We thank the referee for their time, and for their detailed comments and suggestions for our manuscript. Our responses, wherever appropriate, are given below.

---

GENERAL COMMENTS

This manuscript aims at improving the SCS-CN method and the estimation of the corresponding parameters when rainfall runoff data are available. The proposed approach is based on the hypothesis posed by Soulis and Valiantzas (2012) that “the observed correlation between the calculated CN value and the rainfall depth in a watershed reflects the effect of the inevitable presence of soil-cover complex spatial variability along watersheds”. Based on this hypothesis they present a novel and really interesting analysis of the effects of this heterogeneity on initial abstraction and on CN. It includes nice theoretical justifications, and good examples. In a nutshell, their analysis provides another more general perspective extending the work of Soulis and Valiantzas (2012, 2013) by considering separately the spatial variability of Ia and CN, which are linked in the previous studies. Finally, based on their analysis they introduce two modifications of the SCS-CN method considering the spatial variability.

The topic of this study is certainly interesting and relevant to the journal of Hydrology and Earth System Science, because the SCS-CN is the most widely used runoff estimation method, while it is based on previous studies published at this journal. The study is very well written and really easy to understand. The language is excellent and the presentation also of good quality. The theoretical part is also interesting and well written and the interpretations and the methodology scientifically sound.

---

**Referee’s Comment**

However, there are also some important weaknesses that should be addressed. The first important weakness is related to the citation of an unpublished paper. The citation of studies that are not published yet and thus are not available to the readers isn’t helpful. This is not a significant problem at the first instance (Page 2, Line 6), where there is a general reference on "ways to account for the temporal variation of CN, each with its own advantages and shortcomings (Santikari and Murdoch, 2018)". In this instance the citation on the unpublished work should be removed and some citations on studies dealing with this issue should be added. However, in the second instance (Page 23, Lines 14-15) an unpublished paper is used to support the validity of the proposed approach and the performance of the proposed modifications ["Application of these modified models to data from real watersheds is discussed by Santikari and Murdoch (2018)"]. Any information concerning real watersheds examples should be presented in this paper (the part related to the proposed approach). Otherwise the readers will not be able to have a clear picture about the validity and the performance of the proposed approach. Furthermore, there are practical problems in citing unpublished papers. Are you certain that the future paper will be accepted and that it is going to be published before the final publication of this paper?

**Authors' Response**

We understand the referee's concern about the citation of the companion paper which is under review and therefore unavailable to the readers. To overcome this problem, we now cited my Ph.D. dissertation (Santikari, 2017) wherever a citation to the companion paper appears. My dissertation is published and easily accessible to the readers, and it has a chapter that is identical to the companion paper. We also provided a link to the dissertation under references in this response, and we hope it will also be helpful to the referees.

We also feel that the citations to the companion paper should be left in place. The work presented in the companion paper is a logical extension to the work presented in the current paper, so it is beneficial to the readers to be aware of that. However, we will update the reference to the companion paper as "submitted", "under review", or "accepted" based on its status before the final publication of the current paper.

Referee #1 also suggested describing the results from the application of the models to real watersheds. We now added a subsection to briefly discuss these results as follows (copied from Response to Referee #1):

### 6.2. Application to Real Watersheds

The models were also evaluated using rainfall-runoff observations from 9 real watersheds located in different parts of the world (Santikari, 2017; Santikari and Murdoch, 2018). Models' ability to predict the observed runoff was assessed using $\mathrm{NSE}_Q$. Results show that in all the watersheds VIMs performed better than CMs but the difference in performance, $\Delta\mathrm{NSE}_Q$, varied across the watersheds. Between VIMλ and CM0.2, $\Delta\mathrm{NSE}_Q < 0.05$ in one watershed, $0.05 \leq \Delta\mathrm{NSE}_Q < 0.7$ in 6 watersheds, and $\Delta\mathrm{NSE}_Q \geq 0.7$ in 2 watersheds. Between VIMλ and CMλ, $\Delta\mathrm{NSE}_Q < 0.05$ in 3 watersheds, $0.05 \leq \Delta\mathrm{NSE}_Q < 0.1$ in 4 watersheds, and $\Delta\mathrm{NSE}_Q \geq 0.1$ in 2 watersheds. Based on their performance, the models can be arranged from the best to the worst as VIMλ > VIMS > CMλ > CM0.2, which is consistent with results from their application to the synthetic watershed.

A detailed description of the study areas, application, and results (Santikari, 2017) is impractical because that would make the current paper significantly lengthy. The readers can refer to Santikari (2017), which describes the case studies in more detail than could be presented in the current paper.

The current paper, even without a description of application to real watersheds, makes an important contribution to our understanding of CN methodology by providing a theoretical proof that heterogeneity leads to the variation of CN parameters with $P$. The paper also throws light on the long-known but unexplained phenomenon of the calibrated $\lambda$ being much smaller than 0.2, and introduces the novel concept of storage transfer.

---

**Referee's Comment**

A second weakness, is related to the use of solely synthetic data for the evaluation of the proposed approach and of the proposed modifications. (Page 23, Lines 12-14: "The reason for using a synthetic watershed here is that the heterogeneity can be precisely defined and used to evaluate the predictions of heterogeneity by the lumped parameter models. In real watersheds the heterogeneity has to be determined by calibration, and there can be non-uniqueness when multiple HRUs are present.") I agree that using a virtual watershed allows the study and the evaluation of specific aspects of your approach in a controlled and accurate environment. The virtual watershed and the synthetic data follow the logic of your base hypothesis and your theoretical analysis. However, this hypothesis and this analysis, even if they are rational, they are not selfevident. The reason for using also real watersheds examples is that only in this way you may show that your hypothesis is sound, that it is able to describe the behaviour of real watersheds, and that the method actually works. By using only virtual data generated based on your hypothesis (which, I agree, seems reasonable) you cannot support your hypothesis and validate your methodology.

**Authors' Response**

We agree that ultimately the new methodology is judged by whether it describes the behavior of real watersheds, and by whether it works i.e. whether it produces better results than the conventional models when applied to real watersheds. The paper in its current form shows that the new models meet both criteria.

1. The motivation for deriving the new methodology are the observations from real watersheds in our study (Figure 2), and from real watersheds in other studies (D'Asaro and Grillone 2012; Hawkins, 1993), which show that CN variation with $P$ is common. So the variation of $I_a$, which is inversely related to CN (eq. 10), with $P$ is also a commonly observed behavior in real watersheds. Therefore, the new models which allow for the variation of $I_a$ with $P$ describe the behavior of real watersheds better than the conventional models, which assume that $I_a$ is constant.

2. The results from real watersheds, summarized in the newly added Subsection 6.2, show that the new models predict runoff better than the conventional models. This proves that the new

methodology works better even with real watersheds.

**Referee's Comment**

A final weakness concerns the literature review, which is limited and incomplete. For example:

1. Page 1, Lines 26-28: "One of the most popular techniques used for this purpose is the Curve Number method, which has been in use for more than half a century (Soil Conservation Service, 1956)." You should add some citations supporting this statement.

**Authors' Response**

The following citations have been added:

D'Asaro and Grillone, 2012; Hawkins et al., 2008; Kent, 1968; Ponce and Hawkins, 1996; Rallison and Miller, 1982; Soil Conservation Service, 1972

**Referee's Comment**

2. Page 2, Lines 2-3: "CN also varies with the magnitude and spatiotemporal distribution of rainfall." You should add some citations supporting this statement.

**Authors' Response**

Citations have been added as follows:

CN also varies with the magnitude (D'Asaro and Grillone, 2012; Hawkins, 1993; Hjelmfelt et al., 2001) and spatiotemporal distribution of rainfall (Hawkins et al., 2008; Van Mullem, 1997).

**Referee's Comment**

3. Page 2, Lines 3-5: “When heterogeneity is known at sufficient detail, CN variation can be accounted by using a distributed parameter model. Otherwise this approach can introduce more parameters than can be reliably estimated from the available data, and cause large uncertainties 5 in the predicted runoff.” You should add the citations supporting this statement, for example Soulis and Valiantzas (2012, 2013) referred later in the manuscript.

**Authors’ Response**

Citations have been added as follows:

When heterogeneity is known at sufficient detail, CN variation can be accounted by using a distributed parameter model, e.g. SWAT (Gassman et al., 2007). Otherwise this approach can introduce more parameters than can be reliably estimated from the available data (Soulis and Valiantzas, 2013), and can potentially cause large uncertainties in the predicted runoff.

---

**Referee’s Comment**

4. Page 2, Lines 5-9: “CN variation with the distribution of rainfall is usually ignored.” and “CN method is most commonly applied as an event-scale lumped parameter model, which is simple but also limited in its ability to account for the variations of CN. This diminishes the accuracy of its runoff predictions.” You should add some citations supporting these two statements (E.g. Grove et al. (1998); Soulis and Valianzas, 2012).

**Authors’ Response**

Citations have been added as follows:

CN variation with the distribution of rainfall is usually ignored (Hawkins et al., 2008). CN method is most commonly applied as an event-scale lumped parameter model, which is simple but also limited in its ability to account for the variations of CN. This diminishes the accuracy of its runoff predictions (e.g.

Soulis and Valianzas, 2012).

---

**Referee's Comment**

5. Page 4, Lines 26: There more studies providing important information on this issue e.g. Hjelmfeld et al. (2001) and Soulis et al., (2009)

**Authors' Response**

We added a sentence at Page 4, Line 26 as:

This behavior was also observed in several previous studies (D'Asaro and Grillone, 2012; Hawkins, 1993; Hjelmfelt et al., 2001; Soulis et al., 2009), and it appears to be a common phenomenon. Hawkins (1993) and D'Asaro and Grillone (2012) evaluated approximately 100 watersheds in a wide range of settings, and in 75% of the watersheds they observed…

---

**Referee's Comment**

6. Page 7, Lines 10-17: the studies of Soulis and Valianzas, (2012, 2013) should be also mentioned at this point.

**Authors' Response**

Citations have been added for the following sentences:

This is because when CN is constant, $Q$ may be underestimated for small $P$ and overestimated for large $P$ [e.g. Soulis and Valianzas (2012, 2013)]. This problem can be addressed either by treating CN as a function of $P$, e.g. asymptotic fitting (Hawkins, 1993), or by using a distributed modeling approach that accounts for heterogeneity in sufficient detail, e.g. SWAT (Gassman et al., 2007) or Soulis and Valianzas (2013).

---

## Referee's Comment

More important, you should consider previous studies dealing with the same issue with similar or different approaches. I have in mind for example two really important studies by Steenhuis et al., 1995 and its continuation by Tilahun et al., 2016 that investigate the variation of Ia using the concept of "Variable source runoff areas" and propose a very attractive approach to consider it in the SCS-CN method. You should discuss these studies.

## Authors' Response

Although Steenhuis et al. (1995) present an interesting interpretation of the CN method, we think that their approach is fundamentally different to ours, and falls beyond the scope of the topic covered in our manuscript for the following reasons:

1. A fundamental assumption made in the approach used by Steenhuis et al. (1995) is that for a given patch of land, $dQ/dP_e$ is either zero or unity, i.e. either it produces no runoff or 100% of the rainfall becomes runoff. According to this assumption, Figure 1 in our manuscript would look like a step function. However, the assumption in the original CN method, as well as in our manuscript, is that $dQ/dP_e$ increases with $P_e$ and varies as $0 \leq dQ/dP_e \leq 1$. The assumption by Steenhuis et al. (1995) is applicable only in watersheds with variable source runoff areas.

2. At the watershed scale, however, $dQ/dP_e$ is allowed to vary between zero and unity in the approach used by Steenhuis et al. (1995). This is a result of recasting the definition of $S$ from being parameter that describes how the vertical storage is the soil profile is filled to a parameter that describes how saturated areas develop in the horizontal dimension. In other words, in the original CN method, $S$ describes how $F$ or $Q$ vary with $P$ at a given location or for an entire watershed. In the method by Steenhuis et al. (1995), $S$ describes how the areal extent of saturation and the resultant $Q$ vary with $P$ only at the watershed scale.

3. It is impossible to handle watershed heterogeneity using the method by Steenhuis et al. (1995). This is because in each HRU, $I_{ai} \geq 0$ but $F_i = 0$ and $S_i = 0$. This stems from the fundamental assumption of their method (see point 1 above). Before the onset of runoff, $P < I_{ai}$, i.e. the initial abstraction has not yet been overcome. But as soon as the runoff starts, all the additional rainfall becomes runoff, and this can only happen if $S_i = 0$. Thus, $S$ only exists at the watershed scale but vanishes at the HRU scale. Moreover, in their method $I_a$ and $S$ are constants at the watershed scale for given set of antecedent conditions. The topic covered our manuscript, however, is focused on how to include heterogeneity in the CN method, and in our approach $I_a$ and $S$ vary spatially and with $P$.

---

**Referee's Comment**

You should also state more clearly that the proposed approach is based on the hypothesis posed by Soulis and Valiantzas (2012) that "the observed correlation between the calculated CN value and the rainfall depth in a watershed reflects the effect of the inevitable presence of soil-cover complex spatial variability along watersheds".

**Authors' Response**

We would like to clarify here that when we discovered the standard behavior in our watersheds (Figure 2) and sought an explanation, we were unaware of the work done by Soulis and Valiantzas (2012, 2013). Our analysis followed the logic described in Section-2 and lead to the theoretical proof that heterogeneity causes standard behavior. We came across the work of Soulis and Valiantzas (2012) long after we finished our analysis, and we were surprised to see that they reached the same conclusion using an empirical analysis. We did give them the credit in the introduction because they were the first to discover the link between heterogeneity and standard behavior. However, it would be inaccurate for us to say that our approach was based on the hypothesis by Soulis and Valiantzas (2012, 2013).

---

## Referee's Comment

You should also make it clear and add a citation to Soulis and Valiantzas (2012, 2013) in Page 15, Lines 9-10: "Therefore, it is probably more appropriate to refer to any "CN decreasing with P" trend as standard behavior, because it is caused by the inevitable presence of heterogeneity in a watershed."

## Authors' Response

To be accurate with the citations, the above sentence was split and recast as follows:

…supporting the hypothesis that $I_{aW} = I_{aF}$. Thus, as also concluded by Soulis and Valiantzas (2012, 2013), the observed *complacent* and *standard behaviors* are caused by the inevitable presence of heterogeneity in a watershed. Moreover, *complacent behavior* appears to be a special case of *standard behavior* (Soulis and Valiantzas, 2012), where observations from larger rainfalls are unavailable. Therefore, it is probably more appropriate to refer to any "CN decreasing with *P*" trend as *standard behavior*.

---

## Referee's Comment

Finally, you should discuss your results in comparison with other approaches/methods especially at the final section "6.2. Model Suitability".

## Authors' Response

We compared variable $I_a$ models with conventional lumped parameter models in the results from both synthetic and real watersheds. Distributed parameter modeling is another approach that could be compared with modified models. As per referee's suggestion, we briefly discussed this under "6.4. Model Suitability" as:

When the watershed heterogeneity is known in great detail such that the number of calibrated parameters $\leq 3$, a distributed modeling approach [e.g. SWAT (Gassman et al., 2007) or Soulis and Valianzas (2013)] may be preferable over the variable $I_a$ models. A distributed parameter model has advantages similar to the variable $I_a$ models over the conventional models. It would inherently account for the variation of CN method's parameters spatially and with $P$. It would also avoid the false prediction of zero runoffs in small events because HRUs with larger CNs, which generate runoff even in small events, are explicitly considered. When the heterogeneity is unknown, however, the number of calibrated parameters (for values of $\mathrm{CN}_i$ and $a_i$) in a distributed model with $n$ HRUs is $2n$-1. This number would increase further if values of $\lambda_i$ are also calibrated. Therefore, when the number of calibrated parameters > 3, application of a variable $I_a$ model should be considered.

---

**Referee's Comment**

You should also mention other limitations such as the compatibility of the resulted CN values with standard method. For example, CN values with different $\lambda$ values are not compatible.

**Authors' Response**

We added the following paragraph to "6.5. Model Limitations"

Another limitation of VIMs is that the CN values calculated using eqs. (5) or (10) are incompatible with the standard CN values (NRCS, 2003; USDA, 1986) derived using CM0.2. However, this limitation is not unique to VIMs because any method, including CMλ, which involves an altered relationship between $I_a$ and $S$ (i.e. $\lambda \neq 0.2$) leads to CN values that are incompatible with those derived from CM0.2. Given that (i) CM0.2 is a poor predictor of runoff, and (ii) the evidence contradicts $\lambda$ = 0.2 (Baltas et al., 2007; D'Asaro and Grillone, 2012; Shi et al., 2009; Woodward et al., 2003), the above-mentioned limitation is an acceptable compromise.

---

**Referee's Comment**

SPECIFIC COMMENTS

-Page 5, Line 28: You should mention what is presented in the figure.

**Authors' Response**

We mentioned what is presented in the figure on the same page (Lines 15 – 18, and 20 – 21) and in the figure caption (Page 6, Lines 1 – 3). We would appreciate more clarity on what the referee meant by the above comment.

---

**Referee's Comment**

-Figure 2, legend: "(see Santikari and Murdoch (2018) for study area description)" You should avoid citing unpublished work (see previous comments). You should provide at least a short description of the case study.

**Authors' Response**

We now cited my Ph.D. dissertation (Santikari, 2017), which describes the study area in more detail than could be presented in the current paper.

---

**Referee's Comment**

-Page 7, Lines 23-24: How Ia values in Figure 2 were calculated?

**Authors' Response**

Method of calculation is now mentioned as:

The calculated values of $I_a$ [using eqs. (3), (6), and $\lambda = 0.2$] for watersheds…

---

**Referee's Comment**

-Please avoid using plural in parameters symbols. For example, in “Iais” I was initially confused if s was for plural or part of the symbol. You may use other explosion such as “Iai values”.

**Authors’ Response**

We replaced “$I_{ai}$s” with “$I_{ai}$ values” or “values of $I_{ai}$”. “$Q_i$s”, “CN$_i$s” were also replaced similarly.

---

**Referee’s Comment**

-Page 9, Lines 9-11: As it is explained in Soulis and Valiantzas (2012), the reason is the non-linear form of the SCS-CN formula. So, the average of the results is not equal with the result using average value of the parameters.

**Authors’ Response**

It is true that non-linear form causes the runoff from averaging CN to be different than the runoff from averaging $Q$. However, the purpose of lines 9-11 is to mention that averaging $Q$ is a better approach than averaging CN, rather than to explain why they give different results.

---

**Referee’s Comment**

-Page 9, Line 13: “to be”

**Authors’ Response**

Grammatical error corrected

---

**Referee’s Comment**

-Page 9, "2.2. Ia in a Heterogeneous Watershed": It should be mentioned that the following justification is valid in the case that each subarea is directly connected with the drainage network. This is a logical assumption in most cases, especially when there is a dense drainage network, however, it is still an assumption.

**Authors' Response**

The referee makes a good point. However, we would like to point out that all CN models inherently make this assumption when the watershed is discretized. There is no provision in the CN method to handle overland flow crossing subarea boundaries. More process based models, e.g. GSSHA (Downer and Ogden, 2004), may be required to model such interactions between the subareas. In any case, for more clarity, we modified the sentence at page 9, line 19 as:

If each land use type is assumed to be directly connected to the drainage network, the number of land use types contributing to the runoff, in other words the runoff contributing area, increases with rainfall.

---

**Referee's Comment**

-Page 34, Lines 9-14: You could use additional evaluation criteria e.g. the relative NSE (rNSE) and the NSE with logarithmic values (lnNSE) to reduce the problem of the NSE sensitivity to extreme values (see Krause et al., 2005).

**Authors' Response**

We thank the referee for this information. It is impossible to calculate lnNSE because the conventional models predicted zero-runoffs in small events (page 24, lines 23 – 25). However, we calculated rNSE and added the results to Table 3 and the newly added Table 4 (see Response to Referee #1). The updated tables are attached at the end of this response. Large storms still had greater influence on rNSE than small storms because the runoff from large storms was an order of magnitude greater than the average runoff. So the rNSE values are still close to unity but they varied slightly between the models. The relative performance of the models remains unchanged.

---

**Referee's Comment**

-"Model Suitability" section: It would be interesting if you could at least discuss (if it is not possible to compare) with the Soulis and Valiantzas 2012 and 2013 methods, which provided the base for this study.

**Authors' Response**

As we mentioned (page 7, lines 3-7), the methods used by Soulis and Valiantzas (2012, 2013) are equivalent to a distributed parameter CN model. We described in a response above that we added a paragraph under "6.4. Model Suitability" to discuss modified models in comparison with distributed parameter models.

---

Conclusively, based on the above comments, I believe that this paper is really interesting and worth being published in case that the authors are able to address the above issues.

REFERENCES

Grove, M., Harbor, J., and Engel, B.: Composite vs. Distributed curve numbers: Effects on estimates of storm runoff depths, J. Am. Water Resour. As., 34, 1015–1023, 1998.

Hjelmfelt Jr., A. T., Woodward, D. A., Conaway, G., Plummer, A., Quan, Q. D., Van Mullen, J., Hawkins, R. H., and Rietz, D.: Curve numbers, recent developments, in: Proc. of the 29th Congress of the Int. As. for Hydraul. Res., Beijing, China (CD ROM), 17–21 September, 2001.

Krause, P., Boyle, D.P., Base, F., 2005. Comparison of different efficiency criteria for hydrological model assessment. Adv. Geosci. 5, 89–97.

Soulis, K. X., Valiantzas, J. D., Dercas, N., and Londra P. A.: Analysis of the runoff generation mechanism for the investigation of the SCS-CN method applicability to a partial area experimental watershed, Hydrol. Earth Syst. Sc. 13, 605–615, doi:10.5194/hess-13-605-2009, 2009.

Soulis K.X. and Valiantzas J.D. 2012. SCS-CN parameter determination using rainfall-runoff data in heterogeneous watersheds - the two-CN system approach. Hydrol. Earth Syst. Sci. 16:1001-1015. doi: 10.5194/hess-16-1001-2012.

Soulis K.X. and Valiantzas J.D. 2013. Identification of the SCS-CN parameter spatial distribution using rainfall-runoff data in heterogeneous watersheds. Water Resour. Manage. 27:1737-1749. doi: 10.1007/s11269-012-0082-5.

Steenhuis, T.S., Winchell, M., Rossing, J., Zollweg, J.A., and Walter, M.F. (1995). SCS runoff equation revisited for variable-source runoff areas, J. Irrig. Drain. Eng. ASCE, 121, 234–238,.

Tilahun, S.A., Ayana, E.K., Guzman, C.D., Dagnew, D.C., Zegeye, A.D., Tebebu, T.Y., Steenhuis, T.S. (2016). Revisiting storm runoff processes in the upper Blue Nile basin: The Debre Mawi watershed. Catena, 143, 47-56. doi:10.1016/j.catena.2016.03.029

---

## RFERENCES

Baltas E.A., Dervos N.A. and Mimikou M.A. 2007. Technical note: Determination of the SCS initial abstraction ratio in an experimental watershed in Greece. Hydrol. Earth Syst. Sci. 11:1825-1829. doi: 10.5194/hess-11-1825-2007.

D’Asaro F. and Grillone G. 2012. Empirical investigation of Curve Number Method parameters in the Mediterranean area. J. Hydrol. Eng. 17:1141-1152. doi: 10.1061/(ASCE)HE.1943-5584.0000570.

Downer C. W. and Ogden F. L. 2004. GSSHA: Model to Simulate Diverse Stream Flow Producing Processes. J. Hydrologic Engrg. 9(3): 161. doi: 10.1061/(ASCE)1084-0699(2004)9:3(161).

Gassman P.W., Reyes M.R., Green C.H. and Arnold J.G. 2007. The Soil and Water Assessment Tool - Historical development applications, and future research directions. Trans. ASABE 50:1211-1240.

Hawkins R. 1993. Asymptotic determination of runoff Curve Numbers from data. J. Irrig. Drain. Eng. 119:334-345. doi: 10.1061/(ASCE)0733-9437(1993)119:2(334).

Hawkins R., Ward T., Woodward D. and Van Mullem J. 2008. Curve Number hydrology: State of practice. American Society of Civil Engineers, Reston, VA.

Kent, K. M. (1968). "A method for estimating volume and rate of runoff in small watersheds." Technical Paper 149, USDA SCS, Washington, D.C.

Ponce V. and Hawkins R. 1996. Runoff Curve Number: Has it reached maturity? J. Hydrol. Eng. 1:11-19. doi: 10.1061/(ASCE)1084-0699(1996)1:1(11).

Rallison R.E. and Miller N. 1982. Past, present, and future SCS runoff procedure. p. 353-364. In V.P. Singh (ed.) Proceedings of the international symposium on rainfall runoff modeling: Rainfall-runoff relationship, Mississippi State University, Mississippi. May 18-21, 1981 Water Resources Publications, Littleton, Colorado.

Santikari V. P. 2017. Accounting for Spatiotemporal Variations of Curve Number using Variable Initial Abstraction and Antecedent Moisture. Chapter 5. *In* Evaluating Effects of Construction-Related Land Use Change on Streamflow and Water Quality (Doctoral dissertation). https://tigerprints.clemson.edu/all_dissertations/1942

Shi Z., Chen L., Fang N., Qin D. and Cai C. 2009. Research on the SCS-CN initial abstraction ratio using rainfall-runoff event analysis in the Three Gorges Area, China. Catena 77:1-7. doi: http://dx.doi.org/10.1016/j.catena.2008.11.006.

Soil Conservation Service. 1972. National engineering handbook: Section 4, Hydrology. Chapter 10. SCS, Washington, D.C.

USDA. 1986. Urban hydrology for small watersheds. TR-55. USDA-NRCS, Washington, DC.

Van Mullem, J. A. 1997. Precipitation variability and curve numbers. *In* Managing Water: Coping with Scarcity and Abundance. Proceedings of the 27th Congress of the International Association of Hydraulic Research, San Francisco, CA. August 1997. 518-523. Published by American Society of Civil Engineers, New York.

Woodward D., Hawkins R., Jiang R., Hjelmfelt J., A., Van Mullem J. and Quan Q. 2003. Runoff curve number method: Examination of the initial abstraction ratio. p. 1-10. In World water & environmental resources congress, Philadelphia, Pennsylvania, United States. June 23-26, 2003 American Society of Civil Engineers.

**Table 1.** The performance of lumped parameter CN models that were calibrated to the runoff data generated using a distributed CN model for two cases of a synthetic watershed with the storage distribution shown in Error! Reference source not found.. SEE, $I_a$, and $S$ are in mm. (SEE: Standard Error of Estimate, PB: Percent Bias, NSE: Nash-Sutcliffe Efficiency parameter)

| Lumped Model | Distributed Model: $\lambda_i = 0.2$, $I_{aT} = 22$, $I_{a,\max} = 40$, $S_\infty = 112$ | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | rNSE$_Q$ | SEE$_Q$ | PB$_Q$ | NSE$_{Ia}$ | NSE$_S$ | NSE$_{Q50}$ | PB$_{Q50}$ | $\lambda_W$ | $I_a$ or $I_{aT}$ | $I_{a,\max}$ | $S$ or $S_\infty$ |
| CM0.2 | 0.93 | 0.91 | 12.6 | -1.8 | -13 | -2.9 | 100 | 0.20 | 19 | - | 97 |
| CMλ | 0.94 | 0.37 | 5.4 | 0.0 | -26 | -2.9 | 100 | 0.07 | 9 | - | 132 |
| VIMS | 0.97 | 0.13 | 2.1 | 0.2 | -22 | -0.8 | 71 | - | 12 | 64 | 121 |
| VIMλ | 1.00 | 0.06 | 0.2 | 0.4 | -3 | 0.9 | 16 | 0.09 | 11 | 43 | 124 |

| Lumped Model | Distributed Model: $\lambda_i = 0.5$, $I_{aT} = 56$, $I_{a,\max} = 100$, $S_\infty = 112$ | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | rNSE$_Q$ | SEE$_Q$ | PB$_Q$ | NSE$_{Ia}$ | NSE$_S$ | NSE$_{Q50}$ | PB$_{Q50}$ | $\lambda_W$ | $I_a$ or $I_{aT}$ | $I_{a,\max}$ | $S$ or $S_\infty$ |
| CM0.2 | 0.93 | 0.81 | 18.8 | -1.4 | -102 | -2.9 | 100 | 0.20 | 31 | - | 155 |
| CMλ | 0.94 | 0.66 | 13.6 | -0.3 | -166 | -2.9 | 100 | 0.11 | 21 | - | 197 |
| VIMS | 0.96 | 0.26 | 6.9 | 0.7 | -83 | -1.8 | 87 | - | 37 | 130 | 140 |
| VIMλ | 0.99 | 0.13 | 1.6 | 0.7 | -9 | 0.6 | 33 | 0.21 | 33 | 96 | 153 |

**Table 4.** Performance of the models for the cases of $\lambda_i$ = 0.2 and 0.5, when the degree of heterogeneity in the synthetic watershed (Table 2) was increased by doubling the values of $S_i$ for HRUs 3 and 4.

| Lumped Model | $\lambda_i = 0.2$ | | $\lambda_i = 0.5$ | |
|---|---|---|---|---|
| | $\text{rNSE}_Q$ | $\text{SEE}_Q$ | $\text{rNSE}_Q$ | $\text{SEE}_Q$ |
| CM0.2 | 0.92 | 1.54 | 0.92 | 1.30 |
| CMλ | 0.95 | 0.19 | 0.94 | 0.38 |
| VIMS | 0.97 | 0.12 | 0.96 | 0.25 |
| VIMλ | 1.00 | 0.06 | 1.00 | 0.12 |