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Trois énigmes de modélisation hydrologique

Three riddles in hydrological modeling

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by Vazken Andréassian

Outline

Résumé

Ce document présente un aperçu de mes recherches en hydrologie au cours des dix dernières années. Il est structuré en trois parties : la première est consacrée à une revue de la littérature hydrologique afin de replacer la voie dans laquelle je me situe parmi les grands courants de recherche en hydrologie; la seconde présente l'ensemble des résultats publiés et sous presse auxquels j'ai contribué, avec également une revue des résultats de thèses (défendues et en cours) que j'ai en partie encadrées; enfin la troisième partie présente mes objectifs de recherche à court, moyen et plus long terme.

Chapitre 1 : une courte revue de la modélisation hydrologique

Le premier chapitre est donc une revue de la littérature, dont l'objectif est de discuter du positionnement de l’approche suivie dans le cadre plus général des approches classiques de modélisation hydrologique. Dans ce chapitre, j'utilise délibérément le “ nous ” plutôt que le “je” quand je donne mon opinion, tant l'approche que je décris est réellement commune à toute une équipe, que j'ai rejointe en 1995 et qui s’est construite autour des travaux de Claude Michel au Cemagref. J'explique dans ce chapitre pourquoi notre démarche de modélisation se démarque assez nettement des canons de la recherche hydrologique actuelle, aussi bien pour ses bases philosophiques que pour ce qui est de son traitement des échelles de temps et d’espace.

1.1 Approches sous-tendant le développement des modèles hydrologiques

L'approche mise en œuvre dans mon équipe est *descendante* et *empirique*.

- Le terme *descendant* fait ici référence à un débat classique de méthodologie scientifique, sans doute aussi ancien que la pensée scientifique elle-même, mais qui a été clairement discuté dans le cadre de la théorie des systèmes initiée par Ludwig von Bertalanffy : *descendant* signifie ici que l'on s'intéresse tout d'abord au système hydrologique dans sa globalité, et que l'on cherche à décrire ses propriétés émergentes. Cette approche s'oppose à la démarche *ascendante* (ou mécaniste), par laquelle on s'attache à reconstituer les

propriétés d'ensemble de l'hydrosystème à partir de celles de ses éléments constitutifs.

- *Empirique* traduit la règle d'évaluation des modèles de fonctionnement des bassins versants sur lesquels travaillent les hydrologues : elle signifie que seule la capacité des modèles à reproduire le fonctionnement observé des bassins versants est prise en compte, et que l'on fait donc abstraction dans cette évaluation de notre connaissance théorique *a priori* des phénomènes physiques en jeu.

Approche descendante et démarche empirique vont à l'évidence de pair : elles correspondent très exactement aux recommandations de J.E. Nash, l'un des pionniers de la modélisation hydrologique.

1.2 Modèles distribués, semi-distribués et globaux

J'essaie ensuite d'éclairer le débat relatif à la spatialisation des modèles hydrologiques : ce débat est très actuel, dans la mesure où le développement considérable des moyens de calcul et des moyens d'observation de la terre au cours des vingt dernières années a eu pour conséquence un engouement très fort des modélisateurs pour les approches distribuées. Cependant, cet engouement n'a pas eu que des effets positifs, il a aussi ouvert la voie à des supercheries technologiques (les avions renifleurs en sont un exemple), et a repoussé au second plan le questionnement scientifique sur les justifications d'une spatialisation en modélisation hydrologique. J'essaie dans cette section de mettre un peu d'ordre dans la littérature, en proposant une classification des approches : je les regroupe en approches agrégatives, désagrégatives, comparatives et théoriques, et met en évidence les contradictions apparentes entre les résultats qu'affichent chacun des groupes.

1.3 Pas de temps de modélisation et continuité des simulations

Dans mon examen des questions liées aux échelles de temps en modélisation hydrologique, j'essaie de montrer que les désaccords apparents sont, eux aussi, étroitement liés à la philosophie de modélisation : si les approches mécanistes refusent généralement de considérer que la structure des modèles dépend du pas de temps choisi, l'approche empirique considère que la complexité du modèle doit être liée au pas de temps. Il me semble cependant que la fait que les modélisateurs mécanistes soient obligés de définir des paramètres *efficaces* est en réalité un constat d'échec, qui ouvre la voie à une vision plus souple de la paramétrisation des modèles hydrologiques, et à un dialogue entre les deux communautés.

1.4 Comment ma recherche se positionne vis à vis des débats classiques

Pour synthétiser les discussions philosophiques présentées dans ce chapitre, je reprends dans cette section les raisons qui m'ont mené à préférer l'approche descendante empirique : la recherche d'une confrontation avec des problèmes concrets et la volonté de concevoir des modèles qui puissent être aussi utiles à la société. Les intercomparaisons que nous avons effectuées dans notre groupe de recherche, fondées sur des échantillons de bassins versants très importants, nous poussent aujourd'hui à favoriser une approche globale de modélisation, fondée sur des modèles de simulation continue, aussi parcimonieux que possible, et dont la complexité soit adaptée au pas de temps d'intérêt. Ces choix ne représentent pas pour autant une solution miracle aux problèmes auxquels sont confrontés les hydrologues : les incertitudes en simulation et en prévision restent parfois importantes, et il reste un travail considérable pour améliorer les modèles hydrologiques existants. Cependant, la recherche de la parcimonie au sein des modèles empiriques permet notamment d'éviter les désagréments engendrés par la surparamétrisation des algorithmes hydrologiques.

Enfin, je crois que le fait d'être retourné aux fondements des approches de modélisation hydrologique me permet de considérer sous un jour nouveau les débats passionnés qui ont fleuri en hydrologie au cours des quarante dernières années.

Chapitre 2 : Diagnostic des modèles hydrologiques

Le deuxième chapitre est consacré à une présentation de mes principaux résultats de recherche. L'un des fils conducteurs de ces recherches a été l'exploration des limites des modes de représentation adoptés dans nos modèles, car je crois qu'il est essentiel de connaître ces limites et de garantir que nos modèles sont bien sensibles aux forçages auxquels nous les soumettons. Je couvre successivement dans ce chapitre la sensibilité des modèles aux entrées de pluie, d'évapotranspiration et de pluie spatialisée. Ensuite, je propose un diagnostic synthétique sur les problèmes scientifiques qui, à mon sens, gênent les modèles hydrologiques actuels et compromettent leurs progrès, aussi bien au plan des applications que de la théorie.

2.1 Les trois plaies de la modélisation hydrologique

Cette section me permet d'introduire ma vision des problèmes qui se dressent aujourd'hui devant les modèles hydrologiques. A mon sens, ces modèles souffrent pour la plupart :

- de *surparamétrage* (avec des hydrologues qui utilisent des structures souvent numériquement malhabiles, auxquelles ils demandent d'extraire une information parfois absente dans des séries de calage) ;
- d'un *excès de confiance* de la part de leurs concepteurs (qui les empêche d'opérer une validation exhaustive de leurs modèles et de remettre en cause régulièrement les choix qu'ils ont effectués) ;
- d'un *excès de protection*, de la part de leurs auteurs, qui limite les possibilités d'intercomparaison.

2.2 Une approche globale de l'étude de sensibilité

Cette section me sert à définir ce que j'appelle " étude de sensibilité ", en reprenant la définition élargie proposée par Saltelli et al. (2000), pour lesquels l'analyse de sensibilité est l'étude de la façon dont la variation des sorties d'un modèle peut être reliée, quantitativement ou qualitativement, aux différentes sources de variation. Il s'agit de comprendre comment le modèle dépend de l'information qui l'alimente.

2.3 Sensibilité des modèles hydrologiques aux entrées de précipitation

Cette section s'appuie sur deux publications, ainsi que sur les thèses de Ludovic Oudin (2004) et Thibault Mathevet (en cours) :

- Andréassian, V., C. Perrin, C. Michel, I. Usart-Sanchez and J. Lavabre, 2001. Impact of imperfect rainfall knowledge on the efficiency and the parameters of watershed models. Journal of Hydrology, 250 (1-4): 206-223.
- Oudin, L., Perrin, C., Mathevet, T., Andréassian, V., and Michel, C., 2005. Impact of biased and randomly corrupted inputs on the efficiency and the parameters of watershed models. Journal of Hydrology, soumis.

A l'origine des travaux sur lesquels s'appuient ces publications, on trouve dans la littérature une forte controverse entre les auteurs qui rapportent une transmission quasi-linéaire des erreurs de précipitation dans les modèles hydrologiques, et ceux qui relèvent, au contraire, une capacité des modèles à amortir ces erreurs.

Notre premier article, qui étudiait la réaction des modèles aux erreurs introduites par une mauvaise connaissance des variations spatiales des précipitations, m'avait rangé dans le second groupe, dans la mesure où j'avais mis en évidence une capacité notable des modèles pluie-débit à compenser les erreurs. Le second article, qui représente une étude très systématique des deux grands types d'erreurs, a permis d'expliquer les raisons (et les limites) de cette capacité à atténuer les erreurs.

2.4 Sensibilité des modèles hydrologiques aux données de débit

Cette section s'appuie sur une publication, ainsi que sur la thèse de Claudia Rojas Serna (en cours) :

- Perrin, C., Oudin, L., Andréassian, V. and Mathevet, T., 2004. A data resampling approach to assess parameter uncertainty in continuous watershed models. Water Resources Research, soumis.

Nous utilisons le rééchantillonnage des séries de débit (qui servent à l'optimisation des paramètres des modèles) pour montrer que si les performances en calage des modèles diminuent quand les séries de débit s'allongent, c'est le contraire qui est observé en contrôle : cela traduit l'augmentation de la robustesse des modèles sous l'effet d'un apport d'information plus exhaustif. Nous montrons également que les bassins arides sont plus sensibles que les autres, et qu'ils ont besoin de plus d'information pour être calés. Enfin, nous présentons les premiers résultats obtenus dans le cadre de la thèse de Claudia Rojas Serna, et qui offrent des perspectives très intéressantes pour le calage des modèles hydrologiques à partir d'une information hydrométrique très limitée. Ces travaux constituent une contribution originale à la décennie internationale sur les bassins versants non-jaugés, organisée sous l'égide de l'AISH.

2.5 Sensibilité des modèles hydrologiques aux entrées d'évapotranspiration

Cette section s'appuie sur cinq publications, ainsi que sur la thèse de Ludovic Oudin (2004) :

- Oudin, L., F. Hervieu, C. Michel, C. Perrin, V. Andréassian, F. Anctil, and C. Loumagne, 2005. Which potential evapotranspiration input for a lumped rainfall-runoff model? - Part 2 - Towards a simple and efficient potential evapotranspiration model for rainfall-runoff modelling. Journal of Hydrology (in press).

- Oudin, L., C. Michel, V. Andréassian, F. Anctil, and C. Loumagne, 2005. Should Bouchet's hypothesis be taken into account for estimating evapotranspiration in rainfall-runoff modeling? An assessment over 308 catchments. Hydrological Processes (in press).
- Oudin, L., V. Andréassian, C. Perrin, and F. Anctil, 2004. Locating the sources of low-pass behaviour within rainfall-runoff models. Water Resources Research, 40(11): doi:10.1029/2004WR003291.
- Andréassian, V., C. Perrin, and C. Michel, 2004. Impact of imperfect potential evapotranspiration knowledge on the efficiency and parameters of watershed models. Journal of Hydrology, 286: 19-35.
- Oudin, L., Perrin, C., Mathevet, T., Andréassian, V., and Michel, C., 2004. Impact of biased and randomly corrupted inputs on the efficiency and the parameters of watershed models. Journal of Hydrology, soumis.

Comme pour l'étude de la sensibilité des modèles hydrologiques aux précipitations (2.3), on trouve dans la littérature une controverse sur l'importance de l'information relative à l'évapotranspiration (ETP) pour les modèles hydrologiques. Mais dans le cas de l'ETP, on trouve deux facteurs de complexité supplémentaires :

- d'une part l'ETP n'est pas une variable directement mesurable mais le résultat d'un modèle. On doit donc se poser la question de la validité du modèle lui-même (car naturellement, de nombreux modèles différents existent pour calculer l'ETP). Y a-t-il un modèle mieux adapté que les autres pour évaluer la demande évaporatoire à l'échelle du bassin versant?
- d'autre part, l'ETP a un cours relativement régulier chaque année : la question de l'utilisation d'une courbe interannuelle (“ climatique ”) en lieu et place de données “ datées ” peut donc se poser, mais aucune réponse satisfaisante n'avait été apportée, notamment pour expliquer pourquoi tous les efforts déployés pour exploiter l'information la plus complète (les données datées) s'étaient soldés par des échecs.

Il y a eu deux temps dans notre réflexion : dans un premier temps, nous avons essayé et réussi à régionaliser l'ETP de Penman dans le Massif Central… mais nous nous sommes ensuite rendus compte que les modèles hydrologiques n'étaient pas sensibles à un augmentation de la qualité de l'information qui leur était fournie. Les recherches sur ce thème se sont poursuivies dans le cadre de la thèse de Ludovic Oudin, qui s'est intéressé à d'autres formulations (une trentaine au total), et a montré qu'une formulation plus simple que celle de Penman, utilisant pour toute chronique la température journalière (sous la forme d'une courbe climatique interannuelle). La radiation incidente n'y intervient que sous forme de moyenne interannuelle : elle ne dépend dans cette formule que de la date et de la latitude.

Puis, de la même façon que nous nous étions intéressés aux conséquences d'erreurs aléatoires et systématiques sur les précipitations, nous avons traité le cas de l'ETP. Nous avons retrouvé et décrit une capacité à filtrer les erreurs de haute fréquence, et nous pensons qu'il s'agit là d'une propriété intrinsèque des hydrosystèmes, et non pas d'un artefact lié au modèle.

2.6 Sensibilité des modèles hydrologiques à une connaissance distribuée des entrées de pluie

Cette section s'appuie sur une publication, ainsi que sur le DEA d'Audrey Oddos (2002) :

- Andréassian, V., Oddos, A., Michel, C., Anctil, F., Perrin, C. and Loumagne, C., 2004b. Impact of spatial aggregation of inputs and parameters on the efficiency of rainfall-runoff models: a theoretical study using chimera watersheds. Water Resources Research, 40(5): W05209, doi: 10.1029/2003WR002854.

Si la nécessité de prendre en compte l'hétérogénéité des forçages atmosphériques et des propriétés de surface des bassins versants est un leitmotiv récurrent de l'hydrologie moderne, bien rares sont ceux qui se sont donné la peine de réfléchir à la hiérarchisation des sources d'hétérogénéité et à la sensibilité des nos représentations hydrologiques à la fourniture de données spatialisées.

Toutes les études que nous avions réalisées pour essayer de démontrer l'avantage du distribué sur le global s'étant soldées par un ex-aequo désespérant, nous avons décidé de tenter de forcer le trait en comparant les deux approches de modélisation sur des bassins versants extrêmement hétérogènes. Pour cela, nous avons proposé la notion de “ bassins versants chimères ”, dans lesquels on associe deux sous bassins versants de taille proche mais d'origine géographique très variable : on traite la somme des débits des deux composantes comme le débit d'un nouveau bassin virtuel, et on utilise notre connaissance des paramètres hydrologiques de chacune des composantes pour tester une variété de solutions de spatialisation (distribution des paramètres et des entrées de pluie, ou globalisation complète, ou globalisation limitée aux entrées de pluie). La conclusion s'est avérée assez surprenante, car nous avons montré que l'avantage était nettement à la spatialisation du forçage, celui de la paramétrisation des sous-bassins ayant un intérêt nettement moindre. D'autre part, on a pu vérifier sur les bassins chimères qu'il fallait impérativement une hétérogénéité forte (que certains qualifieraient de sur-naturelle) pour pouvoir établir l'avantage d'une modélisation distribuée sur une modélisation globale.

2.7 Un diagnostic de la sensibilité des modèles hydrologiques

Afin de pouvoir porter un diagnostic relativement synthétique sur la sensibilité des modèles hydrologiques, il nous a fallu tout d'abord dépasser les contradictions (parfois les controverses) de la littérature. Ceci nous a mené à réfléchir aux différentes façons d'approcher l'analyse de sensibilité, et à classer ces dernières en deux classes : les démarches *statiques* (qui étudient les conséquences de perturbation sans laisser au modèle la possibilité de se réadapter avec un nouveau calage), et les démarches *dynamiques* (qui laissent le modèle se recaler après perturbation des entrées).

L'approche dynamique, que nous avons utilisée, nous semble être la seule capable de fournir des résultats utiles de façon opérationnelle. Elle démontre la large capacité d'adaptation des modèles hydrologiques, et notamment du modèle GR4J, qui au travers de sa fonction d'échange souterrains, arrive à compenser des biais d'estimation de l'ETP et des précipitations, d'une façon bien plus efficace que les autres modèles testés.

Chapitre 3 : Perspectives de Recherche

3.1 Questions hydrologiques sur mon horizon scientifique ?

Le troisième chapitre donne une présentation rapide de mes futurs objectifs de recherche, que j'organise autour de trois thèmes : les questions d'hydrologie appliquée (avec la prévision des crues et des étiages, et l'hydrologie nivale) ; les questions d'hydrologie théorique (avec la modélisation d'ensemble, les bassins versants non jaugés, et l'étude de la variabilité naturelle du fonctionnement hydrologique des bassins versants), et enfin, ce que je qualifie de questions encore vertes et qui auront besoin de temps pour mûrir (la prédiction de l'impact hydrologique des changements d'occupation des sols et la modélisation hydrologique spatialisée).

3.2 Questions d'hydrologie appliquée

Parmi les questions d'hydrologie appliquée, je m'intéresse particulièrement à deux aspects :

- *la prévision des crues et des étiages :* dans ce domaine, beaucoup de travail reste à faire, notamment en France où le niveau des systèmes opérationnels est très faible. J'aimerais travailler sur ce sujet, notamment pour essayer de comprendre pourquoi c'est en simplifiant encore plus nos modèles que nous sommes parvenus à des systèmes robustes. Pourquoi les approches de mise à jour des modèles de simulation, qui sont très attirantes et satisfaisantes conceptuellement, sont elles mises en défaut par des modèles à mise à jour intégrée très frustres tels que les réseaux de neurones ? Quelles solutions technologiques peuvent permettre de fiabiliser les prévisions ? C'est à toutes ces questions que j'aimerais proposer une réponse.
- *l'hydrologie nivale :* ce domaine m'intéresse en raison de sa difficulté et de son apparente simplicité. *Difficulté* parce que la mesure des précipitations neigeuses étant extrêmement hasardeuse, on est presque toujours dans le domaine de l'extrapolation. *Simplicité apparente* parce que la physique de la fonte d'un seau de neige étant bien maîtrisée, on a l'impression que l'extension de l'approche mécaniste pourra se faire sans difficulté à l'échelle du bassin versant. Mais la santé insolente de l'approche empirique des degrés-jours m'incite personnellement à être extrêmement prudent pour avancer dans ce domaine.

3.3 Questions d'hydrologie théorique

Deux questions d'hydrologies théoriques me semblent particulièrement importantes dans le contexte actuel :

- *La modélisation d'ensemble :* ce domaine m'intéresse en raison des perspectives que j'y entrevois pour une amélioration du traitement des bassins versants non-jaugés. La modélisation d'ensemble offre la perspective de pouvoir s'affranchir de l'épuisante recherche de liens entre les paramètres des modèles hydrologiques et des descripteurs physiques des bassins versants, pour ne plus s'intéresser qu'aux similarités entre bassins. Si on finira toujours par se heurter au niveau limité d'information hydrologique pertinente contenue dans les descripteurs disponibles, on évitera un ensemble de problèmes numériques qui devrait, je le pense, simplifier notablement le problème.

- *L'étude des variabilités du comportement hydrologique :* l'accent est mis aujourd'hui en hydrologie sur l'étude des conséquences d'un changement climatique. Cependant, il me semble qu'on a eu ces dernières années tendance à mettre la charrue avant les bœufs, tant il est difficile de faire la différence entre la variabilité naturelle du comportement hydrologique et les changements d'origine anthropique. J'aimerais pouvoir m'appuyer sur les outils statistiques de détection de changements que j'avais mis au point au cours de ma thèse pour lancer une étude sur un échantillon très important de bassins sur lesquels j'aimerais mieux caractériser la variabilité du comportement hydrologique des bassins versants et ses déterminants.

3.4 Questions lointaines

J'ai choisi de qualifier de " lointaines " les questions que je considère comme importantes… mais embêtantes, au sens où je me sens aujourd'hui très désarmé pour les aborder. Parmi ces questions, deux me semblent essentielles :

- *La prédiction de l'impact hydrologique des changements d'occupation du sol :* ce domaine est une source de questionnements récurrents pour le public comme pour les décideurs, mais il me semble qu'il n'est pas encore possible de le traiter pour l'instant. Une étape nécessaire me semble être la démonstration de la capacité des modèles à détecter les conséquences de changements (a posteriori), ce qui est rarement testé par les auteurs de modèle qui ont l'ambition de s'attaquer au problème.
- *La modélisation spatialisée pour des applications opérationnelles :* là aussi, il me semble que les prétentions des auteurs de modèles spatialisés gagneraient à être validées sur une variété de cas concrets. Par quelle voie avancer dans le domaine de la spatialisation ? Comment identifier une voie permettant de l'introduire progressivement dans les modèles opérationnels sans mettre en cause leur robustesse ? Une réponse à ces questions doit être trouvée avant d'envisager des avancées significatives dans ce domaine.

Abstract

In this document, I present an overview of my research in hydrology over the last ten years and the directions I would like to follow in the future.

In the first chapter, I review the hydrological literature in order to place the approach I adopted (together with the rest of the research team with whom I have been working since 1995) in the wider context of hydrological modeling approaches. My preferred method, which I would define as a *downward empirical* approach, appears quite original compared to the mainstream of hydrological research, especially in its treatment of time and space. Also, I show how it can shed new light on the old passionate debates that have flourished within the hydrological community over the last forty years.

The second chapter is devoted to a presentation of my main published research results. It is organized so as to show the value of assessing the sensitivity of watershed models to their inputs. I cover successively model sensitivity to the input of: precipitation, discharge, potential evapotranspiration, and distributed rainfall. Then, I present a synthetic diagnosis of the main scientific problems, which, I believe, plague modern hydrological models and impede future advances, on both the applied and theoretical sides of hydrology.

The third chapter gives a presentation of my future research objectives, organized into three groups: applied hydrological questions (with the issues of flood and drought forecasting and snow hydrology), theoretical hydrological questions (with the issues of ensemble modeling, ungaged basins and the study of natural hydrological behavior variability) and last, interesting but still unripe hydrological questions (prediction of the impact of land-use change and spatialized hydrological modeling).

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Foreword

From the day I became interested in the hydrological sciences, I dreamed of being able to bring a *significant* and *original* contribution to my field of study. As the true significance of a scientist's contribution can only be assessed over the long term, I became progressively (and, perhaps, unfortunately) more interested in originality, certainly an easier goal to reach in the short term. But I must admit that at the very moment when I read the famous paper by Klemeš (1988) entitled "A hydrological perspective", I knew that I had lost my most important chance of contributing something really original to the science of hydrology: the use of *radio Yerevan riddles*[1] to summarize and analyze hydrological thinking.

Having lost the hope of being truly original, I decided to introduce myself and my work as plainly as possible in this thesis. Even if this could no longer be considered novel, I could not resist the temptation of following Klemeš's path using *radio Yerevan riddles* as the means to synthesize my vision of hydrology and to structure this presentation. As the reader may not be aware of what they represent, I will, in this foreword, give a short introduction to radio Yerevan riddles: they represent a special category of riddles, widespread across the former eastern block, where they were used as a subtle way to criticize the inner contradictions of the soviet system. Klemeš is well-known for his ability to identify the contradictions existing among hydrologists, and he put into the following riddle his own vision of the contradictions plaguing modern hydrology:

Question to radio Yerevan: "*Is it true that hydrologists are the scientists who study the relationships within the water cycle?".*

Answer by radio Yerevan: "*In principle yes, but they are not scientists - they are technologists; and they don't study them - they fudge them."*

Radio Yerevan riddles usually follow the above example, with a stereotypic format which always starts with an inquiry to radio Yerevan, mainly a yes-or-no-question,

[1] Note that these riddles are also often called "Armenian radio" riddles (армянский радио – հայկական ռադիօ) in the former Soviet Union. But here, I decided to follow Klemeš and keep the naming of "radio Yerevan".

and the answer goes "*In principle yes, but...*" to end up by deconstructing the meaning of the affirmative answer. Below is a short list of examples, so that the reader can get used to the very specific logic of the riddle.

- A riddle criticizing the oligarchy of the communist party:

  Radio Yerevan receives a question from an auditor: "Is a junior party member allowed to criticize a senior party member ?"

  Radio Yerevan answers: "In principle yes, but it would be a pity for the junior member."

- A riddle criticizing the reliability of soviet cars:

  Radio Yerevan receives a question from an auditor: "Can you make a sharp turn in a Volga at 100 km/hr?"

  Radio Yerevan answers: "In principle yes, but you can only do it once!"

- A riddle criticizing the deformation of reality by soviet propaganda:

  Radio Yerevan receives a question from an auditor: "Is it true that cars are being given away in Red Square in Moscow."

  Radio Yerevan answers: "That is correct, except for a few small errors. First, it isn't Moscow, but Leningrad. Second, it isn't Red Square, but the banks of the river Neva. Third, it isn't cars but bicycles. Fourth, they're not given away, but stolen."

- A riddle underlining the lack of difference between capitalism and communism:

  Radio Yerevan receives a question from an auditor: "Is there a difference between capitalism and communism?"

  Radio Yerevan answers: "In principle, yes. In capitalism, man exploits man. In communism, it's the reverse."

- A riddle criticizing the freedom of speech in the Soviet Union:

  Radio Yerevan receives a question from an auditor: "Is it true that there is freedom of speech in the Soviet Union the same as there is in the USA?"

  Radio Yerevan answers: "In principle, yes. In the USA, you can stand in front of the Washington Monument in Washington, DC, and yell, Down With Reagan!, and you will not be punished. In the Soviet Union, you can stand in Red Square in Moscow and yell, Down With Reagan!, and you will not be punished."

- A riddle criticizing soviet economy:

  Radio Yerevan receives a question from an auditor: "Is there a difference between a communist diplomat and a communist economist?"

  Radio Yerevan answers: "In principle, yes, but the difference is small. A soviet diplomat is trained to conceal his thoughts, whereas a soviet economist is trained to conceal his absence of thought."

The following three chapters are organized around three questions that we deem fundamental to hydrology, and that could probably have been asked by hydrologists listening to radio Yerevan: in chapter 1, I try to bring elements of an answer to one of the most recurrent questions in hydrological modeling: "*Is there a difference between the various types of hydrological models?*". The review of the hydrological literature which I present in section 1 allows me to present a diagnosis of the main deficiencies affecting all hydrological models. On the basis of this diagnosis, which shows that hydrological models are affected by several common diseases, I attempt in chapter 2 to answer the question "*Can we cure hydrological models*?". There, I use my own and my colleagues' work to identify possible approaches to improve hydrological models. Last, in chapter 3, I investigate a few routes leading to more appropriate hydrological models, asking "*What are the perspectives for appropriate models on the horizon of hydrological sciences?*".

First question to radio Yerevan:

Is there a difference between the various types of hydrological models?

A brief overview of hydrological modeling

1. A brief overview of hydrological modeling

In this section, I wish to present an overview of the different approaches that have been proposed to model hydrological systems. Obviously, many presentations were possible, and I have tried to present a personal and synthetic view, focusing only on those characteristics that are the most significant in watershed modeling. I will discuss successively the main differences between models according to development approach (section 1.1), spatial dimension (section 1.2), and time dimension (section 1.3). In each section, I will explain and justify my modeling choices which are confronted in section 1.4 with the classical debates that have enlivened our science during the past few decades, and with the point of view of the most respected hydrologists. Last, I will attempt to propose an answer to the first question asked of radio Yerevan.

1.1 The approach underlying watershed model development

The first distinction that should be made concerning watershed models refers, in my view, to the approach that underlies its genesis, i.e. its intimate/historical development: what were the underlying conceptions, hypotheses, personal ideas of the modeller when he devised his model? On this topic, we find in the literature two types of classification which (as will be shown later) can probably be merged to a great extent: these classifications refer either to the opposition between *upward* and *downward* approaches, and between *empirical* and *physically-based* approaches.

❑ *Downward versus upward approaches*

The opposition between downward and upward approaches[2] reflects a well-known debate of scientific methodology (see for example von Bertalanffy, 1968). Klemeš (1983) seems to have been the first to classify hydrological models according to this characteristic: he defines the downward approach as the route that "starts with trying to find a distinct conceptual node directly at the level of interest (or higher) and then

[2] Synonyms found in the literature are "top-down" for *downward* and "bottom-up" for *upward*.

looks for the steps that could have led to it from a lower level". On the other hand, the upward approach consists in the classic "mechanistic" (or reductionist) approach, which dominates modern science, where watershed properties are considered to be a summation of the properties of hillslope and streamchannel properties, at all scales.

As Roche (1971) points out, the non-reductionist approach to watershed modeling considers watershed properties to be emergent, i.e. "different from those of each of its elementary phenomena".

Though the supremacy of reductionism in the hydrological sciences is obvious, Klemeš (1983) remains rather critical and advocates a combined downward/upward approach to hydrological modeling, arguing that "a successful solution of a problem is more likely if it is approached from two opposite directions".

More recently, a renewed interest in downward methodology has come from Sivapalan et al. (2003), who consider the downward approach to be "a necessary counterpoint to the mechanistic reductionist approach that dominates current hydrological model development". These authors define the downward approach as "the attempt to predict overall catchment response and the catchment functioning based on an interpretation of the observed response at the catchment scale […]" and oppose it to upward approaches which "rely exclusively on the description of the many individual processes and an a priori perception of how they interact". According to Sivapalan et al. (2003), the result is that, owing to non-existent or inadequate data, many physically-based hydrological models "tend to be over-parametrized with arbitrary and overly complex model structures leading to the problem of equifinality".

Another simple way to present the downward approach could consist in presenting it as a typical systems engineering approach which "typically emphasizes the whole, whereas a mechanistic/reductionist approach emphasizes the individual components or processes that make up the whole" (Heylighen and Joslyn, 1995).

- ***Empirical versus physically-based modelling***

Although I have mentioned the downward/upward distinction first, the major distinction between modeling approaches discussed in the literature remains the one between empirical, conceptual and physically-based models. Many modellers agree on distinguishing between at least these three classes (see for example Linsley, 1982; Bergström, 1991; Wheater et al., 1993; Perrin, 2000), but from my point of view, the conceptual class is ill-defined (see discussion p. 28), and for clarity, I prefer to keep the two extremes: empirical and physically-based models.

Empirical models are exclusively based on the ability of a mathematical structure to match observations. A definition can be borrowed from Nash and Sutcliffe (1970): an empirical model is a model built in an iterative way, for which the modeller is "prepared to accept additional parts [...] only if increased versatility of the model makes it much more likely to obtain a good fit between observed and computed output". Thus, the first and foremost key to model selection is its *efficiency*, i.e. its ability to reproduce the behavior of the watershed system by providing simulations which as closely as possible approach the record. Nash and Sutcliffe recommend building empirical models by successive testing, parsimoniously ("there should be no unnecessary proliferation of parameters to be optimised") and cautiously ("model parts with similar effects should not be combined"). Of course, these authors are conscious of the limits of empiricism, but remain confident that in the present state of hydrology, this route can be interesting: "Hydrologists accept that [empirical models] cannot provide exact solutions. This does not distress them; exact answers are rarely needed." The justification for this belief in the possibilities to identify an adequate model through an empirical approach lies in the fact that "the drainage basin is not a random assembly of different parts, but a geomorphological system whose parts are related to each other by a long common history". Thus, this "encourages the hope that simplified concepts may be found adequate to describe the operation of the basin in converting rainfall into runoff".

Wheater et al. (1993) also discussed the development approaches of watershed models and they used the word "metric" instead of "empirical". Metric models are "based primarily on observations" and seek "to characterize system response from those data". For these authors, the search for an alternative to physically-based models is justified by the fact that "at least at small scale, subsurface stormflows are essentially chaotic, and the ensemble response cannot necessarily be predicted a priori or as an aggregation of spatial components."

Bergström (1991) also expresses his strong support for the empirical-conceptual approaches. For him, the basic assumption underlying this modeling approach is that "we have accepted that the great areal and vertical variability of physical processes is little known, a fact which justifies a more crude, almost statistical approach.”

Physically-based distributed models are at the antipodes of empirical models. They enjoy a wide popularity among a large community, which considers that they “can in principle overcome many of the deficiencies [of empirical and conceptual models] through their use of parameters which have a physical interpretation and through their representation of spatial variability in the parameter values” (Abbott et al., 1986). Physically-based distributed models are based on an attempt to represent all the physical processes occurring on the watershed. Thus, the first and foremost key to model selection is its *physical pedigree*, i.e. its ability to reproduce as exhaustively as possible the representation of the physical processes occurring at the watershed surface. Thus, a physically-based rainfall-runoff model should be judged on its ability to reproduce streamflow records at multiple points, and perhaps also to reproduce other intermediate states of the system.

However, despite their physical nature, the use of physically-based models requires optimization, which creates a conflict of principle: optimizing a physically-based model “may work to accommodate reality, often in a subtle way, to the detriment of the physical basis of the theory on which a model is based” (Beven, 1977). On this same topic, Bergström (1991) states that “the physical interpretation of the parameters of [conceptual rainfall-runoff] models is consequently normally very vague and should be regarded with a sound skepticism. [...] As the physically based model is gradually becoming more and more conceptual the more the calibration option is accepted, the statement that a complex, physically based model is more feasible for studies of effects of land use, scenario simulations, or where input data are lacking, can certainly be challenged.”

❑ ***What about conceptual models?***

Why reject the idea of a separate class for conceptual models, even if they represent the bulk of the models presented in the literature? *Conceptual* models are often defined as *intermediate* between empirical and physically-based models, resulting from the *empirical* selection of an appropriate number of *driving physical processes*. As an intermediate entity, their status is ambiguous, and the criteria applied to conceptual models vary from one author to the other. Fundamentally, I consider this additional class as potentially misleading, since "conceptual" modellers can mix several criteria in their assessment and potentially choose the criterion which optimizes the presentation of their results. Therefore, for clarity, I believe that conceptual models should be viewed as more or less simplified, more or less elegant versions of the physically-based dogma, and I include them in the physically-based class.

Initially, conceptual approaches were intended to combine the efficiency of empirical models, while remaining an appropriate basis for a detailed physical interpretation of watershed behavior... but the reality is that "one does not always master the actual functioning of conceptual models: some model functionalities do not react in the conditions for which they were created, thus loosing their usefulness. Further, the usual reaction consists in introducing new functions to remedy these problems, which accentuates the phenomenon and makes model analysis very difficult" (Michel, 1983).

Table 1.1: summary of the main characteristics of watershed model categories

	Model justification	**Model assessment**	**Parsimony requirement**	**Calibration requirement**
Empirical watershed models	Model efficiency (ability to reproduce the behavior of the whole system); good ratio efficiency/ complexity	Ability to reproduce streamflow records at the watershed outlet	Secondary	Yes
Physically-based watershed models	Exhaustivity of physical representation (reproducing the processes occurring within the system)	Ability to reproduce streamflow records at multiple points	No	In principle, no.[3]
Conceptual *watershed models*	*Mixed: sometimes model efficiency, sometimes physical representation (depending on the modeller)*	*Ability to reproduce streamflow records at the watershed outlet, sometimes on other ungaged watersheds*	*Primary (for the selection of the main "driving processes"*	*Yes*

I have summarized in Table 1.1 the main characteristics of the three main categories of watershed models. Considering the ambiguous definition of the "conceptual"

[3] "In principle" is not a sarcasm, but a quote from Abbot et al. (1986). If I were to be sarcastic, I would seize on this quote to propose the following, somewhat unfair, Radio Yerevan riddle - Radio Yerevan receives a question from an auditor: "Is it true that physically-based and empirical models differ by their calibration needs?" Radio Yerevan answers: "In principle yes, empirical models do require calibration as they are not satisfying without it, while physically-based models do not require calibration as they are not satisfying."

category, I will try to avoid using the term “conceptual model”. Note also that another reason to restrict the classification of watershed models to the empirical and physically-based groups, is that it corresponds perfectly with the distinction between downward and upward approaches discussed earlier:

- *empirical* watershed models result necessarily from a *downward* approach, as an attempt to explain the overall functioning of a watershed, i.e. as an elementary unit. The behavior of this unit can then be considered an emergent property of the watershed;
- on the contrary, even when they are simplified, conceptual and physically-based models need to rely on an “upward” description of the processes.

❑ ***Are empirical/downward approaches feasible? Impact of data quantity and quality.***

Developing a hydrological model *empirically*, following a *downward* approach requires that the modeler relies on a basin sample of sufficient size. Indeed, one of the often-heard critiques aimed at the empirical approach of model development is that the model may be dependent on the watershed sample used for its development. To avoid this problem, we use at Cemagref large data samples : 429 for the PhD theses of Ch. Perrin and S. Mouelhi (Perrin, 2000; Mouelhi, 2003); 308 in the PhD work of L. Oudin (2004), 313 in the on-going PhD work of T. Mathevet, and more than 1100 in the on-going PhD work of C. Rojas-Serna). The smallest of our recent study samples was the sample I used during my PhD research (Andréassian, 2002): it comprised 63 watersheds, of which we had detailed forest cover evolution data on 34 basins. Compared to the samples used in the literature, this is still a large sample. By using large basin samples, we follow the recommendation by Linsley (1982): “because almost any model with sufficient free parameters can yield good results when applied to a short sample from a single catchment, effective testing requires that models be tried on many catchments of widely differing characteristics, and that each trial cover a period of many years”.
But a reproach can be sometimes heard concerning the use of large data samples: as the watershed sample increases in size, it becomes impossible to perform a detailed validation of the raw time series, and this may bias model development. The answer to this reproach was already given by Linsley (1982) more than two decades ago: “if the data are too poor for the use of a good simulation model they are also

inadequate for any other model”. Boughton (2005) added that "there is no way to separate the quality of the modelling performance from the quality of the input data [...] except by calibration of a number of models on a substantial number of catchments for direct comparison of results". In short, poor quality data will disadvantage equally all models. And although one must naturally try to ensure the highest possible standards for hydro-meteorological data in the model development samples, it would be a big mistake to use a model to “validate” the data since we intend to use the sample to develop a model. Thus, I believe that it is fallacious to object to empirical or downward approaches on the grounds of the difficulty to control quality.

- ***Are general watershed models realistic? Do we need climate-specific watershed models?***

There is quite a powerful trend among modellers insisting that watershed models should be climate-specific. This is in line with the prescriptions of the “conceptual” school, which advocate keeping in a watershed model only those “driving processes” that the modeller believes to be important in a given watershed. As “driving processes” may vary depending on hydro-climatic zones, it then seems natural to recommend a climate-specific modeling structure.

However, I believe that transferring this view from the conceptual to the empirical/metric domain would be a mistake: the empirical approach consists in looking for emergent hydrological properties of at the watershed scale. It does not have prerequisites in terms of climate, and only experience can possibly show whether an empirical/metric structure is better adapted to a given zone. In the light of the experience accumulated for the last decade by our team at Cemagref Antony, linking climate conditions with model structure does not seem justified.

Note that developing a watershed model with a certain ambition of generality was recommended by the father of rainfall-runoff modeling himself, Ray Linsley (1982). Alluding to the great variety of driving processes which may affect the rainfall-runoff relationship, he writes that “these differences do not mean that a single model cannot be applied in all cases. The model must represent the various processes with sufficient fidelity so that irrelevant processes can be “shut off” or will simply not function”. And Linsley concludes “that it is no longer necessary for each hydrologist to develop his or her own model for each catchment, since […] a new model for every application eliminates the opportunity for learning that comes with repeated applications of the same model.”

1.2 Distributed, semi-distributed and lumped models

Spatial representation is one of the active fields in hydrological modeling research. As a first approximation, it can be said that the watershed modeller has to choose between a *spatially lumped* and a *spatially distributed* approach. This decision is not trivial:

- lumped watershed models have proved both efficient and robust over the years and their relatively low number of parameters limits the numerical problems such as secondary optima, parameter interaction, poor sensitivity of parameters;
- but many hydrologists believe that distributed models could potentially have a greater ability to take into account the spatial heterogeneity of both rainfall and land surface.

The opposition between lumped and distributed approaches has consequences in terms of the modeling approach as discussed in the previous section (see Figure 1.1):

- most distributed models imply a reductionist/mechanistic approach, where watershed behavior is seen as a combination of elementary subbasin behaviors.
- On the other hand, lumped models imply a non-reductionist approach, where watershed behavior can be considered an emergent property of the basin .

Therefore, the lumped approach can also be considered as the first step in a downward approach, while the distributed approach typically results from an upward view of model building.

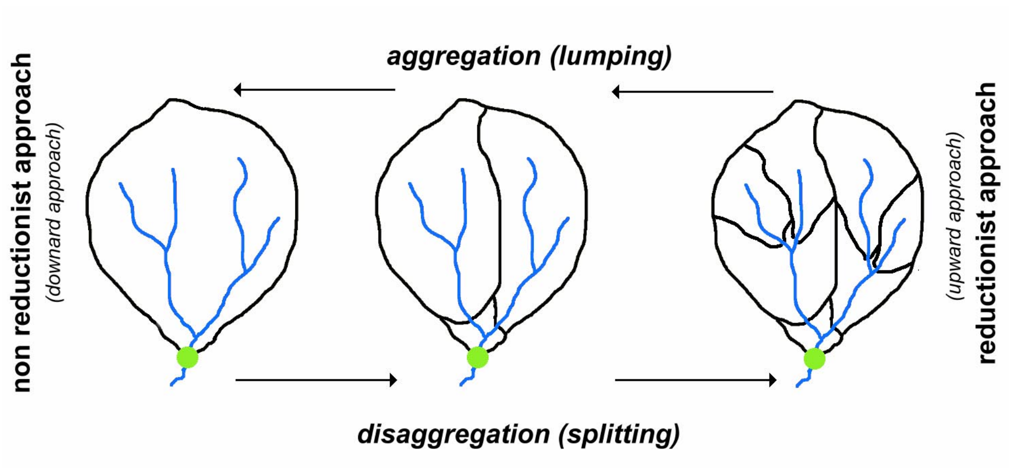


Figure 1.1: Implication of aggregative and splitting approaches to hydrological modeling: on the right side, watershed behavior is seen as a combination of elementary subbasin

behavior, while on the left side, watershed behavior is considered an emergent property of the basin

Last, the debate between aggregation and disaggregation is not only philosophical, it is also technical and economic:

- distributed models are by no means easy to use. The US National Weather Service's attempts to introduce distributed inputs into its flood forecasting system provide a good illustration of this point (Finnerty et al., 1997; Koren et al., 1999; Smith et al., 1999);
- using a distributed model also means acquiring distributed inputs and using a distributed parametrization. Both of these aspects have a definite cost, and it is often difficult for end-users to decide where to focus investments: should they favor input data (invest money to increase the spatial representation of rainfall input by example) or model parameters (by funding the addition and continuous acquisition of spatially distributed parameters, or by asking the modeller for a finer representation of the watershed)?

Although the question of the level of aggregation or splitting that is adequate for modeling has been discussed quite extensively in the hydrological, systems engineering, and ecological literature, there is still no general agreement on the subject. The main premise of many of the proposed fully distributed models is that the spatial detail should lead both to an improved understanding of the watershed behavior and to improved simulations. However, the degree to which the spatial variability of each process needs to be represented is not well understood (Boyle et al., 2001), and there are no generally accepted criteria for deciding which level of detail should be included in a particular model developed to address a specific problem (Muetzefeldt and Yanai, 1996). Several authors (Hauhs et al., 1996) question the very assumption that it is always necessary to increase the level of detail in a model to improve its predictive capacity, and Beven (1996) even considers that because of the non-linearity of the equations governing hydrological processes, "an approach based on the aggregation of small-scale theory is obviously unscientific[4]."

[4] Beven then raises the question of why this approach remains popular, while immediately acknowledging that it still "may be expected to increase in popularity as available computer power increases."

Presenting an overview of hydrological results concerning spatial aggregation/ disaggregation issues is not easy. As Muetzefeldt and Yanai (1996) emphasize, the major problem lies in that "the published comparison of existing models often fails to isolate the question of model disaggregation, because the models to be compared differ in many respects, not just in the degree of disaggregation of well-defined components." For my review, I propose to regroup existing studies of aggregation and splitting issues into four different classes: *aggregation studies, disaggregation studies, comparative studies and theoretical studies*.

- ***Aggregation studies***

The approach most commonly found in the literature is that of the physicist: he perceives a typical hydrological model as distributed by necessity. Since he starts from the widest possible distribution, his studies focus mostly on how to reduce the level of spatial distribution, i.e., on aggregating (in order to reduce the degrees of freedom of the system to be calibrated).

The approach by Haverkamp et al. (2002) is quite characteristic of spatially distributed modellers. The authors state that their objective is to "find the smallest number of sub-watersheds required to obtain good and stable simulation results." They observe that model efficiency becomes stable, approaching an asymptote, while the number of grids increases (note that their study does not vary the aggregation level of rainfall input and only focuses on the number of land-use and soil classes).

To demonstrate the usefulness of a distributed approach, Becker and Braun (1999) bring out the example of an oasis in the middle of the desert. The oasis would lose water at a potential rate (say 2600 $mm.yr^{-1}.ha^{-1}$), while the surrounding desert would have much lower actual losses (say 15 $mm.yr^{-1}.ha^{-1}$), due to the difference in water availability. Although the oasis represents only 2% of the surface of the desert watershed, it can represent around 80% of the total evaporation losses. A lumped treatment of the watershed could provide an accurate estimate of watershed-scale evaporation losses, but its areal value may be physically meaningless. Although this example is very pedagogic, such a situation *cannot be avoided* in hydrological modeling: even within the oasis, one can surely find single trees still evaporating at a potential rate while shallow-rooted species situated under them are close to the wilting point. This "oasis effect" reproduces itself at all scales and is only one of the inherent scale problems that must be dealt with by hydrologists.

Merz and Plate (1997) used an event-based watershed model to simulate the impact of the spatial variability of soil parameters and initial conditions on runoff simulations over a very small watershed. They analyzed the relative importance of splitting and lumping by comparing simulations that used either homogeneous or distributed soil parametrization. They assessed in their study the effects of aggregation according to the type of event and concluded that the effects of spatial variability are small for very small and large runoff events (>10-year return period).

- ***Disaggregation studies***

The second approach is that of the systems engineer: he is more inclined to see a typical hydrological model as lumped for numerical and/or robustness reasons. Thus, he usually starts from a lumped version and tries to increase the spatial distribution to improve the predictive ability of models. Therefore, his research usually focuses on disaggregation issues.

Some of the authors who compared lumped and distributed models report advantages in the distributed version (Boyle et al., 2001; Zhang et al., 2003). But the improvements are usually small.

Boyle et al. (2001) studied several disaggregation options on a single watershed. They concluded that the main improvements (in terms of model efficiency) were provided by the spatial representation of precipitation, while little or no improvement was gained by spatial representation of soil properties.

Zhang et al. (2003) used the same watershed as Boyle et al. (2001): the reason lies in the elongated shape and contrasting soil parameters in this catchment, which suggest that splitting should improve the efficiency of flow simulation. Subdividing the watershed into eight subbasins and using distributed radar rainfall input produce slightly improved results without greatly increasing the computational requirements. The article by Zhang et al. (2003) also provides an extensive literature review on the use of distributed radar rainfall input in RR models. I believe that although many hydrologists confidently express the opinion that distributed rainfall input would improve streamflow simulations, the concrete results reviewed by the above authors should seriously temper this enthusiasm (see in particular Smith et al., 1999; Carpenter and Georgakakos, 2000; and Stellman et al., 2000).

There are also situations where authors report no differences in their comparison of lumped and distributed approaches: Loumagne et al. (1999) used a sample of 15 paired watersheds, where streamflow was measured twice upstream and once downstream, as in Figure 1.2. They compared the lumped and semi-distributed approaches to rainfall-runoff modeling on these basins, and concluded that, from a streamflow simulation point of view, there was no appreciable difference between the two approaches. Booij (2002) worked on the large Meuse River basin using the HBV model, and compared several levels of disaggregation: a lumped basin, a 15 subbasin solution, and a 118 subbasin solution. He found no significant differences in terms of runoff simulation. On the neighboring Moselle River basin, Diermanse (2001) concluded that “the distributed model is hardly any better than the lumped model version”.

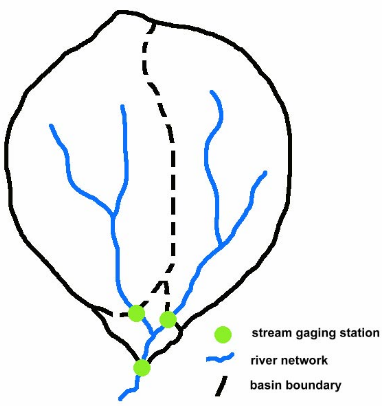


Figure 1.2: Configuration of the streamgaging network ideally needed to validate the hydrological impact of splitting (disaggregating) schemes

❑ *Comparative studies*

The third group of studies is made of comparative approaches, often characteristic of hydrological engineering studies. Their authors take hydrological models as they are and perform comparisons between lumped and distributed models on the basis of their efficiency.

Refsgaard and Knudsen (1996) compared a lumped model to two different distributed models and found very little difference.

Carpenter and Georgakakos (2000) compared a distributed and a lumped model for the Illinois River basin, which was disaggregated into smaller subbasins, simulating selected peak events from 1993 to 1996. Again, flow simulations at the outlet using basin disaggregation were comparable in accuracy to simulations from a lumped model.

Georgakakos and Carpenter (2003) compared a distributed and a lumped version of the same model over the Blue River basin, and assessed the ability of each class of model to simulate peak-flow through ensemble simulations of 25 large events, trying to take into account parameter and radar input uncertainty. They found the distributed model to provide significantly better estimates for 44% of the events, while the lumped model had significantly better estimates for 36% of the events. Again, if the distributed solution appears statistically better, its advantage remains very limited.

- ***Theoretical studies***

The fourth and last approach is usually that of the theoretical hydrologist who sees actual measurements as very imprecise, and thus prefers to use abstract (synthetic) reconstructions of the basin, where he can control all the heterogeneities. The study of aggregation / disaggregation issues then becomes a sort of “hydrologic game”, as Freeze (1980) puts it, implemented through simulations, and conditional to strong hypotheses. This game aims at providing an answer to the following question: “since we know how the physical systems behave at the very local (the lysimeter) scale, let us see how a lumped model will behave under various hypotheses of system heterogeneity”.

Wilson et al. (1979) used a semi-distributed watershed model and a rainfall generation model and concluded that the spatial distribution of rain and the accuracy of the precipitation input have a marked influence on the simulated hydrograph.

Freeze (1980) also based his study on synthetic streamflow data, produced by a theoretical hydrological model. He simulated runoff on a theoretical slope, where he tried several kinds of soil heterogeneity, and attempted to represent the behavior of heterogeneous hillslopes using *equivalent* homogeneous hillslopes. He observes that this leads to large errors, and suggests that “the distribution of hydraulic conductivities over a watershed should be included in parametric representations.”

Beven and Hornberger (1982) used an intermediate, semi-theoretical approach to investigate the effect of spatial patterns of precipitation on watershed modeling. The authors first attempted to analyze actual events that differed in the distribution of rainfall over a small Illinois watershed. But the inadequate number of sufficiently contrasted events made it impossible to draw statistically significant conclusions. Therefore, they used an artificial rainstorm dataset and a semi-distributed watershed model to simulate a large number of stormflow events. They were then able to find a statistically significant difference between the hydrographs of homogeneous and spatially contrasted storms, and concluded that the greatest effect of the rainfall pattern was on the timing of the runoff hydrograph.

The results reviewed above may appear quite contradictory and they are often difficult to interpret: aggregation and theoretical studies seem to indicate that spatial distribution of hydrological models and their inputs is essential, but the results of disaggregation and comparative studies cast serious doubt on the well-accepted opinion that the less aggregated the model, the better its hydrologic simulations. I will try to bring a different and (hopefully) enlightening view to this topic in section 2.6 of the next chapter.

1.3 Time step and continuity of simulation

While the literature abounds in discussions of issues related to spatial representation, time representation appears to be much less of a concern for watershed modellers. However, if I am to examine the differences between existing models, time does have an importance in two respects:

- as the elementary step of numerical computation. *A priori*, any time step can be used to run a model, from the finer (second or minute) to the coarser (month, year, or even decade);
- and also for the treatment of time-boundary conditions. There, a distinction can be made between continuous and event models.

In this section, I examine successively the influence that the treatment of time within watershed models has on model efficiency and what this treatment teaches about the underlying modeling philosophy.

❑ *Fine versus coarse time steps*

The time step is a rarely discussed issue in watershed modeling. While discussions can go on endlessly concerning the merits of lumped and distributed approaches, very few authors have pointed out the possible contradictions arising when coarse time steps (such as the day, or even sometimes the month) are used together with distributed "physically-based" models. With the increasing availability of data at a short time step[5], there is a growing tendency among scientists favoring physically-based models to use very short time steps, of the order of a minute (see for example Moussa et al., 2002; Chahinian, 2004). Hourly and daily time steps will unavoidably require approximations, as well as the use of *effective* model parameters. Hydrologists adhering to empirical modeling even argue that a distributed rainfall-runoff model fed with a coarse time step may have less "physical meaning" than a lumped model fed with a fine time step. Unfortunately, no simple experiment has ever been proposed to compare these solutions.

Klemeš (1983) is rather critical towards the indifference of hydrologists concerning the time step: he states that modellers "tend freely to mix physically-based time

[5] Note that I refer here to the time step at which data are available. Some models may have a shorter computation time step; some modellers also choose to assess the efficiency of their model at a larger time step (e.g. evaluate the performance of a daily model, fed with daily rainfall input, at the monthly time step). In what follows, I consider the usual case, where the time step of data, the time step of computation, and the time step of assessment is the same.

intervals such as the year and the day with administrative ones such as the month, week, hour, minute and second, and do not hesitate to use any of them in conjunction with any spatial scale in our models, often deluding themselves that the space and time scales are only a matter of the capacity of our computers to accommodate a given number of spatial nodes and time intervals".

Among the rare attempts to give time steps a specific treatment, one finds Jothityangkoon et al. (2001) who tried to develop a series of models beginning with a large time step and gradually introducing the complexity required to meet the needs of shorter time steps: "at each scale, we have endeavored to develop parsimonious models, with the minimum complexity that is necessary to capture runoff variability". These authors start with a three-parameter model for the 1-year time step, obtain a nine-parameter model at the monthly time step, and a ten-parameter model at the daily time step. Although all parameters may not require calibration, these "parsimonious models" still seem rather far from what I would personally consider parsimonious.

In his PhD thesis prepared at Cemagref, Mouelhi (2003) took a quite different approach: his aim was to find empirically the models best suited to each time step in turn (monthly, annual, interannual) and, when several structures were shown to be equivalent, to keep the simplest one. He also had a secondary interest in keeping some continuity between the successively selected structures. However, the justification for his search for continuity was model efficiency and not sheer aestheticism (if a structure is good for one time step, part of it should remain so for the adjoining time steps). Mouelhi et al. (2004) detail the approach used to develop this chain of models. They express the strong conviction that hydrological models cannot be supported by physical considerations because when it comes to *lumped* models working at *coarse* time step, there is a huge smoothing over processes at work at the plot scale

As a result of Mouelhi's thesis, three models have been proposed for three large time steps (long-term water balance, annual and monthly time steps), and the connection with the daily time step (which was an outcome of Perrin's thesis, also at Cemagref) has also been made. Presently, in his on-going Ph.D. research at Cemagref, Mathevet is working on a sample of 313 basins at an hourly time step, in order to put the final touch to the coverage of the time steps most needed by a practicing hydrologist, with lumped rainfall-runoff model structures of adequate complexity.

Table 1.2: optimal* rainfall-runoff modeling structures obtained empirically by Mathevet (ongoing), Mouelhi (2003) and Perrin (2000) for decreasing time steps on 429 basins

Time step	Model name	Number of free parameters	Key-elements of the model structure
Long-term model	GR0S	0	- formula
Annual model	GR1A	1	- formula (runoff coefficient includes previous year rainfall providing thus an elementary rainfall routing)
Monthly model	GR2M	2	- SMA store (θ1) - routing store (θ2)
Daily model	GR4J	4	production function: - SMA store (θ1) - underground exchanges (θ3) transfer function: - routing store (θ2) - unit hydrograph (θ4)
Hourly model**	GR5H	5	-

(*): refers here to the requirements presented in section 1.1 and summarized in Table 1.1

(**): preliminary results

Table 1.2 presents the main characteristics of the model chain discussed above:

- The long-term model, GR0S, is very similar to the Turc-Pike model (Turc, 1954). It has no free parameter (which is inevitable since there is only one output). GR0S is similar to models proposed by Schreiber (1904) and Ol'Dekop (1911). However a notable difference is the presence of the empirical coefficient 0.73 which means that the limit of actual evaporation, P-Q, is equal to 0.73E when P goes to infinity.
- In the annual model, GR1A, the underlying model is also the Turc-Pike model. It has only one free parameter, and includes an antecedent year rainfall index.
- With the monthly model, GR2M, there is a jump in complexity: a two-store model is now required (i.e., it proved consistently the best one and it was impossible to find a monthly model with a simple structure derived from the annual one). The GR2M structure is very close to that of the 1-day time step models GR3J (Edijatno et al., 1999) and GR4J (see below).

- The daily model, GR4J (Perrin et al., 2001; Perrin, 2002; Perrin et al., 2003) is a two-store model, depending on four calibrated parameters.
- Hourly model (GR5H): even if the model is still being developed, it seems already clear that a structure derived from GR4J and including at least a fifth free parameter gives the best results (it already surpasses all the other lumped models tested on a large basin sample). Ongoing work focuses on options allowing the representation of recessions (through the introduction of the exponential store presented by Michel et al., 2003) and on refinements of the infiltration subroutine.

The five above models have emerged at the end of a long research process aimed at identifying simple but effective representations of the watershed-scale rainfall-runoff relationship. This quest for efficient models of appropriate complexity involved extensive comparisons, based on large basin samples, with virtually all models proposed in the literature, trying to avoid preconceptions about the physics of the hydrological processes to let the catchments “speak” for themselves, through an approach which we could define as “data-intensive empirical model design”.

❑ *Event versus continuous models*

My review of the time issues within watershed models would not be complete if I did not consider the question from the point of view of the continuity of modeling. Indeed, two kinds of models are found in hydrological engineering, namely *continuous models* (which are meant to simulate continuously water balance and transfer at the watershed scale, over long -pluriannual- time periods), and *event-based* models, which simulate only short (mainly flood) events.

The main difference lies in the necessity to provide initial conditions to the event models, while continuous models keep a continuous accounting of the moisture state within their stores, so that, after a so-called warm-up period (often about one year), the initial errors of storage in the transfer and production functions are without effect on the simulations.

Note that event-models are mostly engineering models that try to adapt to the scarcity of continuous input time series, at the cost of relying perhaps excessively on initial conditions which need to be calibrated.

Which solution should be adopted? My view is that the point made already twenty years ago by Linsley (1982) is quite convincing: “Generally a continuous model is to be preferred […]. Event models do not define initial conditions and hence cannot really aid in defining flood frequency. The assumption that the frequency of the input rainfall determines the frequency of the computed flow is pretty well disproved. Hence, the use of event models with a design storm is likely to lead to answers which are substantially in error.”

1.4 How does my hydrological approach fit the classical debates?

In this section, my aim is to place my research (or, more generally, *our* research at the Cemagref research center in Antony) in the widest context of the general debates in hydrology and science in general.

❑ *Some good reasons to prefer the empirical approach for model design*

During my review of modeling approaches, I have attempted to gather and present the elements that I found useful when deciding which is the most appropriate approach to watershed model development. What conclusion did I reach?
I must admit that from the beginning I was attracted by the ability of empirical / downward models to solve *practical* hydrological problems. I recognize that this statement can be seen as fairly old-fashioned, because the belief that modern computers have overcome all obstacles and that downward and empirical approaches are now useless, is very strong in the scientific community. But as Klemeš (1986) states, modern computing technology also has its drawbacks, one of them being a "tremendous potential to divert talent and resources into the pursuit of the irrelevant".
Nonetheless, a prudent statement would be that both empirical and physically-based modeling are useful… Who would not agree in principle with this statement? But concretely, my day only has 24 hours, and I fear that the same applies to many scientists. Thus, choices must be made, and I believe that if hydrology is a science (in the sense used by Klemeš, 1986, in his famous paper *Dilettantism in Hydrology: transition or destiny?*), then hydrologists are legitimately entitled to studying the emergent properties of the hydrological systems, without needing a systematic reference to other branches of the physical sciences. Empirical watershed modeling approaches make perfect sense in the context of hydrology as a *science of its own*. Those who would like to reduce the aspirations of hydrology to assembling other sciences' contributions would qualify for the appellation "dilettante" hydrologists in the sense proposed by Klemeš (1986).
I believe that much can be learned in hydrological modeling by *progressive* attempts, even if we are sometimes disappointed when we cannot usefully introduce the latest algorithm or assimilate the most recent remote sensing information in our models. But it is a fact that the traditional mechanistic/upward approach has lead hydrology

to a situation where most models "have a degree of 'surplus content' that is not supported by data, but is only introduced to satisfy the modeller's preconceived notions of the catchment's functioning" (Sivapalan et al., 2003). Most modellers, however, still regard a model obtained via this route "as a priori superior to others because it includes a few established concepts (in addition to the many more vague and questionable ones) and manipulates them by elegant mathematics" (Klemeš, 1983). Which way is the best? Should we start with overcomplexification or with oversimplification? As far as I am concerned, I would give my preference to the path indicated by the founder of systems theory, Ludwig von Bertalanffy (1968) who suggested that "oversimplifications progressively corrected in subsequent development are the most potent and indeed the only means towards conceptual mastery of nature." And last, as hydrologists concerned with the applicability of our science, we cannot restrict ourselves to models that find their justification in the contemplation of pure hydrological processes: as the Belgian poet Henri Michaux wrote, "if a contemplative person jumps into the water, he will not attempt to swim, he will first try to understand water. And he will drown.[6] "

- ***Which approach can best take into account watershed spatial variability?***

In section 1.2, I reviewed results relative to spatial aggregation and disaggregation in watershed models. The results presented in the literature are quite contradictory and often difficult to interpret: while aggregation and theoretical studies seem to indicate that spatial distribution of hydrological models and their inputs is essential, the results of disaggregation and comparative studies cast serious doubt on the well-accepted opinion that the less aggregated the model, the better its hydrologic simulations.

To try to draw recommendations from the above review for our research, it must first be acknowledged that the published examples are always based on too few basins to make any generalization possible. Then, one must recognize, as underlined by Muetzefeldt and Yanai (1996), that for most published comparisons of existing models, it is impossible to isolate the impact of model disaggregation clearly, because the compared models differ in too many respects.

[6] "Si un contemplatif se jette à l'eau, il n'essaiera pas de nager, il essaiera d'abord de comprendre l'eau. Et il se noiera."

My point of view is that all the approaches I reviewed in section 1.2 can be considered complementary, but the truth is that the sometimes radically opposed objectives of the mechanists-hydrologists and hydrologic engineers have led to a great deal of misunderstanding: the first advocate an upward approach (leading necessarily to distributed models), while the second favor a downward approach (usually starting with a lumped vision of the watershed).

In line with my inclination for downward/empirical approaches, I do believe that the most appropriate way to progress in distributed modeling is by a progressive, empirical disaggregation approach. I will illustrate later (in section 2.6) what such an approach could be like, by comparing the results of identical models which only differ by their level of spatial aggregation.

- ***About model complexity and the appropriate number of free parameters in watershed models***

Concerning model complexity, the arguments presented by Nash and Sutcliffe as early as 1970 seem not to have aged. Thus, I believe that their advice ("to accept additional parts [...] only if increased versatility of the model makes it much more likely to obtain a good fit between observed and computed output") is of prime importance, not only for empirical models, but also for the physically-based ones. Is it carefulness or just fearfulness? Personally, I would view it as safe behavior, as I agree with Michel (1983) who stresses that "model functionalities often do not react in the conditions for which they were created, thus loosing their interest".

Could this problem then be solved by resorting to more physically-based models? Even Beven (1993) seems doubtful about this, as he writes that "there is no reason to expect that the physical basis of distributed models will mitigate" the problem of multiple parameter sets, and that "given the number of parameters involved, the problem is likely to be much worse".

I completely agree with Martin (1996) when he states that "the prediction obtained with a complex model often points to a simpler model which could have been used in the first place. The challenge here is for the designer who has failed to keep his model simple to recognize the fact when confronted with it." The same point of view was presented earlier by Bergström (1991), who wrote that "going from complex to simpler model structures requires an open mind, because it is frustrating to have to abandon seemingly elegant concepts and theories. It is normally much more stimulating, from an academic point of view, to show significant improvement of the model performance by increasing complexity."

- ***Can we eradicate uncertainties?***

During the past decade, much research has been devoted to the assessment of uncertainties associated with watershed modeling, for two reasons:

- studying parameter uncertainty helps to better understand the influence of data and modeling defects on model efficiency and robustness
- it can also highlight possible inconsistencies in the model structure, as uncertain parameters essentially mean that the modelled system is ambiguously defined.

Then, building on preoccupations concerning parameter uncertainty, several strategies have been proposed to cope with the problem of ambiguous system description, attempting to find responses to the previously mentioned sources of uncertainties. The strategies elaborated by hydrologists around the world mainly explore one of the following four routes (Wagener et al., 2003):

1. the design of parametrically-parsimonious model structures (i.e. with an appropriate number of free parameters) (Michel, 1983; Edijatno, 1991; Jakeman and Hornberger, 1993; Perrin et al., 2001; Perrin et al., 2003);
2. the use of additional data (i.e. multi-variable models) to further constrain the model (e.g. Kuczera and Mroczkowski, 1998);
3. the development of more powerful calibration techniques and the use of multi-objective functions (see for example Duan et al., 1992; Madsen, 2000; Duan et al., 2002);
4. the identification of "model populations" (regrouping equally likely parameter sets) as a response to the observation of non-uniqueness of parameter optima (Beven and Binley, 1992).

Strategy 1 on the one hand and strategies 2, 3 and 4 on the other differ drastically in the way they deal with modeling uncertainties:

- strategy 1 focuses on model structure, with the aim of keeping only the elements and model parameters that can be justified by the model's sensitivity;
- strategy 2 does not attempt to reform model structure directly, but assuming its sensitivity to some conceptual representation of model states, it attempts to constrain these states by observations;

- strategy 3 starts from the observation that highly-parametrized watershed models have quite limited sensitivity to many of their parameters, and it focuses on the search for "miraculous" numerical strategies by which to identify even the less sensitive parameters;
- strategy 4 gives up all hope of finding well-identified parameter values. It somehow considers that hydrological models are doomed to be ambiguously defined and that we need to develop strategies to live with it (the GLUE method being one of those).

I believe that research results of the last decade (see for example Jakeman and Hornberger, 1993; Perrin et al., 2001; Sivapalan et al., 2003) have shown that many hydrologists may have been too quick to give up hope and to look for replacement strategies and that strategy 1 should be at least attempted before proceeding to more complex modeling levels (i.e. multi-objective and/or multi-variable). This first stage in the modeling process may avoid many of the ill-posed problems encountered by strategies 2 to 4 and could clarify the problem of parameter uncertainty that hydrologists face today. As the French say: *les seules batailles perdues d'avance sont celles que l'on ne mène pas*.

❑ *How to improve watershed models?*

In the hydrological community, the most common approach to watershed model improvement is to recreate a new model from scratch. No doubt, our bright and talented hydrologists dream of leaving their name attached to their own model engraved in the hydrological hall of fame. Such an approach is not recommended by the followers of empirical modeling: they would rather try carefully to modify (and perhaps even simplify) the best available models, as it seems to them "always preferable to deepen the understanding of why a model differs from reality in order to imagine other more efficient structures" (Michel, 1983).

When working on a specific model, prudent modellers have tried to progressively increase model complexity. Bergström (1991) followed this approach for the HBV model. Interestingly, he found that "the point of diminishing returns (no model improvement) was reached surprisingly soon with increasing model complexity."

Why are our models so difficult to improve? Why is progress apparently so slow in our science? Do we lack new ideas? Or bright scientists? I personally believe that we have a lot of available talent in the hydrological community, but that many of these talents are somewhat muzzled by what Bergström (1991) calls *the cult of success*. I think that this is actually a plague for hydrologic literature, as I agree completely that "although reports on the lack of success are rare in the scientific literature we all know that a negative advice can be invaluable [...]. Honest presentations of scientific disappointments are important contributions which make the journals more interesting to the reader."

❑ *Can intercomparison help move watershed models forward?*

An approach which has been proposed over the last three decades to improve watershed models (and which is in favor at Cemagref) is model intercomparison (see e.g. Leviandier, 1988). The successive international intercomparisons organized by WMO since the end of the sixties (WMO, 1975; WMO, 1986; Askew, 1989) have been very efficient in promoting a sound competitive spirit in hydrological modeling and to force modellers to question some of their preconceptions. The same applies for intercomparisons organized by single groups (Perrin et al., 2001). Even Loague and Freeze (1985), the famous champions of physically-based modeling, seem to have learned much from the conclusions of their own intercomparison ("the fact that simpler, less data-intensive models provided as good or better predictions than a physically-based model is food for thought.")

Some hydrologists, however, have found the results of intercomparisons disappointing (Wheater et al., 1993): "Intercomparisons could be expected to reveal the strengths and weaknesses of alternative models, but, in general, have failed to identify clear guidelines for model selection." And others such as Woolhiser (1996), even consider the comparisons between simple and physically-based models to be "severely flawed". But note that the last author refers to a small number of comparisons, based on experimental basins, where he knows that the physically-based model used was not appropriate in terms of physical processes representation. Thus, he considers that the only conclusion that can be drawn for these specific cases is that simple models can give "equally bad answers at a lower cost".

My opinion is that as long as the number of watersheds included in the comparison is limited (as has been in most of the comparisons published up to now), its conclusions may well be a matter of luck, and the intercomparison exercise loses most of its interest. What is needed is a statistically significant number of catchments, such as in the intercomparison by Perrin et al. (2001). Even if such comparisons are still the exception rather than the rule, a few more are coming at Cemagref (Mouelhi, 2003; Oudin et al., 2004; Mouelhi et al., 2005; Oudin et al., 2005a; Oudin et al., 2005b; Oudin et al., 2005c; Mathevet, ongoing research), and will shed light on the actual differences between model approaches (at least between empirical/metric and conceptual models), and help to improve models (Perrin et al., 2003). Note however that this may make the comparison very difficult between the simpler models and the physically-based ones, since running a physically-based model on one basin usually require several months of work, thus precluding the use of large samples.

1.5 Where do I go from here?

The review of the hydrological literature presented in this chapter shows that a huge variety of solutions are proposed to develop watershed models, and to deal with space and time within them. Even if in hydrology - like in other sciences - we have fashions and trends, there is no widely accepted approach to building a model and deciding its *appropriate* level of complexity. Hydrological modeling seems to be stuck in a dead-end. Do we know how to get out of it? Do we need to rely on a single approach or on a variety of them? Klemeš (1983) provides a partial answer to these questions: “nobody can be blamed for not immediately knowing the correct way through a complex labyrinth. What he can be blamed for is an insistence on a preconceived idea of the correct route and unwillingness to check it out. Regrettably, hydrologists often behave in this manner: instead of searching for feasible way of conceptualization of hydrological processes, they postulate the structures of their models on the basis of an arbitrarily embroidered high-school diagrams of the hydrologic cycle with little concern for testability”.

We definitely need to raise our models above high-school diagrams. We need to return to more reasonable, i.e. more parsimonious parametrizations, as advocated as early as 1970 by Nash and Sutcliffe and more recently by Michel (1983), Beven (1989), Jakeman and Hornberger (1993) and Wheater et al. (1993). Many of the models we use today are plagued by overparametrization. How can we establish a sound diagnosis, on the basis of which we can attempt to cure them and move them forward? How can we avoid the present “trench warfare” between partisans of opposed approaches such as downard-upward or physical-empirical?

In the next chapter, I show examples from my own work and that of my colleagues, to demonstrate how our research focusing on problems encountered with very simple watershed models allowed us to establish a fairly general diagnosis of the main plagues of hydrological models. Then, in the last section, I will discuss the perspectives of my future research.

1.6 First answer to radio Yerevan

Is there a difference between the various types of hydrological models?

Radio Yerevan answers:

In principle yes, but the difference is small:

- empirical models are easy to use models that cannot be extrapolated, while physically-based models are extrapolable models that cannot be used;
- downward models are intellectually disappointing models which may provide efficient results, while upward models are intellectually efficient models which may provide disappointing results.

Second question to radio Yerevan:

Can we cure watershed models?

A tentative diagnosis of what ails watershed models

2 A tentative diagnosis of what ails watershed models

2.1 The three plagues of hydrological modeling

I believe that hydrological models suffer from three main problems, which are *overparametrization*, *overconfidence*, *overprotection*.

- **overparametrization**: this means that watershed models have more free parameters than they are able to support, too many interdependent parameters which are interacting during the calibration process, thus turning even the most promising structure into an uninterpretable black-box. I believe that overparametrization is the main reason why so many models face the problem of 'equifinality' (Beven, 1993), a situation where different parameter sets yield equivalent model outputs, and where parameters are uncertain and poorly-defined due to serious problems of identification during calibration (see e.g. Gupta and Sorooshian, 1983).
- **overconfidence:** this means that modellers have an unsound and excessive belief that the structure they have built actually works as they had expected it to, and that the parameters that have been calibrated actually keep the physical *face value* that was attributed to them. This point is stressed by the famous French hydrologist, Marcel Roche (1971): "one must above all be wary of one's own experience: [...] how many hydrologists have actually believed they had a universal tool when they had only obtained a regional arrangement of elsewhere useless parameters".
- **overprotection:** I believe that the impact of the two previous plagues would be weaker if the overprotection syndrome was not so deeply rooted among hydrologists. Most modellers are just too protective: many will agree on the usefulness of intercomparisons, but few will actually take part in them. Many will agree on the need for critical dialogue on modeling, but rare are those who will accept criticism without grudge… and actually modify their model if it proves to be less efficient than others

How to cure these three plagues?

Without pretending to propose a universal solution, I will present in this chapter some results that can effectively contribute to a cure. As much of my modeling research has focused on searching for the sensitivity of watershed models to input data quality, and to the treatment of spatial input information, I will start by giving a short background review of sensitivity analysis approaches. Then, I will successively address watershed model sensitivity to precipitation input (section 2.3), to discharge data (section 2.4), to potential evapotranspiration input (section 2.5) and to distributed input handling (section 2.6).

Last, in the next chapter, I will present my ongoing research as well as important scientific questions which I would like to address in the future.

2.2 The need for a global approach to model sensitivity analysis

Sensitivity analysis (SA) is viewed by many hydrologists as one of the essential components of modeling. Bergström (1991) for example, writes that "the analysis of model sensitivity is a very important component of a model development process. It helps to keep the model simple because it reveals model parameters with insignificant effect on the results. It is also a tool to identify interactions between model components and parameters". Saltelli et al. (2000) present an exhaustive list of practical reasons why a modeller may find it interesting to conduct sensitivity analysis. SA can help to investigate:

" (a) if a model resembles the system or processes under study;

(b) the factors that most contribute to the output variability and that require additional research to strengthen the knowledge base;

(c) the model parameters (or parts of the model itself) that are insignificant, and that can be eliminated from the final model;

(d) if there is some region in the space of input factors for which the model variation is maximum;

(e) the optimal regions within the space of the factors for use in a subsequent calibration study;

(f) if and which (group of) factors interact with each other. "

Before looking at how SA helps to diagnose the problems of watershed models, it is necessary to define it precisely, as some hydrologists have historically given SA a very narrow scope, restricting it to the investigation of the *parameters* that most contribute to the output variability (and not to the *factors*, which include both inputs and parameters as stated above). To make things clear, we adopt the definition by Saltelli et al. (2000), who define SA as "the study of how the variation in the output of a model (numerical or otherwise) can be apportioned, qualitatively or quantitatively, to different sources of variation, and of how the given model depends upon the information fed into it." I will show at the end of this chapter how looking at SA from this wider point of view allows us to reconsider the contradictory conclusions of previous SA studies on hydrological models (section 2.7).

2.3 Watershed model sensitivity to precipitation input

There are many studies on the impact of random or systematic errors in rainfall data on watershed model efficiency and parameter values. It is however surprizing to see how contradictory their results can be:

- Most authors, who used the classical Taylor series development approaches to sensitivity analysis (methods known as first-order variance estimation, or FOVE), expressed the view that rainfall input errors are integrally transmitted and sometimes amplified by the hydrological model (see on this topic Phanartzis, 1972; Mein and Brown, 1978; Paturel et al., 1995; Nandakumar and Mein, 1997)
- But some authors consider that hydrological models have a limited but real capacity to buffer these errors: see for example Dawdy and Bergmann (1969), Ibbitt (1972), and Troutman (1982, 1983).

The objective of the work presented in this section was to understand the reasons for such different and sometimes contradictory conclusions.

- ***Impact of Imperfect Rainfall Knowledge on the Efficiency and the Parameters of Watershed Models (Andréassian et al., 2001)***

In this paper, I investigated the sensitivity of watershed models to an imperfect rainfall knowledge. I looked at the impact on both model efficiency and model parameters. For this, I followed a new approach to sensitivity analysis, based on a comparison between the efficiency ratings and parameter values of the models and the quality of rainfall input estimate (quality was measured by two specific indexes). I used:

- data from three French watersheds of increasing size (71, 1120, and 10700 km²);
- three watershed models of varying complexity (3-parameter GR3J model and 6-parameter modified versions of TOPMODEL and IHACRES).

An original aspect of this study was that I introduced the GORE and BALANCE indexes to analyze the link between the performance of a rainfall-runoff model and the representativeness of the areal rainfall estimate used to run the model. The approach in constructing these indexes consisted in comparing the areal rainfall time series obtained with a large number of well distributed raingages with that obtained with a subset of the gages, and the indexes allowed me to measure how the subsets departed from the "true" rainfall input.

Table 2.1: description of the GORE and BALANCE indexes

Formula	Explanation
$GORE = 1 - \frac{\sum_{i=1}^{n}\left(\sqrt{P_i^E} - \sqrt{P_i}\right)^2}{\sum_{i=1}^{n}\left(\sqrt{P_i} - \overline{\sqrt{P}}\right)^2}$	This index is the transposition, in the rainfall domain, of the Nash and Sutcliffe criterion, computed with the square roots[7] of the variables. Like the Nash and Sutcliffe criterion, the GORE index can vary between -∞ and 1. When the estimated rainfall equals the reference rainfall, the GORE index is 1, its maximum value. Otherwise, the index is smaller than 1 and decreases as the estimates become poorer.
$BALANCE = \frac{\sum_{i=1}^{n} P_i^E}{\sum_{i=1}^{n} P_i}$	Quantifies the over- or under-estimation of the reference rainfall by a given raingage subset. The BALANCE index is greater than 1 in the case of rainfall overestimation and smaller than 1 in the case of underestimation.
P_i : "true" precipitation input to the watershed on day i P^E_i : estimate of precipitation input computed with a subset of the raingage network n: number of time steps of the period $\overline{\sqrt{P}}$: mean of the reference precipitation input over the study period	

The two indexes presented in Table 2.1 describe both the quality of rainfall time distribution and the total depth. These two aspects are very important for the hydrological model, which acts as a filter that transforms rainfall input, by modifying its time distribution and depth through its transfer and production functions. Therefore, these two easy-to-interpret descriptors can be quite useful for sensitivity studies.

Figure 2.1 presents an example of the use of the two indexes to analyze model performance sensitivity, and Figure 2.2 shows how they can be used to analyze model parameter sensitivity.

[7] The square root transformation on the rainfall is introduced to reduce the weight of extreme events; it is consistent with the transformation we usually made on streamflows before computing any goodness of fit criterion.

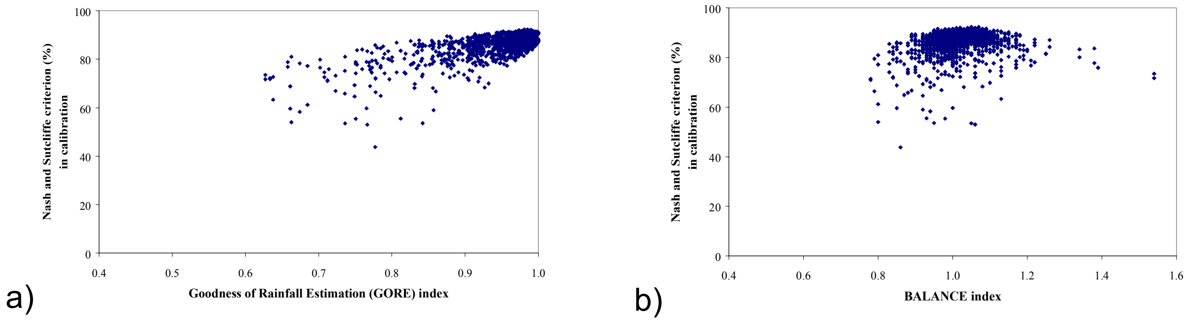


Figure 2.1: Impact of the quality of rainfall estimate on the performance of a rainfall runoff model - Nash and Sutcliffe criterion in calibration for GR3J vs GORE index values (a) and BALANCE index values (b). Results obtained for the Yonne River

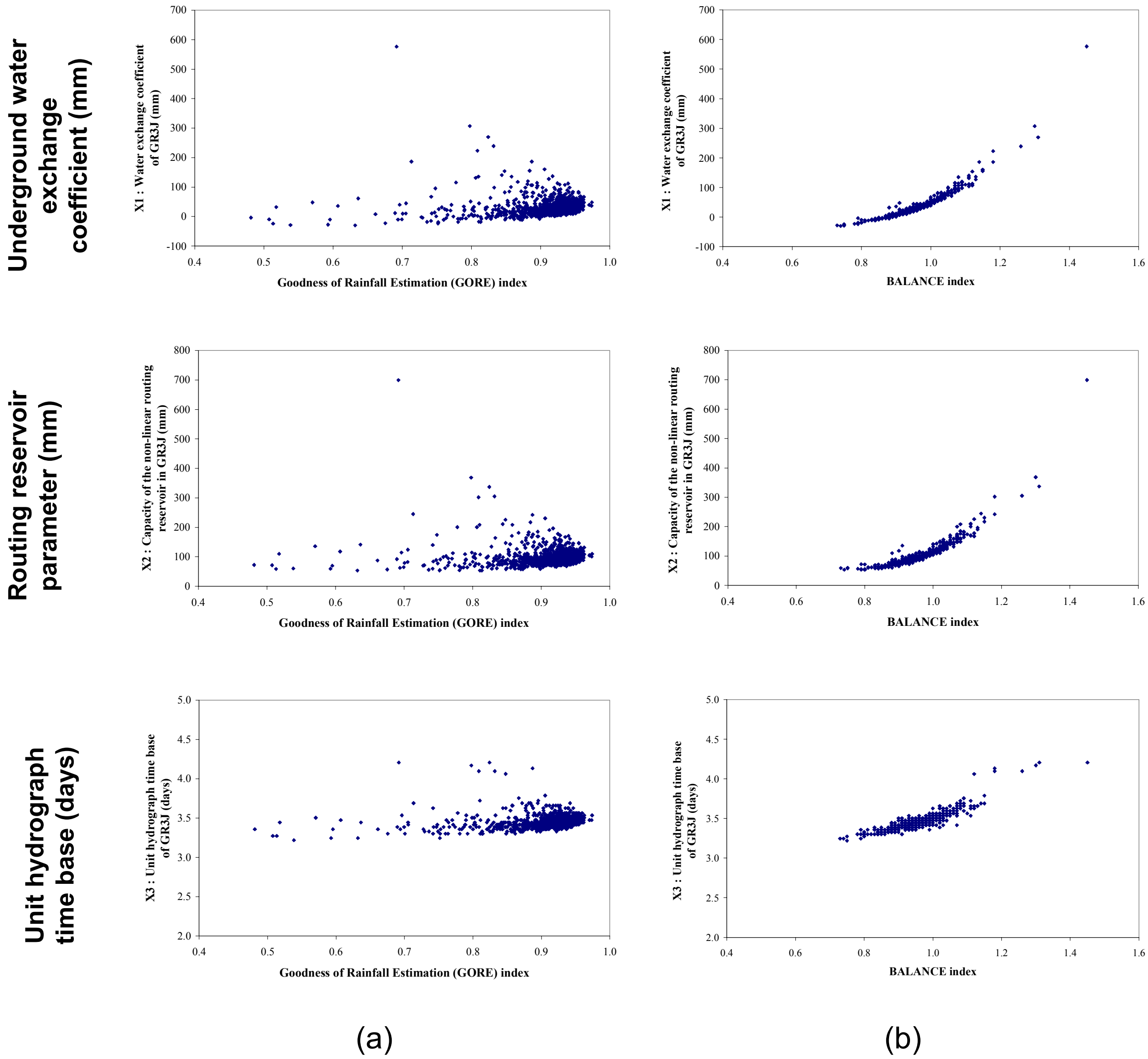


Figure 2.2: Relation between rainfall input estimation quality, as measured by the GORE (a) and the BALANCE (b) indexes, and the values of the three GR3J[8] parameters. Results obtained on the Serein River.

[8] This version of GR corresponds to GR4J where the Soil Moisture Accounting store has been fixed.

As can be seen in Figure 2.1, GR3J shows a quite remarkable ability to cope with imperfect rainfall estimates. Furthermore, the paper showed that both GR3J, IHAC, and TOPMO were capable of reacting to improved accuracy in rainfall input both by increased performance and reduced variability of efficiency.

Regarding parameter behavior, the results are not the same for all parameters and all models. I identified two different types of model behavior: the models either benefit from improved rainfall data by producing more consistent parameter values, or are unable to take advantage of the improvements. I suspect this second undesirable behavior to be the consequence of overparametrization but further research is needed to confirm it.

Regarding the spatial scale, an interesting feature of this sensitivity analysis is that the problem of imperfect areal rainfall estimates is not limited to large watersheds: indeed, on the Réal Collobrier watershed (71 km²), a single raingage provided a greater density per unit area than the 33 available raingages in the Yonne watershed (10700 km²) but proved insufficient to insure good modeling results. However, a larger basin sample would be needed to draw general conclusions concerning potential relationships between watershed size and the precision of areal rainfall estimates needed to insure good modeling results.

- ***Impact of rainfall errors on the efficiency and the parameters of watershed models (Oudin et al., 2005d)***

This paper explores the impact of both random and systematic data errors on the performance and the parameters of two rainfall-runoff models: GR4J and TOPMO. We decided to test two different rainfall-runoff model structures, in order to get a more general picture of the situation. Indeed, we thought that because of its underground exchange coefficient, GR4J may be somewhat more robust than to the other models. A sample of twelve US watersheds, made available by the MOPEX program, is used in the paper. The analysis covers model efficiency and optimized parameter value. To study the sensitivity of the two models to different data errors, our methodology consists in a progressive introduction of errors into input time series. We consider both random and systematic errors.

- **Random errors**

To assess the impact of random rainfall input errors on the value and the uncertainty of model parameters, we corrupted the measured input by a random noise:

$$P_j^* = P_j \cdot \exp\left(\sigma \cdot \eta_j - \frac{\sigma^2}{2}\right) \qquad \textbf{Eq. 2.1}$$

where P_j and P^*_j are, respectively, the original (measured) and corrupted rainfall at the day j, η_j is a Gaussian error and σ is the random error intensity coefficient (which makes it possible to test several levels of errors).

In the tests performed here, we varied σ from 0 to 0.5 (the chosen range for σ values was linked with the decrease in model efficiency). We scaled the precipitation time series in order to retain the same accumulated amount over the recorded period (so that we may study separately random and systematic errors).

The impact of random errors on model efficiency is illustrated Figure 2.3. There is a sharp decrease in model efficiency when random rainfall errors increase. For GR4J, the mean decrease in the Nash-Sutcliffe criterion for all the watersheds is approximately 35% from an error-free rainfall to a heavily corrupted rainfall (σ=0.5). This result is not surprising: it demonstrates that rainfall is a key climatic forcing input.

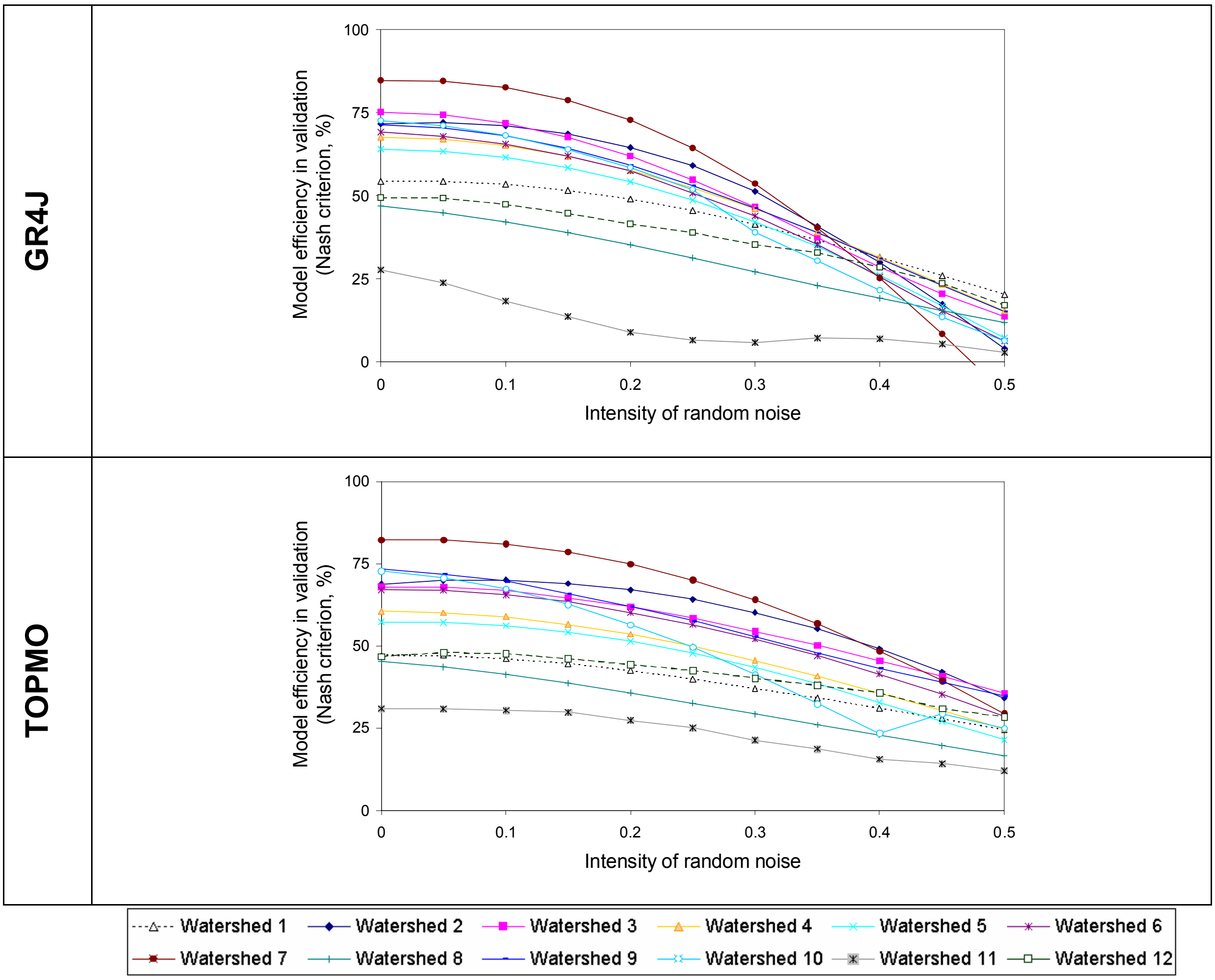


Figure 2.3: Impact of increasing rainfall random errors on the efficiency (in control mode) of the GR4J and TOPMO models over the twelve US basins (each color represents one basin).

The paper also covers the impact on model parameters, but the graphs are not showed here. The main conclusion is that the large model performance decrease noticed earlier translates into an important modification of most model parameters:

- In GR4J, though most model parameters (except θ4, the unit hydrograph time base) are modified, the most significant modifications occur on the capacity of the SMA store (θ1). This corresponds to an increased buffering capacity, in an attempt to limit the impact of rainfall errors on flow. The groundwater exchange coefficient (θ2) is here weakly modified. The increasing capacity of the routing store (θ3) also slightly contributes to buffer the effects of input errors.
- In TOPMO, a similar behavior can be observed, but most of the buffering role is played by the exponential SMA store and the associated functions (including parameters θ1, θ3 and θ6) (similarly to what happens in GR4J). Here, except in a

few basins, the routing store is not much influenced by the errors in rainfall input, and like in GR4J, the θ4 time delay parameter remains stable.

However, all these quite understandable adaptations of model parameters are not sufficient to let the models maintain their initial levels of performance, as seen previously, which confirms the difficulty for rainfall-runoff models to cope with such data errors in rainfall series.

- **Systematic errors**

To assess the sensitivity of GR4J and TOPMO to systematic rainfall input errors, we corrupted the measured input of the twelve watersheds by applying a multiplicative bias on time series. This multiplicative bias affected all the days of the recorded period:

$$P_j^* = k \cdot P_j \qquad \textbf{Eq. 2.2}$$

where *k* is a coefficient that makes it possible to test several systematic under- or over-estimates of P. When *k* was equal to unity, there was no corruption on P time series. Subsequently, we tested several corrupted P time series, with *k* ranging from 0.5 (half P) to 1.9 (almost twice P), for the twelve basins in our sample.

The impact of systematic errors is illustrated in Figure 2.4: it is probably the case where the two tested models showed the most different behaviors. When rainfall is systematically overestimated, the loss in model efficiency is rapid for TOPMO (most basins show a negative performance for multipliers greater than 1.5 for both criteria) while the GR4J model maintains a reasonable level of performance, with a limited drop of model efficiency (less than 5 % losses for both criteria). This behavior is coherent with what can be seen on the effect of a similar bias in PE (see section 2.5) TOPMO can cope with an overestimation of rainfall as far as evaporating at a potential rate is sufficient to lose excess water. Beyond this limit, the model cannot manage to ensure proper water balance. In contrast, the GR4J model uses its water exchange function to handle this water excess.

When rainfall is underestimated, both models show the same trend of progressive decrease in model efficiency, with a loss of about 15 % and 25 % for GR4J and TOPMO respectively. Here again the lesser loss of performance for GR4J is probably due to the water exchange function, that helps to maintain acceptable water balance while TOPMO fails to do so for too large rainfall underestimations (reducing much evapotranspiration losses – i.e. diminishing the actual rate of evapotranspiration – is not sufficient to close the water balance when the amount of rainfall becomes lower than the amount of actual flow).

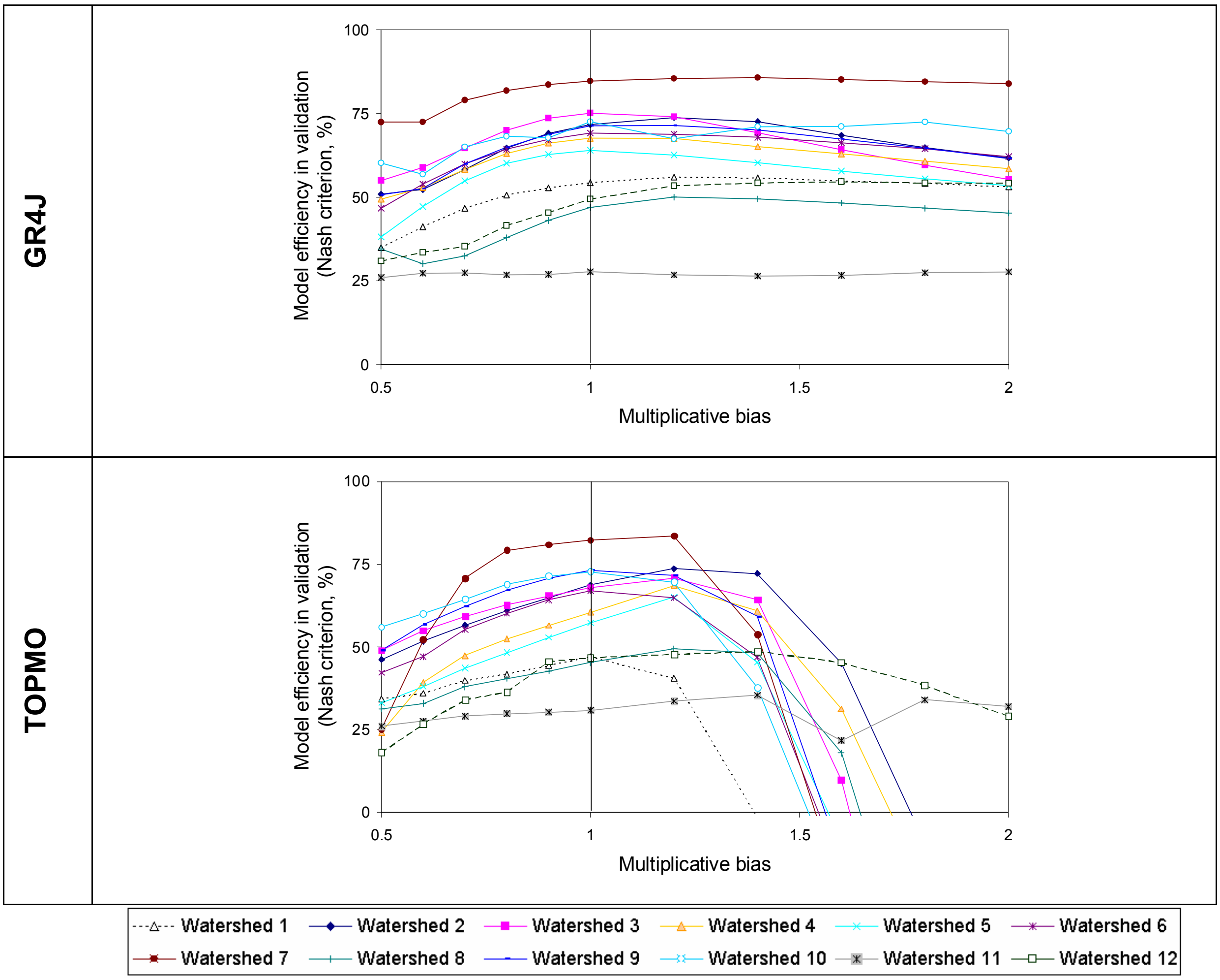


Figure 2.4: Impact of increasing rainfall systematic errors on the efficiency (in control mode) of the GR4J and TOPMO models over the twelve US basins (each color represents one basin).

The paper also covers the impact on model parameters, but the graphs are not showed here. The main conclusions are:

- for GR4J, a bias in rainfall is compensated by production parameters. The capacity of the production store (θ1) increases to cope with an overestimation of rainfall and decreases to cope with underestimation, thus adapting its capacity to hold and evaporate different amounts of water. One finds here a known result for this model, that the capacity of the production store depends partly on the annual amount of rainfall, as shown by Edijatno (1991). The model also adapts its water losses by the exchange term (θ2 drops to very negative values while θ3 increases progressively when rainfall is overestimated). Here again, the time base of the unit hydrograph (θ4), purely a routing parameter, is not much affected by the changes in rainfall amount since timing is not modified.

Note that the θ2 parameter does not have a symmetric behavior for under- and overestimation (like in the case of PE). This may be due to the place of the underground water exchange function in the model structure, which comes after the production store. This store plays the first buffering effect which is not symmetric since the store has, by nature, a lower bound. When rainfall is overestimated, the production store of GR4J can take in the incoming water in the limit of what can be evapotranspirated. Beyond this limit, accepting extra water would make the store continuously full, which would lead to an excess of net rainfall. Thus, the excess water goes to the routing part where the underground water exchange function acts and which is then in charge of evacuating excess water. Conversely, when rainfall is underestimated, the production store can easily accept water and adapt the actual rate of evaporation, therefore leaving only a secondary role to the water exchange coefficient.

- For TOPMO, the model reacts quite complementarily to the case of biased PE, by logically modifying its production parameters to help compensate for over- or underestimation of rainfall: e.g. when rainfall input increases, the capacity of the interception store (θ2) increases to raise evapotranspiration losses at a potential rate. Parameter θ6 value is also raised to compensate rainfall overestimation and make evapotranspiration always at a potential rate in the SMA store, similarly to the previous case of PE underestimation. However, one can see with TOPMO that some of the routing components are not insensitive to changes in rainfall input (see for example the quite erratic behavior of the routing store capacity when the bias becomes large): when bias becomes too large, the input errors become too strong to be handled by production functions, which has an impact on model routing components, especially on the model stores that receive too much (or too little) water. This partly explains the significant drop in model efficiency. Note however that like in GR4J, the time delay parameter (θ4) was not affected by this multiplicative bias in the precipitation, because the chronology of rainfall events was not modified by the input corruption scheme).

- **Synthesis on watershed model sensitivity to rainfall errors**

In this paper, the objective was to investigate the impact of systematic and random rainfall input errors both on the efficiency and on the parameters of the GR4J and TOPMO models. The main advantage of this extensive investigation, based on twelve watersheds representative of a wide range of climates, is that it allows a comparison of the relative impacts of several data errors.

Concerning the *random errors in rainfall*, we observed that this type of error is very detrimental to model performances, which significantly dropped for both models. The models try to exploit the buffering capacities of their production functions - especially their SMA store - to minimize the impact of these errors on flow simulation;

Concerning the *systematic errors in rainfall*, the models showed quite different results. TOPMO performances were drastically affected whereas GR4J better managed to adapt to this type of errors. Here, its underground exchange function let GR4J maintain an acceptable water balance while the TOPMO model was not able to do so.

2.4 Watershed model sensitivity to discharge data

This section is based on a submitted paper entitled *A data resampling approach to assess parameter uncertainty in continuous watershed models* (Perrin et al., 2004), as well as on the (ongoing) PhD research of Claudia Rojas-Serna.

- ***What does data subsampling approaches teach us about watershed model sensitivity to available discharge data? (Perrin et al., 2004)***

The main topic of this paper is to demonstrate the possibility and the advantages of using a non parametric approach to estimate model parameter uncertainty. But a preliminary result relates to model sensitivity to discharge data, and this is what we will focus on here. All hydrologists have noticed that a model calibrated on the same watershed but on two different calibration periods may not yield the same optimum parameters (see e.g. Beven, 1993). This is easy to interpret from a systems theory point of view: as the model did not receive the same quantity of information in the different data sets, it did not converge towards the same optimum. In order to investigate the sensitivity of parameter sets to discharge data, the easiest way is to calibrate the model on successive periods. But this method has its drawbacks: if one wishes to ensure a sufficient amount of information to the model, the calibration period needs to be long enough, in which case the successive periods will be too few to be analyzed statistically.

With the subsampling calibration approach proposed here, one can avoid this drawback: it consists in deciding on a number n of runoff measurements needed for a sound, well-balanced model calibration (here, we will arbitrarily set this value at n=365), and then in selecting randomly within the whole record period the n days that will be used for the computation of the objective function during optimization. Thus, the model can be calibrated on the whole sample (with varied climatic conditions) while it actually sees only part of the sample (Figure 2.5), with a limited information content. The validation period can be the same for all the calibration periods, so that model efficiency in control is perfectly comparable (indeed, if the total number of time steps is large in comparison to the number n used for calibration, we can just validate on the whole time series). And drawing a number N of different subsamples (e.g. N=100), one can assess statistically the sensitivity of a watershed model to the information content of the samples.

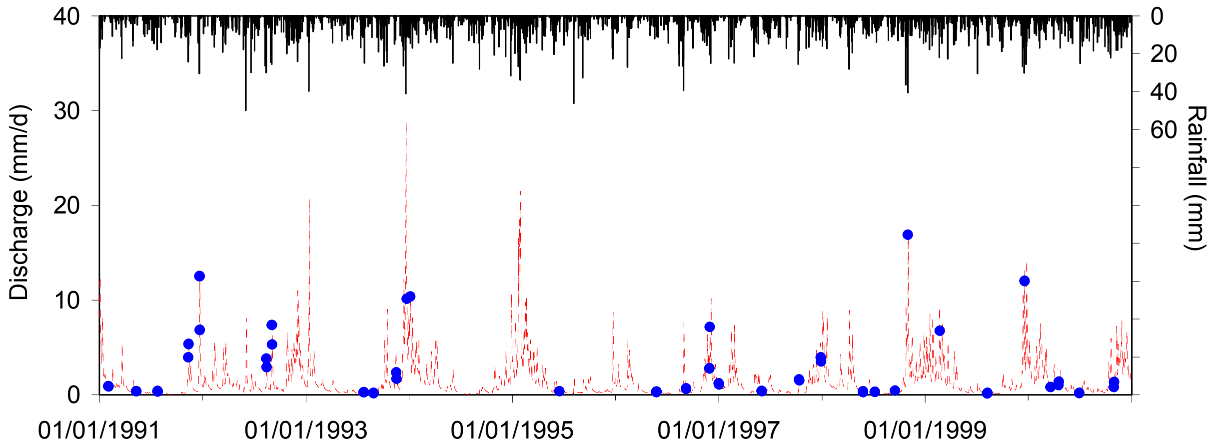


Figure 2.5: subsampling of data made available to the watershed model for calibration: only those days represented by a ● are used to compute the cost-function during calibration. The rest of the discharge time series remains hidden.

- **Model efficiency**

In this paper, we use the twelve US watersheds discussed earlier, and we show in Figure 2.6 the impact of the number of calibration days on model efficiency: we vary the number *n* of days seen by the calibration between 1/8th of a year and 8 years. The effect on calibration is small (except for a slight tendency to decrease mean efficiency when n increases, simply because there are more points in the curve to be fitted). But in validation mode, there is a strong increase of mean efficiency when *n* increases, which means that the model makes better and better simulations when it is provided with more and more information to characterize basin behavior.

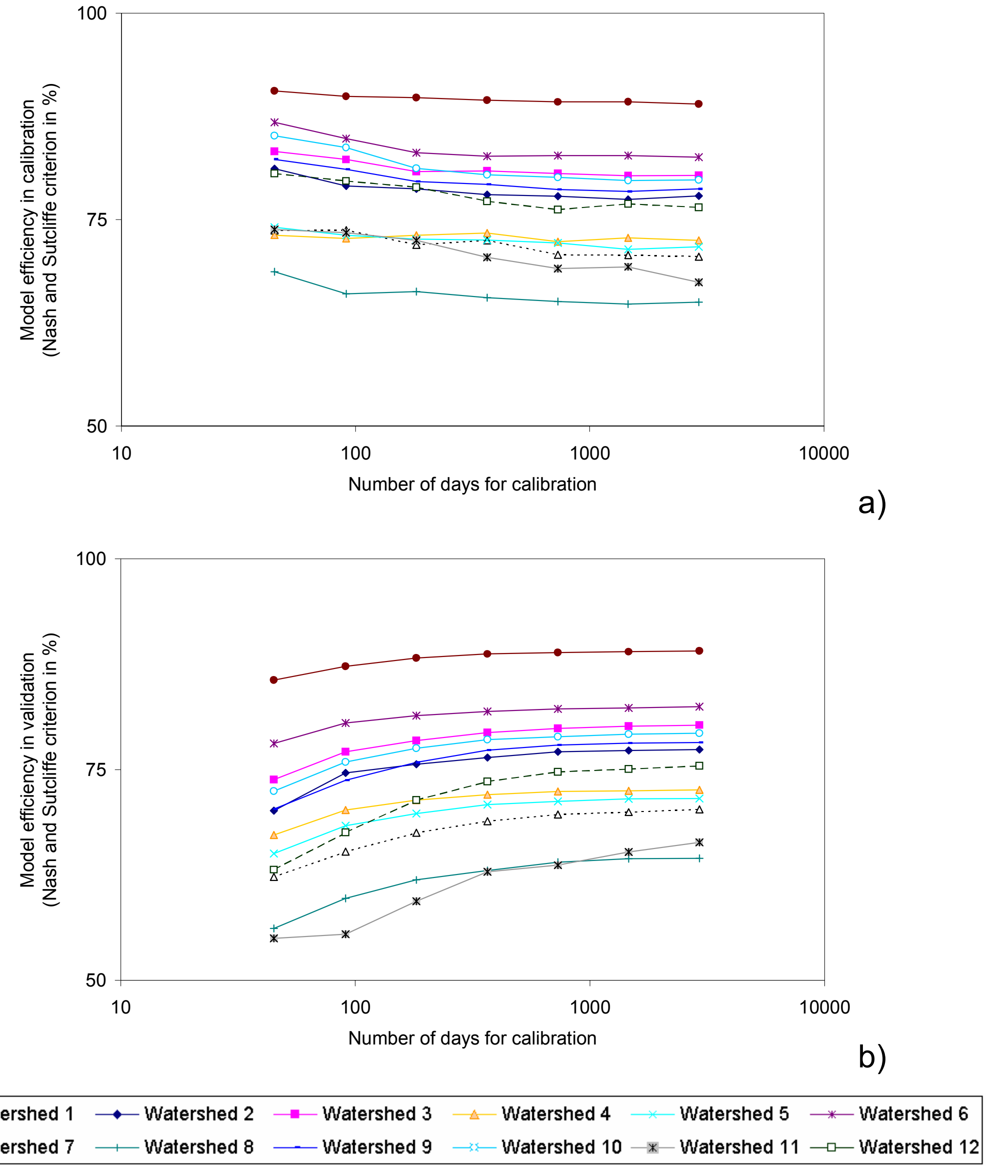


Figure 2.6: evolution of mean model efficiency with the number n of days available to calibration: (a) calibration mode, (b) validation mode

Figure 2.8 shows the range in performance obtained for the 12 watersheds in calibration and in validation mode. The variability in performance in calibration mode can be quite high, but this is primarily due to the fact that the calibration periods are short and can include very different climate events. Looking at the results in validation mode, one can observe that they are much less variable, with less dispersion around the mean value. Interestingly, there is no case of large model failure (negative efficiency). This indicates that the parameters determined over the 365 days are quite robust and able to give a good representation of the characteristics of watershed behavior, though the information available for model calibration was scarce. We believe that this robustness comes firstly from the small number of free parameters in the GR4J model structure. Note however that for the watersheds with low runoff yield (e.g., watersheds 11 and 12 in Texas), the performance variability in validation mode is greater. More generally, it seems that this variability decreases when the watershed yield increases (Figure 2.7). There are two possible explanations:

(i) it could be that the random data sampling on low-yielding watersheds is less informative, given the many cases of almost zero streamflow and the low probability of having flood events in the calibration sample. This would illustrate that on these complex, semi-arid watersheds, the calibration requirements are higher than on the wetter basins;

(ii) but it could also simply be an artifact of the Nash and Sutcliffe criterion. Perrin (2000) showed that this criterion gives always low ratings to low-yielding watersheds, since for them, the reference model which is inherent in the criterion formula ($Q_i = \overline{Q} \quad , \forall i$) is already fairly close to reality.

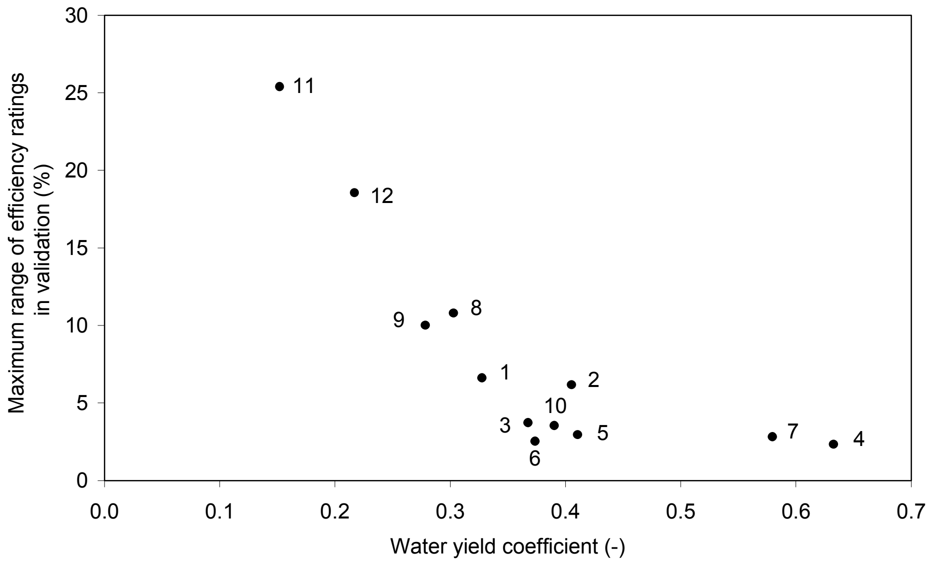


Figure 2.7: Relationship between watershed water yield (mean runoff divided by mean rainfall) and the range of model efficiency in validation mode for 12 test watersheds (the number next to each point refers to the watershed)

- **Model parameters**

The parameters obtained by calibration on the 100 sub-samples can be compared first to those obtained by calibration on the whole period of record which can be used as a reference. Figure 2.9 shows that the median values are very close to the reference values: this indicates that, on average, a small amount of information does not bias the estimation of model parameters. But the variability of model parameters over the 100 calibration runs can be quite large on some watersheds: this confirms that parameter values depend on the available calibration data. However, given the steady mean efficiency ratings shown in Figure 2.8, this parameter variability does on average not reduce model robustness too much: with at least a year (i.e. 365 values) of data, the calibration phase manages to capture the main characteristics of watershed behavior.

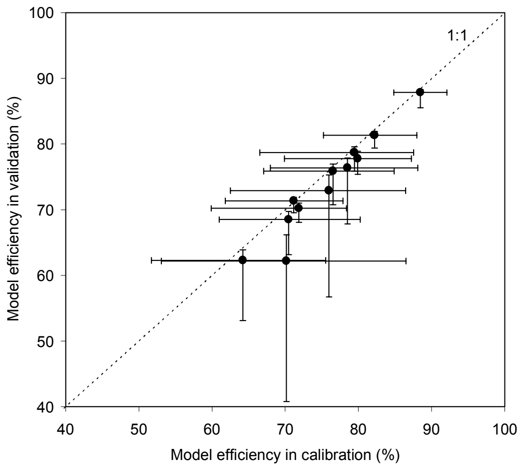


Figure 2.8: Ranges of Nash-Sutcliffe criteria (minimum, mean and maximum values) obtained in calibration and validation for the 12 watersheds (results based on 100 different calibration sub-samples)

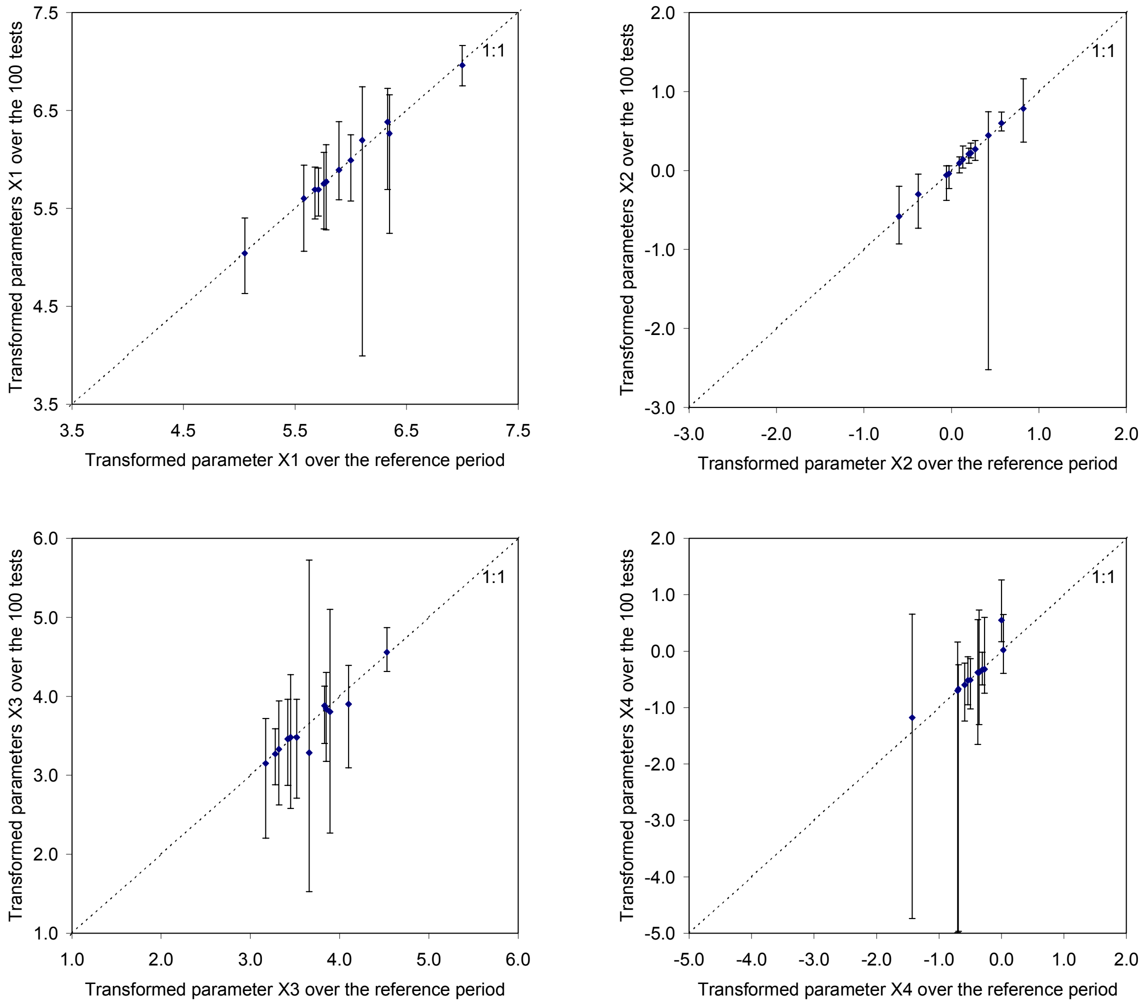


Figure 2.9: Comparison of transformed parameter values obtained by calibration on the reference period of the record (39 years) and median values obtained with the 100 calibration runs on 365 days for the 12 watersheds (with minimum and maximum values)

❑ *What are the minimum discharge data required to estimate the parameters of a watershed model? (PhD thesis of Claudia Rojas Serna)*

I believe that the method presented above is of great theoretical value. But how can we use it to produce results of operational interest? Indeed, what we are interested in (usually, when we do not have any discharge data for the watershed we are studying) is an answer to the following question: what is the minimum quantity of discharge data that we need to collect in order to be able to estimate the parameters of this watershed model?

This is precisely the question we try to answer in the ongoing PhD research of Claudia Rojas-Serna. To address this question, a large world-wide sample (more than 1100 units) of basins has been gathered with rainfall-runoff data at a daily time step. Sub-sampling is used, as presented above, with the difference that the number *n* of days retained to calibrate the watershed model is not fixed: we investigate much lower values of n and we focus on sampling strategies allowing us to keep this number as low as possible. There, we have reached the boundary between gaged and ungaged watersheds, and this is why we also try to make use of the *a priori* parameter estimates that may exist on each watershed. And even if we know that most often these estimates are of quite poor quality, we believe that they may contribute to improve model reliability and robustness and to reduce the uncertainty of parameter estimates when there is very little discharge information available for calibration.

To combine these sources of information, Rojas-Serna (ongoing) introduced a weighted objective function for calibration defined as:

Weighted criterion = α.(parameter deviation from the regionalized values) + (1-α).(sum of errors on the *n* observed streamflow points) Eq. 2.3

where we express parameter deviation (from their *a priori* values) as:

- deviation= $\sum_{k=1}^{p} \frac{\left(\theta_k - \theta_k^0\right)^2}{\sigma_k^0}$ Eq. 2.4

with:

p = total number of model parameters (θ)

θ_k^0 = a priori value of parameter θ_k

σ_k^0 = a priori standard deviation of θ_k values

and where the sum of errors on the *n* known discharge points is expressed as:

- streamflow errors= $\frac{\sum_{k=1}^{n}\left(Q_k - \hat{Q}_k\right)^2}{\sum_{k=1}^{n}\left(Q_k - \overline{Q}\right)^2}$ Eq. 2.5

The reason why we introduce the weight α is that the weighted criterion increases when we depart from the regionalized values, but decreases when errors computed on the (few) known points diminish. It is not possible to tell beforehand how these two components must be balanced: this is why we decided to study a range of solutions for α (see Figure 2.10).

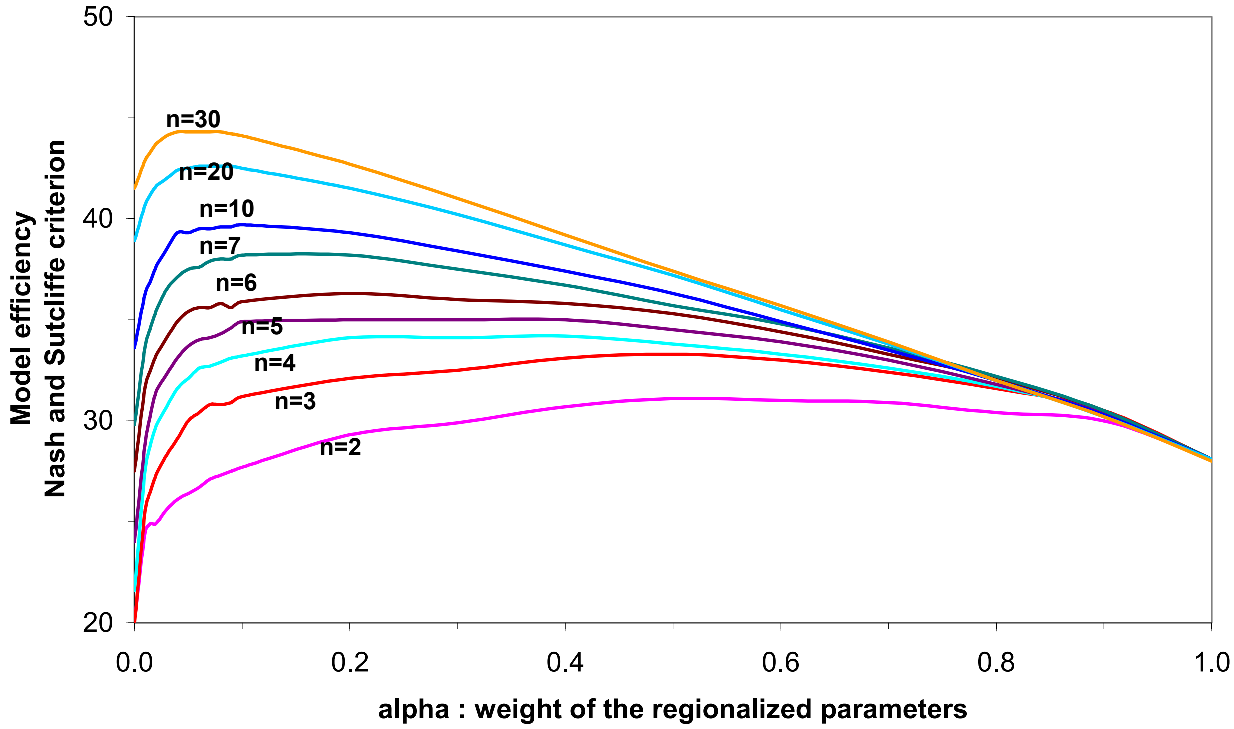


Figure 2.10: mean model efficiency reached on a sample of 1000 basins for several *n* of known discharge points. The x-axis represents the weight α given to the regionalized parameters in the calibration criterion.

The preliminary results presented in Figure 2.10 have:

- ***a very promising side:*** it seems possible to reach “acceptable” model efficiencies with quite a low number (*n*) of known discharge points. This opens the door for a possible sampling strategy where gaging teams could be sent out a realistic number of times into the field to acquire the necessary discharge data;
- ***a quite reassuring side:*** the regionalized model parameters are of some interest (even if they give very low efficiencies when used alone) and can be usefully combined with point gagings, as long as *n* is small. When the knowledge of the discharge becomes more extensive, the regionalized data progressively becomes less useful, as measurements become richer in information.

Presently, our research continues, with the aim of confirming preliminary results on the whole (> 1100) basin sample and establishing catchment-specific discharge sampling strategies.

2.5 Watershed model sensitivity to potential evapotranspiration input

❑ *Literature*

It is enough to look at the conclusions of those who have worked on the topic to understand that watershed model sensitivity to potential evapotranspiration is a quite controversial subject in hydrological modeling:

- One of the first studies of watershed model sensitivity to errors in potential evapotranspiration input was published by Parmele (1972): he concluded that “a bias in PE input data has a cumulative effect and results in considerable error in the computed hydrograph”, whereas the influence of random errors is generally not measurable.
- Andersson (1992) used the HBV watershed model and compared seven different methods of computing PE input. He used the same set of calibrated parameters in each case, allowing only the *precipitation* correction factor to compensate for PE over- or underestimates to obtain, over the calibration period, the same total runoff amount for all the formulae. Expressed in terms of model efficiency, the differences between methods were very small.
- Joukainen (2000) also used the HBV model and modified the routine for computing actual evapotranspiration (AE) from PE to achieve a better representation of rainfall interception by trees, adding eight new parameters to the model. She found a very slight improvement in the calibration results, although she nearly doubled the degrees of freedom of the model. Clearly, the HBV model is not sensitive to such refinements of its AE computation routine.
- Paturel et al. (1995) assessed the sensitivity of the GR2M monthly watershed model to systematic PE errors. Initially using the same approach as Parmele (without parameter recalibration), they found that, compared to errors in rainfall, systematic PE errors induced much smaller output errors. Then they studied the ability of the model to compensate for errors by calibration. They concluded that watershed models have a certain capacity to “absorb systematic input errors.”
- Nandakumar and Mein (1997) studied the effects of random systematic errors in pan coefficients and model parameters on the predictions of a rainfall-runoff model. They found a significant impact of PE errors (10% bias in PE can cause

up to 10% bias in runoff predictions), although these errors do not have as great an effect as those in the rainfall estimate.

- In an exercise aimed at demonstrating the adaptive ability of the IHACRES conceptual watershed model, Kokkonen and Jakeman (2001) modified the formulation used to compute AE from PE. The modification resulted in an increased difference between evapotranspiration losses as computed by the model: values were much higher in summer and much lower in winter. However, this modification did not affect the ability of the conceptual rainfall-runoff model to adequately represent the rainfall-runoff relationship. This is another example of the adaptability of watershed models: they can use some of their internal degrees of freedom to balance the excessive amplitude of evaporative losses and produce acceptable streamflow simulations.
- Vazquez and Feyen (2003) tested three different PE formulations as input to the MIKE-SHE model and calibrated this model with each of the formulae. The authors report large differences, not only in control mode, but also in calibration mode. These substantial differences seem rather surprising, since the model should have enough degrees of freedom to adapt to differences in PE estimates.

Obviously, the authors cited above do not agree in their conclusions, and one of the objectives of the research undertaken on this topic was to understand why we could have such diverging views on a subject which was, a priori, simple.

- ***Impact of imperfect potential evapotranspiration knowledge on the efficiency and parameters of watershed models (Andréassian et al., 2004c)***

In a recent paper (Andréassian et al., 2004c) we studied the impact of imperfect PE knowledge on model efficiency and parameters. We tested the impact of five improved spatial estimates of PE on the efficiency and the parameters of two rainfall-runoff models on 62 French watersheds. In contrast to most of the previous studies, we found model efficiency to have very little sensitivity to PE input, as calibration allowed the parameters to compensate for most of the PE biases. Only the most extreme under- and overestimation scenarios had a detrimental effect on model efficiency.

This study is of particular interest, because it is one of the rare “failure stories” published in the hydrological literature, which potentially has more to teach us than the usual “success stories” we are used to. It started from the observation that previous studies at Cemagref (Edijatno, 1991; Kribèche, 1994) had shown a very low sensitivity of watershed models to PE input, with similar results published by Burnash (1995) and Fowler (2002): I believed that these results could be due to the coarse spatial resolution of PE data. Indeed, since PE models such as Penman's require detailed meteorological data, they were usually computed only at synoptic meteorological stations. The synoptic network is quite sparse, especially in semi-mountainous areas where I suspected that the location of the meteorological stations (usually on low-lying airport sites) introduced a systematic bias in PE estimates.

I used a dense network of recently installed automatic weather stations to obtain a quite successful regionalization of Penman PE over the Massif Central highlands of France... but this *success* turned into a *failure* when I tried to check whether watershed models were actually benefiting from the success of PE regionalization. I found out that watershed models were in fact perfectly insensitive to refinements of their PE input, and I was able to show how calibrated parameters were adapting to the differently estimated PE.

- ***Impact of PE errors on watershed model efficiency and parameter uncertainty (Oudin et al., 2005d)***

I have already commented on part of this paper in section 2.3, as we investigated the sensitivity of watershed models to rainfall input on a sample of twelve US basins. We used the same catchment sample, the same two watershed models (GR4J and TOPMO), and looked successively at random and systematic errors.

- **Random errors**

To investigate the impact of random PE errors, we had to work a little differently than for rainfall, as uncorrelated random errors would probably not have any effect on simulations: the SMA store of watershed models has a definite buffering capacity. Thus, when corrupting PE data, we applied the random noise to a whole month each time, as follows:

$$PE_j^* = PE_j \exp\left(\sigma \cdot \eta_m - \frac{\sigma^2}{2}\right) \quad \textbf{Eq. 2.6}$$

where PE_j and PE^*_j are, respectively, the original (measured) and corrupted PE on day *j*, η_m is a standard Gaussian error applied for all the days of the month *m*, and σ is the random error intensity coefficient (the parameter which makes it possible to test the effects of an increasing PE error). When σ was equal to zero, there was no corruption of the PE time series. In the paper, we tested increasingly corrupted PE time series, with σ ranging from 0 to 0.8, for twelve US basins. As we aimed to test only the relative magnitude of random PE fluctuations, all the corrupted PE time series used to feed the model were made to have exactly the same long-term mean as the original time series.

The impact of an increasing random PE error is illustrated in Figure 2.11: for both GR4J and TOPMO, increasing random PE errors yielded an almost insignificant loss of model performance (less than 2 % of Nash-Sutcliffe criterion and about 1 % of balance index) from an error-free PE to a substantially corrupted PE. These results confirm previous results that brought out the relatively poor sensitivity of watershed model efficiency to random errors on PE, (Parmele, 1972; Paturel et al., 1995; Andréassian et al., 2004c; Oudin et al., 2005b) mainly because of their low pass properties (see Oudin et al., 2004a).

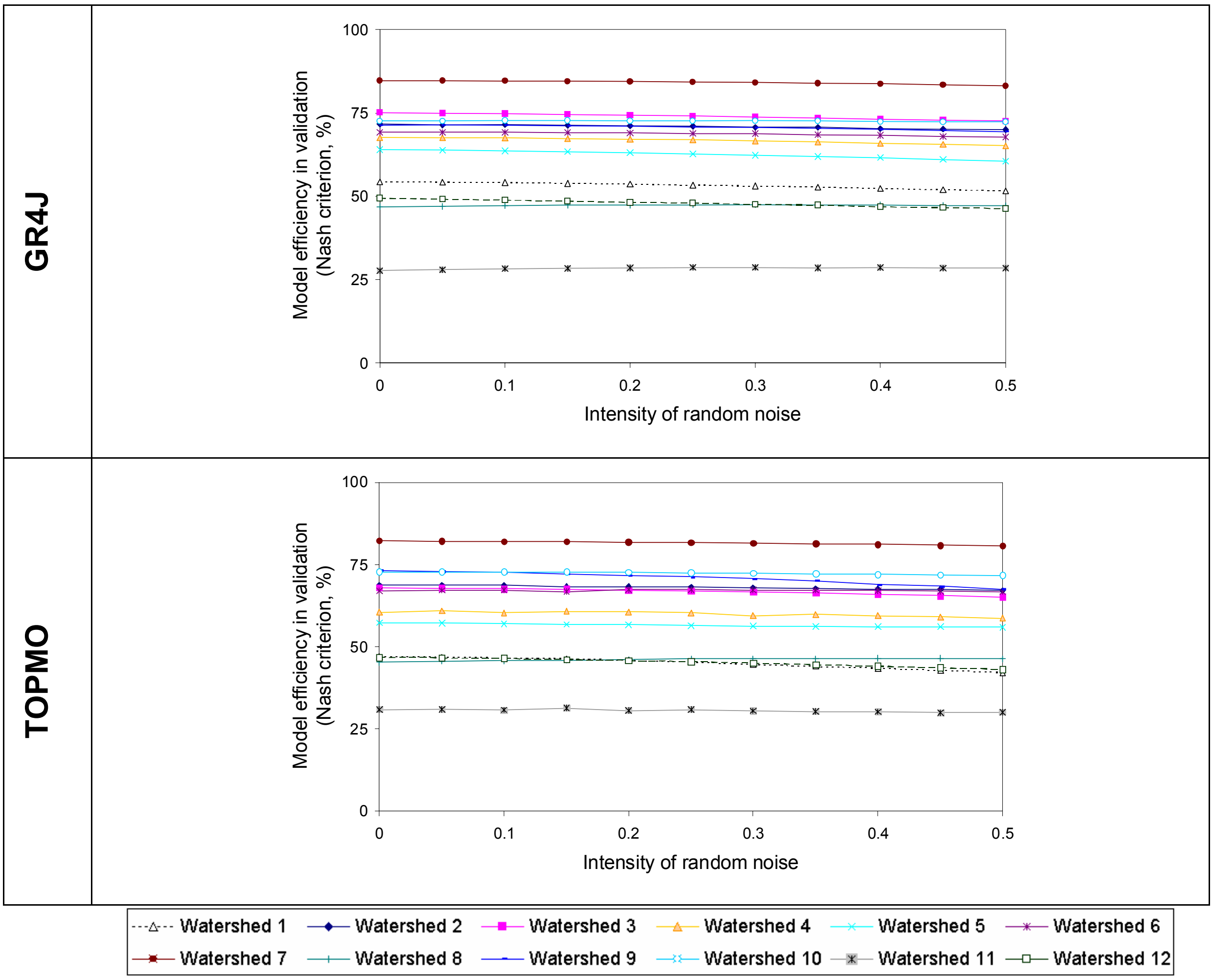


Figure 2.11: Impact of increasing PE random errors on the efficiency of the GR4J and TOPMO models over the twelve US basins (efficiency in control mode).

The paper also covers the impact on model parameters, but the graphs are not showed here. For both models, parameter values are very stable. When the noise in PE series becomes large, a slight impact can however be observed on some of the production parameters (θ1 and θ2 for GR4J; θ2 and θ6 for TOPMO). This is in agreement with the stability of models performances and corroborates previous findings (see e.g. Oudin et al., 2004a) that rainfall-runoff models intrinsically behave like low pass filters. They smooth here all month-to-month variations introduced by the error model without modifying much their parameter values.

- **Systematic errors**

To assess the impact of systematic PE input errors on the value of GR4J and TOPMO parameters, we corrupted the measured input of the twelve watersheds used in this study by applying a multiplicative bias on the PE time series. This multiplicative bias affected all the days of the recorded period:

$$PE_j^* = k \cdot PE_j$$ **Eq. 2.7**

where *k* is a coefficient which makes it possible to test several systematic under- or over-estimations of PE. When *k* was equal to unity, there was no corruption on PE time series. Subsequently, we tested several corrupted PE time series, with *k* ranging from 0.5 (half PE) to 2.0 (twice PE), for the twelve basins in our sample.

The impact of an increasing random PE error is illustrated in Figure 2.12. Compared to the very limited impact of PE random errors, the systematic errors in PE data had a somewhat more substantial impact on the estimation of river flow for both models. This shows that the overall estimation of the total PE amount is more important than month-to-month precision on PE estimates. We observe that the overall impact on model performance was quite similar for both models when PE is overestimated: the Nash-Sutcliffe criterion and the balance index show a slow decrease, with a limited drop of about 10 and 5 % for these two criteria. When PE is underestimated, both models show a loss of performance, which is however more important for TOPMO than for GR4J: TOPMO loses 15 % in Nash-Sutcliffe criterion and 30 % in balance index while GR4J losses are smaller than 5 %. The differences of behavior between the two models are much likely coming from the additional feature provided to the GR4J model by its water exchange functions to adapt balance. It offers to the model a possibility to adjust much more straightforwardly the water balance, which is here highly modified in comparison with the reference case with initial data.

If we restrict our analysis to reasonable systematic errors (say within +/- 30%), the model performances were generally more affected when PE was over-estimated than when PE was under-estimated. The rainfall-runoff models seem to better cope with a lesser quantity of PE, which is supported by both theoretical (Morton, 1983) and modelling investigations (Oudin et al., 2005a). However, when the underestimation of PE becomes too strong and the models have no other means to lose water than by evaporation (i.e. when even evaporating always at a potential rate is not sufficient to lose enough water) like TOPMO, the drop in model efficiency can be quite large since the model is not able to predict the right streamflow amount any more. By contrast, when PE is largely overestimated, the model can almost always adapt its production function to reduce the actual rate of evapotranspiration.

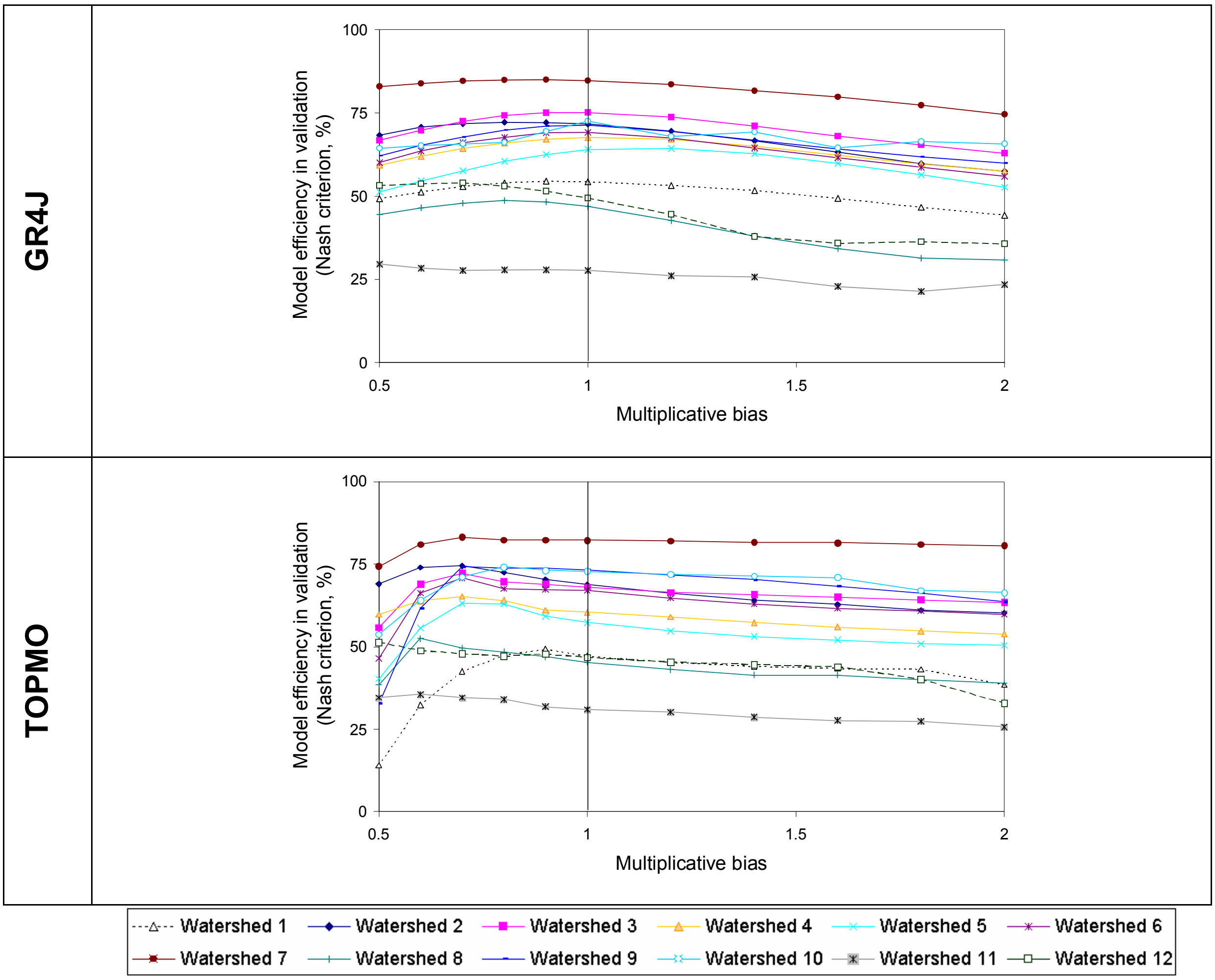


Figure 2.12: Impact of increasing PE systematic errors on the efficiency of the GR4J and TOPMO models over the twelve US basins (efficiency in control mode).

The paper also covers the impact on model parameters, but the graphs are not showed here. As already noticed when analyzing model performance, the behavior of model parameters is here very different from the case with random errors on PE:

- For the GR4J model, the modifications of PE inputs can be accounted for either by changing the basis for calculation of actual evapotranspiration (parameter θ1, SMA store capacity) or by modifying the water exchange term (θ2). Here the water exchange coefficient (θ2) is the most heavily modified. A systematic increase of PE resulted in an increase of θ2, i.e. the model simulated increasing groundwater inflow to the catchment, to compensate for the excessive evaporative demand. Conversely, water exports (negative θ2) increased when PE was under-estimated. Given the mathematical expression of the water exchanges in GR4J that depend on the filling rate of the routing store (R/ θ3), there was logically a progressive decrease in parameter θ3 (routing store capacity) when PE increased, to help increase groundwater exchanges (a peculiarity of GR4J already discussed by Andréassian et al., 2004). The SMA store capacity (θ1) was little affected by systematic PE errors.

- In TOPMO, as could be expected, all production parameters (θ2, θ3 and θ6) are affected by the changes in PE. The increased capacity of the interception store when PE is underestimated let the model evaporate more water at a potential rate. The progressive decrease of the θ6 evaporation coefficient when PE increases is also a solution for the model to limit the actual rate of evaporation. Note that θ6 reaches very high values (at the boundary of the calibration domain) when PE is underestimated, which corresponds to making evapotranspiration always at a potential rate. The corresponding increase in θ3 (parameterization of split of net rainfall) avoids that too much water enters the SMA store, therefore also contributing to limit water losses by evaporation when PE increases. Comparatively, the routing parameters are quite stable (like the unit hydrograph parameter in GR4J), which is logical since they do not intervene in the determination of water volume distribution within the model. As mentioned in the analysis of model performance, the compensations allowed by model parameters do not let the model maintain its performances level when PE is underestimated once the limit behavior of always evaporating at a potential rate is reached; then the model has no solution to lose excess water.

- **Synthesis of watershed model sensitivity to PE errors**

In this paper, the objective was to investigate the impact of systematic and random PE input errors both on the efficiency and on the parameters of the GR4J and TOPMO models. The main advantage of this extensive investigation, based on twelve watersheds representative of a wide range of climates, is that it allows a comparison of the relative impacts of several data errors.

Concerning the *random errors in PE*, we observed that both models were almost insensitive to random errors in PE series. This seems to be the common rule for soil moisture accounting rainfall-runoff models, that behave like low pass filters (see

Oudin et al., 2004a). Therefore, this type of error did not induce significant modifications in the model performances nor in the parameter values;

Concerning the *systematic errors in PE*, there was a moderate impact of this type of error on model performances. The models use their production functions as buffers to compensate for over- or underestimations of PE. Here the GR4J model, that has a groundwater exchange function, proved more flexible to adapt to large PE underestimations, while maintaining an acceptable water balance, contrary to TOPMO.

- ***Search for a potential evapotranspiration model suitable as input to a lumped rainfall-runoff model (Oudin et al., 2005a)***

The objective of Oudin's PhD research was to improve the performance of rainfall-runoff (RR) models due to a better representation of Potential Evapotranspiration (PE). To this end, he put together a large sample of catchments, encompassing different hydro-climate conditions. Streamflow, rainfall and climate data were collected for 308 catchments located in France (221), North America (79) and Australia (8). The advantage of working on a large sample is that the resulting conclusions are relatively free from dependence on any specific catchment characteristics. Moreover, we used four lumped RR models (GR4J and modified versions of IHACRES, HBV and TOPMODEL).

Initially, a sensitivity analysis of these models to PE was carried out. Two main aspects were investigated:

- First, the possible superiority of a precise knowledge of daily PE over averaged data (interannual averages of PE) was checked: results confirmed the lack of sensitivity of RR models to day-to-day fluctuations of PE. Using a regime curve is as efficient (in terms of flow simulation quality) as using detailed PE knowledge.
- Second, a large number of formulae were tested to represent the variations of PE as input to RR models: results showed that RR models are little sensitive to the choice of the PE formulation. In this context, and from a downard/empirical modeling point of view, the wide use of the Penman formulation, based on four climate parameters, is questionable: after all, simple formulae using only air temperature data yield as satisfactory flow simulations as the Penman formula, which may not be the best suited for a PE estimation at the catchment-scale. A

simple PE formula, based only on catchment latitude and long-term averages of air temperature was proposed. This formula (Eq. 2.8) provided a small but significant improvement of the performance of the four RR models over the 308 watersheds.

$$PE = \frac{R_e}{\lambda\rho}\frac{T_a+5}{100} \qquad if \quad T_a+5>0$$
$$PE = 0 \qquad otherwise$$
Eq. 2.8

where:

PE is the rate of potential evapotranspiration (mm day^{-1}), R_e is extraterrestrial radiation (MJ m^{-2} day^{-1}), λ is the latent heat of vaporization in (MJ kg^{-1}), ρ is the density of water (kg m^{-3}) and T_a is daily air temperature (°C), derived from long-term average.

From an operational point of view, these conclusions are very reassuring because one can easily obtain mean temperatures at many locations. Thus, it will be much easier to obtain basin-scale representative PE estimates with temperature-based methods than with Penman-type methods, for which values are often extrapolated from distant meteorological stations. However, from a modeling point of view, these results are disconcerting because they suggest pushing aside the practice of many hydrologists who use Penman's formulation with daily time-varying data. Moreover, models seem to favour extremely simplified representations of the climate information, a rather surprizing finding.

To try to understand the origin of model *indifference* to PE input, a method allowing to track in detail the treatment of PE input within the structure of the RR models was introduced (Oudin et al., 2004). Results confirm the insensitivity of RR models to PE, and show that this can be explained by the fact that the model production (soil moisture accounting) store act as a low-pass filter, smoothing the effect of daily PE fluctuations. In the past, several observational, theoretical and computational studies have investigated how the soil layer acts as an integrator of short-term atmospheric anomalies. The conclusions of Oudin's PhD thesis substantiate these findings. And thus, we believe that the insensitivity of model to erroneous PE is only the reflection of an intrinsic property of watershed systems.

2.6 Watershed model sensitivity to distributed rainfall input: the chimera watershed approach (Andréassian et al., 2004b)

The last sensitivity study I will report here is linked to two recurrent questions of watershed modeling (see for instance Osborn et al., 1982): how should we account for the distributed aspects of hydrological functioning? How sensitive are hydrological models to spatial distribution issues? The literature on this question has already been reviewed in detail in section 1.2. Therefore, I will proceed directly to the presentation of a method I have introduced to investigate in a systematic way the sensitivity of hydrological models to the spatialized accounting of model inputs and parameters.

Ideally, if we are to conduct a proper sensitivity analysis, we need:

- to be able to distribute both inputs and parameters within the same model (and distributing parameters means that we need to be able to calibrate them, i.e. we need at least three gaging stations as presented in Figure 2.13-a);
- to imagine a scheme where *exactly* the same model can be used in distributed and lumped modes.

Baudez (1997) looked for streamflow gaging station triplets, but he could find only 15 in the French hydrometric network. And his results were quite surprizing: the model seemed insensitive to spatial distribution, as both lumped and semi-distributed strategies seemed equivalent, and no explanation could be found as to which approach was preferable on a given catchment (Loumagne et al., 1999). For a thorough sensitivity analysis, what was needed was a test based on a large number of watersheds, and in particular, watersheds with quite contrasted subbasins (as I believed initially that the homogeneity of subbasins in Baudez's sample was one of the reasons for his surprizing conclusion).

This is why I thought about simplifying the scheme in Figure 2.13-a:

- First, I relaxed the requirement of the third gaging station, considering that the test could be conducted on sums of upstream flows rather than on flows actually measured at a confluence: with this strategy, one could already multiply the number of available basin pairs, however without solving the question of heterogeneity (Figure 2.13-b).
- Second, I relaxed the requirement of geographical proximity between subbasins in order to bring heterogeneity (artificially and thus perhaps excessively) into the

hydrological input of each subbasin (Figure 2.13-c). I called the resulting virtual basin a *chimera*.

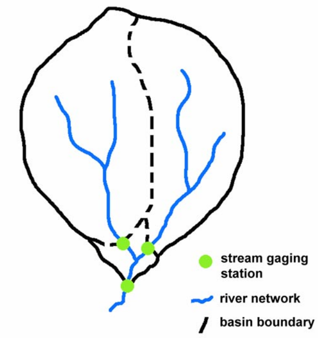
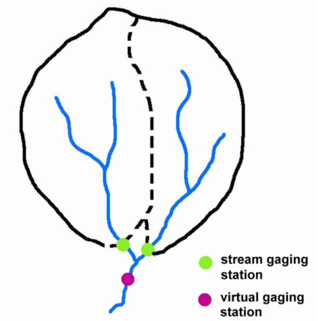
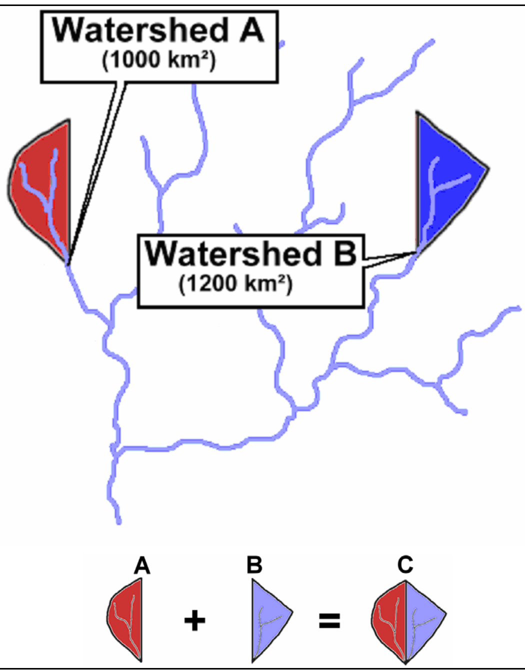
 a)	Desirable network: actual confluence with 3 gaging points
 b)	First simplification: actual confluence with only 2 gaging points, the third one being obtained by summation of upstream flows.
 c)	Second extension: virtual confluence of two basins which are not necessarily neighbors.

Figure 2.13: from actual confluences to virtual confluences: the chimera watershed approach

Figure 2.14 helps to better understand this appellation, showing the analogy between animal chimeras and watershed chimeras: the virtual animal (Elbra) can be used as a model of a third actual animal, the Okapi. Similarly, the virtual watershed can be used to model the streamgaging network ideally needed to validate the hydrological impact of splitting schemes.

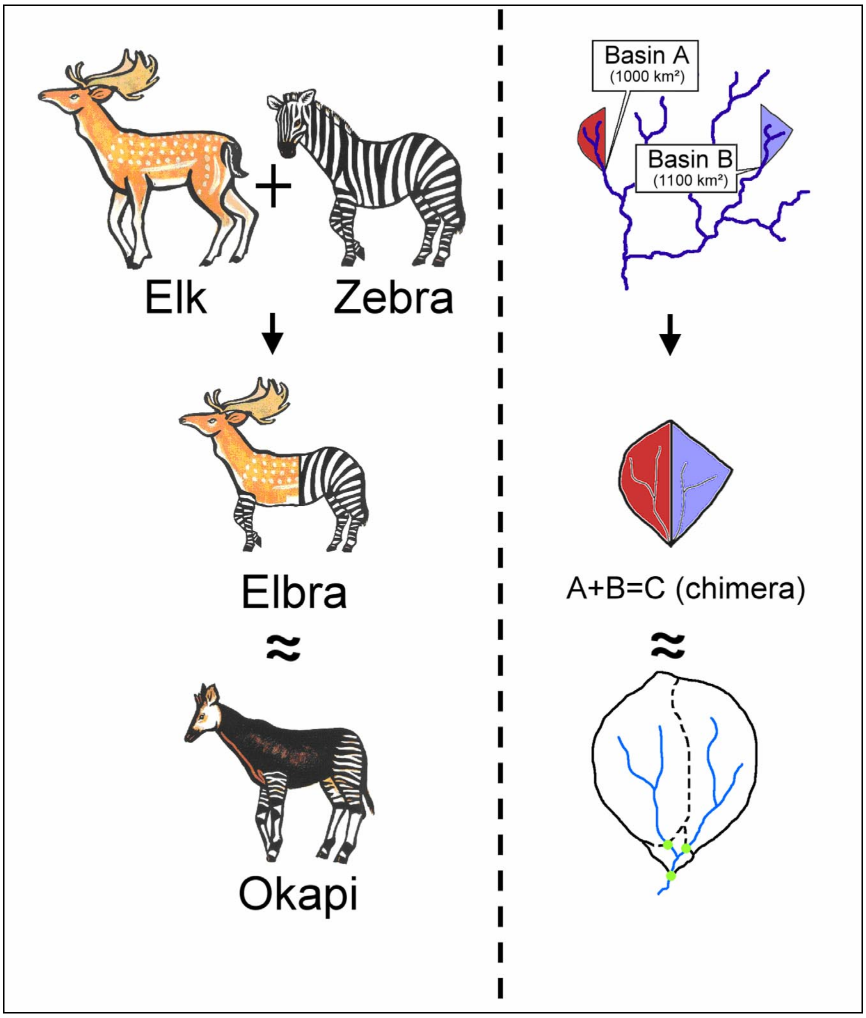


Figure 2.14: Analogy between animal chimeras and watershed chimeras. The virtual animal (Elbra) is used as a model of the Okapi. Similarly, the virtual watershed is used to model the streamgaging network ideally needed to study splitting schemes.

By using chimeras, we aimed to create conditions that would be much more diverse than those on actual watersheds and that would provide a definite advantage to the distributed approach, thus making the analysis of model sensitivity to distributed rainfall input more straightforward. Enhancing the contrast between watersheds was necessary, because several previous studies had failed to show a clear difference between a lumped and a distributed approach (Refsgaard and Knudsen, 1996; Loumagne et al., 1999; Smith et al., 1999), perhaps because watersheds were too homogeneous. By the exaggeratedly contrasted (heterogeneous) chimera watersheds, my aim was to understand the sensitivity of watershed models to distributed information. Also, I wished to establish to what extent and for what reasons semi-distribution (disaggregation) helps to explicitly account for the heterogeneity of hydrological processes. My goal, however, was definitely not to establish the conditions of an objective comparison between the lumped and the distributed approaches, but rather to provide information relative to model sensitivity, for both operational hydrologists and modelers who wish to understand the relative importance of the different sources of hydrological spatial variability.

On the basis of a total sample of 307 French watersheds, we obtained 2500 chimeras, which were built as follows:

- For each watershed of our sample, we first looked for all the basins of approximately equivalent size. The size was the only limitation to watershed combination into chimeras, as we wanted to keep subbasins of comparable size. Here, the ratio of subbasin areas was kept between 1.25 and 1/1.25 (i.e., a basin of 100 km² was considered to build chimeras using only basins between 125 and 80 km²).
- Then, for those pairs of basins of approximately equivalent size, we looked for a common period of record. We required a minimum common period of 10 years, which was then split into two sub-periods so as to identify distinct calibration and validation periods.

❑ ***Lumped, semi-lumped, and semi-distributed solutions used for SA***

In the article, we used four different watershed models (GR4J, SMAR, TOPMO and HBV0) in order to show that our sensitivity analysis was of general value. But here, we only present results obtained with GR4J, since all models have shown the same behavior.

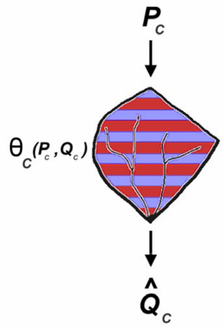
To assess the efficiency of streamflow simulations, we computed the Nash and Sutcliffe (1970) criterion in control mode for each level of spatial aggregation. We followed Klemeš (1986b) by calibrating model parameters on an initial period, and then computing the goodness of fit over a second period. The efficiency of each approach was characterized by the distribution of the 5,000 values of this criterion (5,000 = 2,500 chimeras x 2 periods) in control mode.

Four approaches, differing only in their level of spatial disaggregation, were compared: a lumped, a semi-lumped, and two semi-distributed approaches (see Table 2.2 for a summary).

Table 2.2: Different modeling cases compared in this study

Case	Case name	Precipitation input	Basin moisture computation	Parameters	Discharge information during calibration	Number of of optimized GR4J parameters
a	Lumped approach	L	L	L	L	4
b	Semi-lumped	D	D	L	D	4
c	Semi-distributed approach with disaggregated discharge knowledge	D	D	D	D	4+4
d	Semi-distributed approach with aggregated discharge knowledge	D	D	D	L	8

L: lumped, D: distributed

<table>
<tr>
<td>a- lumped approach</td>
<td>
</td>
<td>Only one vector of parameters, θ_C, is calibrated on aggregated series P_C & Q_C. In the validation phase, the estimate of chimera runoff ($\hat{Q}_C$) is obtained directly by simulation with the lumped model.</td>
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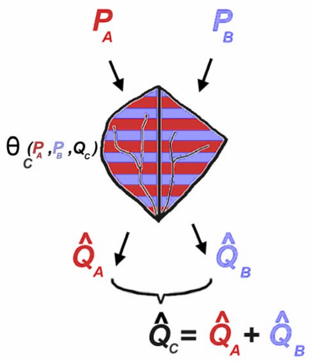
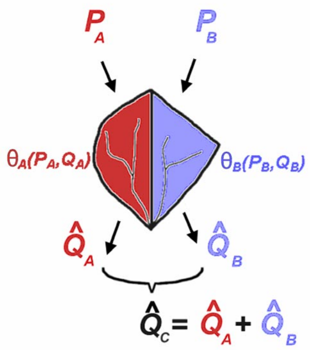
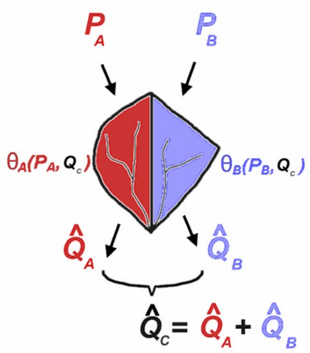
<tr>
<td>b- semi-lumped approach</td>
<td>
</td>
<td>Two models are run in parallel, but there is only one vector of parameters, θ_c, calibrated on (P_A, P_B, Q_C). In the validation phase, the estimate of the chimera runoff ($\hat{Q}_C$) is obtained by adding the contribution simulated for each subbasin ($\hat{Q}_A$, $\hat{Q}_B$).</td>
</tr>
<tr>
<td>c- semi-distributed approach with disaggregated discharge knowledge</td>
<td>
</td>
<td>Two models are run in parallel, with two parameter vectors, θ_A (calibrated on series P_A & Q_A) and θ_B (calibrated on series P_B & Q_B). In the validation phase, the estimate of the chimera runoff ($\hat{Q}_C$) is obtained by adding the contribution simulated for each subbasin ($\hat{Q}_A$, $\hat{Q}_B$).</td>
</tr>
<tr>
<td>d- semi-distributed approach with aggregagted discharge knowledge</td>
<td>
</td>
<td>Two models are run in parallel, with two parameter vectors, θ_A (calibrated on series P_A & Q_C) and θ_B (calibrated on series P_B & Q_C). In the validation phase, the estimate of the chimera runoff ($\hat{Q}_C$) is obtained by adding the contribution simulated for each subbasin ($\hat{Q}_A$, $\hat{Q}_B$). The difference with the previous option lies in the fact that the two parameter sets are calibrated simultaneously, using a single cost function, on an aggregated discharge series.</td>
</tr>
</table>

Figure 2.15: schematic representation of the four lumped, semi-lumped and semi-distributed approaches tested for our sensitivity study

By comparing approaches *a* and *c* one can assess the model sensitivity to an explicit account of spatial variability through disaggregation. Comparing approaches *b* and *c* one can distinguish between the effects of *precipitation disaggregation* and *watershed behavior disaggregation*. In principle, the semi-lumped approach should give results of lower efficiency than the semi-distributed approach (Zhang et al., 2003), as we force both sub-watersheds to have the same set of parameter values. But the results of both should be better than those obtained with the fully lumped (*a*) approach, since the knowledge of rainfall heterogeneity can be exploited. Last, comparing approaches *c* and *d* (both semi-distributed)[9], can help us to better understand how the model makes use of the discharge information used in calibration.

- ***What does the chimera approach teach us about the sensitivity of hydrological models to distributed information?***

The results obtained through the chimera watershed approach can be synthesized as follows

- **Expected superiority of the distributed approach over most of the chimera watersheds**

Our first analysis focused on the difference between the results of the lumped (a) and semi-distributed (c) approaches. Figure 2.16 presents the cluster obtained for GR4J (similar clusters were obtained for other watershed models, see Andréassian et al., 2004a). On the scatterplot, each one of the 2,500 chimeras is represented by two dots, which correspond to the efficiency (in control mode) over the two simulation periods available for each chimera. In Figure 2.16, most of the cluster (79% of the points) is situated above the 1:1 line, showing that the four models perform better in semi-distributed mode than in lumped mode on chimera watersheds. This was an expected result, as the aim of building chimeras was clearly to create very contrasted and truly heterogeneous situations, where a semi-distributed approach would provide an obvious advantage. What was less expected was that, notwithstanding the superiority of the semi-distributed approach, a significant number of watersheds remained under the 1:1 line (21% for GR4J), where the lumped approach gave equal or better results than the distributed

[9] Note that this comparison was not part of the 2004 Water Resources Research paper.

approach. It appears therefore that the superiority of disaggregative approaches is not necessarily absolute, which is a quite surprising result.

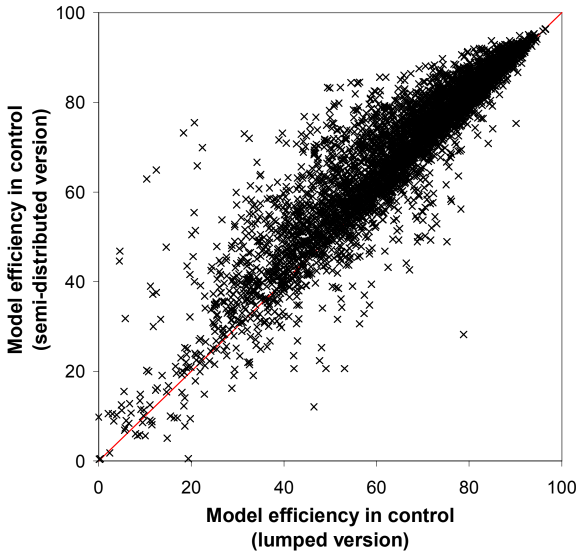


Figure 2.16: Efficiency of GR4J - lumped vs semi-distributed approaches (efficiency measured by the Nash and Sutcliffe criterion in control mode). Each of the 2,500 chimeras is represented by two dots, corresponding to the two validation (control) time periods.

To further interpret the above results, we would like to determine whether the sensitivity to spatial disaggregation comes from the possibility of taking into account distributed rainfall or distributed watershed behavior. To try to answer this question, we now consider the semi-lumped variant (*b*) for GR4J.

- **Advantage of disaggregation mainly due to accounting for rainfall variability**

Having simulated the behavior with the semi-lumped approach, we can now draw the distributions of model efficiency ratings (in control mode) in Figure 2.17 for the lumped, semi-lumped and semi-distributed approaches. The semi-distributed approach (variant c, in black), has its distribution on the right-hand side of the graph, which means that it yields the best results. The lumped distribution (in grey) is on the left side: its results are poorer. The semi-lumped approach (dotted line), is intermediate between the two other approaches.

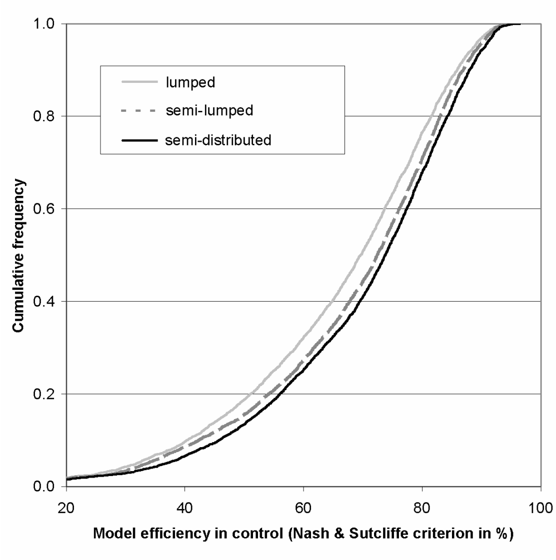


Figure 2.17: Distribution of the efficiency (in control mode) of GR4J, when used in lumped, semi-lumped and semi-distributed modes

From a SA point of view, the most interesting result is that the intermediate distribution is much closer to that drawn in black than to the distribution depicted in grey. This means that most (67% at the median) of the gap between lumped and semi-distributed distributions can be filled by taking into account the spatial variability of rainfall only. Spatialization of watershed behavior has thus only a minor effect on the improvement of simulations.

- **Model sensitivity to disaggregated discharge knowledge**

In Figure 2.18, we compare the two variants of semi-distributed modeling, to understand GR4J sensitivity to disaggregated discharge knowledge. For both semi-distributed solutions (noted *c* and *d* in Figure 2.15), the only difference comes is due to the way model parameters are optimized: in variant *c*, we calibrate separately θ_A and θ_B, by minimizing the square errors on $\left(Q_A - \hat{Q}_A\right)^2$ and $\left(Q_B - \hat{Q}_B\right)^2$ separately. For variant *d*, we calibrate simultaneously θ_A and θ_B, by minimizing the square error $\left(Q_A + Q_B - \hat{Q}_A - \hat{Q}_B\right)^2$. Thus, we simulate a situation where no disaggregated streamflow is available for subbasin parameter calibration.

In theory, as variant *c* uses more spatialized discharge information, I would have expected it to be more robust (i.e. to provide better results in control mode). But surprizingly, the two distributions of the results are very close, with even a slight advantage for *d*. This shows that GR4J is more sensitive to the additional flexibility provided by the lumped calibration (8 parameters simultaneously) than to the richer information content of the disaggregated discharge time series. This offers interesting opportunities for semi-distributed modeling, even when only one streamgage is available for calibration.

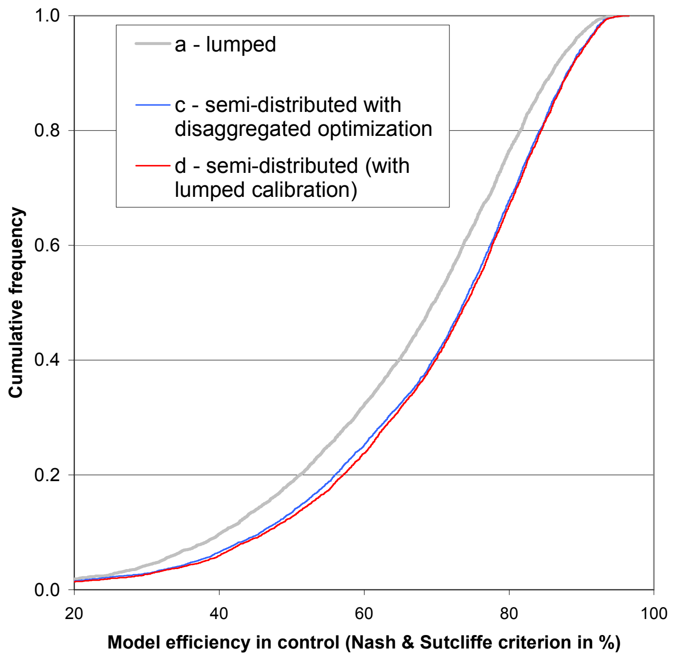


Figure 2.18: Sensitivity of GR4J efficiency to disaggregated discharge data (the distributions of efficiency for the two semi-distributed variants are in color)

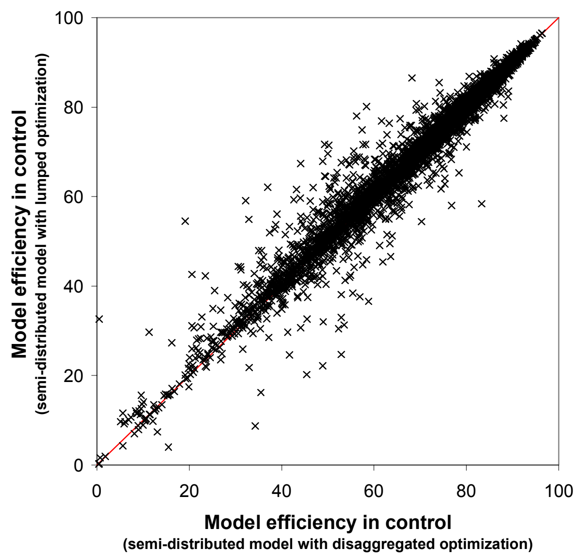


Figure 2.19: Efficiency of GR4J - comparison of two different semi-distributed approaches, variant *c* and *d* (efficiency measured by the Nash and Sutcliffe criterion in control mode). Each of the 2,500 chimeras is represented by two dots, corresponding to the two validation (control) time periods.

- **Synthesis of model sensitivity to distributed rainfall input**

Our sensitivity analysis showed that the greatest improvement that can be made by spatial distribution will be provided by rainfall variability accounting. This is a quite interesting result, which means that if spatial distribution is considered a useful route to improving the reliability of hydrological models, efforts should be directed first and foremost towards the use of spatially distributed rainfall data and only secondarily to the disaggregation of watershed (land-surface) parameters.

These finding are consistent with:

- the study by Diermanse (2001), comparing lumped and distributed variants of the HBV model on a small (114 km²) and a large (28,152 km²) basin. On the smaller basin, the author showed that the effect of averaging rainfall over space was much larger than the effect of averaging topographic characteristics. However, the difference between the lumped and distributed versions was very small. On the larger basin, the difference was more significant. There, the author showed again that the effect of averaging rainfall over space was greater than that of averaging initial conditions.
- the conclusions by Boyle et al. (2001), who compared (on a single basin) different options of spatial distribution, that the main improvements – in terms of model efficiency – were provided by the spatial representation of precipitation, while little or no improvement was gained by distributing soil properties. As the authors stressed “this seems contrary to the general belief among hydrologists that the spatial variability of soil properties exerts a significant control on the hydrologic response of a watershed.” The same applies to our results.

2.7 Sensitivity analysis for watershed models

❑ *Why do sensitivity studies so often disagree?*

The overall conclusions of the studies described in this chapter may at first sight seem surprizing and quite controversial. Some examples:

- In the case of model sensitivity to rainfall input, Paturel et al. (1995) and Nandakumar and Mein (1997) found that the bias in the predicted mean annual runoff showed a linear relationship (slope 1) with the input bias in rainfall, whereas we found (Andréassian et al., 2001) that models are able, to a certain extent, to cope with biased input estimates.
- In the case of model sensitivity to potential evapotranspiration, Nandakumar and Mein (1997) found a clear dependence of model performance on PE inputs, whereas we found (Andréassian et al., 2004c) only very little effect when applying a strong bias on PE (roughly 30%).
- In the case of model sensitivity to spatial distribution, Freeze (1980) found a great advantage in modeling approaches based on distributed parameters, whereas we found (Andréassian et al., 2004b) that distributed models were not always better than lumped ones, and that the greatest improvement that could possibly be contributed by spatial distribution stemmed from accounting for rainfall variability, and not from parameter spatialization.

A likely explanation for this apparent disagreement is the lack of clear consensus on the testing scheme used to assess the impacts of data errors on model performance and parameter determination (moreover, the kind of models used, the number and type of watersheds studied, and the ranges of random error or systematic bias tested in the input data are different). To try to make things clear, I proposed a classification of sensitivity studies into two groups: (1) *static* and (2) *dynamic*.

(1) **Static sensitivity studies** are those that explore model sensitivity to input (PE or rainfall estimates) by first obtaining a calibration considered to be optimal and then leaving it unchanged. Model sensitivity is assessed by comparing flows simulated with “erroneous” input and flows simulated with “perfect” input.

(2) **Dynamic sensitivity studies** involve a reference calibration (and a corresponding reference streamflow simulation), using a reference PE. But

model recalibration is allowed with “erroneous” PE, and the reference simulation is then compared to the flow simulated with the recalibrated watershed model.

The proposed classification might also reveal a more fundamental difference in modeling philosophy: with **static sensitivity studies**, modelers may assume implicitly that the true parameters are watershed-specific (with no decisive influence from the climate input data), while with **dynamic sensitivity studies**, modelers acknowledge explicitly that the calibrated watershed parameters depend on climate input data.

I notice that all the authors who preferred classical, static SA (Parmele, 1972; Nandakumar and Mein, 1997), implicitly considered that the true parameters are determined without any decisive influence by the climate input data, and therefore the impact of erroneous data on the model calibration is not assessed. This is why these studies concluded that data errors lead to proportional degradation of model performance. But those authors who used dynamic SA (Dawdy and Bergmann, 1969; Troutman, 1982; Troutman, 1983; Xu and Vandewiele, 1994; Andréassian et al., 2001; Andréassian et al., 2004c) found that the performance of the models was less sensitive to data errors because they were able to adjust their parameters in order to compensate for input errors within a reasonable range.

To sum up the above conclusions, we drew Table 2.3 in order to summarize the results of the studies cited above and to emphasize the differences between static and dynamic studies (Oudin et al., 2005d).

Table 2.3: Classification of previous studies concerning the impact of the four types of error introduced in data records (three levels of impact are identified; o: no significant impact; +: moderate impact; ++: high impact; blanks mean that this type of error was not investigated).

	Authors	Type of studies	PE Random errors	PE Systematic errors	Rainfall Random errors	Rainfall Systematic errors
Model performance	Dawdy and Bergmann (1969)	*Dynamic*				o
	Ibbitt (1972)	*Dynamic*			o	
	Troutman (1982, 1983)	*Dynamic*				o
	Xu and Vandewiele (1994)	*Dynamic*			+	o
	Andréassian et al. (2001, 2004c)	*Dynamic*		o		o
	Parmele (1972)	*Static*		++	o	o
	Paturel et al. (1995)	*Static*	+		++	++
	Nandakumar and Mein (1997)	*Static*	+	++	++	
Parameter values	Dawdy and Bergmann (1969)	*Dynamic*				++
	Ibbitt (1972)	*Dynamic*			o	
	Troutman (1982, 1983)	*Dynamic*				++
	Xu and Vandewiele (1994)	*Dynamic*				++
	Andréassian et al. (2001, 2004c)	*Dynamic*		++		++

I believe that the disparity in published results from the two groups of sensitivity studies arises essentially from their underlying philosophy: when the model is viewed as a potentially exact, physical representation of the real world, a *static sensitivity study* will make sense, as it is believed that a 'true' (i.e. physical) set of parameters exists; but when the model is viewed as a conceptual or empirical representation of the real world, there is no reason why a 'true' parameter set should exist independently of the calibration data, and only a *dynamic sensitivity study* will make sense. I believe that both approaches are acceptable if their underlying hypotheses are recognized. But obviously, I would personally give my preference to dynamic studies.

❑ *Parameter sensitivity to inputs: tentative synthesis*

The objective of the work that have been doing recently at Cemagref (Andréassian et al., 2001; Andréassian et al., 2004b; Andréassian et al., 2004c; Oudin et al., 2004; Perrin et al., 2004; Oudin et al., 2005a; Oudin et al., 2005b; Oudin et al., 2005d) was to investigate the impact of data errors on performance and estimation of model parameters, as well as the impact of discharge knowledge and rainfall spatialization. Table 2.4 (Oudin et al., 2005d) synthesizes the results obtained for each type of error. Our conclusions generally corroborate and broaden the results of previous dynamic studies presented in Table 2.3.

Table 2.4: Impact of the four types of errors introduced in data records (three levels of impacts are identified; o: no significant impact; +: moderate impact; ++: high impact)

<table>
<tr><td colspan="4" rowspan="2"></td><td colspan="2">PE</td><td colspan="2">Rainfall</td></tr>
<tr><td>Random errors</td><td>Systematic errors</td><td>Random errors</td><td>Systematic errors</td></tr>
<tr><td rowspan="6">GR4J</td><td colspan="3">Model Performance</td><td>o</td><td>+</td><td>++</td><td>+</td></tr>
<tr><td rowspan="4">Parameter Values</td><td rowspan="2">Production</td><td>θ1</td><td>o</td><td>o</td><td>+</td><td>++</td></tr>
<tr><td>θ2</td><td>o</td><td>+</td><td>+</td><td>++</td></tr>
<tr><td rowspan="2">Routing</td><td>θ3</td><td>o</td><td>+</td><td>+</td><td>++</td></tr>
<tr><td>θ4</td><td>o</td><td>o</td><td>o</td><td>o</td></tr>
<tr><td colspan="7">θ1: Capacity of the production store; θ2: Underground exchange coefficient; θ3: Capacity of the non linear routing store; θ4: Unit hydrograph time base</td></tr>
<tr><td rowspan="8">TOPMO</td><td colspan="3">Model Performance</td><td>o</td><td>+</td><td>++</td><td>++</td></tr>
<tr><td rowspan="6">Parameter Values</td><td rowspan="3">Production</td><td>θ2</td><td>o</td><td>+</td><td>+</td><td>o</td></tr>
<tr><td>θ3</td><td>o</td><td>+</td><td>o</td><td>++</td></tr>
<tr><td>θ6</td><td>o</td><td>++</td><td>+</td><td>++</td></tr>
<tr><td rowspan="3">Routing</td><td>θ1</td><td>o</td><td>o</td><td>+</td><td>+</td></tr>
<tr><td>θ4</td><td>o</td><td>o</td><td>o</td><td>o</td></tr>
<tr><td>θ5</td><td>o</td><td>o</td><td>o</td><td>++</td></tr>
<tr><td colspan="7">θ1: Recession coefficient of the exponential store; θ2: Capacity of the interception store; θ3: Topography index parameter; θ4: Time delay; θ5: Capacity of the routing store; θ6: Evaporation parameter</td></tr>
</table>

The main conclusions that can be drawn from the results synthesized in Table 2.4 are the following:

1. Random errors in PE: both models proved almost insensitive to random errors in PE series. This seems to be the common rule for soil moisture accounting rainfall-runoff models that behave like low pass filters (see Oudin et al., 2004a). Therefore, this type of error did not induce significant modifications in the model performances nor in the parameter values;

2. Random errors in rainfall: this type of error was much more detrimental to model performances than in the previous case. This difference between the two inputs is a widely recognized fact (Paturel et al., 1995; Nandakumar and Mein, 1997). The performances significantly dropped for both models. The models try to exploit the buffering capacities of their production functions - especially their SMA store - to minimize the impact of these errors on flow simulation;
3. Systematic error in PE: there was a moderate impact of this type of error on model performances. The models use their production functions as buffers to compensate for over- or underestimations of PE. Here the GR4J model, that has a underground exchange function, proved more flexible to adapt to large PE underestimations, while maintaining an acceptable water balance, contrary to TOPMO;
4. Systematic errors in rainfall: this is the case where the models showed the most different results. TOPMO performances were drastically affected whereas GR4J better managed to adapt to this type of errors. Here again, its underground water exchange function let GR4J maintain an acceptable water balance while the TOPMO model was not able to do so.

The results on systematic errors showed the important role played by the underground exchange function in GR4J: the GR4J proved more sensitive to random errors in rainfall than to systematic bias, whereas TOPMO showed the opposite behavior. This study demonstrates that the sensitivity of a rainfall-runoff model to errors in input may depend partly on the model structure itself. It proved therefore very useful here to use these two models that adopt quite different viewpoints to represent watershed behavior, to show what was common and different between models.

The delay parameter ($\theta 4$) existing in each model was the only parameter that was insensitive to all types of tested errors. This is quite logical since the types of errors investigated here did not include temporal shifts in time series.

As noticed in other studies (see eg. Ye et al., 1997), the low-yielding catchments of our test sample proved difficult to model and showed peculiar and sometimes erratic behavior, especially with regard to the evolution of their parameters. This original behavior still remains puzzling and may be the result of the lack of information content of the streamflow time series of ephemeral catchments (low number of non-zero data), which often leads to calibration problems. Further research is needed to understand the behavior of these ephemeral catchments.

Last, it seems as if the incidence of input errors is lower in the case of GR4J than for other watershed models. I believe that the reason for this robustness lies in its empirical development, as well as in its extreme parsimony (GR4J deals with the rainfall-runoff transformation at a daily time-step with only four free parameters).

2.8 Second answer to radio Yerevan

Can we cure watershed models?

Radio Yerevan answers:

In principle yes, just as the Perestroïka cured communist party leaders in the Soviet Union... it forced most of them to look for another job.

Third question to radio Yerevan:

What are the perspectives for appropriate modeling on the horizon of hydrological sciences?

Research Perspectives

3 My perspectives for an appropriate modeling of watershed systems

3.1 Hydrological questions on my scientific horizon?

At the end of this journey though my scientific activity of the last decade comes the time to present my objectives for the future and the opportunities I see for new and (hopefully) fruitful research. Looking towards the horizon is for me at the same time exhilarating, thought-provoking, and frightening:

- ***exhilarating***, because I have during the past decade encountered many challenging applied scientific questions, that I would like to address, if I have the possibility, in the near future;
- ***thought-provoking***, because hydrology is not a well-established science, based on a well-defined corpus of principles: many theoretical questions are still unanswered, and I feel I could contribute to answer some of them;
- ***frightening***, because looking toward the horizon also makes me realize the infinity of remaining scientific problems, understand the extreme modesty of the hydrological solutions our community is able to offer, and acknowledge the fact that seeking scientific understanding is an endless endeavor.

Fortunately, the fact that I see scientific research not as an individual journey but as team work, implies that I am never alone when looking at the horizon… and this is always reassuring in times of uncertainty.

I will now present my potential hydrological objectives. In section 3.2, I will examine the applied hydrological questions to which I feel the most attracted: I believe that applied questions are those that have the potential to bring me the most personal satisfaction (but the less scientific reward), if answered. In section 3.3, I will look at the more theoretical questions of hydrology that I would like to address. In section 3.4, I will present the more "frightening" difficult questions I may perhaps look at, with no other ambition than to bring a partial and unsatisfying solution. Last, in section 3.5, I will present my answer to the third question asked of radio Yerevan.

3.2 Applied hydrological questions

- ***Flood and drought forecasting***

Forecasting is one of the controversial areas of modern hydrology. However, it seems to me that it is also one of the most exciting, since operational forecasts have a concrete answer... no way to hide model plagues... all errors are paid cash.

I have not myself worked much on hydrological forecasting, but my team has been increasingly involved in it in recent years, through two studies for the design of an operational flood forecasting system: on the upper Oise catchment and the Aisne-Aire catchment, as well as on the Sarthe and Huisne rivers. I also coordinated a research project on the upper Loire catchment (Andréassian et al., 2004a), where we attempted to intercompare different forecasting methods. Again, I believe that intercomparison was very fruitful for our understanding of modeling: the surprizingly good performances of the Artificial Neural Networks (ANNs) stunned us: using only as input the most recent discharge and rainfall, ANNs were able to forecast flows as well as, sometimes even better than conceptual models, corrected through complex updating methods. The question that I would like to see answered is: which combination of information do ANNs use? How do they extract it? After all could we not extract the same information for the use of a classical SMA[10]-type model?

Some work was initiated by François Anctil during his sabbatical stay at Cemagref and reported in Anctil et al. (2003). Other approaches are currently tested by Tangara (ongoing PhD) at Cemagref in order to simplify the structure of the GR4J model to allow it to be calibrated directly in a forecasting mode (as the ANNs are). Will it be enough? Probably not. My opinion is that it would be worth trying the approach initiated by Anctil with the ANNs as updating tools. Feeding them with the different inner states of the model, we could perform a sensitivity analysis in order to identify the elements on which updating algorithms should operate. In a further step, we could try to simplify the ANNs to understand their inner mechanism.

[10] Soil Moisture Accounting

❑ *Snow hydrology*

Snow hydrology is another fascinating field of research. First, because snowpack / snowmelt modeling is very complex: reliable data on snow input are scarce and their acquisition is extremely difficult (see Sevruk, 1993 and Sevruk and Nespor, 1994 for the metrological problems). Second, because mountainous watersheds where snow falls and melts are beautiful. Third, because this is one of the remaining, little explored domains for our modeling research group.

The preliminary work we did on this topic (Eckert, 2002; Eckert et al., 2002) showed that quite simple solutions can provide improvement in runoff simulation. However, our research remained within the scope of a DEA thesis, and was limited to French watersheds that were in the main little affected by snow cover. Thus, I consider that even the modest efficiency improvement we obtained was already encouraging. But in order to refine our snow accounting procedure within the GR4J model, a specific watershed sample is needed. What we need the most are watersheds with dense precipitation and thermographic gaging networks (with a good representation of the higher elevation zones): this will help define the best strategy for the pseudo-distribution that seems definitely to be needed (see for example WMO, 1986; Ferguson, 1999) in order to account for elevation gradients on temperatures and thus on snowmelt. I believe that with the work by Eckert, we reached the maximum possible without a specific sample. However, this does not mean that we must abandon the ordinary watersheds: these must remain part of the total sample, to insure that improvement in the structure of the snow routine does not adversely affect the simulation of flows where snow has only a minor importance. A catchment sample of sufficient size could probably be constituted if US and Soviet databases where searched, and an association with Météo France and EDF is sought.

I would probably start from the degree-day method (Martinec, 1960; Martinec, 1975; Martinec and Rango, 1986), which has proven its robustness and its efficiency, over temporary snowpacks (Kongoli et Bland, 2000) as well as over glaciers in the Himalayas (Singh et al., 2000) or in the Alps (Braithwaite et Zhang, 2000). Furthermore, a recent publication (Ohmura, 2001) discussed the physical basis of this method. Then, I would like to develop further the approach tried with Eckert which consists in an attempt to transform the degree-day method into a PET-day method, where snow would melt on the basis of the energy budget synthesized by the PET forcing.

3.3 Theoretical hydrological questions

❑ *Ensemble modeling for ungaged basins*

In 2002, IAHS launched an international decade for the Prediction on Ungaged Basins (PUB). The main objective of this decade is to provide solutions for the use of hydrological models on ungaged basins. Indeed, as Sivapalan et al. (2003) stressed, "although there are numerous conceptual models around and being developed, […] we are still nowhere near solving the problems related to […] the *a priori* estimation of parameters that hamper predictions." Reviewing the excellent Australian research in this domain, Boughton (2005) added that "there have been many efforts over the last two decades to simplify models. A major objective of most of the studies has been to relate the reduced number to catchment characteristics for use on ungauged catchments. This objective has not yet been achieved".

Traditionally, for rainfall-runoff models, the approach has been to look for methods allowing *a priori* estimating of the value of model parameters. The main method consists in looking for multiple regressions of the type θ_i = *f*(watershed physical parameters). Several approaches exist for these regressions (*a posteriori* searching or simultaneous calibration of the regression and the models (see Hundecha and Bárdossy, 2004). However, results have been mostly disappointing (see for example Merz and Blöschl, 2004), and it is believed that part of the problem is linked to parameter uncertainty and parameter interactions (Kuczera and Mroczkowski, 1998). If some solutions may exist regarding parameter uncertainty (such as simultaneous calibration of models and regression functions), the question of parameter interactions cannot be avoided. What should we do then?

I would like to try a completely different approach suggested by the work we did on multi-models within the DEA research of Miossec (2004). In a multi-model setting, the desired simulation is obtained by a weighted average of simulations of the participating models:

$$\hat{Q}_{multi} = \sum_{i=1}^{n} w_i \hat{Q}_i$$

In an ensemble setting, the desired simulation can be considered to be a weighted simulation of runs obtained by a single model, fed by the same precipitation input, but each using a different parameter set, corresponding to *n* neighboring watersheds where the model was previously calibrated:

$$\hat{Q}_{ung} = \sum_{i=1}^{n} w_i \, \hat{Q}_i(P_{ung}, \theta_i)$$

where:

. $\hat{Q}_{ung}$ is the vector of simulated streamflow for the ungaged basin;

. $\hat{Q}_i(P_{ung}, \theta_i)$ is the streamflow simulated with P_{ung}, the precipitation input of the ungaged watershed, using the model parameters corresponding to watershed *i*;

. w_i are the weights attributed to each of the *n* neighboring watershed in the multimodel.

But how should we compute the weights? This would be the main topic of research, of course, and I think that we should attempt to define a basin similarity index $\omega_{a,k,m}$ between basins *k* and *m*, based on a relevant physical characteristic of the basin *a*:

$$\omega_{a;k,m} = \frac{1}{\left(\frac{a_k}{a_m} + \frac{a_m}{a_k}\right)}$$

where a_k and a_m each represent a relevant physical characteristic of basins *k* and *m* respectively. On this example, several physical characteristics *a*, *b* and *c* could be combined in several ways (the search being made by trial and error), such as:

$$\omega_{k,m} = \frac{1}{\left(\frac{a_k}{a_m} + \frac{a_m}{a_k}\right)^{\alpha}} \cdot \frac{1}{\left(\frac{b_k}{b_m} + \frac{b_m}{b_k}\right)^{\beta}} \cdot \frac{1}{\left(\frac{c_k}{c_m} + \frac{c_m}{c_k}\right)^{\gamma}}$$

or

$$\omega_{k,m} = \frac{\alpha}{\left(\frac{a_k}{a_m} + \frac{a_m}{a_k}\right)} + \frac{\beta}{\left(\frac{b_k}{b_m} + \frac{b_m}{b_k}\right)} + \frac{\gamma}{\left(\frac{c_k}{c_m} + \frac{c_m}{c_k}\right)}$$

Ultimately, the weights in the ensemble simulation would be computed as follows:

$$w_i = \frac{\omega_i}{\sum_{j=1}^{n} \omega_j}$$

❑ *Study of watershed variability*

During my PhD research that focused on forest hydrology questions, I developed a statistical test allowing the detection of gradual trends in the hydrological behavior of a watershed (Andréassian, 2002; Andréassian et al., 2003). I would be particularly interested to use this test in two directions:

- **variability investigation on large basin samples:** I would like to use the availability of large basin samples collected in the course of previous theses (Perrin, Oudin, Mathevet, Rojas-Serna) and of the large American basin sample made available by the MOPEX project, to investigate the spatial distribution of basins showing trends. Of course, all detected trends do not reflect actual basin behavior changes, and some will be artefacts caused by metrological problems and specific tests are needed to try to separate actual (behavioral) trends from metrological trends.
- **search for an index of natural basin variability:** independently of the existence of trends, I would like to complement my study of forest impact on hydrological behavior. In my PhD thesis (Andréassian, 2002), I showed an interesting graph (Figure 3.1), where I plotted what I called the *Potential Hydrological Response* (PHR) of 35 watersheds against the evolution of their forest cover (expressed in terms of basal area).

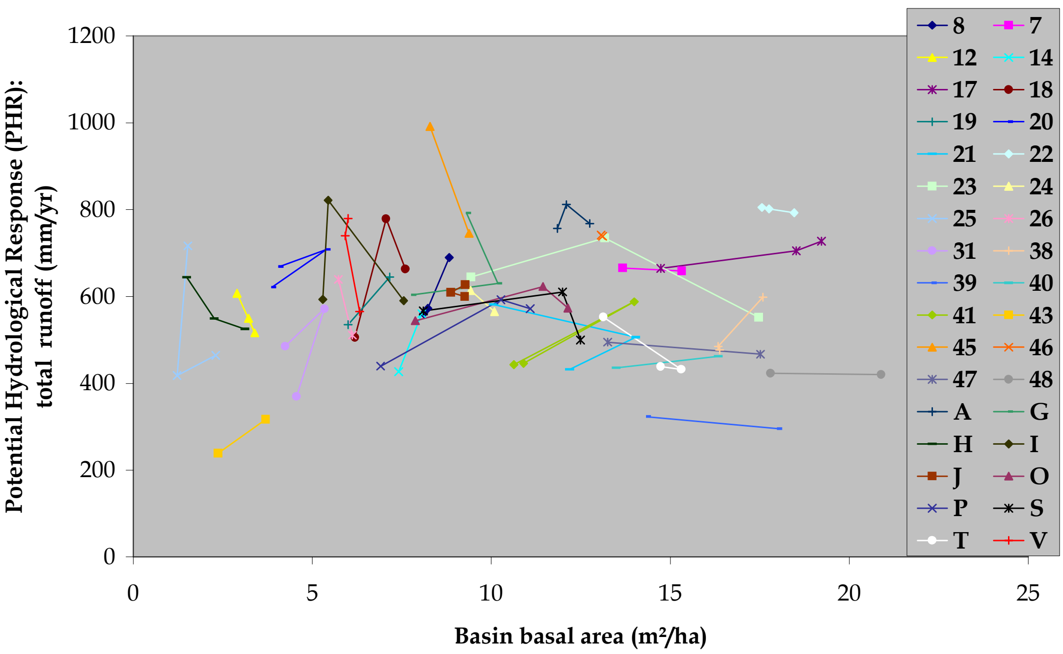


Figure 3.1: Potential Hydrological Response (PHR, in mm/yr) of 35 watersheds plotted against the evolution of their forest cover (expressed in terms of basal area, m²/ha.

PHR corresponds to the hypothetic water yield of a basin, which at several periods of its evolution, would have been subjected to the same long-term precipitation time series. Figure 3.1 is interesting because, even if it does not show that PHR is correlated with forest cover evolution, it seems to show that the variability of the PHR is weaker on forested catchments than on the others. I would like to continue this study on a larger sample, and to work on a specific index by which to measure this PHR variability (perhaps by using the resampling techniques described in section 2.4) in order to investigate this possible second-order link between *intrinsic watershed variability* and *land-use*.

3.4 Questions far on the horizon

There are two important hydrological questions, which I consider to be of interest for me, but far on my research horizon: the prediction of land-use change impacts and the spatialization of operational hydrological models. But if they are interesting, why do I let them remain on the horizon? For two main reasons: first because I feel that these questions may not, perhaps, have any solution, and second because I think we ought to answer some other questions first. Let me now describe how I see these issues.

❑ *Predicting the hydrological impact of land-use changes*

Far on the horizon, I see the question of predicting the hydrological impact of land-use changes. Why does it appear to me so far away? Because I believe that before addressing this issue:

- we first need to demonstrate that we are able to detect the impacts of these changes with our models. This is for me a basic prerequisite, linked to the actual sensitivity of watershed behavior to land-use and its modifications. Unfortunately, I agree with Kokkonen and Jakeman (2002) who consider that “there are still no credible models to predict the effect on hydrological response of land-use change in gauged catchments”;
- we also need to be able to give a satisfactory answer to the questions of regionalization (mentioned in section 3.3) to prove and quantify the actual sensitivity of hydrosystems towards land-use. Indeed, as Boughton (2005) writes, "the problem of estimating runoff from ungauged catchments is closely related to the problem of estimating the change in runoff that will occur when the land use of a catchment changes".

Until actual progress is achieved in the two above domains, I believe that we cannot do anything. I do not agree with the devotees of the upward approach to watershed modeling who considered that only physically-based models can address the question of the hydrological impact of land-use changes. The issue is not necessarily[11] one of model type: it is just one of watershed behavior sensitivity. And

[11] It could however be argued that physically-based models are disqualified on this issue, as they incorporate the answer in their hypotheses. Using them will only lead to rediscover the hypotheses in model results.

in order to model an effect, we have to be sure that the actual watershed is sensitive to it.

❑ *Operational spatialized hydrological modeling*

The *necessity* to build/use a distributed watershed model is perhaps the most popular bromide of modern hydrology. Every hydrologist seems to agree that physically-based distributed models can overcome the deficiencies of good old lumped models through “their use of parameters which have a physical interpretation and through their representation of spatial variability in the parameter values” (Abbott et al., 1986). But concerning the fundamental question of “how can we achieve the most appropriate use of available spatial information?”, very few answers have been found. The question has almost never been tackled by a downward approach, one of the exceptions being our own work reported in section 2.6 and in Andréassian et al. (2004a). I believe that progress can be made through continuing efforts in this direction, on the condition that we move only towards spatialized solutions that are justified by a *well-proved sensibility* of model results to the increasing degrees of freedom introduced.

For potential semi-distributed models:

- I think that Artificial Neural Networks could be used to investigate possibilities offered by the use of spatialized information;
- I believe that we will need to define an appropriate, specific, parsimonious model structure allowing us to represent the hydrological behavior of watershed segments (i.e. parts of watersheds which are not a complete basin with a well-established outlet);
- Last, I believe that we will need extremely parsimonious transfer functions to insure the propagation of flow generated by subbasins. A parsimonious and efficient method such as the lag and route (Bentura and Michel, 1997) could be a good basis to start from.

3.5 Third answer to radio Yerevan[12]

What are the perspectives for appropriate models on the horizon of hydrological sciences?

Radio Yerevan answers:

In principle, the perspectives are bright, but it depends on how we define the horizon.

So, what is the definition of an horizon at radio Yerevan?

Radio Yerevan answers:

Horizon is an imaginary line which moves away each time you approach it.

[12] Original form of this riddle:

Radio Yerevan receives a question from an auditor: "We are told that the communism is already seen at the horizon. But what is exactly a horizon?"

Radio Yerevan answers: "Horizon is an imaginary line which moves away each time you approach it."

Afterword

I started this thesis by discussing the classical form of radio Yerevan riddles, and with a tribute to Klemeš who introduced them to the hydrological community. Then, I organized this document according to three questions to which I attempted to find an answer, based on my own experience. These answers surely represent a very partial view, my view, and I must admit that I have expressed them without any ecumenical considerations, as I believe that hydrological science will better progress on the basis of strong schools of thoughts rather on weak consensuses.

How should I close this thesis? Since I started with radio Yerevan riddles, the most logical would be to end with radio Yerevan riddles. And since the reader will by now be well-used to the standard form, I thought I could try to submit hydrological versions of three famous riddles, which depart from the conventional form:

- Riddle 1

  Original form:

  Radio Yerevan receives a question from an auditor: "What is the best form of contraception?"
  Radio Yerevan answers: "a glass of water"
  "Before or after?" asks the auditor.
  "Instead of."

  Hydrological form:

  Radio Yerevan receives a question from an auditor: "What is the best way to avoid endless controversies in hydrological modeling?"
  Radio Yerevan answers: "Astrophysical modeling"
  "Before or after the hydrological component?" asks the auditor.
  "Instead of."

- Riddle 2

  Original form:

  Radio Yerevan receives a question from an auditor: "Which is the most important city of the Soviet Union?"
  Radio Yerevan answers: "Yerevan, of course !"
  Additional question from the auditor: "And how many atomic bombs does it take to destroy Yerevan?"

Radio Yerevan answers: "On second thought, it appears that Moscow is in fact the most important city of the Soviet Union."

Hydrological form:

Radio Yerevan receives a question from an auditor: "Which is the most promising branch of hydrological modeling?"

Radio Yerevan answers: "Complex physically-based distributed modeling, of course !"

Additional question from the auditor: "And how many angry inundated people does it take to beat their authors after their system failed to provide adequate warning?"

Radio Yerevan answers: "On second thought, it appears that simple empirical models are the most promising"

- Riddle 3

Original form:

Radio Yerevan time signal: Beep ... Beep ... Beep ... Beep ... Beeeeep! It's exactly 9:00, at most 9:30.

Hydrological form:

I leave it to the reader.

Poem of conclusion

Je meurs de soif auprès de la fontaine,
Chaud comme le feu, je claque des dents ;
Dans mon pays, je suis en terre étrangère,
Près d'un brasier, je frissonne brûlant ;
Nu comme un ver, vêtu en président,
Je ris en pleurs et attends sans espoir ;
Je me réconforte au fond du désespoir
Je me réjouis et n'ai aucun plaisir ;
Puissant, je n'ai ni force ni pouvoir,
Bien accueilli, repoussé par chacun.

Rien ne m'est sûr que la chose incertaine,
Obscur que ce qui est tout à fait évident,
Je ne doute que de chose certaine,
Je tiens la science pour accident fortuit,
Je gagne tout et demeure perdant ;
Au point du jour, je dis : “Bonsoir ” !
Etendu sur le dos, j'ai grand peur de tomber ;
J'ai bien de quoi sans posséder un sou ;
J'attends un legs sans être héritier,
Bien accueilli, repoussé par chacun.

Je n'ai souci de rien, malgré tous mes efforts
Pour acquérir des biens sans y prétendre ;
Qui parle le mieux m'offense le plus,
Et le plus véridique est pour moi le plus menteur ;
Mon ami est celui qui me fait croire
Qu'un cygne blanc est un corbeau noir ;
Et celui qui me nuit, je crois qu'il m'assiste ;
Mensonge, vérité, aujourd'hui c'est pour moi tout un,
Je retiens tout, sans rien concevoir,
Bien accueilli, repoussé par chacun.

Prince clément, plaise à vous de savoir
Que je comprends tout et n'ai sens ni savoir :
Je suis d'un parti, et de l'avis de tous.
Que sais-je le mieux ? Quoi ! Reprendre mes gages,
Bien accueilli, repoussé par chacun.

Je meurs de seuf auprés de la fontaine,
Chault comme feu et tremble dent a dent,
En mon pays suis en terre loingtaine,
Lez ung brasier frisonne tout ardent,
Nu comme ung ver, vestu en president,
Je riz en pleurs et attens sans espoir,
Confort reprens en triste desespoir,
Je m'esjoys et n'ay plasir aucun,
Puissant je suis sans force et sans pouoir,
Bien recueully, debouté de chascun.

Riens ne m'est seur que la chose incertaine,
Obscur fors ce qui est tout evident,
Doubte ne fais fors en chose certaine,
Scïence tiens a soudain accident,
Je gaigne tout et demeure perdent,
Au point du jour diz: “Dieu vous doint bon soir ! ”,
Gisant envers j'ay grand paour de chëoir,
J'ay bien de quoy et si n'en ay pas ung,
Eschoicte actens et d'omme ne suis hoir,
Bien recueully, debouté de chascun.

De rien n'ay soing, si mectz toute m'atayne
D'acquerir biens et n'y suis pretendent,
Qui mieulx me dit, c'est cil qui plus m'actaine,
Et qui plus vray, lors plus me va bourdent,
Mon ami est qui me faict entendent
D'ung cigne blanc que c'est ung corbeau noir,
Et qui me nuyst, croy qu'i m'ayde a pourvoir,
Bourde, verté, au jour d'uy m'est tout ung,
Je retiens tout, rien ne sçay concepvoir,
Bien recueully, debouté de chascun.

Prince clement, or vous plaise sçavoir
Que j'entens moult et n'ay sens ne sçavoir;
Parcïal suis, a toutes loys commun.
Que sais je plus ? Quoy ! les gaiges ravoir,
Bien recueully, debouté de chascun.

François Villon

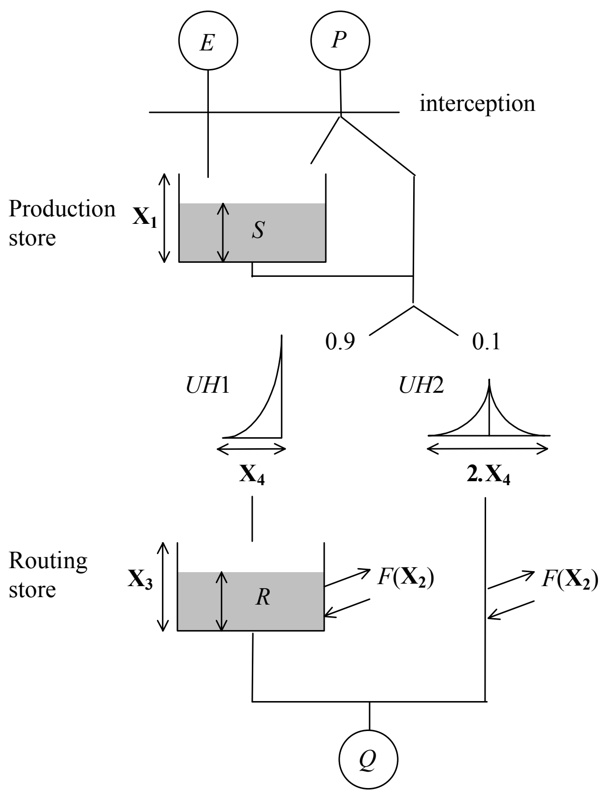
Ballade du concours de Blois

Appendix

In this thesis, the GR models are the most often used to illustrate our demonstrations relative to rainfall-runoff models. We propose here a short description of the functioning of these models: GR1A (at the annual time step), GR2M (at the monthly time step, GR4J (and one of its variant, GR3J) at the daily time step. GR5H, model foreseen to represent the rainfall-runoff relationship at the hourly time step, is still in test and will not be presented here.

Description of the daily GR model: GR4J

The GR4J model (which stands for modèle du Génie Rural à 4 paramètres Journalier) is a daily lumped 4-parameter rainfall-runoff model. It belongs to the family of soil moisture accounting models. The GR4J model is the last modified version of the GR3J model originally proposed by Edijatno and Michel (1989) and then successively improved by Nascimento (1995), (Edijatno et al., 1999), and Perrin et al. (2003). The GR4J model is very parsimonious since its structure involves only four free parameters requiring optimisation (the capacity of the production store, θ1; the water exchange coefficient, θ2; the capacity of the routing store, θ3; the time base of the unit hydrograph, θ4). The model has been extensively tested in several countries and has shown good results in comparison with other rainfall-runoff models. A full description is available in Perrin et al. (2003).

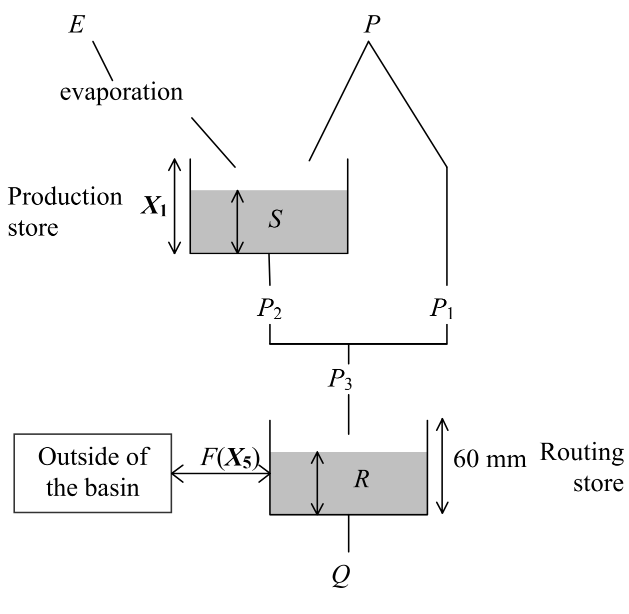


Scheme of the GR4J model

GR4J can be easily applied in many different catchments, provided that inputs of rainfall, potential evapotranspiration (also used by the diffuse pollution model) and streamflow time-series (for calibration) are available. Given its very simple structure and low number of parameters, GR4J can also be run in a spreadsheet.

Description of the monthly GR model: GR2M

The GR2M model (which stands for modèle du Génie Rural à 2 paramètres Mensuel) is a monthly lumped 2-parameter rainfall-runoff model. It belongs to the family of soil moisture accounting models. The GR2M model is the last modified version of the model initially developed by Makhlouf and Michel (1994), on the basis of the daily GR4J model and of previous attempts by Kabouya and Michel (1991).
This present version of GR2M was developed empirically, using exactly the same 429 basin sample used by Perrin et al. (2003) to develop GR4J. It is schematized below:



$$S_1 = \frac{S + X_1\varphi}{1 + \varphi \frac{S}{X_1}} \quad \text{with} \quad \varphi = \tanh\left(\frac{P}{X_1}\right)$$

$$P_1 = P + S - S_1$$

$$S_2 = \frac{S_1(1-\psi)}{1 + \psi\left(1 - \frac{S_1}{X_1}\right)} \quad \text{with} \quad \psi = \tanh\left(\frac{E}{X_1}\right)$$

$$S = \frac{S_2}{\left[1 + \left(\frac{S_2}{X_1}\right)^3\right]^{1/3}}$$

$$P_2 = S_2 - S \qquad P_3 = P_1 + P_2 \qquad R_1 = R + P_3$$

$$F = (1 - X_5).R_1$$

$$R_2 = X_5.R_1$$

$$Q = \frac{R_2^2}{R_2 + 60}$$

$$R = R_2 - Q$$

Scheme of the GR2M model

Description of the annual GR model: GR1A

The GR1A model (which stands for modèle du Génie Rural à 1 paramètre Annuel) is an annual lumped 1-parameter rainfall-runoff model. GR1A was developed during the PhD thesis of Mouelhi (2003), and is quite different from GR4J and GR2M, in the sense that it has no conceptual stores. Despite the attempts to have a coherent conceptual chain from the annual to the daily time step, the best structure that was selected at the end of the empirical search was much simple, close to the classical Turc formula for actual PE.

$$Q_n = P_n \left[1 - \frac{1}{\sqrt{1 + x^2}} \right]$$

where $x = \dfrac{0.6P_n + 0.4P_{n-1}}{\theta_1 E}$, and P and Q are indexed by the year n on which they are measured, and the same value of E is used for all years.

$\theta 1$ is the only free parameter of the model. In the test sample, the median value of $\theta 1$ was equal to 0.70 and a 80% interval was (0.33, 2.0).

In GR1A, the annual streamflow Q_n is a function of rainfall P_n of the same year, but the model still holds an interannual memory, as the rainfall of the previous year P_{n-1} is present in x, and can modify the yield of P_n.

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Curriculum Vitae en français

1. **Nom :** Andréassian
2. **Prénom :** Vazken
3. **Né le :** 11 August 1969 à Paris
4. **Nationalité :** Française
5. **Etat civil :** Marié
6. **Etudes :**

Institution :	Université Pierre et Marie Curie, Paris
Date :	09/1997 à 09/2002
Diplôme :	Doctorat en Hydrologie, école doctorale Géosciences et Ressources Naturelles

Institution :	University of Arizona, Tucson
Date :	08/1991 à 12/1992
Diplôme :	M.S. in Watershed Management

Institution :	Ecole Nationale du Génie Rural des Eaux et des Forêts, Paris
Date :	09/1990 à 12/1992
Diplôme :	Ingénieur du Génie Rural, des Eaux et des Forêts

Institution :	Institut National Agronomique Paris-Grignon
Date :	09/1988 à 07/1991
Diplôme :	Ingénieur Agronome

7. **Langues :** *(note entre 1 et 5, où 5 est le maximum)*

Langue	*Compréhension*	*Parlé*	*Ecrit*
Français	5	5	5
Anglais	5	5	5
Arménien	5	5	5
Allemand	4	4	3
Russe	3	2	2

8. **Associations Professionnelles :**

- Association Internationale des Sciences Hydrologiques (AISH)
- Comité National Français des Sciences Hydrologiques (CNFSH)
- American Geophysical Union (AGU)
- Amicale des Ingénieurs du GREF (AIGREF)

9. Autres : logiciels classiques de bureautique, de traitement d'image. Programmation en FORTRAN.

10. Position actuelle : Chef de l'équipe Hydrologie, U.R. Hydrosystèmes et Bioprocédés, Cemagref

11. Années d'expérience : 11 ans

12. Qualifications principales : Hydrologie – Ressources en eau – Drainage – Gestion des Bassins Versants – Foresterie

13. Experience d'enseignement

Université	*Période*	*Durée*	*Matière enseignée*
Université Paris 6	2002-2003 2003-2004 2004-2005	12 heures (+6 heures de TD)	Modélisation pluie-débit (cours donné en association avec Charles Perrin et des thésards pour la partie TD)
Université Paris 6	2003-2004	3 heures	Hydrologie Forestière
INA-PG	1999 2000 2001 2002	3 heures	Gestion des bassins versants en milieu aride
ENGREF	2002 2003	2 jours	Hydrologie Forestière

14. Ecadrement d'étudiants

Thésards

Nom de l'étudiant	**Années**	**Directeur de thèse**	**Université ou école d'inscription**	**Ecole doctorale**	**Sujet de thèse**
Safouane Mouelhi	1998-2002	Claude Michel	ENGREF	GRN	Vers une chaîne cohérente de modèles pluie-débit conceptuels globaux aux pas de temps pluriannuel, annuel, mensuel et journalier
Ludovic Oudin	2001-2004	Claude Michel & François Anctil	ENGREF	GRN	Recherche d'un modèle d'évaporation potentielle pertinent comme entrée d'un modèle pluie-débit global
Thibault Mathevet	2002-2005	Claude Michel	ENGREF	GRN	Modélisation pluie-débit au pas de temps fin
Claudia Rojas Serna	2002-2005	Claude Michel	ENGREF	GRN	Quelle est l'information hydrométrique nécessaire au calage de modèles pluie-débit?

DEA

Nom de l'étudiant	Année	DEA	Université	Sujet de DEA
Marie-Perrine Miossec	2004	HHGG	U. Paris 6	Approches multi-modèles pour simuler le comportement hydrologique de bassins versants non-jaugés
Marc Plantier	2003	Mécanique et Ingénierie	U. Louis Pasteur	Prise en compte de caractéristiques physiques du bassin versant pour la comparaison des approches globale et semi-distribuée en modélisation pluie-débit
Audrey Oddos	2002	Mécanique et Ingénierie	U. Louis Pasteur	Intérêt d'une approche semi-distribuée par rapport à une approche globale en modélisation pluie-débit
Nicolas Eckert	2002	HHGG	U. Paris 6	Prise en compte des couverts neigeux temporaires au sein d'un modèle pluie débit
Valérie Gentien-Baudry	1999	HHGG	U. Paris 6	La modélisation pluie-débit sur les bassins versants de Nouvelle Calédonie
Corinne Caugant	1998	Géomorphologie	U. Paris I	Impact de l'évolution du couvert forestier sur le comportement hydrologique de bassins versants du Massif Central : croisement des archives de pluies, de débits et d'inventaire forestier

Maîtrise

Nom de l'étudiant	Année	Maîtrise	Université	Sujet de Maîtrise
Nicolas Bleuse	1999	Géographie Physique	U. Paris I	Influence de la forêt sur l'écoulement et la qualité des eaux de deux petits bassins versants ruraux.

15. Expérience Professionnelle

Date	01/1995 jusqu'à présent
Lieu	Antony
Institution	Cemagref
Position	Chef de l'équipe hydrologie (depuis01/1998)
Description	• coordination scientifique d'une équipe d'environ 12 personnes (6 scientifiques, 2 techniciens, 4 thésards ou post-docs); • réponse aux appels d'offre nationaux ou européens; • enseignement (Université Paris 6, ENGREF); • recherche sur le thème de la modélisation pluie-débit et de l'impact des changements d'occupation des sols sur le comportement des bassins versants.

Date	03/1995 à 08/1995
Lieu	Erevan, Arménie
Institution	Banque Agricole Coopérative d'Arménie (projet d'assistance technique de l'Union Européenne)
Position	Consultant, chef de projet adjoint
Description	Création de caisses locales de crédit agricole dans les villages de la plaine d'Ararat.

Date	04/1993 à 02/1995
Lieu	Erevan, Arménie
Institution	Ambassade de France en Arménie
Position	Attaché pour la coopération scientifique et technique (VSN)
Description	Développement et suivi de projets de coopération entre la France et l'Arménie, principalement dans les domaines de l'irrigation, l'agriculture, l'hydrologie et la météorologie, la sismologie, la physique.

List of Publications

▪ Publications in scientific journals

1. Mouelhi, S., C. Michel, C. Perrin, and **V. Andréassian**, 2005. Stepwise development of a two-parameter monthly water balance model. *Journal of Hydrology* (in press).
2. Oudin, L., Perrin, C., Mathevet, T., **Andréassian, V.** and Michel, C., 2004. Impact of biased and randomly corrupted inputs on the efficiency and the parameters of watershed models. *Journal of Hydrology* (in press).
3. Duan, Q., Schaake, J., **Andréassian, V.**, Franks, S., Goteti, G., Gupta, H.V., Gusev, Y.M., Habets, F., Hall, A., Hay, L., Hogue, T., Huang, M., Leavesley, G., Liang, X., Nasonova, O.N., Noilhan, J., Oudin, L., Sorooshian, S., Wagener, T. and Wood, E.F., 2005. Model Parameter Estimation Experiment (MOPEX): an overview of science strategy and major results from the second and third workshops. Journal of Hydrology (in press).
4. Oudin, L., C. Michel, **V. Andréassian**, F. Anctil, and C. Loumagne, 2005. Should Bouchet's hypothesis be taken into account for estimating evapotranspiration in rainfall-runoff modeling? An assessment over 308 catchments. *Hydrological Processes* (in press).
5. Oudin, L., F. Hervieu, C. Michel, C. Perrin, **V. Andréassian**, F. Anctil, and C. Loumagne, 2005. Which potential evapotranspiration input for a lumped rainfall-runoff model? - Part 2 - Towards a simple and efficient potential evapotranspiration model for rainfall-runoff modelling. *Journal of Hydrology,* 303(1-4): 290-306.
6. Michel, C., **V. Andréassian**, and C. Perrin, 2005. The SCS-Curve Number method: How to mend a wrong soil-moisture accounting procedure? *Water Resources Research*, 41(2): doi:10.1029/2004WR003191.
7. Oudin, L., **V. Andréassian**, C. Perrin, and F. Anctil, 2005. Locating the sources of low-pass behaviour within rainfall-runoff models. *Water Resources Research*, 40(11): doi:10.1029/2004WR003291.
8. Cosandey, C., **V. Andréassian**, C. Martin, J.-F. Didon-Lescot, J. Lavabre, N. Folton, N. Mathys, and D. Richard, 2005. The hydrological impact of the Mediterranean forest: a review of French research. *Journal of Hydrology*, 301(1-4): 235-249.
9. **Andréassian, V.**, A. Oddos, C. Michel, F. Anctil, C. Perrin, and C. Loumagne, 2004. Impact of spatial aggregation of inputs and parameters on the efficiency of rainfall-runoff models: a theoretical study using chimera watersheds. *Water Resources Research*, 40(5): W05209, doi: 10.1029/2003WR002854.
10. **Andréassian, V**., 2004. Waters and Forests: from historical controversy to scientific debate. *Journal of Hydrology*, 291(1-2): 1-27.
11. **Andréassian, V.**, 2004. Couvert forestier et comportement hydrologique des bassins versants. *La Houille Blanche*, n°2: 31-35
12. Anctil, F., C. Perrin, and **V. Andréassian**, 2004. Impact of the length of observed records on the performance of ANN and of conceptual parsimonious rainfall-runoff forecasting models. *Environmental Modelling and Software*, 19(4): 357-368.

13. **Andréassian, V.**, C. Perrin, and C. Michel, 2004. Impact of imperfect potential evapotranspiration knowledge on the efficiency and parameters of watershed models. *Journal of Hydrology*, 286: 19-35.

14. Anctil, F., C. Michel, C. Perrin, and **V**. **Andréassian**, 2004. A soil moisture index as an auxiliary ANN input for stream flow forecasting. *Journal of Hydrology*, 286: 155-167.

15. Anctil, F., C. Perrin, and **V. Andréassian**, 2003. ANN output updating of lumped conceptual rainfall-runoff forecasting models. *Journal of the American Water Resources Association*, 39(5): 1269-1280.

16. **Andréassian, V.**, E. Parent, and C. Michel, 2003. A distribution-free test to detect gradual changes in watershed behavior. *Water Resources Research*, 39(9): 1252, doi:10.1029/2003WR002081.

17. Perrin, C., C. Michel, and **V. Andréassian**, 2003. Improvement of a parsimonious model for streamflow simulation. *Journal of Hydrology*, 279: 275-289.

18. Wasson, J.-G., M.-H. Tusseau-Vuillemin, **V. Andréassian**, C. Perrin, J.-B. Faure, O. Barreteau, M. Bousquet, and B. Chastan, 2003. What kind of water models are needed for the implementation of the European Water Framework Directive? Examples from France. *International Journal of River Basin Management*, 1(2): 1-11.

19. Michel, C., C. Perrin, and **V. Andréassian**, 2003. The exponential store: a correct formulation for rainfall–runoff modelling. *Hydrological Sciences Journal*, 48(1): 109-124.

20. Lavabre, J., **V. Andréassian**, and O. Laroussinie, 2002. Les eaux et les forêts. La forêt : un outil de gestion des eaux? *La Houille Blanche*, n°3: 72-77.

21. **Andréassian, V.**, C. Perrin, C. Michel, I. Usart-Sanchez and J. Lavabre, 2001. Impact of Imperfect Rainfall Knowledge on the Efficiency and the Parameters of Watershed Models. *Journal of Hydrology*, 250 (1-4): 206-223.

22. Perrin, C., C. Michel and **V. Andréassian,** 2001. Does a large number of parameters enhance model performance? Comparative assessment of common catchment model structures on 429 catchments. *Journal of Hydrology*, 242 (3-4): 275-301.

23. Meybeck, M., Z. Idlafkih, N. Fauchon and **V. Andréassian**. 1999. Spatial and temporal variability of Total Suspended Solids in the Seine basin. *Hydrobiologia*, 410 : 295-306.

24. Meybeck, M., M. Akopian and **V. Andréassian**, 1998. Le lac Sévan : une catastrophe annoncée. *La Recherche*, 310: 34-36.

▪ Submitted publications

25. Anctil, F., Lauzon, N., **Andréassian, V**., Oudin, L. and Perrin, C., 2004. Improvement of rainfall-runoff forecasts through mean areal rainfall optimization. Journal of Hydrology, submitted.

26. Mouelhi, S., Michel, C., Perrin, C. and **Andréassian, V.**, 2004. Is the Manabe 'bucket' model relevant at the annual time step? Journal of Hydrology, submitted.

27. Perrin, C., Oudin, L., **Andréassian, V.** and Mathevet, T., 2004. A data resampling approach to assess parameter uncertainty in continuous watershed models. Water Resources Research, submitted.

28. Perrin, C., Dilks, C., Bärlund, I., Payan, J.L. and **Andréassian, V.**, 2004. Use of simple rainfall-runoff models as a baseline for the benchmarking of the hydrological component of complex catchment models, submitted.

29. Perrin, C., **Andréassian, V.** and Michel, C., 2004. Simple benchmark models as a basis for criteria of model efficiency, submitted.

▪ Publications in other journals

1. Hurand, A., and **V. Andréassian**, 2003. Le couvert forestier et l'hydrologie des basins versants. *RDV Techniques ONF*, n°2: 37-41.

2. **Andréassian, V.** and J. Lavabre, 2002. Relations entre le couvert forestier et le comportement hydrologique à l'échelle du bassin versant. *Comptes Rendus de l'Académie d'Agriculture de France*, 88(7): 97-98.

3. **Andréassian, V**., M. Tangara and J. Muraz, 2001. Evaluer l'impact de l'évolution du couvert forestier sur le comportement hydrologique des bassins versants : méthodologie et premiers résultats fondés sur les données de l'IFN. *Revue Forestière Française*, LIII (3-4) : 475-480.

4. **Andréassian, V.**, 2001. L'invention des bassins versants expérimentaux pour tenter de résoudre la controverse hydrologique sur les forêts au 19ème siècle. *Bulletin du Groupe Francophone Humidimétrie et Transferts en Milieux Poreux*, n°46: 73-81.

5. Perrin, C., C. Michel and **V. Andréassian**, 2001. Long-term low flow forecasting for French rivers by continuous rainfall-runoff modelling. *British Hydrological Society Occasional Paper*, n° 13: 21-29.

6. Muraz, J., S. Durrieu, S. Labbé, **V. Andréassian** and M. Tangara, 1999. Comment valoriser les photos aériennes dans les SIG? *Ingénieries - EAT*, 20 : 39-58.

7. **Andréassian, V.** and J. Margat, 1997. Prospective des besoins en eau mondiaux à l'horizon 2025. *Ingénieries-EAT*, numéro spécial 1997, pp17-34.

8. Meybeck, M., M. Akopian and **V. Andréassian**, 1997. What happened to lake Sevan? *Silnews*, 23: 7-10.

9. **Andréassian, V.**, P. Tatéossian and L. Minassian, 1997. L'agriculture arménienne dans la tourmente post-soviétique. *Comptes rendus de l'Académie d'Agriculture de France*, 83(8): 5-16.

10. **Andréassian, V.**, 1995. Les forêts d'Arménie. *Revue Forestière Française*, XLVII (3) : 273-278.

▪ Publications in books

1. **Andréassian, V.**, 2005. Pourquoi les rivières débordent ? Editions le Pommier, 56 p.

2. **Andréassian, V.**, V. Sarkissian, W. Chełmicki, V.Al. Stănescu and R. Moussa, 2001. Lexique Hydrologique pour l'Ingénieur : Anglais-Français-Arménien-Russe-Polonais-Roumain-Arabe. Cemagref, Antony. 210 p.

   http://www.cemagref.fr/Informations/Produits/Lexique_hydro /index.html

3. **Andréassian, V.**, O. Barreteau, M. Bousquet, B. Chastan, J.-B. Faure, C. Perrin, M.-H. Tusseau-Vuillemin, and J.-G. Wasson, 2000. What kind of water models are needed for the implementation of the European Water Framework Directive? In : M.

Menéndez Prieto, editor. Seventh Euraqua scientific and technical review. Euraqua, Madrid, pp. 31-46.

4. Lavabre, J. and **V. Andréassian**, 2000. Eaux et forêts. La forêt : un outil de gestion des eaux ? Cemagref, Antony. 147 p.
5. **Andréassian, V.**, 1999. Analyse de l'action de l'homme sur le comportement des bassins versants et le régime des crues. In: Leblois, E., L'influence humaine dans l'origine des crues. Cemagref éditions, pp. 47-65.
6. **Andréassian, V**., 1999. La perception sociale de l'influence humaine sur le régime des crues. In: Leblois, E., L'influence humaine dans l'origine des crues. Cemagref éditions, pp. 15-18.
7. **Andréassian, V.**, 1999. Utilisation de modèles pluie-débit simples pour analyser l'impact de l'évolution du couvert végétal sur l'hydrologie des bassins versants. In : Mathys, N., Les bassins versants expérimentaux de Draix, laboratoire d'étude de l'érosion en montagne. Cemagref éditions, Antony. pp 77-87.
8. **Andréassian, V.** and E. Gaume, 1998. Comment les besoins en eau évolueront-ils? Prospective à l'horizon 2025. In: J. Margat and J.-R. Tiercelin, L'eau en questions. Romillat, Paris. pp 123-148.
9. Meybeck, M., J.-M. Mouchel, Z. Idlafkih, **V. Andréassian** and S. Thibert, 1998. Transferts d'eau, de matières dissoute et particulaire dans le réseau fluvial. In : M. Meybeck, G. de Marsily and E. Fustec, La Seine en son bassin : fonctionnement écologique d'un système fluvial anthropisé. Elsevier, Paris. pp 345-389.
10. Sarkissian, V. and **V. Andréassian**. 1995. Lexique des sciences hydrologiques : Anglais-Français-Arménien-Russe. Erevan. 149 p.

▪ Conference proceedings

1. **Andréassian**, V., L. Oudin, C. Rojas-Serna, C. Michel, and C. Perrin. 2003. *A priori* parameter estimation for the GR4J rainfall-runoff model: a contribution to the MOPEX experiment. IUGG-IAHS General Assembly, Sapporo, July 2003.
2. Oudin, L., V. **Andréassian**, C. Michel, C. Perrin, and F. Anctil. 2003. Which potential evapotranspiration input for a lumped rainfall-runoff model? IUGG-IAHS General Assembly, Sapporo, July 2003.
3. **Andréassian**, V., A. Oddos, C. Michel and C. Perrin. 2003. Chimera watersheds to understand the relative importance of rainfall distribution in semi-distributed rainfall-runoff models. Conference on Hydrology of the American Meteorological Society, Long Beach, 10-13 February 2003.
4. **Andréassian**, V. and J. Lavabre, 2002. Relations entre le couvert forestier et le comportement hydrologique à l'échelle du bassin versant. Séance commune de l'Académie des Sciences et de l'Académie d'Agriculture de France, Paris, 13 novembre 2002.
5. **Andréassian, V.**, E. Parent, and C. Michel, 2002. Using a parsimonious rainfall-runoff model to detect non-stationarities in the hydrological behaviour of watersheds. First biennal meeting of the International Environmental Modelling and Software Society (iEMSs), 24-27 June 2002, Lugano. Vol. 1, pp 458-463
6. Eckert, N., C. Michel and **V. Andréassian**, 2002. Prise en compte des couverts neigeux temporaires au sein d'un modèle pluie-débit. Conférence sur l'hydrologie nivale en méditerrannée, Beyrouth, décembre 2002.

7. Lavabre, J., **V. Andréassian**, and O. Laroussinie, 2001. Les eaux et les forêts. La forêt : un outil de gestion des eaux? 168ème session du Comité Scientifique et Technique de la SHF, Nancy, 26-28 septembre 2001. pp. 135-144.

8. **Andréassian, V.**, 2001. Histoire conjointe des eaux et des forêts. 168ème session du Comité Scientifique et Technique de la SHF, Nancy, 26-28 septembre 2001. pp. 53-60.

9. **Andréassian, V.** (coord.), 2001. Comptes rendus des Premières rencontres Aix-Antony sur la modélisation pluie-débit, Antony, 11 décembre 2001. Cemagref, Antony, 62 p.

10. **Andréassian, V.**, 2001. La controverse sur le rôle hydrologique des forêts en France au 19ème siècle. Colloque OH2 " Origines et Histoire de l'Hydrologie ", Dijon, 9-11 mai 2001.

11. Perrin, C., C. Michel and **V. Andréassian**, 2001. Long-term low flow forecasting for French rivers by continuous rainfall-runoff modelling. *Meeting of the British Hydrological Society on Continuous River Flow Simulation*, *Wallingford, UK, 5th July 2001,* Littlewood, I.G. (Ed.), BHS Occasional Paper n° 13: 21-29.

12. **Andréassian, V.**, 2000. L'invention des bassins versants expérimentaux pour tenter de résoudre la controverse hydrologique sur les forêts au 19ème siècle. 25èmes journées scientifiques du GFHN, Meudon 28-29 novembre 2000, 9 p.

13. **Andréassian, V.**, Barreteau, O., Bousquet, M., Chastan, B., Faure, J.-B., Perrin, C., Tusseau-Vuillemin, M.-H., Wasson, J.-G., 2000. Quels types de modèles de la ressource en eau sont nécessaires pour la mise en place de la nouvelle directive cadre européenne sur l'eau. Le cas de la France. Seventh Euraqua Scientific and Technical Review, Madrid, 17-20 octobre 2000, 15 p.

14. Lavabre, J. and **V. Andréassian**, 2000. Les eaux et les forêts. La forêt : un outil de gestion des eaux ? Colloque international sur l'eau, l'aménagement du territoire et le développement durable, 10-11 février 2000, Sénat, Paris. p 169-178.

15. **Andréassian, V**., C. Perrin, C. Michel, 1999. Sensitivity of catchment model parameters to raingage network characteristics. International Conference on Quality, Management and Availability of Data for Hydrology and Water Resources Management, Koblenz, Germany. 1999.

16. **Andréassian, V.**, 1997. Utilisation de modèles pluie-débit simples pour analyser l'impact de l'évolution du couvert végétal sur l'hydrologie des bassins versants. Séminaire consacré aux bassins versants expérimentaux de Draix, laboratoire d'étude de l'érosion en montagne, Digne, 22-24 octobre 1997, pp 77-87.

17. **Andréassian, V.**, 1996. Analyse de l'action de l'homme sur le comportement des bassins versants et le régime des crues. Colloque sur l'influence humaine dans l'origine des crues, Paris, 18-19 novembre 1996, pp. 47-65.

18. **Andréassian, V**., 1996. La perception sociale de l'influence humaine sur le régime des crues. Colloque sur l'influence humaine dans l'origine des crues, Paris, 18-19 novembre 1996, pp. 15-18.

▪ Theses

1. **Andréassian, V.**, 2002. Impact de l'évolution du couvert forestier sur le comportement hydrologique des bassins versants. Ph.D. Thesis, Université Pierre et Marie Curie, Paris, 781 pp.

2. **Andréassian, V.** 1992. Comparative Hydrology of Mediterranean shrublands. MS Thesis. University of Arizona, Tucson. 139 p.

Curriculum Vitae in English

1. **Surname:** Andréassian
2. **Name:** Vazken
3. **Date of birth:** 11 August 1969 - **Place of birth:** Paris, France
4. **Nationality:** French
5. **Civil status:** Married
6. **Education:**

Institution:	Université Pierre et Marie Curie, Paris
Date:	09/1997 to 09/2002
Degree:	Ph.D. in Hydrology
Institution:	University of Arizona, Tucson
Date:	08/1991 to 12/1992
Degree:	M.S. in Watershed Management
Institution:	Ecole Nationale du Génie Rural des Eaux et des Forêts, Paris
Date:	09/1990 to 12/1992
Degree:	Ingénieur du Génie Rural, des Eaux et des Forêts
Institution:	Institut National Agronomique Paris-Grignon
Date:	09/1988 to 07/1991
Degree:	Ingénieur Agronome

7. **Language skills:** *(Mark 1 to 5 for competence, where 5 is the highest)*

Language	*Passive*	*Spoken*	*Written*
French	5	5	5
English	5	5	5
Armenian	5	5	5
German	4	4	3
Russian	3	2	2

8. **Membership of professional bodies:**
   - International Association of Hydrological Sciences (IAHS)
   - French National Committee for Hydrological Sciences (CNFSH)
   - American Geophysical Union (AGU)
   - Amicale des Ingénieurs du GREF (AIGREF)

9. Other skills: computer literacy in classical software (MS Office), plus in FORTRAN for scientific programming, Image processing software, Basic GIS software.

10. Present position: Team leader, Hydrology Research Group, Cemagref

11. Years of experience: 11 years

12. Key qualifications: Hydrology - Water Resources – Drainage - Watershed Management – Forestry - Soil Conservation

13. Teaching experience

University	Years	Duration	Brief description of the class
Université Paris 6	2002-2003 2003-2004 2004-2005	12 hours (+ 6 hour lab)	Modeling of the rainfall-runoff relationship (class given with Charles Perrin and graduate students for the lab session)
Université Paris 6	2003-2004	3 hours	Forest Hydrology
INA-PG	1999 2000 2001 2002	3 hours	Watershed management in arid environments
ENGREF	2002 2003	2 days	Forest hydrology

14. Follow-up of graduate students

PhD Students

Student name	Years	Main Advisor	Ecole doctorale	Thesis subject
Safouane Mouelhi	1998-2002	Claude Michel	GRN	A coherent chain of rainfall-runoff models at different time steps.
Ludovic Oudin	2001-2004	Claude Michel & François Anctil	GRN	Potential evaporation for the modeling of the rainfall-runoff relationship at the watershed scale.
Thibault Mathevet	2002-2005	Claude Michel	GRN	Rainfall-runoff modeling at the hourly time step.
Claudia Rojas Serna	2002-2005	Claude Michel	GRN	How much hydrometric information is needed for the calibration of rainfall-runoff models?

DEA students

Student name	Year	DEA	University	Thesis subject
Marie-Perrine Miossec	2004	HHGG	U. Paris 6	Multi-model approaches for ungaged watersheds
Marc Plantier	2003	Mécanique et Ingénierie	U. Louis Pasteur,	Accounting of catchment physical attributes for in the comparison of lumped and semi-distributed modeling approaches in rainfall-runoff modeling
Audrey Oddos	2002	Mécanique et Ingénierie	U. Louis Pasteur,	Semi-distributed approaches compared to lumped ones in rainfall-runoff modeling
Nicolas Eckert	2002	HHGG	U. Paris 6	Taking into account temporary snowcovers within rainfall-runoff models

Student name	Year	DEA	University	Thesis subject
Valérie Gentien-Baudry	1999	HHGG	U. Paris 6	Rainfall-runoff modeling in New Caledonia
Corinne Caugant	1998	Géomorphologie	U. Paris I	Impact of forest cover evolution on catchment hydrological behavior in the Massif Central highlands.

Maîtrise students

Student name	Year	Maîtrise	University	Thesis subject
Nicolas Bleuse	1999	Géographie Physique	U. Paris I	Impact of forest on streamflow and water quality in two small rural catchments.

15. Professional experience

Date	01/1995 to present
Location	Antony, France
Company	Cemagref (French Research Center for Agricultural and Environmental Engineering)
Position	Head of the Hydrology team (since 01/1998)
Description	• scientific coordination of a team of approximately 12 people (6 scientists, two technicians, plus 4 PhD students or post-docs); • research contracts with French or European agencies; • teaching (Université Paris 6, ENGREF); • work on the utilization of rainfall-runoff models (GR4J) developed by Cemagref for engineering applications • work on the impact of land-use changes on watershed behaviour.

Date	03/1995 to 08/1995
Location	Yerevan, Armenia
Company	Agricultural Cooperative Bank of Armenia (a EU TACIS project)
Position	Consultant, deputy project manager
Description	Creation and organization of local credit unions in the villages of the Ararat valley. Relationships with counterparts at the Ministry of Agriculture and the Central Bank of Armenia.

Date	04/1993 to 02/1995
Location	Yerevan, Armenia
Company	French Embassy in Armenia
Position	Attaché for Scientific and Technical Cooperation
Description	Duties implied developing and implementing cooperation projects between France and Armenia, mainly in the following domains: Irrigation, Agriculture, Hydrology and Meteorology, Sismology, Physics.

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