

CONF-7909206+-

MASTER

Assesment of Effectiveness of Geologic Isolation Systems

WISAP RELEASE SCENARIO ANALYSIS WORKSHOP

September 13 - 15, 1979

G. L. Benson, Chairman

February 1981

Prepared for the
Office of Nuclear Waste Isolation
and the U.S. Department of Energy
under Contract DE-AC06-76RLO 1830

Pacific Northwest Laboratory
Richland, Washington

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DAY 2
WISAP RELEASE SCENARIO ANALYSIS WORKSHOP

G. L. Benson
Task Leader
WISAP Release Scenario Analysis

INTRODUCTION

Welcome back to the second day of the WISAP Task 1 Workshop. Today, we would like to go into some detail about the geotechnical submodels that we are currently using and describe how they affect the layered-earth model. Primarily, the presentations will be an expansion on the discussions of the submodels that were in the draft report PNL-2892 that we discussed yesterday. However, I would first like to make a few comments based on the questions that were asked yesterday.

I would like to reiterate the basic ground rules and program scope that we are working within. I see two key areas that need clarifying. First, I would like to describe why we are undertaking the type of methodology development that we are. And, secondly, I would like to outline very briefly some of the other work that we are doing. Besides developing this methodology, we are dealing with a number of other parts of the waste management safety assessment problem.

Program Direction

Essentially, we are developing principles, practices, and procedures to assess the safety of the repository over a long time. To do that, we have had to come up with what we feel is a new methodology. Secondly, we are working on the basic premise that there will be no human mitigation of a breach; nor are we accounting for engineered barriers. We feel that our role within WISAP is to identify potential release mechanisms and analyze their consequence. Then the engineered barrier groups can assess the type of engineered system needed to defend against these release mechanisms.

These are really two basic assumptions that we are dealing with. These, as well as those assumptions I outlined yesterday, are keys to why we have chosen our current approach to release-scenario development.

Site Selection

I would now like to describe some of the other work Task 1 is involved in. There were a number of comments suggesting we take a larger role in the site selection, including helping provide criteria for siting and other related matters. We are presently involved in most of these aspects even though they are not formally outlined in our program plan. We attend meetings and participate in almost all aspects of the waste-management program. For example, we attended and participated in the NTS (Nevada Test Site) geological review meetings. We are constantly meeting with repository-site managers and with the geologic exploration groups in the Gulf Coast and in the Southwest. So we are very active in working with those exploration groups and with those people developing the site criteria. Often we end up doing more of that type of work than working on our methodology, just because of our time and manpower constraints.

We are also actively involved in providing input on various reference-site analyses that are currently going on, for example, the salt-dome site. Actually, the major thrust of our program this past year has been to do a reference-site analysis on salt domes. We are using expert opinion techniques to come up with release scenarios and then are working very closely with the hydrologic and consequence analysis task to provide a reference-site safety assessment for the Preliminary Information Report that ONWI is developing.

Also, we are looking at several geologic media, including bedded salts, salt domes, and basalt, and plan to start looking at granite next year. Again, we have been working with both WIPP and NTS people and the programs they have in bedded salts and tuff.

So, it may have appeared at the outset of this meeting that we were spending all our time developing the methodology we presented yesterday and not applying our expertise to other problems. I think in reality we are doing both. We are using our existing technologies as best we can to support the NWTs program.

I also would like to point out that what we have done on the program is preliminary and thus should not be taken as gospel. Part of our effort this year has been to provide a thorough review process of the geotechnical submodels we have developed. One of our main goals in this meeting is to present our ideas and to receive feedback from a broad segment of the NWTs program. This is an ongoing process and is one of our principal reasons for hosting this meeting.

HYDROGEOLOGIC FRAMEWORK OF THE PASCO BASIN

James W. Crosby III

ABSTRACT

Tholeiitic flood basalts of the Columbia River Basalt Group comprise the major rock sequence of the Pasco Basin. These basalts are known to be at least 10,665 ft thick at one place, but the total thickness is unknown. The basalts are commonly overlain by silts and sands of the Pliocene Ringold Formation. Glaciofluvial deposits referred to as the Pasco Gravels and Touchet Silts appear frequently at the surface. Other materials exposed at the surface include the Palouse loess and recent accumulations of dune sands, alluvium, and colluvium.

The Columbia River Group has been subdivided into five formations and numerous members based upon petrography, magnetic polarity, and chemical composition. Adequate knowledge of the stratigraphy is a necessary prerequisite to understanding the ground-water characteristics.

Unconfined ground waters occur in the glaciofluvial materials and the upper part of the Ringold Formation. Confined ground waters occur in the lower Ringold deposits and in the Columbia River Basalt Group. Disposal of liquid radioactive wastes in surficial materials is reflected in the configuration of the water table and movements of waters in the unconfined system.

Ground waters in the confined basaltic environment move largely through the interflow contact zones and certain interbedded sediments. Hydraulic measurements in the basalts have led to contradictory findings about the relative distribution of hydraulic conductivity. For many years vertical movement of water in the basalts was thought to be severely restricted in most places. Some recent studies suggest that vertical conductivity may be much more important than was previously believed.

A substantial amount of research is now being directed to better define the ground-water flow system and the recharge and discharge sites for local, intermediate, and regional systems.

INTRODUCTION

In this paper an attempt will be made to develop an overview of the hydrogeologic setting of the Pasco Basin. The intent is to provide the nonspecialist with a rudimentary knowledge of the area, so he will be in a better position to relate to the nuclear-waste scenarios that will be presented subsequently.

These remarks are not designed to provide the hydrogeologist with any information that is not already at his disposal. Quite to the contrary, it should be made clear that there is little of the writer's own work that is reflected, and credit is owing to the many scientists who have contributed to our present state of knowledge of the Pasco Basin.

Figure 1 shows the location of the Pasco Basin in south central Washington. The Pasco Basin lies within the Columbia River plateau physiographic province and is a structural and topographic low that is bounded on the north by the Saddle Mountains and the Frenchman Hills, on the west by Umtanum Ridge and Yakima Ridge, on the south by the Rattlesnake Hills and the Horse Heaven Plateau, and on the east by a broad, regional monocline that rises toward the neighboring mountains in Idaho.

The Basin is situated in a semi-arid area that encompasses about 2,000 mi², and is host to a sequence of tholeiitic flood basalts, known as the Columbia River Basalt Group. This group is the largest accumulation of Eocene Age continental basalts in the world and it comprises the bedrock of the region. Locally, within the Basin and within the confines of the Hanford Site, the basalts are deformed by eastward extensions of Umtanum Ridge that are above the surrounding fluvial plain as Gable Mountain and Gable Ridge.

The Pliocene Ringold formation in general, conformably overlies the basalt bedrock surface and consists of up to 1,200 ft of silts and sands, with some conglomerate. Its eroded surface is, in turn, overlain by as much as 200 ft of glaciofluvial and fluvial sands and gravels. The coarse and the

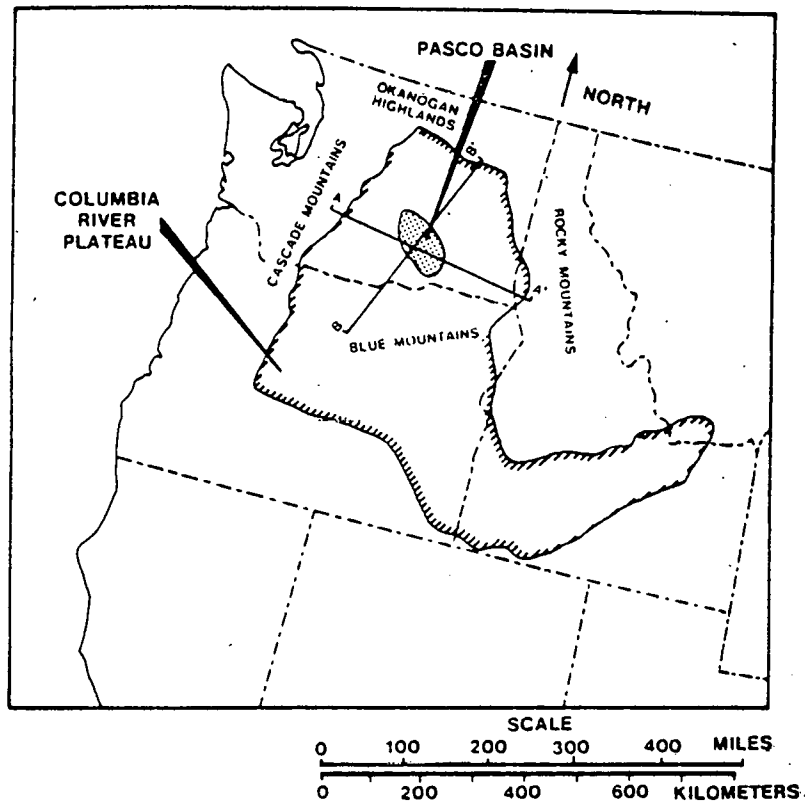


FIGURE 1. The Columbia River Plateau

fine facies of the glaciofluvial deposits have been referred to as the Pasco Gravels and the Touchet Silts, respectively. In places the Ringold formation is, in turn, overlain by windblown deposits of the Palouse formation.

Sand dunes and loess mantle the surface of the Pasco Basin and, together with localized deposits of colluvium and alluvium, comprise the uppermost materials. Individual sand dunes can reach thicknesses of more than a hundred feet.

STRATIGRAPHY

Stratigraphic relationships of the Pasco Basin have been fairly well established (Swanson et al. 1979) through extensive field investigations and

through the drilling of more than 1000 shallow and deep exploratory borings. (A few of the more important are shown in Figure 2.) The Rattlesnake Hills No. 1 well was drilled to a depth of 10,665 ft, penetrating about 100 individual basalt flows (Raymond and Tillson 1968). The uppermost 5000 ft of the section in RSH-1 conforms approximately to the Columbia River Basalt Group whereas the deeper section probably represents older basalts of Eocene to Oligocene Age.

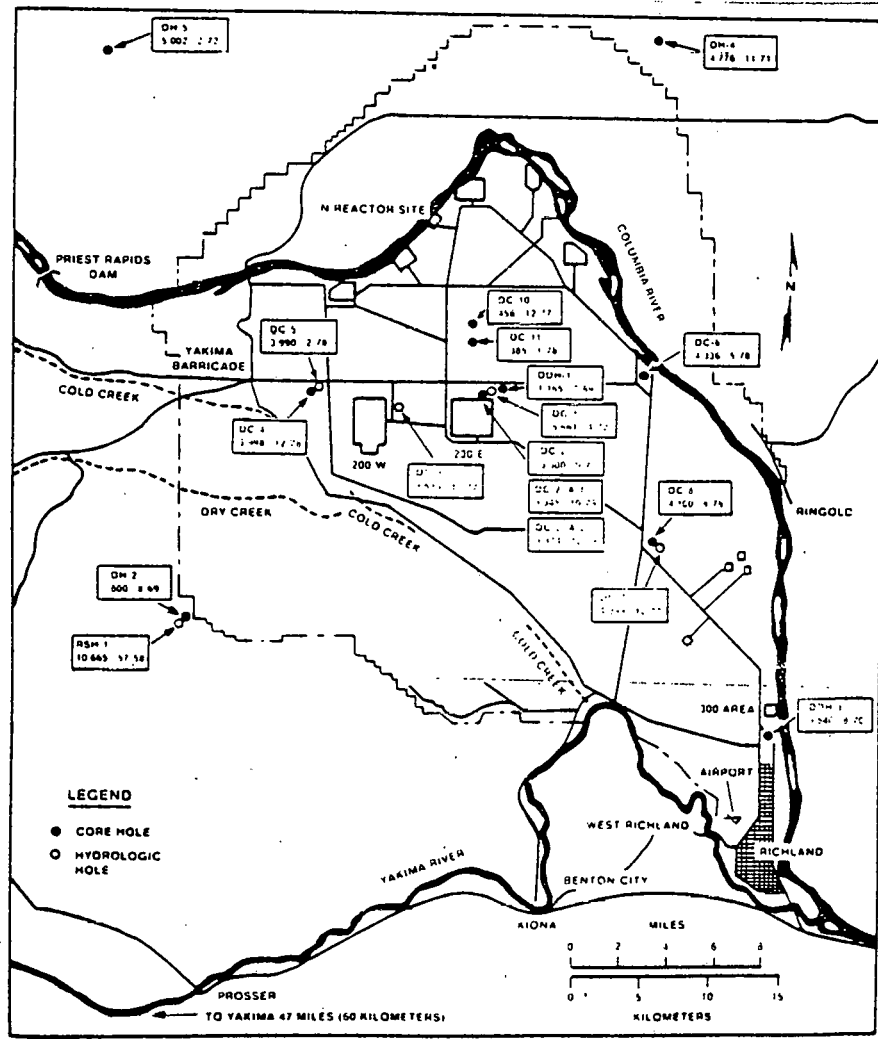


FIGURE 2. Basalt Waste Isolation Program Drill Holes (The Hanford Reservation)

Within the Pasco Basin the total basalt thickness clearly exceeds 10,000 ft and may be as much as 19,000 ft thick. What underlies the basalts is not presently known; however, the recent discovery of a very deep conductive section during the course of magnetotelluric surveys has suggested the presence of sediments underlying the basalts (Deju and Fecht 1979). These may be equivalent to sands and shales penetrated at depth in the Basalt Explorer Well northeast of the Pasco Basin and similar to those found in the Cascade Range. Rocks underlying this proposed sedimentary sequence could be metamorphics or granitics similar to those found in surrounding mountainous regions.

For the purposes of the waste release scenarios, attention will be concentrated on the basalts and associated rocks of the Columbia River Group and the rocks which overlie the basalts.

The Columbia River Basalt Group comprises a sequence of layered-basalt flows and inter-basalt sediments reaching a thickness of approximately 19,000 ft. In total, the basalts have a volume of more than 50,000 cubic miles, cover an area of over 60,000 square miles, and form a plano-convex lens, the surface of which generally dips toward the direction of the area of greatest thickness.

The Columbia River Basalt Group has been subdivided into five formations and numerous members based upon petrography, magnetic polarity, and chemical composition (Swanson et al. 1979). Current stratigraphic subdivision and nomenclature are shown in Figure 3.

The oldest formations in the Columbia River Basalt Group are the Imnaha and the Picture Gorge Basalts. As far as we know, these are not in contact, but the Imnaha is assumed to be older on the basis of magnetostratigraphic data. The Grand Ronde Basalt, the Wanapum Basalts, and the Saddle Mountains Basalt, in ascending stratigraphic order, form the Yakima Basalt Subgroup.

Areas in which each of the basalt formations is known to occur are shown in Figure 4 which also suggests that the Grand Ronde Basalt is the most widespread and voluminous ($150,000 \text{ km}^3$) of the group. This formation reaches its maximum thickness in the Pasco Basin where it is known from

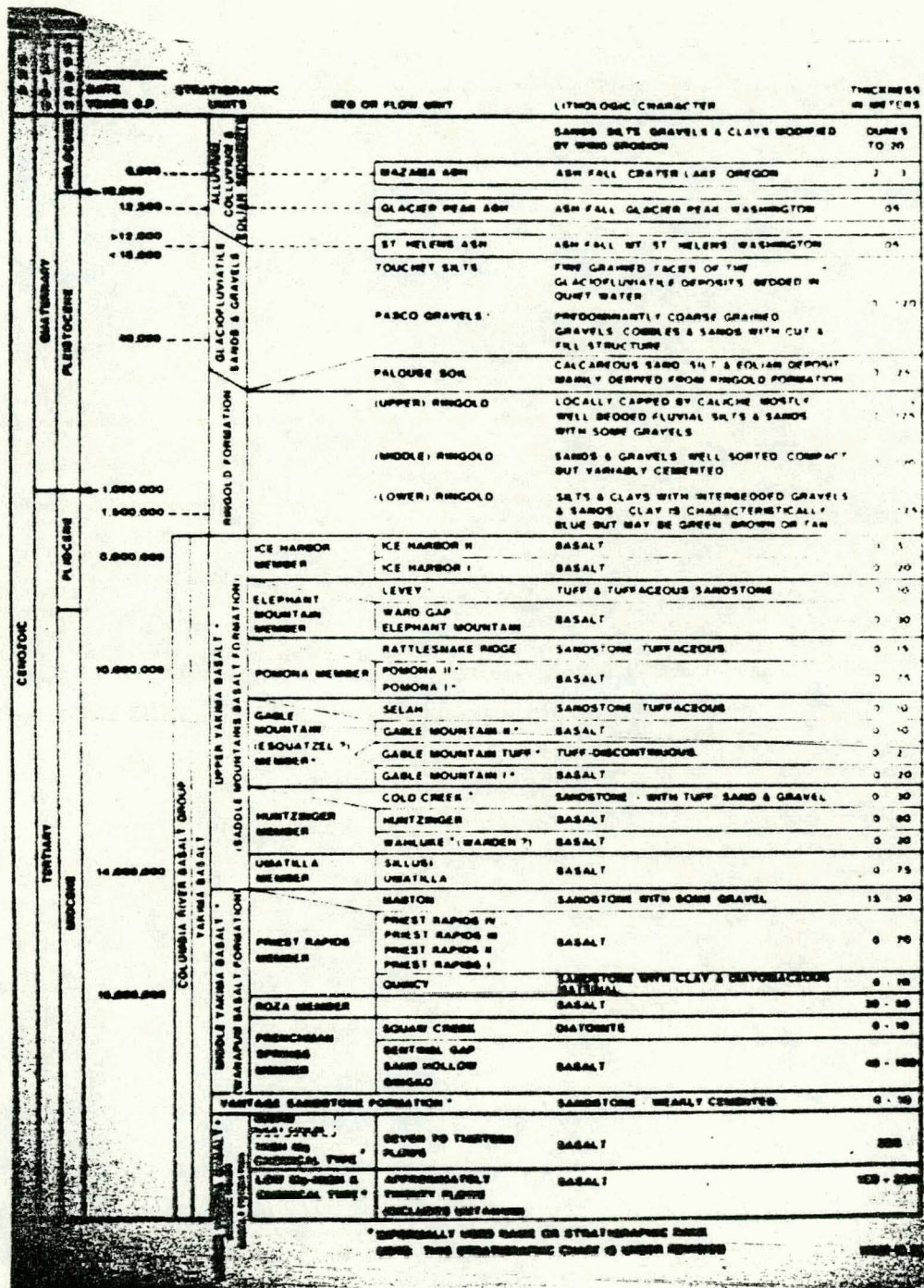


FIGURE 3. Stratigraphy of the Pasco Basin

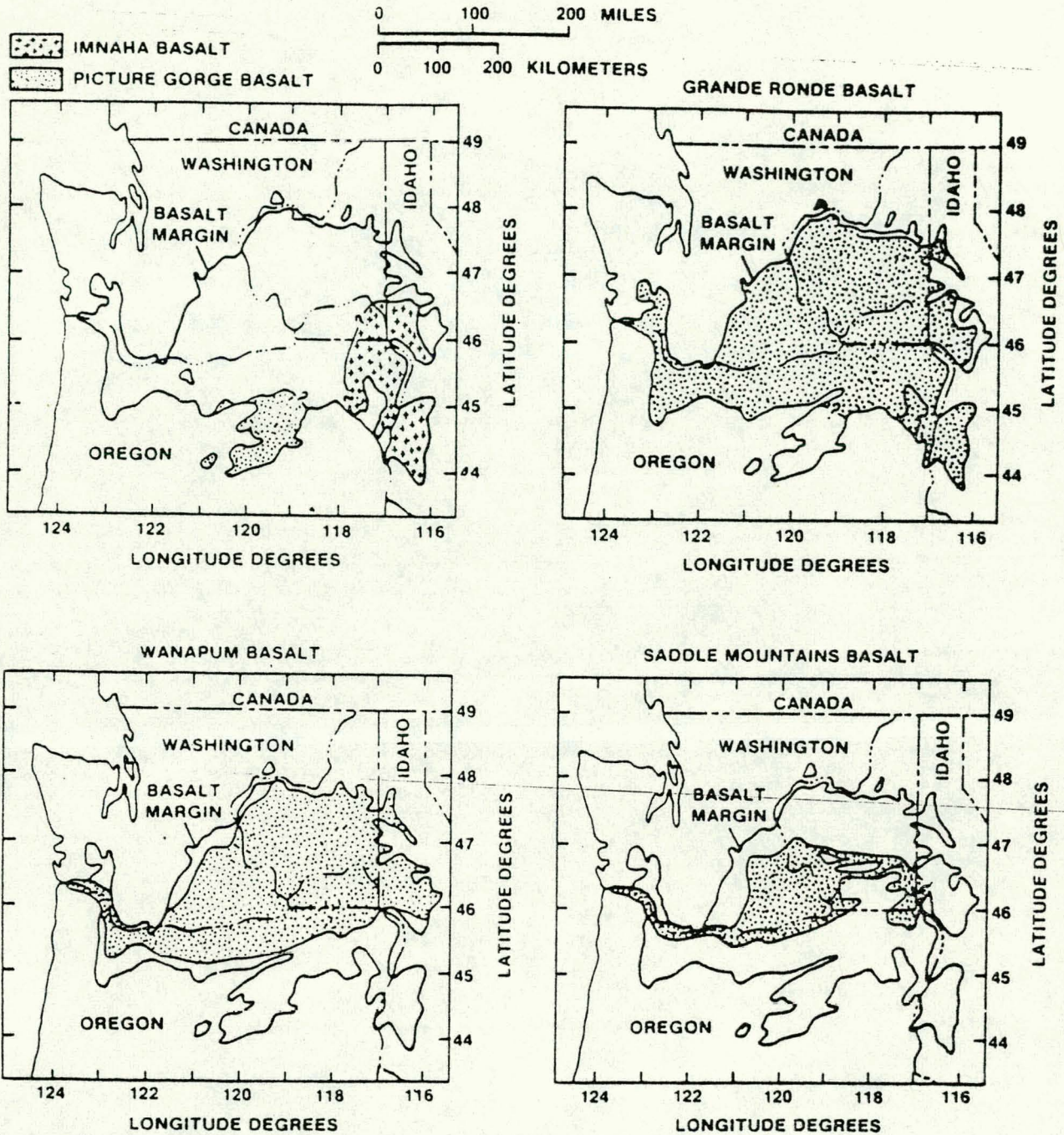


FIGURE 4. Distribution of Formations Within the Columbia River Basalt Group (Modified from Swanson et al.)

drilling to exceed 1000 m. A thick soil zone and localized sediments, possibly representing a time break of a few tens of thousands of years, normally occur on top of the Grand Ronde and separate it from the overlying Wanapum formation. The Grand Ronde conformably overlies the Imnaha Basalt.

The Wanapum Basalt has a probable volume of less than one fifteenth that of the Grand Ronde Basalt, but the Wanapum is the most extensively exposed surficial formation of the Columbia River Group. The Wanapum is locally conformable with the Grand Ronde except for minor erosion or interbedding. Regionally, the Wanapum progressively onlaps older basalts from the center of the Columbia Plateau toward its eastern margin.

The youngest formation of the Columbia River Basalt Group is the Saddle Mountains Basalt, a chemically diverse formation constituting less than 1% of the total basalt volume. The Saddle Mountains Basalt is generally conformable with the Wanapum but may display local angular unconformities in the folded areas. Because the Saddle Mountains Basalt was erupted during a period of waning volcanism, the flows were more sporadic and interflow sedimentary sequences tend to be thicker and more common.

Flows comprising the Columbia River Basalt Group were generally erupted from linear fissure systems in the eastern and southern portions of the Columbia Plateau (Swanson et al. 1979). The period during which the basalts were erupted lasted from about 16.5×10^6 to 6×10^6 years before present. However, the greatest volume of materials was erupted during a 2.5-million-year period between 16.5×10^6 and 14×10^6 years before present (Watkins and Baksi 1974; McKee, Swanson, and Wright 1977).

It should be noted here that the southwest end of B-B' conforms to the 10,665-ft Rattlesnake Hills No. 1 well. Note, also, that the two sections intersect at well ARH-DC1.

Figure 5 is section A-A' and clearly displays the basalts dipping toward the thickest section in the Pasco Basin, only 3000 ft of which are shown. This section extends stratigraphically into the upper part of the Grand Ronde Formation. The variable thickness of the post-basalt sediments can be noted.

Figure 6 is section B-B' and, again, displays a section down to, and including, the upper part of the Grand Ronde. Structural and topographic highs associated with basalt deformation in the Rattlesnake Hills, Gable Mountain, and the Saddle Mountains are shown as they stand out in relief above the fluvial plain. The deep basalt sequence explored by Rattlesnake Hills No. 1 well is indicated.

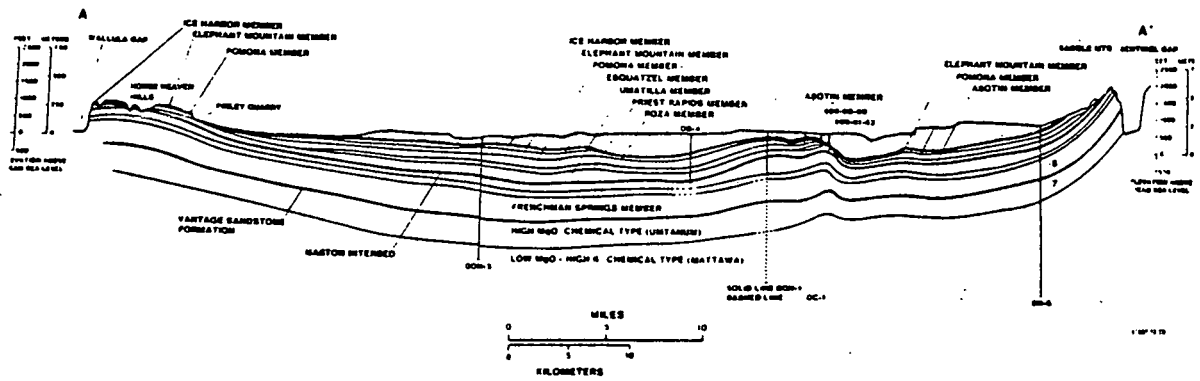


FIGURE 5. Generalized Cross Section A-A' Parallel to Main Axis of the Pasco Basin

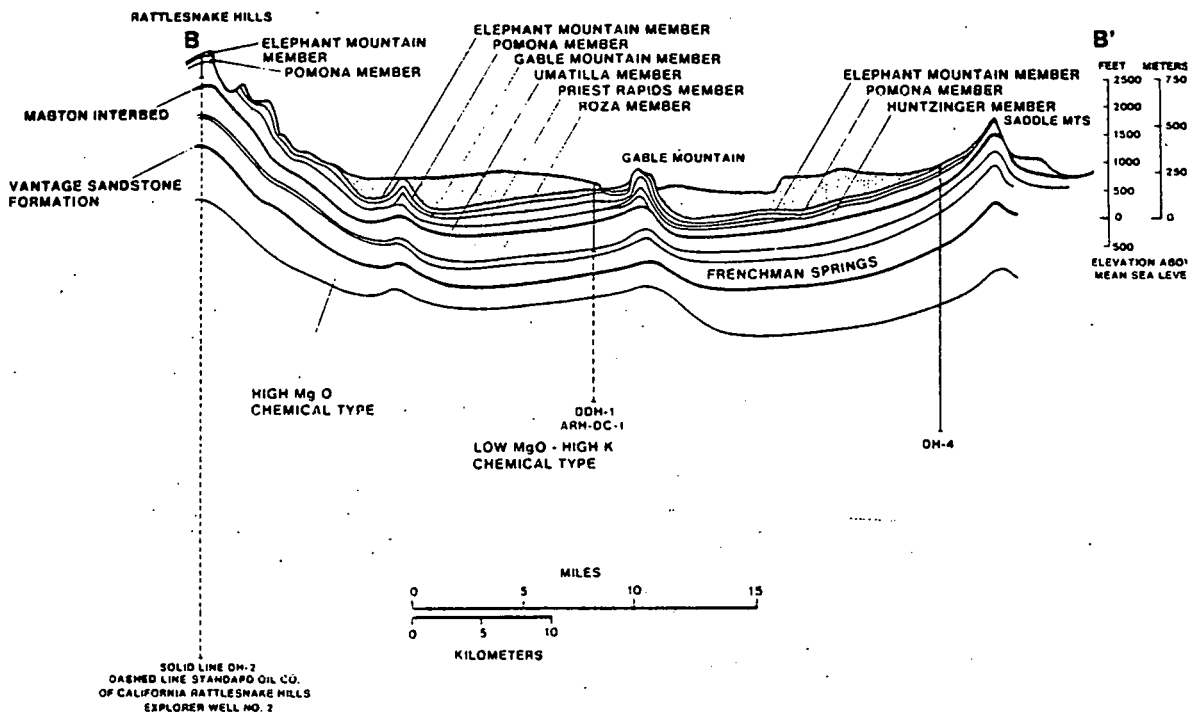


FIGURE 6. Generalized Cross Section B-B' Normal to the Main Axis of the Pasco Basin

Understanding the stratigraphy of the Pasco Basin is elementary and necessary to understanding basin hydrology. As most ground water is stratigraphically controlled, the ultimate disposition of water-borne contaminants can only be predicted through an in-depth knowledge of the stratigraphic and structural framework. The previous discussions of the basalts suggest that we are dealing with what has been termed "layer cake" stratigraphy, an oversimplification, but one that readily can be related to the layered-earth model (LEM).

HYDROLOGY

By way of a quick introduction to the hydrogeology of the Pasco Basin, modes of ground-water occurrence will be cursorily examined. In materials comprising the Ringold Formation and the glaciofluvial deposits, ground water is stored in, and flows through, the interstitial spaces between the clastic particles. This would apply, also, to sediments interbedded between two successive basalt flows.

Storage and transport modes in the basalts, per se, are quite different and are associated primarily with the interflow zones. In a basalt flow the pillow palagonite complex that exists at the base was formed when the flow invaded a body of water. Such zones often are associated with highly productive aquifers. The vesicles at the base and top of the flow, when interconnected by joints and fractures, also comprise highly permeable zones as do flow breccias, cinder zones, rubble zones, ash beds, and some fossil soil zones. The central, columnar portions of a flow generally display a lower permeability, and water moves dominantly through the occasional fractures and joints. The dense, central portions of the flow are thought to restrict the vertical movement of ground water, but Brown (1979) has recently questioned this.

Confined and Unconfined Systems

Ground-water conditions in the Pasco Basin can be categorized into a shallow, unconfined system, and a deeper, confined system. The unconfined

system, as we know it, generally includes materials comprising parts of the Ringold formation and the glaciofluvial deposits. Confined aquifers occur in the lower portions of the Ringold Formation and the underlying basalts and their associated sediments (Research and Engineering Division 1976).

Radioactive-waste disposal practices at the Hanford Reservation have dictated intensive study of the unconfined system and the upper portions of the confined system. This portion of the total section is well known to us because of the very extensive drilling operations. The unconfined aquifers discharge into the Columbia River, and at distances of up to three miles, are very responsive to its fluctuations (Newcomb, Strand, and Frank 1972). Hydraulic conductivities of the glaciofluvial deposits greatly exceed those of the uppermost Ringold materials and tend to control shallow ground-water flow patterns. Although natural recharge occurs at the base of the Rattlesnake Hills and Yakima Ridge along Cold Creek and Dry Creek, the present water-table configuration on the reservation is strongly influenced by waste-water discharges, and some radioactive contaminants tend to migrate rapidly in the unconfined system.

Figure 7 displays these relationships. It should be noted that ground-water flow is essentially normal to the water-table contours.

Figure 8, itself a gross oversimplification, illustrates some of the complex stratigraphic and lithologic conditions in the unconfined system. Note the influence of waste-disposal operations on the water table between 1944 and 1976. Note, too, the interfingering in the Hanford Formation, in the glaciofluvial materials, and within the Ringold. There have been many attempts to work out the subsurface stratigraphy of these materials, but it is generally found that once a few hundred or a few thousands of feet separate drill holes, any type of stratigraphic correlation is very difficult. This is a very complex system in the upper part of the geologic section.

The uppermost confined-aquifer system is generally considered to consist of the lower Ringold sediments and basalts of the Wanapum and Saddle Mountains Basalts Formations (Research and Engineering Division 1976). Aquifers

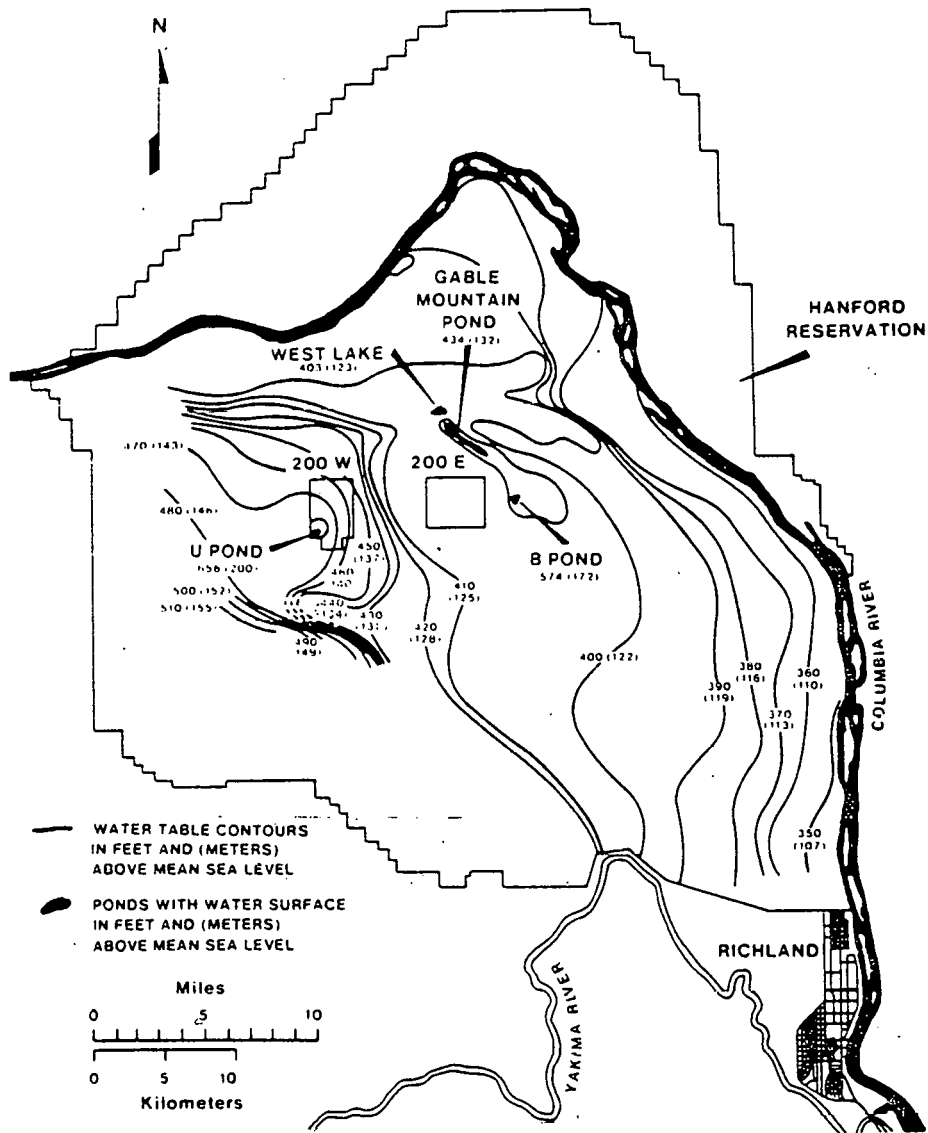


FIGURE 7. Hanford Reservation Water Table Map - December 1975

associated with this rock sequence would include permeable zones in the lower Ringold Formation and interflow contact zones and sedimentary interbeds of the basalts. Perhaps notable among the latter are the Mabton and Quincy interbeds, which are quite extensive and unusually thick units for the basalt sequence.

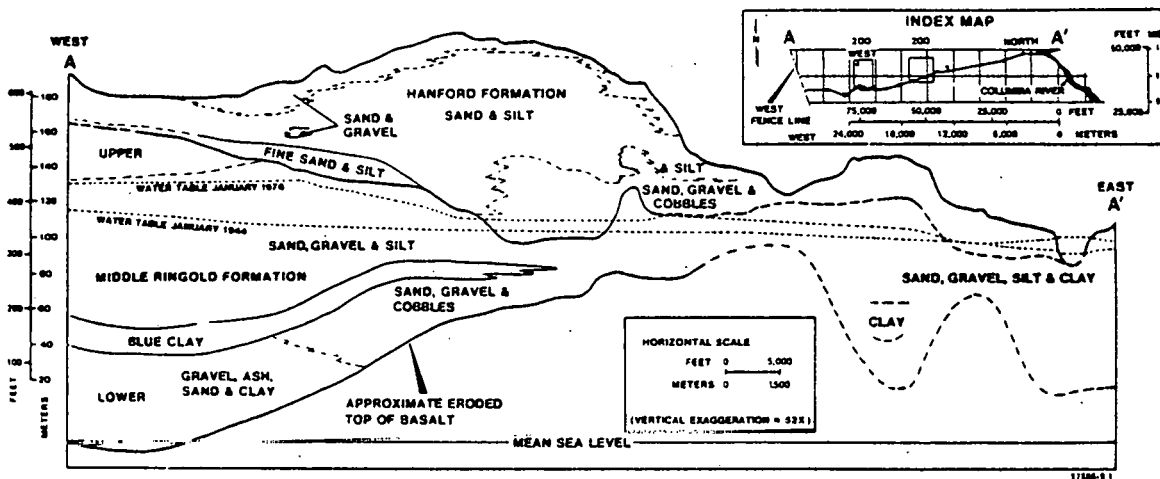


FIGURE 8. Geologic Cross Section Hanford Site

Figure 9 is a recent map showing the hydraulic head in the uppermost confined-aquifer system. The map itself is generalized because it is based upon limited data, and some of these data may be questionable. From the map we can infer that recharge to the uppermost confined system accrues from precipitation and stream flow in the higher areas immediately adjacent to the Pasco Basin. It can be assumed that leakage from the unconfined aquifers also contributes to the system (Deju and Fecht 1979). The potentiometric map also indicates that the upper confined system discharges into the Columbia River in much the same manner as the unconfined system. The deep confined system has not been studied in any detail because there are only about five exploratory borings which currently are thought to penetrate the system. Available potential measurements come largely from zones within the Grande Ronde Formation. Hydraulic heads of aquifers below the Grand Ronde are known only from the work that was done in the Rattlesnake Hills No. 1 well.

Pressure measurements acquired from piezometers and from packer-isolated zones suggest decreasing heads with depth in this deep system, a phenomenon that promotes down-hole flow within the wells. Measurements further suggest that certain zones, such as the Umtanum Flow, constitute permeability barriers that greatly restrict the vertical movement of ground waters (Research and Engineering Division 1976).

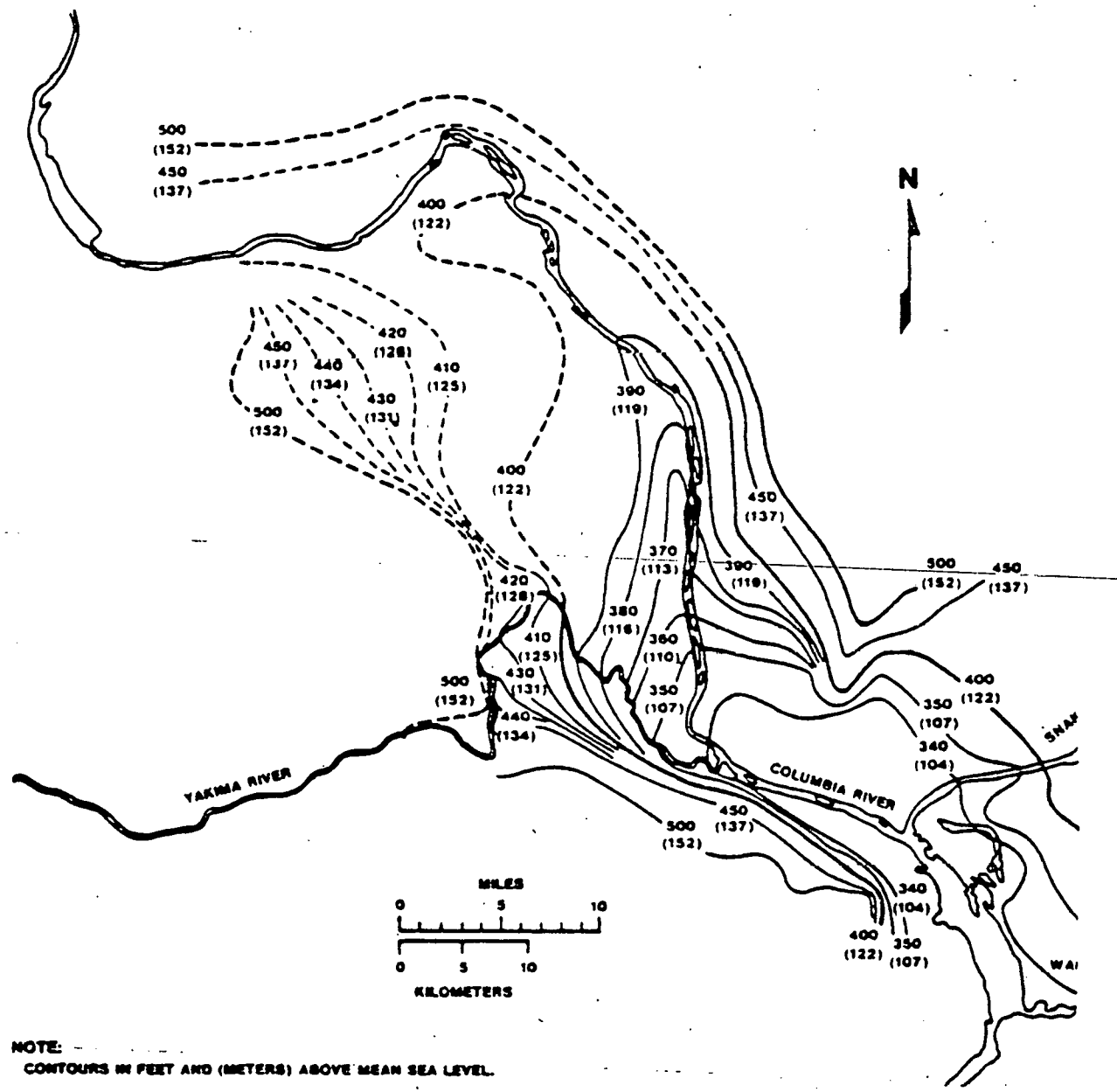


FIGURE 9. Pasco Basin Uppermost Confined-Aquifer Potential Map

Based upon some very limited measurements (Deju and Fecht 1979), it has been suggested that flow of individual zones in the deep system is from east to west with a regional westerly gradient of about 5 ft/mile. Recharge to this system is somewhat speculative, but we may assume that it derives from

rather distant sources, such as the Cascade Range, the Okanogan Highlands to the north, or the Rocky Mountain system to the northeast. Discharge from the system also is speculative. It is thought to occur by slow vertical movement of the ground water into overlying permeable materials with ultimate discharge into the lower Columbia system.

Piezometric measurements generally indicate declining heads with depth in the deep system. Some of the data, however, tend to be contradictory, or even unbelievable, and serve to emphasize the complexity of the problem. To illustrate, in the depth interval 8275 ft to 8351 ft in Rattlesnake Hills No. 1 well, the piezometric level was measured as 4691 ft below sea level. This, it can be assumed, is an erroneous measurement and that it represents some kind of nonequilibrium condition. Nevertheless, only additional information will wipe that figure from the record and provide insight into the shortcomings of the data-acquisition techniques. Again, some of the vertical-flow measurements that have recently been made occasionally indicate that uphole flow is occurring when piezometer data would require downhole flow. Such conflicting information does not make the hydrogeologist's job any easier.

Another apparent dichotomy is associated with the aquifer properties of the deep system. Generally, it has been assumed in the past that the deep aquifers in the Hanford area have low, or very low, transmissivities (Research and Engineering Division, 1976). However, recent development of highly productive wells in aquifers of the deep system in the east central Columbia Basin poses new questions relative to the Hanford area.

Until now the Pasco Basin hydrogeology has been treated as three systems (unconfined, uppermost confined, and deep confined), based upon stratigraphic, lithologic, and hydraulic considerations.

The ground-water hydrology can also be examined as a potential system in which permeability variations are reflected in modifications of ground-water flow patterns (Figure 14). In this instance the ground-water system will be viewed as a steady-state system in which the potential distribution is

controlled by the potential of the water table. In such a steady-state system, the stratigraphic and lithologic units are represented by variations in hydraulic conductivity within a hydraulic continuum. Such assumptions of a potential system are valid if the water table is in a relatively fixed position, at least over a reference period of years, or if the water table fluctuated only slightly, relative to the total saturated thickness. Such consideration of the ground-water system permits examination of basic circulation modes and facilitates the development of conceptual models and flow maps of the system.

Figure 10 illustrates the dominant upper, or local, conceptual flow system of the Hanford Site. The flow-system diagram illustrates the stream lines or flow lines of ground-water movement from recharge areas to the Columbia River discharge line (Deju and Fecht 1979). The influences of the potentials developed from natural recharge and recharge from waste-disposal areas are evident.

Figure 11 (Deju and Fecht 1979) is a conceptual diagram of flow patterns on a regional basis. Local flow systems are seen here to develop in response to some of the smaller topographic features and to discharge into the adjacent water course. An intermediate flow system is postulated to develop in the Cascade Range and to discharge into the Columbia River system. A regional flow system is indicated here to develop in the Rocky Mountain system and to flow in the direction of the lower Columbia drainage.

CONCLUSIONS

The brief summary of the geology and hydrology of the Pasco Basin, presented herein, could easily be expanded into many volumes. Time and space limitations precluded consideration of any details and the magnitude of the subject has required rapid transition from one topic to another. It is hoped that the presentation of the subject matter has been sufficiently integrated to permit the reader to adequately relate to the waste-release scenarios.

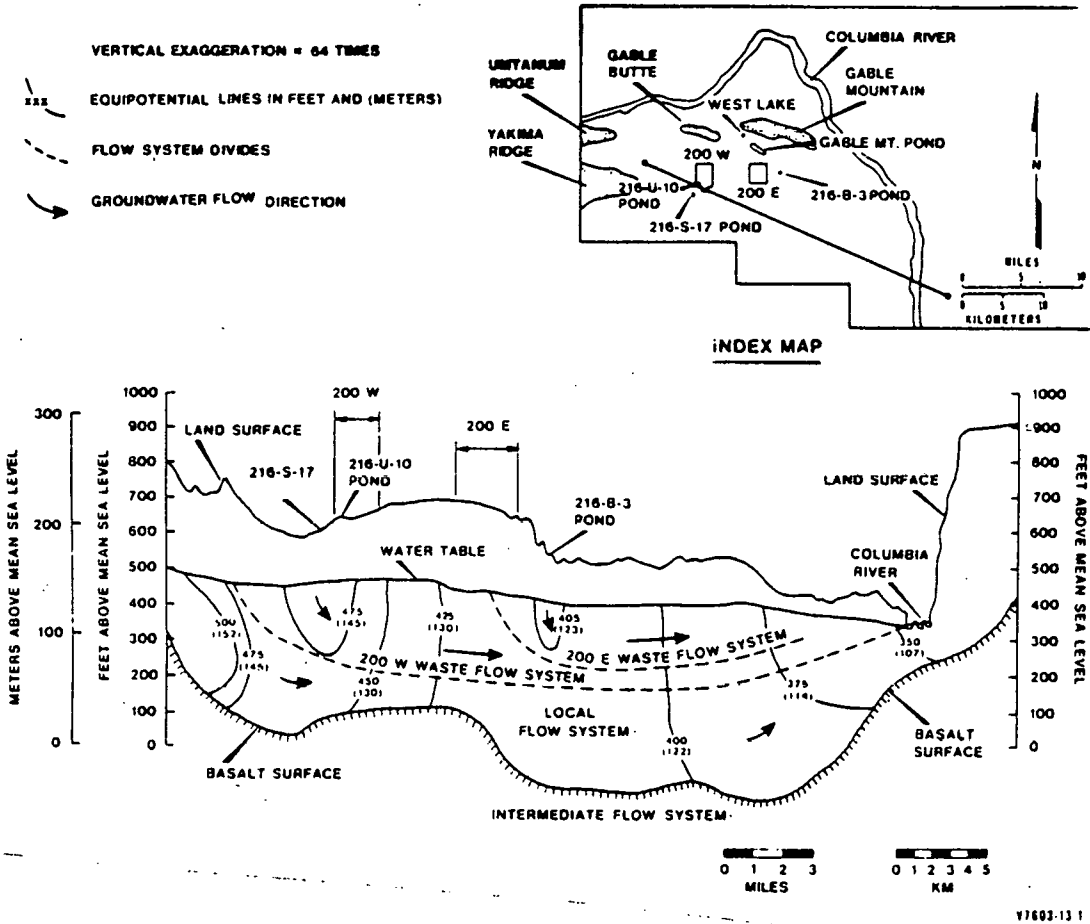


FIGURE 10. Conceptual Flow System for the Unconfined Aquifer System in the Pasco Basin (Deju and Fecht 1979)

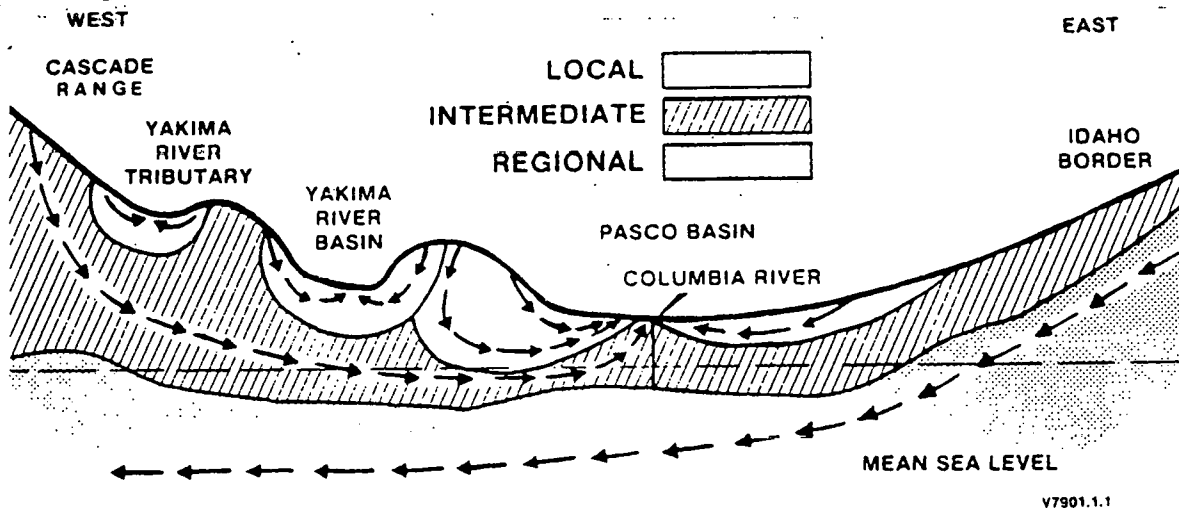


FIGURE 11. Conceptual Subsurface Flow Systems in the Pasco Basin (Deju and Fecht 1979)

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QUESTIONS AND ANSWERS

- Q. Where would the 3000 ft depth or so fall on this?
- A. The 3000 ft depth would fall near the top of the Grand Ronde formation. This particular section was chosen for display because there has been a recent change in stratigraphic nomenclature. Until recently we referred to the lower, middle, and upper Yakima Basalts. Recent work by Don Swanson and his associates has provided us with a new stratigraphic nomenclature. This particular slide shows both the old and the new nomenclature.
- Q. Out of curiosity, what's the source of the tuff layer there at about the middle of the chart?
- A. The tuff layers are generally associated with explosive events (probably Cascadian) that occurred during the eruption of the lavas. They occur in localized accumulations and in rather extensive sheets in various places. If there is a specific source for that layer, I'm not aware of what it is.
- Q. How many borings contribute to this map?
- A. A couple hundred borings probably contribute to this map. When we get down into the deep confined system we're limited to a handful.
- Q. Could you give an indication of a depth scale there, please?
- A. For the local flow system, here, we might be talking about 100 to 300 ft. The intermediate flow system might carry us down several thousands of feet. The deep flow system is a very deep regional system in which the flow would take place predominantly in the lower basalts.
- Q. In which of the systems would your 3000-ft system land?
- A. The 3000-ft depth would probably land within the intermediate flow system.

- Q. Does that intermediate system correspond to the upper confined system that you described?
- A. The intermediate system would be largely in the confined system below the Ringold and in the uppermost basalts.
- Q. I think we more or less contradicted ourselves there. If we're talking about 1000 ft for the intermediate system and the question was asked for a 3000 ft repository depth, this would put us into the regional system rather than the intermediate system.
- A. This is right. I hesitate to put real limits on these things because you have to recognize they are purely conceptual. I say 1000 ft. Actually, what we're talking about is a big gap in our knowledge. We may be dealing with the zone down here at the repository level that could be in either the intermediate or the deep system.
- Q. I think that the main point here is that this is a conceptualization and the regional system is not well enough understood at this point to say positively where it's recharging or where it's discharging.
- A. That's why I hesitate to put real numbers on the question.
- Q. Does your deep indicated movement from east to west lay mainly on the Rattlesnake Hills data?
- A. This is not my own concept here, but a concept developed by Deju and Fecht (1979). The flow in the deep system must be considered to be a pure conceptual system based upon minimal measurements.
- Q. And isn't it based on those deep measurements of the Rattlesnake Hills?
- A. I would certainly assume that's the case.

- Q. Based on the regional hydrology of the eastern plateau developed by the Department of Natural Resources in Olympia, their maps would indicate that the deeper aquifers may have been recharged by the last glacial floods. We seem to see water divide south of the Spokane River with a gradient toward the Spokane River and then a gradient from the divide down across the entire Columbia Plateau centering on the Pasco Basin and the Othello area. Until we get more information, I don't think that we can really say that we fully understand the regional picture. However, that data from the DNR in Olympia does support this concept of the deeper confined system, particularly when you look at the data from the wells that have been drilled through the Umtanum Basalt itself, which indicate that there is a substantial hydraulic separation between that basalt layer and rocks below the basalt layer.
- A. Yes, I certainly concur with that. I think that the information that we have accumulated on the east central basin hydrostratigraphy from geophysical logging of basin wells will support that 100 percent. Here again, I hate to put myself on the spot, saying some of these irrigation wells in the 1500- and 2000-ft range are representative of the deep system or the intermediate system. I don't think we really know.
- Q. I just want to clarify a couple points. One is the question of declining head with depth. We have had some earlier measurements that were made by various investigators using various techniques. Declining head was indicated as a possibility. Right now we have conflicting data with that. We don't know enough about the deeper system yet. We will be addressing that question in the next fiscal year in detail. I don't want to leave in the minds of the people here that the concept is something that's definite--that we do have a declining head with depth. We don't know that at this point. We suspect that it is not true.
- A. Right. That is why I brought up the fact that we do have some conflict here.

- Q. The other thing is that with the piezometers we have some definite data problems there too. We have discovered recently that some of our piezometers are obviously connected; we don't have the separation that was assumed for a long period of time. We're doing a complete re-evaluation of the pre-existing data.
- A. I understand this. It's just another one of those things about which we're trying to gain additional information. A lot of erroneous measurements have been made in the past when we didn't know as much as we do now and perhaps resulted in some misinterpretations.
- Q. Do you have any dates on these? I think this would solve some of the problems.
- A. Yes, there are radioactive carbon dates and some Carbon-14 data on the system. In general, we know that the very shallow system contains relatively recent ground water; whereas if you get down to the deep zone, or even the intermediate zone, you get beyond the dating capability of the Carbon-14 method.
- Q. Are there artesian wells in the deep zone?
- A. I think we can probably say that there are, yes.
- Q. Flowing to the surface?
- Q. In the deep system?
- A. I think that there are some flowing artesian wells that tap the intermediate system.
- Q. Where are the Carbon-14 data published, or where are they available? Maybe somebody else here would know?

- A. The Carbon-14 data are available in reports coming from the reservation, part of the RHO series, or the ARH series, I think. I guess they're in the Atlantic Richfield series, work done by people in the Geological Survey that has been published.
- Q. As I recall, there was a lot of question about the validity of those dates.
- A. Yes, that's true. In some of the radioactive dating that was done in connection with ARH-DC-1, there was a lot of question about contamination. I think some of the dates may be fairly well accepted now as being realistic. I'm not sure if any of the better dates are from ARH-DC-1 though.
- Q. The first data were published by United States Geological Survey; I'm filing a report on that.
- A. Could I interject for just a moment. We're starting to run behind a little bit. This is a good discussion and I hate to break it off, but I also know Rockwell is currently working on a report from the new information they're getting. That may shed some more light on the subject. I really think we should get going.
- Q. May I ask a question about the stratigraphy? The base of the basalt sequence lies on what?
- A. The base of the basalt sequence? I think we have to say that, honestly, we don't know, but some of the magneto-telluric measurements, as I have suggested, now indicate that there may be a sedimentary sequence there about 3 or 4000 ft thick which, in turn, would overlay the basement granitics and metamorphics.

Q. I see. And the nature of the basement is unknown?

A. It's speculated specifically from the surrounding mountains. Nobody knows the basement from the drill holes. It's never been penetrated except outside the Pasco Basin.

Q. Is there any idea of the age of those sediments?

A. Pre-basalt. That's all we can say.

Q. Could you tell us something about how this information is being used for scenario development analysis?

A. I think that question is a little bit premature. We're now just really trying to put it together. I don't think that on the conceptual basis it's been thoroughly incorporated into the scenarios.

EXTENT OF PREVIOUS GLACIATIONS

Dr. Colin Bull

In 1959, we were aware that there had been a sequence of glaciations, during the previous million years or so, centered in the Hudson Bay area of Canada, which had extended south to cover a fair part of Ohio, Indiana, Illinois, and so on. The last distinct glacial stage, the Wisconsinan, reached its maximum extent in this part of the Midwest at about 18,000 BP. Earlier glaciations had been more extensive in some areas; for example, the Kansan stage extended well into Kansas.

In the Pacific Northwest, the largest extent of the glaciers at 18,000 BP was from the Cordilleran Icesheet. Glacier lobes extended south of the Canadian-Washington border. Mountain glaciers, according to John Hollin (Figure 1, Hollin's CLIMAP group), were more extensive at their maximum than now. However, even at the maximum extent they were well away from Richland and the Pasco Basin.

For the WISAP Tasks we need to look at several features associated with glaciers. To start with, what are likely to be the effects of the glacier load on the hydrological parameters of an area. This was referred to extensively in Zellmer's talk and I will not deal with that more, except to say that we must look at the probability that a particular area will be covered by glaciers. We must also look at the change that there will be in the hydrologic regime resulting from a number of indirect effects, for example, the climatic changes associated with that glaciation. Furthermore, we must look at the probability that the glacial erosion will exhume radioactive material from a repository. Some general information is available on the kind of erosion associated with a major glaciation of this kind. Features like the Great Lakes, and the Finger Lakes in New York, may be associated with major erosion by continental glaciations or by lobes from those continental glaciers.

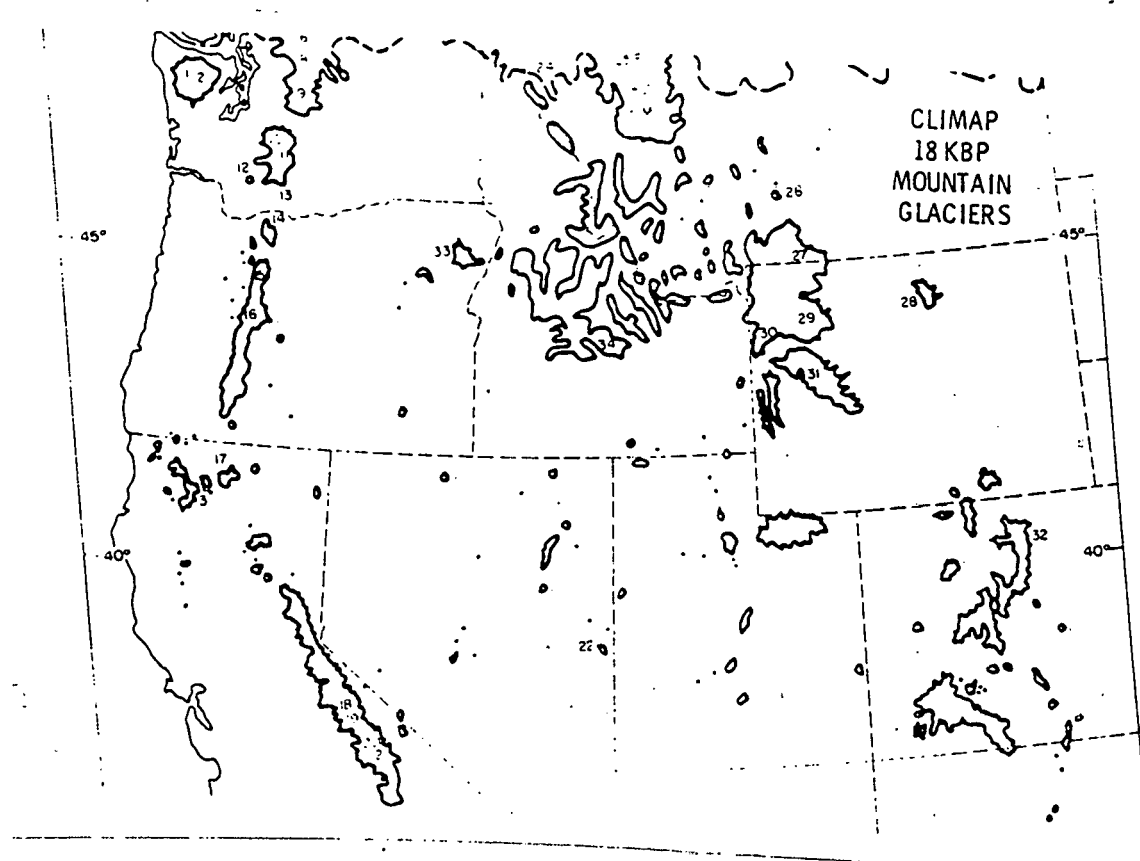


FIGURE 1. Maximum Extent of Glacier Lobes from Cordilleran Ice Sheet and of Mountain Glaciers in Western USA. (Map by John Hollin, CLIMAP Group)

A map of the Laurentide Ice Sheet, from Sugden (1973) shows a center near Hudson's Bay with an elevation of the order of 3400 meters. The CLIMAP Group (Denton and Hughes 1981) have mapped the extent of glaciers in the last glaciation, giving two possible reconstructions. The overall extent of glaciers in the Northern Hemisphere are taken to be at their maximum about 18,000 years BP, although all of these glacier masses individually did not reach their maxima at the same time. The CLIMAP "maximum reconstruction", (Figure 2) shows surface elevations at the center of the Laurentide Ice Sheet exceeding 3800 meters.

EXTENT OF CORDILLERAN ICE SHEET

The CLIMAP "maximum reconstruction" (Figure 3) ice thicknesses calculation shows that in the Cordilleran region there was an intermountain

FIGURE 2. CLIMAP Maximum Reconstruction 18,000 BP Surface Elevations
(from Denton and Hughes 1981)



FIG 2 BULL pag 30

FIGURE 3. CLIMAP Maximum Reconstruction 18,000 BP Ice Thickness
(from Denton and Hughes 1981)

ice sheet, in British Columbia, with lobes extending to the south. The existence of that ice sheet as a coherent mass further north is still very much open to doubt.

In the CLIMAP "minimum likely reconstruction" of the North American glaciation at 18,000 BP (Figure 4), the surface thickness shown in the Cordilleran ice sheet is unknown. Denton and Hughes (1981), who have compiled most of this data, are not certain of the thickness of ice in the center, although they know fairly closely the southern extent of it.

Rates of Advance and Retreat at the Edge of Ice Sheets

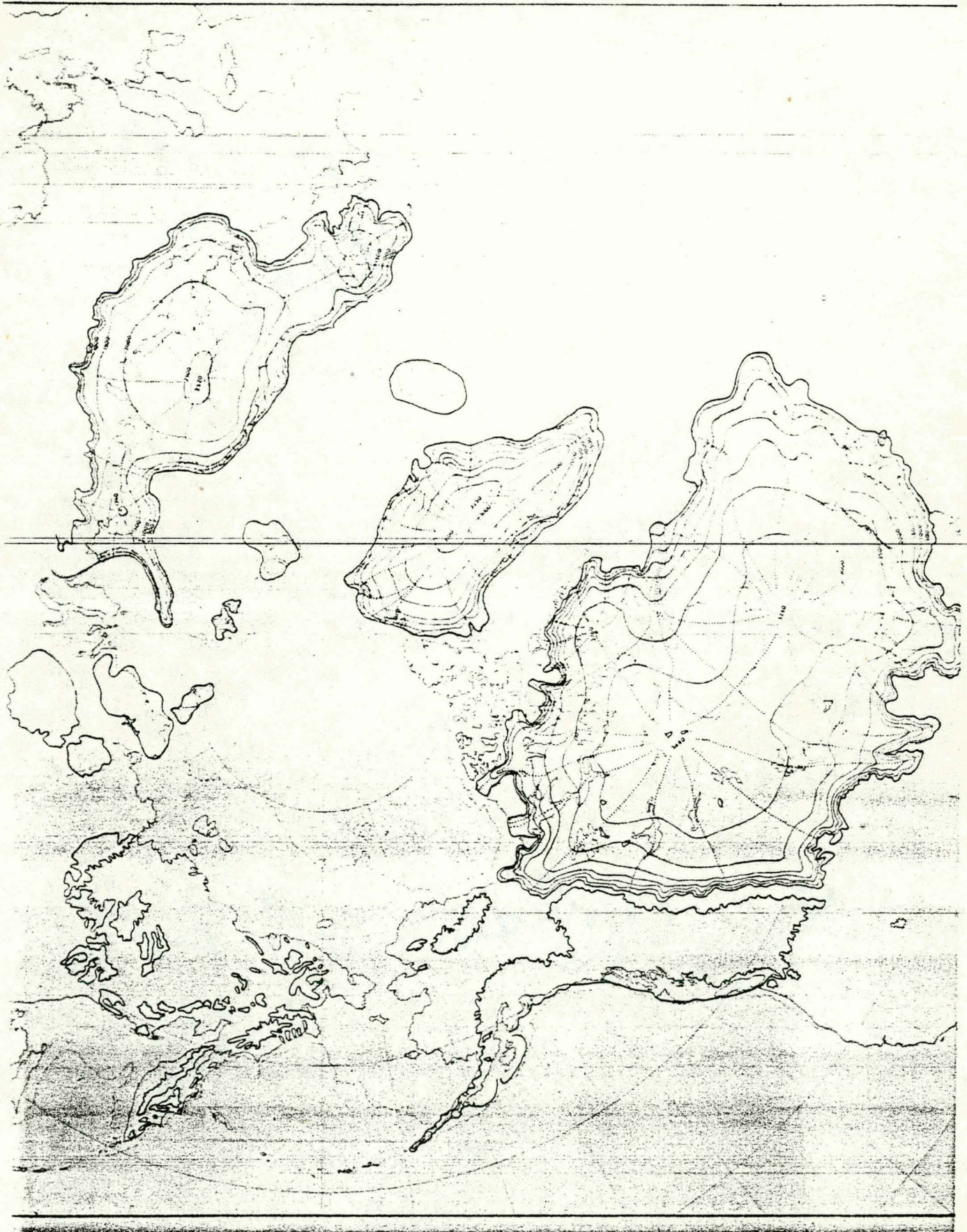
Andrew's (1973), in a map of the deglaciation isochrons of the Laurentide (Figure 6) Ice Sheet gives a maximum of the order of 18,000 years BP. Except for some little ice caps in northern Canada, the ice sheet had entirely disappeared by between 8000 BP and 7500 BP.

The WISAP modelers need information on the rates of advance and retreat of glaciers, of both the continental-glacier margin and the alpine ice-glacier advances. Prest's map (1969) (Figure 5) of the deglaciation isochrons of the whole of North America shows the 18,000 year BP limit, marking the maximum glaciation limit in most parts of the Laurentide Ice Sheet.

However, in the Pacific Northwest the maximum was possibly reached at 14,000 BP, not 18,000 BP. Certainly, in the southwest of the area, the maximum was much closer to 14,000 BP than to 18,000 years BP.

Prest's map (1969) of the Pacific Northwest shows the 14,000 year BP glacier, among others (Figure 6). According to this reconstruction, by 9000 BP there was an ice-free corridor between the Laurentide Ice Sheet and the Cordilleran Ice Sheet. If these isochrons are reliable, one can gain some information on the rate of retreat of the continental glaciers. In general it is not easy to gain very good information on the rates of advance, for several reasons, but particularly because advancing glaciers tend to destroy evidence of the previous extent. I have been forced into assuming that the rate of advance is more or less the same as the rate of retreat. However, Prest's map

FIGURE 4. CLIMAP Minimum Reconstruction 18,000 BP Ice Thickness
(from Denton and Hughes 1981)

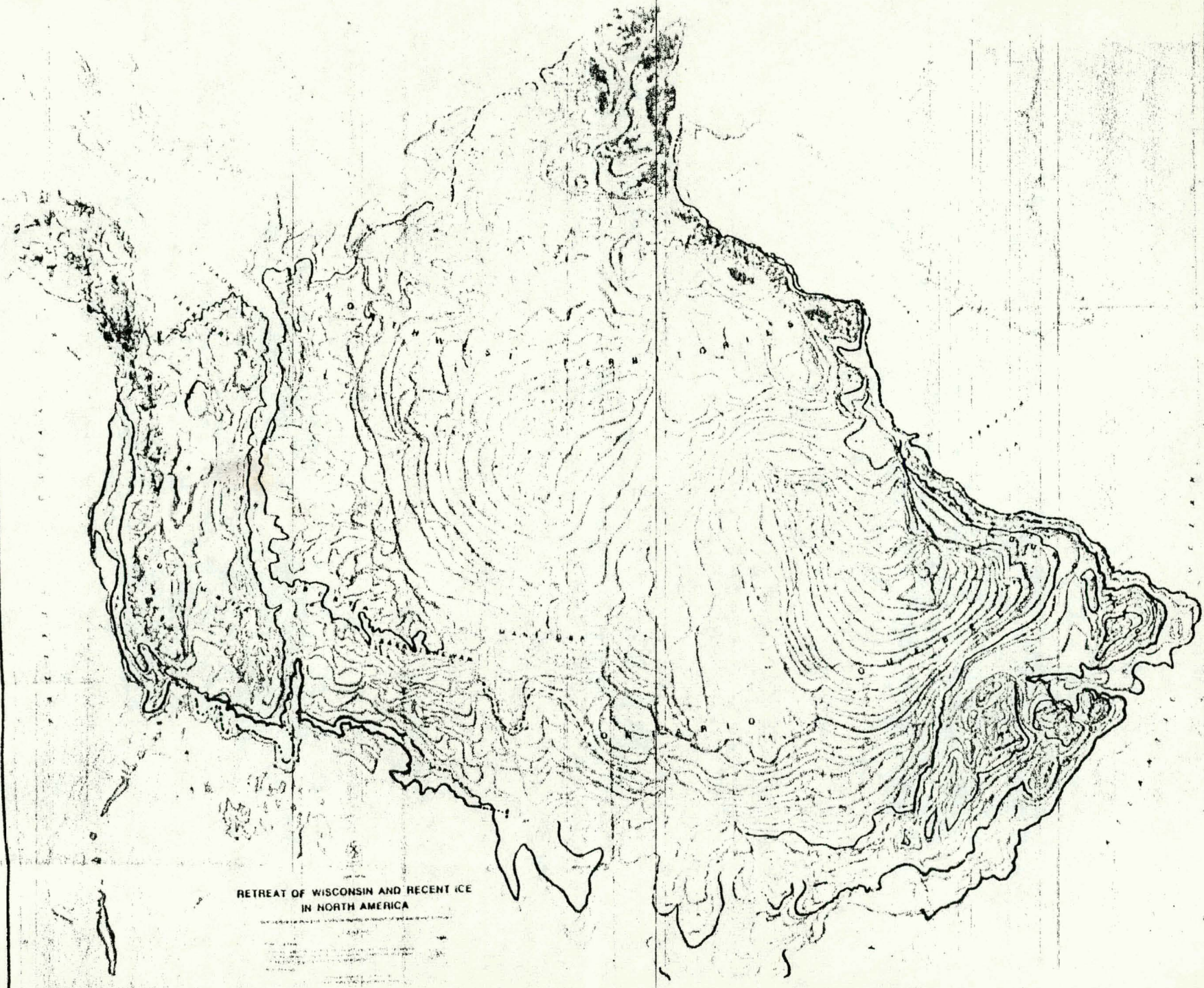


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FIGURE 5. Deglaciation Isochrons of the Laurentide and
Cordilleran Ice Sheets (from Prest 1969.



RETREAT OF WISCONSIN AND RECENT ICE
IN NORTH AMERICA

FIG. 5 - Bull

pg ~~33~~ 34

~ 75%

FIGURE 6. Deglaciation Isochrons and Extent of Glaciers in the Pacific Northwest (from Prest 1969)



Fig. B - Bull

pg. ~~35~~ 35

~ 7570

(1969) suggests that in the Pacific Northwest, rates of retreat of a couple of hundred meters a year were possible during most of the deglaciation phase, between 14,000 BP and 9000 BP.

In a few places, we have more specific information on the rates of change of the glacier margins (Kukla, Matthews and Mitchell 1972; Figure 7). Over a 2000-year period in southern Sweden, the ice sheet margin fluctuated by up to 300 km in a hundred years (Kukla, Matthews and Mitchell 1972). Once or twice during this period of deglaciation at the end of the Wisconsin, change rates of the margin were greater than this.

In the Bergen area of Norway (Mangerud 1970; Figure 8) one sequence is moderately well worked out, with variations in ice-margin position of 150 kilometers in a period 200 to 300 years, based largely on C¹⁴ dates.

But in various areas, one does have to crowd a lot of retreats and advances of glacier margins into a relatively short period (Figure 9). Excellent work has been done by Frye and Willman (1973) on the Lake Michigan lobe. The dating of the various positions of those lobes, over the period from 14,000 to 22,000 years BP, give velocities of retreat and advance, sometimes on the order of 300 meters a year (Figure 10).

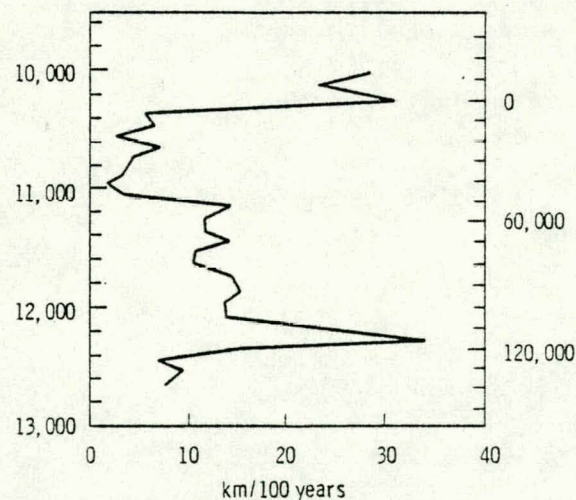


FIGURE 7. Time-Distance Plot for Glacier Margins in Southern Sweden (from Kukla, Matthews and Mitchell 1972)

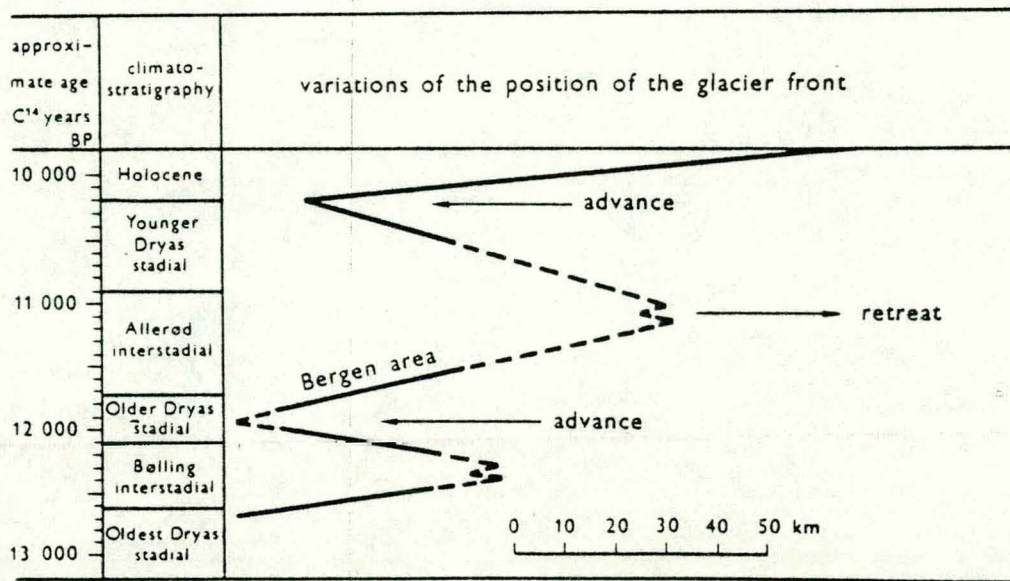


FIGURE 8. Time-Distance Plot for Glacier Margin, Near Bergen, Norway. (from Mangerud 1970)

The margin of the Cordilleran Ice Sheet is only a couple of hundred kilometers north of the Richland site. We do not have very much information on what is likely to have been the major cause of those large glaciations, and must make some allowances for the possibility that in the next glaciation the glaciers may advance considerably further south than they did in 18,000 to 14,000 BP.

HISTORY OF CLIMATIC CHANGE

We need to consider the history of climate change. Reference should be made to Kukla and Berger (1979) as well. Two of the many sets of data on temperature variations come from the Central Pacific and the Caribbean (Emiliani 1966; Figure 11). From the $\delta^{18}\text{O}$ record, the amount of ice can be calculated, and hence the sea level change, and the temperature changes associated with it. The last maximum sea level was on order of 130,000 years ago; the last maximum glaciation, with maximum sea drop, was perhaps 25,000 years ago.

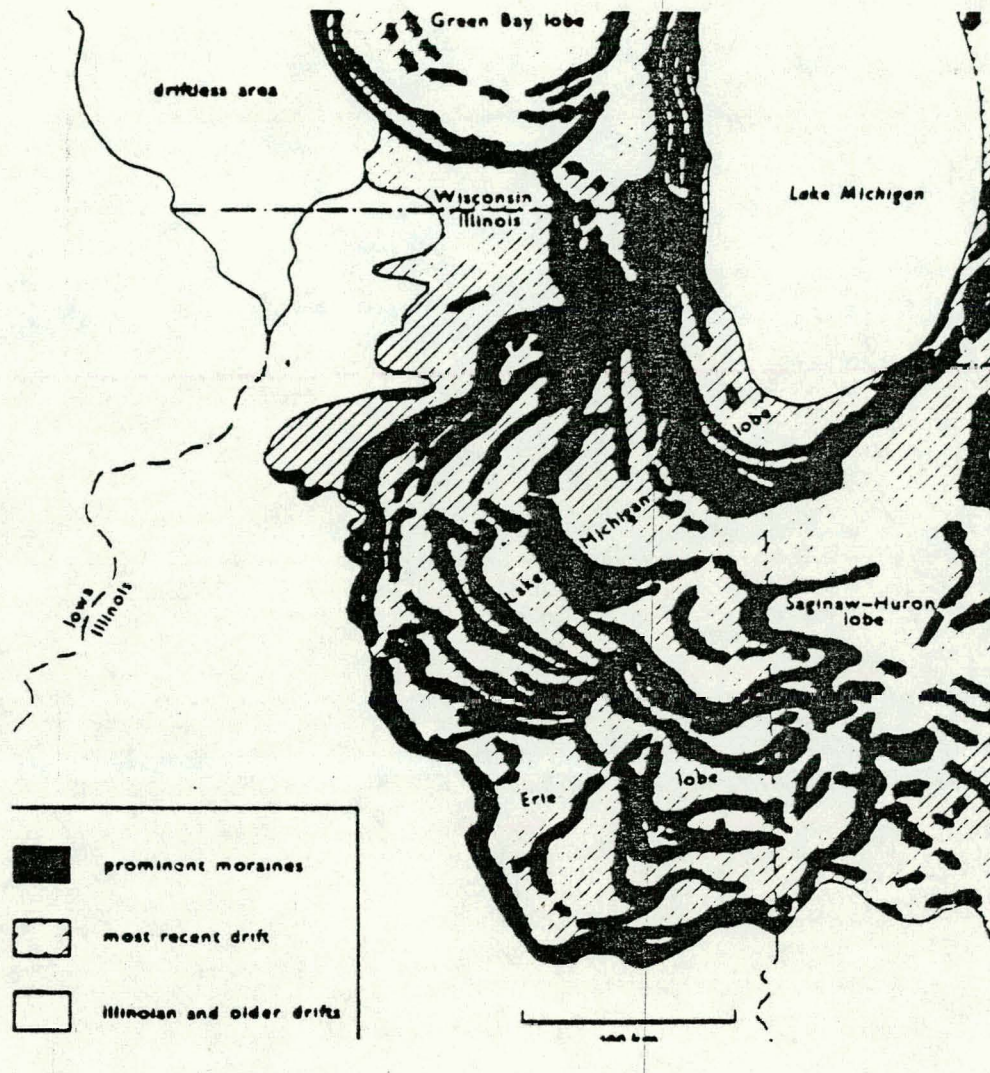


FIGURE 9. The Morainic Loops of the Wisconsin "Lake Michigan Lobe".

Over a longer period, two Central Pacific cores give the best data (Shackleton and Opdyke 1973, Figure 12). One can pick out the Sangamon interglacial of 130,000 years ago, and before that there are many other discernible interglacials. We include 7 or 10 major glaciations, with a periodicity on the order of 100,000 years, over the last 700,000 to a million years. The interglacials, by and large, have lasted something on the order of

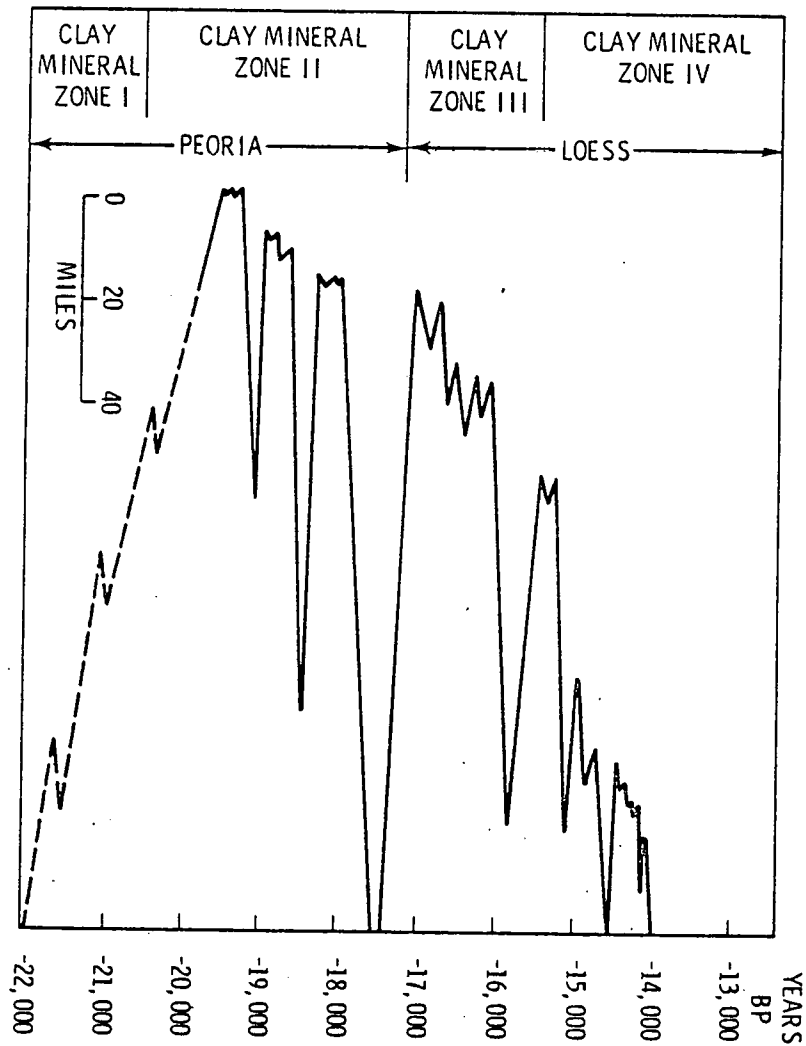


FIGURE 10. Fluctuations of "The Michigan Lobe"

10,000 years. Our current interglacial has lasted 10,000 years so far, and assuming there is no such thing as "human mediation," maybe our next glaciation will be starting soon.

Whillans (Bull, Hughes and Whillans 1981) has used the same data in another way (Figure 13). Starting with $\delta^{18}\text{O}$ profiles from the Pacific cores, and assuming a relationship between the volume of ice in the Laurentide Ice Sheet and the volume in all the other ice sheets that existed at the time,

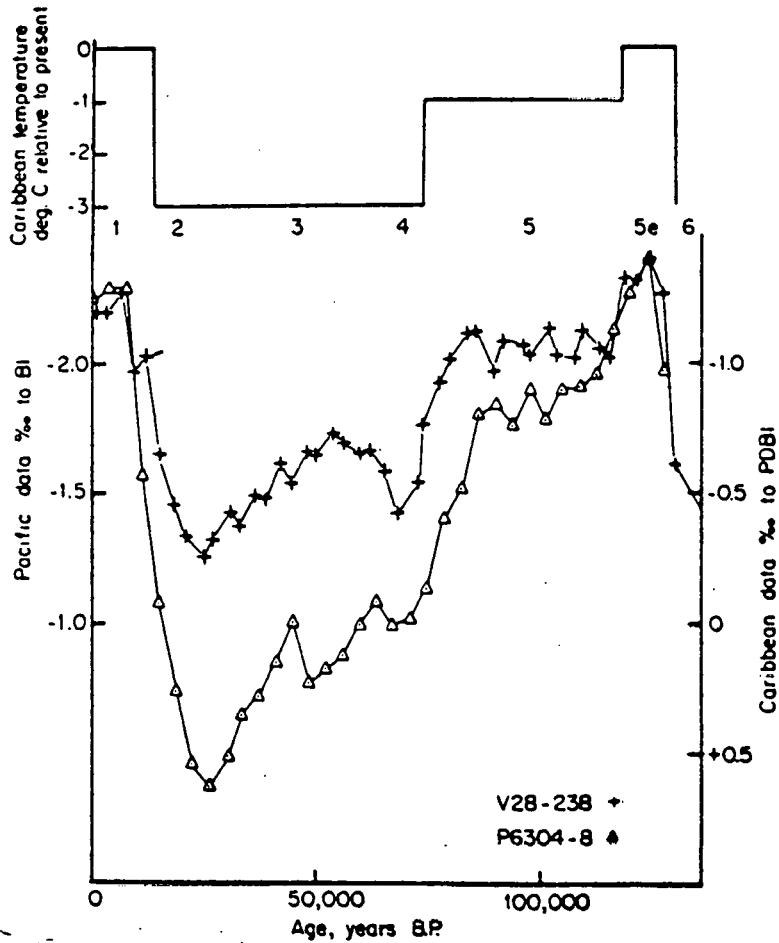


FIGURE 11. $\delta^{18}\text{O}$ and Temperature Records from Caribbean and Pacific (Emiliani 1966)

we can calculate the Laurentide Ice Sheet volume. Then, assuming an equilibrium form for the ice sheet we can calculate its radius, and thus the velocity of the ice sheet margin. The values obtained are on the order of 100 to 200 m a year maximum, which do seem to be consistent with the more sparse direct observations that we have on the glacier advance and retreat records.

Alpine Glaciers and Glacier Surges

We also need to be concerned with the effects of alpine glacier changes. Two pictures (Figure 14) illustrate the changes in a glacier in the Alps between 1867 and 1966 (Lamb 1977). Although the Pasco Basin is distant from the alpine glaciers in the Cascades and the Rockies, the possibility of their extensive advance needs to be considered.

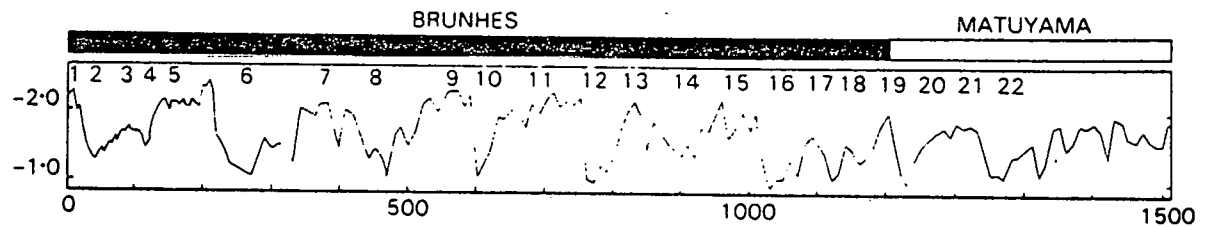


FIGURE 12. Core V-28-238 from the Solomon Plateau (3120 m depth), Pacific Ocean. Oxygen isotope stages 1-22. Core depth (base in cm) δ deviation, per mil, from Emiliani B1 standard. This core has been proposed as a standard for the later Pleistocene. Brunhes-Matuyama reversal (700,000 years BP) occurs in stage 19 and is used as the basis for dating stage boundaries. (From Shackleton and Opdyke 1973)

We need also to take account of particular events that may occur in glaciers, particularly, the surging of glaciers. Some glaciers seem to be unstable, for reasons that we now think we are beginning to understand. After a period of relative stability, the glacier can suddenly advance rapidly down the valley. Some mean annual velocities of the advance of glaciers (Figure 15) show values varying from 2.3 km/yr up to 6.5 km/yr (Sugden and John 1976). Hollin reports that the fastest surge measured so far is probably that of the Kutiak glacier in India, at 360 ft. a day.

Microparticles and Paleotemperatures

Through the ice sheets of Greenland and the Antarctic, we now have complete ice cores, in Greenland at Camp Century and in west Antarctica at Byrd Station. In east Antarctica, at Dome C, a core has been drilled by the French, 900 meters long, in an area of great significance because it is likely to contain some of the oldest ice in the world. An early plot of the variation of the $\delta^{18}O$ values (which is a surrogate measure of ancient temperatures) for the Greenland Ice Sheet, is tagged to climatic variations and glacial events in Europe and in North America (Figure 16) (Sugden and John 1976). The temperature has stayed more or less constant over the last 10,000 years, but before that, there was a considerable temperature change, between about 12,000 years BP and about 10,000 years BP. During the Wisconsinan, the temperature also seemed to have stayed moderately constant,

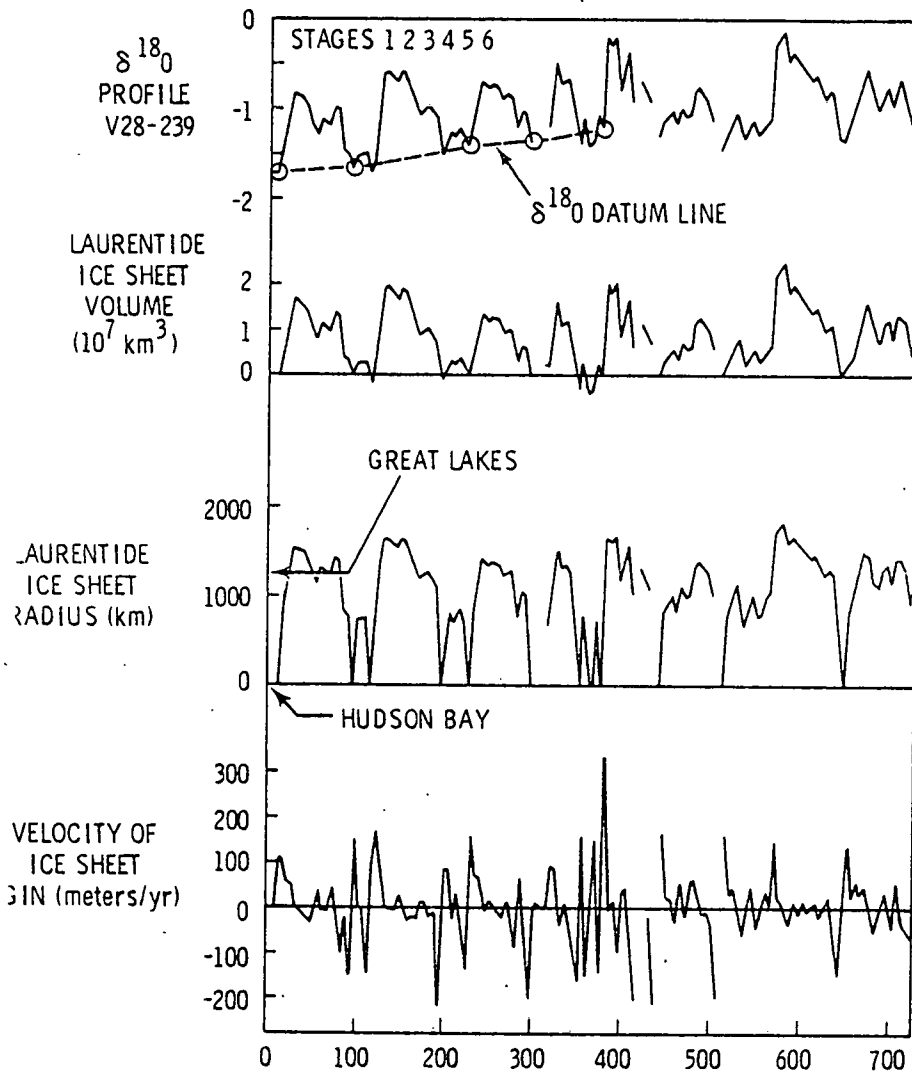
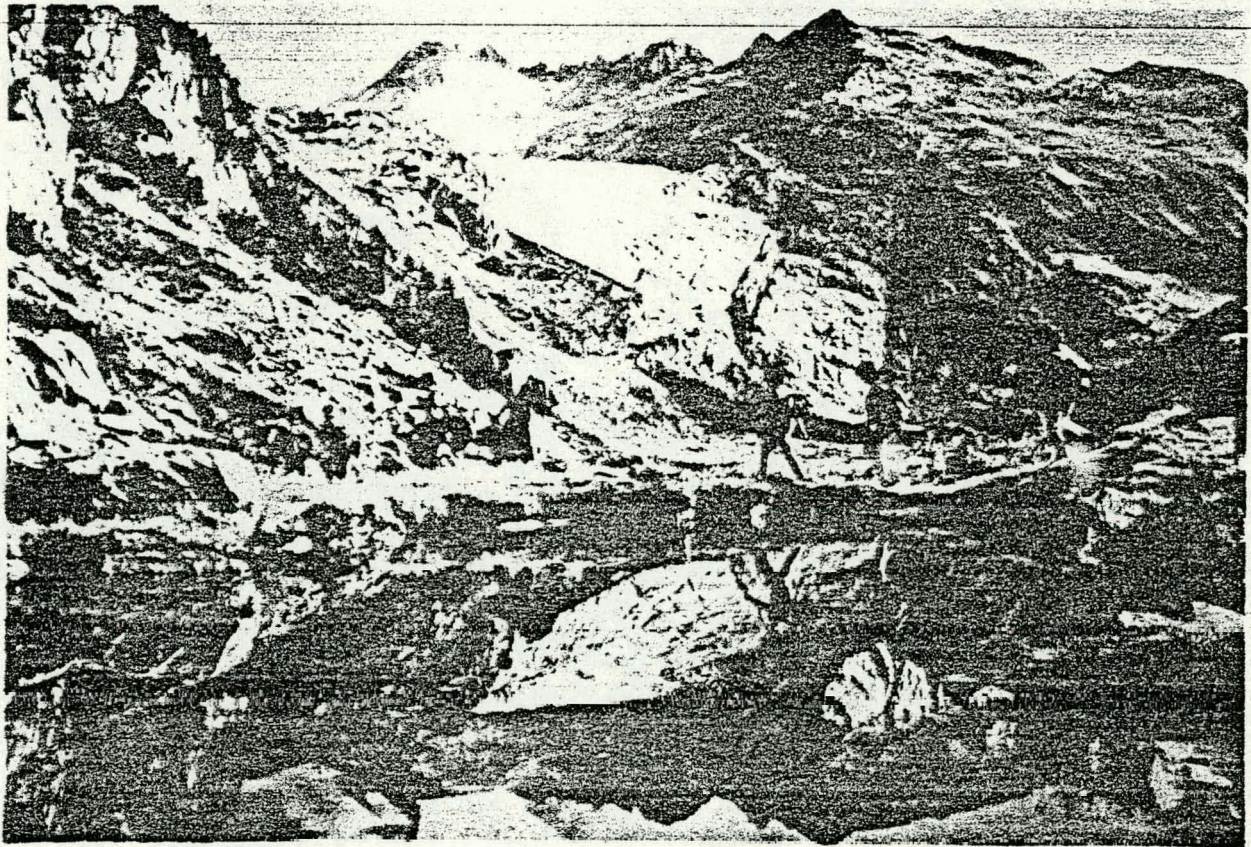
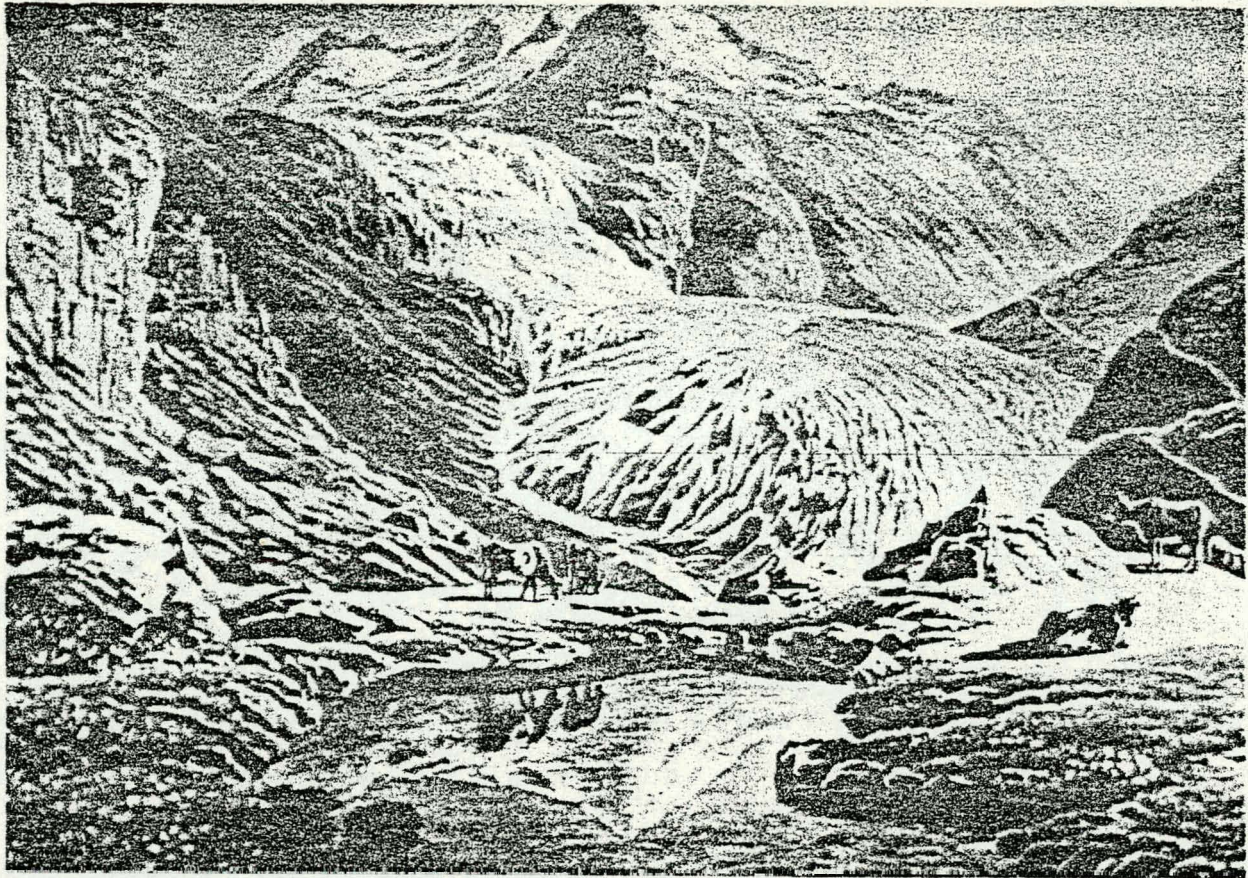


FIGURE 13. Calculation of Rate of Variation of Edge of Laurentide Ice Sheet, from $\delta^{18}O$ Profile of V-28-239 (Shackleton and Opdyke 1973)

though with pretty large variations at some times. In this plot the $\delta^{18}O$ values, at the bottom of the ice sheet return to approximately the same values as at present, and could correspond to the last interglacial. The age of this ice has been estimated to be 60,000 to 70,000 years BP, which could be the onset of the last part of the Wisconsin stage. However, the lowest 10,000 years is represented by only a meter or so of ice in the Camp Century core. An early plot of the $\delta^{18}O$ values from Byrd Station, west Antarctica

FIGURE 14. Alpine Glacier in 1867 and 1966 (from Lamb 1977)



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~ 75%

Glacier	Total area (km ²)	Total length (km)	Length of surging part (km)	Average slope, surging part	Cycling period (yrs)	Duration of active phase	Maximum annual velocity (km/yr)	Maximum displacement
Bering, Alaska	5800	200	153	0.7	30-15	3 yrs	—	9.7 km
Klutian, Yukon	1072	55	40	1.3	30-10	3 yrs	3.2	6.5 km
Walsh, Alaska	830	89	86	1.0	50-10	4 yrs	5.6	11.5 km
Muldrow, Alaska	393	63	46	2.2	50-10	2 yrs	6.6	6.6 km
Variigated, Alaska	49	20	19	4.2	20-0.5	2 yrs	5.0	5.0 km
Tikke, British Columbia	75	19	18	3.6	20-1	3 yrs	1.0	2.0 km

FIGURE 15. Characteristics of Some Surging Glaciers (After Meier and Post 1969)

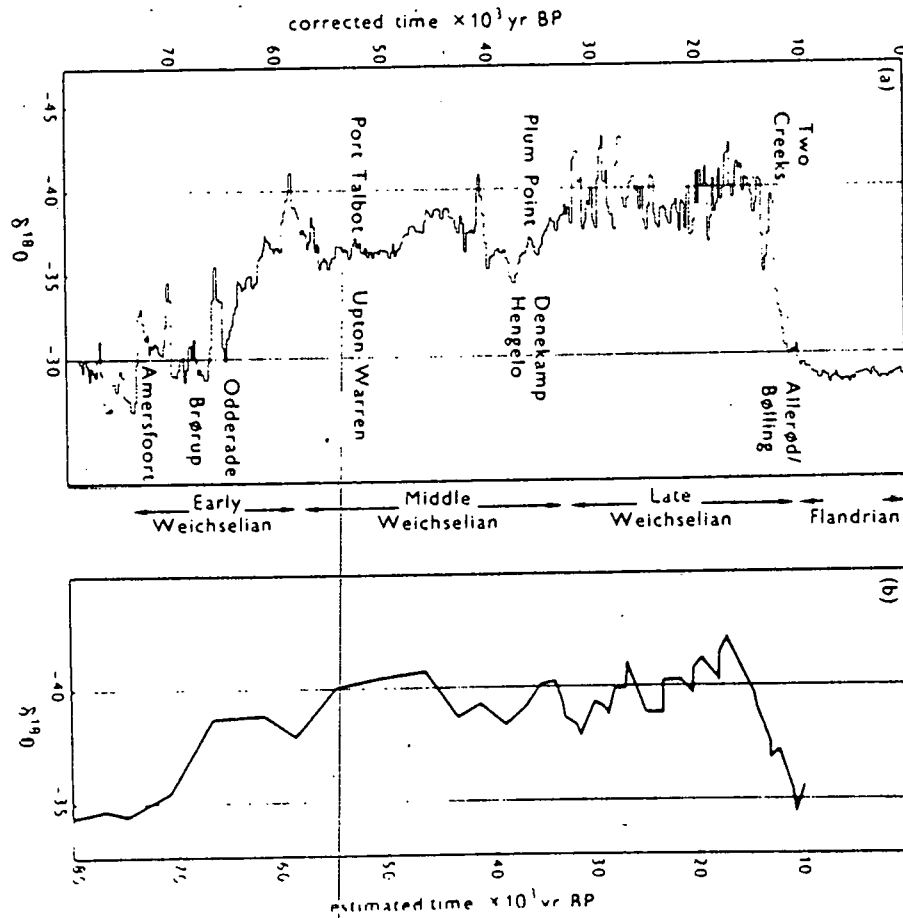


FIGURE 16. Early Plot of $\delta^{18}O$ Variations in Ice Caves from Camp Century, Greenland, and Byrd Station, Antarctica (from Sugden and John 1976)

is similar. The dating of the bottom of the core is not particularly reliable, which is why it is of particular importance to drill at Dome C in east Antarctica where we are sure that the ice is much older.

At the Institute of Polar Studies we are comparing these $\delta^{18}\text{O}$ values, with the microparticle content of these ice cores. The profile of $\delta^{18}\text{O}$ along the core from Byrd Station shows that values have stayed moderately constant down to about 1100 meters, corresponding to about 10,000 to 11,000 years, with more negative values for the Wisconsinan ice. One measure of the dirtiness of that ice is a plot of the number of the microparticles here in the size range from 0.65 microns and 0.82 microns, in unit volume of the ice. Figure 17 shows a clear relationship between coldness and dirtiness (E. Thompson 1978).

The same relationship exists for the Century Station core: when it was colder, it was dirtier. Or maybe, when it was dirtier, it was colder (Figure 18; E. Thompson 1978).

At Dome C, in East Antarctica, the annual accumulation is very low. The French have determined $\delta^{18}\text{O}$ values for the 900-meter core. Again there is a sharp change at the Holocene/Wisconsinan transition. The variation of total particles per unit volume shows an equally pronounced change: when it was colder, it was dirtier, or when it was dirtier, it was colder (E. Thompson 1978; Figure 19). The change is very pronounced. L. Thompson has looked very closely at this core with sampling at 1 cm or less intervals. He found that with few exceptions, all of the Wisconsinan samples are dirtier than all of the Holocene samples.

The Thompsons have now started to analyze some of the microparticles (Figure 20). The elemental analyses, using the X-ray dispersive system of a SEM, are not particularly reliable, but using morphology as well, E. and L. Thompson can distinguish between volcanic and nonvolcanic particles. In the Dome C core, and in the Byrd core, almost all of the extra microparticles associated with the Wisconsinan ice appear to be volcanic particles. In the Greenland core, where the Wisconsinan ice is 50 times as dirty as the Holocene ice, most of the additional particles seem to be of terrestrial origin. It is

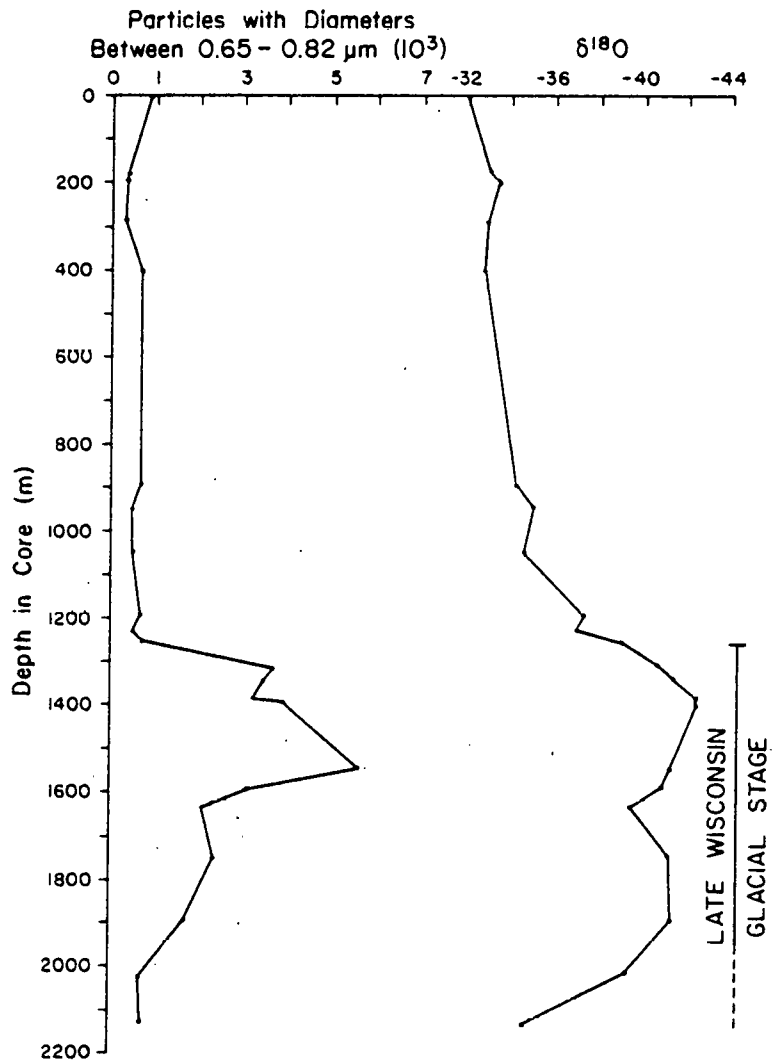


FIGURE 17. Plots of $\delta^{18}O$ and Microparticle Content Profiles for Ice Core from Byrd Station, Antarctica (from E. Thompson 1978)

largely loess blown from the Midwest plains. But in the Antarctica, where there are no local sources of wind-blown dirt, much of the additional material does seem to be volcanic.

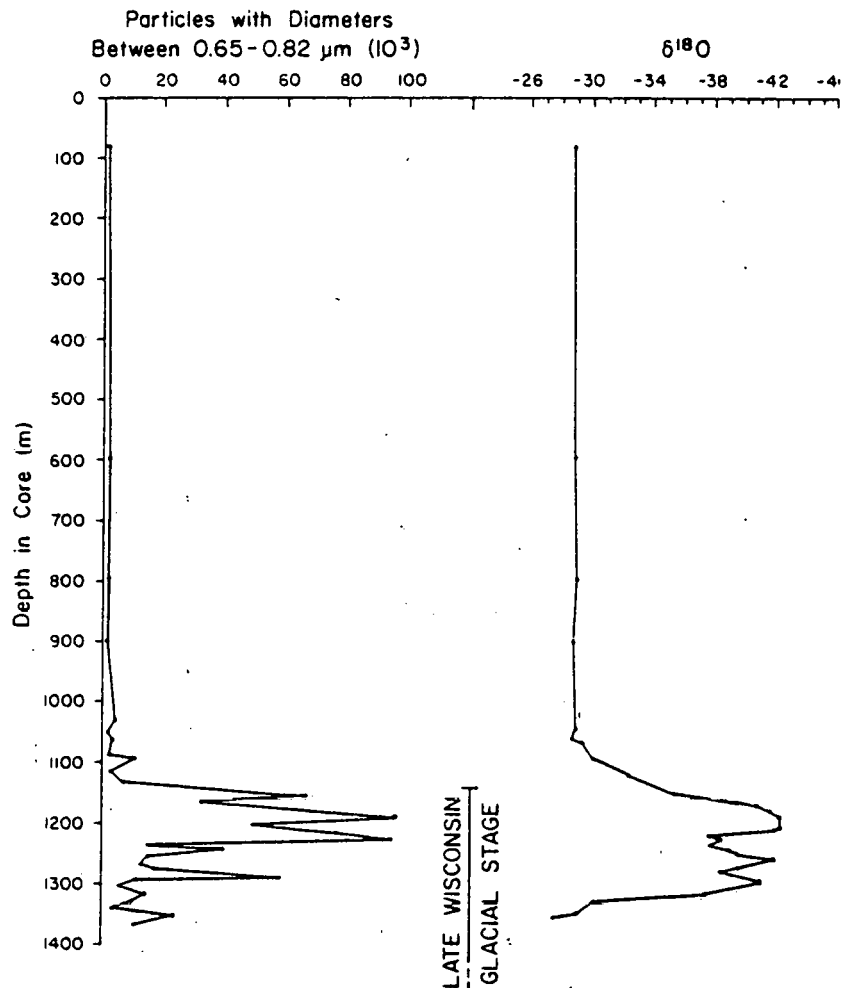


FIGURE 18. Plots of $\delta^{18}\text{O}$ and Microparticle Content Profiles for Ice Core from Camp Century, Greenland (from E. Thompson 1978)

Ellen Thompson has completed a very detailed analysis of a 100-meter core from the South Pole (E. Thompson 1978, Figure 21). The detail at 40-meters depth shows a dirty layer which, by stratigraphy, we count down as being 1884, the date of the Krakatoa eruption. The same kind of detail exists over the whole core. A most interesting relationship exists in the variation in the number of particles (in 50 microliter samples) through the 900-year section. Microparticles were more abundant between about 1850 and 1450, than before or later, except for particular layers, some of which can be related to particular volcanic events. The microparticle profile relates well with

Dome C, Antarctica

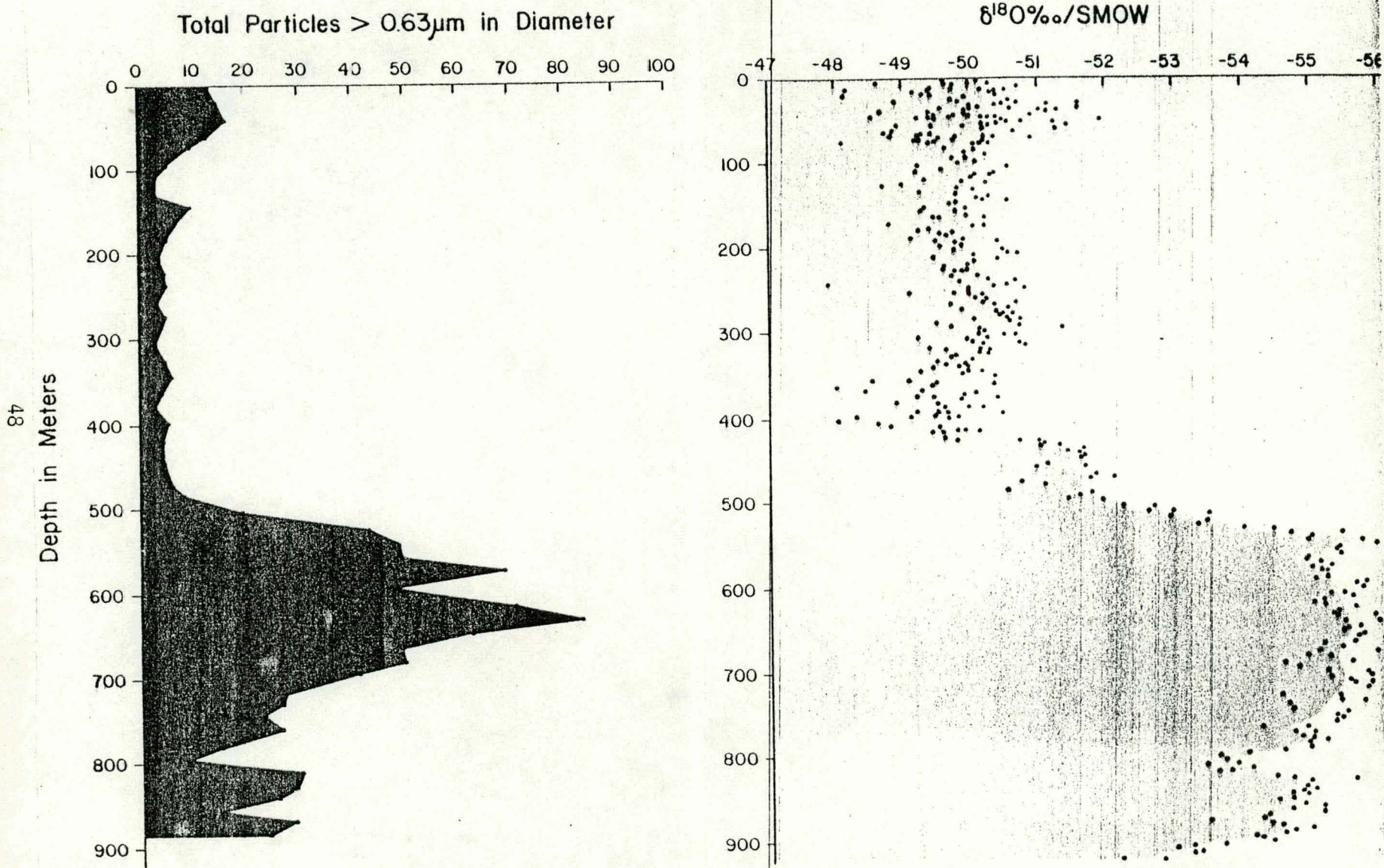
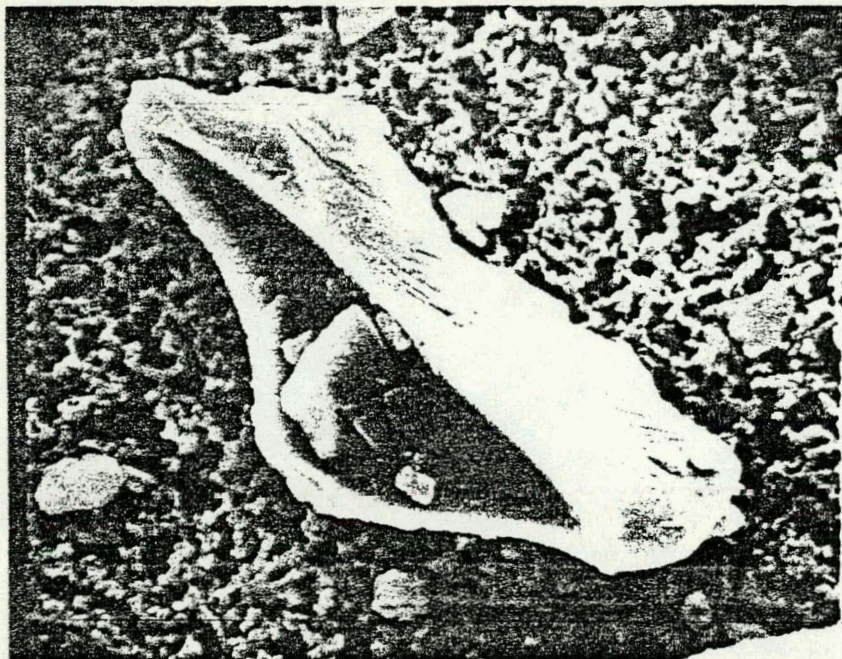
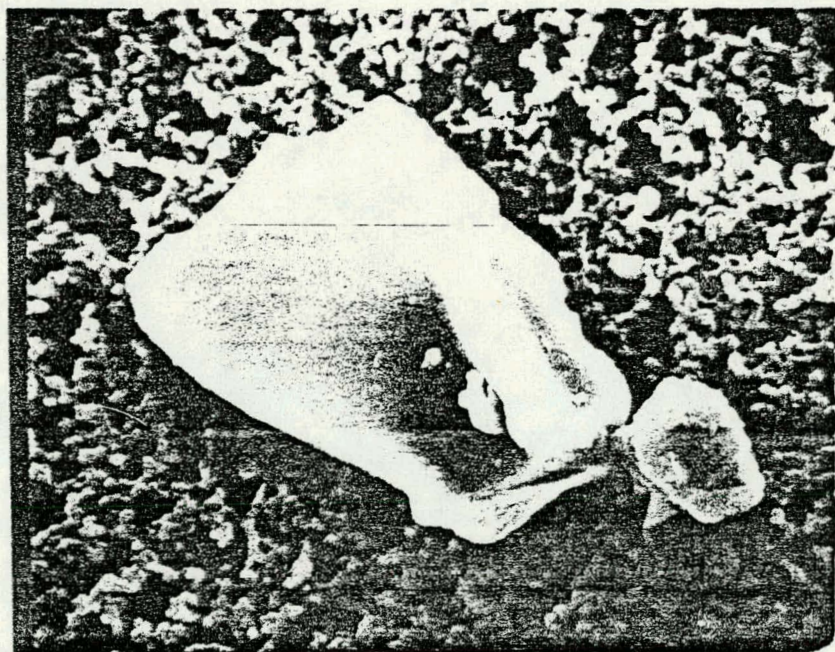


FIGURE 19. Plots of $\delta^{18}\text{O}$ and Microparticle Content Profiles for Ice Core from Dome C, East Antarctica (from E. Thompson 1978)

7. 21-66.



DEPTH IN CORE 871.1 M
ELEMENTS Si, Al, Fe, K, Ca
MAGNIFICATION 2470
LONG AXIS 35 μ m



DEPTH IN CORE 886.5 M
ELEMENTS Si, Al, K, Fe
MAGNIFICATION 3050
LONG AXIS 20 μ m

FIGURE 20. SEM Photographs of Particles from Dome C
Antartica, Ice Core

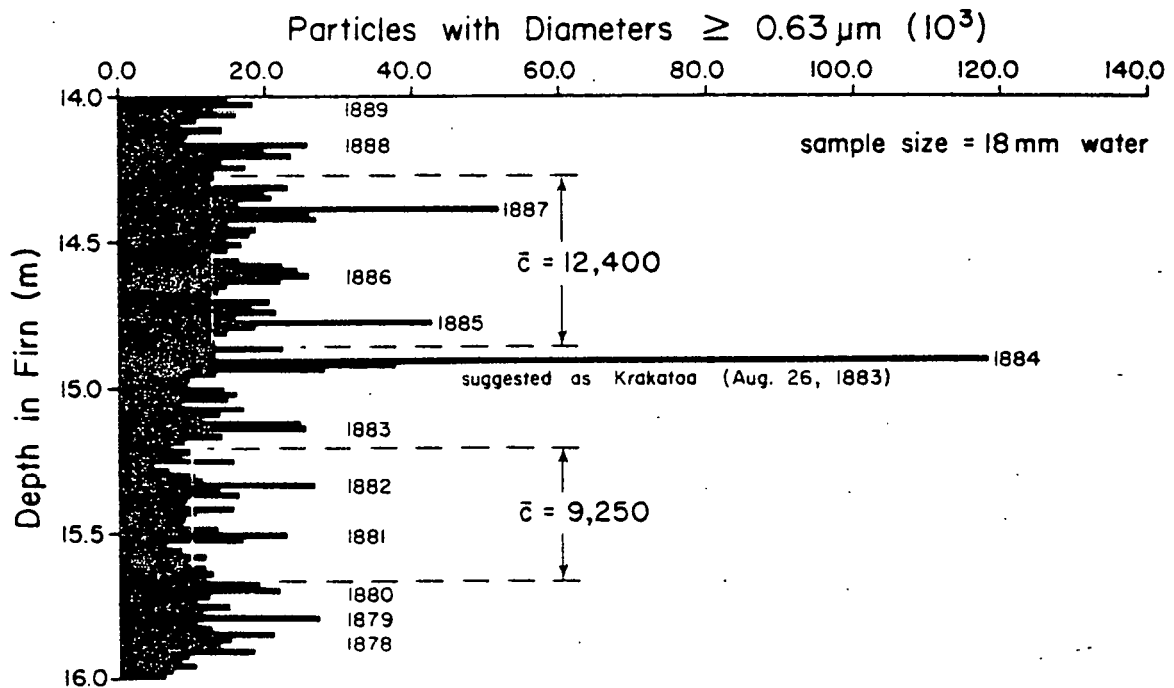


FIGURE 21. Detailed Microparticle Profile of Part of 101-meter Ice Core from South Pole (from E. Thompson 1978)

profiles of global temperature variations. Lamb's plot of temperatures from central England (which may be representative of mean global temperatures) shows lowest temperatures in the period between 1400 to 1800 A.D., the Little Ice Age, when the particle content was also higher, by a factor of two. Again there may be a relationship between dirt, and air temperature. This is important in our glaciological considerations; WISAP will be considering not only natural phenomena and variations of glacial limits, but also the effects of human actions.

The Effects of Carbon Dioxide in the Atmosphere

One point to consider, then, is the total dust content of the atmosphere. The total dirt content of the atmosphere 12,000 years ago was on the order of 10 to 50 times greater than it is now; however it is increasing, and perhaps producing cooling. The other point to consider is the variation in the carbon dioxide content of the atmosphere.

The consensus now is narrowing on the effect that carbon dioxide increases on temperature in the polar areas. If the current rate of increase in carbon dioxide content continues, there may well be temperature changes in the south Polar area on the order of 6°C or so in a period of 50 to 100 years. The west Antarctic Ice Sheet, much of which is below sea level, is probably stable only because of the presence of the Ross Ice Shelf. The 6°C temperature change is likely to make that unstable; it could well disappear. If the Ross Ice Shelf disappears, there would be no change in sea level, but, as a result there could be an instability of the west Antarctic Ice Sheet. The collapse of that would produce a 7-m rise in sea level, a consequence that will be of interest to those responsible for modeling sealevel effects on the Hanford Site and other likely nuclear-repository sites.

The question then is whether the dirtiness of the atmosphere, as it increases, could produce a temperature change, and perhaps the same conditions that initiated the last ice age. On the other hand, if the carbon dioxide content increases, maybe we can anticipate disappearance of a large part of the west Antarctic Ice Sheet.

Stress at the Edge of an Ice-Sheet

As well as the hydrological and erosion effects of glaciers, there are some other effects at the edge of an ice sheet, should it ever come close to Richland, to which we must pay attention. Near the edge of an ice sheet there are large horizontal changes in the stress. Those stress gradients may be sufficient to cause disruption of the underlying rock, and consequent changes in porosity, permeability, and so on. WISAP will have to be concerned with the changes in the rock regime.

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- Q. Is there any evidence of the previous glaciations causing disruption of the rock?
- A. I know of one case only, where there has been something that strongly points towards a disruption. In Maine, the geologic explorations associated with a proposed nuclear power plant on Sears Island showed Phyllite having been pushed up into the overlying till which is 18,000 to 25,000 years old. Obviously we have isostatic uplift in most deglaciated areas, with changes in the hydrologic head, and so on. Those points are well established. It is the fracturing that seems to be worthy of more consideration.
- Q. A clarification for my benefit. Is it because we haven't looked, or is it because we haven't found any?
- A. I think it's because we haven't looked.
- Q. How definite is the evidence that the carbon dioxide accumulation is increasing the global temperature? Did you make a definite statement to that effect?
- A. I made the statement that there was no complete agreement on the point; some think "no," but probably a majority see big temperature effects, especially in polar areas. I think I should refer you to George Kukla on this.
- C. I'd like to make one comment to put Dr. Bull's talk into more perspective. Dr. Bull has been working with the WISAP program for a couple of years now. The question has come up, "Have you identified things that you need to worry about?" Last year we identified glaciation as something we needed to worry about. And so, as you can tell from this year's work that Dr. Bull has been doing, we're now trying to establish, trying to quantify, what kind of rate we're dealing with and with respect to how fast. If the glacier started in 1970 can I expect my house in

Montana to be overrun?[?] That's really what you're trying to correlate from looking at the advance of this section of the ice front, and extrapolating that to the overall advance of the ice sheet. I think the rate you were giving us was a 100 to 200 meters per year.

- A. What we have been forced to do is to assume that the ice sheet is in equilibrium form, in other words, that its top surface is controlled by the rheological properties of the ice. If that's the case, then we have to assume that the centers of ice sheet formation are the same points that hold the vestiges, the last remnants of the last ice sheet. We also know when the last ice sheet vestiges were there; at present we also have to assume that the rate of advance from that initial formation was about the same as the rate of retreat.

EXPECTED ENVIRONMENTS AND CLIMATES
IN THE NORTHWESTERN U.S.A. IN THE NEXT MILLION YEARS
(The WISAP Climate Submodel)

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ABSTRACT

Probabilities of future climate-related stresses in northwestern U.S.A. in the coming million years were estimated as a contribution to a conceptual simulation model for release-scenario analysis of a hypothetical site in the Columbia Plateau. The estimates are based upon the reconstructed history of past climates and environments. The assumption is made that the shifts in climate experienced in the past will be repeated in the future, principally unaffected by man's technology. We focused on the extremes and on the time interval of the first 100,000 years.

We expect that the potentially most serious climate-related stresses will consist of the effects of future alpine glaciers and of the failures of ice-dammed lakes on the ground-water regime and on the geologic structure of the repository's confinement.

Probability of ice advances will increase significantly some 3000 to 5000 years from now. Major catastrophic floods caused by the failed ice dams are only marginally probable over the next 50 millennia but are highly probable thereafter.

Annual air surface temperatures in the Columbia Plateau will drop in the future by up to ~ 10 to 15°C and rise at other times by up to $\sim 5^{\circ}\text{C}$. Annual precipitation is estimated to change by a factor of 0.5 to about 3.0.

OBJECTIVES AND ASSUMPTIONS

The objective of the WISAP climate submodel is to estimate the probability of climate-related processes and events that could adversely affect the nuclear waste repository in the coming million years. Because of the decay characteristics of radioactive waste, increased attention is paid to the next 100,000 years (Logan and Berbans 1977). The extreme climate and environmental shifts, rather than the average states, are in focus. This is because the safety of the repository should be tested for the worst situations that can reasonably be expected, rather than for the average ones.

Prediction of future climates is based upon two basic assumptions:

- 1) The past pattern of natural gross climatic and environmental changes will continue unchanged in the future, principally unaffected by man.
- 2) Man's technology will basically remain at a level that can be reasonably foreseen today.

Both assumptions are questionable. It is known that man already affects local and event regional climates. It is expected that burning fossil fuels may have a noticeable impact on global climates as early as in the next 50 years (Kellogg 1977).

We must also remember that technology develops at an exponential rate. This is well illustrated by the Apollo moon landing, which followed the Wright Brothers first flight in 1903 by only 66 years. Obviously, prediction of a technology a hundred years ahead lies in the realm of science fiction. In such a situation, our second assumption can be justified only if the technology stagnates at the present level, or if it collapses within the coming few decades. While both cases are indeed possible and come close to the worst development to be reasonably expected, we believe that the most probable course leads toward an active modification and gradual stabilization of global climates. In such a case the danger of climate-related stresses, as discussed further, will be totally eliminated (Kukla 1978).

Reconstruction of Past Climates

How do we proceed in the reconstruction of past climates? Obviously, we do not have any direct record of meteorological or climatic variables. Instead, indirect indicators -- so-called proxy-climate indicators -- are used. In one way or another, these are a function of climate. The calibration of the proxy indicators with climatic variables, such as the annual mean temperature or total precipitation, is done mostly for the recent interval for which we have meteorological records. Is the correlation valid even at the times when global climates were significantly different and either more or less variable than at present? This is sometimes impossible to prove. However if several independent proxy indicators are combined to define a past climatic stage, the results are considered reliable (GARP 1975).

Widely used are the geomorphic indicators of the past climates. Figure 1 shows the frontal deposits of former continental ice sheets in Europe, called moraines. They provide unequivocal proof of repeated advances of glaciers into the middle latitudes of the Northern Hemisphere (Flint 1971).

Figure 2 shows the sequence of river terraces in the foothills of the Alps. This sequence was formerly interpreted to represent four major Pleistocene glaciations called Wurm, Riss, Mindel, and Gunz (Penck and Bruckner 1909). We now know that it actually resulted from the four episodes of accelerated crustal movements in the young mountain belts (Schaefer 1953, Kukla 1977).

The best record of the fluctuations of past global ice volume is present in the deep-sea sediments (Figure 3). The ratio of the oxygen isotopes 18 and 16, as preserved in the shells of fossil foraminifera, is a function of the total ice volume deposited on land (Shackleton and Opdyke 1973). Ten major glaciations with a number of secondary oscillations were recorded in the last one million years. (With preceding interglacial, they form a glacial cycle.)

Fossil pollen grains preserved in the lake sediments show the past vegetation surrounding the lake. Figure 4 shows the time-related changes of pollen spectra in Macedonia during the last 600,000 years. Vegetation repeatedly changed from open steppes to closed hardwood forests (Wijmstra 1969,

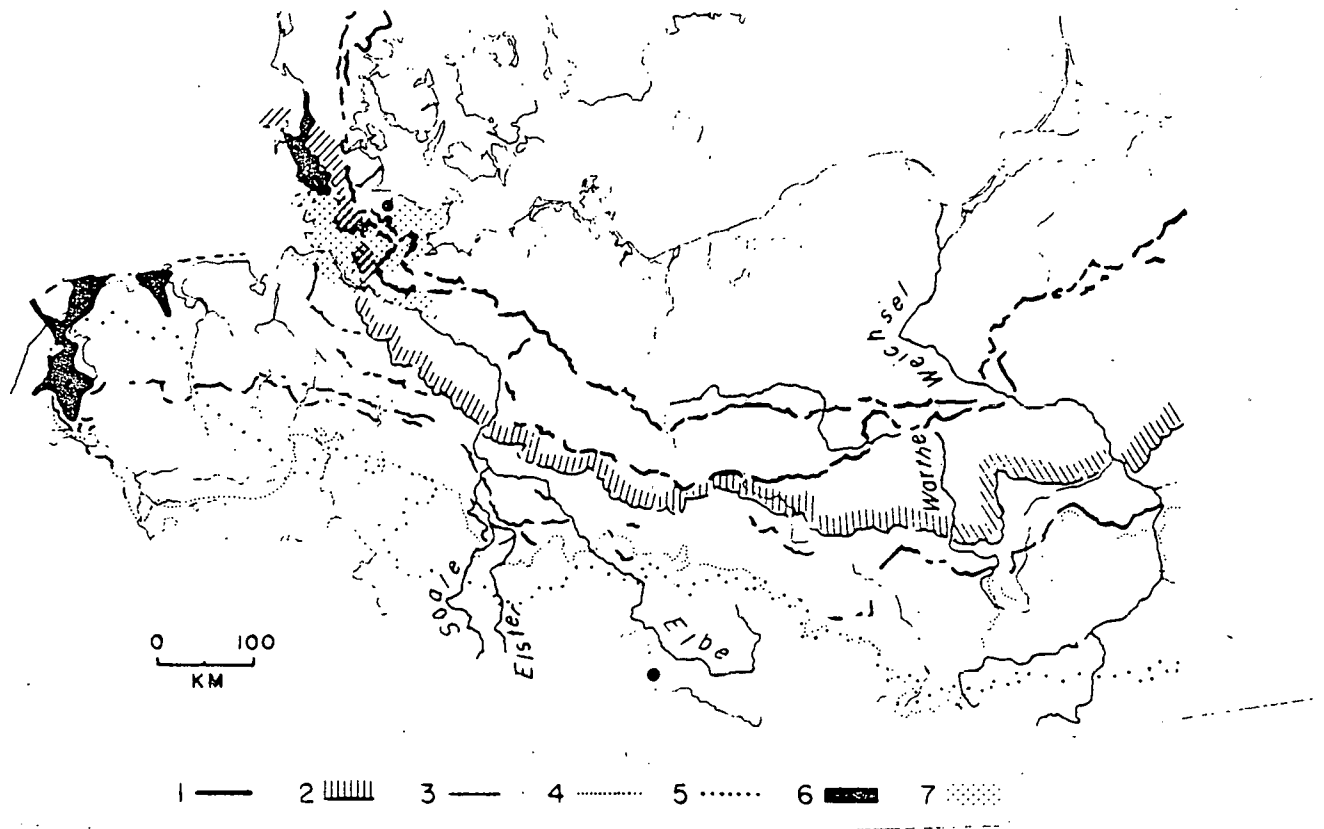


FIGURE 1. Distribution of the Frontal Moraines of the Pleistocene Continental Ice Sheets in Europe. 1. Weichsel glaciation, 2. Warthe advance, 3. Saale, 4. maximum extent of Saale tills and erratics, 5. southernmost extent of Elster tills and erratics, 6. marine deposits of the Eem interglacial, 7. marine deposits of the Holstein interglacial

Van Der Hammen, Wijmstra and Zagwijn 1972). Soil interlayers in the loess sequences (Figure 5) or in lake beds (Figure 6) mark episodes with dense vegetation or low lake levels.

Indirect paleoclimatic evidence of the kind shown above from all around the world enabled the arbitrary classification of four gross repetitive global climate states, that are in the order from the warmest to the coldest:

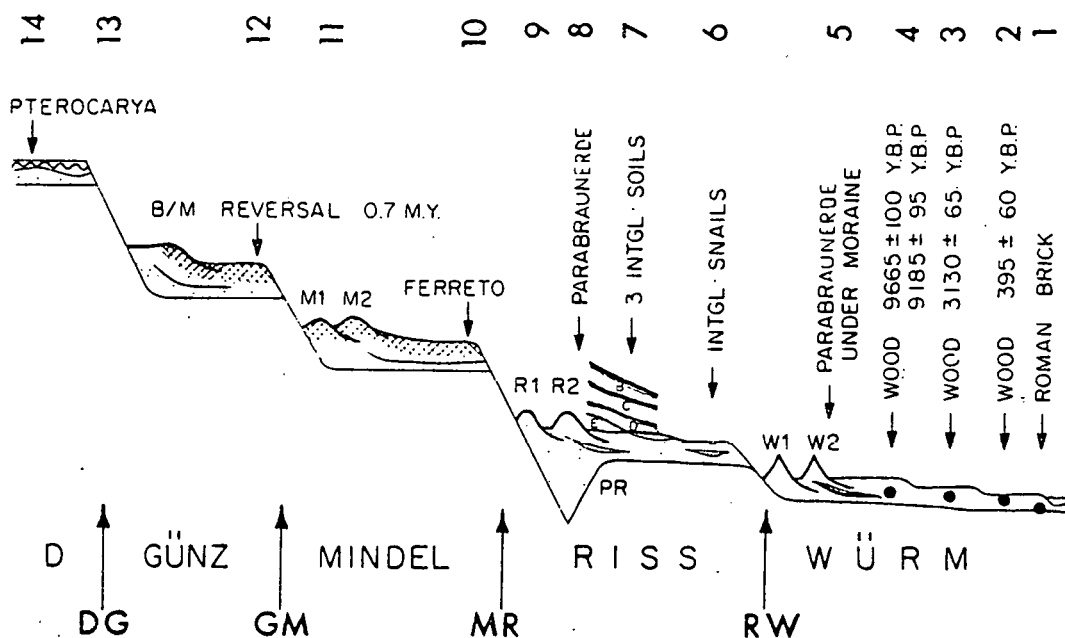


FIGURE 2. Cumulative Cross Section Through the Alpine Terraces in the Type Area of the Classical Pleistocene Glacial Stages Würm, Riss, Mindel and Gunz in Germany and in Switzerland. W1 Through M2: Schematic position of the end moraines of Wurm 1 through Mindel 2. Interglacial clays, silts and soils in black, loess stippled (Kukla 1977).

- 1) interglacial
- 2) temperate interstadial
- 3) interstadial
- 4) stadial

The last three are subdivisions of a glacial.

The global climate during interglacials was as warm or warmer than at present, whereas the contrary holds for the three stages of a glacial state. Because of a considerably different probability of climate stresses we decided to differentiate in the climate submodel between the protostadials occurring at the beginning of a glacial cycle when continental ice sheets are restricted to the high latitudes, and the stadials (or full stadials) that occur later within a cycle, when continental ice has reached the middle latitudes. As a first order approximation, all stadials peaking less than 40,000 years following an interglacial can be considered protostadials.

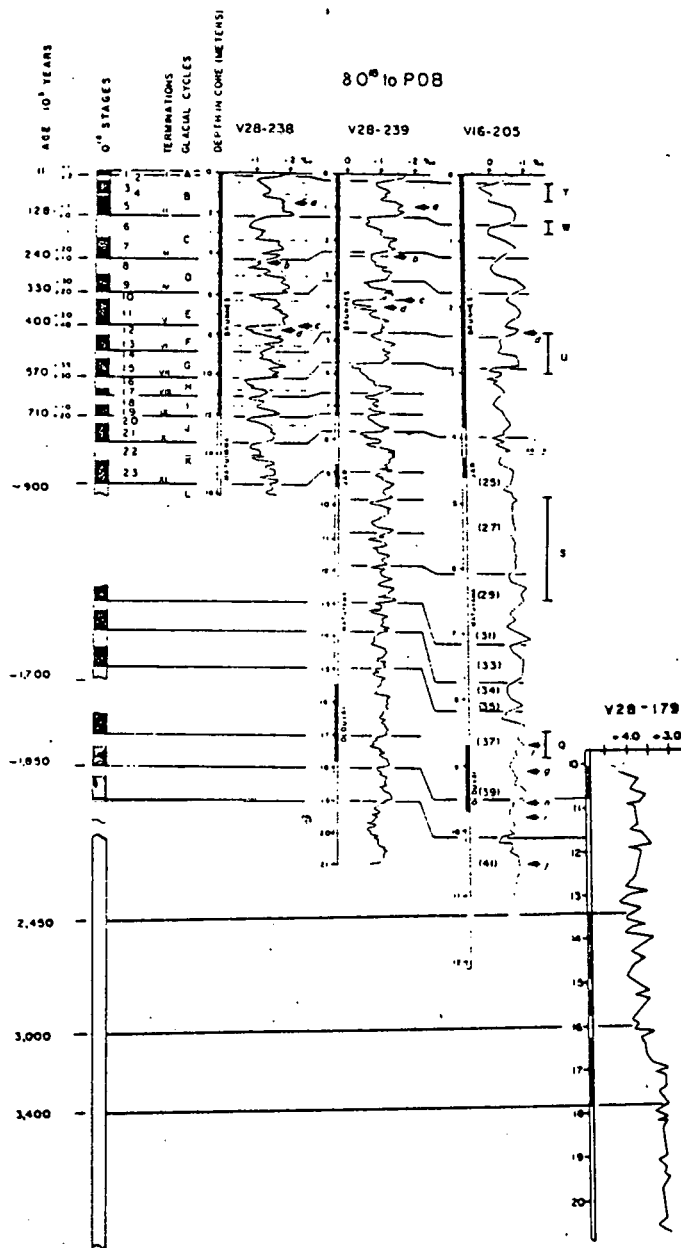


FIGURE 3. Oxygen Isotope Ratio in the Three Long Deep Sea Cores Reflecting Fluctuation in the Volume of Ice Stored on Land. Pacific cores V28-238 and V28-239 from Shackleton and Opdyke (1973), Atlantic core V16-205 after Van Donk (1976). Age estimates based on dating of polarity zones (Normal polarity, black; reversed blank). Terminations after Broecker and Van Donk (1970). Details in Kukla (1977).

NORTH ATLANTIC M A C E D O N I A

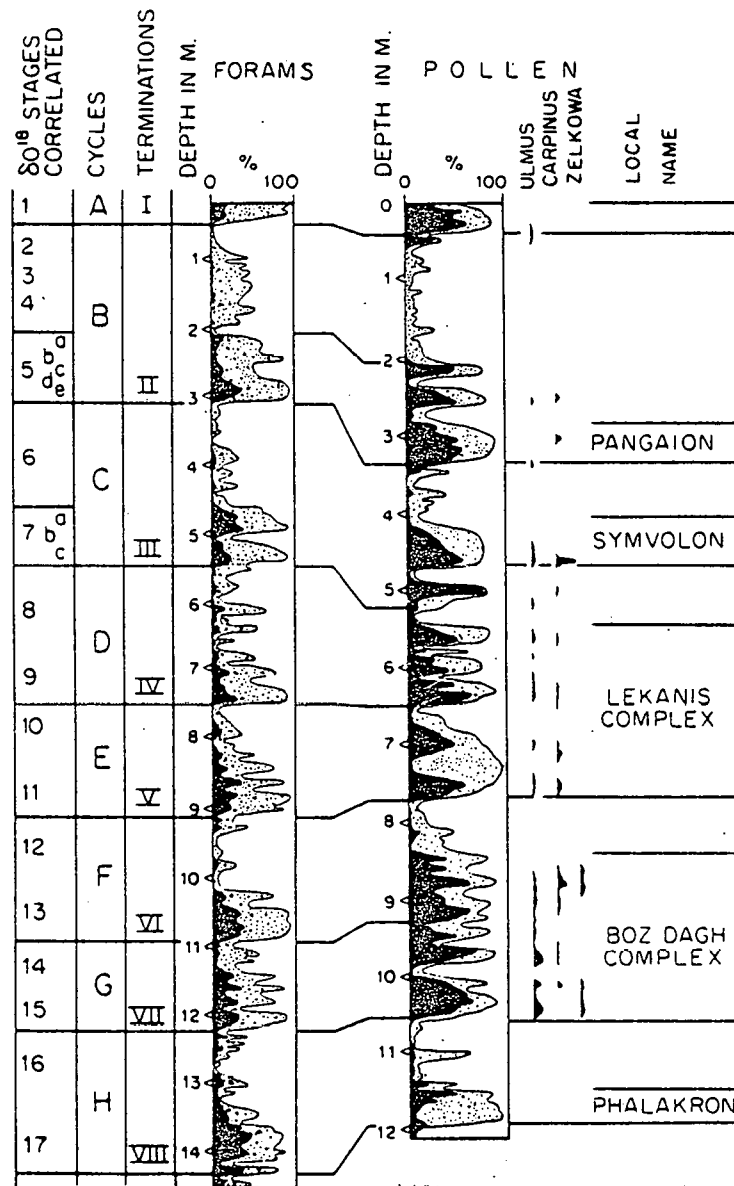


FIGURE 4. Pollen Record in the Tenaghi Phillipon Core in Macedonia (Wijmstra 1969, Van Der Hammen, Wijmstra and Zagwijn 1972) Compared with Distribution of Foraminiferal Assemblages in the North Atlantic Core K708-7. (Ruddiman and McIntyre 1976). Relative proportion of oak pollen and of subtropical and transitional foraminifera in black; Nontree pollen and polar foraminifera blank.

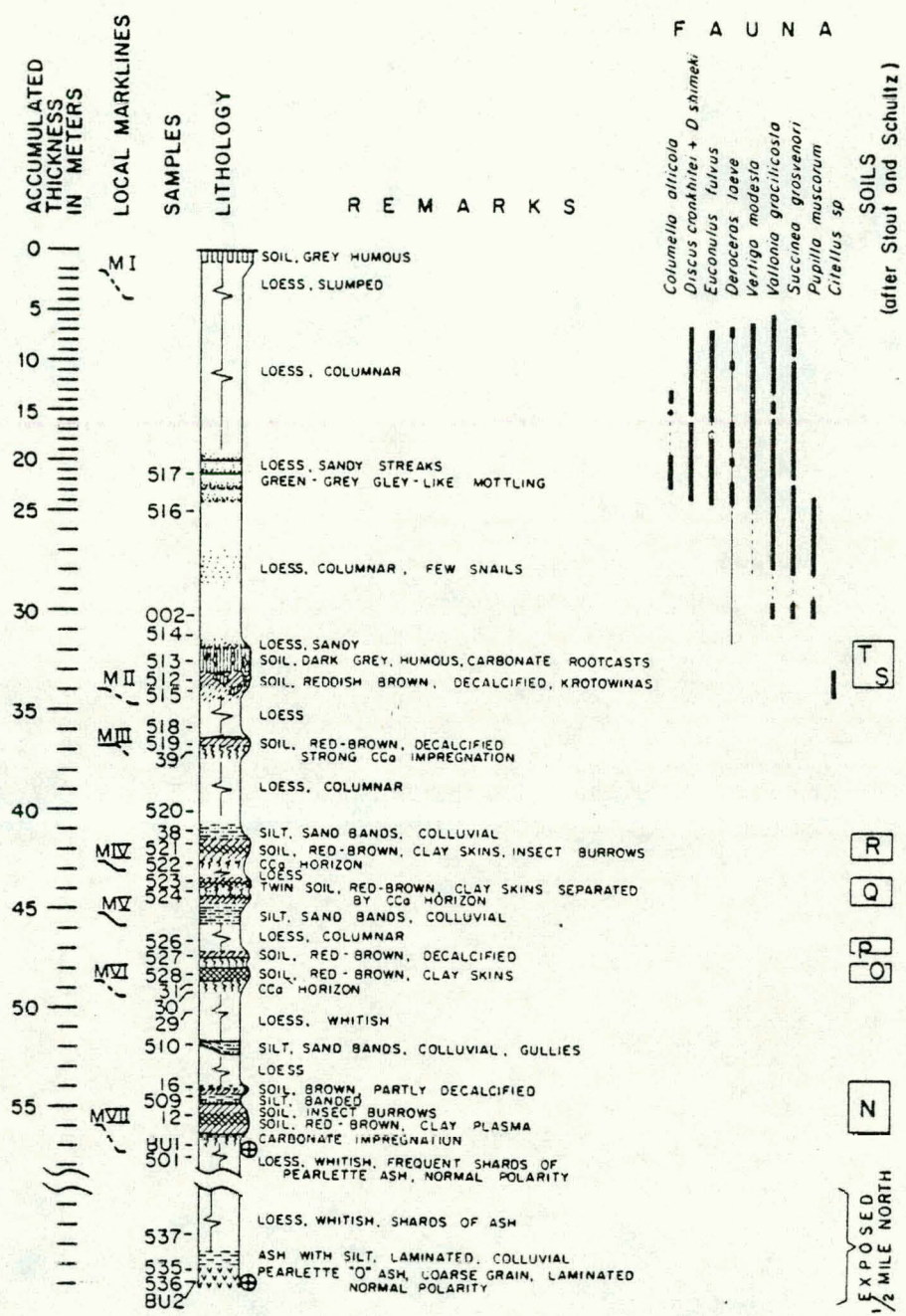


FIGURE 5. Buzzard Roost Loess Section in Nebraska. Pearlette "O" Age at the bottom of the sequence is 0.6 million years old. Encircled crosses show paleomagnetic samples with normal polarity. Further details in Kukla (1978).

BURMESTER CORE 0 - 1005 ft.

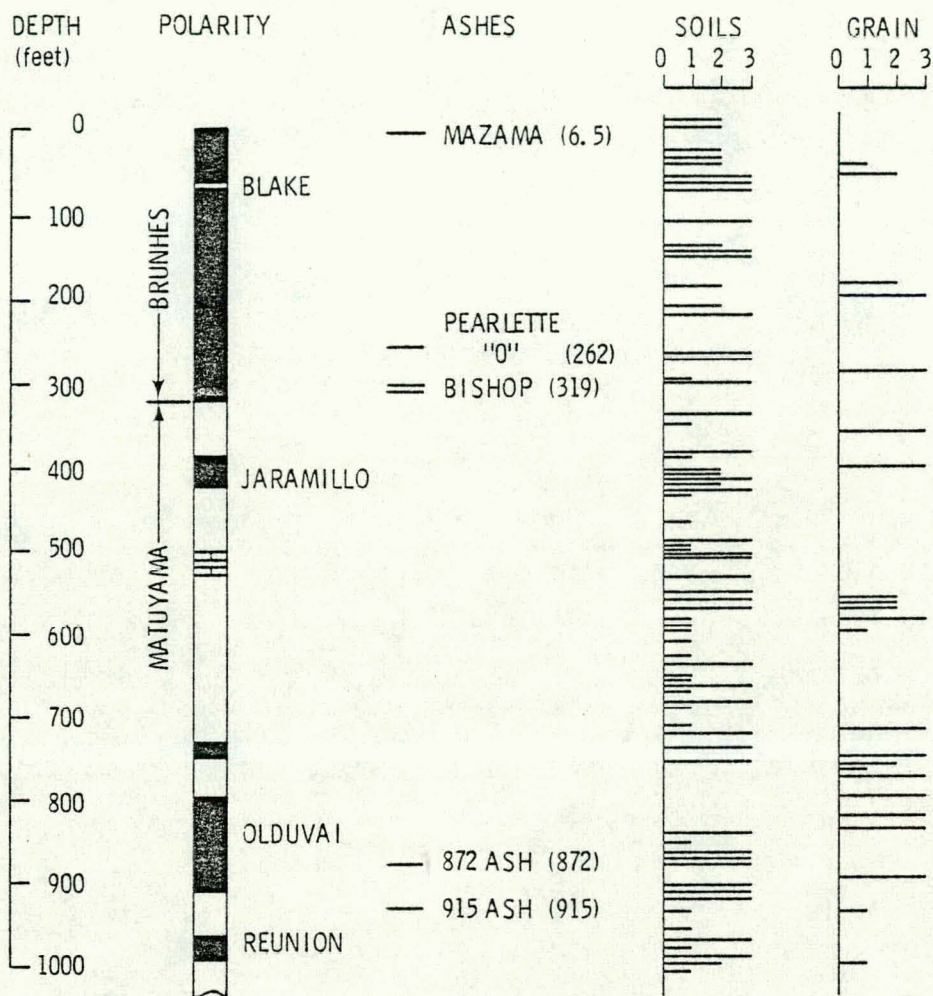


FIGURE 6. Sequence of Soils and Lake Sediments in the Burmester Core, Utah. Normal polarity black, reversed polarity blank.

Paleoclimatic records are dated by various radiometric methods. Correlation of distant sites of the same age is assisted by paleomagnetic techniques which detect the record of the reversals of the polarity of the earth's magnetic field (Cox 1969).

The most widely used radiometric methods and their approximate geologic accuracy are listed in Table 1. Geologic accuracy describes the accuracy limits actually reached by repeated testing of field samples known to be of the same age.

In general, the accuracy and the density of geographic coverage of paleoclimatic records decreases with increasing age. The deeper into the past we go the less accurate and the less detailed are the paleoclimatic time series.

Nevertheless, from the continuous records of proxy data, it could have been concluded that the complexity and the amplitude of climatic spectra have not significantly altered within the last million years or so. This finding justifies (GARP 1975):

- 1) the use of the young well-dated and globally reconstructed detailed records of climate shifts as a model of similar older episodes, and
- 2) a forward extrapolation of the past natural climatic variability into the near future.

The following observations can be made on the climate changes of the globe in the last million years:

- 1) Ten glacials alternated with interglacials in quasi-regular intervals of approximately 100,000 years (so-called glacial cycles).
- 2) A glacial cycle started with an interglacial. Oscillatory cooling followed. This terminated in a peak stadial. At that time the volume of ice stored on land reached its maximum. Glacials ended by a major deglaciation, the record of which in geological sequences is referred to as a "termination" (Broecker and Van Donk 1970), or "markline" (Kukla 1975).
- 3) The amplitude of key paleoclimatic indicators during individual interglacials and peak glacials was rather similar. The ice-volume extremes, the local types of vegetation, the geographic position of the ice terminals, and so forth, were fairly similar during individual glacial cycles of the past million years. The maximum amplitude of climatic oscillations in mid-Pleistocene seems to have remained closely confined to an envelope defined by the last peak interglacial and the last glacial maximum.

TABLE 1. Radiometric Methods for Dating Paleoclimatic Evidence

	<u>Carbon-14 Method</u>	<u>Uranium Decay Series Method</u>	<u>Fission Track Method</u>	<u>Potassium/ Argon Method</u>
Age Range	0 to 45,000 yr	0 to 250,000 yr	up to several million years	up to several tens of millions of years
Accuracy	+100 yr in wood Samples 10,000 yr old	+5,000 to 7,000 yr in samples 120,000 yr old +30,000 yr in Samples 240,000 yr old	5% to 10% of the age	5% to 10% of age in samples about 1,000,000 yr old
	+1,000 yr in wood samples 30,000 yr old			
	+3,000 yr in wood samples 40,000 yr old			

- 4) The interglacials lasted, uninterrupted, from 10,000 to 15,000 years. Their boundaries are defined by the presence of hardwood forests at the type localities of northwestern Europe. Their length was estimated in the annually laminated deposits (Muller 1974; Kukla, Matthews and Mitchell 1972).
- 5) Secondary oscillations occurred within each glacial cycle. Their character and duration are poorly understood. The temperate interstadials, with a climate in the mid-latitudes moderately cooler than at present, are characteristic of the early stages of a glacial cycle. They are marked by a presence of coniferous forests with some oak in northwest Europe. They probably lasted around 8,000 years.
- 6) The (regular) interstadials, with climate in mid-latitudes considerably cooler than at present, were marked by an open vegetation with occasional enclaves of pine and birch in northwest Europe. Their average uninterrupted duration was 4,000 years.

- 7) Stadials and protostadials occupied the time between the two consecutive mild episodes.
- 8) Transitions from one climatic state into another as shown by proxy climatic data were relatively fast (Bryson and Wendland 1967). Thus the time series of reconstructed climate variables approach step functions rather than the smooth sine function (Woillard 1979).
- 9) Combining available information from the land and the ocean, the probability of occurrence of individual climatic state at any time during the past million years is obtained. It is shown in Table 2 (Kukla 1978). The same stochastic probability distribution of natural climates is also expected in the future.

TABLE 2. Gross Climate States Within the Last Million Years

<u>Climate States</u>	<u>Probability of Occurrence, Round to Nearest 0.05</u>
Interglacials	0.10
Temperate Interstadials	0.15
Interstadials	0.10
Stadials	0.45
Protostadials	0.20

Climate as a Deterministic Process

The periodicity of proxy-climate series matches the periodicity of the perturbations of the earth's circumsolar orbit (Milankovitch 1929, 1942; Broecker and Kaufman 1965; Mesolella et al. 1969; Hays, Imbrie and Shackleton 1976; Berger 1978). There is no serious doubt today that the two systems are bound by a cause-and-effect relation. The linking mechanism, however, remains unknown.

The perturbations of the earth's orbit can be computed from the laws of celestial mechanics for several million years ahead and in the past (Milankovitch 1941; Sharaf and Budnikova 1969; Vernekar 1971; Berger 1976).

Computations by individual authors using input of varying accuracy closely agree for the past 300,000 years, but disagree for earlier intervals (Berger 1978). Correspondingly, the extension of astronomical chronology of the earth's orbit into the future 300,000 years can be considered reliable, but beyond that, less reliable.

The projection of future gross climates as a function of earth orbital variations can follow two principal strategies. The first one, widely used but as yet unsuccessful, requires an arbitrary selection of sensitive season and sensitive latitude and an estimate of the time lag between the orbital variations and climate response (Broecker et al. 1968; Chin and Yevjevich 1974; Calder 1974). The second, used by us, Kukla and Berger, and also by Imbrie and Imbrie, uses the harmonic analysis of the three main orbital variables, the tilt (ϵ), longitude of the perihelion (ω) and the eccentricity (e), in direct comparison with paleoclimatic record.

We compared the radiometrically dated peak mild and cold events of the last 150,000 years with the corresponding ϵ , ω , and e .

We found that warm climates correlated with high ϵ and with ω approaching 360° . Cold climates correlate with low ϵ and with ω approaching 180° . High e should accentuate both the cooling as well as the warming impact of ω .

The correlation of ϵ and ω with past climates has received independent support in satellite observations of the recent seasonal cycle of snow and ice covers. Snow and pack-ice fields are highly reflective. To a large degree, they control the heat budget in the high latitudes and influence the weather patterns in the middle latitudes. Their minimum extent is reached at the end of August and the beginning of September (Kukla and Robinson 1980). At that time the proportion of solar radiation absorbed by the surface in the middle and high latitudes is at its maximum. Obviously this quantity is likely to be highest when the earth is near the perihelion and ω approaches 360° . In a similar fashion, high ϵ tends to increase the radiation income in high latitudes during spring and summer. The extent of the reflective snow and ice fields should decrease when ϵ is high.

ACLIN Equations

Based on the above observations, empirical formulas were developed predicting the gross climate state as a function of ϵ , ω , and e . The resulting Astronomic Climate Index (ACLIN) is higher if ϵ is high and if ω is close to 360° . The following criteria were applied in the testing.

Accuracy Level 1: The equation predicts ten interglacials in the last one million years, spread at approximately 100,000-yr intervals and fulfills criteria for Level 3.

Accuracy Level 2: The equation predicts three interglacials in the last 350,000 years matching paleoclimatic records within the accuracy limits of radiometric dating. It does not predict interglacials at approximately 80,000 and 10,000 years BP. It predicts major cold maxima at approximately 18,000 and 65,000 years BP and a less pronounced one at 110,000 years BP.

Accuracy Level 3: The equation predicts correctly the relative amplitude and timing of interglacials and temperate interstadials during the last 150,000 years. It predicts correctly the timing but not the relative amplitude of cold peaks.

Accuracy Level 4: The equation predicts the timing but not the relative amplitude of warm and cold intervals during the past 150,000 years. It does not discern interglacials from interstadials.

We have composed the formula of two terms, A , proportional to the relative intensity of ground-level insolation at very high latitudes at summer solstice, and B , proportional to the relative intensity of solar beam at the autumn equinox.

$$A = \sin \epsilon \cdot \frac{1 + 2e \cos (\omega + 90)}{\sqrt{1 - e^2}} \quad (1)$$

$$B = \frac{1 + 2e \cos \omega}{\sqrt{1 - e^2}} \quad (2)$$

Both A and B are scanned at two different intervals, t_1 (represented by Terms A_1 and B_1) and t_2 (represented by terms A_2 and B_2), separated by an angular measure of 90° in the longitude of perihelion (approximately 5000 to 6000 years). For example, to define the climate state at 6000 years BP, the formula examines the configuration of the orbital elements at the interval studied, and also at the preceding episode when the $\omega \sim 270^\circ$, which was 11,000 years ago. This bilevel scanning assumes a multi-millennia memory in parts of the climate system. In the simplified version, the formula can be written as:

$$\alpha = a(A_1 + b \cdot B_1) + (A_2 + c \cdot B_2) \quad (3)$$

where a, b, and c are arbitrary coefficients. The full expression of the basic ACLIN formula is:

$$\alpha = \left[\sin \epsilon_1 \cdot \frac{1 + 2e_1 \cos (\omega_1 + 90)}{\sqrt{1 - e_1^2}} + b \cdot \frac{1 + 2e_1 \cos \omega_1}{\sqrt{1 - e_1^2}} \right] + \left[\sin \epsilon_1 \cdot \frac{1 + 2e_2 \cos (\omega_2 + 90)}{\sqrt{1 - e_2^2}} + c \cdot \frac{1 + 2e_2 \cos \omega_2}{\sqrt{1 - e_2^2}} \right]$$

where: α is the ACLIN for time t_1

$e_1 \epsilon_1 \omega_1$ are orbital elements at a time t_1 and

$e_2 \epsilon_2 \omega_2$ are orbital elements at a preceding interval t_2 defined by:

$$\omega_2 = \omega_1 - 90 \quad \text{and} \quad |t_1 - t_2| = \text{minimum}$$

For practical purposes various simplified versions of α were studied. The simplest one is ACLIN 1 (α_1).

$$\alpha_1 = \left| \frac{\omega_1 - 180}{90} \right| + \epsilon_2 - 22 + \frac{10^3 e_1^2}{2}$$

where: α_1 is the ACLIN for time t_1
 ω_1 and e_1 are the longitude of the perihelion and the eccentricity at a time t_1 and
 ϵ_2 is the obliquity at an earlier interval t_2 defined by:
 $\omega_2 = \omega_1 - 90$; and $\left| t_1 - t_2 \right| = \text{minimum}$

In ACLIN 1 the term $(\omega_1 - 180)$ is used as a first order approximation of B_1 whereas term A_2 is approximated by $(\epsilon_2 - 22)$. A_2 and B_1 are considered to be of secondary importance and are omitted. For simplicity, although in disagreement with the laws of orbital geometry, the e and ω are handled as independent variables.

ACLIN 1 reached accuracy level 2 and is easy to compute. Plots of α_1 are compared with paleoclimatic evidence from the last 150,000 years in Figure 7, and are shown for the past and future million years in Figure 8.

Imbrie and Imbrie studied a family of curves resulting from the equation

$$x = \epsilon + \alpha [e \sin (w - \theta)]$$

where:

x is the climate state at a time t_1
 ϵ , e and w are respectively the obliquity eccentricity and the longitude of the perihelion at a time t_1 and
 α and θ are adjustable parameters.

Published variants of the curves fulfill our criterion of accuracy level 4. The interglacials are not differentiated from the temperate interstadials.

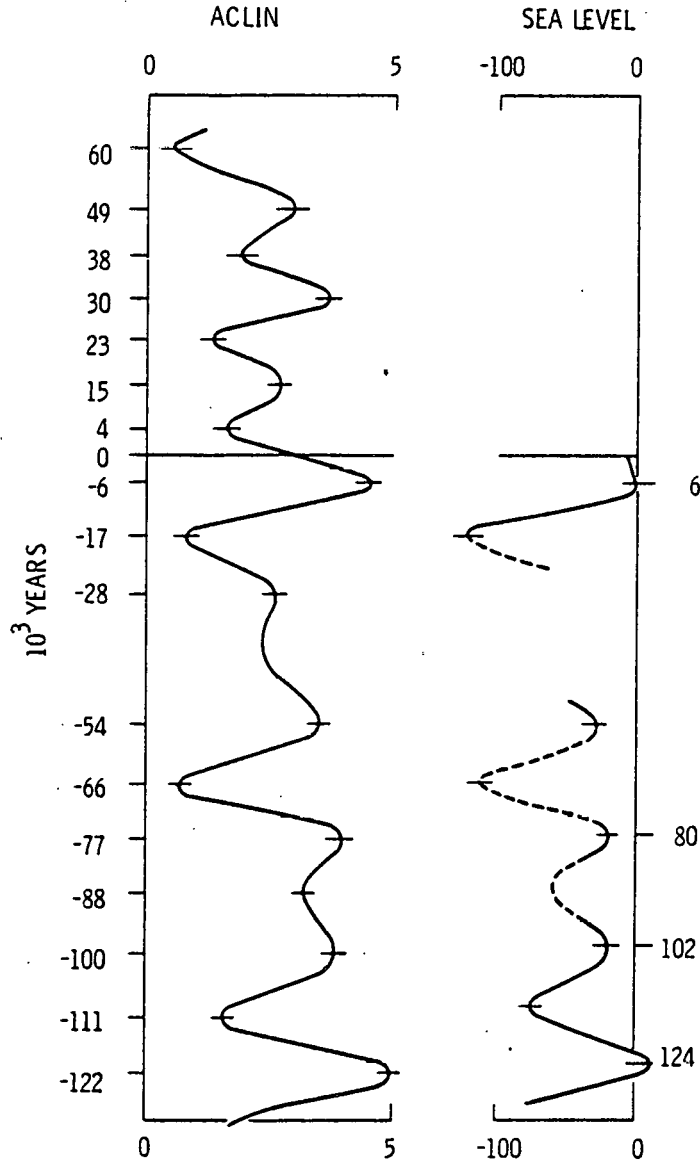


FIGURE 7. Plot of ACLIN 1 for the Past 150,000 and Future 50,000 Years Compared with Evidence of Sea Level Fluctuations in Barbados and New Guinea Based on the U/Thorium Dated Coral Reefs (Broecker et al. 1968)

The ACLIN time series approach smooth sinusoidal curves, whereas the time series of most paleoclimatic indicators, gathered on a sufficiently detailed time scale, take the form of step functions. They show relatively steady intervals several millennia long, interspaced by short episodes of relatively rapid change. Some records of this nature may result from a step response of

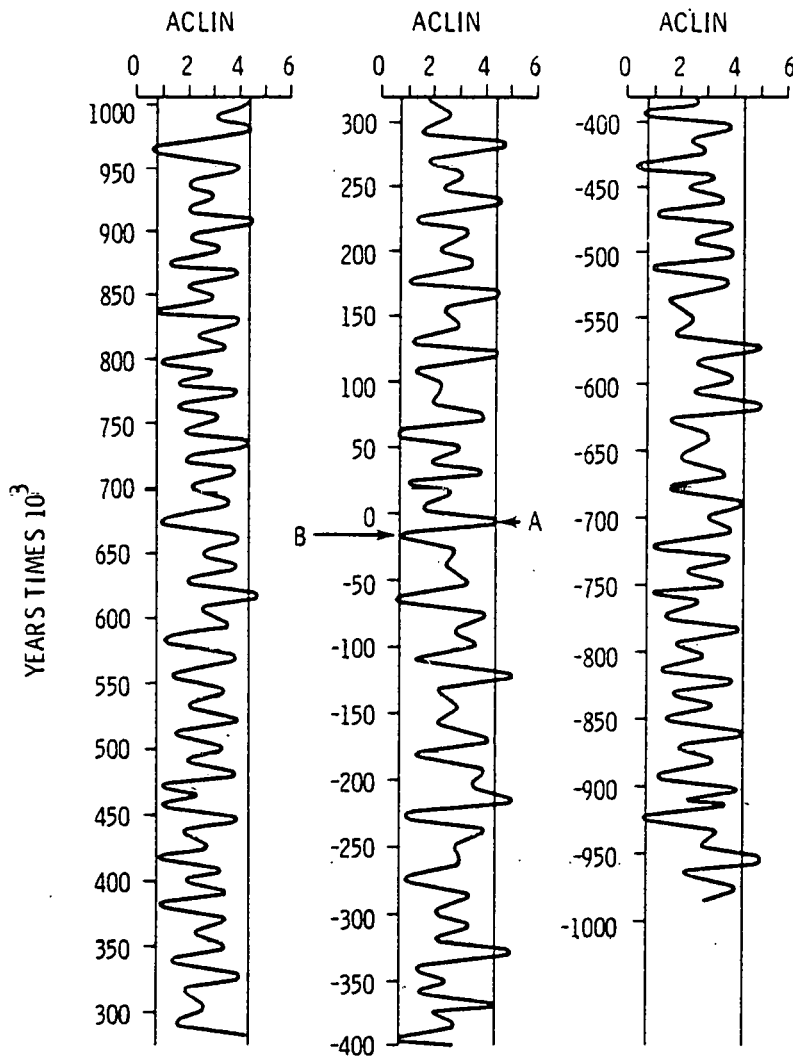


FIGURE 8. Plot of ACLIN 1 for the Past and Future 1 Million Years
 A - the present interglacial optimum
 B - the last glacial peak

a proxy-climate indicator with a broad climatic tolerance to a smooth continuous change of climate. However, existing information shows that the global climate changed in steps along a generally sinusoidal path controlled by orbital variables (Bryson and Wendland 1967, Flohn 1974). Therefore, the prediction of past or future climate states should focus on the nearest ACLIN maximum or minimum.

An interglacial climate can be expected during approximately 10 millennia centered around the corresponding ACLIN 1 high of >4.3. Temperate interstadials lasting about 8 millennia center around the ACLIN 1 peaks of >3.6. Other interstadials about 4 millennia long center around ACLIN 1 peaks of >2.5.

The single most significant achievement of ACLIN is the prediction of the interglacials, including the present one. All paleoinsolation curves proposed to date show higher amplitude for the peaks at 80,000 and/or 100,000 years BP than for the peak representing the present interglacial. This contradicts the paleoclimatic evidence which shows 80,000 years and 100,000 years BP as interstadials with global climate cooler than at present.

On the other hand, the ACLIN performance in simulating the relative intensity of cold events is poor. For instance, the ACLIN values 133,000 years BP are considerably higher than those for 17,000 years BP, although the extent and volume of continental ice was approximately the same at both intervals. The correlation of ACLIN with interglacials at about 400,000 and 500,000 years BP is also poor. ACLIN peaks at 404,000 years and 501 years BP do not reach the level of 4.3.

Climate and Volcanic Dust

The astronomic theory of paleoclimates, frequently called the Milankovitch mechanism, explains the gross climatic variability on the time scale of 10,000 years. However, climate is highly variable on the shorter time scales as well. The cause of these high frequency variations is often sought in the:

- 1) random interactions of the earth-ocean-atmosphere systems (Lorenz 1970),
- 2) variability in solar output, magnetic field, etc. (Willet 1974; King 1974), and
- 3) short-term drops of the atmospheric transparency due to aerosols released during volcanic explosions (Lamb 1970).

While the evidence presented in favor of either of the three factors is inadequate, the correlation of major volcanic explosions with subsequent cool episodes during historical times is relatively high and the physical background of the climatic link is well established.

This still does not exclude the first two factors, which may be responsible for the rest of the short-term variance.

Instead of arbitrarily defining the climate states by using their average estimated length centered around the ACLIN peak, an alternative strategy is possible and now being developed for WISAP. In the latter approach, the climate is a combined function of ACLIN and of the atmospheric Transparency Index (TI). The TI is allowed to fluctuate randomly in agreement with observations made on instrumental records of recent atmospheric turbidity. The rise in TI above a certain threshold will change the climate state into a colder one. The TI threshold is not fixed, but moves as a function of ACLIN.

Summarizing, we can estimate the probability of future natural climate based on:

- 1) stochastic probability of distribution of the five gross climate states (Table 2),
- 2) ACLIN 1 and on arbitrarily defined length of the five gross climate states,
- 3) ACLIN 1 combined with a random variable TI.

The three approaches give practically equivalent results if used for intervals beyond 100,000 years from now. For the near future, however, the application of ACLIN 1 formula decreases the inaccuracy of the estimate.

PAST CLIMATES AND ENVIRONMENTS IN WESTERN NORTH AMERICA

Spectacular changes in environment took place in the western U.S.A. during the last million years. Only about 12,000 years ago many depressions in the Great Basin were filled with extensive freshwater lakes with maximum depths of 200 to 300 m.

Huge Lake Missoula repeatedly formed behind an ice dam in northern Idaho and when the dam collapsed catastrophic floods flushed and eroded the channeled scabland of the Columbia Plateau. Floodwaters were over 100 m deep at the Hanford Site. Dunes of wind-blown dust and sand moved across much of the Columbia Plateau some 15,000 years ago (Richmond et al. 1965; Bretz 1959).

A sizeable body of geologic information on the paleoenvironments in the West was accumulated during the past several decades. Since part of the information was controversial, a workshop, attended by the key specialists, was called to Reno in 1978 to bring the regional paleoclimatic data base up to date.

The picture presented by the workshop participants can be summarized as follows:

- 1) Most of the applicable paleoclimatic information dated with reasonable accuracy comes from northwestern Nevada, Utah, New Mexico, Arizona, and eastern California. The reconstructions are dated with acceptable precision to about 30,000 to 40,000 years. Beyond that little information exists in discontinuous records in which paleoclimatic interpretation is often ambiguous.
- 2) Except for Yellowstone Plateau, northern Washington, northern Idaho, and the Cascades, the only mountain ranges that were ever glaciated in the western U.S.A. are over 3000 m high. Last glacier maxima were reached between 20,000 and 15,000 years ago. Ice retreated rapidly 12,000 years ago but minor readvances occurred about 6500, 3500, 2700 to 2000, 1000 and 600 to 100 years ago (Curry 1969, Flint 1971). In the state of Washington, alpine glaciers descended into the eastern Cascade foothills near the Columbia River 130 km upstream of Hanford (Richmond et al. 1965), whereas the front of the continental ice sheet reached Coulee City, 110 km north of the site (Figure 9).

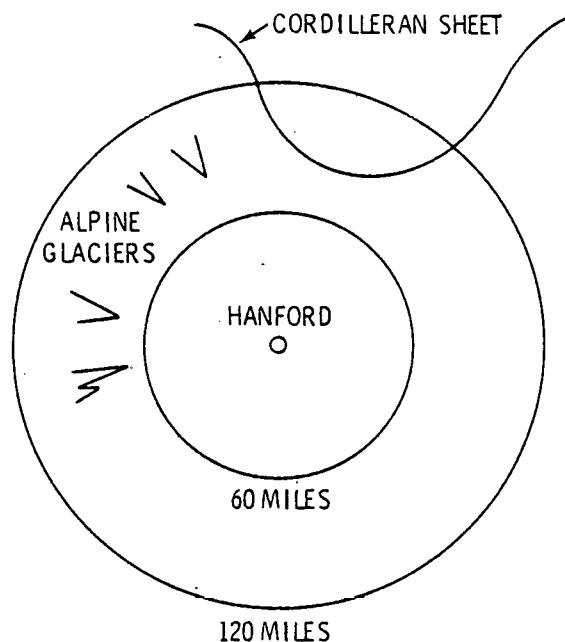


FIGURE 9. Extents of Late Pleistocene Glaciers in the State of Washington

(3) The large freshwater lake systems in Nevada, California, and Utah (Lake Bonneville, Lake Lahontan and the Searles-Manly system), were fed by the meltwater from mountain glaciers and snow. The lake levels were high between 23,000 and 13,000 years ago, but their highest stands were reached 13,000 to 11,000 years ago. The lakes dropped rapidly some 11,000 years BP and attained close to present levels at about 7500 years BP. They stood lower than at present or were completely dry between 7500 and 4000 years BP (Broecker 1980; Benson 1978).

Lakes oscillated in generally low levels between approximately 30,000 and 23,000 years BP, but they were higher than during the last ten millennia (Morrison 1980). The lakes were probably high and close to overflow levels shortly before 30,000 years BP. The length, dating, and stratigraphic evidence of paleoclimates from earlier intervals is not yet sufficiently clear (Mifflin and Wheat 1977).

- 4) Maximum rates in the drop and rise of the lake levels during Pleistocene compare with the rates observed during historic times. Thus, for example, Salt Lake stood 10 m higher between 1860 and 1890 than in 1960. The average observed decadal increases were 3 m in Salt Lake and in Pyramid Lake, and 16 m in Winnemucca Basin. If maintained, such rates would fill the basins to Pleistocene levels in a thousand years or less (Harding 1942).

There is no indication that the rate of fluctuation has ever been higher in the past than it is today.

- 5) The semi-desert nature of vegetation in the Basin and Range province was maintained throughout the last 40 millennia. Sagebrush pollen was abundant in the Pleistocene, pointing to a seasonally dry, open landscape. However, during this time the vegetational cover underwent noticeable changes, even during historical times, both natural and man-made. Grass was thicker, lusher and taller, and there were more trees in the second half of the 19th century than there are today. The difference is due to changed precipitation, introduction of sheep and goats, lumbering of timber for mines, etc. Between 11,000 and 13,000 years BP the vegetation cover was considerably denser, more diverse, and included species that required more moisture. Spruce forests covered mountain slopes where only open woodlands with sparse pines exist today. Also, 13,000 to 23,000 years ago, the vegetation was more dense than today though not as dense as in the following two millennia. Spruce and Ponderosa pine expanded far beyond their present range in much of the Southwest. Vegetational zones were shifted lower than today and deserts shrank (Mehring 1977; King and Van Devender 1977; Van Devender 1980; Wells and Berger 1967).

In the Columbia Plateau dust picked up by wind from seasonally dry, braided river beds covered much of the interior that had only sparse, patchy, xerophytic grass.

- 6) The large Pleistocene fresh-water lakes in the Great Basin near major ice sheets probably affected local and regional climate as follows:
- favored, cloudier summers in the vicinity of the lakes.
 - favored, moister autumns due to the relatively high evaporation from the relatively warm lakes and an increased precipitation in the surrounding mountains. These factors tended to do the following:
 - maintain cold and dry weather in winter and early spring due to the high regional albedo and close-to-zero evaporation rates from snow-covered ice surfaces.
 - maintain high-pressure cells over the ice-covered basins with consequent shift of the winter cyclonic tracks southward. Winter precipitation in Arizona and New Mexico should have increased (Van Devender 1978, Spaulding 1978). Toward the north, however, winters should have been dry in the center of the Columbia Plateau. This agrees with extensive deposits of Palouse loess (Richmond et al. 1965).
- 7) The climatic interpretations of the past environments in the Great Basin are based on the position of the orographic snowline and on the equilibrium between precipitation, evaporation, and runoff necessary to maintain the high lake levels. Estimates refer to the interval 11,000 to 13,000 year BP (Klute 1928; Kukla 1978).

Consensus of most authors is:

- a) annual temperatures lower by 5 to 10°C,
- b) annual precipitation higher by a factor of 1.2 to 3.0,
- c) evaporation significantly lower by a factor of 0.1 to 0.3,
- d) higher cloudiness in summer, and
- e) milder and possibly wetter winters but drier summers in the extreme Southwest.

The climate between about 20,000 and 13,000 years BP compared to the lacustrine interval 13,000 to 11,000 years BP was probably:

- a) colder, and
- b) somewhat drier.

The climate about 10,500 years ago was warm and generally similar to the present, but 4000 to 7500 years ago it was significantly warmer and drier. Around 8000 to 10,000 years ago, briefly 6000 years ago, 2200 to 3600 years ago and around 400 to 600 years ago at least in the mountains, the climate was wetter. The evidence and the assumption that the different authors used to estimate the past temperature and precipitation from reconstructed lake levels are summarized and discussed in the earlier report (Kukla 1978).

- 8) The catastrophic floods of the glacial Lake Missoula penetrated into the Quincy and Pasco Basins on multiple occasions. The oldest known gravel suspected to be deposited by a catastrophic flood is estimated to be older than 150,000 years (Patton and Baker 1978), whereas the youngest flood deposit is about 13,000 years old (Mullineaux et al. 1978). A possibly reworked wood fragment recovered from the floodloams unrelated to the former deposits was dated by the C^{14} method at $32,700 \pm 900$ years (Richmond et al. 1965). In total, deposits of three separate floods were distinguished and traces of at least four additional floods were deduced from physiographic evidence (Patton and Baker 1978). The peak flood discharge at Eddy Narrows was estimated at $9.5 \text{ mi}^3/\text{hr}$ (Pardee 1942). (By comparison, the catastrophic Mississippi flood in February 1937 reached a mere $0.05 \text{ mi}^3/\text{hr}$ at Natchez.) The average depth of the flood current was estimated to be about 100 ft (Baker 1973).

Relation of the Environmental Fluctuations in Northwestern U.S.A. to Global Climate Status

The accepted, well-dated, paleoenvironmental history in western North America is too short to enable a reliable statistical correlation with the

major global climate states. We assume that the events and processes dated to the last 10,000 years reappear during interglacials. Processes and events dated 23,000 to 11,000 years BP are characteristic of stadials. The events and processes dated 32,000 to 23,000 years BP are characteristic of interstadials. Unfortunately, there is very little information on the environment in western North America during the early stages of a glacial state. The reconstruction of environment and climate at these intervals has to be based mostly on speculation.

It is probable that alpine glaciers were extensive during the stadials of an early glacial but that the continental ice sheet then did not reach the Columbia Plateau. Lake Bonneville, judging from the sedimentary record in Burmester core (Eardley et al. 1973), was flooded but considerably shallower and less extensive than during the late glacial.

The implications for the climate of protostadials at Columbia Plateau are as follows:

- 1) Lower than present annual temperatures, albeit not as low as during the glacial maximum. A depression of about 3° to 6°C is the best estimate.
- 2) Alpine glaciers were at about two-thirds of their maximum glacial extent.
- 3) Areas of permafrost were restricted to the mountains, without significant impact on regional ground-water recharge.
- 4) No large ice-dammed lakes existed upstream of the Hanford Site. Danger of the Missoula type floods was significant.

ESTIMATE OF THE PAST PRECIPITATION, RUNOFF, EVAPOTRANSPIRATION AND GROUND-WATER RECHARGE

The precipitation probability distribution for each of the five gross climate states, namely the interglacial, the temperate interstadial, the interstadial, the protostadial, and the stadal is shown in Figure 10. At this stage, the distribution is a rough first-order estimate based upon only a few preliminary observations and numerous assumptions:

- 1) The published precipitation estimates for the Great Basin during the late glacial indicate an increase of annual mean precipitation by a factor of about 2 to 3. The highest published estimate is by a factor of 4.5 (Kukla 1978). Maximum decrease during peak interglacial is given by a factor of 0.5. We assume that the precipitation in the Columbia River watershed changed in a similar range.

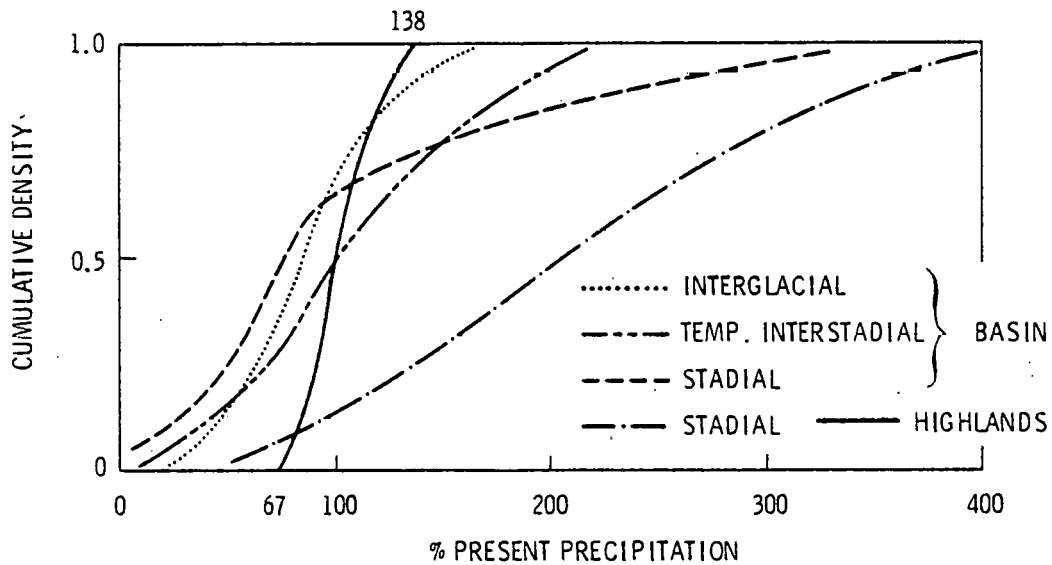


FIGURE 10. Estimated Cumulative Probability Distribution of Precipitation During a Temperate Interstadial (a), Interstadial (b), and Protostadial (c) within the Future 50,000 Years. Also shown are the distribution during interglacials (d), temperate interstadials (e), interstadials (f), proto-stadials (g), and stadials (h) of the interval 50,000 through 1,000,000 years after present.

- 2) The average annual state precipitation totals for Utah and Nevada during the period 1886 to 1938 oscillate within the range defined by factors of 0.5 and 1.5 to 1.6 (U.S. Dept. of Commerce 1942). Thus the recent year-to-year precipitation variability in the Great Basin is the same order of magnitude as the glacial-interglacial range, a fact that could be exploited in reconstruction of the past distribution densities.

- 3) We studied the dependence of the seasonal and annual precipitation totals on surface elevation in the states of Washington, Oregon, Idaho and Montana. Most of the precipitation in eastern Washington comes in winter (November through February). Precipitation is still considerable from March to June. Moisture at this time comes from the Pacific Ocean. Parts of Idaho and Montana have a precipitation peak in summer. At that time the source of precipitating water is the Gulf of Mexico.
- 4) We observe a general increase of annual mean precipitation with height on the western slopes of the Cascades. East of the mountains the precipitation decreases with the increasing distance from the ocean (Figure 11). A similar relation is expected also during the climate states other than an interglacial. The descent of the snowline during stadials and interstadials is expected to increase winter precipitation in the western and central Cascades, while decreasing the precipitation east of the mountains.

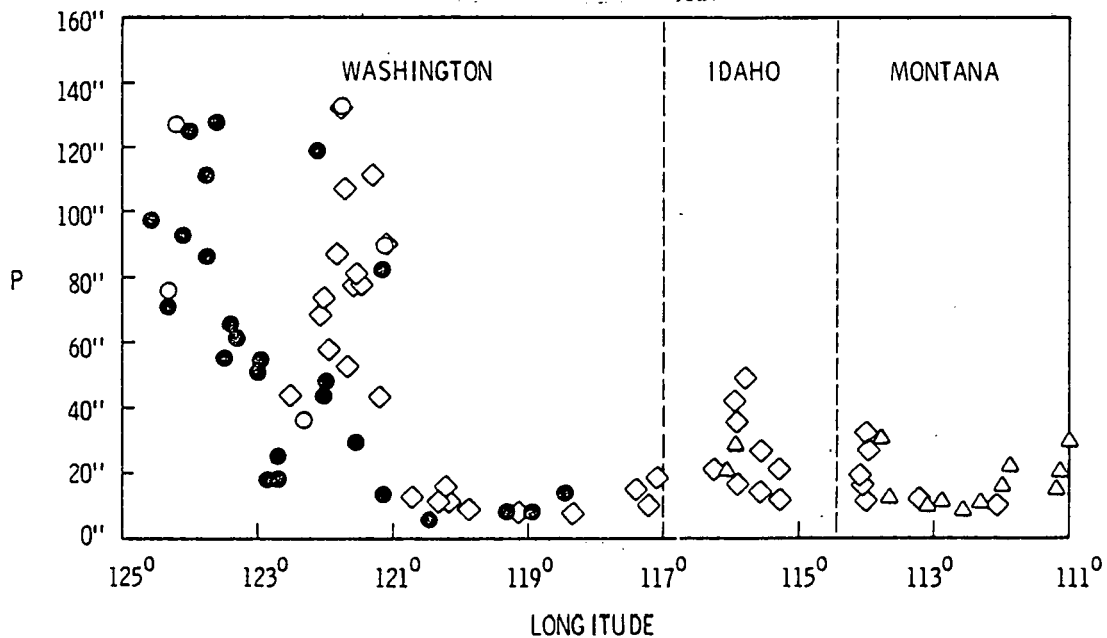


FIGURE 11. Average Annual Precipitation in Inches Between the 47-49 Parallels as a Function of Elevation and Latitudes in Northwestern United States

A still greater drop of precipitation east of the mountains would follow the eventual uplift of the mountain chain, whereas the removal of the mountain barrier would result in a higher precipitation in the interior.

5) Studies of Holocene (Bull, Menges and McFadden 1979) uplift in California suggest that no such changes in relief are expected in the Cascades in the next 50,000 years that could alter the present circulation pattern. Probability of such changes is low, but is significant in the interval of 50,000 to 100,000 years AP (after present) and becomes higher afterwards. Then the crustal movements are expected to change the morphology of the Cascades, the Rockies, and of the intermountain basins. Based on the the above observation and assumption and on the infrequent dated paleoclimatic evidence from the Columbia Plateau, we expect certain environmental stresses during the future natural climate states affecting the Columbia Plateau.

A. From 0 to 50,000 Years After Present (We expect no significant change of morphology by uplift.)

- a) Protostadials: Alpine glaciers up to about 2/3 of their full glacial extent (approximately 150 km upstream of Hanford).
 - No catastrophic floods of the Missoula type
 - Annual precipitation probability distribution according to Figure 10c
 - Ratio of runoff and recharge
- b) Temperate Interstadials
 - Moderate expansion of alpine glaciers with insignificant impact on the site
 - No catastrophic floods of Missoula type
 - Precipitation, runoff and recharge according to Figure 10a.
- c) Interstadials
 - Similar to the above, with precipitation, runoff and recharge according to Figure 10b.

- B. After 50,000 Years After Present (Significant alteration of morphology due to crustal movements is possible.)
- a) Interglacial
 - Negligible extent of alpine glaciers
 - No permafrost
 - Distribution of precipitation, runoff, ground-water recharge, etc. according to Figure 10d
 - b) Temperate Interstadials
 - Same as for B
 - Precipitation, etc. according to Figure 10e.
 - c) Interstadials
 - Expansion of alpine glaciers
 - Discrete possibility of Missoula-type floods
 - Precipitation, runoff, etc. according to Figure 10f
 - d) Protostadials
 - Similar to interstadials, but possibly with more extensive permafrost
 - Precipitation, runoff, etc. according to Figure 10g
 - e) Stadials
 - Expansion of alpine and piemont glaciers with a discrete probability of reaching the site
 - High probability of Missoula-type floods; at least 1 or 2 for each stadial
 - Extensive permafrost, profoundly affecting the ground-water recharge and surface erosion rates
 - Precipitation, etc. according to Figure 10h.

Conclusions

The above discussion and figures can only give a very general idea of what approach we took to estimate the possible climate-related stresses at a given site. The accuracy of such estimates is bound on numerous assumptions the validity of which it is impossible to test at this time. A more thorough discussion of the paleoclimatic evidence and its interpretation, as well as of

some of the key assumptions is contained in the earlier report (Kukla 1978), to which the interested reader is directed. Additional studies, partially underway, can lead to estimates of higher accuracy.

Still, it has to be remembered that projections into the future, of whatever nature, will always remain in the domain of highly controversial, educated guesses.

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VOLCANIC HAZARD STUDIES
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ABSTRACT

Volcanic hazards studies for the WISAP program are concerned with evaluation of risk due to future volcanism, with respect to long-term isolation of radioactive waste through deep geologic storage. Three major areas of research have been examined.

1. Regional distribution of Quaternary Volcanism with respect to repository siting.
2. The consequences or disruption effects due to penetration of a repository by volcanism.
3. Probability calculations of the likelihood of disruption of a repository by volcanism.

DISTRIBUTION

The distribution of Quaternary Volcanism in the United States defines discrete areas of volcanic provinces that are sites of concentrated activity. These areas are restricted to the western United States, west of the Rocky Mountains. They are relatively high-risk zones, with respect to volcanism, for siting of a waste repository. Examples from the Cascade volcanic chain, a continental arc, and the Snake River Plains are used to illustrate volcanic provinces and possible approaches that can be used to predict future volcanic activity within these areas.

CONSEQUENCES

The consequences of volcanic disruption are controlled primarily by the geometry of magma-repository intersection and the nature of volcanism. The geometry of magma-repository intersection is affected primarily by the subsurface geometry of magma feeder systems, which is a general reflection of magma compositions. It is additionally affected by depth of burial of a

radioactive-waste-vault site. The nature of volcanism is controlled by magma composition for the general sequence of basalt to rhyolite. This strongly affects eruptive styles (the nature and total energy of surface eruptions) and maximum dispersal distances of potentially incorporated, radioactive waste.

Consequence studies have been concerned largely with direct-disruption effects, that is, waste dispersal due to direct incorporation of radioactive waste by magma. It is likely that coupled secondary effects produced by perturbations of a geologic system by volcanic disruption (for example, disruption of ground-water flow system and stress-field changes) may be of considerable importance in long-term consequence analysis.

PROBABILITIES

Volcanic probability calculations are modeled as a case of conditional probability. Two parameters are required.

R = rate of volcanism

A = area ratio

The annual probability (P) of both occurrence and the intersection of a repository is then described by the relationship

$$P = \text{volcanic disruption} = R \times A$$

Establishing a rate of volcanism is the major problem for the probability approach. The calculation requires that past rates of volcanism, where definable, be projected into the future. Moreover, it requires the assumption that volcanism is a random process. Studies of known volcanically active areas of the world have shown that the occurrence of volcanism is strongly controlled by the existing structural regime. The area ratio can be approximated with some accuracy, although it requires some assumptions with respect to size and geometry of volcanic-disruption zones. The area ratio is defined as the area of a repository or the area of an appropriate disruption zone (whichever is larger) divided by the area for which the volcanic rate is determined.

NEVADA TEST SITE VOLCANIC RISK ASSESSMENT

The status of volcanic risk assessment for the Nevada Test Site area is described using the summarized WISAP methodology. The Nevada Test Site is cut by a Neogene volcanic belt extending from southern Death Valley, California to central Nevada. Discontinuous volcanic fields, ranging in age from about 6.0 m.y. to recent, of predominantly basaltic composition are present within the volcanic belt. Cone density of volcanic centers younger than 2.0 m.y. for the belt ranges from 10^{-3} to $10^{-4}/\text{km}^2$ for the Nevada Test Site area to $0.1/\text{km}^2$ for the Lunar Crater volcanic field. Cone density of the complete volcanic belt is about 10^{-2} to $10^{-3}/\text{km}^2$. The probability of disruption of a waste-repository site in the southwest NTS is bounded by the range 10^{-8} to $10^{-10}/\text{yr}$. These calculations were determined through field geologic mapping and chronology of eruptive centers using the K-Ar age determinations and the magnetic-polarity time scale. The calculations ignore the structural control of the localization of volcanism in the southern Great Basin. Future work will attempt to incorporate, statistically, the effects of structural control in the probability calculations. The area of contact of a basalt feeder dike, assuming direct intersection of a repository, is about $4.6 \times 10^{-3}/\text{km}^2$. Assuming a 10 km^2 repository and uniform distribution of radioactive waste within this area, the area of direct contact of magma and waste is about 0.046 percent. Magma-dispersal mechanisms are largely controlled by basaltic magma compositions. Major dispersal mechanisms are aerial ejection of pyroclastic debris (maximum distance about 30 to 50 km) and extrusion of relatively viscous lava flows of limited extent (less than 3 km).

SOME COMMENTS ON THE RATE AND AMOUNT OF DEFORMATION
IN THE PASCO BASIN

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ABSTRACT

The structure of the basalts in the Pasco Basin syncline beneath the cover of Ringold sediments has been determined from drill holes and seismic profiles. The oldest Ringold sediments are significantly deformed more or less in conformity with the basalts. Deformation waned progressively during the time of Ringold sedimentation and the uppermost beds are essentially horizontal. The latest faulting in this portion of the Columbia Plateau occurred both to the north and to the southeast of the Pasco Basin.

INTRODUCTION

This study is to determine the amount, kind, and rate of deformation of the basalts in the Pasco Basin, in the hope that from such data an estimate can be made of deformation that might occur during the next million years in this same area. The goal is to provide a safe site for radioactive waste disposal.

The plan is to define the types of structures within the basin area, such as folds and faults, and to interpret, if possible, the stress regime and tectonic setting responsible for these structures. An attempt is made to estimate the degree and amount of deformation. Finally, the timing of deformation is essential to this analysis. Late faulting is of utmost importance and so is the nature of the movements as they reveal the forces at work.

This is a plea to study the dating of the Ringold sediments; they provide a demonstration of the waning deformation over a period of several million years. It is also a plea to observe carefully the sense of movement on all fault planes and zones.

An extraordinary number of workers are now involved in geological and geophysical studies of the Pasco Basin. The data is coming in at a remarkable rate and needs evaluation to guide future studies. The greatest progress can be made by the free dissemination of knowledge to all parties studying the basin.

THE COLUMBIA PLATEAU AND PASCO BASIN

In June 1978 a draft report was submitted to Pacific Northwest Laboratory on the structural geology of the Columbia Plateau as related to the Waste Isolation Safety Assessment Program. Evaluation of the geology is continuing, but with a sharper focus on the Pasco Basin.

Geologic events such as faulting or folding do not lend themselves well to dating, as their timing depends on datable overlying and/or underlying sediments. Thus, the time of the event is usually bracketed between widely spaced mileposts. -I believe we are fortunate in the Pasco Basin to have a more or less continuous record of deformation represented during the deposition of the Ringold sediments. Full use can be made of the records from drilling and dating of these rocks.

We have a remarkable variety of structures surrounding the Columbia Plateau. The Cascade range to the west is a broad arch suggesting east-west compression. To the north, the Okanogan highlands are noted for their huge grabens with steep-walled sides. To the south the Basin and Range province is characterized by numerous block faults. In contrast, the high mountains to the east in Idaho are composed of large granite batholiths.

To interpret what structures prevail beneath the Columbia basalts, we may look to the edges of the plateau and carry those structures underneath the lavas. However, this can be done with confidence for only short distances. This same technique can be used for the Pasco Basin. Here we have a syncline partially filled with lacustrine, glacio-fluvial, fluvial, and loessal sediments that obscure all below. The large anticlinal ridges defining the edges of the Pasco Basin reveal the type of folds and faults that may exist within the syncline now covered by sediments. Fortunately many hundreds of drill holes

have penetrated the sediments and gone into the basalts. None have completely penetrated the basalt sequence in the basin and gone into the underlying basement rock. Recent magneto-telluric studies suggest that sediments may lie under the basalts.

Seismic lines, made as part of a nuclear-site investigation, provide a means of extrapolating between drill holes so contour lines can be made on the top of the basalt. On the north side of the Rattlesnake anticline toward the Cold Creek syncline, there is a slope on the basalt surface of about 200 ft to the mile. This is the type of hard evidence that we must use in defining the structure in the synclines.

The actual surface of the basalt is not a single flow. A series of off-laps indicates the flows were not as extensive in the later stages of volcanic activity. The amount of erosion that has modified this surface is not well understood. In gross features we can say that the top of the basalt gives us a very good indication of the type of structures we have in the basalts beneath the sediments.

From these data we can perhaps determine whether the deformation, in a kinematic sense, is of a decollement type, simple anticlines and synclines, or whether the ridges owe their origin to punched-up basement blocks.

The drill hole and seismic data on the basalts provide a basis for determining the amount and kind of deformation. The structure of the sediments overlying the basalts shed light on the timing of the deformation. Deformation started some 14 to 12 million years ago. The early deformation took place in widely scattered areas. To the north in the Yakima and Umptanum Ridges there was at least 200 ft of movement. The same is true to the south in the Horse Heaven Hills. Some early movement is also recorded in the Arlington area to the west along the Columbia River.

The greatest concentration of movement took place starting about eight million years ago and ending close to four million years ago. Probably the greatest amount of faulting occurred in this interval. Below is the total translational compression, as estimated by Laubscher (1977, pp. 1-26), between

the Columbia Hills anticline on the south and Saddle Mountains on the north. Total compression in the 115 km stretch is 3 km. Laubscher studied the kinematics of this portion of the Columbia Plateau.

Columbia Hills anticline	300 meters
Horse Heaven Hills	200 meters
Toppenish Ridge	200 meters
Rattlesnake anticline	300 meters
Yakima Ridge	400 meters
Umtanum Ridge	600 meters
Manatash anticline	800 meters
Saddle Mountains	230 meters

Shortening took place in the north-south stretch between the Saddle Mountains on the north and the Columbia Hills anticline on the south, a distance of 115 km. Such a shortening demands a certain amount of north-south compression. Although most of the shortening was taken up by folding, there is an unknown component taken up by faulting, mainly thrust faulting. The greatest offsets by faulting are found to the north of the Pasco Basin in Kittitas Valley. Younger cross faults cut these major thrusts and anticlines. They may be related to later movements in the basement rocks or to rotation of large blocks of basalt.

Recently Waitt (1979, pp. 1-8) found faulting just north of Ellensburg that occurred somewhere between 120,000 and 3.7 million years ago. The Dry Creek fault has a displacement of 15 m. Two others, the Reecer and Wilson Creek faults, are less evident and their presence is based on topographic and geologic evidence. These two probably average about 5 to 8 km in length and, together with the Dry Creek fault, provide the best examples of younger faulting.

The youngest faults of all are found to the southeast of the Pasco Basin just southeast of Wallula Gap. Swanson found recent faults with an offset of a meter or a little less (Waitt 1978). I have seen colluvium involved in the

faults and can attest to their young age. Near Wallula Gap is a trench that shows an offset of approximately 50 m. The estimated age of faulting is 15,000 to 30,000 yr plus. The overlying undisturbed beds probably are Kennewick conglomerate. These seem to be older than the Pasco gravels because of the thicker weathering rinds on the basalt pebbles.

A zone in the Columbia Plateau, called the Olympic-Wallowa lineament extends from the Wallowa Mountains in eastern Oregon to the Olympic Mountains in Washington and has been the subject of much debate. Raisz (1945) described it first as a topographic lineament. Others who have worked in Oregon along this lineament find little evidence of its presence. Still others see it, based primarily on geophysical information, as a controlling structure. Bentley (1977) described a stretch of this zone extending from Cle Elum to Wallula Gap as being 16 km wide and as having many faults. Most of the major anticlinal structures bend to the southeast as they come to this lineament. The cause, at present, is not clear. Some structural bending probably could be accounted for by rotation, but strike-slip motion seems a more plausible cause. All workers should look carefully at the fault planes and zones for data on the sense of motion along the faults, especially strike-slip motion. I have been many places where horizontal motion is indicated by gouge along the fault planes. It is more difficult to get data on the amount of offset with these strike-slip faults.

The dominant stress regime in the Columbia Plateau shows north-south compression. The kinematics argue not only for compression, but for a certain amount of strike-slip motion and for some rotation of blocks.

The picture becomes more complicated when the neighboring structures are taken into account. The Cascade Range to the west of the Columbia Plateau suggests it has been deformed by east-west compression. The Basin and Range province to the south suggests tension on a broad scale with major transgressive zones, such as the Brothers fault, slicing diagonally across Oregon. Timing, of course, is critical in attempting to fit these diverse structures into a coherent pattern.

Timing is also important in assessing the rate of deformation. It seems appropriate to make a special plea for the Ringold formation because it transgresses time rather continuously, and fortunately it occupies the section where we need it most, the upper portion of the time spectrum.

The age of the Ringold has been determined from assemblages of vertebrate fossils, giving a span of several million years, beginning about 3.5 m.y. ago. Remnant magnetism studies indicate ages ranging from 700,000 yr to 2.3 m.y. The lowermost Ringold beds in the Pasco Basin are deformed more or less conformably with the basalts on which they rest. However, as one progresses stratigraphically upward, the Ringold sediments are less deformed. The uppermost beds of the Ringold sediments are less deformed. The uppermost beds of the Ringold in the White Bluffs are essentially horizontal.

The same anticlines and synclines in the basalts of the Pasco Basin trend toward and pass under the White Bluffs. Hence we have a demonstration of the lessening of deformation over a period of approximately two million years. This continuum of time, in the stratigraphic sense, provides a basis for estimating the rate of deformation. The thrust of the WISAP study is to gather a data base so the computer can help determine the probable sequence of geologic events for the next million years.

CONCLUSIONS

The Ringold Formation provides a basis for an evaluation of the rate of deformation in the Pasco Basin for the last one to three million years ago.

Deformation of the basalts began 14 to 12 million years ago and reached its zenith four to eight million years ago. Later deformation, primarily in the form of faulting, took place 120,000 years ago to 3.7 million years ago in the Kittitas Valley and "yesterday," in geologic sense, to the southeast of Wallula Gap.

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COMMENTS

This might be an appropriate time to elaborate on the work that has been done here in the past two years and elaborate a bit on the problems that Dr. Coombs and others have mentioned. More than 250 boreholes have been drilled in the Pasco Basin down to the top of the basalt. We have run reflection seismic surveys across the basin in addition to those that Dr. Coombs pointed out, and we have conducted aero-magnetic surveys over certain parts of the basin. The boreholes, in conjunction with the reflective seismic survey, have enabled us to work out structure-contour maps on the top of the basalt. The structure-contour map is very similar to the one that Dr. Coombs showed you. The stratigraphy of the basalts is "predictable." Once you get down to the top layer, knowing the continuity of the stratigraphic relationships, you can predict the depths of the successive horizons below that point. Using your structure-contour map on the top of the basalt and known stratigraphic relationship of bores, you can predict structure-contour maps on the deeper layers. This has also been done.

Surface mapping around the basin is, by and large, completed now. The geologic map of the Pasco Basin is in the process of being printed. The bedrock outcrops that Dr. Coombs mentioned have been mapped to a scale of 1:24,000 and the master has been compiled at 1:62.5. The bedrock structures can be projected from the bedrock outcrops into the same areas where they are covered by those sediments. Using the aeromagnetic map, we can trace the continuity of the bedrock structures through the fill, and we are in the process of working out the continuity of the bedrock structures beneath the fill.

As to the age of deformation, I would like to just agree with, and amplify, the importance of the Ringold. The age of the latest flow in the Pasco Basin is approximately eight million years. We have just done some magnetic-stratigraphic work on the Ringold, and its age is 3.5 million years, based on the current interpretation. The Ringold consists of four units: the basalt, lower, middle, and upper Ringold unit. As Dr. Coombs mentioned, those lower Ringold units are deformed in a similar geometric fashion with the underlying basalt, and it is in the upper Ringold unit that at this time we see no deformation. By the time you get to the middle Ringold in the section, the deformation is largely dying out. The age of the Ringold is now interpreted as being roughly 3.5 million years. We know that above the Ringold there are glacio-fluvial sediments, and those are very young, on the order of a few 10 thousands of years, except for a few dates that have recently been made to show some older glacio-fluvial episodes.

SEISMIC HAZARD ASSESSMENT
TECHNIQUES AND WISAP

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ABSTRACT

The challenge of seismic design for underground repositories which have a design life of 10^6 years is critically analyzed. The problem is decomposed into two separate issues: vibratory excitation and rupture displacement. Models are proposed for the assessment of each of these hazards and preliminary results from these models are provided.

In this paper, we will discuss earthquakes in terms of their ground motion and surface rupture. Of particular interest, in assessing the safety of underground repositories, is the pathlength or "magical descriptor." In the following, we suggest a general methodology by which the probabilities for ground motion and faulting can be calculated. While tremendous uncertainties are inherent in it, we believe that the WISAP program has the capability to surmount these uncertainties.

DISCUSSION

As Dr. Howard Coombs of the University of Washington has indicated, major risks could likely result from earthquakes on undetected features. Also, Dr. Bruce Crowe of LASL has indirectly introduced the general problem of risk or hazard assessment through his studies of volcanoes. Hazard assessment of volcanoes is quite similar to the hazard/risk assessment of earthquakes, in that both types of geologic phenomena share a number of common features. First, at some time in the future, both will likely be subject to man's control. Additionally, and more technically, both volcanoes and earthquakes suggest occurrence models that can usually be described statistically. There

are size variations for both phenomena that also have statistical definition. In both cases, hazard depends upon distance from the source. Finally, both are physical manifestations of activity beneath the earth's surface. One important distinction between earthquakes and volcanoes is the way in which they differ according to the size and dimension of the surface feature; that is, a volcanic eruption is the result of a much greater instability and is, therefore, more apparent than an earthquake, which may not rupture from the surface.

A number of interesting things about earthquakes could be very important to WISAP: How does an earthquake start? What are the physical processes of rupture initiation and crack propagation? What is the effect of rupture propagation on peak accelerations and surface rupture? Currently, studies are being conducted on "rupture chatter" which seems to indicate the source of peak accelerations. It might also be interesting to consider a large earthquake compared to a small earthquake, though most seismologists agree that a large earthquake is simply a progression of small earthquakes. The logical event tree shown in Figure 1 illustrates one approach to some of these complex seismological problems.

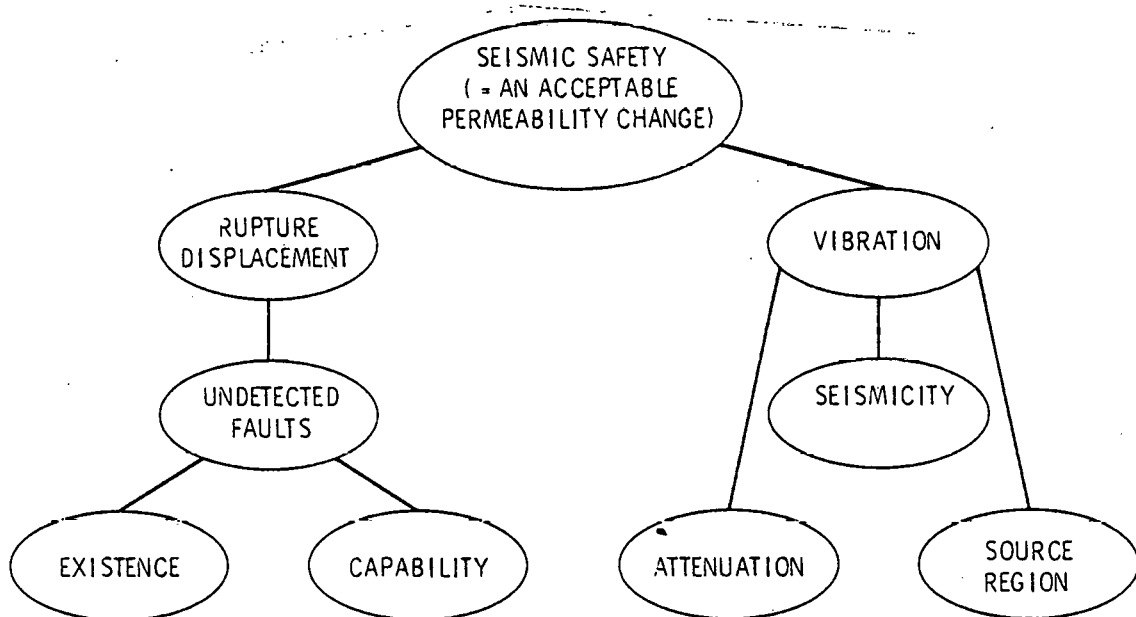
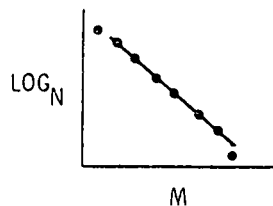


FIGURE 1. Decomposition of Seismic Safety

Though most of these problems remain without solution as yet, statistics based upon earthquake data provide sufficient information for general probabilistic analyses of the risk and hazard associated with earthquakes. It has been stated that the important features of earthquakes, compared to volcanoes, follow a size distribution (Figure 2). This distribution seems to be consistent independently of the regions of occurrence (assuming that there are enough data within that region to allow any sort of statistical fit). The term "region" includes areas, zones, and specific structures such as the San Andreas Fault. With regard to the San Andreas Fault, the size distribution of earthquakes can be plotted along a straight line. This is also true of the size distribution plots of many of the earthquakes in the New Madrid area of southern Missouri, an area in which there is no recognized faulting, yet many earthquakes. Size distribution is usually presented in the form of a graph, both because there are many more small earthquakes than big ones, and because certain of their properties seem to vary with time. These statements are based upon historical records. Instrumental records date back to around 1930; historical intensity records in the United States date back 175 to 200 years.

- SIZE DISTRIBUTION



- HISTORICAL RECORD (1000 yrs) COMPLEMENTED BY GEOLOGIC RECORD

- VARIABILITY OF EFFECT

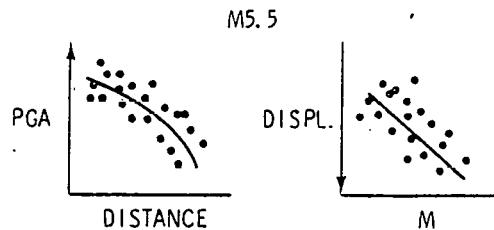


FIGURE 2. Types of Earthquake Statistics Used in Seismic Analyses

In other parts of the world the historical fault-intensity records are much, much longer, and this has been factored into our analysis. Records in China, for example, which date back 4,000 years, have been the subject of many recent scientific papers. As nearly as can be determined, these historical records--both noninstrumental and instrumental--are complemented by certain geologic parameters. The focus of this paper thus becomes something called the "slip rate" (synonymous with what Dr. Coombs calls the "strain rate"). In cases where direct comparison has been possible, it can be seen that the earthquake occurrence models that have been developed from slip rates follow quite closely the occurrence models derived from actual data.

The variability of the effects of earthquakes is important to the schematic analysis procedure. Peak acceleration is plotted in Figure 2 as a function of distance over a certain magnitude range. The factor of two on certainty (a mean multiplicative factor) is significant. The magnitude-rupture displacement data are also shown schematically. The scatter in this data is far greater than the scatter in the acceleration data, and it appears to be a factor of ten in certain types of earthquakes. This scatter is present even though as much of that bias as possible is removed by data partitioning. For example, the plots might be represented over a certain magnitude range, certain focal mechanisms and a certain depth, and recorded and observed at a certain site. Enormous uncertainties reflect our lack of understanding of the physical processes. Incidentally, this acceleration factor of two is, most probably, a direct function of the direction of the earthquake rupture. This relationship is supported by a recent computer-model calculation analysis performed to address the effect of earthquake activity.

At this point, it might be helpful to distinguish between hazards and risks. A hazard has to do with the probability of the occurrence of an event, and a risk is the consequence of that event. This distinction is important as it is possible that a large hazard might be associated with a small risk.

The overall approach proposed here, as outlined in Figure 1, is a partitioning of the "system" into components that most directly influence changes of permeability. Note that the problem has been reduced to two

components: vibratory excitation and rupture displacement. Rupture displacement might be characterized in many ways, though this analysis emphasizes the rupture displacement from unknown features. In considering both vibratory excitation and rupture displacement, the methods that WISAP should employ are the probabilistic hazard-assessment techniques conventionally applied to critical facilities.

Any seismic-hazard assessment involves the issues suggested in Figure 3, which shows the San Francisco area with the major earthquake faults superimposed. The first task, then, is to define boundaries to faults, or areas similar to these; on the basis of historical seismicity and geophysics. Where the seismicity cannot be assigned to known faults, a background area must be defined. The assumption inherent in the definition of these zones is that future seismicity will be uniform and homogeneous within each zone. The next task is to develop future seismicity models for each zone, including specifications of the largest earthquake possible. The final task is to develop a transfer function to specify the effects, at the site being considered, as a function of magnitude and distance.

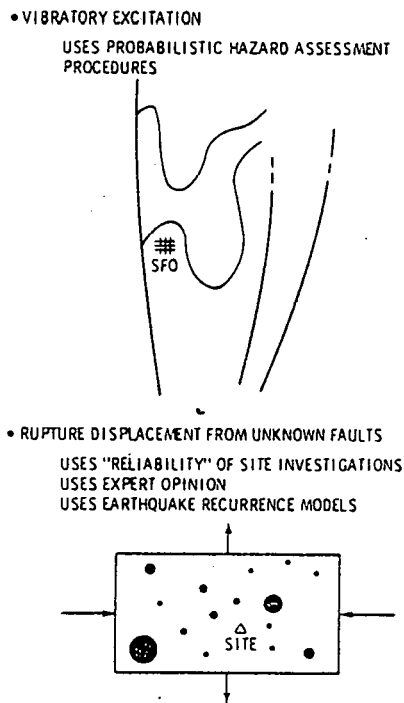


FIGURE 3. Approach for Seismic Hazard Assessment

Clearly, a key element to the problems of rupture displacement from unknown faults can be termed "reliability of site investigation" since the accuracy of the hazard assessment will be a direct and strong function of the site investigation. Certain data permit quantification of that reliability, but every site investigation will be "unreliable" to some extent. It should be clear, therefore, that some definition of acceptable reliability must be developed. This definition will require a careful analysis of the consequences of accepting some degree of unreliability.

The risk associated with the vibratory excitation of underground repositories is very low, with the possible exception of very large, very close earthquakes. Even for those exceptional cases, technology is available for appropriate design criteria. It is, of course, important that any analysis be repository- and site-specific.

Undetected faults pose a considerable challenge to geologists and geophysicists. The following is a summary of undetected faults.

- Faults will always be undetected - the challenge is to:
 - decide what size fault can be neglected, and
 - ensure that site investigations will identify all other faults.
- Seismotectonic data coupled with models for the reliability of site investigations permit an analysis of the undetected fault.

A good introduction to this subject is work done by John Brooke of San Jose State University, who simply counted faults on maps in the California and Nevada areas. He found that on the average, there was one fault within each ten square kilometers. Considering this density, the odds are high--at least in California and Nevada--that a fault might be missed. Geotechnical Engineers, Inc. conducted an analysis, after reading through safety analysis reports for nuclear power plants, and compared the borings for foundation studies with the actual geology revealed by the excavation. The statistics were startling. Many unknown features of various sizes, found in the excavated foundation, had been previously undetected. In both cases, the sites had been subjected to the usual geologic investigations prior to

construction and operation; however, possible faults were discovered after the sites were subjected to more sophisticated investigations than were performed at the time of the initial investigation.

The problem of undetected features is difficult anywhere--nuclear power plants, dam sites, etc. And the problem becomes almost inconceivably difficult at a repository.

First, the depth of a possible fault must be considered. It is not possible, as at a nuclear power plant, to simply drill the site. This is known as destructive testing. Therefore, any investigation must be indirect. Presuming that there will always be undetected features, and more specifically (depending on the definition), that there will always be undetected faults, the crux of the matter becomes fault size. Once likely features have been determined, the extent of possible rupture displacement becomes significant to the design of a repository. An appropriate parameter upon which the design should be based is the probable displacement postulated as a consequence of that feature. The criteria must ultimately be set in accordance with the results of the cost-benefit analysis. This analysis would provide, at least, a definition of the class of features that could be ignored in the site-selection process.

A second element to be considered involves the geologic slip rate. As has been stated previously, coupling slip rate with reliability of site investigation addresses the potential of these undetected features. A calculation derived from data from the Hanford Reservation indicates that the probability that an undetected fault intersects a repository is something less than 10^{-8} .

Based upon experience at the Nevada Test Site (NTS), which is the site of extensive underground nuclear testing, vibratory excitation should not be a major parameter in design and decision-making because:

- a well designed repository should easily withstand moderate events;

- depending on design, long duration-high excitation motion could cause cracking at repository walls; and
- probabilities of excitation level can readily be derived from a seismic hazard analysis.

There is an interesting design problem at the NTS, where, as is often the case in the vicinity of an underground test, the hardware inside an open borehole has to be protected. This is considered to be a routine matter. As another case in point, Professor Charles Dowding, while he was at MIT, collected all the available data on the damage of underground structures from earthquakes. He found that in only a few cases out of 100 could any damage be documented, and, in almost all cases, this could be attributed to rupture displacement rather than excitation. This is consistent with both theory and experiment, which predict that the intensity of excitation decreases with depth. (One of the more intriguing theories has to do with the effect of the free-surface and the near-surface constructive and destructive interference with different modes of energy.)

TERA has conducted several analyses for the Hanford Reservation in particular, and one such analysis follows. The Hanford Reservation (Figure 4) is surrounded by several seismic source regions. These source regions, which are assumed to be regions of uniform seismicity, are noted for their distinct historical seismicity. Region 1 is the Puget Sound area, Region 3 covers part of the Cascades mountain range, Region 2 represents the area around Lake Chelan, and Region 4 covers Milton-Freewater. The basis for this representation of future seismicity is not only the historical seismic record, but also the geology and geophysics of the area. Another very important source region, which is not shown here, is a background region that covers the entire Columbia Plateau. This region represents the seismicity that cannot be attributed to any of the other sources.

An analysis was performed for these source regions using a particular attenuation relationship that predicts peak acceleration as a function of magnitude and distance. The results are shown in Figure 5, which represents acceleration exceedence probability. As part of a sensitivity analysis, this

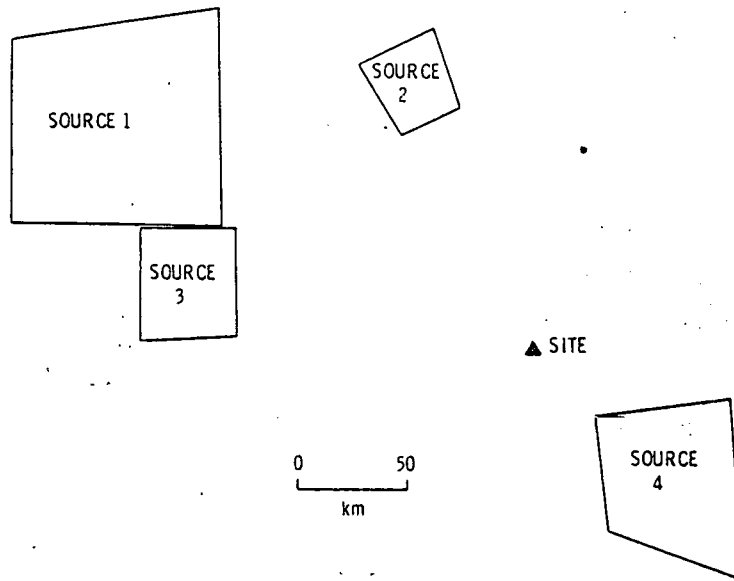


FIGURE 4. Low Seismicity Regions Around the Hanford Site

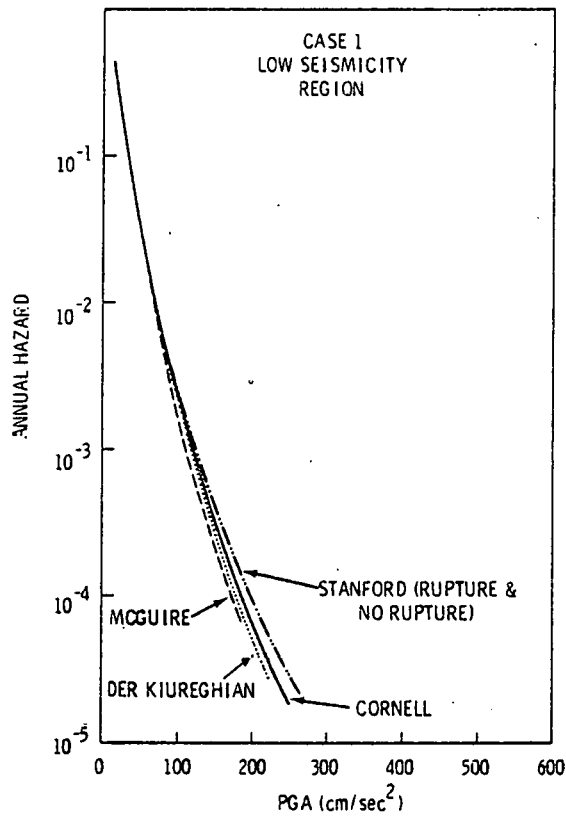


FIGURE 5. Seismic Hazard for the Hanford Site

analysis was performed in accordance with several different methodologies. These results, which are also presented here, appear to be rather independent of the methodology in that they all predict that, at about 20% g, there is a 10^{-4} annual hazard. These results include all of the uncertainty inherent in certain input parameters, and the results should be interpreted as "expected values" (rather than, for example, "50th percentile" estimate).

The preceding has addressed the probabilities associated with excitation, and it remains to consider the probabilities of ground displacement or surface rupture. A qualitative feeling for the reliability of site-investigation techniques can be obtained by comparing microseismic data with surface geology. To introduce this aspect of the problem, it is first necessary to consider the reliability of two common site-investigation techniques: the surface geological mapping and microseismic surveillance. Evaluation of these site-investigation techniques involved reviewing and comparing the data available from 29 microseismic arrays against the previously mapped surface geology. (Summaries of selected investigations follow.)

In 1970, a swarm of earthquakes occurred near the Calaveras fault, (Figure 6) which is part of the San Andreas system near Danville, California, east of San Francisco. These earthquakes, which occurred over a short time and which were all rather small (less than magnitude 4.5), were subjected to detailed investigation. Instruments were immediately deployed, earth models refined, and hypocentral solutions determined. In Figure 6, a cross-section of the location of these micro-earthquakes is shown relative to surface location and depth projection of the Calaveras fault. The notable lack of agreement could possibly be explained by the uncertainty in the depth projection of the Calaveras, though a more likely explanation is that the swarm did not occur on the Calaveras, but rather, on an unmapped fault.

In the case of the Briones Hills earthquakes, which occurred in California two years ago, the University of California immediately deployed portable equipment to monitor the aftershocks. This resulted in very accurate locations. Figure 7 shows the epicentral location of the swarm relative to the several geologic structures that had been previously mapped. Based on the

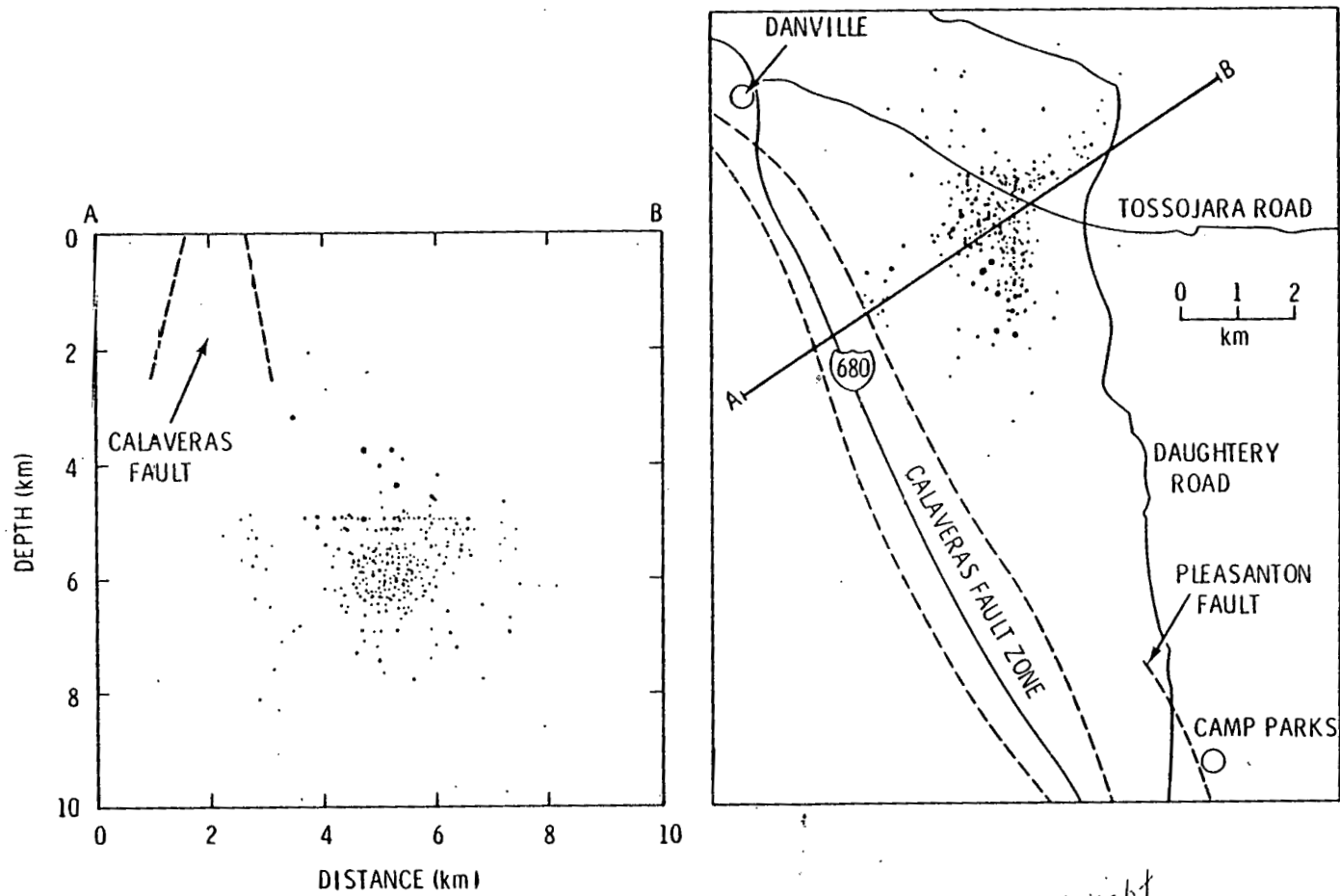


FIGURE 6. Microseismic Activity Along the Calaveras Fault Near Danville, California

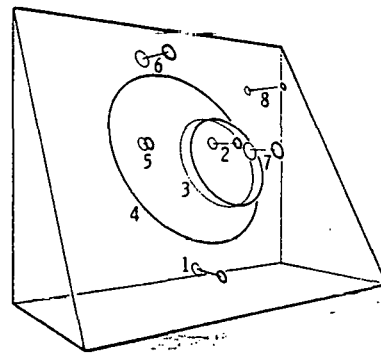
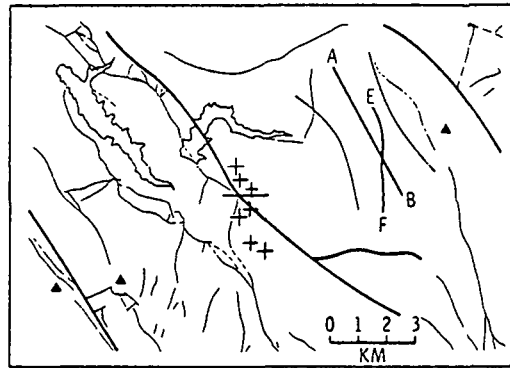


FIGURE 7. Epicentral Locations of the Briones Hills, California Earthquakes

epicentral data, it was assumed that the swarm was associated with one of the previously mapped faults. University of California seismologists performed some source mechanism studies on the swarm and presented the results in a three-dimensional plot of focal mechanism and location. As Figure 7 shows, the data define a plane through which the swarm apparently occurred. Note that the projection of this plane to surface, even when potential analysis errors are included, completely misses the suspected faults. Subsequent investigations established the existence of a previously unknown fault (indicated as EF in the figure).

Perhaps most relevant to WISAP is the micro-earthquake monitoring program of the Columbia Plateau area. Earthquakes reported here (Figure 8) are very, very shallow, possibly occurring in the basalt, and they appear in the form of clusters. Three particular clusters were analyzed in this paper. Comparing

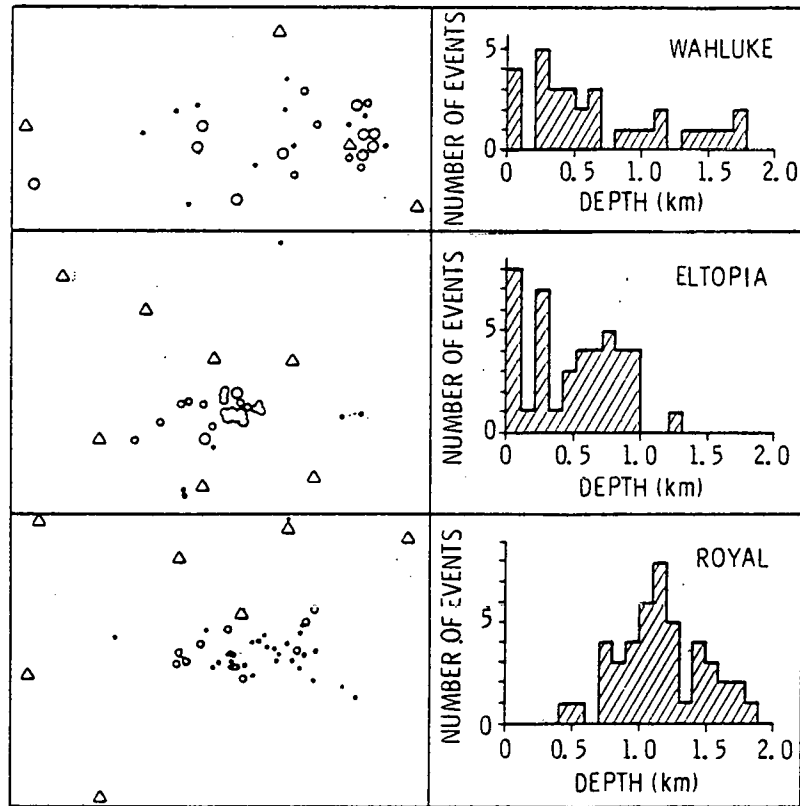
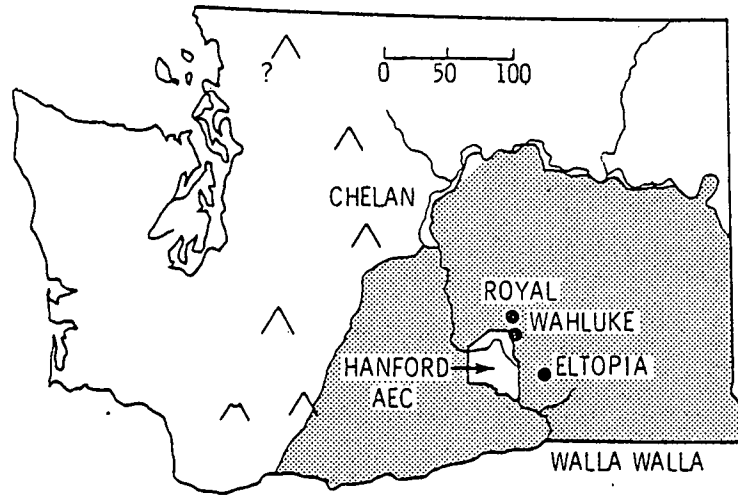


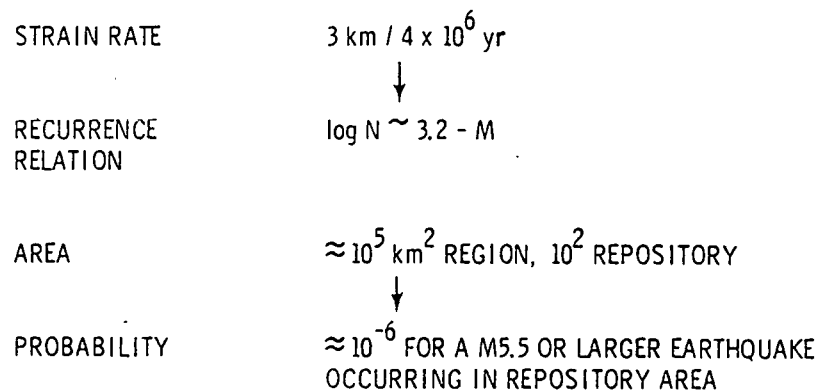
FIGURE 8. Micro-Earthquake Swarms in the Columbia Basin, Washington

the locations of these earthquakes to certain geologies showed no obvious correlation. This brings up an issue that will be discussed later, i.e., that there are two kinds of earthquakes: those that occur in basalt layers and those that occur in sub-basalt basements.

The three examples above were presented to emphasize the obvious--each investigation technique has advantages and disadvantages, and a properly planned program must be based on an appropriate suite of techniques.

As has been previously stated, the undetected feature problem can be discussed in terms of both existence and capability. It might be possible to infer the existence of undetected features from certain reliability statements based upon data such as have been presented here, though statements must also be made regarding the capabilities of these features. One appropriate way to derive such statements would be to consider certain seismic data and geologic slip rates and infer an earthquake-occurrence model. By way of demonstration, consider the worst case, or the largest strain rate, for the Columbia Plateau (Figure 9). Analogous to the approach proposed for volcano hazard assessment, conditional probability of faulting through the site is assigned by calculating the ratio of the site area to the background source area. As shown in Figure 9, the annual probability is calculated to be approximately 10^{-6} for a magnitude 5.5 earthquake. (The probability is correspondingly higher for small-magnitude earthquakes, and lower for large-magnitude earthquakes.) Magnitude 5.5 is particularly significant because the resulting displacement measures in inches, and can be as much as a foot. With regard to the annual probability, the accumulated exposure to a repository, over a 10^6 year life, is over 0.6; that is, this calculation represents a greater than 50% probability that rupture will occur within a 10^2 km area in the Columbia Plateau, within the next 10^6 years.

A number of very simplified and conservative assumptions have been made here. Most important, this approach does not take into account the quality or the reliability of site investigation, and nowhere is the attempt made to screen the sites more closely than to the (average) 10^2 kilometer area.



GROUND DISPLACEMENT DURING THIS M5.5 EARTHQUAKE WOULD BE
1-10 INCHES

FIGURE 9. Worst Case Probabilistic Fault Rupture Prediction
for the Hanford Site from an Undetected Fault

Occurrence probabilities are difficult to determine, not because of the absence of a conceptual approach, but because of the lack of precise data. In illustration of the concept, consider Figure 10. Imagine that this entire area has been subjected to a detailed investigation and that, as a result of the investigation, it is possible to estimate how much better the site is than average. Also shown in this figure is frequency versus displacement, but since displacement is proportional to magnitude, this can be taken to mean magnitude. This plot suggests that site investigations result in a preferential removal of the large earthquakes and, therefore, in a filtering of the background earthquakes for the site area.

A more complete approach is being developed in which many other probabilities (such as sizes and locations of ruptures for a given magnitude) are considered. Accumulating all conditional probabilities (including site-investigation reliability), the resulting curve, which looks very much like the one for vibratory excitation, represents the hazard associated with displacement for different magnitudes. As an order-of-magnitude estimate, it is probable that any site investigation of reasonable quality would screen out enough earthquakes to get the annual probability down to 10^{-8} .

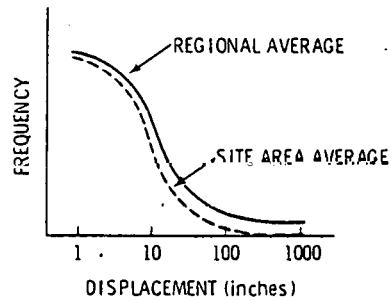
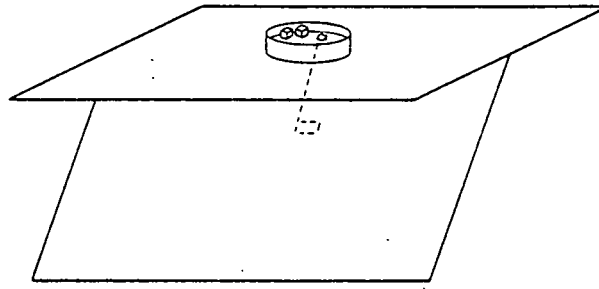


FIGURE 10. Illustration of Seismic Occurrence Probability Relationships

In conclusion, the assessment of seismic hazard at an underground repository over its life of 10^6 years involves substantial uncertainty. Many of the generic approaches presented here will resolve some of this uncertainty, but substantial uncertainty will remain, and the strength of the WISAP approach is that it explicitly acknowledges and accounts for this uncertainty.

QUESTIONS AND ANSWERS

- Q. Could you say what the difference is in all those?
- A. Yes. Basically, its differences are in the detailed components in each model. For example, some models include a rupture model; others include a model for the rupture surface. The uncertainties are treated in a variety of ways, and there are a variety of probabilistic bases. One model, for example, is based on Bayesian statistics. It is comforting that, in spite of the variety of approaches, they all yield approximately the same result.

Q. I have a comment I was waiting for Ralph Kehle to make. One of the most active variants, in terms of fault displacement, is the least active seismically, and that is the Gulf Coast, so you wouldn't dare use this as a generic model to look at all the possible sites in the United States. We tend to get the West Coast mentality involved and directly equate earthquakes with faults or faults with earthquakes; such is not the case.

A. I should have emphasized that at the beginning. You're quite correct, since the method hinges on the availability of slip rate or strain rate data. Such data is generally not available for the eastern United States.

Q. What is the validity of using the occurrence relation as predictive over periods of over 100 years?

A. Recall that the proposed methodology requires the use of slip rate or strain rate data in the analysis. These geologic parameters, which represent the tectonics of an area or a structure over a time frame of several million years, are then used to develop a seismicity recurrence relation. Thus, the methodology does not explicitly require that the seismic record be used in the analysis although, of course, it should be used to check the model seismicity. I have compared such geologically-based seismicity models with the actual historical record and found an excellent comparison.

I find that rather comforting. Furthermore, I should mention the 4000-year seismic history in China and, on the average, the occurrence of earthquakes there is statistically very stable. So I think if one factors in appropriate scientific data, one can make those extrapolations. Now there's one thing I ought to say, of course, and it is an underlying assumption. We have assumed that tectonic processes don't change, and in a million years I think that's a relatively fair assumption.

I might expand on his question of undetected faults in the subsurface and explain how we see this problem within a basalt environment. The approach that we've used to explore the Pasco Basin is that first you map

the basalt surface exposures to generate the surface you'd like to map. Once that's done, you inject the surface relations into the subsurface where the seismic fill covers the bedrock. Then you use geophysics to link between boreholes to make those surfaces together. That more or less gives you an overview coverage of this basin within which you're looking for a specific site. In geophysics, primarily with your boreholes, you are projecting surface geology. Then you select, say, smaller areas within that larger area for more detailed analysis. There are a number of tools in existence at this time that can give you a higher degree of reliability, so you will not overlook an undetected fault beyond a certain size. Such tools as multi-level aeromagnetic surveys and grid-reflection seismic surveys all offer a high reliability to detect structures in the basin. To go one step beyond that, we go to boreholes, log those boreholes, and get fundamental physical properties from the rocks. From logging those boreholes you can get boreholes to surface geophysics, boreholes to borehole geophysics, and you can get a greater degree of reliability for detecting the undetected fault. You then sink a shaft and go out in an exploratory fashion. You can look out in front on the head of that shaft, physically go between shaft headings, and from the shaft to the surface, and you have then a greater reliability of detecting the undetected fault. That, coupled with the knowledge that, geologically, faults tend to set in trends and sets and on certain structural paths, would help you use this randomness that you referred to.

PACIFIC NORTHWEST LABORATORY,
WASTE ISOLATION SAFETY ASSESSMENT PROGRAM

Dr. M. A. Harwell

I would like to outline some of the areas we are interested in. We would like feedback and input from anyone who has some ideas.

The first thing that we intend to do is to finish the package on this model, as we have mentioned earlier. We want to add the statistics part of the scenario model and complete it, based upon basalt geology. Included in the statistical package are some of the things that Jim Stottlemire talked about in terms of the variable-importance analysis aspect. In addition, we're starting to look at variance reduction techniques, which is a statistical-biasing technique, trying to find out some of these variable-probability events that are incorporated in the model. There are ways of showing variance without having to go through a million Monte Carlo simulations. Associated with that are our uncertainty-bounds analyses. We have some statisticians beginning to look at error propagation and at the ways that will enable us to assign some degree of certainty on the response space that we come up with.

In completing the package and getting it ready for the Monte Carlo load, we also are involved in reducing the computer time. One aspect of that is making it a batch type of process and transferring it to machines that are not interactive but will number-crunch more quickly than our interactive system. We're looking at using different time steps. Some of the phenomena that are involved in our models have slower rates of occurrence than others. There may be a difference of several orders of magnitude in the time response of some of the components, which leads to stiff systems. We may look for ways to pick out some of the components and not iterate them in 100-year intervals, but in longer intervals if there is no possibility of significant changes in those stiff components during the shorter steps. Also, we're thinking of reducing time steps as the simulation goes along. We may have more frequent time steps initially and, as we get further out in time, use longer steps. Perhaps by combinations of these we will be able to reduce the CPU time involved.

In the next year we'll also be thinking of how to start incorporating the near-field aspects--I prefer the term waste-induced aspects--explicitly into our model. We're starting scoping studies in the next few months to figure out what it is we need to include.

We're beginning to look at (but not in terms of the model) the human-intrusion aspect. Originally, we included human-intrusion submodels that were being generated in a random fashion. We decided to pull those out of the model. We're looking for ways to treat the human aspects qualitatively; that will minimize the dependence of whatever we come up with on the particular state of predicting human technology over these times.

We are also going to be looking at incorporating geochemical changes, and Bill Deutsch will talk about how he wants to approach this. We want to understand the geochemistry and how it is important to the consequence of safety assessment, and we need to identify how our geochemistry will change over time.

Just a basic point in dealing with the question of thoroughness: we need to know when we have included enough things and what criteria to use to limit what we're including in the model. We continue to evaluate that internally and to try to figure out if there are areas that we're not covering that we should be covering. Through the process of site application, where we exercise entire WISAP methodology, some areas are going to be brought up that we have not yet included. That's one aspect. The other key aspect of it is literally an iterative process. Again, we welcome any ideas or things that should be included that we're not including, and any ideas on how to approach the question of when is enough, enough.

Now I'll turn it over to Bill Deutsch, who will talk about the geochemical aspects that we want to include in the model.

GEOCHEMISTRY AND THE RELEASE SCENARIO ANALYSIS TASK

William J. Deutsch

Pacific Northwest Laboratory

ABSTRACT

The release scenario analysis of a waste repository must include possible changes in the geochemistry of the system because of the importance of the geochemistry in both the dissolution rate of the waste and in the interaction between radionuclide-laden ground water and host rock. The system will be studied by determining the limits of the effect that various natural perturbations will have on the geochemical parameters determined to be of importance.

INTRODUCTION

One area of geoscience that has yet to be incorporated in the release scenario analysis task is geochemistry. By geochemistry, I mean the chemistry of the ground water in the vicinity of the repository: ionic content, pH, Eh, temperature and other characteristic parameters. Also included is the rock chemistry and mineralogy of the host medium surrounding the repository. Not only must the geochemical system be well established at the time of emplacement of nuclear waste but some determination must be made as to how the geochemistry will change in response to natural perturbations over the time period of waste containment. As will be discussed, it is very important to know what the conditions are at the time of a breach of the repository.

THREE MAJOR PROCESSES OF GEOCHEMISTRY

The importance of geochemistry is based on the effect of three major chemical processes on the system:

- the leaching of waste material in the repository by ground water percolating through a breach,
- the retardation of the radionuclides as they move in solution through the host rock--retardation is due in part to ion adsorption on the various mineral phases;

- the changing flow conditions in the host rock due to precipitation of secondary minerals or to the removal of material caused by weathering along the faults and joints.

THE EFFECTS OF THE CHEMISTRY OF THE SYSTEM

I will show that the chemistry of the system will greatly affect these processes. The rate of removal of material from a breached repository will be limited by the solubility of the various elements. Figure 1 (Rai, Serne and Moore 1979) shows how the solubility of plutonium changes with pH, and as you can see, the solubility changes by approximately an order of magnitude for each unit of pH. We believe that the other elements in the repository will react in a similar way. The importance of this is that, in order to know the dissolution rate of the waste material, we need to know the pH of the solution at the time of the breach. We may be able to determine the pH under present-day conditions by sampling, but we also need to put bounds on how that pH, Eh, or whatever, will change over the time period under consideration.

As these solutions move into the host rock, they will react with the host rock in this new environment. Retardation of the radionuclides can be estimated by measuring the K_d , or distribution coefficient, of the various elements. The K_d , as experimentally measured, is determined by reacting a solution of known concentration with a rock or mineral of interest. After equilibration, the concentration is again measured in the solution. The K_d is simply the ratio between the amount of nuclide sorbed on the solid divided by the amount remaining in solution. Consequently, as the adsorption increases, the K_d increases. And, as a ballpark figure, a K_d of 1 means that a particular element will be retarded by a factor of 4 over the rate of flow. Adsorption is basically a surface process, and, as such, it will be affected by both the water chemistry and the rock mineralogy.

Figure 2 (Meyer, Beall and Shiao 1979) shows the effect of water chemistry on the K_d of strontium on montmorillonite. As the ion content of other ions in solution increases (in this case sodium), the distribution coefficient of the strontium decreases. Here it decreases by two orders of magnitude for each order of magnitude increase in the sodium concentration. This is mainly due to competition for the various sites.

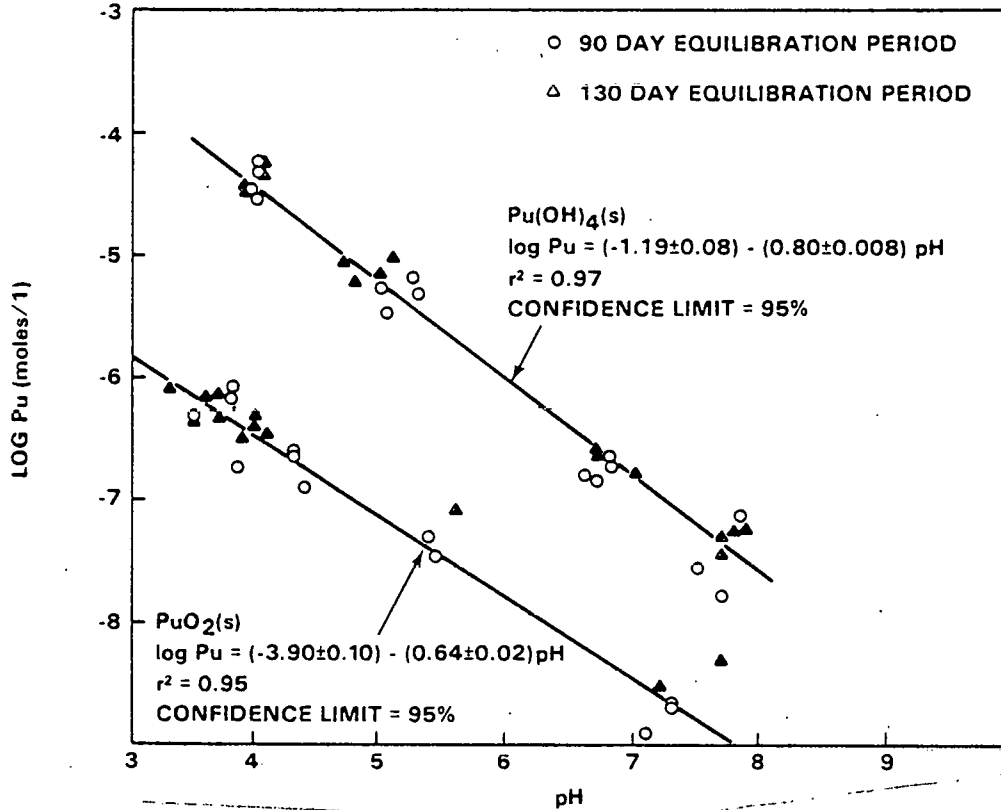


FIGURE 1. Concentration of Pu in Filtered ($0.1 \mu\text{m}$) Solutions Contacting $^{239}\text{PuO}_2(\text{s})$ and $^{239}\text{Pu(OH)}_4(\text{s})$

Mineralogy will also affect the K_d of the system. Figure 3 (Relyea et al. 1977) shows typical sodium groundwater. The K_d of Sr can vary by two orders of magnitude, depending on which mineral species are present. Consequently, we need to have a good idea what minerals we can expect these solutions to contact. The site managers can give us some idea of what the mineralogy is at the present, but we also need to estimate how that mineral suite may change with time.

To recapitulate, for the release scenario analysis task it is important for us to know the earth's chemistry. Specifically, we need four things:

- a general environmental description of whatever area is to be used,

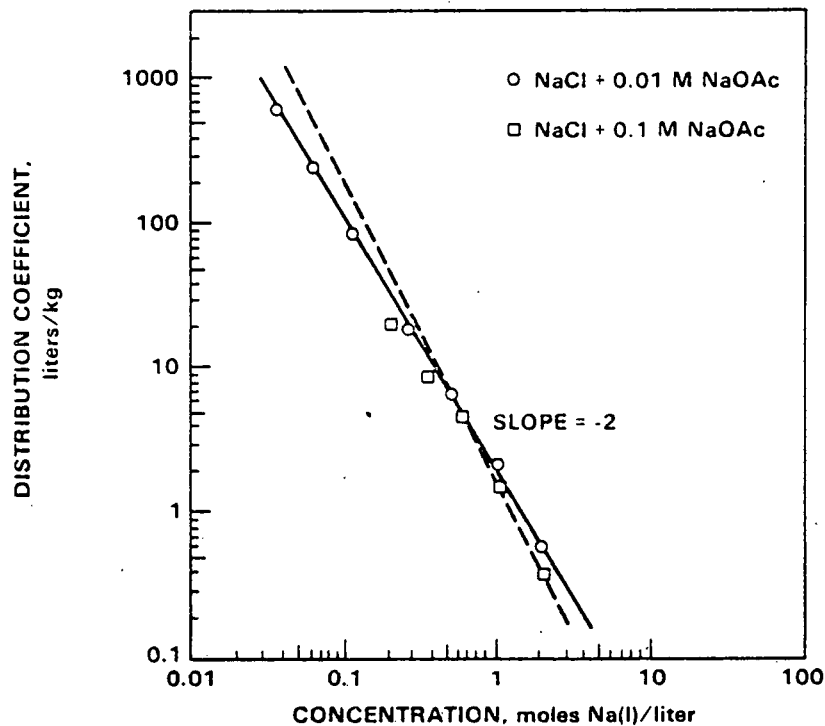


FIGURE 2. Adsorption of Sr(II) on the Sodium Form of Wyoming Montmorillonite (Loading: 6×10^{-5} - 2×10^{-2} M Sr(II)/kg, pH 5, Equilibrium for 45 hr)

- to know the chemical parameters of the solutions that are going to be leaching waste material (in order to do realistic leach rate analyses),
- to know, not only the chemistry of the solution, but the mineralogy of the host rock in adsorption-desorption studies,
- because minerals may be precipitated in cracks, and fractures, and may be removed in those systems due to weathering, we need to know the solution chemistry. It has an impact on the hydrology of the system.

The approach of Task 1 will be to do a regional characterization of each area based on data provided by the various site managers. Secondly, we will be looking into how changing environmental conditions are going to affect the water chemistry and mineralogy.

EQUILIBRATION TIME - 10 DAYS

<u>MINERAL</u>	<u>NaCl BRINE</u>	<u>Ca G.W.</u>	<u>Na G.W.</u>	<u>HCO₃ G.W.</u>
ILLITE	1.4 ± 0.3	6.6 ± 0.7	63.7 ± 6.0	487 ± 35
KAOLINITE	0.6 ± 1.0	0.7 ± 0.3	79.9 ± 7.7	205 ± 20
MONTMORILLONITE	0.9 ± 0.3	28.2 ± 2.0	1,155 ± 82	1,237 ± 146
VERMICULITE	1.7 ± 1.3	25.7 ± 2.2	988 ± 178	1,190 ± 242
QUARTZ	1.0 ± 1.5	1.0 ± 0.7	10 ± 1.5	507 ± 6
ALBITE	4.4 ± 0.7	4.6 ± 0.9	128 ± 7	486 ± 52
ANORTHITE	3.1 ± 0.8	3.6 ± 0.1	63.2 ± 4.2	422 ± 23
MICROCLINE	3.5 ± 0.3	3.2 ± 0.5	194 ± 12	501 ± 37

FIGURE 3. Batch K_{dSr} Results (Average of 3 Results)

We will need to extrapolate the experimentally determined leach rates, and obtain numbers for actual field conditions. Finally, we will synthesize our geochemistry with a hydrologic transport model.

FUTURE STUDY

In the near future, our development will be data acquisition from the various site managers and a study of ground-water variability; that is, how it can vary depending on rock type and various climatic factors. We will need to determine weathering series for the various host rocks under consideration to determine what sort of secondary minerals to expect in the future. We will need to get a handle on weathering rates to determine not only what minerals are going to be present, but to get some idea of the quantity. We're looking into precipitation products in fractures and joints to determine their effect on fracture healing. Also we'll be looking at other reactions between radionuclides and host rock. Some of the radionuclides may be incorporated into stable secondary minerals, immobilizing them; whereas others may form ion complexes which would mobilize them at a higher rate than we would normally consider from the simple K_d measurements. We also need to determine the adsorptive capacities of the entire system. Figure 4 (Meyer, Beall and Shiao

1979) shows that the distribution coefficient of cobalt decreases as the concentration of the cobalt increases in the solution. This is mainly due to a loading effect. The cobalt has assumed position on all available sites, and consequently there can be no retardation of cobalt by the particular medium. The final step will be to determine the best way to integrate the geochemistry with the overall assessment model.

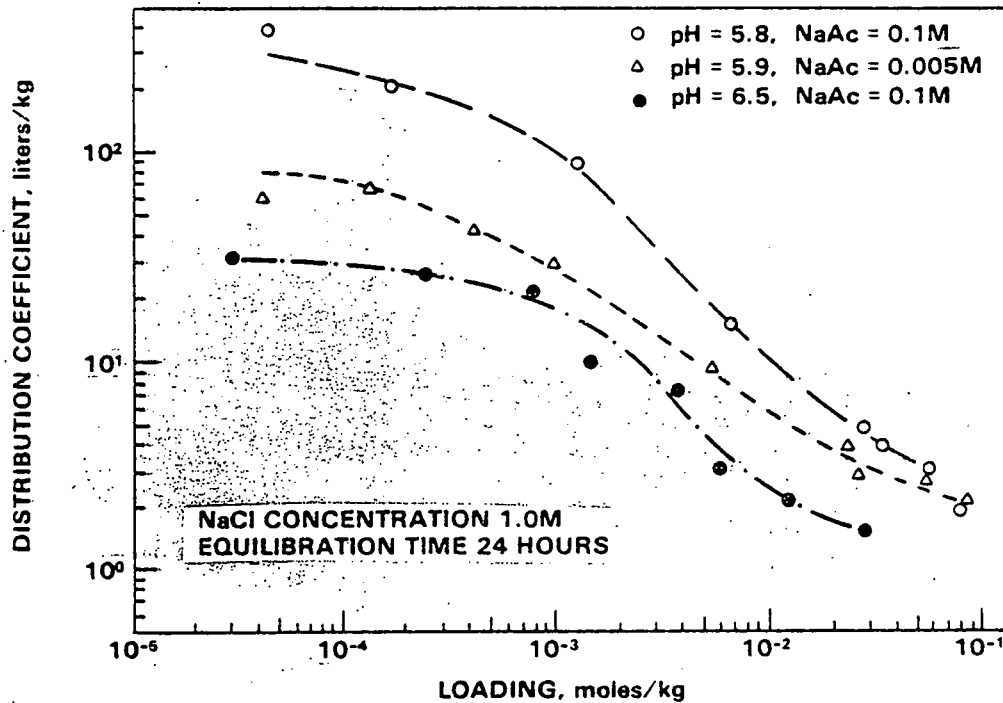


FIGURE 4. Loading Effect on Distribution Coefficient of Cobalt

CONCLUSION

We will be characterizing the present-day geochemical systems using data provided by the local site managers. The measured parameters may change with time due to natural perturbations, and we will estimate the magnitude of the effect of these events and processes. As I've attempted to indicate, small changes in the chemistry may have large effects on the integrity of the repository. A decision as to whether a particular breach will lead to a failure of the system may depend, to a large degree, on the geochemistry of the system.

QUESTIONS AND ANSWERS

- Q. What environmental conditions are going to affect the water chemistry and mineralogy? Increased temperatures, for example?
- A. Whatever the changes may be. We're interested in how external conditions may influence the subsurface. Any conditions which are going to change the leaching of the repository or the host rock.
- Q. I noticed you did not include the effects upon ground-water chemistry of having engineered barriers between the waste form and the host rock. As an example, you showed the effects of cobalt binding up all the available host rock sites. Some of the materials being considered as engineered barriers are as exotic as cobalt and I'm quite concerned that a more conservative approach might not be looked at, which assumes no absorptive capacity. I'd like to see that be looked at as a possibility.
- A. I'm sure the model can be run with a K_d value of zero to see, in fact, if the system can contain a breach at that level. I don't think it's that important to talk about whether cobalt happens to be present or not. It's a matter of quantity of the other minerals at the sites; it's more a total water chemistry that I would look at.
- Q. Relative to the last comment, how would you characterize current uncertainties in predicting either solubilities or retardation factors?
- A. Well, we don't know a lot about solubility. We can rank the natural systems, but to determine what the situation of the repository is Let me sidestep that a little bit and suggest that you ask the people who will be holding the Task 4 Workshop, October 15-17, that question, as they're dealing with that problem.
- Q. Is diffusion treated somewhere else? You didn't list the effect of diffusion from a fracture surface into the matrix of the rock, which is, of course, entirely possible for a great many of these migrating species.

- A. Can I put this in a different context? What we are trying to outline here required about a month to scope out, that is, to address a changing geochemical environment and put some bounds on it. For the next couple months we will be continuing that scoping study and will start the next fiscal year by actually doing the work. There are a wide variety of things we haven't addressed yet, and diffusion is probably one of them. We are beginning to discover that the geochemical area tends to fall within one of these gray areas. A comment was made yesterday about organizations that bind you and then don't allow you to solve the problem. To some degree we're finding that in our programs, and are attempting to deal with that problem. As such, we have experimental tasks actually measuring K_{ds} , actually measuring leach rates, and other data-base development work. We also have the Task 1 and Task 3 groups trying to analyze what might happen to the system if the release occurs. This requires the data tasks feeding information back into the system. We found that we were both assuming the other side was really worrying about characterizing that geochemical environment. So this is a new phase in the Task 1 work. We're going to try to step into that gray area and start looking more closely at the geochemical environment.
- Q. I don't think there's any argument that diffusion will be taking place as the aqueous solutions migrate, with retardation taking place and all of those things. Diffusion into the matrix of the rock is a very important part of the migrating process.
- A. I think you'll find the radionuclide transport models do have provision for transfer diffusion and that Jeff Serne's Task 4, as well, is doing some lab experiments to address that. I don't know if it's a top-priority item in the WISAP program, but I know it is one of them.
- Q. I didn't see a bulletin describing scaling problems. Are you going to address these problems? While I'm on the subject, we found that crack flow and some impermeable flow in some of the materials that we tested

simply cannot be addressed accurately by using distribution coefficients. I think Dr. Segol addressed that earlier. How about the scaling problems?

- A. This is something that gets over into the Task 3 of WISAP. We do have some efforts going on in the methodology development. Part of that task is starting to address those problems, but it's out of the scope of the scenario task. The same is true of all diffusion. We have some specific work going on looking at the level and importance of diffusion and the migration process and some parametric types of studies and some theoretical types of studies. These barriers were not within the scope until just recently.

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OPEN FLOOR DISCUSSION - DAY 2

- Q: Concerning permafrost, I have one trivial remark and one rather important one. What happens to the depth of the permafrost when the glacier comes along, i.e, is the permafrost thickness greater or less under glaciation? It is less, because the temperature distribution in the ice is controlled largely by the air temperature, which decreases with increasing elevation by about 1 per 100 m. Permafrost accumulation is also affected by the temperature gradient in the ice, which rises roughly one degree per 44 m. So the warmer the temperature, the thicker the permafrost. That's the trivial part. Now the important part: how is it that the glaciological consultant and the task force leader forgot to consider permafrost in developing the model? We did that some years ago. One hour ago, to my embarrassment, I realized that we have done nothing about it in this study. Would you see to it that it is included?
- C. Something I might point out, which may not be clear, is that the Task 1 program does not do any field work. Most of our information is taken from the existing literature; we're dependent upon input from other groups.

I certainly hope that in the future, prior to our convening as a large workshop, we will receive the feedback that Wes Myers from Rockwell has provided this afternoon. We'd like to get critical appraisals earlier in the game, so that we can adapt our methodology accordingly. Again, we are not charged or funded to go out and do any field work. We'd love to go to northern Washington and see if we can correlate any of the faulting structures with possible glacial loading, but that's outside the provisions of our charter. We can only identify the potential problem and have gentlemen like Colin Bull and Dr. Park of Cornell do a literature search, talk to some of their colleagues, and use their expertise to discuss these things in more detail. We then depend on geologic project managers and site investigators to go out and actually look for that type of information.

- Q. Have solubility curves been worked out for all the major ions that we're expecting, including the fission products? Have any been worked out for elevated temperatures? I know all of them have been done for low temperatures.
- A. Right. They haven't been worked out at high temperatures.
- A. Bill was using an example of the type of data we're generating in Task 4. Task 1 has not developed any of these data, nor is it implementing experimental procedures. The Task 4 staff does have plans to start monitoring elevated temperature and pressure effects on solubility and Kd values, in general. They have been looking at a large number of different radioisotopes with different mineral types and ground waters. That effort will continue as we get increasingly site-specific.
- Q. When you said that dissolution of waste was limited by leachability, were you referring to dissolution of the waste form itself -- actual physical dissolution - or did you mean that the availability of nuclides is limited by solubility levels?
- A. Let me try to rephrase your question. You're asking whether we are leach- or solubility-limited in a repository situation. Is that your question?
- Q. If you're talking about availability of nuclides, that's the question. If you're talking about actual disappearance of waste form and probability change, the question has no meaning.
- A. Probability is going to be a scenario-specific situation, and it's premature for us now to say we're going to be under solubility or leach restraints.
- Q. Or allow yourself to be under those restraints, either. You were not saying only solubility?
- A. No, we're not. In experimental work to date, Task 2 has been focusing on leach rates in a nonequilibrium situation, so we're not solubility-limited. We are initiating static leach tests designed to find out what

solubility limitations there are in the waste form in contact with simulated ground waters and in contact with some representative rock types. These tests are just getting under way now, so we're going to be looking at both aspects.

- Q. Are there any data on the permeability or hydrologic effects of faulting? Some estimates suggest a great increase in permeability, while some suggest a significant decrease in permeability. Do you expect the sites to provide that kind of data, or do you guys get out and hustle the system for that?
- A. We're not really funded to provide those data. We can only identify the need for such data and make suggestions to the various groups. Obtaining pertinent data is quite a problem. We're going to need numbers. We have probably exhausted the data from the available literature on some of those things.
- Q. Would the experts on borehole sealing in basalt give me a quick summary of the preferred approach to this activity?
- A. Again, we're not doing any experimentation to come up with methods of sealing boreholes. We identify classical pathways and the types of permeability you might expect in a borehole that has failed because of either acoustical mismatch or degradation of the sealing material. As to what to put down the borehole to seal it, I would have to direct that question to people who have been actually doing that work. We can only put in a range of values and choose from that range of values, based on a random probability of having a failed borehole. We base the probabilities on various seismic occurrences.
- C. Most of that work is being done at Penn State University, where scientists are looking at both crushed-rock backfill, hydrothermal deposition, various types of hydrothermal cements, and the like. They released a report on the matter about a year ago.

- A. Three consulting firms have compiled reports on failed boreholes. The Oak Ridge report and workshop, in particular, received much attention. If you take, for example, a hydraulic gradient of about 0.001 and put two boreholes into the repository, it becomes evident that filling those boreholes with fine sand will not cause enough flux through that system to cause problems over the long run. Unless you have a lot of boreholes, or unless you have some large shafts and a lot of problems with them, or unless you have tremendous head differences, the borehole plugging problem is overstated.

Some of the reference-site analyses indicate that most borehole failures tend to give a release, but the release tends to be a small one. It is a matter of outlining limits in the licensing process.

- A. I heard you say that at one other meeting. Since then, I did some calculations and tried the idea on a group of university students. We were just sitting around talking, they thought about it for a minute, then asked what the borehole technology would be a hundred years from now. Why aren't the boreholes 10 meters in diameter? That points out why you can't keep the staff members on the staff for more than about a year.
- A. I assume you're all aware of the work the Swedes have done on the KBS program. They made considerable claims for their mixture of 85% quartz and 15% bentonite--American bentonite. Apparently, enough criticism was directed at that work, which was largely a laboratory investigation, that the Swedes, plus four other countries in Europe, are going to do a backfill experiment. It'll be designed in 1980, and is planned to run for three subsequent years, a total of four years in the Stripa project. They will be looking at how to plug openings up to 5 meters in diameter. Implanting sand and quartz and bentonite in a very dense form--which is what they claim they're going to do--turns out to be quite a field problem. Putting this material around a canister in a borehole is another problem. Finland, Switzerland, and Sweden, especially, believe that a great deal of effort has to be directed toward the backfilling

problem of holes of all sizes - from the shallow sizes (six inches) up to five meters. It is considered a very tough problem, and they don't seem to agree with you. They're willing to conduct a four-year project that's costing an estimated \$10 million.

A. I'm not arguing the public's apprehension, or even the apprehension in the technical world, about the problem of borehole plugging in the United States. I think we have probably spent four or five times as much on borehole plugging research to date as has all of northern Europe.

Q. In the field?

A. Unfortunately not; it's all been in the laboratory. We do it incidentally all the time in the field. Whenever we stimulate an oil well with hydraulic well fracturing, we have the opportunity to test that particular scheme of plugging wells. About one out of three fails, and the only way we can get the well back in production is to wash the sand out of the hole. Even if the sand is only 100 feet above the perforation, and we create a differential of 3000 to 4000 psi across that, no fault develops. So, in effect, we're testing the procedure.

A. I'm well aware of the oil industry activities, having started work in that field 40 years ago. I'll talk with you later.

A. The Nevada Test Site has been plugging holes from nuclear explosions for years.

Q. Shall we talk about some migration that's occurred?

A. I'd like to limit the discussion . . . to the main workshop topic.

A. One comment and a word of caution here, when you say it's a very small release and, therefore, you don't have to worry about it. The current version of EPA standards is that, in 10,000 years, you will release no more than 1 curie. That's mighty low, so don't discount little things.

A. I don't think Gary was implying that WISAP has had an evaluation and decided that borehole plugging is not a problem.

Q. Before we end the discussion on boreholes, let me pose one more question for the geologists here. Do you believe that the bentonite sand or the techniques applied in granite are likely to apply to basalt, because of similarities in rock mechanics? Or, are they dissimilar enough that we may need a different program?

A. In that regard, we're looking at a number of alternative techniques for hole plugging. We have done a lot of screening and have identified a succession of materials and material properties that look promising. We're talking about holes or shafts up to 30 feet in diameter, where you must also be concerned with removal of the casing, sidewalls, and so forth, to get down to bare rock. Under these circumstances, implantation presents both mechanical and hydrological problems.

Q. Is the granite work applicable to the basalt work?

A. I would say no.

Q. I keep hearing questions about your development of the WISAP methodology. It seems to me that people from the audience are looking to you for the answers, rather than for the methods to get there. I interpret your role as providing a tool by which the experts, the people in the field, can input their range of data for a specific site. If they know they have a fault going through a particular zone, they can input their own parameters, taking into account a specific fault's permeability. The field scientists can make measurements and predictions, and specify the data's range. Identifying the range is not your task. Some members of the audience seem to think that it's your job to come up with these exact parameters, to input to the model, and that all that they do is sit back and run it. It's the same thing with borehole plugging. What are you guys doing on borehole plugging? Well, you're really doing nothing more than saying it's an escape avenue, put in your own parameters! This whole philosophy follows through the methodology. You're not charged with providing the answers. Rather, you're designing a tool which we--the audience--can use to determine the answers for ourselves.

- A. You just took my ending remarks. When would you like to go to work on WISAP.
- C. I've been trying politely to suggest that, at some level, we have to get a little bit closer to phenomena instead of just applying a number of hydraulic resistances and providing a nonphenomenological approach to developing the leakage paths for something such as boreholes. I suspect that one of the main arguments for the development of this formula was the potential interaction of phenomena. If the modeling is not done on a phenomenological basis, then you've defeated one of the major justifications for this type of model.
- A. I'm impressed with the degree to which you are dealing with phenomena. After reading the report and hearing the presentations, I think that this is an order-of-magnitude improvement over previous attempts to model disruptive phenomena. You are dealing with real phenomena -- that's my impression. You've got details to fill in, and maybe borehole plugging is one of them, but overall, I'm impressed with the completeness.
- A. Thank you. And I agree. We're at a very infant stage, but we are dealing with phenomena. As a matter of fact, I believe that we've been putting more emphasis on geologic phenomena than on the mathematical details of the methodology. You are correct that there are things we have to fill in. We're definitely going to look at those gaps.
- Q. A few moments ago, when he was talking about future studies, Mark said that the package model would be completed. You can read that two ways. One, you will complete the methodology and description of the model, and that will be all. Or two, you will complete the package on the model by implementing and testing it at a site. Which is it going to be?
- A. When I say we'll complete the model next year, we're going to have an intact package, which then will be ready for site application and the whole Monte Carlo scheme. The model will develop over time; we'll update, correct, and change it, adding and deleting portions over time. The model will never be complete in an absolute sense. At some point

we'll have a package ready for site applications. Perhaps we'll transfer it to some other contractors, while we continue developing the model. Presently, all we have is a model on a computer that's up and running. You can interact with it now, but we dare not apply it to the Hanford Site, because we know it's not appropriate: it doesn't have all the data, and it doesn't have all the statistical package needed to handle the Monte Carlo. In answer to your question, we're going to have a package available for site applications, but at the moment it's still a milestone of an evolving methodology.

Q. Won't the model evolve in terms of its complexity, if the methodology is applied in regions other than the one in which it was developed? Are any of the submodels going to change? Do you add new submodels that accommodate characteristics specific to New Mexico or Louisiana or South Carolina?

A. Yes. You add new submodels and you alter the system-response rules of your existing submodels, because the system is responding differently.

A. So, when we go to bedded salt, we'll use some of the things that are incorporated in this basalt model, but largely we'll restructure things and add or delete whatever is necessary.

A. I think "the model" is an inappropriate term. It is actually a framework that allows us to take a tremendous amount of data, whether it be from Rockwell or other entities, and put it in a pigeonhole, if you will, in some sort of a logical fashion, so we can go back and find it. If we want, we can pull something out and put something else in its place. (Next week we'll go back and put in different things to keep Colin happy.) That's hard to do if all we've got is an occasionally-published report. We went to computer methodology, so that we could add a little bit of logic, a little bit of systematic referencing, to all the data you've given us. That's all "the model" is.

A. Analytically, the problem is very broad. You have to take into account a lot of variables. Some of them may be very dramatic driving parts of the

system. For those issues on which public attitude is focused, you may have to prove or disprove an action's supposed effect on the system. In formulating this tool--this model--we have broken the problem down into a set of small, definable, workable units. If you talk to our consultants during the social hour in the next few minutes, they will probably tell you that we ask a lot of difficult questions. I mean, it's difficult for Colin Bull, who is essentially a glaciologist, to have the expertise to really describe all the faulting mechanisms or how a glacier may affect the regional stress state, and what effect that may have on hydrology. So we need to break the inquiry apart, and let a consulting hydrologist, like Dr. Crosby, handle the hydrology questions, given Colin's parameters. Next, we'll take our model onsite. We'll go to the people who collect the data, who pick up the rocks, who really know the place. We'll get more sophisticated by adding another level of detail. Once we've gathered the hard data, perhaps we will find that some aspects of the model are untestable. (At this point, our site studies are quasi-generic. And so it's easy to modify the model in the face of new data.) Once we identify something that is truly a driving force, we intend to pass this information on to the site managers, so that they can restructure or reorient their exploration program to better help us qualify and quantify the phenomena. Some of these changes may be very significant.

We are charged with the responsibility of convincing others of the importance of obtaining quantifiable data for purposes of analysis. We need to be able to tell someone who studies salt domes in the Gulf Coast why it is we want him to spend \$3 million on gathering numbers. He has spent months or years orienting the drilling for an exploration program and doesn't want a laboratory scientist to walk off the street and tell him that he's got to change it. For the same reason, when we apply for an operating license, we must be prepared for a lot of hard and embarrassing questions. It will not suffice to answer questions with unsubstantiated opinions or in terms of what we think is or is not important. The licensing board will be tempted to go out and get another

expert. (Reactor licensing is a current case in point: experts are being pitted against experts with no end in sight.) When we apply for a license, we want to present a framework that incorporates geologically realistic data. That means we can't take the uplift of the Colorado Plateau, the maximum downcutting of the Colorado River, and add the two together to come up with a river erosion rate. That's not realistic. You deal with one number; you can't add them.

This new framework provides a systematic way of determining a range of values. And if some data don't fall within that specified range, then we will have to question, by way of a very strict review process and consultation with a wide variety of geoscientists, how it is that the incongruence exists. If the experts can create a theory or a mechanism describing how certain excluded data should be included, then we'll probably have to go in and alter a submodel or add another component to our model. Finally, in testing to see what effect the changes have on the model, we shall look at the entire system's response, not just the immediate unit's response.

- Q. When will the model, including equations and codes, be available for closer scrutiny?
- A. Probably this time next year. We hope to have it ready for internal review in a couple of months and then start a revision cycle, followed by another input cycle. After this processing, we should be prepared for general review.
- C. Let me qualify that. Gary's timetable is based on our using hypothetical data and example submodels. Rockwell, or any of the other site managers, is not going to feel comfortable with us coming out and running a specific assessment of its site until it has had more time to analyze our preliminary work. We can demonstrate the methodology and show input and output, but it's going to be hypothetical and have caveats on every page.
- Q. When can I start reviewing DOE's work and fulfilling my role as an advisor?

- A. Again, sometime next year. By then we plan to have the methodological framework and statistics in hand. Of course, when we get down to applying it to a site, we will probably have to make some adjustments.
- C. As Gary indicated, instead of concentrating on model development at this time, we are putting together a working document that contains specific equations and identifies the appropriate interdisciplinary foci. We'll have to look into what the clearance procedure will be on that.
- A. This afternoon we have heard from two people who have conducted field studies. Those of us who haven't had that benefit are probably rolling dice. Bob Williams asked me what field evidence we've got for faults that were actually produced by the stress differentials at the edge of an ice sheet. The only example I know of is this thing in Searsport. Let me redirect Bob's question: Is there any field evidence that some faults in the northern part of Washington are caused by stress distributions and differential loading?
- A. None that we know of.
- Q. Have you looked?
- A. Oh, I've been looking, yes.
- A. Let me just respond first. At our request, the area outside the Pasco Basin has been mapped on a regional scale of 1:250,000. We were on the lookout for closed stratigraphy, and so forth. Most of the mappings were directed at the basalt itself. With geology and many fields of scientific endeavor, however, too discriminating and selective a search can lead to misrepresentation or overemphasis or even failure to identify certain important elements.
- C. With that question I'm afraid we have to adjourn for today. As you know this concludes the formal presentations and discussions about the WISAP Release Scenarios Methodology. I do encourage all the participants to continue their discussions following the meeting and to respond with any written comments that seem appropriate. I would like to thank you for your interest and participation on behalf of myself as WISAP Task 1 leader and WISAP as a whole.

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