Response to Reviewer 1 Comments

Anonymous Referee

for

Data-driven equation discovery of a sea ice albedo parametrisation

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by

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Authors' Response to Reviewer 1

General Comments. This paper presents a novel and well-executed application of symbolic regression for data-driven discovery of a sea ice albedo parametrisation based on satellite and reanalysis data. The authors demonstrate how an interpretable machine-learning approach can be used to derive a physically constrained, low-complexity functional form that substantially improves upon the very simple albedo parametrisation currently used in FESIM. The methodological framework is sound, the ML methods are appropriate and carefully applied, and the Pareto-optimal perspective on error versus complexity is particularly convincing. The paper is well written, the figures are clear and convincing, and the presentation is very well structured. The appendices are comprehensive and helpful, and the authors do a good job at explaining and interpreting the discovered equation in physical terms. Overall, this is a very nice example of how observational and reanalysis datasets can be used in an innovative and effective way to inform parameterisations of physical processes in climate models.

My comments above are mostly in the spirit of model development and robustness, and aim at strengthening the physical interpretation, generality, and long-term applicability of the proposed approach.

General Comments. Main limitations and suggestions for future work

In its current form, the study remains an offline, data-driven evaluation: a proper assessment of the impact of the new parametrisation within FESOM itself is still missing. While I do not think this is a blocker for the present paper, it is an important next step if the results are to become truly impactful for climate modelling.

In addition, the benchmarking is currently limited to statistical and ML-based baselines. The study would be significantly strengthened by a comparison against a more physically based or numerically robust albedo scheme, for example radiation schemes used in models such as Icepack, or a parameterisation like that of Holland et al. (2012, 10.1175/JCLI-D-11-00078.1). Even if such schemes are not implemented in FESOM, they would provide a much more meaningful physical reference than polynomials or neural networks.

More generally, some of my comments raise concerns about generalisability, in particular:

- the reliance on predictors that may encode reanalysis-specific biases (e.g. the T0m–T2m difference),

- the question of transferability to different forcings (e.g. coupled configurations, future reanalyses),

- and the applicability to other regimes (e.g. Antarctic sea ice, future climates, palaeoclimate states).

Finally, given that the study does not yet demonstrate the impact of the scheme inside FESOM, it might be worth toning down the emphasis on FESOM/FESIM in the framing. At present, the work is best seen as a general, observation-driven albedo parameterisation study rather than a demonstrated FESOM model improvement.

Overall recommendation

Overall, I find this to be a strong, original, and well-executed study that convincingly demonstrates the potential of interpretable machine learning for parameterisation development. The paper is already of high quality, and addressing (or at least discussing) the points raised above would further strengthen its physical robustness, scope, and long-term relevance for climate modelling.

Response: We thank the reviewer for the insightful summary and positive feedback on our work. In the revised manuscript, we will address the reviewer's specific comments, clarifying the physical robustness, scope, and long-term relevance for climate modelling. The reviewer's concern about generalisability and robustness will be addressed in detail in the next comments. Here, we would like to respond to the suggestions regarding comparing against a more physically based or numerically robust albedo scheme and toning down the emphasis on FESOM/FESIM.

First, for an "apple-to-apple" comparison, we need data which come with the same spatial and temporal resolution like the data products we chose for our methodology. As pointed out in L496-498, there is no data available on a daily, pan-Arctic scale e.g. for snow grain size, black carbon or algae which are needed to compute albedo using more sophisticated albedo schemes including explicit melt pond treatment like Holland et al. (2012) or Flocco et al. (2010). We acknowledge that comparing with more sophisticated albedo schemes would significantly strengthen the study. On the other hand, the simple PW79 is still a well-established albedo scheme used in ESMs like in AWI-CM3 (Streffing et al. 2022).

Second, we acknowledge that our study remains an offline, data-driven evaluation. We will revise the manuscript in such a way that we town down the emphasis on FESIM/FESOM, including changes suggested by Comment 1 of reviewer #2.

Abstract

In many sea ice models, a single-category, zero-layer thermodynamic scheme is employed, in which sea ice albedo is prescribed based on surface types depending on snow cover, surface temperature, or sea ice thickness. The Parkinson and Washington parametrisation (PW79) is a commonly used one, which assigns four constant albedo values corresponding to distinct surface types. This parametrisation is too simple to capture the spatiotemporal variability of observed sea ice albedo. $\cdots$

$\cdots$, showing that the equation excels in balancing error and complexity and reduces the mean squared error by about 51 % compared to PW79.

Comment 1

Line 25: The statement that thinner and younger sea ice necessarily has a lower albedo than thicker, older ice seems too general. While thicker ice can indeed accumulate more snow, which increases surface albedo, the albedo of bare ice does not necessarily decrease with decreasing ice age or thickness. In particular, younger ice typically has higher salinity, which may increase scattering and thus lead to higher bare-ice albedo. The authors may wish to clarify the conditions under which this statement holds (e.g. snow-covered vs. bare ice) or provide a supporting reference.

Response:

We acknowledge that this statement holds for more idealised data (see Grenfell 1979) and will clarify the lines accordingly.

> **L24 ff.**
>
> Neglecting microstructural features such as salinity or atmospheric aerosols, thinner and younger sea ice, prevalent due to these changes, has a lower albedo (Grenfell, 1979), which leads to a higher absorption of the solar radiation by the sea surface, thereby promoting sea ice melting and the formation of melt ponds (Perovich et al., 2002; Light et al., 2022; Niehaus et al., 2024).

> **Comment 2**
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> Line 115: It is unclear whether the reference here is still to PIOMAS. The sentence would benefit from greater clarity and, if appropriate, an explicit citation. In addition, the discussion would be strengthened by considering relevant recent work such as Cocetta et al. (2024, The Cryosphere), which may be pertinent to the points being made.

Response: The references are the Quality Information Document (`https://documentation.marine.copernicus.eu/QUID/CMEMS-ARC-QUID-002-003.pdf`) and the Synthesis Quality Overview (`https://documentation.marine.copernicus.eu/SQO/CMEMS-ARC-SQO-002-003.pdf`) of TOPAZ4b. We will add these references and also the reference you suggested to the respective lines, which also Reviwer #2 pointed out in Comment 6.

> **Comment 3**
>
> Line 132: Over the course of one week, sea ice can drift substantially. It is therefore unclear whether sea ice motion is accounted for in the weekly accumulation of snowfall and rainfall. Could the authors clarify whether these accumulations are computed in a Lagrangian (ice-following) or Eulerian framework? If not accounted for, this mismatch may introduce inconsistencies between surface forcing and ice properties. Relatedly, could sea ice velocity or the atmospheric wind beyond the day before provide additional predictive skill for albedo, particularly in regions with strong ice drift?

Response: The weekly accumulated snowfall and rainfall used in our analysis are computed in an Eularian framework and do not take into account sea ice advection. The reviewer has a good point that sea ice motion could produce a mismatch in these weekly

accumulated fields and the albedo state. We now acknowledge this within the manuscript and that this could modify the influence of these precipitation fields for determining the albedo.

Regarding whether sea ice velocity or winds beyond the day before could provide additional predictive skill for albedo, we expect that they might but also expect that the general conclusions from our study that snow thickness and surface temperature are the most informative predictors would remain the case. Indeed, it is possible that other predictor variables at additional lead times could also be impactful but exploring this is beyond the scope of our study. We now include text within the manuscript to reflect this.

> **L131**
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> Additionally, we adjust rain and snowfall using a cumulative sum from the preceding seven days to consider a weekly memory effect. This cumulative sum is computed using an Eulerian framework and so neglects the potential influence of sea ice advection. This could modify the influence of these fields on the albedo state. We acknowledge that some of our predictor fields could be considered at different time lags and that other variables not considered here could influence the surface albedo. However, we have retained the existing set of variables as a reasonable balance between completeness and feasibility.

> **Comment 4**
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> Line 188 (Point 3): With respect to point 3, please see my earlier comment regarding the role of salinity. The assumption that thicker ice necessarily has a higher albedo under freezing conditions may not always hold for bare ice. An explicit dependence on ice age, which is closely linked to salinity and microstructure, might therefore be more appropriate or at least worth discussing.

Response: We thank the reviewer for their perspective. However, the sequential feature selection method consistently ruled out ice age from the most informative input features, suggesting that for the given dataset, ice age is not a key input feature that drives sea ice albedo. Therefore, it would be obsolete to formulate a physical constraint based on an input feature which is not used in the equation.

However, we acknowledge to clarify the physical constraints as approximations which do not take into account microstructural features and will revise the lines accordingly.

> **L190 ff.**
>
> The PCs are approximations which we assume for large-scale applications and for simplicity. We do not account for microstructural characteristics such as salinity and atmospheric aerosol deposition. For instance, younger, bare ice typically has higher salinity, which may increase scattering and therefore increase albedo comparable to multiyear ice (Light et al. 2015; Perovich and Grenfell 1981), while the deposition of atmospheric aerosols reduces albedo independent on snow or sea ice thickness (e.g. Warren and Wiscombe 1980; Hansen and Nazarenko 2004).

> **Comment 5**
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> Line 229: The statement that snow is "the most reflective medium in nature" appears too strong and would benefit from clarification and a supporting reference. While fresh, clean snow is among the most reflective natural surfaces, particularly in the visible spectrum, its albedo depends strongly on grain size, age, wetness, and impurities, and decreases substantially in the near-infrared. Moreover, other natural media (e.g. certain cloud types) can exhibit higher broadband reflectance. A more precise phrasing such as "snow is among the most reflective natural surfaces, especially when fresh and dry" would be more accurate.

Response: We agree with the reviewer and will revise the lines accordingly.

> **L299**
>
> h_{snow} being the most informative predictor is plausible since snow is among the most reflective natural surfaces, especially when fresh and dry. When present, snow represents the uppermost layer where solar radiation initially impacts.

Response: We thank the reviewer for this insightful observation and acknowledge further clarification of the correlation matrices shown in Appendices A1 and A2.

While the correlation matrices of the satellite and reanalysis data (A1) exhibit a moderate to strong Pearson correlation between snow and sea ice thickness (0.8 and 0.62, respectively), the merged, standardised dataset (A2) shows a significantly weaker Pearson correlation of 0.19. However, the difference in the correlations is not attributed to the standardisation, since standardising solely rescales variance which does not alter linear relationships. What actually happens is that the satellite and reanalysis data represent different seasons, namely from March until mid April and from mid April until September, respectively. The fact that both datasets show similar correlation reveals that they exhibit similar internal correlation structures, but the reduced correlation of the merged dataset implies seasonal heterogeneity, suggesting that the relationship between snow and sea ice thickness is regime-dependent. This is physically plausible since from March until April the dataset includes both multiyear (Central Arctic) and seasonal sea ice (e.g. Barents Sea, Kara Sea), while from April onwards, multiyear ice dominates the dataset due to the melting season. This is evident in Sec. 5.2, where we showed a reversed weighting of snow and sea ice thickness due to regime-dependent variance structure of snow and sea ice thickness when comparing pan-Arctic with Barents Sea data.

Furthermore, we ran SFS ten times where on average, sea ice thickness is ranked as third most predictive input feature. If sea ice thickness was only a proxy for snow thickness, sea ice thickness should add little additional predictive value after selecting snow thickness. However, evaluating the error-complexity plane (Fig. 6 in the manuscript), we see that after adding sea ice thickness (3-feature NN, third purple point from the right), the MSE is reduced by 0.0022 compared to the 2-feature NN, indicating that sea ice thickness contributes information beyond what is explained by snow thickness alone.

Overall, we agree with the reviewer to weaken the physical interpretation and instead interpret the ranking in a statistical sense. However, the differing correlations between snow and sea ice thickness in A1 and A2 stem from seasonal heterogeneity and not from the standardisation of the features, therefore not explaining the SFS ranking.

L239

As sea ice has a higher optical depth than snow, h_{ice} ranked below h_{snow} seems plausible, implying that h_{snow} provides larger marginal predictive improvement than h_{ice}.

Comment 7

Line 245: While the interpretation is plausible in principle, in practice it should be treated with caution. The 2 m air temperature (T2m) is taken from a reanalysis product that does not assimilate sea ice or snow thickness, nor near-surface Arctic observations, apart from surface pressure from stations and drifting buoys. In contrast, T0m is derived from satellite observations. The fact that the machine-learning model does not fully exploit the strong physical relationship between T0m and T2m may therefore reflect inconsistencies between these two datasets rather than genuine additional predictive skill of T2m for sea ice albedo. This distinction would be worth discussing more explicitly.

Response: We thank the reviewer for raising this point. We will incorporate it into the revised version, including changes suggested by Comment 3 of reviewer #2:

L244 ff.

Additionally, this may be due to the fact that $T_{2\mathrm{m}}$ can exceed the melting point, whereas $T_{0\mathrm{m}}$ cannot. Another consideration is that ERA5 does not assimilate sea ice or snow thickness, nor near-surface Arctic observations, except for surface pressure from stations and drifting buoys. Previous studies have shown that this leads to warm temperature biases in ERA5 over the Arctic, particularly during

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polar winter clear-sky events (Batrak and Müller 2019; Zampieri et al. 2023). Such biases could introduce inconsistencies between the satellite-derived T_{0m} and the *ERA5*-biased T_{2m}, which might partly contribute to the predictive skill attributed to T_{2m}. The documented warm bias is particularly large during polar winter stable boundary layer conditions. Our exclusive use of polar-day samples will thus help to mitigate the influence of this bias. However, this documented warm bias in *ERA5* and data inconsistencies between surface and 2 m air temperatures may play some role in our results, although we believe it is unlikely to fully explain the relationship identified here.

Comment 8

Subsection 3.2.2: The interpretation of the weighted difference between T0m and T2m as a physically meaningful seasonal proxy deserves further scrutiny. The seasonal variation of this difference largely reflects a well-documented bias in the ERA5 reanalysis, which has been shown to be larger in winter than in summer (e.g. Batrak and Müller, 2019; Zampieri et al., 2023). As such, this signal may primarily encode characteristics of the reanalysis system rather than an intrinsic physical control on sea ice albedo.

This raises the question of whether it is desirable to base a model parametrisation on a feature that is strongly influenced by a known, non-stationary reanalysis bias. In particular, the robustness of the proposed parametrisation is unclear if different forcing datasets are used to run FESOM (e.g. a future ERA6 reanalysis or a coupled model configuration), where the magnitude or structure of near-surface temperature biases may differ substantially.

Moreover, ERA5 temperature biases are not static in time and are themselves affected by climate change. It is therefore uncertain whether the derived parametrisation would generalise well under future climate conditions.

If the intention is to encode seasonal information, a predictor more directly linked to the physical drivers of seasonality, such as variables related to insulation, radiative forcing, or surface energy balance, might be more appropriate than relying on the difference between two temperature fields with differing observational constraints. A more explicit discussion of these limitations and their implications for generalisability would strengthen the manuscript.

Response: We thank the reviewer for highlighting this issue. Please see our response to Comment 7. We agree that the warm temperature bias in ERA5 documented in Batrak and Müller 2019 and Zampieri et al. 2023 should be taken into consideration when interpreting the temperature drivers of α. At the same time, our dataset exclusively contains polar-day data which help mitigate the influence of this bias. Moreover, the reviewer is right to point out that using other predictors more directly linked to the physical drivers of seasonality might be more robust to encode seasonal information. We will acknowledge these points in the manuscript, which could be explored in future work, including changes suggested by Comment 3 of reviewer #2.

L304 ff.

While we expect that ΔT^* is providing meaningful physical information, the seasonal cycle that is reflected in ΔT^* could be influenced by the aforementioned bias in *ERA5* T_{2m} which is largest during the cold season and not present during summer months. Nevertheless, it does suggest that information on the seasonal cycle is useful in providing a constraint on the surface albedo. Other possible predictors that encode information on the seasonal cycle, such as solar insolation or the surface energy balance, could also provide useful information and could be explored in future work. Considerations of training data biases and prioritisation of predictors that enable results to be generalised across regions and different climate states are important for possible ML-based parametrisations that could be developed based on this work.

L478 While the PW79 sea ice albedo parametrisation only uses the surface temperature as a proxy to define freezing and melting conditions, our equation shows that a weighted temperature difference between the surface and the air at $2\,\mathrm{m}$ better encodes information on the seasonal cycle. As our physical interpretation could be influenced by the warm $2\,\mathrm{m}$ air temperature biases in *ERA5*, other possible predictors that encode information on the seasonal cycle, such as solar insolation or the surface energy balance, could be explored in future work.

Concerning the robustness due to non-stationary ERA5 biases and generalisability of Eq. 4, please see our response to Comment 11.

Response: We thank the reviewer for this remark. The reviewer is right to address the functional form of the proposed equation which includes two hyperbolic tangent functions with an asymptotic behavior. We already analysed the infimum and supremum of our proposed equation in L307-320, which is constrained by $\tilde{a}$ and $\tilde{b}$. So mathematically speaking, it is not the functional form of the equation but the coefficient values that limit the equation to predict extreme albedo values. We will add a reference to Sec 3.2.3 to clarify how the functional behavior of the equation plays a role in predicting extreme values, including changes based on Comment 4 of reviewer #2.

L367-368

In contrast, Light et al. (2022) reports highly localised albedo values for each surface type. Furthermore, as examined in Sec. 3.2.3, the lower and upper limits of Eq. 4 are determined by the coefficients $\tilde{a} = 0.84$ and $\tilde{b} = 2.19$, which have been optimised using the pan-Arctic dataset. With these coefficient values, Eq. 4 is unable to reproduce extreme albedo values. On the basis of these considerations, we infer that the upper albedo limits prescribed by Eq. 4 is physically plausible, particularly given that the reference observation is noisy and represents average albedo over a large area.

Response: We thank the reviewer for this suggestion. The motivation behind analysing regional and seasonal differences is to explore how well the functional form of the proposed equation transfers across different regions and seasons. Section 5.1 serves more of a diagnostic tool to uncover what drives reproducing accurate α, revealing that the coefficients are sensitive to the prevailing sea ice regimes.

We acknowledge that encoding spatial and temporal information could be useful to apply our proposed parametrisation to an Arctic-wide operational model, since in this way, we potentially capture these regime variations. The core of the idea would be to make the parametrisation regime-aware. Please see our response to Comment 11, where we elaborated on the study by Nath et al. 2026.

> ### Comment 11
>
> Section 6 (Conclusions): The conclusions would benefit from a more detailed discussion of several important aspects related to the applicability and implications of the proposed parametrisation.
>
> First, FESOM is a global model. It would therefore be important to discuss how this parametrisation might be applied to the Antarctic sea ice, and whether the authors expect the same functional form and predictors to remain meaningful in the Southern Hemisphere.
>
> Second, it would be valuable to comment on whether the proposed parametrisation is expected to generalise beyond the present-day climate, for example to future climate change conditions or even to palaeoclimate regimes. Given the emphasis placed on the physical interpretability of the discovered equation, the authors are in a good position to discuss this point.
>
> Third, FESOM is used in a variety of configurations (e.g. coupled versus ERA5-forced). Introducing this parametrisation removes an important tuning knob that currently exists in the system. Is there a plan to retain some degree of tunability (e.g. through selected coefficients), and can the authors provide recommendations for how this scheme should be integrated and tuned in different model configurations?

Response: We agree with the reviewer to add a discussion on the generalisability of our proposed parametrisation and elaborate further on the applicability and implications when running online simulations, including consequences for tuning.

We will revise the following paragraphs, including changes suggested from Comment 3 of reviewer #2 and Comment 1 of reviewer #3.

> **L473 ff.**
>
> This case study gives a first insight on how Eq. 4 can be transferred to different ice regimes, specifically from a stable, pan-Arctic regime dominated by multiyear ice in the Central Arctic to a fragile ice regime characteristic of the Barents Sea. Although we acknowledge that the MSE in the Barents Sea remains relatively high compared to other regions after fine-tuning (see Sec. 5.1), the functional form of Eq. 4 remains to be physically reasonable. This enables a comparison between the coefficients obtained from global and regional optimisations demonstrating that the optimal coefficients are state-dependent.

> **L502**
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> As this mirrors present-day conditions, other subregions are underrepresented in our dataset where the impact of climate change is more pronounced such as the Barents Sea.

> **L508 ff.**
>
> An important question concerns how well Eq. 4 generalises well beyond Arctic sea ice regimes represented in the training data. Ship-based field measurements in the Antarctic by Brandt et al. 2005 and Tersigni et al. 2025 demonstrate that already a thin snow layer of a few centimetres substantially increases sea ice albedo, emphasising that snow fractional coverage is more impactful than snow thickness. Here, snow redistribution is mainly driven by strong winds, particularly in the marginal ice zones. Since our dataset has a spatial resolution of 25 km, retrieved h_{snow} likely reflects a combination of snow thickness and fractional coverage. The strong sensitivity of Eq. 4 to small variations in thin snow therefore suggests that the influence of snow on albedo is captured in a physically meaningful way.
>
> Nevertheless, Sec. 5 indicates that the optimal coefficients of Eq. 4 are state-dependent. While the functional form remains transferable, as demonstrated by the Barents Sea case study, the relatively high MSE in this region after fine-tuning suggests that additional processes, such as oceanic heat fluxes, may drive sea

ice albedo but are not explicitly represented in Eq. 4. Oceanic heat fluxes also drive sea ice melt in the Antarctic (Brandt et al. 2005), which may influence sea ice albedo, implying that further offline investigations are required to assess the robustness of the parametrisation outside the pan-Arctic region. This is presently limited due to the lack of data availability on a daily, Antarctic scale.

In practice, Eq. 4 naturally retains a degree of tunability that facilitates its implementation in ESMs. Integrating Eq. 4 online into an ESM or operational sea ice forecast model would not require substantial changes in existing tuning protocols, as the parameter space can simply be expanded by the seven coefficients of Eq. 4 to obtain physically plausible sea ice states. Under different atmospheric forcings, either in an ocean–sea-ice stand-alone configuration driven by an atmospheric reanalysis product or in a fully coupled configuration, we hypothesise that distinct optimal values of these coefficients will emerge, particularly those controlling ΔT^* where the sea ice model receives $T_{2\mathrm{m}}$ from the atmosphere, since biases in atmospheric temperature fields vary across forcing datasets (Batrak and Müller 2019). This highlights the potential value of regime-aware parametrisations, as suggested by Nath et al. 2026, in which the parameter space is dynamically adjusted in response to the prevailing climate state, allowing the scheme to remain applicable across Arctic, Antarctic, and potentially future or paleoclimate sea ice regimes.

Overall, our results suggest that the functional form of Eq. 4 provides a physically interpretable representation of sea ice albedo variability, while the optimal coefficients depend on the prevailing climate state. This state dependence implies that the globally optimised coefficient set may not remain optimal across different regions or climate states. Yet, the explicit formulation and limited number of coefficients make the parametrisation well suited for online implementation in ESMs, where the coefficients can be tuned to the model's specific climate regime. Further evaluation, particularly in Antarctic conditions and under future or paleoclimate conditions, will be necessary to assess the broader applicability of the approach.

Batrak, Yurii and Malte Müller (Sept. 2019). "On the warm bias in atmospheric reanalyses induced by the missing snow over Arctic sea-ice". en. In: *Nature Communications* 10.1, p. 4170. DOI: `10.1038/s41467-019-11975-3`.

Brandt, Richard E. et al. (Sept. 2005). "Surface Albedo of the Antarctic Sea Ice Zone". en. In: *Journal of Climate* 18.17, pp. 3606–3622. DOI: `10.1175/JCLI3489.1`.

Grenfell, Thomas C. (1979). "The Effects of Ice Thickness on the Exchange of Solar Radiation Over the Polar Oceans". en. In: *Journal of Glaciology* 22.87, pp. 305–320. DOI: `10.3189/S0022143000014295`.

Hansen, James and Larissa Nazarenko (2004). "Soot climate forcing via snow and ice albedos". In: *Proceedings of the National Academy of Sciences* 101.2, pp. 423–428. DOI: `10.1073/pnas.2237157100`. eprint: `https://www.pnas.org/doi/pdf/10.1073/pnas.2237157100`.

Light, Bonnie et al. (Nov. 2015). "Optical properties of melting first-year A rctic sea ice". en. In: *Journal of Geophysical Research: Oceans* 120.11, pp. 7657–7675. DOI: `10.1002/2015JC011163`.

Nath, Pritthijit et al. (Jan. 2026). "Making Tunable Parameters State-Dependent in Weather and Climate Models with Reinforcement Learning". In: arXiv:2601.04268 [cs]. DOI: `10.48550/arXiv.2601.04268`.

Perovich, Donald K. and Thomas C. Grenfell (1981). "Laboratory Studies of the Optical Properties of Young Sea Ice". en. In: *Journal of Glaciology* 27.96, pp. 331–346. DOI: `10.3189/S0022143000015410`.

Streffing, Jan et al. (Aug. 2022). "AWI-CM3 coupled climate model: description and evaluation experiments for a prototype post-CMIP6 model". en. In: *Geoscientific Model Development* 15.16, pp. 6399–6427. DOI: `10.5194/gmd-15-6399-2022`.

Tersigni, Ippolita et al. (2025). *Seasonal Variability of Snow Cover and Impact on Albedo and Thermal Properties in the Antarctic Marginal Ice Zone*. Version Number: 1. DOI: `10.48550/ARXIV.2507.10916`.

Warren, Stephen G. and Warren J. Wiscombe (Dec. 1980). "A Model for the Spectral Albedo of Snow. II: Snow Containing Atmospheric Aerosols". en. In: *Journal of the Atmospheric Sciences* 37.12, pp. 2734–2745. DOI: `10.1175/1520-0469(1980)037<2734:AMFTSA>2.0.CO;2`.

Zampieri, Lorenzo et al. (June 2023). "A Machine Learning Correction Model of the Winter Clear-Sky Temperature Bias over the Arctic Sea Ice in Atmospheric Reanalyses". In: *Monthly Weather Review* 151.6, pp. 1443–1458. DOI: `10.1175/MWR-D-22-0130.1`.