Comments from Referee #2 (responses are in blue):

Overall Impression:
This manuscript presents a timely, and valuable study that addresses a critical challenge in hydrology: monitoring intermittent rivers and ephemeral streams (IRES). The application of a relatively simple logistic regression model to classify flow states from field camera imagery is both pragmatic and innovative. The methodology is clearly described, the results are robust and convincingly presented, and the discussion thoughtfully places the work in the broader context of IRES monitoring, climate change, and water management. The integration of image classifications for quality control of stage data is a particularly strong and practical contribution. The manuscript is generally well-written and structured. I believe it represents a significant contribution to the field and is a strong candidate for publication after revisions.

Thank you for your thoughtful review.

Abstract

The abstract effectively summarizes the study's motivation, methods, key findings, and implications. It clearly states the problem (monitoring challenges in IRES), the proposed solution (image classification), the location, and the broader significance of the work. However, I am including some comments and questions that can improve the abstract.

1. The abstract mentions the model was used for quality control of the stage time series. Could you briefly hint at the nature of the discrepancies/uncertainties/errors found (e.g., sensor drift, noise during high flow) to immediately highlight the practical utility of the Method?
Yes, we propose adding the following to the abstract: "We then used image classifications to perform quality control on the continuous stage time series, which allowed us to identify when the stream was dry and when the sensor malfunctioned."

2. The term "Imagey" in the title appears to be a typo for "Image-based." Was this Intentional?
"Imagery" was intentional, but upon review, we agree that "Image-based" more correctly emphasizes that we are using images as an input for classification, so we propose using "Image-based" instead. Thank you.

3. The abstract focuses on categorical classification. Did the model's probability output itself provide any additional, continuous-like insight beyond the three discrete categories?
Although we did not explore the probability results beyond what was relevant to our categorical classifications (i.e., Figure 6), we did observe some overlap between 'low water' and 'high water' image classifications, and a range of corresponding probabilities assigned to these classes. For example, some photos labeled as "high water" were classified as "low water" and

vice versa. Nevertheless, we ultimately evaluated only the probability distributions for ‘any water’ (the combination of low and high water; Figure 6).

Introduction

The introduction provides a comprehensive and compelling background, effectively building the case for the importance of IRES and the difficulties in monitoring them. The literature review is extensive and covers relevant areas, including remote sensing, citizen science, and various modeling approaches.

1. While you cover technological methods, could you briefly mention the organizational/funding challenges of maintaining sensor networks in remote IRES to further justify the need for low-cost methods?
Yes, we mention this in line 60: “In addition to inaccessibility, developing an in-situ monitoring network for stage and discharge on IRES is difficult because nascent gage networks may have less expertise, support, or funding compared to established national programs that generally focus on perennial streams (Vlah et al., 2023).”

2. You mention that deep learning has been mainly applied to perennial streams. Could you elaborate on one or two key reasons why these methods are particularly challenging to directly transfer to IRES (e.g., more dynamic channel geometry, greater debris, longer dry periods)?

We mention deep learning approaches in the Introduction (l. 101-111) and the Discussion (l. 463-465), and we propose adding the following to the Discussion: “Current deep learning models in the USGS Flow Photo Explorer (USGS, 2024) estimate relative flow states but cannot distinguish dry streambeds (Gupta et al., 2022; Goodling et al., 2025), potentially due to the dynamic channel morphology, shifting debris and vegetation, and ambiguous flow states of IRES – all of which can make training deep learning models challenging.”

3. The introduction effectively sets up the use of machine learning with imagery. Would it be valuable to more explicitly state the core hypothesis: that visual features in daytime imagery are sufficient to reliably classify IRES flow states for monitoring purposes?
Thank you for this suggestion. We propose replacing “Here, we explore the use of image classification for categorizing water levels in IRES” with “Here, we explore whether visual features in daytime imagery are sufficient to reliably classify IRES flow states for monitoring purposes.”

4. Have you considered citing studies that discuss the hydrological significance of the "pooling" phase in IRES, which your method can detect but stage sensors cannot? For example, Stubbington, R., et al. (2017). The biota of intermittent rivers and ephemeral streams: aquatic and terrestrial assemblages. In Intermittent Rivers and Ephemeral Streams (pp. 217-245). Academic Press. could strengthen this point.

Thank you for highlighting the importance of the pooling phase in relation to biota in IRES. We discuss this later on in the Discussion section (4.1), and also propose including your suggested reference in that discussion..

5. The transition from the broad introduction to the specific objectives of the study is clear, but could the final paragraph be slightly more structured to explicitly list the primary aims of the paper?
Yes, we propose adding the hypothesis from your point #3 to this paragraph to make it more explicit.

Methods

The methods section is exceptionally detailed and reproducible, a major strength of the manuscript. The description of the study site, data sources, image preparation, model training, and validation is thorough. The handling of unbalanced classes and the development of a confidence metric are particularly sophisticated and commendable.

1. You limited image analysis to 9 am–4 pm PST to avoid low-light issues. Was any consideration given to using the camera's flash-illuminated nighttime images for a simple binary "water"/"no water" classification, given that this is a defining feature of IRES?
Yes, we address this directly in Section 2.2.1 and again in the Discussion section.

2. For the image cropping to 1000x1200 pixels, was this specific size determined empirically? Did you experiment with different crop sizes or aspect ratios to optimize feature recognition?
We describe the method for preparing images in Section 2.2.1. We propose adding the following to that section: “To apply our method, all images were required to have the same dimensions. We selected a resolution of 1,000 x 1,200 pixels (fig. 3a) because it was low enough to ensure that each image focused on the staff plate and streambed. Thus, the image size was determined by resolution constraints rather than through empirical or experimental testing.” In addition, we propose adding the following to the Discussion: “For sites with consistent camera types and viewing angles, a useful exercise could be to find the optimal image resolution and cropping extent for feature recognition. In such an exercise, the cost of increased computing power for higher resolution images should be balanced with model performance.”

3. The manual weighting scheme (3.5 for water categories, 3 for obstructed, 1 for 'no water') is interesting. Could you provide a sentence on the rationale behind these specific weight values?

Thank you for noting the omission of our reasoning behind the selection of manual weights. We propose adding the following explanation to this section: "We assigned manual weights to emphasize water presence categories (‘high water’ and ‘low water’) over ‘no water,’ and gave the ‘obstructed’ category a weight higher than ‘no water’ -- reflecting its smaller sample size -- but lower than the water categories, given its lesser importance."

4. The confidence level assignment is well-explained but based on qualitative assessment of probability distributions. Were any quantitative metrics (e.g., maximizing Youden's J index) explored to define the probability thresholds more objectively?
This is an interesting question, and we agree that we could have experimented with methods further in this case. However, because our primary objective was avoiding false positive classifications, the distribution (boxplot) assessment approach met our needs in this case. We agree, however, that a more objective strategy might be applicable in a generalized version of this study, and propose adding the following in the discussion: “In addition, a more objective strategy for evaluating classification confidence for other sites could be developed.”

5. You use soil moisture data from a nearby site (DRW) with probably different geology. How might this spatial disconnect influence the interpretation of the relationships between soil moisture and stage at PEC?
We discuss the differences in the DRW vs. PEC site, including soil features, in Section 4.3, l. 432-441, and propose adding the following to that section: “Specifically, the soil hydrologic group at DRW is Group B, which indicates moderate infiltration rates, while the PEC watershed contains Groups C and D, which indicate slow or very slow infiltration rates, respectively (SSURGO, 2024).”

In addition, a section of uncertainties would be great, since I can see some sources of uncertainties in your work/modelling, for example: image quality and environmental variability (the classification model's performance is inherently tied to the quality and consistency of the input imagery), sensor data reliability and spatial mismatch (the "ground truth" data used for validation and comparison are themselves sources of uncertainty), limited training data and site specificity (the model was trained on a relatively small, manually labeled dataset (537 images) from a single site. This raises uncertainty about its performance when transferred to other IRES with different channel morphology, substrate, vegetation, and water clarity. the model's features (e.g., learned from the specific staff plate and rocks at pec) may be overly tailored to this unique location). Do not get me wrong, I still think there is a lot of value in publishing this paper, however, it is good to show the uncertainties and potential bias of the approach.

The issue of transferability to other sites was also raised by Reviewer #1, and we appreciate you also bringing in the related concept of uncertainty, and how that relates to transferability.

The primary purpose of this work was to provide a proof of concept for this method using the Perry Creek site and its unique geographic context. Accordingly, we used a model that outputs prediction probabilities, allowing uncertainty in the predictions to be explicitly represented. We expect that both the distribution of probabilities and the assignment of confidence levels would likely vary across sites, making classification uncertainty primarily a within-site issue. Exploring this variation would require cross-site comparisons, which are beyond the scope of the present study. We can nevertheless provide a parsimonious set of examples to demonstrate the method's transferability.

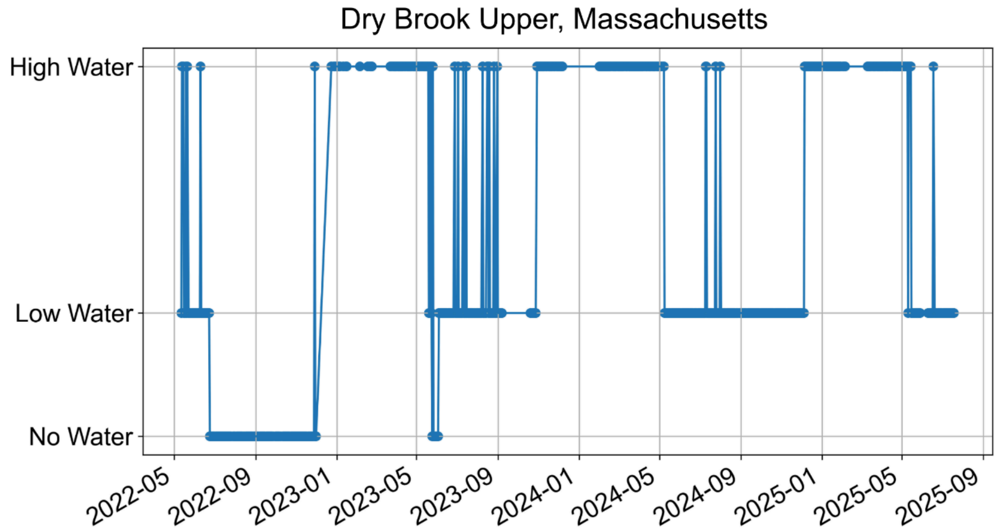
We tested the model code (which is posted on the HydroShare repository associated with this paper) on two example sites from the USGS Flow Photo Explorer to produce time series of categorical flow states. These sites are Dry Brook Upper in Massachusetts and USGS streamgage 10247170 on Troy Creek in Nevada. Below, we have included a draft of a proposed new appendix text and figures, which would be referenced in a revised Discussion section, where extensibility and the USGS Flow Photo Explorer in particular are discussed.

Proposed appendix on transferability to other sites:

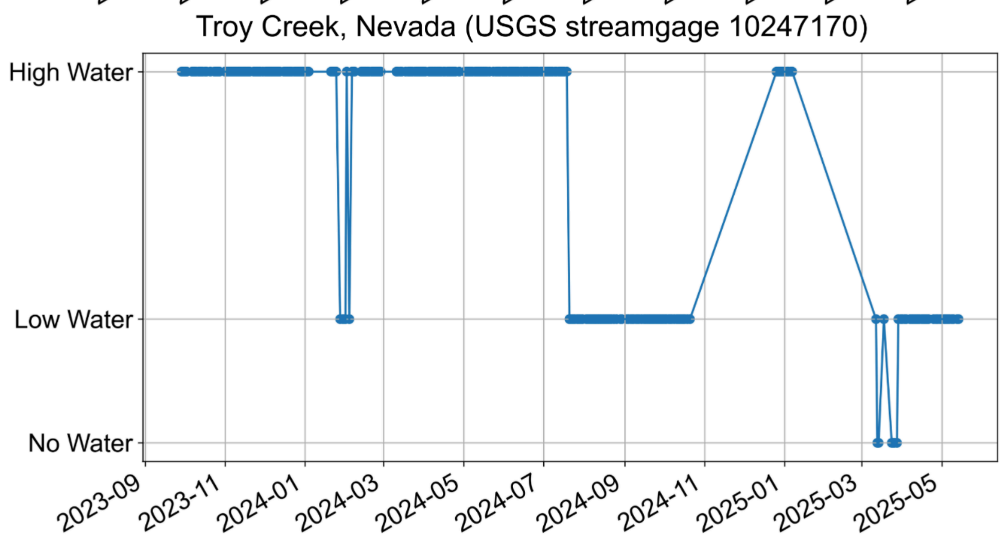
Although the main goal of this work was to demonstrate proof-of-concept at the PEC site, we also tested our model on two additional U.S. sites from the USGS Flow Photo Explorer: Dry Brook Upper in Massachusetts and USGS streamgage 10247170 on Troy Creek in Nevada (USGS, 2024). We selected these sites because they are both IRES with thousands of photos available. After labeling only 105 and 111 photos, respectively, the model achieved 84.4% accuracy at Dry Brook Upper and 76.5% at Troy Creek. The resulting time series of categorical flow states from model predictions (for all confidence levels) are shown in figure A12. This exercise was performed with fewer labeled photos compared to the PEC case, no photo cropping, and no changes to the model code (aside from updating the formatting of dates).

Based on this preliminary transferability analysis, we find that about 100 labeled images — with all categories represented in both training and testing sets — appear sufficient to transfer this method to other sites with consistent imagery. Notably, all photos used in this exercise were taken at noon, which likely enhanced model performance due to minimal variation in sun angle. While additional labeled photos would likely improve performance at any site, those with unbalanced categories or dramatic changes in illumination would benefit most.

(a)



(b)



Current deep learning models in the USGS Flow Photo Explorer (USGS, 2024) estimate relative flow states but cannot distinguish dry streambeds (Gupta et al., 2022; Goodling et al., 2025). Our method could complement the existing relative streamflow method, for example by being included in a conditional two-step approach: detect water presence first with our simple model; if present, estimate relative discharge using a CNN. This would preserve the simplicity and high accuracy of our model while enabling conditional estimation of streamflow when relevant. This approach would be well-suited to watersheds managed for both water supply and fishery health since both streamflow volume and stream connectivity would affect watershed management. With hundreds of thousands of photos available on the USGS Flow Photo Explorer (USGS, 2024) and the likelihood of increased IRES prevalence with climate change, this screening for IRES-specific states would be especially valuable. Instructions for applying our methodology to USGS Flow Photo Explorer images are available on HydroShare (see Data and Code Availability statement).

Results

The results are clearly presented, with appropriate use of tables, figures, and statistics. The model performance metrics are convincing, and the comparison between image classifications, observed stage, and modeled discharge is effective in demonstrating the value of the approach.

1. The confusion matrix shows that 'obstructed' images were most often misclassified as 'no water'. Given the ephemerality of the stream, do you think this misclassification might be functionally acceptable in many cases, as it likely reflects a true dry state?
Yes, this is a good point. We mention this in the Results section at l. 325-326 with: “In addition, ‘obstructed’ images were occasionally misclassified as either ‘high water’ or ‘no water’ (1.1% and 1.8% of these classifications, respectively). Due to the stream’s ephemerality, it is likely there was in fact no water at PEC in the ‘obstructed’ images misclassified as ‘no water’.”

2. Figure 7/9 and the text describe how image classifications identified sensor malfunctions. How many erroneous data points would have been missed without this image-based quality control? A rough percentage or count would powerfully quantify this benefit.
We describe our quality control of data (i.e., removal of erroneous stage data) at l. 370-383 and Appendix 2; we also propose revising Section 3.3 to improve clarity based on suggestions from Reviewer #1. Therein, we did not specifically calculate how many erroneous data points would have been removed using some other method, e.g., simple visual inspection or use of a value threshold vs. image-based quality control, but instead note that the image-based approach could either replace other methods or augment them. Even so, we propose adding to Section 3.3 that “we flagged and removed almost a month of these data from the record”.

3. In Figure 10, the results show a notable discrepancy where the NWM reported zero discharge during periods of observed high water (e.g., Jan-Feb 2018). What is your leading hypothesis for this systematic underestimation by the NWM in this specific Catchment?
In Section 4.2, we discuss reasons for the discrepancies between the NWM, observed discharge, and image classifications that are shown in Figure 10. To respond further to your question, we propose adding to Section 4.2: “Many ‘high-water’ image classifications occur during January - February 2018, when the NWM shows little discharge. This is illustrated by two manual discharge measurements from January 2018 (Figure 10), which record substantially higher flows at PEC than those simulated by the NWM. We hypothesize that the NWM struggles to represent early wet season flow processes in the steep slopes and low-infiltration soils of the PEC watershed. Later in the season, when soils in the PEC watershed are likely more saturated, the NWM discharge aligns more closely with PEC stage data and manual measurements, suggesting that the model performs better under saturated conditions.”

4. The relationship between stage and soil moisture at 5 cm is stronger than at 100 cm. Does this suggest that flow at PEC is primarily driven by shallow subsurface flow or saturation-excess overland flow rather than deeper groundwater contributions? If so,

that should be discussed, showing how the changes over time may impact the local hydrology of the watershed and river.
Yes, we discuss the relationship between stage and soil moisture in Section 4.3, l. 526-532, and propose adding to that discussion: “This, combined with our understanding of the geology of the PEC watershed, suggests that runoff generation at PEC is primarily driven by shallow subsurface flow and saturation-excess overland flow.”

5. You mention that high flows remain unmeasured due to safety. Could the image classifications be calibrated against the NWM output or other hydraulic models to provide a rough estimate of discharge during these extreme events?
We interpret this question as asking whether the images and NWM (or another model) could be used to estimate (continuous) discharge during extreme events, given the lack of observed discharge data. This would be beyond the scope of our study. Our model was not trained to predict continuous flow, as some previous studies have done (see references to Gupta et al., 2022 in the Introduction and Discussion sections). Instead, our model predicts categorical flow states, which we then compared to NWM discharge values and the limited available discharge observations. Because we have very few observed discharge measurements (as described in Section 3.3 l. 402-405), we cannot calibrate or validate hydrologic model estimates of discharge. Consequently, linking image classifications to potentially uncertain modeled discharge estimates would not provide meaningful results in this case.

Discussion

The discussion successfully interprets the results, acknowledges limitations, and explores the wider implications of the work. The sections on unique site features and extensibility are particularly thoughtful and elevate the manuscript beyond a simple methods paper.

1. You rightly note that temporal correlation of flow states could be used for further quality control. Could a simple Hidden Markov Model be a natural next step to incorporate this temporal dependency?
Thank you for this question. There are a number of different methods that might be used to incorporate temporal dependency. Because evaluation of the suitability of methods for this is beyond the scope of this paper, we declined to list potential methods to avoid confusion.

2. How does the performance of your logistic regression model (91% accuracy) compare, in your view, to the potential trade-offs of using a more complex but data-hungry model like a CNN for this specific task? That could be a good paragraph in the discussion, showing the drawbacks and positives sides of using a parsimonious but effective model.
In Section 4.1, l. 447, we describe that our method “prioritizes a simple, accurate, site-specific model that requires minimal manually-labeled training data.” We also note in Section 4.1, l. 463-465, that a CNN may be more suitable for images classified as ‘low water’ or ‘high water’. In accordance with this suggestion, as well as prior feedback from both you and Reviewer #1,

we propose including a discussion of potential applications of our model, in conjunction with CNN models, within a new appendix focused on model transferability (see above).

3. In the biggening (abstract and objectives) you state the method is "transferable." Could you specify the primary condition for transferability (e.g., the presence of a staff plate or a consistent field of view of the streambed)? Maybe a bit of discussion on the costs of this equipment set up would also help the reader to have an idea of how much it would cost. Perhaps that could be included in the methods?!
In accordance with suggestions made by you and Reviewer #1, we proposed editing relevant sections of the Discussion and adding a new appendix that specifically addresses transferability (see above). Furthermore, with respect to the cost of setup, we propose adding a brief text appendix that details our setup costs, and propose referencing that appendix in a revised Section 4.5, l. 605-606 where we mention the low cost of our method: “The (2025) cost of field cameras similar to those used in this study range from €100 - €300. The mounting accessories and telemetry equipment add about €100, though costs may vary depending on specific hardware choices. The telemetry system enables near real-time image access but requires an annual renewal fee of about €70. Total installation costs can vary considerably depending on site accessibility and labor expenses. Sites typically require biannual servicing to maintain a consistent power supply, clear vegetation that could obstruct the camera’s view of the stream, and to perform routine maintenance.”

4. The discussion on the potential for subsurface flow bypassing the PEC site is fascinating. Could this hypothesis be further supported by comparing the water level in Lake Mendocino with dry/wet periods at PEC?
We agree that this hypothesis could certainly be explored further, and observations at PEC and other sites in the same watershed suggest that exploration may be worthwhile. However, that exploration is outside the scope of this work.

5. You mention that your code is transferable. What is the minimum number of manually labeled images you would estimate is necessary to achieve reasonable performance at a new, similar site?
Based on suggestions made by you and Reviewer #1, we proposed adding a new appendix that specifically addresses transferability, including discussion of performance with a minimal number of manually labeled images (see above).

6. In the context of climate change, how might your method help in detecting shifts in the timing of flow initiation and cessation in IRES, which is a key impact of warming Temperatures?
As described in Section 4.1, l. 457-459, this method is well-suited for IRES since it focuses on water presence or absence. Therefore, it is well-suited to identify the timing of flow initiation and cessation in IRES. We propose making this point more explicit by adding the following to this same Discussion section: “Our method could be expanded to detect IRES-relevant states including wet streambeds, isolated pools, standing water, trickling water, snow, or ice.” We discuss the context of climate change in the Introduction, and describe how our method

supports IRES monitoring under climate change in the proposed new conclusion and transferability appendix.

7. Have you considered referencing studies that have successfully implemented low-cost, image-based methods in data-scarce regions? For instance,
- Noto, S., Tauro, F., Petroselli, A., Apollonio, C., Botter, G., & Grimaldi, S. (2022). Low-cost stage-camera system for continuous water-level monitoring in ephemeral streams. Hydrological Sciences Journal, 67(9), 1439–1448. https://doi.org/10.1080/02626667.2022.2079415
Yes, we cite this work.

- Rodrigues, R. M., Braga, B. B., & Costa, C. A. G. (2025). Efficiency in river discharge measurement: combining Chiu's method with particle image velocimetry techniques. RBRH, 30, e31.
We have not cited this work, but we propose referencing it in the introduction section discussing image-based approaches to monitoring IRES.
These papers could broaden the perspective on transferability and cost-effectiveness.

Conclusion

The conclusion effectively summarizes the main findings and their significance. It compellingly argues for the role of this low-cost method in improving IRES monitoring and, consequently, water management in a changing climate.

1. Could the conclusion more explicitly state the single most important recommendation for water managers seeking to implement this method?

In response to this and feedback from Reviewer #1, we propose including a new Conclusion section that summarizes the overall contribution (see below), and incorporates discussion of practical recommendations:

"This work demonstrates that a simple machine learning algorithm can classify timelapse field camera images to identify no, low, or high water levels in IRES, providing a low-cost, transferable method for monitoring water occurrence in these sparsely observed systems. Given the prevalence of ungaged IRES, field cameras and image classification offer a practical approach to improving understanding of their role in climate-impacted freshwater systems. For example, the FIRO program at Lake Mendocino (Fig. A1) currently uses streamflow observations from EFR to inform reservoir inflow models. As climate change is expected to increase drying in IRES, unmonitored contributions from tributaries such as Perry Creek (Appendix 3) could affect reservoir storage. Thus, as the FIRO program expands, field cameras and image classification may offer a cost-effective approach to integrating information on the presence and magnitude of IRES contributions.

This approach can also support monitoring of critical habitats, including tributaries where salmon passage depends on streamflow connectivity threatened by drought and water diversions (see e.g., Scott River, 2025). Installing cameras at tributary confluences could inform targeted habitat restoration. More broadly, formally recognizing IRES as integral to river systems can incentivize monitoring and protect them from degradation due to climate change, mining, and urban development (Acuña et al., 2014). The 2023 exclusion of ephemeral streams from U.S. Clean Water Act protections highlights the vulnerability of IRES and the importance of cost-effective monitoring approaches like ours for understanding the impacts and effectiveness of water management efforts.

We conclude by offering practical recommendations for implementing our method. Cameras should be installed along IRES reaches that are important for monitoring water management objectives (e.g., fish passage, drought contingency planning). Installations should be in stable locations with clear views of the streambed and minimal vegetation interference. Consistent camera types and viewing angles should be used, as they improve the robustness of time series and the effectiveness of classification. Long-term maintenance budgets are also recommended to support sustained monitoring. Finally, this approach can be integrated with complementary methods (Gupta et al., 2022; Goodling et al., 2025) and deployed through accessible platforms such as the USGS Flow Photo Explorer (USGS, 2024) and the CrowdWater mobile application (SPOTTERON GmbH, 2025)."

2. Beyond FIRO, can you speculate on one other specific water management decision (e.g., environmental flow allocations, drought contingency planning) that would benefit from the categorical flow data your method provides?
Yes, our proposed new conclusion section discusses monitoring for salmon migration (see above).

3. While the method is low-cost, the conclusion could acknowledge the ongoing costs and challenges of maintaining field cameras in harsh environments as a consideration for long-term deployment.
Yes, our proposed new conclusion section recommends including a budget for site maintenance in all long-term monitoring programs.

4. What do you see as the next critical technological or methodological advancement needed to make IRES monitoring truly scalable across vast river networks?
While the broader topic of scaling IRES monitoring approaches like ours is beyond the scope of our paper, we propose addressing this briefly at the end of our new conclusion section, and expanding on this in the new appendix (see above).

I congratulate the authors on an excellent piece of work. I hope that with these revisions, the manuscript will be an even stronger contribution to the literature.
Thank you for your thorough review. The revisions based on your feedback will undoubtedly strengthen the manuscript.