their protocol breaks down, but systems deemed unstable could nonetheless persist, but with oscillations.  Maybe systems with long chains are actually persisting, just not at a stable local equilibrium.  
   In the case of Lotka-Volterra models, these oscillations can for instance be heteroclinic, which would imply deviations to such low numbers that extinction is still highly likely.  So there still could be a dynamical constraint on food chain length. Is that what happens with these nearly donor-controlled systems, which include strong predator interference?   I would like the authors to provide a better analytical understanding for why instability occurs, and when it actually leads to extinctions, say working it out for a small number of cases. With 3 species, there are three Routh-Hurwitz criteria that need to be satisfied, for local stability, maybe just one of them gets violated when things go unstable.  The dimensionality of the system is low enough that Levins' loop analysis approach could shed some insight into what is happening.   In other words, for at least this limiting case, lay out more clearly the forces at work in the 'dynamical selection' that may be limiting food chain length a bit even with 3 species.   After you create your 'universe' of possible food webs, each with a corresponding matrix your stability criterion in effect cleaves this class of matrix into two subclasses, one with eigenvalues permitting local stability, the other not.  Are there any obvious patterns in the strength of interactions in these matrices, for instance, that would explain the difference?  Maybe the unstable ones all have on average higher interaction strengths.  
   Universal omnivory is one limiting case.  Another that seems to be embedded in your model would be pure donor control, where the predators have no impact on their prey at all.  But the eigenvalues of a block triangular matrix is just the set of its diagonal elements, which in your case are by assumption all equal to -1.  So in that limit, as you mention, all food chains of any length in your model would be stable!  So the limit to food chain length you observe in your simulations, must somehow essentially have to do with deviations away from donor control.  You need to reflect on the mechanistic underpinning of your results a bit more.  
   Thinking this through is I think important for understanding your results.  
   Section 3.1 is titled "Sensitivity analysis".  I think this phrase is often used in a more narrow sense than this.  Were one examining a matrix projection model, sensitivity analysis would be an analysis of how eigenvalues change with small changes in each entry of the matrix.  Here  you are doing  something broader, varying connectivity, asymmetry, etc.  Maybe call this "steps towards robustness" or something along those lines.

1. It seems to me that the two parts of Figure 1 report different things.  The figure on the left is the distribution of the trophic position of species, which is not the same as the number of trophic levels within a community (which is what is shown on right).  So the same label is used for the two abscissa, when actually they are quite different.  What happened to species at level 1?  There must be something down there!  **Changed the figure to be more accurate**
2. In looking at the suite of figures as a whole, a pattern that is considerably stronger than the food chain length result, is the overall great decline in likely QSS with increasing number of interactions.   So 'dynamical selection' should be culling out communities with high interaction density.  The authors should discuss this.  Maybe there is 'selection' on the overall richness of interactions, and the effect on food chain length is a kind of correlated by-product.
3. Minor to medium points:  
   Line 19. State whether species with no prey, are at position 0 of 1  **Done**  
   Line 23.  In Figure A1 (which as noted above should be in main text), there are actually more webs with a maximal chain length of 6, than with 3, so you should write something like "between 3 and 6"  **Changed to a more quantitative description based on percentages.**   
   Line 56. No comma after "levels". **Done**  
   Line 65.  "four species food chains" should be "four-species food webs"; Pimm and Lawton include some examples with omnivory, hence not simple, unbranched chains. **Done**  
   Line 97.  I think a hyphen is needed after "quasi". **Perhaps, but in Allesina and Pascual 2008 they did not include one**  
   Lines 123-124. Using "chains" seems odd here.  Replace with "webs".  On next line thought about lynx etc. should be put in parentheses. **Done**  
   Lines 149-150.  "positive values on one side…"  I really do think   this seems biologically quite implausible.  The authors need to provide some references or additional arguments that this limit is not totally unrealistic.  
   Line 159.  If this figure is not in printed version (and I think it should be), then you will need a more clear description in text to describe what you are doing.  
   Line 163.  Put the phrase "an independent" before "random".  **Done**  
   Lines 186-187.  The whole text in this section is hard to follow.  I would be hard-pressed to replicate what the authors have done.  Shouldn't some of the entries be negative, as well as the positive 1's, since these are predator-prey interactions?  Do the number of interactions on 187 refer to predator-prey interactions (direct interactions).  So for a 10 species unlinked chain, there are 9 predator-prey pairs, and one might select 3 additional omnivory pairs from these?  So higher connectance implies more omnivory?  
   Lines 190-191.  So there were 1000 random draws for each of the 40,500 food webs?  
   Lines 196-198.  I assume you mean you found the eigenvalue with the largest real part.  
   Line204.  Add a comma after "species". **Yes**  
   Line 208. Replace commas with parentheses, around "Figure 2"  **Done**  
   Line 215.  The effect of an increase in quasi sign stability seems rather minor, so add a word like "slightly".  **Done**  
   Line 230.  Say "number" not "numbers". **Done**  
   Line 234. Insert "non-basal" before "species". **Done**  
   Line 254.  The 24 and 28 interactions are not shown in Figure 2, but instead in Figure S3 etc.  Are these figures basically the same? **Yes this was done for clarity.**