

Coexistence and climate change: The role of temporal-variability in structuring future communities

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Question: Which species are doomed?

Abstract

Predicting community shifts with climate change requires fundamental appreciation of the mechanisms that govern how communities assemble. Much work to date has focused on how warmer mean temperatures may affect individual species via physiology, generally producing range shifts towards the poles and uphill, which fails to predict the wide diversity of observed shifts. Climate change has and is expected to affect far more than mean temperatures, including widespread affects on growing season length, variability and shifts in extreme events. Additionally, cascading effects on species and communities are qualitatively predicted but there have been no efforts, to our knowledge, to predict shifts based on coexistence theory. Here we extend the two possible mechanisms for species coexistence based on variable environments—relative nonlinearity and the storage effect—to predict how communities will respond to climate change. We focus on both (1) shifts in climate variability and extreme events that link to stabilizing coexistence mechanisms and (2) traits that may make species the most vulnerable to climate change. We examine how coexistence via the storage effect shifts under non-stationary climate regimes, and how outcomes vary with the ability of species to phenologically track the timing of major climate events. *Findings go here. Such as: Species that can track variability are least vulnerable to climate change (perhaps). Also, we add an emphasis on integrating intra and inter-annual scales here, if we manage to make that happen well.*

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1 Things we may or may not have done yet... to review

- Make model take biomass for each species at its peak value (not at its end of season biomass).... pretty sure we do this.
- **Check R^* component of model is working:** We suggested doing this in January 2016, but leaving it here to make sure we're okay:
 - Behavior of each species once R^* is reached
 - Why do all species have a positive change at the last timestep?
- **Decide on q parameter values:** Lizzie needs to do this by getting some estimates of how much SOS will shift and then make it work that way in the mode. Some info on the model bit: q is part of the β distribution that define τ_p so we need to define an appropriate q to start with and what q to end with that will yeild a realstic amount of environmental change for our simulations. Right now we starts with p and q each being 2, yielding a normal distribution centered on 0.5 and pretty wide (remember β is bounded at 0,1 and 1, 1 would yield a uniform—as the numbers get bigger the distribution gets narrower), but then we just vary q so we end up with a skewed distribution.. something like 5,15 may be better.

2 Overview of project and directions

1. We consider the effects of climate variation with a model that considers dynamics at both the intra-annual and inter-annual scale.
2. We look at how species traits related to their responses to climate variability effect coexistence and long-term persistence in the community maintenance. (This is the tracking part of the project.)

We also aim to make this project more interesting, useful and forward-thinking than others by making scenarios somewhat most realistic—we'll try to mention/use existing data to rule out and in shifts in abiotic variables (and possibly species traits). The *Physical Sciences Basis* of the IPCC came out in September 2013 so we have good recent estimates of how climate has and will shift.

Products: We currently envision two papers:

1. *How phenology and climate change structures future communities*
2. *Mayan megadroughts and climate change*

See below (or see other files) for outlines and info on each.

3 Notes from meetings

These are notes from our meetings that basically outline what we did. I have tried to remove an critical information. Meaning: you don't really need to read this if you don't want to, unless you're looking for notes on why we didn't do things or when we did certain things.

3.1 Notes from 18 April 2018

This was an exciting meeting as we had some big progress in deciding to work up a concept paper for the tracking project and coming up with an outline that seemed good with easily (err, we think) executable model runs. As such we've been working on the outline for the new paper, the model runs and culling/organizing these notes.

3.2 Notes from 26 October 2017

Okay, this was a short not super-productive meeting, but it was very short. We basically discussed:

1. Needing to do the runs in ModelsRuns/RunHistory.csv
2. Applying for some funding to get next project done on invasions: Maybe an NSF in January with Janneke Hille RisLambers and Eric Seabloom?

3.3 Notes from 16 May 2017

We worked on an outline for the paper (in April 2018, Lizzie moved these notes to phencc.tex).

Misc: Look at 1 species coexistence where the better tracker went extinct. This is sort of just another version of take two coexisting species and show that the tracker can drive the lower R^* extinct.

Plot for ‘this part of parameter space that was coexisting goes away first’ could be the old coexisting parameter space with color coding for how quickly during the non-stationary period they go away.

3.4 Notes from 7-13 January 2016

We started up in the library on Coconut Island and reviewed ...

1. Project that modeled how nonstationary environments shift coexistence via storage effect and other coexistence mechanisms.

(a) We reviewed two ways to try to do this.

- i. Empirical – thinking about papers that have actually done this as reviewing papers suggesting how to do it in the past (e.g., work by Sears and Chesson) has proved difficult

A. Godoy & Levine (2014) does it, but they use Lotka-Volterra, which is not super relevant to us

B. Angert *et al.* (2009) does it, but does not include species loss and is sort of weird

- ii. Analytical: We tried this, defining E^*, C^* given R^* model was too hard, so tried Lotka-Volterra models, but still really tricky.

(b) We decided we really need to table this for now (really, even though we have said that a lot in the past)

2. Project focused on concepts and simulations. Yeah, let’s do this!

We made some decisions about how to model nonstationarity and coexistence:

1. Nonstationary: We will do runs:

(a) with a stationary period, followed by a nonstationary period, followed by a stationary period (that’s 1 run with 3 pieces)

(b) same as above but no nonstationary period and each stationary period is run separately (compare results to above)

2. Coexistence, see also 5.4 below

(a) Long-term persistence from time-series data we will have from simulations.

(b) Show distributions of each species abundance over x number of years at start of stationary period and end of stationary period do not change.

- (c) Return from low density: We think we should only do this if we really need to (e.g., reviewers ask for it) because it will probably require more simulations to do formal invasibility criterion. Megan thought perhaps the time-series we will have could work for a cheaper version of it.

We also:

1. Fixed the ODE solver so it jumps out when $R^* = \min(R^*)$
2. Made it so the model updates each year to figure out the $\min(R^*)$
3. Decided we should vary c in order to vary R^* .

3.5 Notes from 8 August 2015 that are still true in January 2016

In May 2014 we wrote, ‘To review, the goal really is the Mayan invaders project. Once we’re there we’re in good shape.’ And that’s basically back where we ended up after a week of work in August 2015, skipping for now all the mechanisms of coexistence.

The longer story behind this outcome is that we spent about a week working through Chesson 1994 (TPB, section 5.5) and Chesson 2003 (TPB also I think) trying to figure out how to fully sort out the mechanisms of coexistence using a simple Lotka-Volterra model we started working on in January-February 2015 (though I think we started re-working on mechanisms of coexistence as of December 2013 at least). We decided for now to table this work and switch back to the R^* with a focus on the phenological tracking questions.

3.6 Overview (August 2015, still true in January 2016)

After thought, discussion, up and downs we have decided to move ahead on... **the R^* model of 2014 and before and phenological tracking.** This has several benefits:

- It focuses the work on a biologically-meaningful topic Lizzie can write easily about.
- It makes the work hinge less strongly on how we model nonstationarity.
- It does not require calculating the mechanisms of coexistence, which really would be handy to make sense out of any project where we focused more broadly on answering ‘What happens to communities when the environment becomes nonstationary?’
- The R^* is complicated enough to have interesting dynamics and make it less obvious why we do not calculate mechanisms of coexistence.
- It still lets us get to the Mayan megadrought question (which requires a pulse size to have droughts occur). Yay!

3.7 January-February 2015

Megan’s notes from 27 January 2015, are now below the end of document. Following Chesson (2003) and Godoy & Levine (2014), she tried to calculate out coexistence via mean fitness differences vs. the storage effect vs. relative non-linearity vs.

4 Current plans for products

4.1 Phenology & climate change paper

See also 7 and 6, both below.

*Many notes are now in phencc.tex. I just left notes (below) that are not part of our current approach.

1. Two species case: take the coexisting 2-species communities from stationary systems and add nonstationarity in τ_P and ...
 - (a) see how long it takes to lose one species.
 - (b) see which ones persist longest and mark on PhenTrackFig. 1 (e.g., re-do PhenTrackFig. 1 with bubble plots or such for how long the two species persist together).
2. Multi-species ($n > 2$) case: take the coexisting multi-species communities from stationary environments and add nonstationarity in τ_P and ...
 - (a) stop at X timepoint and re-do PhenTrackFig. 2 and 3 to see how they have shifted (e.g., you may lose the middle species — those that are not the best competitors nor the best trackers ...).
 - (b) extract timepoints when 10% and/or 50% of species are lost.
 - (c) extract when each species is lost in a community and order the species loss of PhenTrackFig. 2.

Measuring coexistence, a couple options

- Long-term persistence
- Run the model to persistent-looking conditions and show each species can increase from low-density using long-term mean densities of other species.

4.2 Connections to Mayan Megadroughts project

See also 8 below.

We broke down this trade-off to: Wider (probably shorter) germination curves trade off with shorter seedbank life such that ...

- Exotics have wider (probably shorter) germination curves – so they go basically whenever a pulse is, no matter what size it is.
- Natives have narrower (probably taller) germination curves – so they go only when a pulse happens at a certain time.
- Exotics have short-life seedbanks, natives have longer-life seedbanks.
- Thus, if you add in mega-droughts (repetitive low-pulse size years) the exotic species will continue to germinate a high fraction of seeds each year, but get little biomass and new seeds out of the germinating seeds, leading to a negative growth rate that

does not rebound before the whole seedbank is wiped out. Natives don't do this. (See also Figure 1 below).

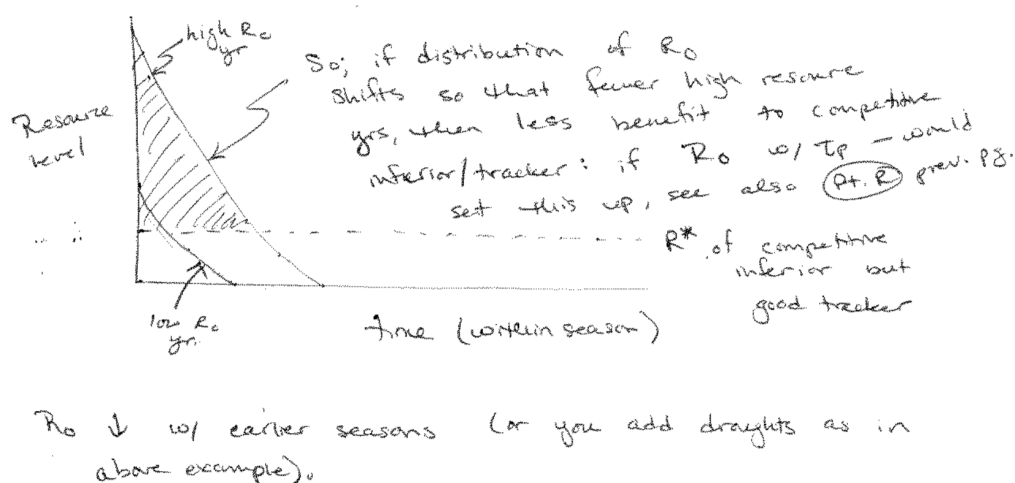


Figure 1: **How drought could negatively impact phenological trackers:** Two curves for the same species—a competitively-inferior phenological tracking species—one showing how it responds to a drought (low resource pulse, R_0) and another to how it responds in a high rain year (high resource pulse).

We talked some more about this in March 2016. A few major points from this:

- I thought we might need to correlate R_0 with τ_P such that native species' τ_i are aligned with higher R_0 . Megan pointed out if you go all the time and droughts become more frequent you still might go extinct—meaning we may not need this correlation. Especially, methinks now, if the natives have seedbanks lasting well longer than the megadrought intervals.
- Megan had this brilliant idea to flip things around. We have some parameters for this work from the Levine lab, but instead of using those we could see what parameters we get out of the species under different environments. Basically do a ton of runs with varying some species' parameters and then see what species remain. Do you see longer seedbanks and narrower germination curves in periods of megadroughts (for example)?

5 Equations and related notes: R^* model

For a species i let (note: see dimensional analysis for full list of current terms, I need to update this here someday):

N_i seedbank of species i

s_i survival of seedbank of species i , buffered pop'ln growth occurs via this constant

δ total time of growing season

B_i biomass of species i

R resource

$f_i(R)$ resource uptake rate of species i of R

c_i conversion of uptake to biomass of species i

m_i partial mortality of species i

a_i uptake increase for species i as R increases

θ_i shape of uptake of species i

u_i^{-1} max uptake of species i

$g_{max,i}$ max germination of species i

h_i max rate of germination decrease of species i following a pulse

τ_p time of pulse

τ_i time of max germination of species i

ϵ abiotic loss of resource

ϕ_i conversion of biomass of species i to seedbank, includes overwintering of seeds (as of December 2013)

b_i seedling biomass of species i

α phenological tracking of species i

5.1 Dimensional analysis

Exciting new product as of August 2012, oh la la!

Table 1: Table of parameter values, their definitions and lightweight version of their dimensions (i.e., not yet deemed ‘grams’ or such).

Parameter	Definition	Unit
N_i	seedbank of species i	seeds
s_i	survival of species i	unitless
δ (peak biomass)	total length of growing season	days
B_i	biomass of species i	biomass
R	resource	resource
c_i	conversion of R uptake to biomass of species i	$\frac{\text{biomass}}{\text{resource}}$
m_i	maintenance costs of species i	days^{-1}
a_i	uptake increase as R increases for species i	days^{-1}
u_i	max uptake for species i	$\frac{(\text{days})(\text{biomass})}{\text{resource}}$
ϕ_i	conversion of biomass to seedbank for species, includes overwintering of seeds i	biomass^{-1} , but conceptually $\frac{\text{seeds}}{(\text{biomass})(\text{seeds})}$
ϵ	abiotic loss of R	days^{-1}
$g_{\max,i}$	max germination of species i	unitless
h_i	controls the the rate at which germination declines as τ_p deviates from optimum for species i	days^{-2}
g_i	germination fraction	unitless
τ_p	timing of pulse	days
τ_i	timing of max germination of species i	days
α_i	phenological tracking of species i	unitless
θ_i	shape of uptake for species i	unitless
b_i	seedling biomass of species i	$\frac{\text{biomass}}{\text{seeds}}$
$f_i(R)$	R uptake $f(x)$ for species i	$\frac{\text{resource}}{(\text{days})(\text{biomass})}$
d_i	death rate of species i , used in calculations of lifespan	unitless
t	between year time (formerly T)	years
$0 \rightarrow \delta$	within season time (formerly τ)	days
b_0	initial biomass per germinant (seed)	biomass
ξ	$\frac{\text{final biomass}}{\text{initial biomass}}$	unitless

Note for δ : $B(t+0)$ is start of season biomass while $B(t+\delta)$ is the end of season biomass. Also note: that s applies to seeds *in* seedbank, while ϕ thus includes some death for current season seeds (see red/blue diagram from 12 May 2014 whiteboard photos).

5.2 System of equations

System of equations, for a community of n species based on resource competition:

$$N_i(t+1) = N_i(t+\delta)s_i$$

where

$$N_i(t+\delta) = N_i(t)[\text{germination fraction}][\text{seeds produced per germinant}]$$

so then:

$$N_i(t+1) = s_i(N_i(t)(1 - g_i(t)) + \phi_i B(t+\delta))$$

where:

$$\frac{dR}{dt} = - \sum_{i=1}^n f_i(R)B_i - \epsilon R$$

$$\frac{dB_i}{dt} = [c_i f_i(R) - m_i]B_i$$

and the initial condition is:

$$B(t+0) = N_i(t)g_i(t)b_{0,i}$$

where:

$$g_i = g_{max,i} e^{-h(\tau_p - \tau_i)^2}$$

$$f_i(R) = \frac{a_i R^{\theta_i}}{1 + a_i u_i R^{\theta_i}}$$

Adding phenological tracking to model (October 2013 version):

$$\alpha \in 0 \rightarrow 1$$

$$\hat{\tau}_i = \alpha \tau_p + (1 - \alpha) \tau_i$$

thus:

$$\text{when } \alpha = 0 : \hat{\tau}_i = \tau_i$$

$$\text{when } \alpha = 1 : \hat{\tau}_i = \tau_p$$

Getting this into simulation-landia means:

$$B_i(0) = [\text{number of seeds}][\text{germination fraction}][\text{seedling biomass}]$$

which also looks like:

$$B_i(0) = N_i(t) g_i b_i$$

$$B_i(t + dt) = B_i(t) + [c_i f_i R(t) - m_i] B_i(t) dt$$

5.3 Notes related to equations

Also note that I (Lizzie) made one change from the February 2011 board: I think we used h accidentally twice for different meanings: one was in the equation for g_i which we stole from Chesson *et al.* (2004) (appendix, see next note), and then one was for the total length of time for the growing season. Thus I changed this ‘season-length’ h to δ .

Finally, equations for $\frac{dB_i}{dt}$, $f_i(R)$, g_i , $\frac{dR}{dt}$ were taken from the appendix of Chesson *et al.* (2004) (*Oecologia*).

R^* does not change with nonstationarity because it is based solely on species-specific characteristics not related to the environment (says Megan, December 2013).

5.4 Definitions of coexistence

In December 2013, we discussed how we will define coexistence. There are a couple options, the second one is better:

1. Persistence of species in simulations: This is not ideal since you would have to run things a long time and still not know what would truly coexist. But if we do have to do this it is not all bad: if we run stationary then nonstationary environments (one after another, not separate) we can think of it as ‘here are the species co-occurring.

Sure, some may be on Hubbellian random walk to extinction, but that is probably how the real world works anyway.’

2. Recovery from low-density: For this we need to calculate long-term low density growth rate from either (1) simulations or (2) equations. Equations would be better and would allow us to run stationary and nonstationary simulations separately. This equation should come out of final storage effect equations.

5.5 How biomass is converted to seed and growing seasons end

The way the growing season ends in the equations is interesting. First, as brilliantly stated: the growing seasons ends [in these equations] when plants stop growing. And, related, the equations do not deal with setting the end of the growing season. In my head (Lizzie), abiotic forces can stop a growing season, but in reality with plant phenology data, the start and end of the growing season are fundamentally different: at the start species are most sensitive to abiotic cues and climate change effects are large and often consistent. For the end of the season climate change effects have been more muted and variable—suggesting plants in some way do seem to set the end of the growing season more than abiotic cues do, at least when compared to the start of season. (And the model follows this.)

So what we do about it... It used to happen for all species at δ —so, early species may have peaked and have lower-than-their-peak biomass at δ while later season species may be at peak. Instead now we let resources go to the lowest R^* and then we take peak biomass (for each species) instead of biomass at δ (when the growth rate goes below 0), to convert to seeds.

5.6 Conceptualization of our germination equations

A long debate with much thought in October 2013 (see also notes on meeting with Sally Otto, which offers an equation with a clearer cost, but also effectively assumes plants are dumber. Or to say that last bit more positively, ‘Peter’s model assumes plants are smarter,’ says Megan). Below are mainly Lizzie’s thoughts in December 2013.

I think you can compare our formulation to the basically 2-cue system discussed in Chesson *et al.* (2004). From pg. 238 he describes how each species basically has a temperature-dependent germination and how far from the pulse is from that optimum determines how much a species germinates each year. Importantly, he notes ‘The phenological difference in germination discussed above involves timing that is independent of water availability, in the sense that rain at the wrong time of the year or wrong temperature would not bring on germination or physiological activity.’ I can think of two examples you could conceptualize that are similar to this and relevant to the systems we have been thinking about.

- Snow meltout date in alpine communities as the pulse and temperature requirements (the sum of chilling and spring warming) as the other cue (this maybe works also for some other meadow communities).

- Nutrient flush date (due to microbial biomass turnover) in temperate systems as the pulse and temperature requirements (again, winter chilling and spring warming together) as the other cues.

These are a little sloppy in that the pulse is supposed to be separate from the cue I think (according to Chesson *et al.* (2004), see page 244); but this is sloppy in almost all systems in reality because climate is correlated and evolution means that species probably use that to their benefit.

So, basically I am happy with either the Chesson *et al.* (2004) formulation or the Otto formulation (which is Table 6.1, model 2 in Chesson (2008), ours is actually also in Table 6.1; it's model 3 but with variable adult biomass). The Chesson *et al.* (2004) seems one step ahead of Otto: costs in Otto's model would eventually remove most species with τ_i before the pulse while Chesson says species never germinate before the pulse, they just germinate to varying amounts depending on τ_p . One benefit of Sally's model though is that it may be better for climate change scenarios because it allows species to make 'bad decisions' (if you will) and get hammered for them; as many species have in recent years (e.g., Inouye, 2008).

Note: h yields different bet hedging strategies.

5.7 A few assumptions spelled out

We assume that:

1. All species 'go' each year, at least a little; that is, we're not looking at communities where some species have true supra-annual strategies.
2. There is one dominant pulse of the limiting resource (e.g., light or water) at the start of each growing season; thus we model a single pulse per season.

5.8 Simplifying within-year dynamics thoughts

Can we simplify within-year dynamics to focus on a better model for timing (see photo of October 2013 whiteboard that Megan has)? To rephrase this: Our internal equation is basically R^* rankings, so could we create a 'deterministic look up table' for the within-year dynamics so we can think more carefully about the timing part of this. (Related note from 2012 meeting: Decide if we need to ODE solve the intra-annual dynamics, then use the discretized version only inter-annually. Note from Lizzie: best ODE solver is now in package 'desolve'.)

6 Coexistence with climate change: Notes related to implementing and writing

6.1 Environmental variables of interest

We consider 3 primary traits of the environment (ϵ , R , τ_p , which code to evaporative stress, inter-annual variability, and start of season pulse for our approach basically) and 1 species response trait (phenology, specifically flexibility in phenology as modeled by a species' ability to shift τ_i) to model the dominant expectations of current and future climate change:

1. *Changes to R* : Shifts in climate means and variability (greater var \approx extreme events) as modeled by changes to μ and var of R , which can lead to:
 - (a) Changes in inter-annual covar(E , C)
 - (b) For variability: changes related to buffered population growth: for example, when the periodicity of certain extreme events declines such that species with certain buffering times no longer get their 'good' years enough (e.g., periodicity of rainy years every 5 years, switches to 10 and the species seedbank is 7 years). This means for simulations changing $var(R)$ must be consider in concert with the scale of $s_{i...n}$.
2. *Changes to ϵ* : Shifts in climate means that lead to greater abiotic stress on environments, as modeled by changes to ϵ . For example, warmer growing seasons may produce greater evapotranspiration, shifting competition for the remaining resource. (By the way, we have notes about treating ϵ as a function itself.) This should affect:
 - (a) Changes in inter-annual covar(E , C)
 - (b) Note that one basic prediction could be a decline in the storage effect with declines in ϵ . From Chesson *et al.* (2004):

... large evaporational [sic] water losses decrease the contribution of shallow-rooted plants to soil moisture depletion ... therefore decreasing the link between uptake and resource shortage As a result, the effectiveness of temporal resource partitioning by the storage effect would be lower in shallow-rooted species, because covariance between environment and competition would be less pronounced. Conversely, the storage effect should be stronger for plants rooted in soil layers where plant use of water is the dominant mode of depletion
3. *Changes to τ* : Longer growing seasons, with several scenarios:
 - (a) Season is longer (earlier τ_p but community of species do not shift their timing (e.g., no change to $\tau_{i...n}$)
 - (b) Season is longer (earlier τ_p and some species ('climate-trackers') change their timing (community shift in temporal—phenological—synchrony), that is (e.g., certain species change to $\tau_{i...n}$) such that the distance $\tau_p - \tau_i$ is constant across years.

- (c) Could also look at complementarity (histogram of variation in $\tau_{i...n}$; could pull $\tau_{i...n}$ from a beta distribution. (Note: I also wonder if we shouldn't just use variation due to above to look at this, versus a whole new approach.)
- 4. How do these variables shift with climate change and co-vary?
 - (a) R_θ increasing inter-annual variance with some giant years (extreme events), for snowpack systems it's decreasing generally
 - (b) τ_p getting earlier, also for snowpack systems earlier years probably also have higher evaporative stress (ϵ , due to warmer year)

6.2 How the world changes with climate change: Thoughts on correlated environmental variables

In July 2011, I looked at whether the start of spring has gotten more variable (using some key datasets from NECTAR) and it hasn't, at all. No change.

Take-home for below: So, where did we arrive at after all this? Even though we used to be interested in correlated shifts in these we're not so much anymore. Instead the *best place to start* seems to be: earlier τ_p and increases in ϵ and ϵ extreme events. Then maybe move on to R_0 (mean and extreme events). And again, just skip correlations.

Environmental shifts with climate change, from October 2013 meeting:

Systems we're thinking about (see above) but, effectively, alpine where snowpack meltout is start of season (SOS), nutrient turnover SOS and some precip controlled systems with just one pulse.

1. τ_p will get earlier in many systems (alpine and nutrient), not sure on precip—there it might just get more variable
2. ϵ increase in mean and increase in extreme events (this seems pretty possible across a lot of systems—good one)
3. R_0 increase or decrease in mean maybe (who knows what happens in precip system), increase in variance (for precip systems), increase in extreme events
4. Correlations in τ_p and ϵ - this basically says 'does the first day of the growing season correlate with the average temperature of the growing season?' which, yeah, is weird. Lizzie thinks there's probably a weak positive covariance here (just because there's a lot of noise in annual weather but wet soils hold cold, and dry soils hold heat so *all other things being equal* you could see this correlation. From an email from Ben Cook from 30 May 2013:

So, both Europe and the Northeast US have had super crappy springs in March and April (so annoying!). Mostly it was due to an almost unprecedented negative swing of the NAO, which just funneled lots of cold arctic air in the region. As for the summer? Harder to tell. If an area gets lots of rain

in the spring, it can mean really wet soil which can keep things cooler in the summer. But other than that, we don't really have much skill in predicting summer time climate.

Otherwise, not sure anyone has looked at this at all in precip systems and for snowpack there may be something here since Pederson et al. 2011 says that snowpack control has basically shifted from precipitation control to temperature control so with climate change you could start seeing a correlation in alpine systems between τ_p and ϵ (which is kind of cool, but sort of something that is not low-hanging, obvious fruit – or maybe it's low hanging fruit and only obvious to us).

5. Correlations in ϵ and R_0 maybe for alpine you could see, with climate change, increases in R_0 and decreases in ϵ , but we're still bickering on this and what ϵ really is (incident irradiance vs. relative humidity?)
6. Correlations in R_0 and τ nothing to date or in future? Maybe in alpine systems?

Note that increased extreme events for ϵ will effectively reduce biomass (you could also consider modeling this so it ties to m_i or modify g_i to lead to total loss, perhaps).

Frost note here! While here, though, I will mention that we discussed *which* correlations will shift with climate change and the big one seems to be the North American alpine story: where the relationship between how τ_p affects mortality has shifted with species' τ_i such that there is now higher mortality imposed on species with earlier τ_i . We did briefly discuss how to model this and think maybe the best way is to add in, external to the within-year integral, a fraction that germinated and died as a $f(x)$ of τ_i and frost event time.

7 Tracking thoughts & fun

In August 2012 Megan came up with a new way to model phenological tracking, it's linear and better than the one presented here. We also discussed three key ways to think of tracking:

1. Species can have fixed flowering/leafing (not track).
2. Species can phenologically track pulse.
3. Species can phenologically track something at least at one time in history correlated with the pulse.

We agreed that while (3) is interesting—would allow you to look at mismatch ideas etc.—(1) and (2) have more biological support for plants and are interesting enough in and of themselves, so we will focus on and model (1) and (2) and not (3).

In August 2013 an issue we worried about: Will tracker always win? Answers: No, not if it's a poor competitor, that is, if it responds quickly to resource pulse but then does poorly

at low resource levels. I also think it's important to remember that not all tracking species will track perfectly so some species should grow before trackers some years, when their fixed τ_i corresponds well to τ_p . Also, we have a note that high intraspecific competition could dampen trackers, especially when greater than interspecific competition.

This tracking angle matches to the 'Generalists, specialists and plasticity' section of Chesson *et al.* (2004). You could imagine by removing the benefit of trade-offs associated with not being plastic, then nonstationarity could favour generalists (plastic species, that is). Here's the most relevant bit (according to Lizzie):

However, plasticity, or any generalist resource consumption behaviors, including those involving drought resistance, may come at a cost In such circumstances, there is no contradiction that a generalist can coexist with specialists so long as the specialists are in fact superior performers during the times or conditions that favor them, and there are some times when no specialists are favored so that the generalist is then superior.

As of August 2015 we decided **against doing tracking with joint distributions** (see notes above 4.1), but here are the old notes on it:

Next up, I need to work on how to make joint distributions in R (all different types, not just multivariate normal ones) so we can link α_i with c_i . For this I need to look into copulas and the R package `copula`. See also these pages:

<http://www.r-bloggers.com/copulas-made-easy/>

<https://sites.google.com/a/wisc.edu/jed-frees/multivariate-regression-using-copulas>

Without tracking we may predict benefits to early-colonizers decline with earlier seasons. As start-date moves earlier, early folks lose benefit (assuming they tend to often go at optimum time) and you get more late folks. Late species may be less different than one another—and less responsive to environment. Early folks, effectively, become more similar to environment.

8 Notes on megadroughts paper

Temporal redness, Mayan megadroughts and Californian invaders

or

Red noise and the sunset of Mayans and European invaders

Can we use this model to test if certain strategies (say, high tracking) can increase over shorter timescales but be completely excluded from this system given rarer, long-term (red) periods of environmental ‘harshness’?

To expand: My theory works as such: If a closed population of a species has a relatively short length for its buffered population growth—for example, imagine an annual grass with a 3 year seedbank—and enters a new system where the climate has long term fluctuations, could that population possibly do well during certain environmental phases, but be completely excluded (local extinction) during others. For example, the scenario of California annual invasive grasses: they do great now, but less well in drought years and large very long droughts (Mayan megadroughts are part of the ENSO cycle, possibly, and are decade or multi-decadal) could possibly throw them entirely out of the system. Thus the answer to the question I always get: If European annual grasses do so well in coastal sage scrub why weren’t there any native annual grasses? would be—wait a while and then you’ll see.

There is also an evolutionary angle to this: short lifespans and short seedbanks with these sorts of long-term cycles could lead to evolving quickly, during one climate phase, towards a damn stupid strategy when the next climate phase kicks in. But I don’t want to go there, just to point it out.

Opportunity to coin new term (from one term I hate and one that I like): **invasion extinction debt**, the number of invaders that would go extinct if you wait a very long time.

We could bring Ben Cook on this project to give us some info on how frequencies of droughts have shifted over the past 2K years or so in North America. Or, maybe we could look at some shifts in drought periodicities globally just to press home the message that these can vary a lot across time.

Refs from Doug: (???)

9 Storage effect notes

9.1 Miscellaneous notes

The lost paper: *Shifting drivers of coexistence with climate change*, which includes all the work to date (December 2013), plus phenological tracking.

Predictions: Even though we are not shifting $var(R_0), var(\tau_p), var(\epsilon)$ we are shifting E (species' responses to environment) and thus $var(E)$ may increase. That could be cool.

9.2 Notes on coexistence mechanisms

We spent a substantial amount of time carefully working through Chesson 1994. A few notes on how this might apply to nonstationarity

- In the General Model, growth rate is divided into E and C components. To examine the effects of fluctuations using a Taylor approximation, r is examined around E^*, C^* , where growth rate is 0.
- The model is reparameterized in terms of curly- E , curly- C , which is the complete growth function, reparameterized around C^* and E^* , respectively. To make the Taylor approximation, it is limited to variability around E^*, C^* of order O_2
- the approximation is also limited to the behavior of the function around the point E^*, C^* , such that if the curvature of the function changed at a point far from E^*, C^* , this nonlinearity would not be captured.
- the approximations are made with the assumption of a stationary process, without temporal autocorrelation and drawn from the same distribution through time. This approximation is the one that makes studying nonstationarity with these approximations difficult.
- we came up with a couple ideas about how coexistence mechanism partitioning might still be useful, even without stationarity. For example, you could compare coexistence and coexistence mechanisms present at the beginning and end of a period of change. The likely outcome is that species will continue to persist after they should have gone extinct because of lags. It is hard to think of a case when the converse would be true, at least under the case of mean change. If the variance is changing, then the outcomes are less obvious and might include increased coexistence

I worked through Section 4: Analysis of the General Model, however, actually doing those calculations with any particular model is tricky, particularly making an assumption for E^*, C^* . Using Section 5.5 as an example, I tried to work through the calculations for our simple model. Equation 95 is largely equivalent to our simple model. The only difference is that we originally parameterized competition as $(1 + \sum_{j=1}^{j=n} g_{j,t} * X_{j,t} * \alpha_{ij})$, but equation 96 has competition as a simple sum, with no additional $+1$.

Section 5.5 is the same as our simplified model. Understanding through Equation 99 is clear. Table II lists the parameters of the seedbank model, and the definition of these terms is clear. To understand the terms in Table III, we still need to define ρ and σ and to make the correct assumption about the value of E^* and C^* . The assumptions around E^*, C^* are a sticking point and where I got stuck. Looking back at the notes now, however, it seems like there may still be some room forward here. We are proceeding on the simulations,

but Megan will spend a little more time to see if we can actually make the calculations in Table III.

9.3 Tiny notes about the storage effect to help Lizzie

The 3 ingredients of the storage effect are:

1. differential response to the environment (subadditivity)
2. $\text{covar}(E, C)$
3. buffered population growth

Ingredients for the storage effect *in our model*:

- buffered population growth: s_i
- variable response to the environment: τ_i , which generates:
- $\text{covar}(E, C)$

10 Miscellaneous notes

We keep an eye on who has cited Chesson et al. 2004 and for actual modeling work it's just Chesson, and for that it's all his seed predation work.

More random notes: Chesson (2000) (pg. 354) r holds temporal variation in δI and δN so effectively:

- species can coexist via intra-annual temporal mechanisms (for example, maybe this is what most mid-season species in the mesic temperate zone do), and/or
- species can coexist via inter-annual temporal mechanisms (maybe early-season species in most mesic temperate environments)

10.1 Some random notes from the whiteboard

Relative nonlinearity is:

$$\left(\frac{d^2}{dR^2} \right) (\text{var}(R))$$

Non-additivity (γ) is (in general, still working on what it is for our equations) when considering population growth (r_i):

$$r_i = \omega_i(E_i, C)$$

$$\gamma = \frac{\partial \omega}{\partial E \partial C}$$

but, what is E and C in our system?

$$C = - \sum_{i=1}^n f_i(R) B_i \rightarrow f_i R$$

is this (above) the response to competition? An alternative note we had with many question marks was:

$$\text{covar}(E, C) \approx \text{covar} \left(R_i, \sum_{i=1}^n B_i \right)$$

10.2 Small, somewhat random, but *fascinating* questions

So what does it mean if species co-exist in the same community via mechanisms operating on totally different timescales?

New random query: Is there humidity without plants?

Also, someday, you could wonder if earlier τ_p allows for more niche space for species at the end of the season that have a really low R^* .

Also, totally random note for Lizzie: Trees use previous year's resources to flush leaves/flowers in current year.

11 References

References

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12 Almighty figure aspirations

Figure 3 and 4 are ideas from Lizzie as of 11 December 2013, see also some older figure ideas at end of the paper (from 2011).

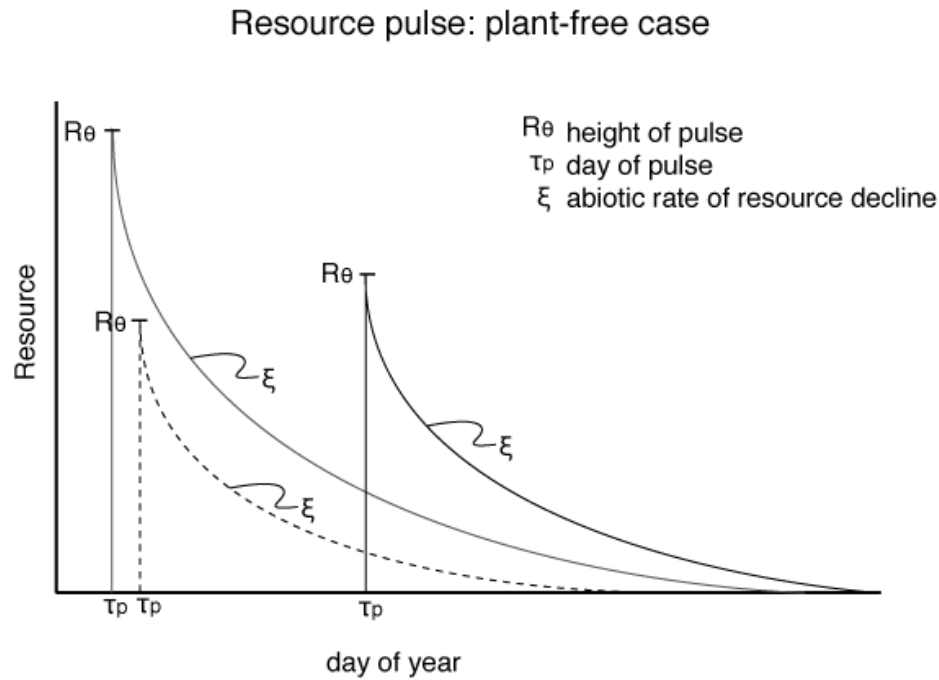


Figure 2: **Major coexistence variables directly affected by climate change** We focus on three major coexistence variables that have been (or will be) influenced by climate change—a couple examples of how varying them changes the resource pulse (without plants).

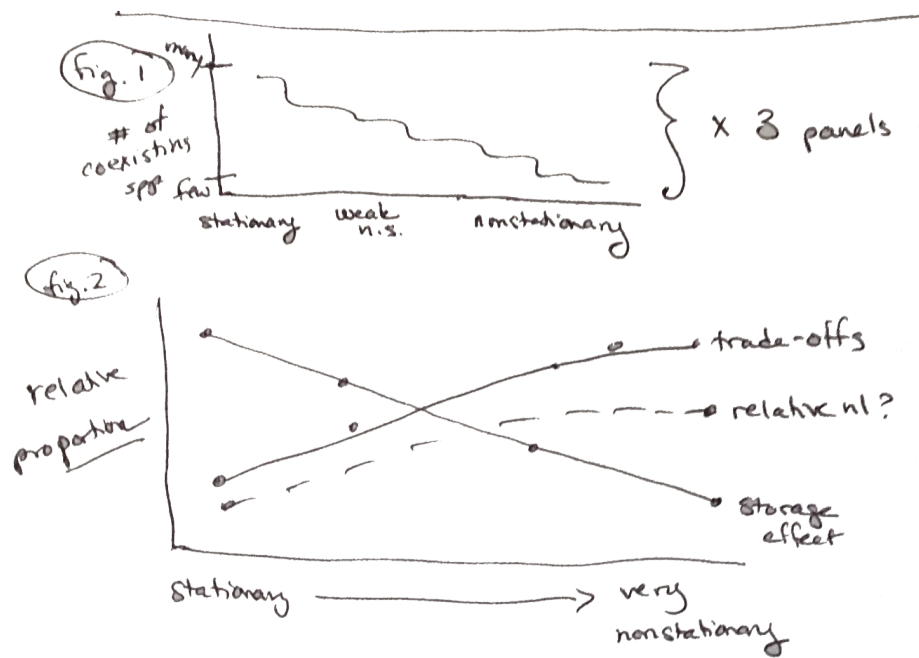


Figure 3: **How does climate change affect mechanisms of coexistence?** Two figures: the first is simple it's just the number (or percent of total possible) of species coexisting; the second figure is to look at how mechanisms shift with scenarios that are stationary, weakly nonstationary or strongly nonstationary. Each of these figures should be one panel for each variable manipulated (ϵ , τ_p and maybe R_0).

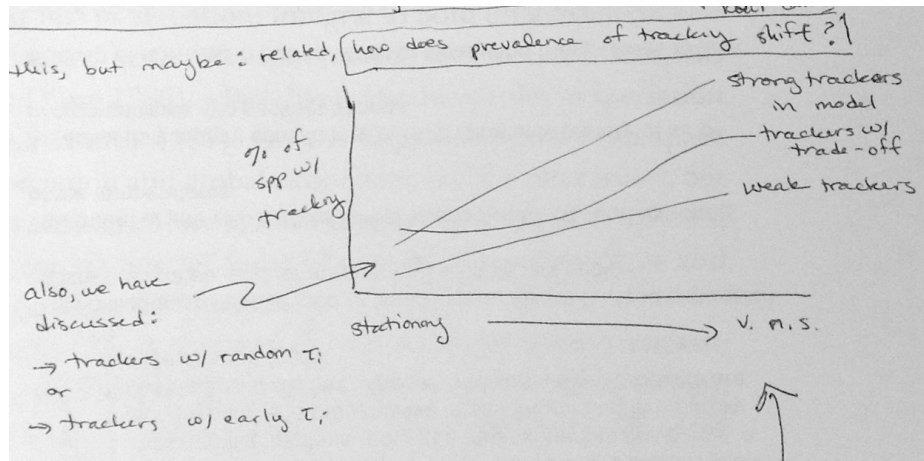


Figure 4: **How does the prevalence of tracking shift with nonstationarity?** See the section on tracking questions above for the full plethora of questions related to this (e.g., this is also the realm of how strong does the negative correlation between tracking and competitive ability have to be to make tracking non-advantageous). But, basically we want to look at how tracking shifts in stationary versus nonstationary systems. So we could do something like this (we also discussed looking at trackers with random τ_i versus trackers with early τ_i as you usually see in nature) and/or we could re-do the mechanisms of coexistence figures above with trackers in the mix.