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Interactive comment on "Fractal analysis of urban catchments and their representation in semi-distributed models: imperviousness and sewer system" *by* Auguste Gires et al.

Anonymous Referee #2

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General comments:

The topic of this manuscript is potentially interesting for publication in HESS. Yet, I identify a number of weakness in the manuscript, at different levels, as explained below. The whole manuscript should be carefully revised.

1. Spatial patterns analysis using non-linear or fractal analysis is getting more attention in a wide range of disciplines. This manuscript applies this approach to characterizing patterns relevant for urban pluvial drainage. The idea behind this manuscript can be seen as an interesting contribution to the hydrological modelling of rainfall-runoff processes in urban basins, and therefore to the hydraulic design and operation of sewer systems. I encourage the idea of using scaling, (multi)fractal tools to support hydrolog-

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ical studies and appreciate the attempt in this study to conduct this application in urban hydrology.

2. However, I see several weaknesses in this work, and they are of different origin, e.g.: i) technical issues; ii) description of data and models; iii) presentation and interpretation of results; iv) insight into the relation between the mathematical exercise of determining the fractal dimension for a geometrical entity and the physical entity that it represents (in this case vital for the hydrological/engineering application pursued); v) conclusions that are not supported by evidence in the text.

3. In general, the manuscript is written only fairly clearly and there are typos. The overall structure is standard, but there are parts that could be improved. Across the manuscript many parts/paragraphs run in new and new information without a clear main point/focus, which makes the reading difficult and weaknesses the message in the text. The selection of figures, and the figures themselves, should be revised (see below). The literature review could be improved; it misses to include important contributions in the literature related to using scaling frameworks in hydrology.

Specific comments:

4. Data: - The study areas are not adequately described bearing in mind the type of study and application. Key properties of the drainage areas and networks (e.g., sewer density, impervious surfaces, slopes) are also not clearly described, the methods used for its characterization are unclear. - The definition given for the catchment slope (Table 1, footnote 2) suggests that the information in the GIS and available Digital Elevation Models is not fully explored, since it would to be expected that such tools would lead to a much better estimate of catchment slope than the extremely simple one described. - After referring to 10 pilot/study areas, there are figures where some of those areas are unexpectedly split in different parts (e.g. W/E, N/S), and there are situations when simply some of the announced basins are ignored. This should be clarified. - There are basic elements missing: for example, the definition of sub-catchment (SC) is crucial for

the study. - The reference to the basins is not consistent; for example, in the legend of Figure 10, the reference to “Centrum” is inadequate.

5. Methodology: - This section needs to be much improved; there are many confusing sentences. - Proper credit to the methods introduced should be given. For example, a reference for Universal Multifractals is not given (page 5, line 18).

6. Results and discussion: - One technical issue is related to the interpretation of the results obtained with the box-counting method to find the (central, here) fractal dimension. The box-counting plots should be more adequately discussed, including an interpretation for the straight sections in the plot, the cross over between them, the critical scales – the material presented should be carefully checked for misinterpretations. When applying the box-counting method to complex drainage systems (natural or/and artificial drains), there are different issues that need to be better addressed: i) issues related to the resolution of the data set (for example, that only drains above a certain size are displayed) that are known to interfere with the pattern “recognized” by the box-counting algorithm; ii) the space filling character of complex drainage channels that are randomly distributed over the plane and therefore lead to a fractal dimension 2 (all boxes are filled above a critical size; thus, a (box-counting) power-law dependence with a fractal dimension D=2 contains the same level of information as a scale invariant regime with D<2 ?); iii) what would the interpretation be for D in the limit size -> 0? iv) how would the scaling range change with map resolution? Overall, what is the information indeed useful for this urban hydrology application?

- In my view, there is now no clear evidence in the manuscript of the added value of this framework. The authors are not successful in taking the exercise of applying the box-counting method to a geometrical entity - that is highly dependent on the definition of the urban drainage system itself - beyond it: more discussion should be provided that clarify the usefulness of the approach as a practical tool for characterizing (urban, periurban) drainage networks. Examples should provide insight; the ones given are, in my view, not discussed at length. The physical properties of the drainage networks

should be given and this is important for the sake of comparability; the fact that pilot areas/case studies have different origins might imply differences that need to be well established. Although I understand that the authors might have found important to give many different examples (different urban environments, from different countries), for full understanding of the results the physical interpretation should be allowed – and this implies a good definition of the systems. Per se, a list of fractal dimensions estimated for not well understood drainage systems is of very limited interest to the community.

- It also does not help the manuscript the selection of figures included therein. The selection of figures should be better though. The present selection of figures does not allow the reader to follow the analysis and characterization for one of the study areas. Every time (i.e. for different figure's content, methods) different cases (study areas) are show, in an apparently ad hoc mix, which does not allow one to attempt reasoning about one single drainage system, and therefore to interpret results.

- For comparison purposes, there is clearly a mismatch between having aprox. 2000 SC or 9 SC in different study areas; this should have been carefully discussed because there is no evidence in the manuscript on the origin of such huge difference that can be thought as resulting from differences in the geomorphology of the basins or simply from the definition of the study unit (SC). The practical implications need more attention.

-There is no attempt to validate if the assessment of imperviousness proposed in this work is more successful than the approach routinely used in the model.

-It should be clarified if the same hydrodynamic model was used for all study areas, or if there are different models being applied. In fact, such model(s) is(are) not described. The type of model would highlight the relative importance of the input, such as the description of the fractal dimension of the drainage network or the fractal dimension of the impervious soil cover spatial pattern. It is not discussed at what resolution should the hydrodynamic model (or models) run for adequate output. The evaluation of the model performance is also not carried out or discussed.

-For the hydrologic/engineering application in mind, qualitative assessments (e.g., page 4, line 5-6, “slight difference” or “more pronounced”) should be converted to quantitative assessments.

- There is (apparently) too much reliance on the magnitude of the regression coefficient (r2) and not enough attempt to explain the “meaning” of some results (e.g., page 10, line 4).

-The reference (last paragraph in section 4) to green roof tops is marginal to this work, this reference is not well embedded in the text.

8. Conclusions: - This section includes statements that lack support from results and discussion, as they are presented in the manuscript. - Despite the attempt to include this work in a framework of applied hydrology, in my opinion the work is not successful in providing insight into the application of the scaling analysis proposed in the context of the physical characteristics of an urban pluvial sewer system, which are key in engineering applications.

9. References: - There is mismatch between the references’ list and the works cited in the text. - Many references in the list are not complete, there is missing information about the publication (e.g. journal name). - When several references are given in the text, they are not organized alphabetically for chronologically.

Technical corrections

(not exhaustive list) - Page 2, line 18. Should be: Lovejoy and Mandelbrot, 1985. - Page 2, line 28: the reference to fractal analysis of soil features is not relevant for this study. - Page 4, line 5: revise “. . .the values of imperviousness are uniform. . .”; also, Fig. 2 shows “proportion of imperviousness” - Page 4, line 15-16: - Table 1: Numbers used to call footnotes can be confused with powers – change notation. Within each column, there should be consistency in the way the values are presented. - Tables 1 and 2: The two tables organization should match, to allow easy cross reading. -

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Figure 2: the distortion introduced by the different scales used in the 2-directional axes is confusing, for the sake of comparability between the different panels in this figure. Also, the reference in the caption that “The axes correspond to the number of 2 m pixels” might be confusing: does this mean that the distance along one axis between 0 and 100 corresponds to 200 m? One expects to have distances along the axes. - Figures 3 to 6: Units for the axes are missing. - Figure 6: in the caption, The reference is to Eq. 1. As one expects distances to be represented in the upper panels, it is unexpected to have the bottom panels referring to distances (1024 m) that are larger than the (origin) data represented above (?). The same occurs in other figures. Maps should provide distances. - Captions of the figures need to provide understanding of the material plotted. For example, the fractal dimension in Fig. 7 was estimated for which scaling range?

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