The article is a projection of global biodiversity a hundred years from its publication date in 2000. The paper uses established climate, vegetation and land use models to predict worldwide changes in terrestrial species richness (I think it’s richness, at least), identify the political regions and biomes that are most vulnerable, and to see which force is most responsible for the changes. They identified five variables, or “drivers,” and modified them on a scale of 1 (minor) to 5 (major) and assessed the results. The authors concluded that land use by humans would be the most culpable of the five drivers, followed by climate change, flux of nitrogen, biotic exchange and pCO2 (in that order). Their modeling showed that each biome would owe its biodiversity change to one driver over the others, and that these primary drivers varied across biomes. With this, they modeled the possible outcomes in which the five drivers either didn’t interact, did interact, or where each biome’s biodiversity was determined only by its primary driver of biodiversity change. The tropics, arctic, temperate grasslands and the Mediterranean were all identified as the most vulnerable, although to different drievrs an combinations of drivers.

I found the study to be well-constructed (barring one or two notable issues) and in service of a crucial goal. The layfolk are bombarded daily with soundbites about global climatic disaster and how we’re losing the tree frogs and ocelots and whatnot and that’s a bad thing. Predictions about the longevity of one or another photogenic species are problematic in that they blur out the true extent of the damage done to an ecosystem (media love to seize on these prognoses). When a broad array of organisms are affected, pinpointing a few can distort the public’s understanding of biodiversity and its importance, and can offset any sense of urgency or responsibility. So, studies such as these that make projections for all current known life on earth are essential to understanding how humans will affect life (not just climate) and in what political regions our impact will be most severe and immediate. While there are some problems in the way they constructed their study, their results are solid and align with both common sense and previous work in these areas. In that sense, the paper is as much a synthesis as it is a prediction, and this gives it an added level of confidence. The authors pushed their results further than expected; obtaining both the expected magnitude of biodiversity change in each biome, and the primary driver of change for each would have made for a paper on its own. They went one step ahead and combined their results in three separate ways, to get an even better (if possibly lopsided) view of how global biodiversity might trend.

I’m puzzled by the decision to exclude marine organisms from the study, as marine organisms are among the best-studied in the fossil record and their responses to past climate change are fairly well-known. Oceanic responses to rising CO2 levels, human use of its resources and the circulation of micronutrients are a hot topic in the sciences these days, and the ocean’s state is regarded to be crucial to climate patterns worldwide. So, I should think that marine biotic responses to anthropogenic climate change would be especially illuminating when examining the future of biodiversity (especially when CO2 levels are one of the key drivers considered in your study). Additionally, the authors discuss the impact of land use on freshwater waterways, without considering the fact that all waterways lead to the global ocean. I’m unfamiliar with the climate model they used, and whether or not it accounted for ocean processes, but my gripe stands.

Obviously, this is a model, and is therefore inaccurate but useful, but since climate change and atmospheric CO2 are indelibly linked (largely thanks to oceans), land use contributes to atmospheric CO2 and N deposition, climate change will alter how humans use land, and changes in any of these will change how organisms interact, it seems maybe a tad too simple to consider each individually. They acknowledge this, but I expect their synergistic projections may be a bit wonky due to them altering all five in unison without accounting for changes within drivers. I realize this critique is presumptuous, but I’d have liked to see some assessment of how their projections might have varied had they had the budget/time/ability/technology/inclination to account for the interdependence of their independent variables.

The figures were serviceable, although the authors were oddly repetitive in their use of charts and graphics. Figures 1 and 2 were both very useful in assessing the relative magnitudes of the five drivers, and I especially appreciated that they broke down the drivers’ magnitude by each “biome,” as this helped me to organize their methods and results while I was reading. However, these graphs were paired with tables 2 and 3, which were so similar in purpose (although not exactly the same) that they didn’t advance the paper’s argument or aid in conveying its content. They were also hard to interpret, as it is easy to misread the charts when they’re displayed on a glowing screen or are in tiny print. They would have been better used as another graph, or perhaps combined with graphs 1 and 2. I liked figure 3, the map; seeing the changes laid out, especially with regards to the interactions among drivers, was very helpful and an excellent choice of visuals. In fact, I can’t think of another part of the paper that needed such illustration, as the map summed up both the intensity and spatial patterns of changes in biodiversity—the paper in a nutshell.