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7979 DISCUSSION

Flood estimation for small East African rural catchments

D. FIDDES

Mr M. R. Hasan, Sir William Halcrow and Partners

Having read the Paper in conjunction with the report[12] on which it is based, I should like to comment on §§ 49–53 and Table 10.

58. I am pleased that Richards' method[11] is one of the two 'most commonly applied methods of flood estimation for small catchments in Kenya'. Recently my colleagues and I found that Richards' method is used extensively by French hydrologists in North Africa and Iran. British consultants have also applied it to the semi-arid and arid regions of South America, Africa and the Middle East.[13–15] It has been used in the design of many water resources and hydro-power projects in the UK and recently a comparison of Richards' method and a new method[16] yielded encouraging results.[17]

59. Having established the fact that Richards' method has been applied in a variety of climatic conditions with a good degree of success, the Author's statement in § 50 should not go unchallenged. I have read Richards' papers[11,18,19] and these show the Author's statement to be inaccurate and that the Author has misrepresented Richards' method.

60. First, Richards' method does not assess the runoff factor on the basis of rainfall and catchment characteristics as the Author asserts.

61. Second, the method can best be described as quasi-rational inasmuch as it makes the fundamental assumption that the peak flood flow from a catchment results from a rainfall of some average intensity extending to the whole of the catchment and having a duration at least equal to the time of concentration T_c.

62. It is in the determination of T_c that Richards' method differs significantly from other methods. Although most formulae specify T_c in terms of a function of catchment slope and length (e.g. references 20–22) Richards' method, by inclusion of rainfall parameters and especially by the innovative rainfall coefficient, attempts to make allowance for the theoretically correct concept that stream velocity is dependent on depth. The time of concentration is based on the velocity of overland flow which Richards visualized as a sheet of water, the properties of which conform to the Bazin equation. Despite this quasi-theoretical treatment of the progress of overland flow and some other drawbacks (e.g. subjective selection of the runoff factor K) the method recognizes that velocity (and hence T_c) is basically a function of depth and catchment roughness.

63. Third, Richards' method does not compute floods of specified return periods (say ten years). What it simply does is, depending on the chosen values of the rainfall coefficient, storm shape factor area, reduction factor and runoff factor, give a peak flood

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flow. What assumptions did the Author make in arriving at his ten year flood estimates using Richards' method?

64. The Author has grouped catchments of sizes varying in area from 45 ha to 83·5 km² in Table 10. Such a grouping of small, medium and large catchments may not be hydrologically sound and efficient. Except for the largest catchment in Table 10 (Migwani), ten year flood estimates derived from the TRRL method and computer model are lower than those obtained from the other three methods used in the comparison. The Author justifies his significantly higher Migwani estimate by saying that a flood exceeding the computer model ten year estimate occurred after the research programme was completed. Does this imply that if this recorded flood had been lower than his ten year estimate, he would have called the latter an overestimate?

65. How can the Author be sure that his results are 'the best estimate of the ten year flood' (§ 52)? Simply because the predictions by two (or even three) methods used in comparison show good agreement the Author infers that it 'is not necessarily a confirmation of the accuracy of prediction' (§ 53). Why did the Author not chose standard methods such as those in references 16 and 20 in his comparison (the method in reference 16 has been applied to catchments in New Zealand to establish its universality of application)?

66. The TRRL did some useful hydrological work on surface and storm drainage before the Institute of Hydrology was created. I think the TRRL and the Institute should now collaborate in hydrological research on problems pertaining to roads and bridges in the UK and overseas.

Mr J. O. Robertson, Land Resources Division, Ministry of Overseas Development

The Paper represents a useful advance in the study of flood runoff. I was concerned with flood estimation in Kenya from 1950 to 1963.

68. Was the measurement of soil moisture on a few key catchments considered? If so, what were the reasons for exceeding this parameter in view of the large absorption, often up to 50 mm, which was observed on many catchments before there was appreciable runoff?

69. I find the delineation of the zones in Fig. 2 difficult to comprehend, e.g. the Kitui area is described as one in which the soil is likely to be at field capacity, whereas areas such as Kericho and Kisil are classed as dry. Is the more dense vegetation in Nyanza causing greater moisture stress?

70. In § 49 reference is made to the East African Railways and Harbours nomograph. Is this the 'ray' diagram employing tabulated coefficients of shape, slope and vegetation? I always understood that that originated in South Africa and was introduced by Stanley. Could the Author give further details?

71. Both this method and Richards' method require great care in selecting the right coefficients. It is easy to assume generous values and produce an over-generous design.

72. A third useful check was the general flood area diagram, which gave an overall envelope peak Q of $1600A^{1/2}$ where Q was measured in cusecs, and A in square miles, with suitable adjustments made for abnormal shapes, slopes and vegetation. About 70% of the catchments which I examined for bridge design could be considered as normal. The return period of this envelope was greater than 25 years, and so a ten year flood was probably about $800A^{1/2}$ or say $14A^{1/2}$ in SI units. Applying this to Table 10, for the Tiwi catchment Q_p equals 36 m³/s and for the Migwani catchment Q_p equals 128 m³/s. Similarly, for Kajiado Q_p equals $14\times1{\cdot}9=26{\cdot}5$ m³/s. Normally, the equation was not used on catchments smaller than 2 square miles (5 km²).

73. I agree that the old methods could overestimate flows if they are not used properly. However, rational design of main bridges and culverts was based on a much higher return period than ten years, namely 25–30 years, and therefore hydrograph attenuation resulting from the effects of vegetation and so on was not nearly so marked as on a ten year flood.

74. Irrespective of the progressive refinement of hydrological computation, the bridge designer must ask himself, 'What waterway must I allow to take care of blockage by debris?' The aim was to allow a minimum width of 25 ft on bridges and spillways, but this was not always possible. It follows that the designer is interested in another parameter, namely the 'stability' of trees and shrubs on the catchment, and so he should seek expert advice from a range land specialist and a forester to determine tree and shrub root systems, spacing, age, and so on, and he himself should be able to assess, if only roughly, the debris-producing characteristic of the catchment, if this specialist information is not available.

Mr P. F. Scott, East African Engineering Consultants (Kenya)

Results obtained using the Kenya Ministry of Water Development approach based on Richards' method and the East African Railways and Harbours nomograph popularly used in Kenya to estimate runoff from rural catchments have in some instances been open to doubt. Table 10 highlights the extent of the problem, and goes a long way towards providing an accurate empirical approach for the design of small river bridges and culverts, which will prove helpful to those involved in such work in East Africa, although the Author's catchments tend to be small compared with those normally analysed by my firm. The major difference in analysis resulting from this variation in catchment size is that the Author has assumed rainfall covering the complete catchment, whereas in many cases on larger areas only a section of a catchment receives rain at any one time.

76. A reliable approach to culvert design is to investigate in detail the performance of existing structures used by each particular stream under consideration coupled with an analysis of recent rainfall and past trends in storm patterns. In most circumstances in Africa an old road runs near to or coincident with a proposed highway route and, while using empirical methods of flood estimation as a check, final culvert sizes have been generally influenced by observation on site.

77. The Author has endeavoured to consider all relevant factors in relation to runoff from catchments in East Africa. However, there seem to be certain omissions and apparent anomalies in the Paper.

78. Paragraph 23 appears to ignore significant additional factors which affect initial retention. For instance a value of 5 mm cannot be assumed in all semi-arid areas. The state of compaction or in situ density of soils significantly influences initial retention. If the ground is sufficiently hard and baked dry, little absorption takes place. If it is loose, water soaks in easily. Often a layer of impervious material forms a crust on top of the ground inhibiting retention until it is eroded a considerable time after the start of the storm, and retention occurs in mid-flood.

79. Depression storage (i.e. the amount of rainwater which collects in surface puddles and ponds) also influences the amount of initial retention. This retention becomes particularly significant in flat or depressed countryside as in the Kajiado catchment area. It is therefore surprising that a retention of zero is recorded for this area; I would have expected depression storage to have provided a significant retention figure. Although the TRRL method takes account of the effect of vegetation on lag time, I do not consider that its influence on retention should have been omitted. Column 2 of Table 6 therefore seems to contain certain ambiguities. For example, I would have expected the Saosa catchment containing dense forest to have an initial retention greater than zero.

80. The Mudanda and Eseret catchments are similar in topography and vegetation, yet surprisingly their retention factors vary considerably. How did the Author obtain his retention figures?

81. In connection with the design of road drainage and predicted flow capacity requirements for culverts, the Paper does not mention the influence of groundwater although this is of great significance, particularly in the semi-arid territory of north-east

Kenya. An analysis of the type propounded by the Author is difficult in such circumstances and each case has to be designed on its own merits and by specific observation at the particular culvert site. However, in certain circumstances it has been necessary to cater for groundwater emergence amounting to twice the predicted surface runoff.

82. In § 2 the Author says that only rainfall and stream flow were measured on site. Were intensity of rainfall measured, porosity or permeability of soil, depth of soil to rock, evaporation, wind direction and velocity, temperature and so on not measured?

83. How does the Author define 'aridity'? The nomads of north-east Kenya would not consider their area as semi-arid, nor would the people of Kitui and western Nyanza think their areas are wet and dry respectively.

84. With reference to § 15, it is contentious to suggest that there is no runoff when the catchment surface is dry and that only areas adjacent to the stream contribute. Surely this depends on the duration and intensity of the storm. Of course, as soon as a storm starts the catchment area is no longer dry.

85. Tables 3–5 show figures which have been derived in an empirical fashion. How does one compare 'slightly impeded' with 'impeded' drainage? Is 'very flat' not well drained and is the coefficient zero? If so, C_A is zero and there is no runoff, which seems strange. Also I would have expected the factors for swamp filled valley and forest to be different.

86. Time is a factor in percolation. If a severe storm occurs runoff must be high as the ground will be unable to absorb moisture sufficiently quickly to reduce the soil moisture deficit.

87. In the comparison of floods in Table 10 there are surprising variations in the results obtained using the various methods. For instance, Migwani catchment area is about 12 times the size of Tiwa, yet the maximum ten year flood at Migwani is about 68 times that at Tiwa. This is contrary to all flood studies where the smaller catchment gives a proportionately larger flood flow (there is less dissipation of storm and the time of concentration is less). Was the flood at Migwani in § 52 perhaps the 100 year flood?

88. Although the statement that the use of other methods will lead to over design is not substantiated, there is no doubt in many cases that the use of the TRRL method may lead to economy.

89. The Paper does not give the complete answer but it is an excellent study of an important problem in East Africa.

Dr M. J. Hall, Sir William Halcrow and Partners

In § 1 the Author refers to the relatively high proportion of the total cost of road building in developing countries which is absorbed by the construction of small culverts and river crossings. Despite the economics which would accrue from the application of improved flood estimation procedures in the design of such crossings, few attempts have been made to carry out systematic hydrological studies for the purposes of devising appropriate methods of approach. This Paper is therefore particularly welcome.

91. Even with four years of data (§ 13) from each of 14 catchment areas (Table 2), the Author has been faced with the problem of synthesizing design flood hydrographs corresponding to recurrence intervals well in excess of the available record length. His solution to this problem has been to use the simple catchment model described in §§ 10–12. Unfortunately, neither this description nor that in reference 12 provides more than superficial details of the manner in which the model was fitted to individual catchment areas.

92. In § 11 the Author describes a sub-catchment model with three parameters. The criteria for determining the appropriate number of sub-catchments are not mentioned. In § 12 the translation of sub-catchment runoff to the catchment outfall is dismissed in a brief reference to the use of a modified finite difference scheme. Both the number of sub-catchments and the translation procedure are fundamental to the structure of the

catchment model. In particular, they determine the number of parameters that are required to transform the rainfall input into a streamflow output. In turn, the number of model parameters provides an indication of the difficulties that are likely to be encountered in fitting the model to a given catchment. The statement in § 13 that the model was run for each large storm on a catchment varying only K and C_A appears to indicate that several parameter values were fixed arbitrarily. Such a procedure implies either a high degree of confidence in determining such values from maps or site inspection or a lack of sensitivity of the discharge output to variations in the level of such parameters (in which case an even simpler model could have been devised). Could the Author give further details of the model fitting procedure? In particular, why did he not let the data speak for themselves and apply an automatic optimization procedure[23] to determine the model parameters?

93. Having obtained what he refers to in § 13 as the optimum values of K and C_A (and these values are only optimal with an unstated number of additional parameters held constant), the Author then generalizes these values to permit the application of the model to ungauged catchments. In the course of this, two major assumptions are made. In § 19 the reduction in C_A was assumed to vary linearly with soil moisture deficit and in § 23 initial retention Y was assumed independent of soil moisture deficit. Could the Author explain the background to these assumptions?

94. Given the catchment model and the generalized parameter values, the Author describes (§ 24) the synthesis of ten year floods for each catchment. A further assumption is made that the ten year flood can be constructed from the ten year storm profile. The derivation of such storm profiles for East Africa has been described elsewhere.[24] Are the available data sufficient to support the one to one correspondence between storm and peak discharge frequencies?

95. Paragraphs 27–43 describe a short-cut flood estimation procedure which avoids the use of the simplified catchment model. Owing to the wide variation in the ratios of the time to peak to the time base of the design hydrographs for the East African catchments, dimensionless hydrograph shapes of the type described by equation (2) were found inapplicable. Would the variations have been as marked if lag time (defined as the time interval between the centroids of the rainfall input and the discharge output) had been used instead of time to peak? This method of scaling has been successfully applied to unit hydrographs from catchment areas subjected to urban development in the UK[25] and rural catchment areas in Hong Kong.[26]

96. In estimating the time base of the design hydrograph for the short-cut method, the rainfall time T_P is defined as the time during which 60% of the total storm rainfall occurs. Could the Author clarify this definition? Also could he state whether equation (8) for the flood wave attenuation time is a regression equation? If so, what are the multiple correlation coefficient and the standard error of estimate?

97. It would be helpful to potential users of the proposed method if the Author could give a worked example.

Mr Fiddes

I referred to Richards' method because it is widely used in East Africa. I am sorry if Mr Hasan did not like my description; I should be happy with his quasi-rational label for it. The method was applied as recommended by the Kenya Ministry of Water Development except that once the time of concentration had been calculated the ten year return period rainfall intensity was computed using the techniques described in reference 9.

99. In § 52 I tried to explain why I should expect the TRRL method generally to give lower estimates of peak flow than the other methods with which it was compared. The catchments were selected to cover a wide range of response, resulting in there being few which gave high specific runoff. For this reason the Migwani result stands out in Table 10. The large flood subsequent to the research programme was therefore noteworthy.

100. It is generally accepted that a flood estimate based on even limited flow data from a catchment is more reliable than one based on catchment characteristics.[16] The computer model estimate was based on all available high flow data on the catchments. In most cases these data were very limited due to the short period of measurement, but even so with the computer model they provided the most reliable estimate of the ten year flood.

101. When I compared the estimates given by the short method with other techniques I had to be selective. I considered it most useful to confine attention to methods I knew were widely used by engineers in tropical Africa. I should have liked to have made an exception and considered the methods described in the *Flood studies report*.[16] However, the hydrological research programme at TRRL was terminated before their publication. Unfortunately this means also that Mr Hasan's final suggestion is not possible.

102. I did consider the measurement of soil moisture in the field as suggested by Mr Robertson but had to abandon the idea due to cost and difficulty of sampling the variation over the catchment adequately. Recharge during the early part of the rainy season was calculated from the difference between measured rainfall and estimated evapo-transpiration, there being negligible runoff at this time. On most catchments the recharge before field capacity occurred, shown by the value of C_A levelling off, was of the order of 150 mm.

103. The antecedent wetness zones shown in Fig. 2 appear to have caused some confusion. It might have been better if I had used labels other than 'wet' and 'dry' which most people tend to relate to annual rainfall. In this context they refer to the probability of soil being at field capacity when a large storm occurs. Thus a catchment in an area of high annual rainfall can be dry if the rain is fairly evenly spread throughout the year and heavy storms tend to occur after a few dry days. Whereas a catchment in an area of lower annual rainfall can be wet if the rain is concentrated in short seasons and heavy storms tend to occur after several days of lighter rain. Reference 5 describes a study in which 12 years of rainfall records at 153 stations throughout East Africa were examined. Three measures of antecedent wetness were calculated for the period immediately before each storm greater than 50 mm. These were the rainfall in the previous two and seven days and the estimated soil moisture recharge in the previous 30 days. From these results it was possible to identify the zones in Fig. 2 where there was a high probability of the catchment being wet when potential flood producing storms occurred.

104. The East African Railways and Harbours nomograph is indeed the ray diagram originally introduced from South Africa. I agree with Mr Robertson that using this and Richards' method over-design on many catchments is easily achieved by the inexperienced. The same criticism can be made of envelope curve methods.

105. Mr Robertson has raised a very interesting point in his reference to the difficulties of allowing for the passage of debris by bridge openings. I would question the practice of designing for a return period as long as 25 years on relatively small catchments, partly because of the uncertainty of such predictions and partly because such floods inevitably carry large amounts of debris which could block bridge openings. I would suggest that a more economic solution is to standardize on a ten year return period with consideration given to alternative routes for the safe passage of excess flood water from larger floods. For example, the approaching embankments to a bridge can be set at a slightly lower level than the bridge deck itself so that a safe spillway is available at times of high flood or if the bridge opening is blocked. This also limits the scouring velocities in the bridge opening.

106. Mr Scott's suggestion that the past performance of any existing structure on a stream or any similar stream should be considered, is good practice and should be part of the initial route reconnaissance. I would also add evidence of historic flood levels found from marks on the river banks or from discussion with local residents.

107. Mr Scott has highlighted some of the problems in flood prediction in semi-arid

areas. Another would be the reduced flood potential of storm rainfall once a cover of grass has become established as the rainy season proceeds, due to increased infiltration and retention. The TRRL flood method could not allow for all of these because of the lack of sufficient semi-arid catchments in the sample from which it was developed.

108. In fitting the computer model to the recorded flood hydrographs it was found that the estimation of retention was not critical. Lag time is of much greater significance. For this reason the estimation of initial retention was kept as simple as possible. The values for retention in Table 6 were therefore arrived at from the considerations described in § 103. Although described as 'level to depressed' in a Kenya atlas, Table 9 shows that ground slopes in the Kajiado area are surprisingly high.

109. With reference to the points raised by Mr Scott in §§ 84 and 85 it is important to remember that my aim was to develop a flood model. A much more comprehensive model would be required to simulate accurately the resulting stream flow from any rainfall event, given any antecedent conditions. Differentiating between impeded and slightly impeded drainage can be difficult in some cases. As a guide a simplified map based on the East African soils map[27] is given in reference 12. In Table 3 no figure is given for a well drained very flat catchment as such a combination is unlikely to be found. The response of swamp filled catchments is assumed to be very different from that of forest (Table 6).

110. I would have agreed with Mr Scott's observation in § 87 if the soil types and vegetation on the two catchments were similar. The soil at Tiwi is very sandy and because of the adequate coastal rainfall is covered with lush vegetation. The soil at Migwani is much less permeable and the vegetative cover much more sparse. This accounts for the much higher specific runoff at Migwani.

111. I must agree with Dr Hall that I did not discuss the computer model at length in the Paper. My reason was that I wished to concentrate attention on the short method and a comparison of results with other techniques. More details are given in reference 12, particularly in the derivation of the translation equations which are fully covered as an appendix. A second appendix gives a worked example.

112. So long as a reasonable number of sub-catchments were used the fitting process was not sensitive to the actual number. For the larger catchments, sub-catchments indicated by the natural stream branching proved acceptable. For the smaller catchments a minimum of five was generally necessary. To check that an adequate number were being used it was necessary to repeat the computer run for a sample of the storms using an increased number of sub-catchments and to check that the same optimum values for the parameters were obtained.

113. The model contains four parameters which are not fixed by the catchment geometry: K, C_A, Y and n. Of these Y and n were held constant. A crude estimate of Y, the initial retention, was found to be adequate as it affected only the toe of the hydrograph and was calculated using estimates of evapo-transpiration after the last period of rainfall. It was not considered appropriate to optimize Manning's n and this was estimated from tables.

114. The reduction in C_A varying linearly with soil moisture deficit can be justified by plotting the optimum values of C_A against soil moisture recharge. The relationship could not be seen clearly on all catchments due to the short period of record, but supporting evidence was obtained from a parallel study on urban drainage.[28]

115. Mudanda was the only typically semi-arid catchment. Here the appropriate initial retention appeared to be about 5 mm and independent of soil moisture deficit. This conclusion has been reported by Fogel and Duckstein,[29] although when dealing with semi-arid catchments the reservations made in § 107 must be remembered.

116. The assumption that a ten year flood will result from a ten year storm falling on a catchment with soil moisture conditions assessed by a statistical analysis of past rainfall records, although commonly made, is one that Dr Hall is right to question. The only way to check it is to do an extreme value analysis of the flood flows and compare the return period of each flood with the return period of the corresponding rainfall.

Obviously with only four years of data this was not possible. I should expect it to hold reasonably well for wet zone catchments but it could be an over-simplification for dry zone catchments. Further study on this would be useful.

117. The use of lag time between centroids of rainfall and runoff would be an alternative to the use of hydrograph base time. However, I prefer the latter as I consider it is easier for potential users to visualize.

118. The rainfall time T_p allows for the effect of storm duration on hydrograph base time. Values of 50–90% were considered; 60% gives the most consistent values of T_A when substituted in equation (9). Equation (8) was generated using synthetic data derived from the simulation exercise described in § 31. Measures of goodness of fit can therefore be misleading. It would be useful to re-examine this equation using real data when sufficient from East Africa are available.

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