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Reply on RC2

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Author comment on "Carbon dioxide removal via macroalgae open-ocean mariculture and sinking: an Earth system modeling study" by Jiajun Wu et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-104-AC5, 2022

Authors present results of assessing the efficiency and impacts of macroalgal ocean sequestration (MOS) within an Earth System Model. The model assumes that MOS infrastructures appear throughout deep ocean sites where long-term sequestration is possible and macroalgal biomass will grow where there are adequate nutrients to sustain an annual crop. They then look at several MOS scenarios to address the long-term impacts on ocean ecosystems and the carbon cycle.

The paper is interesting and potentially important as many are looking to seaweeds as a CO2 removal (CDR) strategy. However, there are several issues that make it not quite ready for publication at this time in my opinion. I have issues with the overall premise of how the authors envision MOS, there are missing and odd elements of the farmed macroalgae model and the presentation is not adequate and the manuscript needs both a reorganization and some editing to make it more easily readable. I will detail these overall concerns and follow with specific comments by line number.

The basic MOS scenario created by the authors have created is in my opinion an unrealistic possibility which degrades the relevance of the model results presented. As I understand it, they are trying to assess is whether MOS (farmed everywhere it can) can by itself keep global temperatures within the Paris accord targets while still allowing moderate emission scenario (following RCP4.5). This seems to me to be an odd thing to test as I cannot imagine that actually happening. The recent NASEM report on ocean CDR suggests that portfolio of several CDR approaches is more likely solution to the negative emissions quandary. Further the presumption that MOS infrastructure can be deployed everywhere that macroalgae can grow requires a number of logistical hurdles to be overcome. Together, I am having a hard time understanding the actual relevance of these scenarios understanding the efficacy and impacts of MOS as a CDR strategy. Within that basic frame, the individual cases for stopping MOS, no decay of biomass and adding artificial upwelling cases, all make sense. But the overall premise does not, at least in my opinion.

Response: Thank you for your thoughts on the manuscript, we are sorry that the intentions of our study were not more clearly communicated in the manuscript and we will revise it so that there is no confusion. The overall goal of the study is not to see if MOS alone can limit warming enough to reach the Paris Agreement goals, this is as you say only potentially possible with a portfolio of CDR approaches and as much reduction in emissions as possible (emissions reductions are the primary means of meeting the Paris Agreement goals, CDR is no substitute). The study is also not supposed to suggest that macroalgae farming could be done everywhere. Instead the study is designed to explore the maximum natural physical/biogeochemical potential of open ocean macroalgae farming with sinking as a means of long-term CDR. This approach is valuable in a number ways. First, we can put an upper limit on what is physically / biogeochemically possible. Second, some side effects may only become evident at the very large scales found in our study. Understanding at this level is useful for deciding what CDR approaches to prioritize with further research. Finally, by initially seeding the macroalgae everywhere in a dynamic model, with simulated climate change, we can help determine locations where open ocean macroalgae farming is viable. This goes beyond previous studies (e.g. Lehahn et al., 2016; Froehlich et al., 2019), which used static models of environmental conditions to determine where macroalgae might initially be farmed in the open ocean. To make the aims of the study clearer, we will revise the text as follows.

Abstract: (1st sentence) "In this study we investigate the maximum physical / biogeochemical potential of macroalgae open-ocean…".

Introduction: (5th paragraph) ".... The aim of this study is to (1) investigate the maximum physical / biogeochemical CDR potential of MOS, (2) the side effects of such large scale deployment, and (3) to understand where offshore macroalgae farming would be viable if done at a large scale. This information is needed to help prioritize further research into CDR, to understand if there are potential MOS side effects that become evident only at large scale, and to provide information on the viability of large-scale offshore macroalgae farming in different regions over time by accounting for the implications of nutrient utilization and climate change."

I have some quibbles with the modeling that I think requires some discussion. Carlos Duarte and his colleagues have focused on the importance of recalcitrant dissolved organic carbon (DOC) that is released as the farmed macrophytes grow to long-term carbon sequestration. This mechanism is not included in the model nor is it discussed why it is not included. Given the long lifetimes (1000's years) of the recalcitrant DOC pool in the ocean, even a small fraction of recalcitrant DOC released during growth could be important.

Response: The production of recalcitrant DOC is something that we have thought about, however, refractory DOC dynamics are difficult to include in a global model and beyond the scope of this study. This is because as far as we are aware no global models have successfully simulated refractory DOC cycling in a dynamic manner that resolves all source and sink terms. Most attempts have used data constrained models (offline tracer-modeling techniques to constrain DOC)(Letscher et al., 2015) or are still at the theoretical stage of development (Mentges et al., 2019; Zakem et al., 2021) and not coupled to a 3-D physical ocean model, let alone an Earth system model. Global 3-D models that do include DOM cycling, usually only resolve labile or semi-labile pools of DOM with a limited number of DOM source and sink terms that are poorly constrained and thus, unable to realistically simulate DOM spatial distributions or concentrations (Anderson et al., 2015). This is why the marine biogeochemical model that we use as the basis for the work does not explicitly resolve DOC and is instead parameterized to implicitly include these dynamics in a manner that allows the model to simulate other key marine biogeochemical variables in a reasonable manner (Keller et al., 2012). Furthermore, there is also not enough information on the production and bioavailability of DOC from different macroalgae species to add a new parameterization to a model of DOM cycling, if one was available. Few studies exist and the uncertainties about the release of DOC are large. For example, in the Barrón et al., (2014) study, which is often cited by C. Duarte and colleagues, the release of DOC by macroalgae from a few species is reported to be 23.2 ± 12.6 mmol C m-2 d-1 with no information on bioavailability. This does not mean that we think that DOC release by macroalgae is unimportant, we fully agree that it could be, however, we are unable to confidently include such dynamics in our model.

To address this issue we will include a statement in the methods section noting that we are unable to include DOC release by macroalgae due to a lack of information. This sentence will state:

"The parameterization of DOC release by macroalgae could not be included because of the lack of enough information. Few studies exist and the uncertainties about the release of DOC are large. For example, Barron et al., 2014 reported a release of DOC by macroalgae from a few species of 23.2 ± 12.6 mmol C m-2 d-1 with no information on bioavailability. Meanwhile, refractory DOC dynamics are difficult to include in a global Earth system model and beyond the scope of this study (Anderson et al., 2015; Mentges et al., 2019; Zakem et al., 2021). Thus, the DOC release from macroalgae is not included in this study.".

We will also add text to the discussion section to highlight the need for more research on this topic. This sentence will state:

"Macroalgae has been reported to release a considerable amount of DOC to the global DOC export from coastal to open ocean waters. The estimated total DOC release of macroalgae habitats is 730 PgC yr-1 (Duarte and Cebrian, 1996). The averaged DOC release rate by macroalgae ranges is 23.2 ± 12.6 mmol C m-2 d-1 (eq. 8.5 ± 4.6mol C m-2 yr-1), but with a high range of 8.4 ± 1.6 to 71.9 ± 33.1 mmol C m-2 d-1 (Barron et al., 2014). If we simply multiply this annual averaged DOC release rate with the MOS occupied area (S_MOS, Tab.5), the estimated annual DOC export by MOS would be 7.1 ± 3.8 x103 PgC (MOS) or 12.9 ± 5.8 x103 PgC (MOS_AU). Although the refractory DOC released by macroalgae could potentially be an additional contribution of carbon sinking by MOS, the available information of the generation and composition of the macroalgae DOC is not enough to either parameterize a model of this process, and more research on the topic is needed (Krause-Jensen et al., 2016, Barron et al., 2014, Barron et al., 2015)."

On another issue, I do not see the rationale to have zooplankton graze on farmed macroalgal biomass directly (the Trancoso et al. paper provides no observational evidence supporting this). It makes no sense to me that the same modeled organisms that would ingest phytoplankton would also affect the farmed biomass.

Response: Thank you for the opportunity to clarify why this was parameterized in the model, as both you and one other reviewer picked up on this point. In this study, we simulated the grazing on macroalgae in the marine nutrients–phytoplankton–zooplankton–detritus (NPZD) module of the UVic ESCM by having zooplankton graze on macroalgae. The confusion about having pelagic zooplankton graze on macroalgae likely comes about because we used the original terminology of such models, which designates all higher trophic levels as "zooplankton". We admit that there is little evidence that pelagic zooplankton graze on macroalgae. However, in our model "zooplankton" represent all higher trophic levels, thus, our parameterization is meant to include known macroalgae grazers such as amphipods (Jacobucci et al., 2008), gastropods (Chikaraishi et al., 2007; Krumhansl et al., 2011), sea urchins (e.g. Yatsuya et al., 2020) and fishes (e.g. Peteiro et al., 2012). Thus, we included this food web pathway to assess the sensitivities of macroalgae to all potential grazers in the ocean, assuming that with large macroalgae farms the pelagic larva of some grazing organisms like fish or urchins, would settle within the farms. Text will be added to the manuscript to clarify this point and what "zooplankton" actually represents in the model.

However, it is worth noting that even with the assigned grazing preference (ψ = 1×10−4), the "zooplankton" communities do not have large effects, via grazing, on macroalgae NPP nor the biomass of the zooplankton community. Please see the figure below (Fig. B12 in the revised MS).

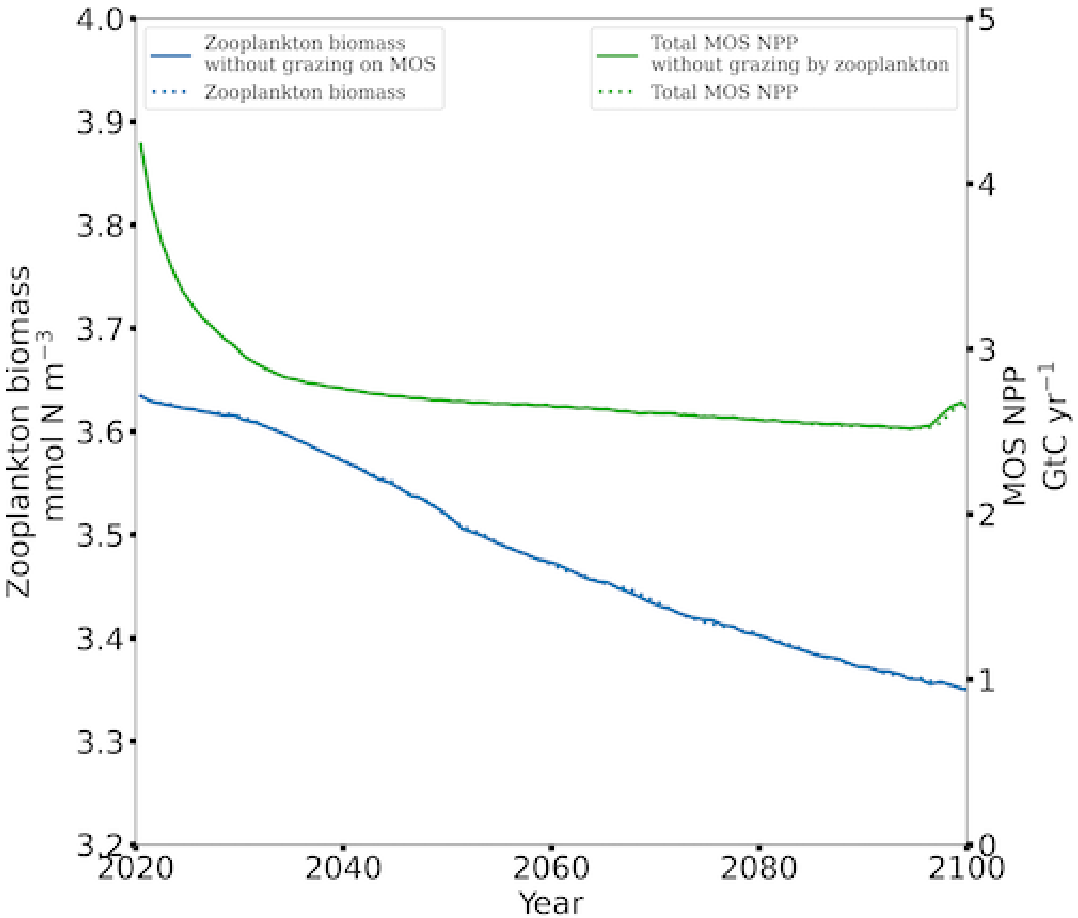

Fig. B12: Global profiles of average zooplankton biomass (left y-axis) & macroalgal NPP of MOS (right y-axis) in comparison with/without zooplankton grazing on macroalgae. The macroalgal NPP drops to zero as MOS is terminated at year 2100. The "zooplankton" communities do not have large effects, via grazing, on macroalgae NPP nor its own biomass.

Last, I am not convinced that biomass once harvested would be transported to depth without any losses. This assumption needs to stated or losses along the sinking path accounted for.

Response: Thank you for the opportunity to clarify our assumption, which is a key part of the idea that we test, of sinking biomass to the seafloor to avoid remineralization and maximize the sequestration of carbon. As we test an idea, we have had to assume that once harvested the biomass could be engineered and sunk in some manner. We cannot define exactly how this would be done as some engineering work would need to be conducted to find the most efficient way to sink biomass. However, one could imagine potentially baling the biomass and weighing it down to sink.

To clarify that this is our assumption we will add text to Sect. 2.2.2. after the 1st sentence of the paragraph stating that, "This assumes that the harvested biomass could be engineered to sink to the seafloor in a rapid and efficient manner with no remineralization along the way.”

My last major issue is the writing – both organization and in its execution. There is no single statement of the high-level modeling goals, assumptions and scenarios to be used and the rationale supporting their validity. That information is spread out from pages 4 to 14 (and beyond), making the paper very hard to read and review. This information clearly needs to be in one place – right after the introduction. I am sure that a serious relook at the organization of the paper would really help its overall presentation. With regard to execution, you spend too much time referencing what you are doing that is similar to other works, but do not say in the text what you’re actually doing. This is especially annoying in the model introduction. For example, in lines 96-97 you refer to models that yours is based on and then say how yours differs from these, but don’t say upfront what your generic adaptation of their will actually do. Also, lines 131-134 were particularly obtuse, but there are many other examples throughout. The model introduction section needs to be understandable without making the reader refer to a stack of other papers. This is a correctable writing issue that made it hard to review the text but requires serious attention.

Response: Thank you for the suggestion to improve the structure of the manuscript and to provide some more key information on the aims and details of the study. We will revise the organization of the manuscript and add more information to correct these deficiencies. First, as mentioned in the response to your first general comment we have added sentences stating the overall aims of the study to the 5th paragraph of Sect.1: Introduction.

We have also added text throughout the manuscript to highlight how our study addresses these aims:

In the 1st paragraph of Sect. 2.2.1, we added a sentence that states, “...Instead, the C:N:P ratio of the macroalgae biomass is assumed as a constant (Tab.1), which is based on seasonally averaged measurements of the biomass composition of these genus (Zhang et al., 2016; Martins and Marques, 2002).”

We have modified the description of the limiting factor of solar radiation intensity functions (Eq.11 & Eq.12) in Sect.2.2.1.

To better describe the model so that the reader doesn’t have to reference other articles we have made the following changes in Section 2.2:

- We added separate two paragraphs at the beginning of Sect. 2.2: “In this study, the modelling of macroalgae is done with a macroalgae growth model coupled in the UVic ESCM. In the macroalgae model, the net growth rate is affected by several limiting factors, including nutrients, temperature, and solar radiation intensity. The cellular C:N:P ratio of macroalgae is fixed. The loss of macroalgal biomass includes erosion and grazing by zooplankton. The deployment of MOS is done with an algorithm considering spatial and temporal conditions.

The macroalgae model is also connected to the global marine biogeochemical processes, including the inorganic carbon and nutrient pools. On the surface layers, it impacts on phytoplankton via nutrients competition and canopy shading. The zooplankton communities are also designed to graze on macroalgae. In the bottom layers, the remineralization of sunk macroalgal biomass will consume the dissolved oxygen, which in turn limits the rate of remineralization.”

- Sect. 2.2.1: “ The macroalgae model is an idealized generic model of genus *Laminaria* and *Saccharina*, mainly based on Martins and Marques (2002) and Zhang et al., 2016. The rate of biomass change is governed by Eq. 1 as the imbalance of **NGR** (net growth rate, d-1) and **LR** (loss rate, fraction of daily biomass loss due to mortality, erosion and grazing by zooplankton, d-1).

The aim of the macroalgae model is to investigate the carbon sequestration capacity of MOS as well as the potential impacts on marine biogeochemistry. Modelled macroalgae is seeded 5 meters underwater, considering the light requirement and reduction of damaging risks (Eq. 11). The deployment of macroalgae considers ambient nutrients availability and avoidance of winter periods (Sect. 3.1).

- At the end of Sect. 2.2.1: For simplicity and to limit the number of state variables, we made the following modifications to the macroalgae model:

- We did not include a dynamic C:N:P ratio or a representation of luxury nutrient uptake and storage (Broch and Slagstad, 2012; Hadley et al., 2015). Instead, the C:N:P ratio of the macroalgae biomass was set as a constant (Tab.1), which is based on seasonally averaged measurements of the biomass composition of these genus (Zhang et al., 2016; Martins and Marques, 2002).
- The macroalgae life cycle processes (e.g. alternations of generations) are also not considered in our model (Brush and Nixon, 2010; Trancoso et al., 2005; Duarte and Ferreira, 1997). We thus assumed that the plantlet (e.g. sporophytes for Saccharina) will be reseeded annually on the MOS infrastructure. The assumed deployment strategy, i.e., timing of seeding and sinking of MOS is latitude-dependent according to the seasonality of solar irradiance (see Sect.3.1). Whenever conditions are unfavorable for macroalgae and no growth occurs during an annual cycle, no re-seeding of macroalgae will occur in these regions.

Detailed comments follow.

Lines 131-146 were hard to follow without having the macroalgae cultivation assumptions stated earlier and a separate section outlining modeling goals and assumptions is needed.

We have added the 2nd paragraph in Sect. 2.2.1, which reads:

"The aim of the macroalgae model is to investigate the carbon sequestration capacity of MOS as well as the potential impacts on marine biogeochemistry. Modelled macroalgae is seeded 5 meters underwater, considering the light requirement and reduction of damaging risks (Eq. 11). The deployment of macroalgae considers ambient nutrients availability and avoidance of winter periods (Sect. 3.1)."

Meanwhile, the general goal of this study is described in the Introduction, which reads:

"The aim of this study is to investigate 1) the maximum physical / biogeochemical CDR potential of MOS; 2) the side effects of such large scale deployment, and 3) to understand where offshore macroalgae farming would be viable if done at a large scale. This information is needed to help prioritize further research into CDR, to understand if there are potential MOS side effects that become evident only at large scale, and to provide information on the viability of large-scale offshore macroalgae farming in different regions over time by accounting for the implications of nutrient utilization and climate change."

Lines 146-155 – Please tell the reader why is R_erosion a constant without having to refer to the reference.

Thank you for pointing out this issue. We have now revised these sentences as : "where the erosion of biomass (ER) is controlled by the individual erosion rate Rerosion. As the frond morphology of macroalgae is not modelled here, we set the Rerosion as a constant independent of physical impacts (Trancoso et al., 2005; Zhang et al., 2016).”

Lines 204-214 – Definition of FR is out of order and needs to be moved down after the sunk biomass is defined.

Thank you for the suggestion. We agreed and have moved the Eq.23 of FR downward. Now it reads as:

“The DIC from remineralization of sunk biomass will eventually be conveyed back to the ocean surface and may leak back to the atmosphere. Eq.23 calculates the ocean-retained fraction (FR, %) of MOS-captured carbon (MOS-C),

where the Ccaptured is carbon in cumulative sunk biomass, CSunk Biomass is the carbon in sunk macroalgal biomass that still remains on the seafloor.

FR=CretainedCcaptured=(DICremineralized + Csunkbiomass)Ccaptured (23)”

Table 1 – What is the column denoted “Reference” referring to? The references in this paper are not numbered.

Sorry for the confusion. These were in an early version, but were inadvertently left out of the final draft. The references have now been added directly to the table.

Table 1. Model parameters

Symbol	Parameter		Unit	Value	Reference
a_{mos}	Macroalgae carbon specific shading area		$m^2 kgC^{-1}$	11.1	Trancoso et al. (2005)
d	Distance between the cultivating ropes		m	10	Van Der Molen et al. (2017)
$R_{erosion}$	Individual erosion rate		$\% \ d^{-1}$	0.01	Zhang et al. (2016)
I_{opt}	Optimum light intensity for macroalgae growth		$W \ m^{-2}$	180	Zhang et al. (2016)
NO_3	Nitrate Concentration		$\mu mol \ l^{-1}$	model calculation	Keller et al. (2012)
PO_4	Phosphate Concentration		$\mu mol \ l^{-1}$	model calculation	Keller et al. (2012)
K_N	Half-saturation constant for nitrogen uptake		$\mu mol \ l^{-1}$	2	Zhang et al. (2016)
K_P	Half-saturation constant for phosphorus uptake		$\mu mol \ l^{-1}$	0.1	Zhang et al. (2016)
k_w	Coefficient of light attenuation through water		m^{-1}	0.04	Keller et al. (2012)
k_c	Coefficient of light attenuation through phytoplankton		$m^{-1} (mmol \ m^{-3})$	0.047	Keller et al. (2012)
M_{mos}	Thickness of MOS macroalgae canopy		m	10	Trevathan-Tackett et al. (2015)
$MR_{C:N}$	Molar C:N ratio of macroalgal biomass		-	20	Atkinson and Smith (1983)
$MR_{P:N}$	Molar P:N ratio of macroalgal biomass		-	0.05	Atkinson and Smith (1983)
$MR_{C:P}$	Molar C:P ratio of macroalgal biomass		-	200	calculated
$MR_{DW:WW}$	Ratio of DW to WW of macroalgal biomass		-	0.1 (values reported:0.05~0.2)	Aldridge and Trimmer (2009); Conover et al. (2016); Van Der Molen et al. (2017)
$MR_{C:DW}$	Carbon content of dried macroalgal biomass		%	30	Chung et al. (2011)
$MR_{N:DW}$	Nitrogen content of dried macroalgal biomass		-	0.16	Duarte et al. (2003)
R_{max20}	Maximum respiration rate at 20°C		$\% \ d^{-1}$	1.5	Martins and Marques (2002); Zhang et al. (2016)
r	Empirical coefficient for macroalgae respiration		d^{-1}	1.047	Martins and Marques (2002)
Seed	Initial macroalgal biomass	per kilometer cultivating line	$kgC \ km^{-1}$	2.5	Van Der Molen et al. (2017)
		concentration of N	$mmol \ N \ m^{-3}$	0.02	calculated
T_b	E-folding temperature of biological rates		°C	15.56	Schmittner et al. (2008)
T_{opt}	Optimum temperature for growth		°C	20(values reported: 13-30)	Zhang et al. (2016); Martins and Marques (2002)
T_{max}	Upper temperature limit above which growth ceases		°C	35	Broeman (1988)
T_{min}	Bottom temperature limit below which growth ceases		°C	0	Martins and Marques (2002)
u_{max}	Maximum growth rate		d^{-1}	0.2	Zhang et al. (2016)
w	Areal mean artificial upwelling rate		$cm \ d^{-1}$	1,10	Oschlies et al. (2010b)
Y_{max}	Maximum yield of macroalgal biomass on MOS		$t \ DW \ km^{-2} \ yr^{-1}$	3300	
ψ_{ma}	Zooplankton grazing preference on macroalgae		-	1×10^{-6}	Trancoso et al. (2005)
ρ_{ma0}	Remineralization rate of sunk macroalgal biomass at 0 °C		$\% \ d^{-1}$	7	Partanen et al. (2016)

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Line 236 – The units for Seed and KN do not match in Table 1. ???

Sorry for the confusion. To make the concept of Seed more comprehensive, we provide the unit of *Seed* in Table 1 as kgC km-1, referring to 'Initial macroalgal biomass per kilometer cultivating line'. However, in the UVic ESCM, macroalgae biomass is calculated as concentration of N in the ocean model (Seedconc, mmol N m-3). It can be converted according to the mass conversion functions in Sect. 2.2.4 and Appendix A1 as:

Seedconc = Seed/(12 x MRC:N x d x Depth1st_layer)

Where the Depth1st_layer is 50m, referring to the depth of the top layer of UVic ESCM. Accordingly, the Seed of 2.5 kgC km-1 is equal to Seedconc of 0.02 mmol N m-3.

We have modified the item of *Seed* in Table 1. Now it reads as

Symbol	Parameter	Unit	Value	References
Seed				