General comments

The experimental data on WDV in combined wave-current flows is most valuable for scientists and engineers working with nature-based coastal defences. The presented data is unique, relevant, complete, well-structured, and uses state-of-the-art methodologies. The use of force transducers is viewed as an accurate way to measure forces on vegetation, which is not yet commonly used in other WDV experiments. However, several suggestions are made to improve the manuscript accompanying the data. I am confident that a revised manuscript will be an impactful article in ESSD.

Major specific comments

Comment 1:

(Section 1) The perspective chosen in the introduction is in my opinion less suitable for ESSD. I would expect that the present dataset is presented from a background of earlier experimental datasets (Anderson & Smith, 2014; Augustin et al., 2009; Bradley & Houser, 2009; Foster-Martinez et al., 2018; Jadhav et al., 2013; Koftis et al., 2013; Mendez & Losada, 2004; Ozeren et al., 2014; Sánchez-González et al., 2011; van Veelen et al., 2020, and many others). The introduction insufficiently addresses why this new dataset is complementary to existing data sets. Instead, the authors approach the introduction from a perspective of why combined wave-current flows are relevant. While this is certainly important, this takes up a lot of space at the cost of relating the present work to prior experimental research.

Comment 2:

(Section 2) Which type of vegetation is represented by the artificial vegetation used in the experiments? The authors refer mostly to the generic term of coastal vegetation.
However, seagrass, salt marsh vegetation and mangroves have very different vegetation properties (dimensions, flexibility). Can the authors clarify how their artificial vegetation compares to real vegetation? And how useful are the outcomes for coastal vegetation types not represented by the artificial vegetation? If the authors choose provide clarifications in the manuscript, I recommend that they also do so in the abstract and section 1.

Comment 3:

(Section 2) The presented experiments are specifically designed to study wave-current interactions. Figure 1 shows that the in- and outlets are at a specific locations in the water column (bottom/top). This raises the question on how the velocity profile is distributed over the water column. Can the authors comment on the velocity profile? Was this controlled? And how was this verified?

Comment 4:

(full manuscript) The E14 experiment has been previously published by the authors in Hu et al. (2014), hereafter Hu14. Compared to their previous paper, they have added an extra set of experiments (E19) and they have now published their data free to use. However, the partial reuse leads to several issues:

- The manuscript does not set out how the current manuscript should be read complementary to Hu14.
- The introduction reads “A subsequent laboratory study revealed that following current can either increase or decrease WDV (Hu et al. 2014)” (R48), followed by “contradicting conclusions” (R50, also R17) and “relevant datasets are still scarce” (R53). This suggests to the reader that you will address the conflicting views. However, the majority of the experiments presented (according to table B1), is identical to the ones that you seem to challenge in the introduction. It is no surprise that the results in this manuscript match the results in Hu14. I recommend that the authors approach this manuscript as complementary to Hu14 rather than a test to of Hu14.
- The description refers to Hu14 to explain certain parts of the experimental setup (R104, R161, R167). I recommend that the experimental setup can be understood stand-alone from the present manuscript.
- As a positive comment, section 3 is concise and clearly shows the added value of E19 to E14.

Comment 5:

(full manuscript) The referencing in this manuscript can be improved. While the selected literature is relevant, it could often be cited a more appropriate location. Some examples:

- R176: The work by Pujol et al. (2013) is very relevant for this manuscript, but why is it only mentioned at a trivial equation and not in section 3.2, which addresses a topic
extensively covered by Pujol et al. (2013).
- R182: Why not Keulegan & Carpenter (1958) for the KC-number?
- R147,152: would it be possible to reference a more time-independent source? Also these references are not listed at the back of the manuscript.
- R378: This reference suggests that the conclusions of these experiments are made by the authors.
- R380: why only provide reference for root and leaf impacts, but not for flexible vegetation. There are many to choose from.
- The introduction would benefit from more references to relevant experimental studies (see also comment 1).

**Minor specific comments**

Comment 6:

(R17) “Previous studies” is rather vague. Could the authors be more specific on the type of studies?

Comment 7:

(R58) “CD was introduced” feels off to me as it was already a common parameter in other scientific fields. I recommend rephrasing this sentence.

Comment 8:

(Figure 1)

- It might be helpful to mark FT2 as not working in the diagram.
- The green-red colouring may be an issue for those with colour blindness.
- The meaning of the asterisks is not immediately clear.
- Small z?

Comment 9:

(section 2.1)

Can the authors comment on how wave reflection was addressed? Especially for E14 this may be relevant as no wave absorber appears to be present.

Comment 10:

(R144) The water depth in E19 is shallow. Were the authors able to maintain regular
waves under these conditions? The velocity time series in Figure 5 appears to contain some nonlinear components. If not, what would be the implications? And was any additional impact of bottom friction observed?

Comment 11:

(R156)

I recommend devoting a separate section to this paragraph, e.g. 2.3 wave conditions.

Comment 12:

(R166) The middle water depth is chosen as representative for the depth-averaged velocity. However, Figure 4 shows that velocities are attenuated inside the canopy and amplified above the canopy. These appear to be two separate regimes that cannot easily be captured at a fixed water depth. The main use for $u$ is the derivation of the drag force. With this in mind, I would think that a measurement that is representative for the canopy regime is most appropriate. Have the authors considered using a velocity halfway the vegetation height rather than water depth? It seems that the current definition would not be sufficient if the canopy height is below half water depth.

Comment 13:

(Eq 4,5) Do $u_{\text{max}}$ and $u_{\text{min}}$ include the contribution of the mean current?

Comment 14:

(Eq 5) The KC-number is based on measured velocity. How can the velocity be estimated when no measurements are available? It will be interesting to shortly address this in Section 4.1, where potential applications are discussed.

Comment 15:

(R195) Did you find any difference in turbulence between the two canopy types? Or was this not measured?

Comment 16:

(Figure 2)

It is confusing to me why subplots a-c are fitted but d-f are not. I recommend making this consistent within the figure, and within the methodology as a whole.
Comment 17:
(Figures 2,3,6,C1) A confidence interval and/or a measure of goodness-of-fit would be valuable. This may also concern individual measurement points where relevant. I noticed that you already included goodness-of-fit measures in your published database. However, I believe that it will also benefit readers to get an impression of the variability of the data from the manuscript.

Comment 18:
(Section 3.2) While the velocity and force data are part of the database, section 3.2 feels out of place in its current form. This section is not introduced anywhere before in the manuscript. Those focus fully on WDV and CD. I recommend that this is addressed.

Comment 19:
(Section 3.2, Table B1). It is unclear during which tests the a full velocity profile was measured. As far as I understand, this was only done for a subset of the experiments. Could you please specify which tests had the full velocity profile measured?

Comment 20:
(Figure 4)
- I recommend leaving the no veg and current-veg cases out for clarity.
- Please add the meaning of each dashed line to the caption.

Comment 21:
(Figure 5)
Could the authors clarify in the caption which timeseries are measured and which are calculated?

Comment 22:
(R361, R380-386) The CD relation of E14 was also applied in van Veelen et al. (2021, Coast. Eng.), which is a use case of expanding the CD-relation such that it is useful for flexible vegetation too. This may be relevant, especially as the authors discuss expansions towards more realistic vegetation conditions in R380-R386.
Comment 23:

(Section 4.2) The title of section 4.2 does not reflect the contents well in my opinion. As a result, the main message of this section remains unclear to me.

**Technical corrections**

- R10: is the institute of reference 5 correct?
- R30: systems has
- R161: possible influenced
- R190: Hpw and Hcw is
- R197: the accompany currents
- R364: lasty (capitalization)
- R372: in adequate