Hydrometeorology and river flood estimation

by

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Mr L. B. Escritt (Chartered Civil Engineer) referred to § 38 and observed that Bilham's formula relating intensity, duration, and frequency of storms would appear to be generally adequate so far as Gt Britain was concerned and there had been more recent contributions in that direction. He understood that some work was in hand relating to the change of intensity of precipitation during the build-up of storms from start to peak and tail-off to end. Dr Glasspoole had made a study of the areas covered by the most widespread and disastrous downpours in Gt Britain. Mr Escritt's own formula, which gave a reasonable interpretation of Glasspoole's data, was:

$$R = \frac{5 \log_{10} T + 2 - 1.5 \log_{10} A}{T}$$

where R was inches of rainfall per hour; T was duration of rainfall, in hours, and A was area covered by that rainfall, in sq. miles.

59. The formula gave reasonable agreement with the data between 1 and 500 sq. miles. It should be noted also that the data did not include any falls totalling less than 1 in. of rain during storm, and that the formula was not applicable where $RT < 1$.

60. What was still required was a body of statistics giving the areas covered, not by extremely rare storms, but by those occurring say, once a year, or once in three years.

Mr Asit K. Biswas (Lecturer in Civil Engineering, University of Strathclyde) congratulated the Author for his Paper on a subject on which much work had still to be done. However, Mr Biswas thought that to get a more comprehensive picture (with perhaps a slightly more technical view) of the role of hydrometeorology in river flood estimation the Paper should be read in conjunction with those of Merrill Bernard,¹⁷ and A. K. Showalter and S. B. Solot.¹⁸

62. It would not be out of place to stress that the maximum storm did not necessarily result in the maximum flood. The 'effective maximum storm', i.e. the storm that produced the maximum flood, depended mainly on a combination of a number of critical criteria. Briefly, they were maximum storm, minimum losses to runoff (infiltration, storage, evaporation, etc.) amount of snow on the ground before the storm (if applicable) and its condition, and the air temperature during the storm.

63. Undoubtedly there was an upper limit for the maximum flood that could ever occur and it might be a good idea for the designer to have a prior knowledge of the critical storm the structure might have to withstand. To quote R. E. Horton:¹⁹ 'A small stream cannot produce a major Mississippi River flood for much the same reason that an ordinary barnyard fowl cannot lay an egg a yard in diameter; it would transcend nature's capabilities under the circumstances'. How far one would be

justified in the use of the maximum effective storm as determined by the hydro-meteorological studies would depend mainly on the size of the project, consequences of any disaster, and finally on the discretion of the designer, who might well decide that the damage caused by the flood would not warrant the extra added cost of the structure or otherwise. In general, for most average projects, the designer was faced with the problem of determination of maximum possible value—as low as his judgement would allow. Thus one had to choose for the maximum probable value instead of maximum possible so that the scheme did not become uneconomic.

64. The Author had given some examples where the probable effective maximum flood had been determined by hydrometeorological studies. It would have been a good idea if the results computed from storm transposition could have been compared with the statistical analysis of the streamflow data from a few cases.

65. Maximum flow expected could be predicted with reasonable accuracy for a specified probability from the streamflow records. The Author was perfectly correct in stating that a 16-year flood could be defined from 50-year records available; extending the analogy, to have a sound idea of a 1000-year flood, one would need to have records for 3125 years!

66. Storm transposition was not a new technique and it had been substantially developed by Bernard, Hathaway, and others.¹⁷ It was absolutely essential that proper control should be exercised for such a manoeuvre. Before any attempt could be made to transpose and superimpose a storm from one basin to another, one had to consider meteorological, topographical, and geographical features. Since it was reasonable to assume that two basins could not be exactly similar, two problems had to be answered satisfactorily: (a) what were the chances of such an effective maximum storm occurring within the project's lifetime, and (b) how much addition or subtraction should be done to the transposed storm with reasonable accuracy?

67. Synoptic statistical records of storms had been kept for certain areas for the past 40 years or so and in most cases for a lesser period. In the absence of observations of upper-air soundings, they could only be assumed or estimated. In such a case storm transposition might prove to be a difficult problem. Precipitation varied directly with the moisture content and, as such, the precipitable water could be computed easily from the upper-air conditions. But sometimes it might be too much to presume that all the precipitable water would desiccate completely on the basin in question.

68. Bernard¹⁷ had pointed out: 'An engineer is not prepared to deal adequately with precipitation, the principal hydrologic factor, until he has gained a working knowledge of the processes which take from the sea, transport, and precipitate over the land the myriad of rain drops constituting a great storm. Thus equipped he will find his concepts broadened, his techniques made flexible, and his judgement strengthened'.

69. One could not agree more with the Author about the importance of hydrology and better co-operation between hydrologist and meteorologists. But Mr Biswas was glad to contradict the Author about the realization of the importance of hydrology in Commonwealth countries, specially in India. Hydrology was a compulsory subject in its own right in the Civil Engineering Undergraduate courses at the Indian Institute of Technology, Kharagpur (which was also Mr Biswas' Alma Mater). Two Associate Professors (Cornell and Cal. Tech. men) were concerned with the subject and postgraduate courses had been available for the last eight to ten years, dealing with 'Advanced Hydrology' and had been very well attended.

Dr T. M. Prus-Chacinski (Partner, Messrs C. H. Dobbie & Partners) congratulated the Author on a concise and comprehensive summary of a most promising line of approach to the problem of estimation of floods. He wished, however, to draw attention to the disturbing possibility that the weather and precipitation might be subject to periodic or other long-term changes. The possibility came to his mind when

he was reading some recent publications about countries as far apart as South Africa and Gt Britain.^{20,21} One dealt with the 'persistence' of dry and wet periods, and the other with probable changes in weather in Gt Britain. In that connexion it might be of interest to recall a passage from reference 22, in chapter 17 of which, entitled 'Periodicity; Introduction', it was stated that there was some evidence for quasi-periodicity of annual rainfall in Gt Britain for four periods of 51·7 years each, i.e. from 1739 to 1945. The probability of a wet year was almost double when the annual rainfall was at its maximum. As the last maximum had been in 1926 the next should be in 1978. It would seem reasonable to assume that the probability of floods was greater in wet periods than in dry periods. If it were so, then floods should be more frequent in Gt Britain during the next, say 20 years, than during the past 20. It was an interesting line of observation, but it complicated the line of approach presented by the Author and demonstrated the necessity for studies on a very broad front.

Mr Adolpho Santos, Jr (São Paulo Light S.A., Serviços de Electricidade, Brazil) limited his comments to some of the remarks made by the Author under the headings: 'Hydrological design and security', 'Frequency methods and their limitations', and 'Extrapolation in hydrometeorology'.

72. The Author had stated that '... stream flows, particularly in the subtropics, can be highly variable with spectacular peak flows and disastrous low flows ...'. To prove his point he had then quoted the ranges of annual flow and the peak discharges of certain Australian streams, while giving no indication of the size of the drainage areas involved, nor of the climatic conditions prevailing over the watersheds. Mr Santos subscribed to the view that the range was a rather poor measure of dispersion (especially when the length of record was not given), and that better measures were the standard deviation and the coefficient of variation. Be that as it may, the figures quoted by the Author showed that the ratio of the lowest to the highest observed annual flow was 1:10 in the case of the Goulburn River and was 1:11 000 in the case of the River Darling. Such wide ranges seemed to indicate arid rather than subtropical conditions. Was it not true that the Darling had been dry for as long as 18 months?

73. Mr Santos was acquainted with two Brazilian rivers that straddled the Tropic of Capricorn and which belied the thesis that tropical conditions implied high variability of flow. One of these rivers was the Tietê, with flow records extending over fifty years at a gauging station where the drainage area was 1900 sq. miles; the other was the Paraíba which had been gauged for more than forty years at a station where the drainage area was 12 000 sq. miles. The annual flow of the Tietê had ranged from 1400 to 4500 acre-ft/a, or in the ratio of 1:3·2 with a coefficient of variation equal to 0·25. In the case of the Paraíba the annual flow range was 8200 to 24 600 acre-ft/a, the ratio was 1:3, and the *CV* equalled 0·23. The variability indicated by these figures was more typical of humid subtropical conditions than those appearing in the Paper.

74. It was well known that the methods developed by the modern English school of statistics took account of the fact that the research worker often had to base his conclusions on a small number of observations. In the words of Fisher—one of the founders of the school—such methods enabled one to tackle small sample problems on their merits, and made it possible to apply accurate tests to practical data. Moran²³ had shown how to apply those modern methods to the estimation of return periods of flood flows. Earlier attempts at such estimations were generally based on naive statistical techniques which had led Moran to state: 'It is here that much confusion of thought about the correct statistical procedure is apparent in the literature',²³ and to reaffirm: 'A great deal has been written on this subject, most of which is wrong in principle.'²⁴

75. A few decades previously it had been the fashion to apply statistical methods

in the treatment of flood flows. However, as Clark²⁵ had asserted: 'Too many users of these methods knew too little about statistical analysis', with the result that statistical methods fell in disrepute among those unacquainted with the true capabilities of the theory of statistics. There had then come into use the so-called rational meteorological methods based on the unit hydrograph and on synthetic rain-producing storms. That was currently the fashion, and the Author seemed to be one of its converts.

76. The estimation of the future flood flows of a stream could be made either by a deterministic approach or by statistical methods. In the former, an attempt was made to describe the physical system by mathematical equations, or their equivalent, and to solve them when possible; in the latter a mathematical model was used which could be applied with little or no knowledge of the actual flood-producing processes. In the deterministic approach there was no way of knowing the probability of occurrence of the resulting flood; in the statistical procedure not only were the resulting forecasts made in terms of probability of occurrence, but confidence limits of any desired level could be placed on each forecast. The advantage was evidently on the side of the statistical approach; the deterministic one was unrealistic, to say the least.

77. Two types of frequency distribution were particularly useful for fitting flood flows; the logarithmic normal and Pearson's type III. Gumbel's 'extreme value' distribution was unsuitable because, as Moran²⁴ had said: 'This is the asymptotic form of the distribution of the largest member of a sample from a given distribution. If we knew the latter exactly we could estimate the parameters of Gumbel's distribution. However, Gumbel's distribution depends solely on the form of the tail of $f(x)$ which, as we have just seen, is usually outside the range of observations and can only be guessed at. No amount of mathematical presdigation can remove this uncertainty. Another defect in this approach is the fact that Gumbel's extreme value distribution is attained very slowly—in the case of the normal distribution only for samples of size at least 10^{12} .' Moran²³ had said also that: 'The distribution (Gumbel's) is also awkward to fit mathematically by efficient methods'. For these reasons Gumbel's distribution is not relevant to the problem considered in this paper.

78. The Author had stated that 'the once-in-1000-years flood, estimated from 50 years of stream flow record, has confidence limits so wide that the results are of doubtful value.' Mr Santos disagreed emphatically with that statement; the results were of value provided that the statistical analysis had been carried out by correct and efficient methods. In the first place, one should not lose sight of the fact that too good an agreement between experimental data and theory should be suspect, because it was highly unlikely that all observed points fell exactly on the theoretical curve. In the second place, the confidence limits defined the interval, no matter how wide, within which one could be stochastically certain that the estimated flood of any given probability would lie. In itself, that was information of great value which could be supplied by PMP methods. In the third place, it had been Mr Santos's experience that the width of the confidence intervals was consistent with the usual precision of stream flow data. For instance, the upper confidence limit, at the 5% level, of the 1000-year flood of the Tietê River was 20% higher than the flood estimated by fitting the log-normal distribution, and only 6% higher than the flood deduced from the type III distribution. The corresponding figures for the Paraíba River were 28% and 5%.

79. The Author stated in § 11 that 50 years of stream flow record could give a sound idea of, say, a once in 16 years flood. In § 44 the Author had implied that from precipitation records of 50 to 100 years one could extrapolate to the PMP, which was an estimate applicable to the last few thousand years. Mr Santos confessed that he was unable to follow the Author's reasoning at that point. After all, was not runoff and streamflow highly correlated with precipitation? Was it not true that the variability or dispersion of precipitation was greater than that of stream flow? Why should 50 random values of precipitation, at any one or several gauges, have

smaller dispersion than the 50 corresponding values of stream flow which were, by the very nature of the hydrologic balance, an integral function of the former? Finally, how could it be demonstrated that the PMP had a probability of occurrence of 1 in 1000?

Mr A. P. Campbell (Senior Engineer Hydrologist, Ministry of Works, New Zealand; Chairman N.Z. National Committee, International Hydrological Decade) observed that, by his very thorough statement on hydrometeorology, the Author had performed a valuable service for hydrologists everywhere. Moreover its presentation at a time when the water conscience, to which Mr Weisner had aptly referred, was stirring throughout the world for the work of the International Hydrological Decade, was most appropriate.

81. Hydrologists in New Zealand agreed fully with the main points stated in the conclusion: the importance of making careers in the work, and the need for data, training, research, and co-operation between hydrologists and meteorologists. If those requirements were not met, specialist work, however skilful, could bring only transitory benefits.

82. Hydrometeorology had been recognized by New Zealand's National Committee for the International Hydrological Decade as a subject on which much attention would be concentrated. Data on Probable Maximum Precipitation would be a considerable help in flood estimation and data on precipitation would give the simplest means of extending—in space and in time—the surface water information obtained from a network of primary stations in a regional system. That extension was particularly valuable in the high rainfall areas of mountainous regions where the losses—evaporation and ground absorption—were not a large proportion of the precipitation. Local examples of that situation were in the Hooker regional catchment where the annual runoff was 255 in. and at Milford Sound where the annual rainfall was 253 in.

83. The Author's observations on the possibilities of storm transposition, even between countries for generalized data, were particularly interesting. Such studies would fit in well with proposals which New Zealand would put forward in planning for the programme of I.H.D., suggesting co-operative work between countries with similar hydrological characteristics.

Mr F. W. F. Waitt (Consulting Engineer, Melbourne) wrote that the Author's summary of the subject was very valuable in drawing the attention of engineers to the assistance which could be given to their work by hydrometeorologists. Mr Waitt, when employed by the Commonwealth Department of Works, had occasion to use the PMP estimates of Mr Wiesner and his colleagues and he acknowledged his gratitude for them.

85. It seemed necessary, however, to assess the confidence with which those estimates could be used for the design of structures. The Author had rightly drawn attention to the wide range of confidence limits in the extrapolation in time of river flow records. However, the flood values obtained from the PMP estimates used by Mr Waitt were so very far in excess of any which could be obtained by extrapolating river records that rigid critical examination of the apparent discrepancy was desirable. It was possible that the true extreme value distribution differed so far from the normal as to account for it, but more evidence would seem to be needed to justify such a conclusion.

86. It seemed very likely that there must be very wide confidence limits in PMP estimation as well as in statistical extrapolation, although the range of those limits might not be so readily determined. For example, in §§ 24–26 the Author had referred to the use of theoretical models and had indicated that the models must be checked against actual storms, but discounted the use of such models as they might give rather high values of PMP. If one ignored high values because one had not equalled them

in actual experience was one not departing from the principle of a 'Probable Maximum'?

87. The Author had then proposed to maximize observed storms, stating that the most effective combinations had already been experienced and that the contention was supported by the physics of theoretical models. Could the Author please explain the apparent inconsistency between that and the 'rather high' values referred to above?

88. The engineer who designed flood controlling structures was faced with the determination of a calculated risk, except in those few instances where he had to design for the full value of the Probable Maximum Flood, and that risk would be measurable in frequency of occurrence. To use the Probable Maximum Precipitation he was dependent not only on dew point considerations, which might be fairly reliable, but also on the estimator's judgement of other factors, or the maximization of a very small sample of storms, for the assessment of that precipitation. To relate that rainfall to his flood hydrograph he was faced with the hazards of some form of unit hydrograph based on approximate estimations or on small samples. He had then to relate his assumed risk to the Maximum Probable Flood to obtain his Design Flood.

89. Mr Waitt therefore considered that one could regard the Maximum Probable Flood estimation as only a very rough guide to the decisions the hydraulic engineer had to make, but one which was valuable because no known method could give any real hope of accuracy.

The Author, in reply, agreed with Mr Escritt that empirical formulae, fitted to data on heavy rainfall, were most useful, especially where there was adequate information and where the distribution of the rainfall was well known and of low variability. The lack of a frequency factor or a limiting value restricted its use, however, especially in larger catchments and where serious consequences, such as loss of life, could follow under design.

91. Replying to Mr Biswas, he pointed out that there were many interesting technical points not covered in the Paper which was an introduction to hydrometeorology. The association of frequency factors with PMP as indicated in § 64 was one such point to which he had given some thought. Undoubtedly, the return period of the PMP would depend upon the size of the storm, its duration, location, season, and rainfall or storm regime, as well as the type of statistical distribution assumed. The next stage of applying the storm to the catchment to get runoff, as pointed out in § 62, introduced further probabilities. In a study on the South Esk River in Tasmania, by Munro,²⁶ the probable maximum flood of 250 000 cusecs might be compared with the once in ten years flood of 60 000 cusecs and the once in 100 years of 175 000 cusecs. In this study the once in 1000 years flood approached the maximum flood of 250 000 cusecs. A different result might have been obtained by using another form of extreme distribution.

92. The Author congratulated the Indian Institute of Technology, Kharagpur, on their attention to hydrology as a post-graduate subject. They had a worthy disciple in Mr Biswas.

93. In reply to Dr Prus-Chacinski, the Author wrote that if trends or periodicities in weather were well defined they could be incorporated into storage and flood design. The recent techniques of Maas and others²⁷ might also be further elaborated if it were possible to include the variations of the weather into this statistical approach to water resources and their simulation.

94. The subtropical areas referred to by Mr Santos were those south and north of the tropics where the weather was dominated by the subtropical high pressure cells and aridity was to be expected. The high variability of rainfall in such an area was well described by Loewe.²⁸

95. The Author did not condemn or abandon statistics; in fact, he used it exten-

sively. Where records were of limited length and of high variability, he considered that the PMP approach was the better for important structures.

96. He could not agree that the extremes of rainfall or stream flow could be so well fitted to a distribution that they could be extrapolated twenty times beyond the length of record. The Brazilian rivers quoted had low variability and consequently smaller confidence limits than the Australian streams which he mentioned. The data must also be expected to fit the theory reasonably well.

97. Regarding § 79, he would refer to § 43 which was the basis for his statement in § 44. Mr Santos had apparently missed the point that they did not study the actual precipitation figures, but studied the physical processes which gave this precipitation. They extrapolated the more basic and conservative elements involved in precipitation, chiefly the precipitable water which had less variability than either streamflow or rainfall. It was debatable whether the storm efficiency should also be increased, but it was currently agreed that 'the efficient storms' had occurred, and it was not necessary to allow for this factor.

98. Mr Santos also asked how it could be demonstrated that the PMP had a probability of once in a thousand years. The Author did not know the source of this statement, but suspected it could come from confusion between the statistical and the hydrometeorological approach. He did not agree with it, as the PMP might be approached in some localities more frequently than in others. He suspected that places where rainfall had low dispersion, or the air masses concerned with heavy rains occurred frequently or with small variations and intense storm mechanisms were common, achieved maximum rates more often than places where such conditions did not apply.

99. The interest of New Zealand was well known and the Author welcomed the comments of Mr Campbell. He would suggest caution in transposing storms of other countries to New Zealand. The so called 'thunderstorm' values were satisfactory for small basins and short durations (not above 400 sq. miles and 12 hours). Topography must also be of prime importance in New Zealand where local storms and individual valleys should be investigated. It might be possible, with care, to transpose storms within the local area. There was merit in attempting one of the more sophisticated topographical models as described by Knox.²⁹

100. On re-reading § 42, the Author noticed that the word 'tangent' appeared. That should read: 'the sine of the angle of inclination of the mountain barrier to the horizontal plane'.

101. In reply to Mr Waitt, the high values of PMP referred to were probably the early ones which were derived in Australia when they were relatively inexperienced. There were several reasons for these high values. Firstly, the areas over which storms could be transposed in Australia had not been then, and were still not, well defined. Thus when data was not available at a site, it was necessary to use whatever was to hand, although it was doubtful whether the same values would be attained in the new site. For instance, it might not be wise to use wind velocities recorded on the coast at a site some distance inland, but if no other data was available, there was little alternative. Some coastal storms also might have been transposed outside their meteorologically homogeneous zone, e.g. a coastal storm into the mountains. Each of these actions gave considerably higher values, and the cautious worker, feeling his way, would err on this side. Secondly, the high variability of Australian rainfall as demonstrated by Loewe,²⁸ meant that high values of PMP could be expected. Thirdly, estimates of PMP were often applied to a saturated catchment. This could give very high flood flows. If a runoff of 60% were assumed, and that was more likely, that alone made a big difference to the estimates. It did not necessarily follow either that PMP be accepted in design; a fraction of it might be appropriate.

102. He would add that on the east coast of Australia, storms of small area and duration had approached the PMP on a number of occasions and compared with

similar storms in the United States. That did not hold for the larger storms and he would agree with Mr Waitt that further investigation was necessary.

103. In regard to § 86, he would think that as the precipitable water was a more conservative property with less variability than rainfall, that any confidence limits applied to PMP would not be as wide as those for rainfall. It was an interesting thought and worthy of research.

104. With regard to storm models and high values, the Author thought that the early models of 'upglide' and 'convergence' certainly gave extreme values. He would not use them now, except for preliminary estimates and instruction. Their main value lay in indicating the significant factors in a storm. This enabled one to maximize such factors in an actual storm. For instance, precipitable water was found to be significant, and the other elements such as inflow wind, depth of cell, outflow, etc., could be incorporated into an efficiency factor. For reasons in § 26, efficiency was not maximized. These maximized values of actual storms were not as high as the simple model and indicated that modification of the model was necessary.

105. The hurricane model⁹ was an example of such a modification. Also Knox²⁹ used a refined orographical model for the Feather River catchment in California and obtained a figure of 19.3 in. for 72 hours which was 10% less than the previous estimate. This showed that the sophisticated models based on actual storms could give more realistic values.

106. He did not know of similar models being used in Australia, and more information might be needed to use them. At present, he preferred to maximize actual storms for precipitable water and assume high efficiency was achieved in them. Transposition must be done with caution and only within the meteorologically homogeneous zone. There was a need for further work in Australia on the use of more elaborate storm models and transposition. Estimates based on these would be considerably lower than the early ones mentioned by Mr Waitt and more realistic.

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