Associate Editor's Report to Authors of
"Informative autonomous sampling of oceanographic variables using joint excursion sets"

General comment:

Your manuscript has been reviewed by three experts and me. One reviewer did not provide much feedback, but the other two did. All reviewers found the problem considered by the authors interesting, and the paper well-organized and (grammatically) well-written. But the latter two reviewers, plus myself, believe that the paper has a number of weaknesses. Some of these have to do with what seems to be the authors' inadequate consideration of the audience of *Annals of Applied Statistics*. Some of the terminology the authors use (without definition) will be unfamiliar to most readers. Furthermore, without additional explanation very few readers will understand why it is important to estimate the boundary of a plume, or what kind of movements the AUVs can make (hence what sort of designs you are restricted to considering).

The following are my more specific comments.

Substantive issues:

1. General: The inferential problem considered is the paper is that of estimating the "boundary" of a plume. However, the authors never define exactly what a "boundary" is. Such a definition should be provided, as well as some discussion of why determining the location of a boundary is so important. Why is it not sufficient to create a map of predictions (with an accompanying map of prediction error variances) for the variable(s) under study throughout the region of interest (a problem for which there is a huge literature on sampling design)?

2. Page 8, second paragraph of Section 4.1: The assumptions of separability and isotropy, and even stationarity, are not really necessary for the methodological development of Sections 4–6. I suggest that you wait to make these assumptions until you get to the simulation studies and example.

3. Page 9, second paragraph of Section 4.2: It has been assumed here that the measurement errors for temperature and salinity at any given location are uncorrelated. Is this a reasonable assumption? This could easily be checked by taking replicate measurements at each site.

4. General: Related to the previous point, I do not understand why sampling locations are being restricted to a regular grid. In fact, it seems unwise because it is well known (see, for example, Chapters 9 and 22 of *Handbook of Environmental and Ecological Statistics*, 2019, CRC Press) that the quality of inferences for spatial fields can be improved substantially by taking some observations very close to others.

5. Page 13, last line: Specifically, what was the design? We aren't even told what the sample size (n_y) was used in these calculations.

6. Page 18, first paragraph of Section 7.1: The numerical values of the regression parameters given here are not meaningful without knowing the units of "West."

7. Page 24, first paragraph of Fig. 8: Please define "the modeled quadratic form of the residuals" more precisely. Perhaps a mathematical expression is needed for clarity.

Minor issues:

- Page 3, line 19: Change "bi-variate" to "bivariate". The same change is needed on the next-to-last line of Page 12.
- Page 9, three lines after (10): I presume that all off-diagonal elements of $\mathbf{R}$ are zeros, but this should be stated explicitly.
- Page 19, fourth line from bottom: Change "as also" to "as well as"
- Page 30, second and third references listed: "et al." is listed after the authors' names for each of these two references, but actually there are no additional authors beyond the two listed.