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U.S. Programs in Research Drainage Basins — an Interim Assessment

Final Report of the
Work Group on Representative
and Experimental Basins
of the
U.S. National Committee
for the
International Hydrological Decade

NATIONAL RESEARCH COUNCIL

NATIONAL ACADEMY OF SCIENCES
Washington, D.C., 1977

NOTICE

The project that is the subject of this report was approved by the Governing Board of the National Research Council, whose members are drawn from the Councils of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. The members of the Committee responsible for this report were chosen for their special competences and with regard for appropriate balance.

This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

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Preface

In 1965, the U. S. National Committee for the International Hydrological Decade (IHD) was established to guide U. S. participation in the IHD Program (1965-1974). The IHD was a cooperative international program, coordinated by the United Nations Educational, Scientific and Cultural Organization (UNESCO), that involved the efforts of more than 100 nations. Its purposes were to strengthen the scientific basis for water-resources use and conservation, to stimulate education and training, and to improve the ability of developing and developed nations alike to cope with their water problems.

The principal laboratory for studying hydrological phenomena in the field is the instrumented watershed or drainage basin. The United States has a large number of basins that have been instrumented to satisfy many scientific objectives, ranging from basic water-budget studies to studies of the hydrologic effects of land-use and land-treatment practices. The Work Group on Representative and Experimental Basins sought to bring the results of this diversified program into focus within this country and to make the United States' techniques and experiences available to other nations, many of which had no such programs at the beginning of the Decade.

During the course of this endeavor, the US/IHD Work Group maintained liaison and cooperated with its international equivalent the UNESCO/IHD Working Group, and actively supported its activities. It also was in close touch with many U. S. basin studies directly, with many individuals involved in both field and conventional laboratory research and activities related to basin studies, and with university personnel using basin-study data and methods in their teaching. As a result of these activities the Work Group has arrived at some general conclusions it thinks may be useful in the development and assessment of future basin studies.

The basic U. S. contribution to the IHD in this area was the selection of 60 research basins whose results were assured to be available for international distribution. The US/IHD Work Group prepared two reports describing these basins and the data and results achieved in the studies.

In this final report, the Work Group has summarized its accomplishments, attempted to assess the overall impact and results of the basin program, and made some recommendations for future work. The Work Group remains convinced that research drainage basins, regardless of shortcomings in their use in many instances, remain the best means for determining hydrological relationships, for testing theoretical and empirical hypotheses, and for monitoring changes in the earth's environments.

About the title of this report: the term "research drainage basin" is used because the Work Group, having argued against the use of the expression "representative and experimental basins," felt that its use in the title would be inappropriate. Also, although this is its final report, this Work Group is confident that drainage-basin research will continue to be done, continue to be important and difficult, and continue to need periodic assessment. Thus, the Work Group considers this report as one in a succession of interim assessments.

R. F. Hadley, Chairman (1967-1975)

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Acknowledgements

This report on research drainage-basin programs in the United States represents an assessment of their success and their contribution to hydrological knowledge, and submits some recommendations for the improvement of future programs. There are a great many instrumented research basins in the United States, but they do not comprise a cohesive or coordinated program. The Work Group on Representative and Experimental Basins selected 60 of these basins to constitute a program and to represent the U. S. contribution to the IHD in this area.

The National Committee provided the terms of reference for this Work Group and during the course of the Decade monitored and guided its activities through a liaison member. This report has been reviewed, accepted, and is endorsed by the National Committee, and portions of it and its recommendations will be included in the National Committee's own final report.

The National Committee for the International Hydrological Decade wishes to express its gratitude to members of the Work Group for their fine efforts in their formidable and almost intractable task of assessing the wide range of activities involved in basin research in the United States during the Decade. We also wish to express our appreciation for the cooperation of the many individuals and agencies who contributed to the IHD program. Thanks also are due to John O. Ludwigson, who served as editorial consultant for this report, and to C. M. Skan, W. E. Sopper, and R. H. Waring, who added materially to the value of the report with their cogent reviews.

The National Committee appreciates the support provided by the National Science Foundation for its work, for the committee's Secretariat, and for the activities of the Work Group.

H. Garland Hershey, Chairman
U. S. National Committee for the
International Hydrological Decade

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Introduction

Objectives of Drainage-Basin Research

In the United States, the use of small drainage basins as outdoor laboratories has been a common method of hydrological study for several decades. Such studies, all or most of which have been commonly lumped to constitute drainage-basin research, have been conducted by federal and state agencies and universities. The objectives of these studies can be place in five general categories:

1. Study of hydrological processes
2. Definition of regional hydrological character
3. Study of geochemical processes
4. Evaluation of effects of land use or land treatment
5. Analysis of economics of water management.

The common basic premise underlying these studies is that the individual basin integrates the physical and chemical forces acting upon water within its boundaries. This applies directly to studies of hydrological and geochemical processes. Assuming that all processes are active and integrated, it should then be possible, by differential control, to isolate and analyze individual phenomena. This has proven to be difficult because the drainage basin is a complex natural unit, and most of them are too large to permit the precision of experimental control attainable at the laboratory bench. Lysimeter-type studies may be an exception, but no matter how carefully constructed and instrumented, they too fall short of simulating true natural conditions.

Some basin features are relatively fixed (e.g., size, relief, and drainage pattern) and need to be measured just once, or at infrequent intervals; others vary more or less continuously (e.g., water quality, surface and subsurface discharge, precipitation, and erosion) and must be measured on a continuing or frequent basis. In all basins, the individual features are part of a dynamic system. Probably the only way that the complex natural phenomena of the basin and their even more complex interrelationships can be effectively studied is through attempting to measure them in their natural setting. In spite of the difficulties inherent in this method, much has been learned about the interactions of hydrological processes involved in the soil-plant-water complex of a basin, and the use of a basin as an integrator of hydrological processes remains the basis of many necessary hydrological studies. The drawbacks must be assessed against the value of their study to water-management practices.

A second important aspect of basin studies, particularly applicable to the evaluation of land use or land treatment and to the definition of regional hydrological characteristics, is the assumption of transferability of of results--that is, that the results obtained in one basin are applicable to other basins with similar physical and hydrological characteristics. Transferability implies that it is not necessary to study every basin separately because reasonably reliable overall conclusions may be reached from studies made at a few carefully located sites; transferability thus has considerable economic as well as scientific appeal.

These two potential attributes of drainage-basin research--integration of hydrological phenomena and transferability of results--have made it extremely attractive to researchers. The United States has been the leader in drainage-basin research since the beginning of this century. In all, about 1,000 drainage basins have been instrumented and studied in the United States, although there are probably not more than a few hundred under study today. Leaders in the use of basin research have been U.S. federal agencies such as the Forest Service, the Soil Conservation Service, the Agricultural

Research Service, and the Geological Survey, and private and state universities, particularly in the western part of the country.

IHD Drainage-Basin Program, 1964-1968

Hydrologists who were concerned with preparing a program for the International Hydrological Decade in the early 1960's recognized the need to maintain liaison with and provide guidance to the many research-basin studies then under way or being established around the globe. The growing worldwide use of drainage basins, or parts of them, for research and the growing recognition of the potential of such research for monitoring environmental health of large regions, had encouraged many countries to initiate new programs or to enlarge existing ones. Accordingly, the UNESCO/IHD Working Group on Representative and Experimental Basins was authorized by the First Session of the IHD Coordinating Council (May 24 - June 6, 1965), with the United States assenting, and established soon thereafter. One member of the Working Group was from the United States, and he served for the duration of the Decade.

The first charge given to this Working Group was to develop general guidance material for the establishment of representative and experimental basins and to make recommendations to the Coordinating Council based on a Symposium on Representative and Experimental Basins, held in Budapest, Hungary, in September 1965.[1] Exchange of views among hydrologists of many countries at the Symposium indicated that few countries were ready for the sophisticated programs in research basins such as were already established in the United States. The thrust of the International Hydrological Decade program, and of the US/IHD program as well, was therefore redesigned to serve two purposes--to assist emerging countries in planning and beginning their basin studies, and to encourage

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1/ International Association of Scientific Hydrology, 1965, Symposium of Budapest, Representative and Experimental Basins, IASH Publications 66.1 and 66.2.

countries with greater experience to carry on experiments compatible with their capabilities and interests.

In the United States, the program originally envisioned by the National Committee in 1965 had as its principal objective for hydrological research in drainage basins the determination of basic processes and the interrelationships governing the flow of water and the movement of constituents. Faced with the need for more elementary programs abroad, the National Committee adjusted its program to encompass both national and international objectives.

The U. S. National Committee established the US/IHD Work Group on Representative and Experimental Basins in 1966, and the Work Group's first task was to develop a program of activities in support of both international and national objectives. The Work Group planned to support and assist the international Working Group in its program, and outlined a program to encourage the planning of new projects that would integrate and supplement the current activities of federal and state agencies and universities. The proposals for new activities at home were based on the expectation of adequate financial support and included projects such as (1) analyzing and synthesizing already completed studies, (2) comparing analyses of similar parameters in different areas, (3) evaluating changes in hydrological benchmark basins, (4) testing the transferability hypothesis, and (5) attempting regional syntheses.

However, significant funding for the proposed program of new work did not materialize. Without abandoning its interest in potential improvements in domestic drainage basin research programs, the Work Group began to concentrate its efforts on assisting international activities on the basis of the U. S. experience. Often it was impossible to keep discussions of national and international programs separate because the demands of the international program continually required evaluation of domestic activities.

In this report, the Work Group summarizes its activities and assesses its experience in connection with the international program toward improving current and future domestic research activities.

US/IHD Activities

By 1968, the program to which the Work Group directed its efforts comprised the following activities:

1. Encouragement of more timely publication of results from IHD basins in the United States and the circulation of bibliographic lists to countries participating in exchange of data;

2. Categorization of U. S. experimental basin studies with similar research objectives and evaluations of results on a regional basis according to climate, soils, geomorphology, and vegetation;

3. Participation with UNESCO/IHD Working Group in a classification of research basins to promote regional cooperation on a worldwide basis;

4. Investigation of the possibility of a cooperative venture with IBP (International Biological Program) and SCOPE (Special Committee on Problems of Environment) using selected representative basins as a base for monitoring environmental changes or trends; and

5. Determination of the feasibility of obtaining standard sets of data from the experimental basins in the United States for testing models designed to predict drainage-basin behavior.

Many of the program objectives were achieved, but in many instances not to the extent anticipated. On the one hand, more reports were completed than originally planned, but on the other, cooperation with other international programs proved to be far more difficult and time consuming than expected. In addition, some activities that were not listed in the formal program, such as participation in symposia and promotion of exchange of scientists, proved to be useful and were therefore given considerable attention by the Work Group.

Publication

The first major effort of the US/IHD Work Group was to encourage the preparation of papers for an international guide for research and practice on instrumented drainage basins. The guide was proposed by the Budapest Symposium and 15 of the 52 papers in the guide were written by U. S. hydrologists[2]. In addition, the Chairman of the U. S. Work Group was a principal member of the panel of editors, and his advice and commentary gave the papers, which came from 13 countries and two international agencies, a large measure of their continuity and coherence.

Before the end of the Decade, the U. S. Work Group prepared a catalog of information regarding 60 research basins in the United States (1969) and a catalog of data available from these 60 research basins (1972), and drafted this final report. In addition to the guide to research and practice described above, it contributed initially to the UNESCO Technical Documents in Hydrology (both in 1974), and to two international symposia (1965, before the Work Group was formally organized, and 1972). These are briefly discussed and referenced as appropriate.

Categorization and Classification

The Work Group's efforts to promote a project to categorize the drainage basins of the United States on

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2/ Toebes, C., and V. Ouryvaev, eds., 1970, Representative and Experimental Basins - An International Guide to Research and Practice, UNESCO Studies and Reports on Hydrology no. 4, UNESCO, Paris, 348 p.

the basis of their hydrological characteristics, found support only toward the end of the Decade. And that support came from the interest of the UNESCO/IHD Working Group in promoting regional cooperation among researchers working on drainage basins. The UNESCO/IHD report on classification of research drainage basins was prepared by its U. S. member, who also was the chairman of the US/IHD Work Group. As a result, the final report, although issued by the international working group, was based in part on many discussions held in the United States.[3/]

Global Monitoring Networks

Another aspect of the international program to which the US/IHD Work Group contributed was the identification of stations at which hydrological data are collected. These networks include one of research drainage basins. Potentially this network will provide indices of long-term changes and the basis for intercomparative studies.

To form the U. S. part of the IHD network of research basins, the Work Group selected 60 of a much larger number of research basins recommended by the agencies conducting studies. Two stipulations governed the selection of basins for the network: (1) that there be a reasonable assurance of continuity of work in them through the Decade, and (2) that the data collected in each basin would be available to all interested scientists.

The Work Group compiled a catalog[4/] of these basins, providing essential information regarding location,

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3/ Hadley, R. (F.), 1974, Classification of Representative and Experimental Basins (1st Ed.), UNESCO Technical Document in Hydrology, Sc/75/WS66, Paris, 16 p., 1 map.

4/ (US/IHD) Work Group on Representative and Experimental Basins, 1969. Decade Representative and Experimental Research Basins in the United States, Denver, Colo., 267 p., 68 maps, 1 foldout explanation. (Available for sale at National Research Council Printing and Publishing Office, 2101 Constitution Ave., N.W., Washington, D.C. 20418, $3.50.)

hydrological regime, objectives of investigations, equipment, and key publications. A map of each study area was included. A summary of the IHD network of research basins also is given in Table 7 of the catalog summarizing all IHD stations and networks in the United States[5/].

Nearly five years later, after there had been time for the accumulation of data of special interest to the IHD, a second catalog was issued, listing specifically the data from the 60 basins that were available for exchange[6/].

Two general purposes were intended for these two catalogs: (1) To provide a concise guide to the availability of raw and reduced data for the use of all parties interested in the IHD program, and (2) to encourage the use of data from US/IHD basins as a baseline for assessing environmental changes and as a basis for regional studies.

The catalogs met the first objective by being printed and distributed. Similar success cannot be claimed for the second. The first catalog invited inquiries regarding the data available. Fewer than 50 requests were received at the 60 research basins, suggesting that the need for raw data or for data in early stages of processing was of less interest to most researchers than interpretations of results.

The global network of research basins remains an excellent basis for a more specialized network to monitor environmental changes. However, the criteria for their selection by the United Nations' Environmental Program

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5/ U. S. National Committee for IHD (compilers), 1972. Catalog of International Hydrological Decade Stations and Networks in the United States. National Research Council, Washington, D. C. 66 p.

6/ (US/IHD) Work Group on Representative and Experimental Basins, 1974. International Hydrological Decade Representative and Experimental Basins in the United State: Catalog of Available Data and Results 1965-1972. National Academy of Sciences, Washington, D. C. 149 p.

have not yet been issued, and cooperation with IRP and SCOPE in this regard remains premature until a clear basis for the global network is established.

Standard Data Sets

One of the results of the Wellington Symposium (see following section) was an apparent need for a standardized system for collecting and recording sets of data in order to make quantified comparisons of mathematical models of research drainage basins. A draft report was prepared for the international working group, and the members of the US/IHD Work Group and their colleagues in the United States reviewed the report at several stages during its preparation. The final report[7/] is considered by the US/IHD Work Group to be insufficiently flexible to be applied to the wide variety of research basin studies underway in the United States. Nonetheless, the report is a first step toward large-scale comparison studies and is recommended for study by those involved in modeling.

Symposia and Other Activities

Halfway through the Decade, it was felt by the UNESCO/IHD office that the level of sophistication of researchers in all countries had advanced sufficiently to justify a second international symposium on the research drainage basins. The Symposium on Results of Research on Representative and Experimental Basins was held in Wellington, New Zealand, in December 1970, where 17 out of 91 papers were given by U. S. participants[8/]. The US/IHD

7/ Ibbitt, R. P., 1974, Representative Data Sets for Comparative Testing of Mathematical Models for Representative and Experimental Basins, UNESCO Technical Documents on Hydrology SC. 74/WS/11, Paris, 15 p.

8/ International Association of Hydrological Sciences, 1972, Results of Research on Representative and Experimental Basins, Proceedings of the Wellington Symposium, Wellington, New Zealand; UNESCO Studies and Reports in Hydrology no. 12, UNESCO, Paris, 2 volc., 1,215 p.

Work Group served to engender and screen U. S. papers, its members chaired meetings, helped to review the final drafts of papers, and helped U. S. participants to obtain travel grants.

The US/IHD program of activities related to basin studies also included the use and promotion of personal contacts, visits, and tours, both by U. S. hydrologists elsewhere and those from abroad here in the United States. These contacts took place through consultant work, international meetings, informal visits, and by mail. The preponderance of movement of students and technicians was to the United States, and that of assistance by mail from the United States.

Foreign governments interested in updating or establishing hydrologic networks and small study basins have traditionally encouraged and supported graduate study in American universities that have expertise in this type of research. Federal water resources research funding has supported most of these studies as well as many postdoctoral programs related to drainage-basin research. Students from Canada, Japan, New Zealand, India, Thailand, Austria, Switzerland, Israel, West Germany, the Philippines, Australia, and many South American and African countries have taken graduate and postgraduate training in the United States. In addition, technicians and administrators from developing countries who have visited federal and university programs in the United States have gained a knowledge of small-basin research that could be applied to their own hydrological programs. The following are examples of training and exchange activities sponsored by the United States:

- An on going project that began with a postdoctoral assignment from Japan is continuing to promote the exchange of data and ideas on mountain catchment hydrology five years after the student returned home. The objective of the project is to develop a universal model for predicting stormflow and flood peaks from mountain watersheds.

- A project involving West Germany and the United States began with a joint graduate training program in watershed hydrology and continues as an international effort to determine the hydrometeorological effects of forest cover on the water balance.

- A graduate program in the United States has led to close cooperation and exchange of information with the Canadian catchment research program at Calgary.

- Hydrologists from New Zealand and Australia have studied with U. S. agencies (particularly the U. S. Geological Survey, Agricultural Research Service, and the Forest Service) and returned home to work with experimental watershed programs.

- The U. S. Department of Agriculture Hydrograph Laboratory of the Agricultural Research Service has a cooperative agreement with the Hydrologic Institute of Vituki in Budapest, Hungary, to test hydrologic models.

Many agencies and institutions made up the U. S. representative and experimental basin program for the IHD, and these activities constituted a significant part of the entire U. S. contribution to the IHD Program. The lessons learned, both in terms of successes to follow and of failures to avoid repeating, as expressed in this report, constitute a final contribution of the U. S. representative and experimental basin program to the International Hydrological Decade, and to all those who may in the future undertake basin studies.

Drainage Basin Research— An Overview

Research on drainage basins has posed a dilemma for many years. It has long been acknowledged as the only way to obtain field data capable of being analyzed and synthesized in terms of hydrological theories. It also has been remarkably successful in providing insights into hydrological relationships, and this record of successes plus the inherent logic of the method continue to make drainage basin studies a desirable, if not predominant, part of any field research program. At the same time, the method has been extensively criticized. It is expensive because of extensive equipment and long-term commitment of manpower. Its results may be inconclusive because an experiment may take so many years to provide the desired base of experimental data that the final results sometimes have little to do with the initial objectives. Also the programs for the use of research drainage basins often make insufficient allowance for changes in personnel and in ideas over the years that a single experiment is conducted. Sometimes, an experiment appears to be perpetuated simply for the sake of accumulating data for data's sake.

Rather than extend its own remarks regarding the strengths and weaknesses of drainage basin research, the US/IHD Work Group has included a condensation of a paper by J. D. Hewlett, H. W. Lull, and K. G. Reinhart as Appendix 1. The Work Group considers this statement to be as pertinent today as when it was written and to be as cogent a presentation of the problem as it has seen. It is strongly recommended to the interested reader.

Drainage-Basin Research

The long and extensive experience in basin research in the United States was doubtless the reason for the requests from overseas in the early years of the Decade for guidance in the establishment of small-basin studies and in the methodology of research. Many countries participating in the IHD began the Decade without any research basins, and certainly none had an integrated and comprehensive network of research basins. The obvious need for basin research coupled with the surge of interest in hydrology due to participation in the IHD furnished the necessary impetus for many countries to begin the collection of basic hydrologic data, which had been long neglected or which had had lower priority than other national research needs. In many countries, research on drainage basins is still in sufficiently early stages to incorporate some obvious, yet often overlooked, planning and operational practices. These practices are applicable not only to countries like the United States, Australia, and New Zealand, which already have many basins, where new ones continue to be established, and where the program of existing ones are subject to review and revision.

Semantic Difficulties. One overriding necessity in exchanging information is mutual acceptance of the meaning and usage of scientific terms. The terminology used to describe research basins, like that of any discipline with origins in many different countries, has been and still is loosely defined, sometimes to the point of inconsistency. For example, a watershed (drainage basin) is separated from other watersheds (drainage basins) by a watershed (drainage divide).

Words are not only understood and used differently in different countries, but they may be used differently in the same country, and sometimes by the same people when speaking in different contexts. Sometimes, the terms are applied to studies whose objectives they do not fit, and a single basin may host several different studies, each with different objectives, and yet be identified by only one term.

Consider the terms "representative" and "experimental," which appear to be fundamental divisions of hydrological research, and which have been widely used within the Decade program. According to Toebes and Ouryvayev (1970, p. 22), "Representative basins are basins which are selected as representative of a hydrological regions, i.e., a region within which hydrological similarity is presumed. They are used for intensive investigations of specific problems of the hydrological cycle (or parts thereof) under relatively stable, natural conditions...a sparse network of representative basins may reflect general hydrological features of a given region and their variations over large natural zones." The same authors define experimental basins as, "...basins which are relatively homogenous in soil and vegetation and which have uniform physical characteristics. On such basins the natural conditions, i.e., one or more of the basin characteristics, are deliberately modified and the effects of these modifications on the hydrological characteristics are studied... Research on experimental basins is normally a study of comparisons and therefore they are operated in groups of two or more basins."

This seems clear enough. Yet the term "representative" has been applied to basins whose study objectives were simply to inventory hydrological phenomena or to collect hydrological data. The term "experimental" has also been misused, to say nothing of the fact that an "experimental" basin may be as representative of a region as any basin designated "representative." It is little wonder that confusion is generated when the results of different representative-and-experimental-basin studies are compared without spelling out the specific objectives of the different studies. Many workers now believe that no absolute distinction can be made between the two terms, and that their continued use tends to perpetuate the illusion of distinctions that cannot be made consistently in practice.

Similarly, other words and terms have meanings that change with each user or whose hydrological definitions are so subtle that they are easily confused or blurred. Among these terms are "watershed," "bench mark," "barometer," and "vigil stations." "Watershed" is used interchangeably with "basin" and "catchment" by many hydrologists but it also means a drainage divide[9/]. "Catchment" is used most commonly in England, while "basin" is used in the United States. The terms "bench mark" and "barometer," as synonyms for "representative" basins, should probably be dropped to avoid confusion. "Vigil station," small basins or areas instrumented or marked to monitor long-term changes, appears to have some long-term value.

The main difficulties, however, are with the terms "representative" and "experimental" when applied to basins. Both types of basins are equipped and used for research into hydrological processes, prediction, transfer of data, and effects of changes. Perhaps the principal differences are whether the changes are natural or man-made. Today, our use of the land influences the hydrological cycle almost everywhere, so that even the differences between natural and man-made changes are now difficult to distinguish, and these differences no longer constitute firm criteria for classifying field research in hydrology.

Some workers have suggested that a more fundamental and useful definition of basin studies would be one based on the stated study objectives, provided the work done is consistent with those objectives. These would be in terms of inventorying hydrological phenomena to provide records of changes of quantity and quality of water in space and time. Such data would be used for routine water-budget analyses or for comparison with or extrapolation to other similar areas. If the objective is defined as experimental, this would mean simply that somewhat different sets of data would need to be collected simultaneously. The important thing is not the "representativeness" or "experimentality" of a basin--since every basin is representative of some set of regional characteristics--but rather it is the objective of the

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9/ With luck and wide distribution, W. B. Langbein's short note, Runoff from a New Watershed, published in the Hydrological Sciences Bulletin, v. 2, no. 19, June 1974, p. 359-60, may discourage the use of watershed as a synonym for drainage basin.

study and the extent to which the results fulfill the objective.

Some researchers have recommended that an international scientific organization should compile an acceptable multilingual terminology related to all aspects of research on drainage basins. A compilation of multilingual hydrological equivalents has been published under the IHD program[10/] but the specialized terms of individual subdisciplines remain to be undertaken.

Problem Definition, Research Capabilities, and Research Objectives. Fundamental scientific research into a problem often involves a hypothesis and a method to test it. It is then necessary that resources--manpower, time, money, equipment, and control of the experimental site--exist or can be obtained to implement the research program. Once these elements--hypothesis, method, and resources--are assured, the research objectives can be defined so that a feasible work plan can be proposed. It is possible, of course, to lay out a program of work for research on a basin without adequate consideration of all three of these factors, but its goals would soon become nebulous and its results random and fragmented. A systematic approach to designing drainage-basin research is given in Appendix 2.

Several complementary factors must be considered during the early analysis a proposal should receive before it is accepted: What is the geographic scope of the proposal--is it local, regional, national, or even broader? Is the basin selected for the study typical of the conditions to be examined or is it, perhaps because of the nature of the underlying geology, unusual or even unique? What is the disciplinary scope of the proposed study? Can it be treated strictly as a hydrological problem? Or must it consider the physical, chemical, and biological interactions that occur in natural environments? Or is it at an even more complex level so that one has to consider the full gamut of man-made stresses of use and pollution? If the study must be

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10/ WMO/UNESCO, 1974, International Glossary of Hydrology, WMO Publication No. 385, 393 p.

interdisciplinary, have steps been taken to obtain knowledgeable input from the nonhydrological disciplines, to effect coordination with related studies nearby, and to exchange information with those working in similar environments at a distance or abroad?

The interdisciplinary input should be obtained as early as possible, because it is quite likely that points of view from outside the hydrological community might require a change in the definition of the original problem. At the same time, care must be taken that an adequate proportion of hydrological expertise is included as a part of the larger systems viewpoint. And finally, what is the planned end result of the proposal? How will the result augment what is already known? Are the objectives geared to an overall plan for eventually understanding the interrelationships of basin hydrology, or are they designed to provide a one-shot answer to an isolated problem, or just meant to reiterate what has already been learned elsewhere?

A special problem, not unique to hydrology or to drainage basin studies, is faddism in research. A few decades ago, emphasis was placed on vegetation cutting and cutting patterns. This was followed by attention to changing and conversions of vegetation species. During the 1960's, the "in-thing" was mathematical modeling, which many investigators now believe to be more representative of basin behavior than indicative of linkages between hydrological processes. The current fad appears to be concerned with nutrient leaching and water quality. Each of these has a valid place in the sequence of learning more about hydrological phenomena, but overemphasis on any single aspect or set of aspects results in much repetition, reinforcing and confirming what is already published but adding little new to what is known. Each investigator should carefully examine the objectives of his study in order to maximize the value to science of the time and effort expended.

The joint consideration of the geographic and disciplinary scopes of proposed research, already involving a hypothesis, proposed test, and preliminary assessment of capabilities, may result in additional rounds of program planning. These, however, are indispensible to providing scientifically realistic terms of reference for the investigation.

Coordination of research in different basins is often proposed as a means of achievement of common goals, and coordination can be highly desirable at regional and national levels as well. For example, in northeastern United States, about 30 or 40 research basins are undergoing some type of forest manipulation. Yet, not enough is done to coordinate these experiments sufficiently so as to obtain the maximum amount of useable information, and often it is difficult to fit together the results from different basins into a formulation of definite conclusions or into the design of further research programs.

However, coordination is expensive when applied to actual research activities, requiring more careful planning than most other phases of scientific research and development because it must involve the procedural idiosyncracies of two or more projects and, possibly, agencies. Moreover, the progress of research, once the objectives are made clear, does not benefit from over-coordination, over-standardization, or forced cooperation. Coordination is most needed at the levels of problem identification, definition, and analysis. Further, it seems clear that a set of research objectives outlined and developed through interdisciplinary cooperation will fit better into large-scale problem-solving efforts than objectives identified through the analysis made within the scope of a single discipline. Some aspects of international cooperation are discussed separately in following sections.

Finally, there is the matter of the anticipated results of a proposed study. As yet there exists no workable model of the whole hydrological system of a small basin. Pieces of basin models do exist, such as those for evaporation, streamflow, and sediment discharge, and some are partly linked, but no model represents the entire system in terms of mass and energy fluxes. Basic research should be directed toward improving and enlarging the existing linkages, toward completing our understanding of the entire system by adding some bit of new knowledge. Research should be directed toward testing existing models and hypotheses designed to explain critical phenomena. It is not useful for a researcher to suggest a study that will lead to a new model unless the existing ones have been tried and found wanting. The criterion for acceptability should be the prospect of improvement, not mere proliferation, of models. Small-basin research is sometimes required to answer questions or to clarify functional relationships that arise from on-going modeling efforts. The key here is to plan research designed to provide answers to questions resulting from an incomplete understanding of hydrological relationships rather than to instrument a drainage basin to obtain data with which to build a new model.

One cautionary note may be in order. Sometimes, under the pressure to attain useful results, a research program may be hurried into applying treatments before the basic hydrological behavior of the basin is sufficiently understood. The investigators may then have trouble explaining the results, or the repetition of treatments may provide conflicting results. Simply having a control basin for comparison does not remove the necessity to understand the hydraulic behavior and response of each drainage basin being studied.

In conclusion, proposals for research on drainage basins should be designed to test hypotheses that stem from gaps in the basic knowledge of hydrological phenomena. The testing should be done systematically and on as broad a disciplinary basis as possible; and the results of the investigation should augment and expand understanding rather than simply reinforce or reiterate what is already known.

Research Drainage-Basin Networks

Drainage-basin research methods have proved to be useful though difficult tools for analyzing the interrelationships of hydrological phenomena in individual basins. The data collected for individual basins and the results of local studies have even greater potential, however, in that they provide the basis for regional, national, continental, and even global syntheses, monitoring systems, and assessment of environmental changes. The development of programs that will provide integrations of information from many widely distributed research basins requires a high level of coordinated planning. Perhaps the closest approach to an integrated continental-scale system has been that of Australia[11/]. Starting in 1965, Australia has developed a rational approach to the representative basin concept. Ninety-three catchments collectively represent a wide range that exists in Australia in distribution of precipitation, vegetative cover, soil storage, hydrological conductivity, and temperature. The basins are being selected to form the basis for the extrapolation of hydrological data to ungaged catchments, for improving the understanding of catchment characteristics on the rainfall-runoff process, for improving national water resources assessments, and for predicting the hydrological effects on land use and management. At this time (1975), many of the selected basins are as yet insufficiently instrumented or manned to meet their ultimate objectives, but the basis for a coordinated system of research basins has been established.

In the United States, basin research traditionally has been conducted to meet local or mission-oriented objectives, often with minimal communication among researchers in different basins. Though much innovative and useful research was done, the fragmented approach had shortcomings. Research objectives were usually evolved independently by individual agencies and university departments, fostering both overlaps and gaps among the results obtained. The independent approach also resulted in a geographic distribution that concentrated study sites in some regions to the neglect, though not complete disregard, of others. Other problems with the independent approach were that many studies were terminated before their data were fully analyzed or that results for some studies were published on only a limited aspect of the total effort.

11/ Representative Basin Program Panel, 1969. The Representative Basin Concept in Australia (A Progress Report). Australian Water Resources Council Hydrological Series No. 2, Department of National Development, Canberra, Australia, 24 p.

Representative Basin Program Panel, 1974, Australian Representative Basins Programme - Progress 1973, Australian Water Resources Council Hydrological Series No. 8, Australian Government Printing Service, Canberra, Australia, 47 p.

The Work Group's catalog of the results of U.S. research basin investigations participating in the IHD network[12/] indicated both strengths and weaknesses. Requests for raw or processed data from abroad and from within this country were so few as to make the Work Group believe that its program of data exchange was either naive or premature. Most basin research was directed toward studying the effects of vegetation on water yield and water quality. However, some hydrologists were proceeding with, or had already prepared, comprehensive analyses of the effects of crop- and forest-cover manipulation on some aspects of the hydrological cycle. The Work Group anticipates that in the future, as their reports are completed, other hydrological responses will receive the greater attention they need.

The 60 research basins, 5 glacier basins, and 58 Vigil Network and Hydrological Bench Mark Stations participating in the US/IHD program[13/] are concentrated in the intermountain west and in the Appalachian regions. Notable shortages of study basins exist in the Great Lakes region and in parts of the Central Plains. The only other readily available sampling[14/] repeats the pattern, except for concentrations of study basins along some of the main river systems such as the Missouri, Ohio, Tennessee, and Mississippi.

Despite the uneven distribution of research basins,

12/ (US/IHD) Work Group on Representative and Experimental Basins, 1974. International Hydrological Decade Representative and Experimental Basins in the United States: Catalog of Available Data and Results 1965-1972. National Academy of Sciences, Washington, D.C. 149 p.

13/ U.S. National Committee for the IHD, 1972, Catalog of International Hydrological Decade Stations and Networks in the United States. National Academy of Sciences, Washington, D.C. 66 p.

14/ American Geophysical Union Section of Hydrology, 1965. Inventory of Representative and Experimental Watershed Studies Conducted in the United States. American Geophysical Union, Washington, D.C. 153 p.

a selection of sites from among existing agency and university basins probably could be identified to represent most of the geological, climatic, topographic, and ecological conditions of the conterminous United States. Coordinated regional studies and analyses could be made under the guidance of some interagency group, such as the Hydrology Committee of the Water Resources Council or the Committee on Water Resources Research of the Federal Council for Science and Technology, without greatly modifying established programs. The identification of a cooperative, coordinated network could lead to important research in the nature of change from region to region, the identification of data badly needed to improve national water-resources assessments, such as precipitation, especially snowfall, and the regionalization of data to assist planning and management of water resources from the local to national levels. Important as coordinated regional studies might be, they should not be considered as substitutes or alternatives to the substantive investigations now underway.

For at least the past 10 years, the general impression has been that much of the data collected in research basins was neither made available nor analyzed and published. Some agencies have published the data collected in their research basins, or parts of it, and many descriptive and interpretive reports have been released. In fact, some confusion regarding the amount of published reporting on drainage basin research has resulted from the failure on the part of critics of basin research to distinguish between experimental results and hydrologic data. Many papers on the hydrology of drainage basins have used data from unnamed experimental basins or have incorporated conclusions drawn partly or wholly from unpublished data on the basins. Indeed, it is probably safe to say that most personal or indirect experience gained in nearly all of the basins gaged in this country has found its way into print. On the other hand, the great majority of the hourly data and the detailed basin descriptions have not been published and therefore can be obtained only with great difficulty by other researchers.

Although it is unreasonable to expect reports from all research basins, some reporting of ongoing activities is needed. For example, the 60 US/IHD network basins produced a total of 237 new reports, excluding data compilations, for the period 1965-1973. However, these 237 reports came from only 27 of the 60 research basins, and 176 of them came from only five basins.

The Work Group believes that agencies and organizations should seek funds to examine their individual situations and to make existing data and information more readily accessible and available to the concerned scientific community.

Information Exchange and Regional Cooperation

The increasing interest and activity in basin studies in many countries necessitates intensified exchange of information on basin research. Such exchanges of information will be of increasing benefit to the United States: They are the best means by which the United States can maintain its leadership in this field of hydrological research. One direct result will be to encourage foreign students to obtain advanced hydrological education in this country, with the consequent long-term benefit of friendship for and cooperation with this country. Less directly, it will lead to the export of U. S. expertise, techniques, instruments, and related construction capabilities.

The use of standard means of information exchange, such as participation in and attendance at international meetings, exchanges of visiting scientists for periods of observation and study, exchanges of literature, preparation of teaching and training materials, and translation of relevant reports, should be continued and increased. Three special aspects of information exchange deserve particular attention because of their peculiar relationships to basin research.

Scientific information exchange generally has involved methodology and results, and the methodology has generally been technically oriented. The U.S. experience has been that the successful completion of drainage-basin research programs and the usefulness of their results depends mostly on the quality and intensity of problem definition and on the consequent terms of reference. Consequently, the Work Group urges that U. S. scientists emphasize the roles of problem analysis and

and planning in the successful implementation of research programs.

The US/IHD experience also has been that requests for raw data, or even partly processed data, are as yet far fewer than anticipated when the IHD program of basin-data exchanges was initiated. Fewer than 50 requests for information have been received at all 60 US/IHD research basins, suggesting that it might be more productive to concentrate for the next few years on the exchange of information about the planning, methodologies, and results of studies rather than about the preparation of raw data for joint cooperative analysis. A possible reason for the seeming lack of interest in available raw data might be the difficulty of handling masses of new data with incompatible computer systems. In order to have useful exchange of unfinished data for various comparative studies, procedures and methods for using basin information require far more rigid standardization and more complete intercomparison than are practiced today. This does not, of course, preclude the exchange of raw or partly processed data among those who are ready to use it.

Another aspect of the IHD program of information exchange was that it was envisaged as global in scope. Exchange of ideas, techniques, instrumentation, results, and data was urged without sufficient regard for common problems and interest. This scatter approach to exchange sent masses of literature and data to places where they often went unused; much of the data had first to be carefully examined before their uses could be identified. By the middle of the Decade, the significance and usefulness of exchanges on the basis of common problems of geographic proximity became accepted. The International Working Group strongly promoted regional cooperation on the basis of geographic similarities, using climate, soils, and physiography as criteria for their identification. In 1974, the Swedish National Committee for the IHD invited regional cooperation on drainage basin research in regions of high latitude and low temperature. Canada, the Union of Soviet Socialist Republics, and the United States joined Sweden and other Nordic Council countries (Denmark, Finland, Iceland and Norway) in this program, and plans are underway to establish a regional working group for cooperation in high-latitude basin research under the post-Decade International Hydrological

Program[15/]. Informal exploratory talks continue to be held to assess coordinated regional cooperation in research in arid and semiarid zones.

15/ Editor's Note: In April 1975, the Swedish National Committee hosted a workshop at Edefors, Sweden, that resulted in an outline of common objectives, a plan for organizing coordinated research, and a tentative second meeting to be held in Alaska in 1977.

Conclusions and Recommendations

The Work Group suggests that the U.S. experience in drainage-basin research should be documented and disseminated worldwide to show how such studies have been and can be the source of useful scientific and practical results, and to help new research efforts avoid pitfalls.

To this end, the Work Group offers seven recommendations. Four are concerned with methodological matters, two urge further use of data already collected in the United States in order to benefit water science and management in the United States, and the seventh seeks to improve and increase regional cooperation.

At the Basin Level

Problem Definition and Identification of Objectives. Many small-basin projects apparently have been initiated, established, and completed without adequate definition of study objectives, or of the relations of these objectives to the availability and accuracy of instrumentation, conceptual and institutional capabilities, or the feasibility of attaining the decired results. The Work Group considers the following recommentation to be fundamental, and perhaps the most important single piece of advice it can give.

> Recommendation 1: A rigorous analysis of the problem, the objectives, the proposed study, and the available research capabilities should precede any decision to begin a drainage basin research project.

The Need to Consider the Full Scope of Hydrology. A number of basin studies appear to have been established with relatively limited consideration of the complexity of the biological, chemical, and physical interactions that take place in natural systems. Encouragingly, some of these programs have been redirected and enlarged to attack basin problems on a more comprehensive basis. This practice, however, is not yet as extensive as the Work Group believes it should be.

> Recommendation 2: Hydrological research in the field should incorporate entire hydrological units, such as drainage basins, and the scope of research in these basins should include the response of the entire hydrologic system of physical, chemical, and biological processes to the particular variable of interest.

The Need for Interdisciplinary Cooperation and Coordination. The definition of study objectives must include consideration of the interaction between the hydrological regime and the total ecological regime, including the influence of man's activities. The final problem may well be defined strictly in hydrological terms, or in terms of any other single discipline, but this definition should be accepted only if it remains satisfactory after thorough analysis of its long-range and cross-disciplinary effects.

> Recommendation 3: In order to establish a framework within which specialists in disciplines appropriate to particular research projects can select and refine their objectives, drainage basin research problems should be analyzed and defined on as broad an interdisciplinary base as possible.

The Project as Part of a National Research Effort. The spate of mathematical and related models proposed in the hydrological literature in the past decade suggests that many research-basin investigations were carried out to provide data for the investigators' own models. This certainly may have been necessary in the early stages of modeling research, but the Work Group believes it is far more scientifically productive to plan projects so that the resulting data will test existing hypotheses or develop new unknown functions. Little real progress can be made until the scientist can separate the useful from the unuseful, inappropriate, or inept in the many models introduced into the literature.

> Recommendation 4: Hydrologists should plan basin research in terms of new experimentation or of the critical test, rather than in terms of gaging a basin as an aid to constructing their own models.

At the National Level

U. S. Program of Research Basins. Probably more than 1,000 basins have been instrumented in the United States. Yet, nationwide assessments of the interrelations between hydrology and land use are based mainly on data derived from many unrelated studies. Many of these studies were designed for other purposes, and consequently the resulting assessments are uneven in quality and usefulness. There is also, as yet, no consistent understanding of the differences and similarities between the water budgets of adjacent areas, especially in the neighborhoods of their boundaries or divides, or of the similarities between areas that are geographically separated but have similar landforms, climate, geology, vegetation, and land use. An assessment of water resources based on similarities and differences between hydrological characteristics and on the interrelations of these characteristics with those of the surrounding environments would mark a long step forward in the advancement of water science in this country and would be of great value to planning and management. In addition, the hydrological work underlying such assessments would also be the basis for improving techniques for extrapolating hydrological insights from gaged to ungaged streams of similar physical character.

> Recommendation 5: To improve this country's capability to assess water resources, the Work Group urges that U. S. agencies doing basin research seek support for coordinated studies in a nationwide system of basins that can be selected so as to use

existing facilities and that represent the distinct combinations of geological, topographic, and climatic characteristics of the country.

Accessibility and Availability of Existing Data. Information from many instrumented small-basin programs that existed long before the IHD and continued during the Decade is not readily available to researchers and others interested in water-related aspects of drainage-basin management. Such information includes hydrologic data that have not been reduced, analyzed data that have not been synthesized, and results that have not been published or released. In the interests of justifying future support of drainage-basin research, the Work Group believes it would be to the benefit of both scientists and managers if agencies accelerated their programs for making existing data and information accessible and available.

> Recommendation 6: U.S. agencies with large amounts of unreleased data from research basins should seek funds to accelerate current and future programs for making data and information from drainage-basin research readily accessible and available to scientists and managers who need it to aid in the solution of the hydrologic problems.

At the International Level

Exchange of Information and Regional Cooperation. Exchange of information has usually been limited to techniques and research results. In connection with drainage basin investigations, the Work Group sees the need to make information exchanges more effective by also stressing the importance of and techniques for defining problems, designing investigations, and organizing programs. The Work Group urges the encouragement of the recent trend toward intensifying communication among researchers in adjacent countries or in regions with common characteristics and problems.

> Recommendation 7: Exchange of information about hydrological research in drainage basins should stress techniques of problem definition and

program organization and should foster cooperation and coordination of activities of organizations and individuals working in similar environments or on similar problems.

Appendix 1
"In Defense of Experimental Watersheds"
A Condensation

In Defense of Experimental Watersheds[a]

J. D. Hewlett, H. W. Lull, and K. G. Reinhart

Abstract: Recent criticisms discount the contribution of experimental watersheds to the science of hydrology and to watershed management. The critics cite as disadvantages the cost of experimental watersheds, their unrepresentativeness, leakiness, difficulty in applying results to other areas, and the lack of progress in basic knowledge about hydrological processes. Some critics propose mathematical synthesis, statistical analysis, plot studies, soil moisture studies, meteorological methods, and the study of individual hydrologic processes as alternatives to experimental watersheds. The criticisms lack weight, because published results of catchment experiments were not carefully reviewed. The alternatives are obviously aids rather than substitutes for experiments on watersheds. By reference to recent and older results, the authors argue that the experimental watershed method has produced much of our present knowledge about the land phase of the hydrologic cycle and man's influence on it, that the method is sound, and that its future in any comprehensive research program is secure.

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a/ Extract from Hewlett et al., 1969. "In Defense of Experimental Watersheds," Water Resources Research, Vol. 5, No. 1, pp 306-316. Copyrighted by American Geophysical Union.

Introduction

We propose to [present the case for the experimental watershed] by considering first the disadvantages the critics cite, rebutting them, and then giving the advantages. Next we shall discuss the alternatives to experimental watersheds, and following this, the unique contribution of some classical catchment experiments to hydrological knowledge. Finally, in a reappraisal, statements from critics and proponents will stress the inevitable role of experimental watersheds in future research.

In terms of results from experimental watersheds, we shall refer chiefly to quantity and timing of discharge as they are influenced by physical and biological changes in land use...based chiefly on experiments with forests and alternate types of cover, for they constitute our experience.

Disadvantages

Table 1 lists major criticisms in the order in which they will be discussed.

They are costly... This criticism, we believe, lacks specificity. Costs can be properly evaluated only in relation to returns or to costs of alternative methods for securing the required information. We know of no rigorous analysis of cost benefits in watershed research (a sorely needed type of investigation), nor are we aware of alternatives to watershed experiments that would provide quantitative information, for example, on the influence of forestation or deforestation on the amount and timing of water yield.

TABLE 1. Major Criticisms of Experimental Watersheds

	Slivitzsky & Hendler	Ackermann	Panel Watershed Research	Renne	Reynolds & Leyton
They are costly	x	x	x	x	x
They leak		x			
Unrepresentative	x	x	x	x	x
Changes too small for detection		x		x	
Difficult to transfer results	x	x	x	x	
Integrated results conceal processes	x	x			x

Furthermore, some of the criticism of earlier "experimental" watersheds stems from a misreading of history. The money spent setting up gaged watersheds during the 1930's was not spent solely to further our knowledge of hydrology. The investment had the additional objective of relieving unemployment and pumping life into the sagging economy of the nation. Therefore, not all costs associated with the return from these studies are chargeable to experimental watershed research, even though termination on some of the hastily installed hydrologic networks was overly delayed. These early efforts did serve to show that a vast job of research lay ahead of us and revealed what we now take for granted, that the water balance of a watershed is complex and not easily determined. To say that it was wasteful is like saying that the waste attendant upon the opening of any frontier is unjustified. We must, however, agree with Ackerman [See References, p. 45] that there is no longer any excuse for poorly planned watershed research.*

...Watershed research, by reason of recording instruments, probably collects more data per doller expended than any other branch of field research; we readily concede, however, that accumulation of data without analysis is pointless. Lately, data reduction and analysis have been speeded by use of computers...

Time is certainly a costly element. Seldom in this country has anyone recommended a paired watershed calibration period of less than six years, but some long-term experiments...showed that calibration periods as short as three years give useful information when the treatment period is longer... Furthermore, simultaneous calibration of a group of watersheds can reduce the time element. Once a set of catchments has been gaged together for one experiment, a sequence of superimposed experiments becomes far less costly and far more valuable, because their results are additive... Some troublesome, but not at all unsolvable, problems regarding statistical confidence limits and serial correlation of climatic variables

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* In fact it was to avoid the shortcomings of earlier experimental research that Ackermann pointed to difficulties and to the need for careful watershed characterization in order to produce meaningful results (Ed.).

remain, but the critics might do well to look closer at the accumulating results from experimental watersheds before they conclude that the costs are too high.

They leak. Ackerman [and others] point out that in small experimental watersheds, the subsurface divide can make much of the underground flow bypass the principal channels or permit substantial inflow from the outside...

Most watersheds leak; we can agree that the runoff at the weir is the residual after evapotranspiration, changes in basin storage, and leakage [either in or out] are summed and subtracted from precipation. Damaging as this at first seems, the situation is not so hopeless. To begin with, no one should be advised to undertake experiments on water yield in karst terrain, in complex glacial deposits, in tilting strata known to contain interbedded aquifiers and aquicludes, or on watersheds so undefined that the surface and subsurface catchment areas differ by 10 percent or more...

The use of paired or grouped catchments isolates and offers control over the leakage problem, as revealed by the solution of the watershed experiment... [The authors present a mathematical analysis to demonstrate this control.]

...Moisture flow beneath the surface, assuming that we have avoided hazardous terrain in selecting the experimental pair, is known to be very slow and responds rather reluctantly to small changes in the hydraulic head that might increase or decrease the flow. It will be difficult to reason that the changes in hydraulic head at the zone of leakage are more than temporary and minor. At all times the easiest pathway would be the stream channel, where the change in yield will be measured. The reasoning in specific cases is arguable, but the burden of proof should rest upon those who seek more difficult explanations for the substantial effect that watershed treatments have had on the water balance of experimental catchments.

We conclude then that leakage is endemic to watershed research. It can be guarded against by careful selection of watersheds; it is less damaging in determining yield changes due to treatment than in water-balance studies. It must always be considered in analyzing watershed experiments, but...it does not reduce results from watershed experiments to "circumstantial evidence." The question of leaking weirs is not a comfortable one, but it is no more nor less a problem to the watershed researcher than, say, circuit failures are in a laboratory.

They are unrepresentative. The unrepresentativeness of experimental watersheds was noted by all critics...

Considering the great variation among watersheds, of what should they be representative? If a 100-acre experimental watershed had exactly the same proportionate land use as one of 100 square miles, including 10 percent forest cover, then clear-cutting the forest area would be likely to have no detectable effect on water yield on either the model or the prototype. But water-yield increase per unit area from an experimental watershed totally forested, then clear-cut, gives some understanding of the magnitude of the effect and perhaps a reasonable estimate of the amount of increase to look for on the larger watershed. If there is no increase, why is there none? This question in itself should be a target for research, not a reason to abandom the experimental watershed approach.

Unrepresentativeness has been more feared than studied. It should be axiomatic that within a physiographic region the rain falls on experimental and nonexperimental areas alike, and that the similarities in response may be more meaningful than the differences.

Changes are too small for detection. Ackermann points out that the effects of land-use change on the timing of discharge and on total water yield are of lesser magnitude than the effect on sedimentation and may not be detectable, or may be detectable only in the growing season. This criticism hardly applies when experiments have been properly planned and conducted, as many have. Fairly small changes in yield, as low as 5 to 10 percent have been readily detected after cover changes on forested watersheds. There should be no concern in cases where changes are detected in one season and not in another... On the contrary, these findings are of considerable significance, since they reveal how the watershed operates to deliver water from storage...and point to interception losses as a major factor in changing water yields... Furthermore, although changes in yield in the growing season may amount to only a small proportion of total annual flow, they may still be of real importance, because this is the season of greatest need. In any case, changes too small to detect should not worry us, since it is the detectable changes that are really important.

It is difficult to transfer results. This is closely allied to the criticism of unrepresentativeness... The difficulty in answering this [criticism]...suggests why there have been many papers describing watershed research results, but very few describing how they may be applied.

Translation of results is made more difficult by the fact that the experimental treatment itself may be unique. No two basins contain the same soils or vegetation arranged, structured, and exposed in the same way, and thus no two basin manipulations can be carried out in exactly the same manner. Unfortunately, this is the nature of the system we are working with; even if we understand the physics perfectly, we would still have difficulty prescribing exact treatments to produce exact results. Therefore, attributing the difficulty in translating results to the experimental catchment approach is hardly reasonable. Furthermore, there have been until recently few quantitative results to translate. [Since the middle 1950's however,]...results from about 30 experiments [offer]...no real doubt about the main conclusion: Evapotranspiration and streamflow are influenced by the type, size, and quantity of vegetal cover to an extent not thought possible a few years ago. We are just now beginning to learn what a key role vegetation plays in the water balance of small-to-medium-sized watersheds. Much of what we have learned has come from paired and grouped catchment experiments.

Integrated results conceal processes. Another limitation in watershed research, according to Slivitzsky and Hendler, is "our complete ignorance of the precise causes and effects of the different components of the hydrologic cycle." ...This is no longer the situation. Simple curiosity and the need to explain watershed experimental effects have led to supplementary observations and studies on plots, sample stands, watershed models, and atmospheric factors. Mechanisms of water loss, storage, and delivery are now studied on the experimental watershed by means of soil moisture measurements with nuclear probes, net radiation measurements, surface and subsurface-flow observations, and all the trappings of modern science. We agree with Wicht [1966] that studies of this kind are "indispensable to the complete understanding of catchment management effects as demonstrated by catchment experiments." We further add that study of physical and biological processes is most useful when carried out on and in the light of watershed experimental results...

Many plot and laboratory studies have failed to yield valuable results because they were not related to precipitation and streamflow on whole watersheds. Sometimes quite false ideas have emerged from such piecemeal approaches. For example, no one has really succeeded in relating infiltration tests to storm runoff, yet many hydrologists still present plot infiltration data as an explanation of storm hydrographs from whole watersheds.

...Although integrated results tend to conceal individual processes, it should be remembered that this can be an advantage if the objective of our work is to provide practical guidelines in terms applicable to the real management situation.

Advantages

The first argument for the watershed experiment is perhaps that, if we wish to manager watersheds, we shall have to study watersheds. Most critics will agree that plot and laboratory studies are not enough...[but they also] seem unaware that watershed research illuminates processes. Students of hydrology will admit that we do not yet fully know what physiological, structural, and quantitative vegetal factors cause different evaporation rates from place to place. But up to now more has been learned about these complex factors from experimental catchments than from any other approach to the problem...

In several important ways, experimental watersheds provide more precise data than plots. A built-in drainage system conveniently collects its water and delivers it to a measuring station, where, with a continuous recorder, we can measure all changes in discharge. Plot observations, on the other hand, provide diminutive samples such as those obtained for precipitation and soil moisture.

For example, Ackermann states that, with the neutron probe, the problem of soil-moisture measurement is "satisfactorily resolved." However, even now, the accuracy of soil-moisture measurement relative to streamflow measurement is second-rate. Even with the obvious advantage of the neutron method, we cannot detect differences between mean moisture content of small watersheds within 1 percent of volume; we cannot even properly sample a watershed soil mantle. But consider a whole catchment; a 1 percent difference in mean soil moisture content, when translated into streamflow, may be readily detected at weir (in a 100-acre watershed with mean soil depth of 6 feet, 1 percent moisture by volume is about 260,000 cubic feet, which may well be a month's total streamflow in many areas).

The principal advantage of the paired or grouped watershed experiment is the high correlation commonly secured between two carefully instrumented catchments near each other. This high correlation serves as the best experiment control we can get over climatic variation from season to season and from year to year.

Alternatives

...Alternative procedures appear to be of value to the extent that they employ watershed data, or that their results are tested on watersheds, or that their findings are interpreted in light of watershed research results. Currently, there are three of these approaches.

Evapotranspiration theories. The addition of energy balance studies to the study of water balance on watersheds was a genuine advance in our efforts to understand the water cycle over land areas... But calculations of energy and vapor transfer have not yet become a substitute, as some seem to conclude, for the watershed experiment. Evaportation equations have so far failed to predict or to explain the astounding differences in water yield that can be produced by different plant covers on watersheds... Computing vapor losses and energy transfers is not an alternative to watershed experimentation but rather must be considered one part of the larger job of explaining how the water cycle works.

Mathematical synthesis. Our limited understanding of hydrologic processes has in part been employed in digital simulation methods to reproduce the hydrograph. Watershed storage, losses, and transmission rates are assumed as estimates of interception, infiltration, and soil moisture retention and detention... Analyses are repeated, adjusting estimates for selected processes, until a good fit is achieved... [However] simulation studies are of real value only so far as they utilize good input data, many of which come from watershed studies; without sound input data, simulation studies are merely an exercise in numbers... Simulation may be a good way to use existing knowledge, but it is a poor learning process. The continued study of both runoff process and whole watersheds will be necessary to improve these methods. We should beware of the notion that mathematical simulation itself may be substituted for the study of watersheds and hydrologic processes...

Statistical analysis. A somewhat related approach seeks to develop predictive methods and basic principles from the thousands of hydrologic records now on file... Inherent to this approach seems to be the thought that useful synthesis of the hydrologic cycle from our knowledge of deterministic processes is so far off that we must be satisfied with average relations based on physiography, runoff, rainfall, and other commonly measured geophysical data. Over-simplified, the approach amounts to assembling available data into a statistical analysis, assuming that all processes are expressed in a stochastic, and thus nondeterministic, manner. The method has obvious merit in a country with a large backlog of relatively unused data; useful comparisons among hydrologic regions have resulted...and have even helped to determine the influence of selected environmental factors on the hydrology of basins... Similar synoptic methods permit assessment of the regional applicability of streamflow behavior measured on experimental watersheds. Certain long-term streamflow records may be particularly useful in identifying trends associated with major changes in land use. Statistical summary and multivariate analysis will broaden the applicability of experimental watershed results but will not substitute for the experiments themselves.

Contributions from Experimental Watersheds

Management Implications. Differences in streamflow after calibration and treatment provided the first sound evidence that forest cutting increases water yield, and that forest growth reduces it... Later these experiments provided evidence that the magnitude of increase was proportional to the amount of cut... In several studies the conservative effect of careful forest cutting and regrowth on peak flows and water quality has been demonstrated...

...Multiple treatments on experimental watersheds, unless carried out chiefly as a pilot test, have taught us little, because the hydrologic effects cannot be traced to specific practices. Nevertheless, scientific management of the land will probably be aided more by better planning, design, and execution of watershed experiments than by wholesale retreat to the laboratory and computer.

Scientific Implications. Observing hydrologic processes on experimental watersheds has brought about fundamental advances in scientific hydrology. For example, the lack of overland flow from forested catchments has led to reexamination of the whole mechanism of flow production by watersheds. Investigation of the source of subsurface flow suggested that stormflow comes from a variable source area along stream channels... Under these conditions the concepts of infiltration and the unit hydrograph, the latter predicated on the idea of overland flow, have no real application, despite the many efforts to relate storm flows from forested watersheds to excess rainfall derived from infiltration curves...

Sources of base flow on small watersheds have also been clarified. Groundwater was always assumed to provide the flow during the growing season, but groundwater is often limited in steep areas, and experiments on small watersheds have shown that much of the base flow stems from the flow movement of capillary water through the unsaturated soil mantle...

Other observations on watersheds deal with the role of rainfall interception in evapotranspiration, the persistence of snow interception on conifers in relation to its evaporation, the relatively limited storage capacity of the organic layers of the forest soil, and the flood potential of snow accumulated under confers. Other studies showed how tree roots absorb water from very deep layers of soil and how impossible it is to specify 'available water' under such conditions...

Reappraisal

Critics of experimental watersheds reluctantly agree upon the necessity for them, despite their alleged disadvantages. Slivitzsky and Hendler state that "contrary to the impression that we have given up to now, we do feel that watershed research programs do have a place in the science of hydrology"; apparently they feel the place is small but note that work with catchments develops an instinctive feeling or appreciation for the interplay of hydrologic phenomena and aids the development of measuring equipment and techniques. Ackermann is more generous: "We still need, and in fact have no alternative to conducting experimental watershed research." His only qualification is that the research should be properly conducted, a qualification we can readily accept. Reynolds and Leyton also will not abandon watersheds despite the promise of alternate methods, for "the catchment must always remain the ultimate proof of hydrological conclusions using other techniques." Pereira (1962) waxed eloquent on this subject, emphasizing the need for watershed research in East Africa "...so that, in these critical land-use problems, the bright plan for a brave new world in Africa may be based on locally tested fact, rather than on opinion from overseas."

Possibly our bias is showing. We agree with Slivitzsky and Hendler that much more thought has been given to presenting the results of watershed research than to presenting their use. We agree with Ackermann that in years past small watershed research was gaged simply because it had not been before, and a bad habit of calling these "experimental watersheds" began. We agree with Reynolds and Leyton that there are promising methods of hydrological research other than the watershed approach. Nevertheless, although we are still looking for alternative approaches, we feel that we would be poor scientists if we did not offer this defense and remind our colleagues how much our basic hydrologic knowledge has come from well planned watershed experiments and how much more we can learn from them in the future.

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Appendix 2

Design of a Basin Program
An Outline

Appendix 2
Design of a Basin Program

I. DEFINE AND ANALYZE THE PROBLEM FORMALLY

a. What is the PROBLEM to be solved?

1. Is the problem real?
2. Who has the problem?
   A developing country, an agency, a landowner, a scientist, a land manager--?
3. Define the problem in terms of scientific and management objectives.

b. Analysis of the problem.

1. What is known already?
2. Is it need **for** information, or for a technique, or for new scientific insight?
3. Is it needed to cut costs, to anticipate impacts, to settle a resources policy matter?
4. Can it be done with foreseeable resources?

c. Review definition and analysis of problem with interdisciplinary specialists.

II. PRESENT CLEAR OBJECTIVES

a. What is the specific question to be answered?

1. What answers are already available? Are they useful?
2. Is anyone else working on the question?

b. Is it a researchable question, including the need for technique development, or a proposal for more routine data?

   1. If researchable, what are the hypotheses to be tested?
      - Alternative research strategies.
      - The cost of each strategy.
      - Criteria for evaluating and selecting strategy.
      - Is a research basin study the best choice?
   2. If need is for new data, exactly what data?
      - Alternate methods; cost and value of a data unit in each method.
      - Choice of methods.
      - Is a research basin study the best method?

III. DESIGNING THE EXPERIMENTAL FRAMEWORK

a. Analyze past experience, using libraries and consultants.

   1. What are the useable approaches and how can they be applied?
   2. What sources of delays and errors can be anticipated and how can they be controlled?
   3. How many samples of basins, treatments, time units, gaging sites, etc., are going to be needed?

b. What models and functions should be used?

   1. What models are already available? or which relationships are already known?
   2. Are they adequate?

c. Restate the hypotheses and how they are to be tested.

d. How much time should the study take? Set termination date.

e. What personnel, instruments, and field sites are needed?

f. What standard units and methods of data reduction are to be used?

g. What reports will be needed? If expected results appear to be not worth reporting, reconsider adviseability of project.

h. Total the costs and benefits, and re-evaluate.

1. Will the results be worth the cost and effort?

IV. ANALYZE ALL DATA COLLECTED

a. Keep objectives clearly in mind.

b. Use proper computational methods to reduce and analyze data.

c. Draw inferences in the light of statistical tests and limitations they impose.

d. Review progress and objectives often enough to assure attainment of objectives.

e. If changes must be made, make them consciously and modify work-plan accordingly and formally. (See I.)

V. PREPARE NECESSARY REPORTS

a. Different audiences require different reports.

b. Allow time for preparation of reports.

c. Different reports have different objectives, but at least one report must satisfy objectives of the investigation.

d. Arrange for publication or other means to disseminate results of investigation.

VI. PLAN FOR NEW WORK

a. Write new problem analyses (i.e., start back at Item I) before continuing the collection of data past terminal date.

b. Do not fall into the "one-more-year-of-data" trap.

VII. TERMINATE THE WORK ON SCHEDULE