Referee comment on Dupuits et al., 2017, Using Graphs to find economically optimal safety targets for multiple lines of flood defences.

General comment:

Before reading this referee comment, the reader must be aware of the fact that the authors of this paper actively asked me to referee their paper. I thank them for this opportunity and making me aware of this and previous papers. I have had a meeting with two authors of this paper to discuss my first impressions. This referee comment benefitted from the insight the authors provided me in this meeting.

In this general comment I will state that this paper copies earlier work. No proper references are made to this work. Hence, this paper doesn't meet minimal scientific standards. I will provide references to plenty published reports and articles to support my claim. In the Netherlands, many involved experts, including many full professors at various Dutch universities (which wrote and published referee reports on this earlier work or supported the development of earlier work) can be asked to confirm my claim.

The main claim by the authors that a shortest path/dynamic programming approach (previously presented and discusses by other authors) to solve economic optimal dike heightening is 'advantageous' needs to be elaborated a lot more. Many previously stated and published arguments against dynamic programming are not mentioned. Moreover, the scientific ambition of this paper is not clear. Furthermore, no calculations are presented by Dupuits et al. (2017) to support their claim. I will provide arguments for this claim.

This paper copies the approach by Zwaneveld & Verweij (2014a) for finding an optimal configuration for interdependent lines of flood defences. In Zwaneveld (2012; section 1.1; in 2014 provided to the authors by email) several approaches are discusses to solve this model. This modelling approach by Zwaneveld & Verweij (2014a, 2014b) including graph-based (shortest path or minimum cost flow problems) solution approaches and the preferred ILP approach was earlier copied and described by Yuceoglu (2015, Chapter 5: Safe Dike heights in the Netherlands). This PhD-thesis builds on the work by Zwaneveld and Verweij (2014a , 2014b) and discusses graph –based algorithms to solve the so-called Diqe-Opt model (see later for a discussion of this model). Zwaneveld & Verweij (2014a) identify several algorithms to solve the problem both to proven optimality and to solve the problem heuristically (with the advantage that computing times remain limited). Zwaneveld & verweij92014a0 aplly their model to ral world problem instances to support crucial Cabinet decisions for the Netherlands. I apply and explain an algorithm to solve the problem to proven optimality. Their algorithms requires hardyly any programmng efforts and little solution time ('less than one minute or so'). The ideas to solve the problem heuristically were not implemented in practice due to the fact that the algorithm to solve the problem to proven optimality was superior according to Zwaneveld and Verweij (2014a).

In line with Brekelmans et al (2012), Eijgenraam et al, (2010; in revised version published as: Eijgenraam et al. 2016) a dynamic programming (read: shortest path approach) is identified in Zwaneveld (2012) as one of the options to solve the model. Zwaneveld and Verweij (2014a, Annex A, Figure A; 2014b) and Zwaneveld (2012) contribute to these earlier papers by identifying that the dike optimization problem can be seen as a graph based problem. For example, Zwaneveld and Verweij

(2014b, p.29) state that the the dike optimization model ‘satisfies the most fundamental of all network flow problems (Ahuja et al., 1993), namely the minimum cost flow model’. This point was missed by earlier published work and also by Dupuits et al. 2017. Dupuits et al. (2017) present a graph based representation which is identical to the graph representation of Zwaneveld and Verweij (2014a) and Zwaneveld and Verweij (2014b). For example, compare Figure A in Zwaneveld and Verweij (2014b, p. 29) with the figures of Dupuits et al. 2017. They are clearly (almost) identical.

Unfortunately, this paper by Dupuits et al. (2017) does not clearly refer to these previous papers and reports which they copy and build upon. In my opinion, this paper needs a thorough revision to correctly and clearly refer to the work of previous mentioned authors to meet minimal scientific standards.

This paper states in the introduction that “However, existing cost-benefit analyses tend to focus on flood defences with independent lines of (Kind 2014), or are not readily generically applicable (e.g. Zwaneveld and Verweij (2014a). Therefore, the aim of this paper is to find general, computationally efficient approach… with arbitrary number of lines”

The authors do not mention the fact that Zwaneveld and Verweij (2014a, including background papers), Bos and Zwaneveld (2012) and Zwaneveld and Verweij (2016, UK CPB discussion paper on previous Dutch reports) do present for the first time a generic , computationally approach to assess dependent flood defense systems whit arbitrary number of flood defense lines. Moreover, these authors do apply their approach in a real world environment and under time pressure to obtain economic optimal flood protection policy measure for the Lake IJssel region (including many dependent dike rings and barrier dams). Dupuits et al. (2017) are aware of this approach and solution method since they apply it in section 4.3. Zwaneveld and Verweij kindly provided Dupuits et al. (2017) with their programming code and data to allow scientific reuse of their earlier work.

Although this approach is not yet published in in a UK written scientific journal (the authors are working on it, see Zwaneveld and Verweij , 2016)), the scientific quality had been assessed by two different committees with professors and other experts (see Donders et al., 2013; Van Ierland et al., 2014). This was due to the fact that very important hydrological and economic policy decisions are based upon the application of the Diqe-Opt model (in Bos and Zwaneveld 2012; Zwaneveld and Verweij 2014a). The Ministry of Infrastructure and Environment had to be sure about the quality of the Diqe-Opt model and the two reports. Documents are published on the UK and Dutch based CPB-website. A few documents are also presented to the Dutch Parliament: they can also be found at the website of the Dutch Parliament.

The latter 2014-committee of four professors at Dutch universities conclude in Dutch: “Het CPB heeft met deze studie een belangrijke stap vooruit gezet in het onderzoek naar waterveiligheid. Het is een indrukwekkende studie waarin zeer veel hydrologische en economische kennis op een prachtige manier wordt samengebracht. Met name het meenemen van afhankelijkheden in de overstromingskansen van dijken is een belangrijke innovatie. Het ontwikkelde model Diqe-Opt is een vernieuwend en zeer nuttig instrument” [UK translation: “CPB has made with this study an important step forward in the search for water safety. It is an impressive study in which very many hydrological and economic knowledge is combined in a wonderful way. In particular, the inclusion of dependencies in the flood dikes of opportunities is an important innovation. The developed model Diqe-Opt is an innovative and very useful tool””. Note that Zwaneveld and Verweij (2014) name their generally applicable method: Diqe-Opt. Due to the generally applicable of the Diqe-Opt approach the model is by request from the Ministry of Infrastructure and the Environment being transferred to hydrological consultancy company Deltares. Deltares can use the model as long as proper references are made to earlier CPB-work by Zwaneveld and Verweij. The Dutch institutions setting of the CPB (employer of Zwaneveld and Verweij) prohibit these activities by CPB. This innovative Diqe-Opt approach was also recognized by a recent Dutch handbook on water safety (ENW, 2016, Literature list to Chapter 4).

Hence, Dupuits et al. (2017) should state that they copy the Diqe-Opt model by Zwaneveld and Verweij (2014a , 2014b) instead that the “aim to find an … approach”. Proper and clear references are missing towards this earlier work in the starting sections of this paper.

Dupuits et al (2017) present a shortest path algorithm to the problem definition as presented by Zwaneveld and Verweij (2014a). For the cases presented in section 4.1 and 4,2, almost identical and probably more efficient dynamic programming approaches (shortest path approaches) are presented in Eijgenraam et al. (2010) and Brekelmans et al. (2012). Proper references should be made to this earlier work. I do not see the value added by the shortest path approach of section 2 and section3 in addition to these two papers.

Zwaneveld and Verweij (2014b , Annex A) present an alternative approach for these two cases in section 4.1 and 4,2 based upon an ILP-model. Applying this approach requires no programming effort whatsoever since user friendly software can be used to model the problem. No efforts are required to solve the stated model since standard LP/IP solvers can be used. Note that an LP-solver is at present a plug in tool in Microsoft Excel and modelling languages as CPLEX, GAMS and AIMMS are easily available. Free and easy to use solvers are easily available.

The authors should mention in section 4.1 and 4.2 the use of these competitive and in some cases almost identical approach and should compare it with their approach. This comparison was already presented in Zwaneveld and Verweij (2014b, Annex A). They provide arguments and conclude that an ILP-approach is by far more preferable than a dynamic programming approach. Dupuits et al. (2017) should – as a minimum- discuss this work by Zwaneveld and Verweij (2014b).

Furthermore, it is unclear why Dupuits et al 2017 conclude that an dynamical programming approach is ‘relatively easy’. From a discussion with the authors, I learned that their intention and scientific ambition is to present a heuristic approach to solve the model by Zwaneveld & Verweij (2014a). Although heuristic approaches are presented in Zwaneveld & Verweij (2014a, 2016), these were not yet implemented. The advantages of this heuristic approach is to reduce calculation time with the disadvantage that a non-optimal solution is found. Furthermore, to help the authors, some persons may prefer a dynamic programming approach over an ILP approach. I also learned that a dynamic programming approach is easier to understand for many people than an ILP-approach. Therefore, Zwaneveld & Verweij(2014a, 2014b) always present their model as a graph problem and then introduce that they prefer to solve this graph problem to optimality by using an ILP-approach.

However, the ambition by the authors, as I learned from personal communication with them, doesn’t meet their statement on page 5 of Dupuits et al. (2017) : “For our applications, we did not come up with a heuristic function, which reduces the choice of a graph alfgorithm to either Dijkstra or UCS". Hence, this requires more explanation by the authors. I cannot see why both claims are valid.

For the case presented in section 4.3 proper references should be made that this is a simplified version of Zwaneveld and Verweij (2014a). Again, an explicit discussion of the pro's and cons of solving the model by Zwaneveld and Verweij (2014b) by using an ILP-approach and their approach should be presented. Zwaneveld and Verweij (2014a) and Zwaneveld (2012) do present such a comparison and they conclude that the ILP-solution approach is superior to dynamic programming (or: shortest path) for real-life applications. This earlier assessment should be presented. Why do Dupuits et al. (2017) conclude the opposite? Why do the authors present only 'toy problems instances'which can easily be solved using existing approaches?

Zwaneveld and Verweij (2014a) and Bos and Zwaneveld (2012) were capable of solving very large real-time problem instances given very short research leadtime and research capacity. Moreover, setting up a dynamic programming algorithm is requires very substantial programming efforts as is clear from section 2 and 3 from this paper. The approach by Zwaneveld and Verweij (2014a) requires only the code "SOLVE DIQE-OPT MODEL USING CPLEX" to obtain the proven optimal solution. Hence, the claim that a dynamic programming is 'more easy' than a ILP-approach by Zwaneveld and Verweij (2014a) is not valid or – at best - not properly motivated in my opinion.

Specific comments

Section 2.1:

The representation of the problems copies the approach by Zwaneveld and Verweij (2014a) and Zwaneveld and Verweij (2014b). Especially the graphs in this section are strikingly identical to Figure A from Zwaneveld and Verweij (2014a) See also almost identical figures in Yuceoglu (2015). Also references should also be made to Dynamic programming approach by Eijgenraam et al. (2010) and Brekelman et al. (2012) which seems to be mathematically identical. Proper references are missing to this earlier work. Dupuits et al. (2017) should clearly state that they copy previous work.

The cases presented in paragraph 4.1 (single flood defense) and 4.2 (independent lines of defences) can be solved by the dynamic programming (or shortest path approach) which is extensively discussed in Eijgenraam et al (2010) (a revised version of this paper was published as Eijgenraam et al 2016) and briefly discussed in Brekelmans et al. (2012). Zwaneveld & Verweij (2014b, 'paper under revise and resubmit') make the point in Annex A that these shortest path problems can be much easier solved to proven optimality using LP-relaxation or IP-model formulation. All this should be mentioned.

Section 2 and 3:

The presented approach is basically the well known shortest path algorithm. The discussion should can be deleted or removed to an electronic companion . I do not see any scientific added value in comparison with earlier work by Brekelmans et al (2012), Eijgenraam et al (2010) and the large literature of shortest path problems and dynamic programming. I personally prefer to refer to the well-written UK –based Wikipedia discussion of the subject(see Zwaneveld, 2012).

The claim that repetitiveness of vertices is in most cases incorrect. Note that vertex 12 represent a later year than vertex 7 (see Figure 9) . Due to yearly increases of economic growth and flood probabilities al risk calculation has to be calculated again. Hence, vertex 7 and 12 are not identical and no calculation time is saved.

Note that Brekelmans et al. (2012, p.1343) state that a simple 'homogenous case can be conveniently solved using dynamic programming. Unfortunately, this is not possible for the nonhomogenous cases, because the state space explodes…. We show how the nonhomogeneous diek height problem can be solved as a MINLP-problem.' A more or less similar statement by Brekelmans et al. (2012, p. 1345): "Unfortunately, the state space grows too large ….which implies that the dynamic programming approach is not applicable". This is the – very good- reason why Brekelmans et al. (2012) prefer their MINLP approach.

Dupuits et al (2017) do not properly discuss this exploding problem, i.e. exploding state spaces and exponential calculation time of all sorts in the problem size. Nor do they refer to these previously mentioned authors which did identify this problem before.

From the paper I have got the impression that they apply a shortest path algorithm to solve the problem to proven optimality given – theoretically- computing time which are exponential in the problem size. From personal communication with the authors, I did get a different impression, namely that they aim to present a non-optimal solution approach given limited computing time. The authors should clarify that ambition.

A more or less similar remark holds for the algorithm. From the paper I get the impression that they implemented the algorithm themselves to find a proven optimal solution. From personal communication, I did get the impression that they use standard plug-in heuristic procedures to solve the graph. Hence, no programming effort whatsoever is required. The latter would make their approach of course more easy to use but also make their algorithm less innovative. The authors should clarify their ambition.

Section 5:

The authors state that Kind (2014) proposes an linear programming approach. This is incorrect. Kind (2014) doesn't propose any method. He uses the approach by Brekelmans et al. (2012), which is an MINLP-approach. The IP-approach was proposed by Zwaneveld and Verweij (2014b, 'paper in revise and resubmit' to an academic journal). An IP-approach is not identical to a linear programming approach.

The claim that the application area is roughly similar to Zwaneveld and Verweij (2014a) is incorrect. The application area is completely identical and copied from Zwaneveld and Verweij (2014a). Furthermore, reference should be made that dynamic programming/shortest approaches of cases in section 4.1 and 4.2 to Eijgenraam et al (2010) and Brekelmans et al. (2012). And to heuristic ideas (and some attempts) to solve the dike height problem in previous work by Eijgenraam, Brekelmans and Den Hertog and Zwaneveld & Verweij (2014a, 2014b, 2016)

Lines 17-22 Page 17: The authors should mentioned that fact that the approach by Zwaneveld and Verweij (2014b) was especially develop to include other flood defence systems than height-dependent dikes. The fact that Dupuits et al (2017) can also include these approaches is a direct consequence of the fact that they copy the approach by Zwaneveld and Verweij (2014a, 2014b) and, therefore, both have identical application areas. The Diqe-Opt model was already used to asses many of these alternative flood defence systems in Bos and Zwaneveld (2012) and Zwaneveld and Verweij (2014a). See also Donders et al. (2013) and van Ierland et al. (2014)

Section 6

The authors claim that it is an advantage that 'their approach do not need pre-calculate risk which linear programming approaches do'. However, the IP-approach by Zwaneveld and Verweij(2014a) – again this is NOT a linear programming approach – indeed does require risk estimates in a pre-processing step. In addition, stating that risk calculation can be performed 'on the fly' is complete impractical in a real-world setting of Zwaneveld & Verweij (2014a), Bos and Zwaneveld (2012), Brekelmans et al (2012) and Eijgenraam et al. (2016), since it requires in general running hydrological models. Hence, the approach by Dupuits et al. (2017) requires in each iteration to consult a hydrological experts to run their model and to report the result back. Doing these calculations in a pre-processing step as advocated by Zwaneveld and Verweij (2014a and 2014b) and Bos and Zwaneveld (2012) has very significant practical advantages. For real-world instances, risk calculation were no problem whatsoever in the approach by Zwaneveld & Verweij (2014a, 2014b), Brekelmans et al. (2012) and Eijgenraam et al. (2016).This argumentation is missing in this section.

Finally, the claim by Depuits et al. (2017) that their approach requires less risk calculation than the graph-based ILP—approach by Zwaneveld and Verweij(2014a) is not supported by calculations.

Technical corrections

References:

Please state that Zwaneveld and Verweij (2014a) CPB Communication 14 january 2014. Please provide the internetlink: https://www.cpb.nl/publicatie/economisch-optimale-waterveiligheid-het-ijsselmeergebied

PLeas state that Zwaneveld and Verweij (2014b) is not a technical report but. CPB Discussion paper 277. Please provide the internetlink: https://www.cpb.nl/en/publication/safe-dike-heights-at-minimal-costs-an-integer-programming-approach

Due to the merits of the above given comments, other technical and additional specific comments have to wait for a revised version of this paper.

Additional references to be added to the paper.

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