Endangered Species Act Status Review Report: Sunflower Sea Star (*Pycnopodia helianthoides*): ID454

Peer Review Comments

We solicited review of the draft Endangered Species Act Status Review Report for Sunflower Sea Star (*Pycnopodia helianthoides*). Five people agreed to serve as peer reviewers. Reviewer comments are compiled below, but not in the order that reviewers are listed. Editorial and content suggestions to the text are incorporated into the final status review report document as appropriate.

Reviewers (listed alphabetically):

Dr. Alyssa Gehman
Hakai Institute and
Institute of Fisheries
University of British Columbia

Dr. Sarah Gravem
Department of Integrative Biology
Oregon State University

Dr. Sara Hamilton
Bodega Marine Laboratory
Coastal and Marine Sciences Institute
University of California, Davis

Dr. Walter Heady
California Chapter
The Nature Conservancy

Dr. Jason Hodin
Friday Harbor Laboratories
University of Washington

Overarching Responses to Draft Status Review (reviewer numbers not associated with order of names as they appear above)**:**

Reviewer 1:

Much of this report is well-researched and technically sound, and I was pleased to learn some new facts about *Pycnopodia* and sea stars. The larval and reproductive ecology section is particularly well researched. I found the discussion about creating three different regions valuable and convincing. The report identifies most of the key stressors and threats contributing to sunflower sea stars well. I was convinced by the report's logic that the southern region didn't constitute a biological significant part of the total population, despite being initially skeptical. This report clearly took a lot of work and I appreciate the co-author team's hard work.

I do have some outstanding concerns about the report that I will take some time to lay out here. I feel like some of the phrasing in the report may underplay some of the current risks to the population, especially given several pieces of emerging and qualitative evidence. My main concern is that the report consistently presents two scenarios for *Pycnopodia* population trajectory moving forward, one being that SSWS no longer significantly impacts the population and that the growth rate will reach pre-epidemic levels in the coming years and the other being that SSWS is still significantly impacting the population and the growth rate remains depressed. While that first scenario is possible, I don't find it realistic. For one, we know that SSWS is still impacting sea stars across the region. It's not at epidemic levels any more but as far as I understand and have seen, it is still quite common to see stars with SSWS like symptoms in the field from CA - AK. Even if the remaining Pycnos have some

resistance to SSWS, we know this disease is still in the system. While I'm not aware of any studies that document continuing SSWS in Pycnos in the last couple of years, given that the majority of the population now lives in places sparsely populated by humans and given that Pycnos often die within 24-48 hours of showing symptoms, I would honestly be surprised if these mortality events were being regularly documented. It's kind of like COVID right? Even though it's not at epidemic levels anymore doesn't mean it's gone or that it is no longer a real threat to humans, particularly vulnerable populations. Given what we know, it's very likely SSWS is still a real threat to Pycnos and I'm not sure why this report seems to present a no-SSWS scenario as a realistic potential trajectory for the species. At the minimum I think the authors should explicitly state that this scenario is an absolute best case scenario that is not very likely based on what we know about diseases in general and SSWS in particular. Perhaps you did state this and I missed it!

Another real concern I have with this scenario is the idea that population growth rates will rebound in the coming years. Hamilton et al. (2021) found that no region showed any meaningful recovery in population density or occurrence since 2016. This is especially true concerning the southern third of the range. At this point, it's been almost a decade since Pycnos were wiped out in this southern region and we've seen no meaningful rebound in populations despite the fact that the public and scientists up and down this region have been actively looking for Pycnos. We know the remaining stars are probably very isolated given their low densities and likely susceptible to Allee effects. Just because, across hundreds of miles and thousands of divers, *maybe* 5-10 Pycnos have been spotted, doesn't feel reassuring to me. To me the fact that so, so, so few have been found despite the high potential fecundity suggests a population that is in real trouble, not one that is likely to sudden starting rapidly reproducing without significant intervention.

As for the other two regions, while I agree that in Alaska we are generally so data-limited, that its possible populations are rebounding, I also am suspicious that the Salish Sea region will see any meaningful population rebound anytime soon. Again, in the inner waters of Washington and British Columbia, most surveys show no meaningful rebound in populations yet, despite the fact that the disease first showed up in Washington in 2013. There are a lot of divers in that region (at least the southern half) and if they haven't begun to document serious recruitment pulses and new adults, then I think we have a real reason to be worried. Further, I'm worried that some of the increases seen, for instance, in REEF surveys or other surveys, are small pulses of juveniles that *may or may not* be living to adulthood, rather than reproductive stock. Overall, I agree that it's possible these populations in the northern half of the range will begin growing again in the coming decade, but that doesn't feel very likely to me. I'm concerned that having this be one of the main scenarios considered in the report may overstate the likelihood of this to readers.

Additionally, I have a statistical concerns with the analysis. I understand that MARSS analyses are a regular approach used to do PVA modeling, but I fundamentally disagree with the technique of replacing 0s with NAs, particularly when it comes to dealing with endangered species. I understand the issues around non-detection, but why do you only mistrust the 0 values? Just because divers spotted 2 Pycnos, doesn't mean there aren't more Pycnos in that area, but you aren't adjusting for nondetection around non-zero values. I read the appendix, I understand most of the argument for removing the 0s, and I do feel heartened that some of the analyses show a minimal difference between an analysis with NAs vs one with very small values. But look at Fig 8. The difference between the full model with NAs and the full model with very small values is very different and when you keep small values in there, the extinction risk is much, much higher. Overall, I understand the statistical reasons for doing this, but I just don't feel good about throwing out 0 values when assessing a species' decline. I worry, also, that this might seem suspicious to readers of this report who don't have a deep statistical background.

Reviewer 2:

I recommend acknowledging the contributions of all those authors and other contributors that were cited in the Gravem et al. IUCN report and Hamilton et al. (2021). These folks did a lot of work, and the data contributors especially were very generous. Also, it would be good to acknowledge The Nature Conservancy for funding the majority of the work on *Pycnopodia* in recent years. Special consideration and acknowledgement of the Canadian First Nations contributions should be included.

Reviewer 3:

My overall sense is that the SRT met their charge. I thought that the review of the data as well as the analyses seemed thorough and about as comprehensive as could be expected.

The most disappointing thing about the document was not the fault of the SRT: namely, the data that the SRT relied on was in many cases (maybe most cases) too incomplete to be able to fully assess the risk of extinction. Over and over again the report speaks about key gaps in our understanding, including absolutely crucial gaps such as whether or not there has been any acquiring of resistance to SSW in wild populations! That is such an essential piece of information to asses future risk that without it, it is very difficult to be confident in the conclusions. Available information was also too incomplete to be able to be confident about the assignment of what constitutes a "significant portion of the range". Additional basic data on population connectivity would be extremely helpful in assessing whether the three/four regions that the SRT adopted as a way to divide up their range was a reasonable division. And finally, the data on which the "West Coast" portion of the range is not considered a significant portion of the range is also based on fairly flimsy grounds in my reading. If it turns out that the southern populations are or were unique in some way, then that could significantly change this conclusion, and would have presumably resulted in a "high risk of extinction" conclusion in that part of the range. This would presumably have, in turn, been likely to change the overall recommended status from threatened to endangered. As such, that one difference could have huge implications.

I do get why the SRT concluded that these thousands of kms of their historic range was not "significant" but it is an odd conclusion. It is even more odd given one apparent consequence of their near disappearance in California: a complete ecosystem collapse in kelp forests in NoCal. While I get the logic of the SRT, the SRT must also have been aware how odd it sounds to say that the near total disappearance of this one species may have significantly contributed to a near complete ecosystem collapse of the most important subtidal ecosystem in the region. And yet sunflower star disappearance from this region would not be grounds for an endangered species recommendation because this region is not considered "a significant portion of their range." Perhaps this is pointing out the inadequacy of the ESA rather than the inadequacy of the SRT, which as I said, appears to have done its work admirably according to the charge.

So, in sum, to me the major shortcoming here is lack of relevant data, and it seems to me that there should be a mechanism by which the SRT or NOAA or some government body could come to the conclusion that we need much more and better data, and we need it urgently. And then once we have such data on basic life history and population issues and the like, then this conclusion of moderate risk should be revisited.

There is one thing I would have liked to have seen that I did not see in Appendix A: an explicit statistical analysis of whether there is any signal of recent recovery in any region - or rather what is the confidence (p value as it were) in the conclusion that there is no sign of recent recover except in a couple of individual datasets. I feel as though the conclusions here were rather anecdotal rather than statistical, but I also readily admit that I could have missed where these statistics on this specific question were presented. The larger issue is: are declines due to SSW still occurring? If it is a density-dependent disease (which I would think it probably is) is any slowdown in rate of decline simply due to lower transmission risk because of smaller populations? And how can this be assessed? I feel that the report could have made this analysis more clear or said clearly what data would be needed to be able to undertake such an analysis

In my review I pointed to some additional information that I and others have collected but not yet published that could impact the conclusions for the Salish Sea region. I did not mention that the emerging population genetics info (unpublished) from UC Merced team Schiebelhut & Dawson is critical for assessing the aforementioned issues of population connectivity, especially as it relates to the "West Coast" portion of the range. I urge the SRT to contact these researchers and see if they have relevant data to share.

I was surprised not to see more discussion of an emerging picture of association of juveniles with eelgrass in WA state and Alaska, though again I suppose most of that info is anecdotal.

I am skeptical of reports of juveniles failing to grow up into adults unless wasting was actually observed in the field in these locales. Once they reach a certain size I imagine they stop sheltering and are mobile. They could migrate significant distances from their nursery sites at that time, and hence still be alive, just dispersed.

It was enjoyable to read the report. I really did think it was very well done, the most comprehensive review of *Pycnopodia helianthoides* biology that I have seen, so it's a wonderful resource for that reason alone.

Reviewer 4:

The Status Review Team did an exemplary job evaluating and reviewing the available data on Pycnopodia. The summary of the state of knowledge around the biology of Pycnopodia was thorough. For the new analyses, I think the approach was appropriate, that the consideration of the uncertainty was carefully thought through and that there was clear and demonstrated evaluation of assumptions and inferences.

Reviewer 5:

Within each section, there often is a lot of information synthesized to draw inference to *P. helianthoides* providing a very thorough and in-depth review of available information. The information presented varies in how directly related to *P. helianthoides* across many factors including taxonomic relatedness, laboratory vs. field, geography, and time-frames creating challenges in drawing appropriate or relevant inferences. These disparate allusions are often presented without how they do or do not relate to *P. helianthoides* leading to confusion of the matter. Often following these arrays of information the authors provide only one brief or even no sentence summarizing the information or suggesting how it applies to *P. helianthoides*.

For each sub-section, I recommend the authors: 1. Focus on the most relevant information (including deleting potentially irrelevant, confusing, or distracting references), 2. make effort to interpret these sometimes confusing and conflicting analogies and relate to *P. helianthoides* as they are presented, and 3. Provide a summary paragraph that clearly and directly summarizes how these inferences likely apply to *P. helianthoides*, demographics, population viability factors, and extinction risk. This is particularly important to Sections 2 and 3 (but relevant to all sections) – to set up factors and relationships to be considered in subsequent sections 4-7. I recommend beginning each section with how this information is important to assessing population status. A good example of this is Section 2.3.1.4.2. Then discussing what is known. End with a summary of how what is known does apply to *P. helianthoides* and implications to population stability and extinction risk.

Cross-referencing sections that describe or use a given point being made will help the reader understand that either more details are to come or more details have been described in another fashion in these cross-referenced sections. This will help the reader navigate this report and help with the organization, structure, and utility of the report.

<u>Editorial Comments by Report Section</u> (reviewer numbers are not associated with order of names as they appear above)**:**

Reviewer 2: Global - Change Cote to Côté throughout.

Reviewer 2: Page iv - "The species is most abundant, however, in the waters off **south**eastern Alaska and British Columbia."

Reviewer 2: Page iv - *"Pycnopodia helianthoides* hunts a range of bivalves, gastropods, crustaceans," – Add echinoderms to this list since a main prey is sea urchins.

Reviewer 2: Page iv - *"P. helianthoides* fills the role of a keystone mesopredator." - Change "keystone" to "key" or "important". Keystone predator refers to an outsized effect on biodiversity, and that has not yet been established for this species.

Reviewer 2: Page v - "Environmental factors such as temperature and dissolved oxygen likely contributed to the pandemic" – The evidence for dissolved oxygen contributing to the disease outbreak is weak. There is evidence that very local (in fish tanks) low DO exacerbates disease, but there is no convincing evidence that the disease outbreak was linked to low DO events on a large scale.

Reviewer 1: Page v - "individual growth rates have not been validated" – I know Sarah Gravem and Alyssa Gehman both have been doing cohort-based studies tracking growth over time. Much of that work isn't published yet, but is probably worth mentioning.

Reviewer 5: Page 4 - "*Pycnopodia helianthoides* is generally more common from the Alaska Peninsula to Monterey, California." - It is misleading to use 'present tense' when describing historical distribution. "Pycno *were historically* more common from…" Presently Pycno *is* generally absent along the outer coast of WA, OR, CA, MX – and therefore *is* generally more common from Puget Sound to Alaska.

Reviewer 5: Page 4 - Insert "While considered a habitat generalist it is unknown if certain habitats may influence survival or growth of certain life stages disproportionately including for spawning aggregations, recruitment, or as nursery habitats; understanding such relationships will help inform population viability and extinction risk as well as management and conservation actions (Heady et al. 2022)," and cite TNC Roadmap to Recovery for the species.

Reviewer 2: Page 5 - Suggested change to: "2) the propensity to record all sea stars as "sea star unidentified" when they occur incidentally in various survey and fishery records **in deeper waters**."

Reviewer 5: Page 5 - Change to read "Like most asteroid species, *P. helianthoides* has separate sexes . . . "

Reviewer 5: Page 5 - "Sex ratios are commonly equal in sexually reproducing sea stars . . ." - This is interesting and needs clarification – the scope of cited investigations – geographic/habitat diversity scales, species considered, etc. It is worth noting that this is unknown for *P. helianthoides* – AND – that 1) sex ratios are affected by many abiotic and biotic factors including impacts considered in this assessment (environmental temperature and disease), and 2) that sex ratios can have an important impact/influence on recovery potential, recovery rates, and population viability. Thus this merits further research and should be a consideration for any conservation or recovery efforts.

Reviewer 5: Page 5 - "Some asteroid species brood embryos and bypass a planktonic larval phase or produce a pelagic larva that does not feed in the plankton (i.e., lecithotrophic) (Strathmann 1987; Chia and Walker 1991; Byrne 2013)." - The reason for including this is unclear. Is this potential in *P. helianthoides*? Should this be explored or considered? If so – that should be stated clearly here. If not – this seems distracting and irrelevant and should be deleted.

Reviewers 1+4: Page 6 - "no one has examined temporal changes in gonad index (aka, gonadosomatic index) or seasonal changes in oocyte diameter in field-collected *P. helianthoides*" – doesn't Sarah Gravem have some of this data?

Reviewer 2: Page 6 - "no one has examined temporal changes in gonad index (aka, gonadosomatic index) or seasonal changes in oocyte diameter in field-collected *P. helianthoides*." – I should have these seasonal gonad index data within a few months from southeast Alaska. I also have been collecting spawning records. From my preliminary results, it appears that they are most ripe in the early spring (high gonad indices), and that spawning most often occurs in May and June, at least in the Salish Sea.

Reviewer 3: Page 6 - "Through top-down predatory control of sea urchins and other kelp predators, *P. helianthoides* fills the role of a keystone mesopredator." – My understanding is that most ecologists would not agree that Pyc meets the definition of keystone predator. They are clearly a top predator.

Reviewer 3: Page 6 - "*P. helianthoides* is known to seasonally aggregate," – *Pycnopodia* aggregate throughout the year, not just seasonally.

Reviewer 2: Page 6 - "Gravem et al. (2021, Supplementary Information) cited Feder and Christensen (1966) as saying that the breeding season is May-June around Vancouver Island, British Columbia. However, we were unable to locate any information on the breeding season of the sunflower sea star in Feder and Christensen (1966)." – It seems that the incorrect reference was accidentally inserted during revisions of Gravem et al. (2021). It should have been Greer (1962) and Mortensen (1921).

Reviewer 3: Page 7 - "Parameters such as age/size at first maturity, fecundity, longevity, reproductive life span, and individual growth rate have not been validated." – Age at first maturity is 3 years in the lab. Females release millions of eggs per spawning event, as do most medium to large sea stars with feeding larvae. Lifetime fecundity is unknown because life span is unknown. We have extensive lab-based growth rate data. Ken Collins has field data that matches our lab data quite well.

Reviewer 1: Page 9 - "We were unable to find direct estimates of fecundity for female *P. helianthoides*" – again, doesn't Sarah Gravem have data on this?

Reviewer 5: Page 11 - "Without additional information on the size at first maturity, fecundity, reproductive seasonality, and reproductive senescence of the sunflower sea star . . ." - This is a better summary. It is recommended to begin this paragraph with a statement summarizing the lack of information specific to *P. helianthoides* specific to this section.

Reviewer 2: Page 12 - "This uncertainty is highlighted by two conflicting statements in the Supplementary Information section of Gravem et al. (2021) . . ." - While these statements in Gravem et al. (2021) could

certainly have been worded more clearly, they aren't "conflicting". They state that *Pycnopodia* sometimes aggregate, but it is not clear whether these aggregations are intentional nor related to spawning. It could easily be that they aggregate in favorable foraging grounds.

Reviewer 4: Page 13 - I am not sure if it is necessary, given that there are images of domed spawning postures in *Pycnopodia*, but I can add a pers. comm of the observation. I saw a domed *Pycnopodia* spawning in the Central Coast of BC in May of 2022.

Reviewer 5: Page 14 - "Nevertheless, successful fertilization rates of the crown-of-thorns sea star *Acanthaster* spp. during a natural mass spawning event were estimated at 83% (Babcock and Mundy 1992)." - The way this is written implies that crown-of-thorns do not need close proximity or synchrony; and crown-of-thorns have been compared directly in size and fecundity to *P. helianthoides*. However, in order to make this connection, this statement needs clarifying context. What was the density, nearest neighbor distance, and degree of synchrony observed by Babcock and Mundy (1992)? While explained below in another thread – these contexts need to be provided here.

Reviewer 3: Page 15 - "Very young *P. helianthoides* generally have less than a dozen rays" – This is an odd sentence. *Pycnopodia* settles with between 3 and 5 arms (usually 5), and then continually adds them as they grow. They get to about 12 arms by the 2nd or 3rd year. So, technically, "very young" individuals *always* have less than 12 arms.

Reviewer 3: Page 15 - "additional rays are added by budding in symmetrical pairs as the individual grows" – That's not true. Arms are sometimes added in pairs and sometimes are added individually. Arms can be added three ways as we show in Hodin et al. (2021), but the two most common ways are budding from the armpit, and an arm bifurcating in two. As the stars get older (3rd year or so), they seem to only add arms by budding in the armpits.

Reviewer 5: Page 15 - "While *in situ* gamete viability and fertilization effectiveness studies do not exist for *P. helianthoides* . . ." - This closing summary statement is unnervingly vague and short relative to the extensive array of sometimes conflicting information presented above from different species, lab vs field, different geographies that is hard to navigate, interpret, and determine best applications to *P. helianthoides*. Given over 1 page of information above, varying in both the agreement on any given topic and the relation to *P. helianthoides*, I recommend 1 paragraph summarizing how the SRT interprets these information and recommends applying to *P. helianthoides*, assessing *P. helianthoides* status. This may include further research to inform population assessments or to guide and inform any recovery efforts.

Reviewer 5: Page 16 - "larvae from Washington at 11°C and 14°C and observed first spontaneous settlement of larvae at seven weeks, in larvae held at 10-11°C." - You present earliest and peak associated with 10-11C but do not report out on range of days observed or anything from 14C – both of which are relevant. Present in comparisons in same units of time (e.g., days) throughout for consistency and ease of comparison

Reviewer 5: Page 16 - "The Supplemental Information . . ." - This is not a relevant discourse to assessing the population status of P. helianthoides and is a distraction to a report that is already challenging to navigate and interpret. Delete.

Reviewer 5: Page 17 - "Planktotrophic larvae of many species of asteroids can delay metamorphosis in the absence of suitable settlement cues (Metaxas 2013) . . ." - This is an important point – but settlement cues and drivers have not yet been discussed – so the statement seems out of context here. This and the next section discuss much of the same material. I recommend combining sections 2.3.1.4.2 and 2.3.1.4.3 providing a logical flow of the combined material and ending with a summary of how this affects *P. helianthoides* population status, stability, and extinction risk – including such statements as the closing sentence of this paragraph.

Reviewer 5: Page 18 - "In general, sea stars "are thought to have relatively low annual recruitment of larvae, punctuated by unusually strong settlement events in some years" (Sanford and Menge 2007, p. 9) . . ." - This is important – please follow up with how this influences population dynamics and risk of extinction, particularly with an eye towards changing ocean conditions and uncertainty.

Reviewer 4: Page 19 - It might help to clarify that *Pycnopodia* is not known to be one of the species that can reproduce asexually through whole body fission.

Reviewer 1: Page 22 - "No studies have been conducted to establish natural growth rates…" – I would rephrase

this to 'no published studies', because I know folks are working on this and I've seen early results.

Reviewer 3: Page 23 - "While no studies have been conducted specifically on the age of maturity of *Pycnopodia*" – We are currently raising larvae (and now settled juveniles) that are offspring of our captive sunflower stars, 3 years old, both males and female parents. We had them spontaneously spawn this year as well as spawn in response to 1-MA injection. We don't know for sure if our stars are becoming mature faster than in the wild, but Ken Collins' field data cohort analyses on growth rates in the wild are similar to ours in the lab. So, I would say we have emerging evidence for first reproduction at 3 years of age.

Reviewers 1, 4, and 5: Page 24 - The Diet and Foraging Behavior section seems really sparse compared to everything about larval development, growth, cloning, etc. Where is Jenn Burt's 2018 paper? Or Aaron Galloway's 2023 paper? Or Duggins 1983? Definitely more literature out there. Galloway et al. (2023) expands on the information on *Pycnopodia* feeding rates, particularly on urchins, and is worth incorporating. It reports on *Pycnopodia* consumption rates of purple urchin, preference between fed and empty purple urchin, and used modeling to investigate potential *Pycnopodia* top down predatory control of purple urchin to maintain and recover kelp forests.

Reviewer 4: Page 25 - For what it is worth, we haven't seen any evidence of a seasonal pattern in density for the fjord sites we study in the Central Coast of British Columbia. We survey May, August and October, and we have seen spawning *Pycnopodia* in May. However, if anything, we find the lowest density of *Pycnopodia* (which would likely be related to aggregation) in May. These data are unpublished that I will email to the review team.

Reviewer 5: Page 25 - "While individuals are generally considered solitary, documentation of seasonal, patchy distribution **combined with observed gregarious behavior in the lab (Hodin et al. 2021)** suggest . . ."

Reviewer 5: Page 26 – "Prior to the onset of a coast-wide sea star wasting syndrome (SSWS) pandemic . . ." - This is statement is very misleading, in detriment to the very studies needed (and used) for population assessments and recovery guidance including this one, and needs editing. It is true – there were very few focused studies or monitoring efforts exclusively for *P. helianthoides*. There is very little known about the species due to a paucity of focal studies on *P. helianthoides* as pointed out by Gravem et al. (2021) and Heady et al. (2022). BUT – it was long-term monitoring via SCUBA in kelp forests and trawl in deep water (although not explicitly focused on *P. helianthoides*) that provided the best spatial and temporal coverage of Western North America to inform Gravem et al. (2021) IUCN, and this assessment. That needs to be clearly stated here. It is long-term monitoring efforts that are crucial to informing population, community, and ecosystem changes. Long-term studies contribute disproportionately to our scientific understanding and policy, yet support for and therefore their number and impact is declining (Hughes et al. 2017, BioScience 67:3, 271-281). This is a place to highlight the importance of, value of, and need of increasing support for long-term studies to inform assessment and management needs as changing ecosystems makes the need of such information all the more crucial and urgent.

Reviewer 1: Page 26 - "this suggests that deep waters may serve as a biomass reservoir for the species". Hamilton et al. (2021) found that deep populations declined at a similar rate to shallow ones, Harvell et al. (2020) specifically notes that they found no evidence of a deep water refugia. You need to cite these studies as well if you are going to keep the suggestion that there is a deep water refugia since prior analyses have not found this.

Reviewer 5: Page 26 - In the first paragraph of the Social Behaviors section, before the last sentence, insert: "The seasonal cycling between complete absence of *P. helianthoides* and high densities of *P. helianthoides* coincident with hypothesized spawning season observed by Kjerskog-Agersborg (1918) further supports spawning aggregation behavior."

Reviewer 4: Page 26 - Tristan Blaine, who works for the Central Coast Indigenous Resource Alliance (CCIRA), has done drop-camera surveys for rockfish around the Central Coast in the last few years. I have reached out to see if he can share the results with the review team.

Reviewer 2: Page 27 - "While population connections between these sea stars and those in shallow water remain unknown, this suggests that deep waters may serve as a biomass reservoir for the species (J. Waddell, pers. comm.)." – This implies that the disease did not reach these depths, but there is no support that these deeper animals escaped the outbreak. Suggested rewording: "While population

connections between these sea stars and those in shallow water remain unknown, this suggests that deep waters are an important habitat for the species (J. Waddell, pers. comm.)."

Reviewer 1: Page 27 - "We are not aware of any observations of P. helianthoides recruits or adults in California or Mexico since 2017." - Have been two sightings in northern California by fishermen: here and here.

Reviewer 5: Page 27 - Last paragraph before Section 3.2. You need some statement or even a sub-header (disease related declines) alerting the reader that you are now talking about SSWD declines – everything up to this point has described historical abundance and density and how that varies by region – this is an unclear transition creating ambiguity if you are talking about historical decline by latitude or post SSWD relative declines. Also, Switching the 1st 2 sentences provides this clarity and a topic sentence. The use of sub-headers – as suggested also helps. Also, change the last sentence to read something like "There were no observations of P. helianthoides recorded from OR, CA, of MX from at least 2017-20 (Hamilton et al. 2021; MARINe). However, there have been a handful of isolated reports of the species in CA, OR, and WA per recent news articles."

Reviewer 5: Page 27 - Last paragraph, and elsewhere it is used: "south of Washington outer coast" implies that WA outer coast is not part of that group – and is incorrect. I recommend clearly stating "Washington outer coast south."

Reviewer 3: Page 28 - "The bipinnaria and brachiolaria are sequential developmental stages, the bipinnaria always developing first (Chia and Walker 1991; McEdward et al. 2002)." – This is only true for sea star taxa with feeding larvae. It is common for non-feeding asteroid taxa to develop a non-feeding brachiolaria with no intervening "bipinnaria"-like stage. See McEdward & Miner 2001, Can J Zool, 79: 1125.

Reviewer 5: Page 28 - "As relatively sessile, benthic organisms in the settled juvenile through adult life stages . . ." - This is misleading and needs revision. Section 2 as well as 3 paragraphs below talk about how mobile the adults are; capable of moving relatively fast and far. They are not considered "relatively sessile"

Reviewer 1: Page 29 - "The Sunflower sea star is, generally speaking, capable of tolerating handling during regular fishing operations, such as removal…" – This sentences feels like it is contradictory to the sentence directly above it.

Reviewer 3: Page 29 - "Strathmann (1978) emphasized that the duration of pelagic larval life is important in recruitment dynamics and, ultimately, to the distribution of a species." – A note about cloning might be relevant here since it is common in *Pycnopodia*, and larval cloning in echinoderms was not recognized when the 20th century studies cited here were done.

Reviewer 5: Page 30 - "Whether stress associated with such handling might increase short- or long-term susceptibility to SSWS and/or compromise reproductive potential and resulting population stability or risk of extinction is unknown and will require systematic evaluation to clarify proximate and ultimate causes." - Concerns like these as well as information gaps and important next steps to inform population assessments, management decisions, and conservation actions with much discussion of the risks, potential impacts of, and important information gaps of SSWS are discussed at length in Heady et al. (2022).

Reviewer 1: Page 30 - "There are two prior status assessment of *P. helianthoides*…" – I would also include Harvell et al. (2020) as a third study.

Reviewer 4: Page 31 – Hakai has recent dive data updated for 2021 and 2022 we can share. One note is that for the IUCN data share we removed all juvenile *Pycnopodia* (radius <10cm) from our data in order to match other monitoring efforts.

Reviewer 5: Page 31 - "However, many of the time series in the IUCN data set contain only presence/absence data." - To the unfamiliar reader "many" implies a great proportion of. Many could work by definition (i.e. more than 5) but considering the relative proportion I think "some" is more appropriate – or state the number and then out of, or state the proportion.

Reviewer 1: Page 32 - Dividing your model into a pre-2013 and a post-2013 periods is problematic because of much of the northern half of the range, the pandemic did not break out until 2014, 2015 or 2016. Thus, you have multiple years of 'pre-pandemic' data being counted as estimated the 'post-2013' period, which is likely to inflate post-pandemic numbers, particularly in the northern part of the range.

Reviewer 1: Page 32 - Excluding 0 values in the MARSS assessment. I read the justification as to why to do this, but I still think it is problematic. I understand the problems around non-detection, but it seems weird to apply

concerns around the accuracy of a density measurements ONLY when the animal is not seen. Why do you trust a density of 2 Pycnos/km2 but not 0 Pycnos/km2? Just because the divers saw 2 Pycnos doesn't mean they were 2 Pycnos there. Additionally, no matter the statistical justifications, it still feels sketchy to throw out 0 density values when assessing whether a species is at risk of extinction. I feel better when seeing 0 values replaced with very small numbers rather than NAs.

Reviewer 4: Page 32 - I appreciate the use of both the models with NA and the models with min. I think this is a good way to explore the patterns and evaluate the assumptions of each way of fitting the model.

Reviewer 4: Page 32 - I'm curious whether there is any issue with using a universal cut-off of 2013 for the whole coast, since some northern areas didn't report signs of wasting until 2015 (Hamilton et. al. 2021). It seems the models are still showing signs of decline (reading below), but is having the 'after' time period start earlier than the recorded exposure from the field dampening the level of decline?

Reviewer 5: Page 33 - Last paragraph before Section 3.4.2.2. Were 0s never considered? For any region? Given that the "full model" establishing 3 regions with independent growth rates it seems this should allow for independent handling of 0s and full testing of the best handling of 0s among regions. It seems AICc model selection, some other model selection criteria, and/or a detailed understanding of the sampling methodologies used, implications of data handling as presented here combined with likelihood of sampling efforts being close to true densities and understanding of likely processes and how each of these likely vary by region should be considered. For example perhaps 0s for West Coast from coastal Washington to Baja California, NA for AK, and min for British Columbia and Salish Sea.

Reviewer 5: Page 33 - Section 3.4.2.2 MARSS Results. This section is hard to follow, interpret, and evaluate the degree to which the SRT met their charge because the models considered, how they were evaluated, and results are not clearly presented, organized, and articulated. Please provide clear, consistent structure of presenting model types and names throughout to help maintain continuity and ease of interpretation of results. Consistent model nomenclature is needed. Then clear articulation of what is being presented is needed. I recommend subsection headers "Rangewide MARRS Results: Full model considering all regions together" Discuss this analysis, results and implications. Then segway that, because in this full model evaluation – region specific growth rates was the only model that met selection thresholds we can then investigate each of those regions independently – as described below. Regional MARSS results Use consistent nomenclature clearly laid out in section 3.4.2.1 and which model was best for each reason, why, and implications to population assessment, viability, and extinction risk. In Regional model evaluation it seems to me that using 0s should be default and "alternatives" should be replacing 0s w/ NAs or small non-zero numbers. This does not seem to be the case – which is confusing. Regardless – what and how things are structured and considered is confusing and unclear.

Reviewer 5: Page 33 - "best-fit w/NA" - While this is true – please use a different model name – to help understand 1. What the model structure is, and 2. How it relates to other model structures; i.e. use the actual model name (prior to AICc selection and you can add "best-fit" to that name or in a parenthetical statement). Then – be consistent on nomenclature throughout. Below when figures are presented the term "full model" is used – which has not been presented here – so the reader cannot understand what you are presenting or how it relates to 1. models considered, 2. AICc results, and 3. how model structure affects probability of populations dropping below 1% of 2013. This is arguably the most important part of this assessment and the reader cannot easily track or interpret what is being presented. This section needs clear, organized, and easy to follow presentation of the material. AICc – list all models considered ranked by their AICc score / difference in AICc relative to best fit model – but importantly, list them with appropriate model names that the reader can determine what was modeled – either short names that allude to what was considered and then have a paragraph or table describing factors for each name – or – have each name explicitly list modeled factors in each model name.

Reviewer 5: Page 34 - "Estimates for this period in Alaska in the best-fit model are likely being driven by low data availability, as well as low process and observation variance coming in from data sharing from the rest of the data." - It is worth conveying here that while this region has the most scant data pre and post it is simultaneously the largest region in consideration with high variability in environmental conditions – this is important for a few reasons as discussed in Heady et al. (2022): 1. This large area will likely be important to

population resilience and recovery; 2. That habitat variability could give rise to population structure. That habitat variability will be important to facilitate studies to inform the life history, demographics, recovery trajectories, and management actions varying throughout that may then be transferred to other geographies that may share unique attributes to sub-geographies within this large, heterogeneous region.

Reviewer 5: Page 34 – Table 1 legend. - Each of the three best fit regions? This seems in error – or a misuse of the term.

Reviewer 3: Page 34 - "The IUCN *P. helianthoides* assessment lists several observations of juvenile growth rates from anecdotal observations and laboratory studies as being between 3 and 8 cm/y, and 2 cm/y for mid-sized individuals (Gravem et al. 2021)." – These are very low. Our mean size estimates are 3-5 cm in the first year, 10-15 cm in the second year, and 20-30 cm in the 3rd year. I believe Ken Collins' cohort data yield similar estimates.

Reviewer 3: Page 34 - "Some sea stars can persist for long periods with little or no food (Nauen 1978; Deaker et al. 2020; Byrne et al. 2021)," – And when sunflower stars don't eat they can shrink.

Reviewer 5: Page 35 - "For completeness, similar models were run for the other two regions and for all regions with the zeros replaced by NAs or by minimum values (Table 1, Figures 4b, 5, 6)." - I don't think "completeness" is the right term? For equal representation? Comparability, consistent modeling among regions? This point is lost amongst paragraphs and figures and its impact is not realized by the reader. – playing to the organization suggested at the beginning of this section – ensure that this point is clearly and boldly stated and easily gained. This is important. **Is this truly the best approach?! Treating all regions the same – replacing zeros with NAs or minimums for ALL regions simply because it is a data driven necessity for AK?** If so – this needs some further elaboration of why this is or is not a good idea. It was discussed earlier how replacing 0s w/ NAs or small non-zero numbers dramatically (and not necessarily appropriately) changes model results. It seems that perhaps, true best fit for the other two regions makes sense and handling AK differently largely, because of reasons described earlier – it is the largest region, yet has the scantest spatial and temporal range and resolution of data, out of sheer necessity for AK would make sense. Particularly since when evaluating models with the three regions considered together the best fit allowed for independent consideration of growth among regions.

Reviewer 5: Page 35 - "Results for British Columbia and the Salish Sea were similar to those for the best-fit models" - Clarify what is meant by this statement. What is being compared here? Is this comparing to the "full" = "best-fit" model results?

Reviewer 5: Page 35 - "The models with zeros replaced by NAs estimated a 72-74% decline since 2013, while the alternate models estimated over a 92% decline" - What are "alternate models?" if only 1 alternative "min" is presented, at which point just say "min" for clarity.

Reviewer 5: Page 36 - "West Coast region only models were similar to their best-fit versions (Table 1)" - What is meant here? WC region only compared to best fit from model comparisons of all data with different regions? Please clarify.

Reviewer 5: Page 36 - last paragraph - This does not seem "interesting" but rather "as expected" – as you stated that this mathematically happens in paragraphs above. I suggest restating this paragraph in more of an, "as expected" (rather than "interestingly") framework: As expected…..because….therefore we… And explain why 0s were not considered at all – or better yet – present model results with 0s for each independent region and 3 regions together.

Reviewer 1: Page 36 - Using the logged values of density seems problematic to me here because in Figure 5, it visually underestimates the level of decline in this animals. Will non-technical experts be using this report? If so, logged values displayed like this are likely to be misinterpreted and insinuate a very minimal decline (please see: https://www.nature.com/articles/s41559-018-0610-7). I have the same problem with Fig 6 as well.

Reviewer 5: Page 37 - "see Appendix A. Population viability analysis of *Pycnopodia helianthoides*" - Provide a section within Appendix A.

Reviewer 5: Page 37 - "Under these conditions, the risk of declining below 1% of the 2013 population size is essentially 100% and the only question is how quickly that threshold is reached." - Which may be true and/or likely, is important to explore, and the timing would be helpful to inform viability

parameters and extinction risk.

Reviewer 5: Page 37 - "Allee effects are negligible" - How likely is this? Did you model likelihood of this and/or sensitivity of MARSS results to Allee values? This seems likely variable by region. E.g. Allee likely in WA, OR, CA, MX. Or if not local Allee, then isolated occurrences limiting regional population recovery potential or timeline

Reviewer 5: Page 37 - "based on both the pre-2013 and post-2013 estimates of population growth for each region." - It does not seem appropriate to use only growth rates from pre 2013 or post 2013. "Present to future" growth rates clearly are: 1. Not pre 2013 levels and will take quite some time to get there, perhaps longer than 30 years; and 2. Perhaps not post 2013 rates which include peak and pandemic levels. Hamilton et al. (2021) defined multiple time frames – each with distinct characteristics. This assessment has further benefit of years since data considered by Hamilton et al. It seems more appropriate to consider Pre-epidemic, during-epidemic, and post-epidemic. Again this study can add several years to post epidemic to those used by Hamilton et al. I also suggest using different years to define those phases among the three regions you consider in this assessment as differences were clearly observed and quantified by Hamilton et al. 2021. Then comparing pre- to during-pandemic growth rates informs declines – and comparing pre- to post-pandemic growth rates informs recovery potential and extinction probability. By doing so – post-epidemic growth rates may better represent actual recovery growth rates and probability of extinction.

Reviewer 5: Page 38 - "Extinction risk for *P. helianthoides* in the Alaska region from four models. The Full models the best-fit model with zeros replaced by NAs or minimum values." - Stated from four models – but 5 shown and stated that two are not shown – that doesn't make sense. Please clarify what is what here; commas and using names that match the legend which match descriptions in report body will help.

Reviewer 5: Page 38 - "The question of whether SSWS ceases to have strong impacts on *P. helianthoides* population growth rates is central to evaluating the realized risk of extinction. All of the models here suggest there is a high risk that *P. helianthoides* populations will drop below 1% of their 2013 sizes if SSWS does not abate and population growth continues to be negative (Figures 7-9); this result is hardly surprising given the projected continued population decline from assuming post-2013 rates of change." - SSWS dynamics into the future are indeed of central issue to the viability of populations and extinction risk both by region and range-wide. Heady et al. (2022; Page 25) highlight the importance of incorporating disease dynamics into population assessments: "The multi-host nature of SSWD highlights the potential importance of identifying species-level variance in susceptibility and transmission potential to identify reservoir or super-spreading hosts (McCallum 2012). Importantly, rather than aiming for the potentially unachievable goal of disease-elimination, integrating disease modeling into population viability analyses can enable the establishment of threshold levels of disease prevalence, multi-host dynamics, and other key parameters that give rise to population stability or recovery (Cleaveland et al. 2007)." This highlights the importance of objectively considering how SSWS is and will affect population viability factors and extinction risk – i.e. with little to no recovery observed (except in the central region) that these grim projections of certainty of extinction, leaving only "when" may be realistic and inform extinction risk and resulting urgent recovery actions.

Reviewer 2: Pages 38+39 - Figs. 7, 8, 9: Suggest changing Y axis to 'Extinction Probability' for clarity.

Reviewer 1: Page 39 - "Clearly understanding whether SSWS will persist and continue to kill off recruitment pulses is key to understanding future population dynamics" – But we know that SSWS is still very much in the system and continuing to impact sea stars, we see it all the time in *Pisaster* and other species. If it's still present in *Pisaster*, why wouldn't it still be present in Pycnos? Especially considering that few Pycnos live in populated places and that the disease kills them in a matter of 24-48 hours, even if hordes of Pycnos were continuing to die of the disease, humans probably wouldn't notice it. I think it's inappropriate to suggest that there isn't any evidence as to whether SSWS is persisting in the population, when we know it is for many other star species.

Reviewer 3: Page 39 - "Further, while anecdotal observations indicate *P. helianthoides* recruitment continues in the U.S. portion of the Salish Sea, British Columbia, and Alaska, few of these juveniles appear to survive to adulthood (A. Gehman, personal communication)." – I question the generality of this observation. The data from Saratoga Passage suggests that juveniles exist in different habitats than adults, with juveniles sheltering

more and adults more in the open. A wide size range of stars has been recorded in WA in the past several years (Michael Kyte and Ken Collins' data) so we assume that successful recruitment is happening.

Reviewer 5: Page 39 - "Clearly understanding whether SSWS will persist and continue to kill off recruitment pulses is key to understanding future population dynamics." - Understanding SSWS and how it varies by life stage and geography are critical to understanding population viability and extinction risk as discussed along with key questions, key next steps and summaries of our current understanding and current research in Heady et al. (2022).

Reviewer 2: Page 40 - Table 2. It is unclear from the table whether data from Hamilton/Gravem regions were lumped into the MARSS regions or if some regions were not included in MARSS. Suggest outlining table cells to show how regions were lumped.

Reviewer 5: Page 40 - Table 2. I suggest you re-order the studies from left to right as: Gravem, Hamilton, MARSS. That is the order of occurrence and alphabetical.

Reviewer 3: Page 41 - "Whether stress associated with such handling might increase short- or long-term susceptibility to SSWS and/or compromise reproductive potential is unknown" – The open wound caused by damage or autonomy would be my biggest concern here.

Reviewer 1: Page 42 - "…we estimated was sensitive to how missing data were treated…". It's not missing data. It's zeroes that you took out.

Reviewer 1: Page 42 - "We were also able to statistically estimate extinction risks assuming either that SSWS impacts are ongoing or that pre-2013 population growth rates will manifest in the near future… and show low levels of risk under certain conditions." But you also found high levels of risk of extinction given other circumstances that you don't highlight? This statement feels like it is underplaying the risks to me. Plus, it's been nearly a decade since the pandemic started and we aren't seeing any evidence that populations are rebuilding in the vast majority of the range (Hamilton et al 2021), so why would we assume that pre-2013 population growth rates will manifest in the near future? That doesn't seem like a likely scenario given the evidence.

Reviewer 5: Page 42 - "We were also able to statistically estimate extinction risks assuming either that SSWS impacts are ongoing or that pre-2013 population growth rates will manifest in the near future (3.4 Species Status Assessment)." - This seems out of place here – there was an entire section dedicated to it above – and this seems better referenced in Section 4.3 Reproductivity/Growth Rate. Delete from here and consider referencing (with amendment from comment below) to 4.3 if appropriate. This is misleading. You used two very different growth rates – and not surprisingly, risk was directly related to those growth rates. Stating that you modeled either SSWS impacts ongoing or using pre-2013 growth followed by, "show low levels of risk under certain conditions" infers it is not related to this dichotomy and is very misleading. Please replace "show low levels of risk under certain conditions" with a statement describing that risk is directly related to how SSWS impacts are assumed and whether pre-epidemic healthy population growth or post epidemic population growth rates were used.

Reviewer 4: Page 43 - We don't have any information or data on whether there were *Pycnopodia* in the fjords before 2018. I don't know of any monitoring that was done previous to the CCIRA and Hakai data. So, we don't know if the populations we are finding in the fjords are same or more than historical abundance, or if what we observe now is a decrease from pre-wasting time periods.

Reviewer 5: Page 43 - "Therefore, a substantial gap remains in our understanding of the spatial distribution of early life stages." - I recommend a sentence or two articulating here the importance of this gap to understanding the population viability and extinction risk. For example as Heady et al. (2022) discussed, as it relates to distribution/connectivity. Key questions that will help inform future population assessments and management decisions include – do early life stages associate disproportionately with different habitat types? Therefore are these life stages randomly or non-randomly distributed at a landscape scale? How does this in turn affect population connectivity and other viability factors?

Reviewer 5: Page 44 - Section 4.3 Productivity/Growth Rate - This is a nice overview of how productivity/growth rate are important to understanding population viability among taxa – but does not discuss what is known or not known about *P. helianthoides* and how this affects *P. helianthoides* extinction risk. Please elaborate the final paragraph to do just that. This is where filling identified gaps in knowledge, as discussed in (Heady et al.

2022), and mentioned here will help future assessments. Then describe, given those gaps what is known – e.g. modeling from Section 3.4 and how results can explicitly be considered with context from life history of other similar species referring to summaries from Section 2, to inform what can be inferred about *P. helianthoides* here.

Reviewer 5: Page 45 - In the case of *P. helianthoides*, very little is known about morphological, physiological, or genetic diversity on any spatial scale. The species occupies a broad array of habitats in which physiochemical traits vary over wide ranges, suggesting resilience and adaptability that may stem from regionally distinct diversity. Considerable additional research is needed, however, to confirm this broad assumption." - Refer to Heady et al. (2022), which discusses the importance of genetic diversity to demographics, population and threat assessment, disease resistance, recovery, and summarizes and refers to current and ongoing genetics and genomics research throughout *P. helianthoides* range including genomics and genetic structure throughout the species range. Mike Dawson and Lauren Schiebelhut, co-authors of that report as well as researchers cited within that report may know of advances and updates since the publication of that report.

Reviewer 1: Page 46 - "Other team members stated that some signs of recovery (i.e. recent recruitment events) indicate that the southern range is still occupied…" – what recruitment events? I've not heard of any real recruitment event in the southern third of the range. Maybe an occasional stray juvenile here and there, but that's over hundreds of miles and thousands of eyes in the water.

Reviewer 2: Page 47 - "The Commercial trade to support a curio market is thought to affect primarily small individuals, and at low levels of impact," – As is pointed out in Gravem et al. (2021), it's very possible that curios marketed as 'sunflower stars' are actually different species with harder skeletons that are better for drying.

Reviewer 4: Page 47 - For clarity, we have a few timelines of disease we measure in the lab. Since we control exposure, we can estimate the time from exposure to mortality, which is generally 5-14 days. We also estimate the time from first visual signs of disease to mortality, because in the field we can't know when exposure happened. We have found that for *Pycnopodia* the time from first visual signs of disease to mortality can be 2-4 days.

Reviewer 5: Page 48 - "Beginning in 2013, SSWS caused ~72-100% declines in locally monitored populations of P. helianthoides across its range. The global *P. helianthoides* population declined by an estimated 90.6% due to SSWS (Gravem et al. 2021). Some local populations were functionally extirpated within a matter of weeks." - Reference where the 72-100% range comes from – is that from MARRS analysis summarized in Table 2? Curious why the global decline statistic was pulled from Gravem et al instead of Hamilton et al? Suggest using Hamilton et al which improved upon Gravem et al for an improved estimate 94.3% due to SSWS (Hamilton et al. 2021). Consistency. "local extirpation" was refuted in previous paragraphs, pg 42. Stating facts is still compelling – something like, some local populations did not have a single *P. helianthoides* observation within a matter of weeks….

Reviewer 1: Page 48 - " As modeled above in section 3.4 Species Status Assessment, if recent SSWS-associated population declines continue extinction is all but certain throughout the range. If population growth rates are able to return to pre-pandemic levels in coming years, however, likelihood of population persistence is moderate in the Alaska Region and the British Columbia and Salish Sea Region, but lower in the West Coast Region." Considering that 1) we know SSWS is still impacting sea star populations and 2) that after 5-10 years in all regions, no region is showing recovery in occurrence or abundance, I'm not sure it's realistic to assume that growth rates will return to pre-pandemic levels any time soon. Thus I think the risk of extinction, particularly in at least parts of the range, is underestimated.

Reviewer 4: Page 48 - 'Symptoms' need to be self-reported, so sea stars can only show 'signs of disease', not 'symptoms'.

Reviewer 1: Page 51 - "There is evidence of such a pulse in the Salish Sea, Coastal B.C., and elsewhere, indicating that reproductive potential still." But the folks who have been tracking these recruitment pulses (Sarah Gravem, Alyssa Gehman) aren't seeing these juveniles get to adulthood from what I've heard. Is there other evidence that recruitment pulses are resulting population adult population growth rates? If so, I'm not seeing it in this assessment.

Reviewer 5: Page 51 - "However, if populations are able to return to pre-pandemic population growth rates long-

term persistence is moderate to high." - Clarify here. What does persistence is moderate to high mean? Probability or likelihood of persistence is moderate to high?

Reviewer 5: Page 52 - "Whether the causative agent of SSWS exists in an environmental or biological reserve, however, is also unknown. If it does, this recruitment pulse could be short lived and individuals may not survive to reach reproductive age." - Heady et al. (2022) discuss the potential complexity of a multi-host disease with susceptibility, expression of the disease, and mortality varying among the multiple host species; the challenges this creates to determining *P. helianthoides* population trajectories; and how multi-host dynamics should be included in population modeling and extinction risk assessment.

Reviewer 5: Page 58 - "Abundance prior to the SSWS pandemic was substantially greater in northern portions of the range from Alaska to the Salish Sea, and declines in these areas were less pronounced." - I find this last clause misleading. Declines had more uncertainty, more variability. Declines were less than to the south – but still by population assessment and conservation standards were "pronounced," some would even consider extreme. For example, the MARRS analysis of this assessment determined 81-100% declines in Alaska (quoted text below). This seems pretty "pronounced" if not extreme. This statement should be reworded to more accurately reflect declines throughout the range. I recommend some statement to the effect of: declines in abundance in the northern portions of the range, while still significant (72-100), were lower than to the south with less chances of stochastic events to lead to regional extirpation. "The three major analyses *of P. helianthoides* population status, Hamilton et al. (2021), Gravem et al. (2021), and the MARSS analysis here, all generally agree on substantial declines in abundance across the species' range (Table A4.10). In part, one should expect this result given that all three analyses use similar data sets. Declines were strongest in the southern portion of the species range (here West Coast region). However, the results are somewhat more variable for Alaska ranging from 40-100% for Hamilton et al. (2021) and 61%-95% for Gravem et al. (2021) across various Alaskan regions. MARSS estimates of decline in Alaska since 2013 ranged from 81-100% depending on whether zeros were replaced with NAs or minimum values, with w/NA models estimating smaller declines. The combined British Columbia and Salish Sea region match most closely with the core region from the significant portion of its range analysis (section 6.5.2). Here MARSS estimated an average 84% decline across models, ranging from 72-96%. Both Hamilton et al. (2021) and Gravem et al. (2021) estimated declines around 88% for British Columbia, although their estimates for the Salish Sea suggested higher loss. While there is some variation among modeling approaches and studies, the extinction risk analysis here (the probability that population size drops below 1% of the 2013 population size over the next 30 years) is high if population growth rate continues to be negative, but much lower if *P. helianthoides* population growth returns to pre-2013 levels."

Reviewer 5: Page 58 - "The current range-wide (i.e., global) population estimate for *P. helianthoides* is nearly 600 million individuals, based on a compilation of the best available science and information (Gravem et al. 2021)." - If truly based on a "compilation" more citations are needed here or at least a statement of 'including references within'.

Reviewer 5: Page 58 – "How segments of the population in these poorly sampled areas contribute to and are connected with the overall health and stability of the species remains largely unknown. *Pycnopodia helianthoides* in these areas are less susceptible to impacts from nearshore stressors and could serve as source populations to support population rebound, but evidence to support this role is lacking." - Heady et al. (2022) discuss the importance of strategically filling data gaps in geographies to inform local, regional, and rangewide population trends and extinction risk. Noting the importance of environmental variability and potential environmentally driven differences in survival, growth, reproduction, and population growth, Heady et al. (2022) discuss the importance of modeling population trends in a nested model with local forcing nested within regions and regional forcings nested within geography irrespective of jurisdictional boundaries.

Reviewer 3: Page 59 - "*Pycnopodia helianthoides* has very few predators" – As adults.

Reviewer 5: Page 59 - "The standing crop of individuals capable of generating new recruits has been decreased, possibly to levels where productivity will be compromised on a regional or global basis." - Very much agreed. Please also add a time reference to this statement; state how productivity will likely be affected within foreseeable future, e.g. 30 years.

Reviewer 3: Page 59 - "Predatory risk is likely highest during the planktonic larval phase, but this stressor is

tempered by the fact that *Pycnopodia helianthoides* is a broadcast spawner, creating millions of offspring per female during a spawning cycle." – And for juveniles?!

Reviewer 5: Page 60 – "Until more is known about the underlying biology of the species, this parameter, and its effects on long-term viability, will remain poorly defined." - This statement is tautilogical. Please instead discuss suggested information needed, next steps, sequencing – much of which is discussed in Heady et al. (2022) -and/or- Discuss how this lack of understanding and resulting uncertainty on viability effects this and future assessments of extinction risk.

Reviewer 2: Page 60 - *"P. helianthoides* still occupies the whole of its historic range from Alaska to northern Mexico, though in nearshore areas from the outer coast of Washington to Mexico the species is now rare where it was once common." And "Curtailment of the range of *P. helianthoides* has not yet been demonstrated, despite the fact that the species has become rare from the Washington coast south to California since the SSWS pandemic, in areas where it once was common." – To my knowledge, less than 20 animals have been seen since ~2016 in OR, CA, and MX combined. I wouldn't say that this qualifies as 'occupying its historic range', and 'rare' doesn't really capture this extremely low statistic. Perhaps the definitions of ESA are different, but I don't think this sentence captures the reality of how uncommon they are now in the southern 1/3 of their range.

Reviewer 5: Page 60 - "Despite substantial population declines from 2013-17, *P. helianthoides* still occupies the whole of its historic range from Alaska to northern Mexico, though in nearshore areas from the outer coast of Washington to Mexico the species is now rare where it was once common." - Is this true? With certainty? It is stated in this report – and in others cited in this report that *P. helianthoides* has not been observed in central California, southern California mainland, or Mexico in over 5 years – which constitutes a significant portion of the range that *P. helianthoides* has NOT been observed to occupy. It seems misleading to state with certainty that "*P. helianthoides* still occupies the whole of its historic range."

Reviewer 5: Page 60 - "but reports of newly settled juveniles and occasional adults in these regions" - Please ensure these statements are accurate. Newly settled juveniles in California?

Reviewer 2: Page 61 - "Undersampled, deep-water habitats represent the majority of suitable habitat for *P. helianthoides* by area" – It is worth noting that the densities in these habitats were historically much lower than for shallower habitats, see supplemental table in Gravem et al. (2021).

Reviewer 5: Pages 64+65 - Section 6.4.3.1 Disease - This section is good. Consider reviewing and referencing Heady et al. (2022) which also summarize the importance of understanding SSWS, disease dynamics, variability among life history and geography, etc. affect population viability and also discuss recent findings and experiments underway to improve our understanding of how disease may affect population viability, as well as the importance of research and collaborative communication among researchers and aquarists regarding discerning stress, injury, other disease, and SSWS for informing management decisions.

Reviewer 2: Page 69 - I'm a bit perplexed that the West Coast section was not deemed a 'significant portion of their range' since it's a huge expanse of coastline (2700 km) spanning the outer coast of 3 large states. An extirpation over nearly the entire contiguous US coastline seems 'significant'. But I acknowledge that it didn't necessarily encompass the bulk of the population.

Reviewer 5: Page 74 - Section 7 Summary and conclusions – I'm sure there is a standardized approach followed for all species status assessments – but this Section seems too brief and high level. As is, the summary leaves the reader wanting more it summarizes the process and conclusion is the determination. This summary and conclusions section would help the audience if it also provides a summary of the details driving the assessment specific to this species – disease, climate change, listing the "other factors;" how those do or do not vary geographically; how those affect the demographic risks to *P. helianthoides* range wide: abundance, spatial distribution, productivity, and diversity. Then provide a summary of findings of these VP descriptors which is the crux of the assessment but absent from this summary. It could still be concise and brief – perhaps around 1 page. However, a one paragraph summary, half of which simply repeats the ESA Section 4(a) approach for assessments doesn't seem appropriate for a 100 page document of such extensive, in-depth work

and importance to informing and guiding the recovery of the species as well as marine science and conservation.

Reviewer 4: Page A7 - In Figure A3.7 there appears to be one point well outside the geographical location of the other points (e.g., outside the Salish Sea) that is worth double checking.

Reviewer 5: Page A32 - "There was one model with $\Delta AICc \leq 2.0$ (Table A4.2), and this was chosen as the best-fit model (below this is "best-fit w/NA"). It had an AICc value 8.39 points lower than the second best model and 424 estimated parameters." - It is unclear how and when 0s vs NAs vs min are evaluated. It is presented that overall model evaluating regions, growth rates, and process and observation variance is evaluated 1st – which determined that 3 regions with independent growth rates was "best fit." and then using "best fit" the handling of 0s was assessed – yet "best-fit w/NA" is the "best fit" straight away. Some clarity is needed here. AICc – list all models considered ranked by their AICc score/difference in AICc relative to best fit model – but importantly, list them with appropriate model names that the reader can determine what was modeled – either short names that allude to what was considered and then have a paragraph or table describing factors for each name – or – have each name explicitly list modeled factors in each model name.

Reviewer 5: Page A39 - "For the West Coast only (Figure A4.4), MARSS provided relatively consistent estimates of post-2013 rates of population decline (Table A4.4) at around -23.5% annually. However, the WC-only model suggested a higher rate of decline at -37.2%." - Both of these sentences describe "West Coast-only" yet the second sentence states, yet this one is higher… Please provide clear nomenclature and clear comparisons of models.

Reviewer 4: Page A39 - 'West Coast only' is a bit confusing as a geographic name. I needed to keep looking it up to confirm which area was included in the grouping.

Reviewer 5: Page A44 - "An alternative scenario is that the effects of SSWS abate, Allee effects are negligible, and *P. helianthoides* populations begin to grow at annual rates seen prior to 2013." - How likely is this? Did you model likelihood of this and/or sensitivity of MARSS results to Allee values? This seems likely variable by region. E.g. Allee likely in WA, OR, CA, MX. Or if not local Allee, then isolated occurrences limiting regional population recovery potential or timeline

Reviewer 2: Page A46 - The least pessimistic post-**2013** u model, the full model with NAs,