**Responses to Angeler Reviews (**in bold)

Chapter 1: No comment

Chapter 2:

* *Second, papers introducing a new method or approach to identifying regime shifts are not often proposed in publication outlets with aims of disemminating new quantitativecmethods (e.g., Ecological Modelling, Methods in Ecology and Evolution). Rather,cmany new methods are published in journals with refined (e.g., Entropy, Progress in Oceanography), as opposed to publications with broader scopes (e.g., Ecology andcNature).* \*\*\*Any idea why? General journals too conservative or actually based on actual first choices of authors?
  + **This is an interesting question, and cannot be answered in the dissertation as I do not have any evidence (e.g. interviews) suggesting one way or another. I believe this was intended as a defense question.**
* *I again used the Web of Science database to identify patterns in the development and persistence of the ecological regime shift literature. I conduct a systematic literature review using ISI Web of Science, and use these results to conduct exploratory bibliographic analyses.*  
    
  *TS=(“regime shift” OR “regime shifts” OR “regime change” OR “regime changes” OR “catastrophic change” OR “catastrophic shift” OR “catastrophic changes” OR “catastrophic shifts” OR “sudden change” OR “sudden changes” OR “abrupt shift” OR “abrupt shifts” OR “abrupt change” OR “abrupt changes”) AND WC=(“Ecology” OR “Biodiversity Conservtion”)* \*\*\*There is a large overlap of search terms between this search (development in regime shift detection) and the previous search (available research). It was not clear to me how you processed the information to obtain different results given the similar search structure.
  + **I have clarified the methodologies of the review and the comprehensive methods list in this chapter such that this no longer exists in its present form.**
* *Figure 2.5: Chronological direct citation newtwork suggests the intellectual structure can be mapped to a few papers. This historiograph identifies important works explicitly in chronological, as opposed to absolute, order.* \*\*\*I really like this figure! It shows that there was quite some action between ca 2004 and 2011. Not so much in more recent years. Why? Is it because we have enough methods or good-enough methods for our goals as ecologists to detect regime shifts. It this is the case why would we need more/other methods? It seems counterintuitive to the long list of limitations of early warning techniques presented earlier in chapter 1.
  + **The direct citation network was conducted on the regime shift literature and not the ‘methods-only’ literature. I have clarified the methodologies of the review and the comprehensive methods list in this chapter such that the latter point (“…it seems counterintuitive…”) is addressed.**
* \*\*\* *Comment*: Section 2.4.2. (model free EWIs): falls short of considering multivariate methods (e.g. redundancy analysis where time is modeled with a  asymmetric eigenvector approaches [aka Magic]) and their ability to distinguish scale-free vs scale-explicit approaches; an important consideration given that scale is an important characteristic of resilience. There would have been also some space for the study of Burthe et al. 2016 (J Appl Ecol) who showed different forms of failures (Type 1 and 2) when using the traditional EWIs.
* *P. 47: “Many of the methods metnioned in this review were not identified using a systematic search process in Web of Science and Google Scholar–rather, they were methods of which I was either previously aware and/or highlighted in the few methods reviews (Andersen et al., 2009; Boettiger et al., 2013; Clements & Ozgul, 2018; Dakos et al., 2015a, 2015b; deYoung et al., 2008; Filatova et al., 2016; Kefi et al., 2014; Litzow & Hunsicker, 2016; Mantua, 2004; Roberts et al., 2018; S. N. Rodionov, 2005; Scheffer et al., 2015)* \*\*\* *red highlighted text*: I am confused now!!! Is the whole review thus substantially based on your expert knowledge rather than the search? Such expert knowledge is often very subjective and often biased towards the experts own research (i.e. specialist vs generalist knowledge). In fact, I found the potential of many multivariate techniques not considered in this review. Many of these are mentioned in Roberts et al. 2018, which you coauthor, but I could not avoid “my subjective feeling” that you criticize this study more for being incomplete than for the novel overview of multivariate methods it provides.
  + **The methods have been re-written for clarity.**
  + **I have adjusted the tone to reflect the aims of the Chapter, which are not to criticize the existing reviews, but rather is to attempt to compile a comprehensive review based on both prior knowledge AND a literature review.** 
    - **Although I will note that I think it is important to justify the efforts of this Chapter in presenting a comprehensive list, rather than many prior reviews which are field-specific or shift-type-specific, which for good reasons are not comprehensive of the techniques proposed and applied.**

Chapter 3: manuscript draft has been previously commented, so comments will not be reiterated here.   
 **N/A**  
  
Chapter 4:

* What is the Fisher Information derivatives method and how does it differ form the traditional Fisher Information?
  + **I have included a brief description of the Fisher Information, noting that it differs from FI as a regime detection method, however, did not feel it was necessary to detail the mathematical differences.**
* *Importantly, when using community or abundance data, rare or highly abundant species can influence the size of states criterion, thus influencing the assignment of each point into states. Finally, Eq. (4.3) assumes equal spacing (in space or time) between sampling points. Each of these violations can be avoided by using Eq. (4.4) (Cabezas & Fath, 2002; Fath et al., 2003) to calculate the Fisher Information measure (see Chapters 3, 5 for detailed discussions on this topic).* \*\*\*What does this mean exactly? That one should use presence/absence data?
  + **TO ADDRESS**
* *…derivatives method (Eq. (4.4)) estimates the trajectory of the system’s state by calculating the integral of the ratio of the system’s acceleration and speed in state space (Fath et al., 2003).* \*\*\* how is acceleration and speed measured if FI runs systematically with constant window sizes (and I assume bins) over the data? This sounds rather like “constant speed” measured of system dynamics
  + **The acceleration and speed are measured instantaneously, while the FI uses window analysis.**
* *Interpretation of FI is still a qualitative effort. Fisher Information is proposed as an indicator of system orderliness, where periods of relatively high values of FI indicate the system is in an “orderly” state, possibly fluctuating around a single attractor.* \*\*\* Why can’t a low order system fluctuate around a single attractor (stable state) if this “orderly state” is part of process-feedback relationships of the system? That is, where is (if there is) a way to say with certainty which kind of order represents chaos vs stability?
  + **I very much agree with this comment and appreciate the feedback. This was also discussed during the closed-door defense. I do not see opportunity to address this issue in this Chapter.**
* *When a system occurs within any number of states equally, i.e.,****p(s)****is equal for each state, both the derivative, (dq****(****s****)****and****I****are zero.* \*\*\* This reads like quantum mechanical superposition, which definitely does not work in macrosystems. If we say it does really occur, at least theoretically, then how and which “parallel dimension” does FI pull out or recognize?
  + **Although I appreciate this comment very much, this is beyond the scope of this Chapter.**
  + **“**This reads like quantum mechanical superposition,” – **The statement in red is not my idea, rather, it is derived from numerous, published studies of FI (e.g. Fath and Cabezas 2003, Eason et al 2004, etc.)**
* *Interpreting the Fisher Information is currently a qualitative effort. As suggested earlier, rapid increases or decreases in FI are posited indicate a change in system orderliness, potentially suggesting the location of a regime shift. Using this method yields inconclusive results regarding the location of ‘spatial regimes’ (Figure 4.7). Of the three spatial transects analyzed in this chapter (see Figure 4.5), Figure 4.7 is representative of the lack of pattern observed in the Fisher Information values across all analyzed transects.*\*\*\* Examining these figures it seems to me that some clear structure is present, contrary to the above affirmation. Is it because the y-axis is too much stretched out to see something pronounced when it is subtle in reality? If this is so, what is the utility/robustness of FI for such an analysis? – also looking at some of the maps, I tend to agree that no abrupt shift is detected. Nevertheless, FI seems to capture neatly two regimes, one at the west and a larger one on the east, but again Figure 4.10 is interpreted as no clear structure being present (I am confused).
  + **Although there is some sort of pattern in the Fisher Information in 2010, the expected patterns of *abrupt change in FI* is not observed (Figs. 4.7, 4.10). The purpose of the FI is that an *abrupt change* indicates a regime shift. Slow changes in the FI values, especially across spatial transects, should not be interpreted as spatial regime boundaries.**
  + **I have clarified this in the discussion of the results by emphasizing that *I did not observe abrupt changes in the scaled and centered values of the FI, values which should abrupt changes at the onset of rapid change moreso than the ‘raw’ results.***
* *I found no evidence of spatial regime shifts in the avifauna in my study area. Further, the absence of autocorrelation among spatially adjacent transects suggests Fisher Information may not be a reliable indicator of changes in bird community structure.*  
   \*\*\*Maybe no evidence of regime shifts, but delineation of two regimes. Anyways, in accordance with previous research, the inability of FI to statistically locate thresholds, make interpretation very subjective. How might this affect the message we have to give the military regarding the management of their properties.
  + **See previous comment regarding the presence of slow versus abrupt changes in the resulting FI results**
  + I have
* *Fisher Information reduces and removes the dimensionality of these middle-numbered systems, which omits critical information*. \*\*\* no kapisch!
  + **I have removed the reference to middle-numbered systems entirely.**

Chapter 5: I liked this chapter and don’t really have any useful comments. I wonder however, how this new methods differs in information content from other turnover metrics used in ecology, like Bray-Curtis. Is velocity, I assume, an analysis in Einsteinian spacetime, while the other are only temporal and thus ignoring space? But then looking at the data, they are only of temporal nature too, so no spacetime. Anyways, despite this confusion, I am asking because, the thesis seems to be a form of practitioners guide. How can you convince a manager to use velocity instead of traditional metrics when they give similar results, as seems the case with the paleodata (congruence between velocity and previous methods)?   
**This question is discussed in the Conclusions Chapter, and because the purpose of Chapter 5 is to present the methodology, rather than convincing management to use this (versus alternative) metrics, this question is not addressed in Chapter 5.**   
   
  
Chapter 6:  
  
This is an important chapter! Having used only three metrics the study indicates a potential huge variability if more of the available metrics are compared. This suggests, as indicated in the chapter, that further exhaustive comparisons are warranted. The chapter sets out with guiding the practical ecologist. However, I feel that the chapter ends too abrupt and that the management implications could be better developed.  Also the results of FI are interesting. I agree that the method is insensitive to more “subtle” changes in the system.

A recent reanalysis of the data used by Spanbauer and colleagues suggests some major changes in the transition which the FI (but also the follow up study using discontinuity analysis) did not pick up.

* **I am not aware of this ‘re-anlaysis’, and unless it is published I do not see the point in alluding to it in this chapter. I have, however, included a note on the potential implications of data quality of the diatom data (especially dissolution, Spanbauer, pers. comm.).**
* **Although this study has direct implications for managers in that the quality of these and other metrics are relatively under-tested, this study is meant for the audience of e.g. Methods in Ecology & Evolution—i.e., ecological modellers or those developing/refining ecological regime shift detection methods. I have adjusted the Introduction in an attempt to redirect the focus to this matter, while still pointing out the down-stream effects on decision making.**

Chapter 7:  
  
Last paragraph in introduction: difficult to follow and study goals unclear. Does not follow from previous descriptions. Reminder of the chapter too underdeveloped for meaningful review. But here are some quick thoughts, which might only be relevant if my assumption is valid. Data from military base were used. If the surrounding landscape changes but not at the military level, than discontinuity analysis might bot be regarded as failing to detect change. Rather, it could indicate that management is good enough to coerce the system into a “dead regime walking”. That is, management currently succeeds in maintaining the conditions of a grassland regime, but as soon as it ceases it will flip into the woodland regime that has now encroached the entire surrounding area.   
**The introduction and discussion have been improved for clarity and tone. Data from military bases were not used in this Chapter. Although I understand the point made re: ‘dead regime walking’, this is beyond the scope of this study.**   
   
  
Chapter 8:

* *This dissertation demonstrates that, while potentially useful, regime detection metrics are inconsistent, not generalizable, and are currently not validated using probabilities or other statistical measurements of certainty*.\*\*\* Punchy conclusion. I agree that as soon as we have a toy in our hands we use it in an uncritical way and take its validity or performance for granted. An interesting paper linked to this discussion is Spears et al. (Nature Ecology and Evolution).
  + **Spears et al., indeed, is a highly related paper of which I only recently became aware. It has been incorporated into the discussion when referring to the lack of probabilities used to identify the “change points”.**