Dear Editor,

I read carefully the notes “Les Houches lectures on Theoretical Ecology: high-dimensional models and extreme events” from Ada Altieri, which I find interesting and timely. Nevertheless, before publishing, I find that an additional important effort to improve readability and clarity must be done. Here below I list all my comment and observations, which I hope will be addressed by the author for the benefit of the reader.

GENERAL OBSERVATION

I think that it is in general needed a better contextualization of the analitical results in the framework of ecological systems. Which are precisely the consequences for ecological models and the conclusions which can be drawn about from the analytical predictions obtained with the techniques discusses in these notes? Perhaps it might be useful to state/list the most relevant open questions about ecological system to which an answer can be given by exploiting techniques in disordered systems, and, point by point, state them along with the exposition of results.

DETAILED OBSERVATIONS

Here I proceed listing Section by Section my main observations/suggestions/remarks

ABSTRACT

“Given the characteristic high dimensionality of the dataset, ***alternative*** *elegant approaches…”*
-***Alternative*** to what? No other approach has been previously mentioned, please clarify.

“In the second part, we will highlight timely directions …”
- Perhaps better “ … we will highlight timely research directions” ?

INTRODUCTION

Line 35: *“whether from sudden shocks...”* → “either from sudden...”

Line 41: *“This raises a central theoretical question: is the number of feasible equilibria in such ecosystems exponential or sub-exponential relative to system size?”*
- There is a sudden shift in the discussion from the problem of the ecosystem stability to external perturbations to the problem of the number of equilibria, by implicitly assuming that the two things are related. Is that correct? In this case please say explicitly why it is to, explain why and how the number of equilibria is related the ecosystem stabilty.

Line 43: *"Local multistability refers to…"* → Please explain clearly what is meant for "local multistability". Local in which sense? Does it means that the equilibria are close to each other in phase space? I am familiar with the concept of "local stability" but less with that of "local multistability", which I assume to be also true for the generic reader.

Line 46: *"...or on a much smaller scale remains..."* → I would rather say "...or in a much slower way remains…". This said, I would like to ask a clarification also here: why and to what is relevant, for the present context of large ecosystems stability, how the number of local equilibria scales with system size?

Line 48: Please explain with a couple of words what *"fragmentation"* and *"segregation"* means for ecological systems.

Line 50: Please explain what *"dispersal"* means in this context.

Line 53: Please explain what *"tipping points"* and *"hysteresis"* means for ecological systems.

Line 55 – Line 99: I suggest to organize better the discussion and, in case, reorganize the list with bullets. At some point is written *"...we shall discuss three reference models: neutral models, the Generalized Lotka-Volterra model and the Consumer-Resource model."* so that the reader would expect a bulleted list of these three models, while on the contrary there are bullets only for "neutral models" and "Niche Models", with just few words at the end on Consumer Resource. Maybe something more can be written for the Mac Arthur model. I suggest to improve the clarity of this discussion, maybe reorganizing it. Also, at the end of the introduction, a bulleted list of the main questions to which the following analysis will provide an answer could be useful.

2 – MAC ARTHUR'S CONSUMER RESOURCE MODEL

Line 107 and Line 110: Here are given inline the expressions of $g_i(R)$ and of $F_\alpha (R_\alpha)$. I suggest to put them as equations since they are referred to afterwards in the discussion. Please also explain better the physical meaning of χ_i in the equation. Please also explain with few more words the meaning of the impact vector $q_{i\alpha}(R)$.

Line 119: The title of the subsection is *"Low-dimensional criterion for feasibility …"* but in the following text I do not see where and how the arguments presented are related to the low dimensionality of the system. It seems they are simply geometric/algebraic constraints valid in any dimension. Please explain better.

Line 138 – 139: The sentence *"The aforementioned geometric criteria … low-dimensional systems"* seems redundant, or at least not very useful in introducting the following discussion. I would rather start from *"In recent years…"* at line 139.

Line 141 – 142: I would replace *"Such a picture … large communities"* with *"In this regime one neglects microscopic interaction details and focuses on the typical behaviours emerging in large communities."*

Line 155: I would replace *"each entry of … probability distribution"* with *"all the entries of the adjacency matrix presenting species interactions are random variables sampled from the same probability distribution, characterized by few parameters"*.

Line 157: I would replace *"Within a statistical physics scenario … that is:"* with *"One of the simplest options is to assume that each microscopic parameter is a normally distributed random variable:"*

Line 177: Perhaps rather than *"local susceptibilities"* we are talking about *"mean susceptibilities"*? It seems that expressions are defined by means of an average over all degrees of freedom.

Line 203 – Eq. (7): The equation in the second line has a term
dR_\alpha/dt = r_\alpha / K_\alpha (K_\alpha – R_\alpha) + ...,
while in the same formula in Eq. (1) we have that the term is
dR_\alpha/dt = (K_\alpha – R_\alpha) + …
Please explain better how the two things can be consistent from the dimensional point of view.
Is the coefficient r_\alpha / K_\alpha dimensionless? Which is precisely the difference between the second line of Eq. (1) and the second line of Eq. (7)?

Line 216 – 227: Here the author introduces an analogy between the search of stationary solutions of the Consumer-Resource (CR) model, viewed as a constraint-satisfaction problem, and the perceptron. It is not clear to me how this analogy would help the reader to get a better insight into the problem. First, it has not to be taken as granted that any reader known what the perceptron is, nor it is clearly explained here. Second, it is not clear which particular feature of the perceptron, also assuming a good knowledge of this model, helps in gaining insights on the CR, besides the very general fact that it is just another constraint-satisfaction problem. In order to improve clarity and simplicity I would suggest to skip any (in my opinion useless in this form) reference to the perceptron, which at this point of the discussion is just distracting the reader, and to simply state that the search of dN_i/dt = 0 solutions is equivalent to a constraint satisfaction problem. This is perfectly enough, simple and clear. Differently, I encourage the author to expand the discussion on the analogy CR-model/perceptron and better explain in what it helps to gain insights.

Eq. (8): Why there is the proportionality sign and not simply the equality?

Line 230: *"This novel framework introduced by Tikhonov and Monasson …"*: why the author speaks about a *"novel framework"*? Isn't Eq. (8) the same as the first line of Eq. (1) and Eq. (7) with a different name given to the term on the right hand side? Please explain better or simplify. For instance, if the "resource surplus" \Delta_i is something different than what appeared before, it must be introduced and defined clearly before the considerations exposed in lines 230-235.

Line 243: Rather saying that \epsilon is an "infinitesimally small parameter", which, unless a specific scaling with the number of species S or the number of resources M is given means nothing, I would simply write "small parameter", "\epsilon << 1".

Line 248 – 249: Since these are lecture notes and not a research paper I suggest to spend few more words to introduce the Lyapunov function, to clarify the difference between equilibria of a Hamiltonian system and dynamical attractors or stationary states of a system with a dynamics which is intrinsically a non-equilibrium one. For instance Eq. (9) could be introduced by something like (just to give an idea): *"Due to the non-conservative nature of the interactions, the microscopic dynamics can be only defined* *in terms of a Lyapunonv function (add some references), which allows to study the stability of non equilibrium stationary points."*

Lines 255 – 270: I find quite confused the discussion in these two paragraphs. The authors goes back and forth from equilibrium to non equilibrium statistical mechanics without many explanations. From line 254 to line 255 there is a sudden jump from a discussion on non-equilibrium stationary dynamics to thermal systems. Temperature just comes out of the blue, without any previous clarification of the context. Furthermore, in this paragraph the author starts suddenly speaking about phase transitions controlled by either \epsilon or T, without giving a little bit of context and somehow assuming that the phenomenology of the model is already known by reader. Also, it is stressed an analogy with the SAT/UNSAT transition in constraint-satisfaction problems which in my opinion, if not explained in more detail, rather than clarifying the framework is even more confusing. Again, the author cannot assume that the average reader of these lecture notes knows what the SAT/UNSAT transition is and that just quoting it without any further explanation is of any help to him. General suggestion: remove the quoting of these analogies or explain them in more detail. Too vague references to the phenomenology of other models, most likely unknown to the reader and unexplained here, are of no help.
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Line 263: *"the logarithmic term in the effective free energy…"*: which logarithmic term in which effective free energy? I have seen no free energy up to here in the text.

Line 266: *"…one can extract the spectral density in both phases..."* : the spectral density of what?

Line 267: *"...This signals the onset of marginal stability…"*: Again, here the authors makes reference to another context presuming it is known by the reader, while it is not. What is "marginal stability"? Which is its definition and its implication in the context of ecological systems?

Eq. (10): Please explain better the difference with the model in Eq. (1), also there the same matrix c_{i\alpha} appears both in the first and in the second equation. It might be also worth stress that the generalized Lotka-Volterra model can be defined in a completely independent way from the CR Mac Arthur model, as the high-dimensional generalization with random interaction matrices of the original prey-predator Lotka-Volterra two dimensional system. And then explain how, under certain assumptions/approximations, the Mac Arthur model can be reduced to a generalized Lotka-Volterra one.

Line 298: *"...one can use the cavity method…"*: I suggest to spend a couple of words more on what the cavity method is in statistical mechanics. Which is the main idea and which are the main assumption of this method. Also, please clarify explicitly the difference between sparse and fully-connected networks, why the cavity method can be applied successfully (if so) in both cases. Let me also take this occasion to note that the difference between sparse and dense network of interacting species should be specified better across the whole note, saying explicitly when the equations considered refer to sparse or dense interactions and which are the approximations and/or asymptotic results valid only the case of dense networks, but not in the case of sparse ones.

Line 318: *"...according to the dynamics (21) …"*: Is this number of equation correct? Perhaps the author wanted to refer to Eq. (16)? Eq. (21) is a constraint equation, there is no dynamics.

Line 319: *"The dynamics of the former S species can be written as:"* : I do not understand: this sentences introduces and equation, Eq. (20), where there is no time derivative and not dynamics, this is just a constraint equation for the stationary state solution. What does the author refer to as "dynamics"?

Eq. (22): please explain better how this equation is obtained, preferably introducing the susceptivity chi_{ij} without making reference to the CR model, since here we are discussing the generalized Lotka-Volterra. Explain under which approximation Eq. (22) is valid. The addition of a small pictorial representation of the cavity approximation might also be useful at this step. Even if not compulsory, it is often a successful strategy to explain the cavity method.

Line 335: *"...the typical fluctuations…"*: by which term in Eq. (25) are represented these typical fluctuations?

Eq. (26): please recall explicitly which sort of average is represented by the angular parentheses in Eq. (26). Please also make clear in the notation the equalities between the first and the second term in both the first and the second line hold only for S >> 1. In introducing the equations I would also say that one has to *"exploit"* the self-averaging properties rather than saying *"...need to ask.."*. In the way the sentence is formulated now it seems that one has to assume the self-averaging properties, somehow guess that it is true. I think in the present case this is not just an assumption, it can be proved to be correct for the present system, right? Also, beside stating clearly what the self averaging property is, it must be specified for which observables it holds in the present case. It is in fact well known that in the same disordered system the self-averaging properties holds for some observables whereas for others do not hold. For instance, in standard spin-glass model self-averaging holds for the free energy but not for the overlap probability distribution.

3 – HIGH-DIMENSIONAL RANDOM GLV EQUATIONS WITH STOCHASTICITY

I am first of all puzzled by the title of this section: why is it emphasized the "high-dimensional" nature for this part of the lecture notes? It seems to me that even almost all the results of the previous section are asymptotically exact only in the high-dimensionality limit? Did I understand correctly or am I wrong? Please explain better. On the contrary, what should have been stated more clearly since the beginning of section 2 is the absence of noise in the dynamics of all the models discussed so far, before the beginning of Sec. 3. Maybe a discussion preliminar to Sec. 2 or at the beginning of Sec. 2 is also worth, anticipating which is relevance and the role of noise in the dynamics, that models respectively with or without such noise will be discussed in the notes, trying to give an idea of why is worth discussing both. At present no words have been spent on the problem of deterministic VS stochastic dynamics until the title of Sec. 3, so that the clarification that stochasticity is now present comes a little bit as a surprise.

Line 356: *"… in the absence of interactions (deterministic scenario, i.e., A_{ij}=0)"*: it is not clear to me why the absence of interactions is referred to as the deterministic scenario. Even in absence of interactions still we have noise in Eq. (30). So, why is it deterministic?

Line 359: *"The amplitude T turns out to be … the smaller the demographic noise strength"*: Explain better this argument. What does exactly means that the amplitude of the noise T *"turns out to be inversegly proportional to the total number of individuals in the pool"*. Temperature T is a parameter, not an observable, and it has no dependence on the total amount of the population.

Line 367 – Sebsec 3.1: I am quite concerned about this whole subsection, it looks like an attempt of explanation which, try to saying something but not everything, only raises more doubts in the unexperienced reader. The difference between Ito and Stratonovich prescription in the discretization is a delicate technical point, for which, in the lack of a clear and complete explanation, is better to refer to the appropriate literature. Just to list few remarks. Why, as written at line 368, “Eq. 30 is meaningless without selecting a proper discretization of the noise”? This is a continuous-time equation, so that in principle why should be meaningless without choosing the discretization? I understand what the author means, but this is just provocative to stress the delicacy of the point. Then, it is totally misterious why and how Eq. (31) should derive from Ito rather than Stratonovich prescription? What one would have from Stratonovich? It is not at all clear the role played by the multiplicative noise and the by choice of Ito prescription in the steps going from Eq. (31) to Eq. (33), so my suggestion is: either expand a lot the discussion making it **much** more clearly and precise or remove it. In my opinion it would be perfectly sufficient, avoiding to go into intricate discussion, simply start by Eq. (34), saying that a Fokker-Planck equation of this kind can be obtained by adopting a Ito prescription. In fact, an option for me is to simply remove Subsection 3.1, if not expanded, and attach Eq. (34) and Eq. (35) to the previous paragraph, simply saying that thanks to the Fokker-Planck equation one has a Boltzmann like equilibrium distribution with effective Hamiltonian as in Eq. (35), then skipping any comment in Stratonovich, which is completely useless for the following discussion.

Line 396 – Sec. 3.2: *“The disordered average of the free energy…”*: here we find a sudden jump to the calculation of a free energy. How this could happen? I think a little bit of context and explanation must be given. There has been a sudden jump from the end of subsection 2.5, where the stationary solutions of deterministic equations where discussed, to the calculation of a free energy. Why, if we were initially interested in stationary solutions stability of a deterministic equations system, we now about to compute the free-energy related to the Boltzmann equilibrium of stochastic equations? What statistical ensembles have to do with stability of a large dynamical system? The discussion must be expanded and, in particular for the didactic nature of lecture notes, cannot be simply based on the axiom: “I have a Boltzmann weight with random disordered couplings, I do the replica calculation of the free energy”. Just to raise a point, it could be for instance worth to state that the replica calculation comes into play only when a self-averaging property holds for the free-energy, which is the case for Gaussian random couplings, but might not be the case for other kind of couplings. Therefore, since the choice of Gaussian random couplings is somehow arbitrary and not completely justified on the basis of ecological systems phenomenology (maybe it is worth to comment more on the opportunity, limitations and justification of this choice), it should be stressed how even the replica calculation is based on quite specific and possibly not even realistic assumptions.

Line 470: *“The dynamical equations can then be integrated …”*: please make an explicit reference to which equations.

Line 471: *“…, to analyze variability, …”*: isn’t it just to average over disorder?

Line 476 – 478: *“...the correlator appear to satisfy the time translational invariance…”*: perhaps better to stress that it is not a general property and that it holds only for certain temperatures, while in the presence of a glass phase aging is expected rather than TTI. Does the presence of aging or TTI makes any difference for the phenomenology of ecological systems?

Line 487-488: *“In the asymmetric case, the instability bound is slightly modified to take the correlation between asymmetric coefficients into account [34]”*: here more explanations are needed. The author is talking about the thermodynamic stability of the replica-symmetric solution of the model: we are therefore talking about equilibrium statistical mechanics. Once the asymmetric couplings are introduced there is no more equilibrium statistical mechanics, no Boltzmann weight, no free energy, no RS solution of the thermodynamics. Therefore, I find quite misleading to simply state the sentence above, from which it seems that using slightly asymmetric matrices the whole equilibrium calculation leading to the same RS solution can be done, with the only effect of having a slightly shifted stability bound. I ask the author to please reformulate more clearly and more precisely the statement.

Eqns. (57)-(61): I find that a better choice of the equations can be done here. Showing Eq. (58) without writing explicitly the expression of the three kind of matrix elements M_{ab,cd}, M_{ab,ad}, M_{ab,ab}, is quite useless. I suggest to drop Eq. (57) and Eq. (58), leave the rest and just say that the replicon is a function of the stability matrix elements. Eq. (62) also looks quite misterious. I suggest to remove it or explain in more detail the meaning of the averages appearing there.

4 – DERIVATION OF THE PHASE DIAGRAM AT FINITE NOISE

I am not 100% that this discussion deserves an individual section. In first place the "derivation" of the phase diagram is represented by the free-energy calculation and the stability analysis contained in the previous section, so that this title would equally well, or even better, fit as the title for Sec. 3. Second, I would not emphasize so much noise, since the whole derivation of the phase diagram is made from the theory of ensembles and from equilibrium statistical mechanics calculations, rather than from the analysis of stochastic equations dynamics. I suggest to remove this title and section, merge the discussion up to Eq. (65) to the previous promote to Sec. 4 the text from line 575 on, naming it **"Conclusions and Perspectives"**.

Line 536 – 562: Well, as often happen, from the calculation one finds a that, upon varying the parameter the RS phase is eventually unstable with respect to a 1-RSB phase, which in turn is eventually unstable with respect to a full-RSB phase. Could the author please comment more on the inplication and interpretation of both 1-RSB and full-RSB phases in the context of ecological systems. Beside the analogy between the generalized LV system and glassy systems, which insights on the LV system itself and the ecological problem itselft are we getting from this RSB solutions and the corresponding transition? More comments on that point would be for sure very appreciated by any reader.

Line 563: *"Zero-temperature limit of the RS solution"*: it would be very beneficial to the reader an explanation of why we are interested in the zero-temperature limit of the solution, why and how it connects with the possible phenomenology of the deterministic gLV model.

Line 566: *"Vanishing trend of the logarithmic term in the Hamiltonian (35)"*: what do you mean as "vanishing tred" (quite inappropriate expression for a term in a Hamiltonian), does it vanish or not in the $T \rightarrow 0$ limit? To me it doesn't look vanishing, since the prefactor is (T - \lambda). Or perhaps is lambda itself assumed to be a function of temperature? Please clarify this point.

Eqns. (64), (65): The author presents some saddle-point solutions for the population abundance in the $T \rightarrow 0$ limit without any further comment. I think that is overall useless to show the T=0 solutions of the replica calculation without explaining why we are interested in that and, furthermore, without even commenting the implication/meaning of the results for the ecological system we are studying. What should we learn or understand in general about large ecosystems and in particular about the stability of gLV equilibria from the results in Eq. (64) and Eq. (65). Please expand the discussion or remove the subsection.

Line 575: *"Conclusion and perspectives"* : I would like to see more extended comments on the implications of the results shown in the notes for the understanding of the gLV phenomenology. What do we learn from the glassy solutions exhibited on the stability of large ecosystems. Which are the implications of having or not having a glass phase? The prediction of these phases is robust against changing the model parameters, e.g., the species interaction connectevity matrix, the probability distribution of matrix elements, the scaling with system size of such probability distribution moments, etc etc.

Line 582: *"Non reciprocal interactions break detailed balance."*: I think that such a such sentence is not the best way to introduce asymmetric interactions when the problem of detail balance has never been discussed in these lecture notes. Also, I definitely would not say *"… defining a free-energy and minimizing its Hessian on a proper subsector becomes unfeasible"*, alluding to the fact just as a mere technical bottle-neck. Much more simply, I would just say that in the presence of non-reciprocal interactions there is no equilibrium statistical mechanics, hence no free energy to be computed and so far and so on. Also, let me say, one minimizes the free energy, not the Hessian.

Line 588-590: I would say something more about the rationale behind passing from the many-body problem in Eq. (66) to the one degree of freedom problem in Eq. (67), something which very likely leaves any inexperienced reader completely speachless. For instance, at least I would stress that this is possible only for fully-connected models, trying to argue that the reduction from many interacting degrees of freedom to a single one is somehow related to a central limit argument and a coarse graining procedure.

Line 599 – 612: Here the author talks about model with heterogeneities...but how the DMFT method, which is precisely based on the absence of heterogeneities, apply to these models?

Line 636 – 646: The description in this paragraph would benefit of some formulae, otherwise being somehow difficult to follow.