ORIGINAL

TIC

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

IN THE MATTER OF:

SUBCOMMITTEE MEETING

on

GENERAL ELECTRIC TEST REACTOR

Place - Burlingame, California

Date - Wednesday, November 14, 1979 **Pages** 1 - 331

1462 001

Telephone:
(202) 347-3700

ACE - FEDERAL REPORTERS, INC.

Official Reporters

444 North Capitol Street
Washington, D.C. 20001

NATIONWIDE COVERAGE - DAILY

7912030 005

10 CFR
PT9.7

CR8180

PUBLIC NOTICE BY THE

UNITED STATES NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Wednesday, November 14, 1979

The contents of this stenographic transcript of the proceedings of the United States Nuclear Regulatory Commission's Advisory Committee on Reactor Safeguards (ACRS), as reported herein, is an uncorrected record of the discussions recorded at the meeting held on the above date.

No member of the ACRS Staff and no participant at this meeting accepts any responsibility for errors or inaccuracies of statement or data contained in this transcript.

1462 002

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE MEETING

on

GENERAL ELECTRIC TEST REACTOR

Woodside Room,
Airport Marina Motel,
Burlingame, California.

Wednesday, November 14, 1979.

The ACRS Subcommittee on the General Electric Test Reactor met, pursuant to notice, at 8:30 a.m., Prof. William Kerr, Chairman, presiding.

BEFORE:

Prof. William Kerr, Chairman.

DR. DAVID OKRENT, Member.

DR. J. CARSON MARK, Member.

CONSULTANTS:

Messrs. T. Pickel, M. White, S. Philbrick, G. Thompson, P. Pomeroy, and J. Maxwell.

DESIGNATED FEDERAL EMPLOYEE:

Elpidio Igne.

1462 003

C O N T E N T S

ebl

P R O C E E D I N G S

PROF. KERR: The meeting will come to order.

This is a public meeting of the Advisory Committee on Reactor Safeguards, specifically the Subcommittee on the General Electric Test Reactor.

My name is William Kerr. I'm Subcommittee Chairman. On my right is Dr. Carson Mark and on his right, Dr. David Okrent, who are also members of the Subcommittee.

Present today as consultants are also Messrs. Philbrick, Thompson, Maxwell, Pomeroy, Pickel and White.

The purpose of the meeting is to review geologic and seismologic data having to do with the General Electric Test Reactor site. The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act and the Government in the Sunshine Act, and all other applicable laws and regulations.

Mr. Elpidio Igne is the Designated Federal Employee for the meeting.

Rules for participation of been announced as part of the notice of the meeting published in the <u>Federal Register</u> of October 30th of this year.

A transcript of the meeting is being kept. Each speaker, therefore, should identify himself and if microphones are reasonably readily available, should try to use the microphone.

eb2

Incidentally, can you hear me from this mike?

We have received requests for oral presentations from Mrs. Helen Hubbard.

Is Mrs. Hubbard here?

And from Mr. Andrew Ball.

If it is convenient for you I would like to schedule those just before lunch. I believe each of you has asked for about ten minutes.

We will proceed with the meeting. The schedule calls for a brief executive session. The purpose of the executive session will be for me to ask for comments from members of the Subcommittee or consultants, or question is they have any.

I, however, should point to the written agenda. Does everyone who needs one have a copy?

We're scheduled on the agenda to finish by 7:00 p.m. I'm told we must vacate this room at 6:30. Hence, anything that remains after 6:30 will have to be carried on in the hallway. I hope therefore we will be finished at least by 6:30. We'll try to schedule the lunch break at about the time scheduled on the agenda, which is roughly 1:15 p.m.

I don't know of any other logistical details with which we need to deal.

Let me ask the members of the Subcommittee or consultants if there are any comments or questions that they might

eb3

want to raise at this point.

DR. OKRENT: I have an administrative question.

I recently received a copy of the report entitled "Probabilty Analysis of Certain Structural Set" dated April 12, 1979. Is this the first time I was sent this report?

MR. IGNE: Yes.

DR. OKRENT: Is there some reason why, if it's dated April, we received it in October?

Can the Staff tell me?

MR. IGNE: The Staff isn't here yet.

MR. REED: I'm Bob Reed. I can't answer the question right now but I think when Chris Nelson gets here he may be able to address that.

DR. OKRENT: Will you try to get the answer?

MR. REED: Yes.

PROF. KERR: Are there other questions or comments?

Mr. Darmitzel is the GE spokesman, I believe, and I shall hence call upon him to begin the GE presentation.

Mr. Darmitzel.

MR. DARMITZEL: Thank you.

My name is Bob Darmitzel. I'm manager of the Radiation Process and Operation at the Vallecitos nuclear site.

General Electric has requested this opportunity to present its position regarding the geology and seismology aspects of the General Electric Test Reactor site. Our

eb4

consultants have completed extensive geologic investigations and supporting studies during the past two years which should be compared with the Staff's Safety Evaluation Report.

We do not agree with the Staff's current position regarding the origin of the shears observed at the base of the hills near the General Electric Test Reactor, nor do we agree with their assessment of faulting and the landslide hazard at the GETR site.

We urge that this matter be sent to the full ACRS for a recommendation that the NRC Staff reconsider their seismic input values.

Our presentation of the evidence to support our position will be the following:

(Slide.)

I will start off the presentation stating the General Electric position.

I will be followed by Mr. Dick Harding of Earth Sciences Associates, who will give a brief description on the geologic investigation scope.

Mr. Doug Hamilton, also of Earth Sciences Associates, will give a description of the regional tectonic setting as it applies to the General Electric site.

Mr. Doug Yadon will describe the investigations that were conducted onsite and also on some trenches that were dug off the General Electric property.

eb4

Mr. Roy J. Shlemon will describe the soil stratigraphy and the age dating that was done in the trenches onsite.

Mr. Harding will then give the conclusions that were derived from those investigations.

Professor Jahns of Stanford University will give a geology overview.

Mr. Jack Benjamin of Jack Benjamin Associates will discuss application of probability methods to a problem such as surface offset.

Mr. John Reed will describe the probability risk assessment for surface offset.

Dr. Charles Richter will discuss site seismology as it relates to the Calaveras Fault.

And I will summarize the General Electric position, and that will conclude our presentation.

(Slide.)

The General Electric position is as follows:

The origin of the low-angle shear-like structures observed at the GETR site cannot be absolutely determined. General Electric's consultants and the California Division of Mines and Geology believe the most probable origin is large-scale landsliding. The postulation of a tectonic origin results from conflicts with the observed physical evidence.

Secondly, evidence shows the postulated Verona Fault does not connect with any faults to the northwest or to the east,

eb5

limiting the length of the postulated Verona Fault to approximately eight kilometers.

Thirdly, no surface displacement or offset has occurred in the vicinity of the Vallecitos site in the past eight thousand years. A maximum offset of three feet has occurred at one point in the past ten thousand to twenty thousand years.

Fourth, no offset was observed on any plane which, if extended, would break the surface beneath the GETR. This shows that faulting has not occurred in the foundation area of the reactor for at least 128,000 and more likely 195,000 years.

Fifth, a conservative value for the probability of any future offset of any size occuring at the foundation of the GETR is calculated to be less than 10^{-6} per year.

Measurements indicate that the average rate of strain relief over at least the last 70,000 to 125,000 years is extremely low, on the order of two thousandths of an inch per year. This rate of relief is at least two orders of magnitude lower than for a system such as the San Fernando Fault and comparison of the structure of the San Fernando system to the postulated Verona system indicates its use as a model is not proper.

Seventh, the Staff value of 2.5 meters of surface offset cannot be generated by a minor fault such as the postulated Verona. One meter of offset on the observes shears is

eb6

an appropriately conservative value.

Lastly, 0.56g effective ground acceleration as a result of a 7.5 Richter magnitude even on the Calaveras Fault is an appropriately conservative value.

For the sake of expediting the review on the GETR, a .8g horizontal ground acceleration value was used to evaluate the GETR structures and systems.

I will now turn the meeting over to Dick Harding who will describe the geologic investigations that were conducted.

PROF. KERR: Before we go into the next section, are there any questions from the Subcommittee?

(No response.)

PROF. KERR: Please proceed.

(Slide.)

MR. HARDING: My name is Dick Harding, with Earth Sciences Associates, consultants to General Electric.

We have been studying the geology at the GETR site and in the region around the GETR site for a period of two years now, and these studies and investigations have included this scope of investigations:

Literature Review, including a review of all literature available, published and unpublished, including reports of other private consultants, oil well data, water well data, and geophysical data.

Aerial Photo Interpretation included examination of at least six sets of black-and-white stereo pairs, one set of color stereo pairs, and one high-altitude false color IR set.

Aerial Reconnaissance was conducted by taking over-flights of the area and shooting pictures on several occasions in different season of the year at different times of day, including times of low sun angle.

Detailed Field Mapping was conducted around the GETR site and at selected locations throughout the Livermore Valley, looking at specific outcrops of significant features.

Subsurface Exploration included over two miles of trench excavations which were logged in detail as well as large-diameter borings which were entered and logged down hole.

Soil Stratigraphy Studies were conducted in order to determine the age of the soils on the site and tell us something about the Quaternary history at the site.

Age Dating techniques included radiocarbon analysis, radiomagnetic analysis, samples, Paleontological analysis of samples, and Paleoclimate profile correlations.

Geophysical Studies included seismic refraction surveys, high-resolution shallow seismic reflection surveys, and shear wave velocity measurements.

Engineering Studies included slope stability analyses and liquefaction potential analyses of the GETR foundation area.

eb8

Groundwater Studies included mapping springs and wells, water levels as well as water quality studies.

Now it would take at least two days to go through all the details that were developed and all the data that was developed during this study. We have tried to condense this information into our presentation today, realizing that it is a long presentation but nevertheless, there's an awful lot of data to cover.

(Slide.)

In order to make interpretations of the shear features that we see at the GETR site in terms of their origin or what can be determined in relation to the design criteria for the GETR, we must take into account the known geologic relations, the regional geologic and tectonic setting.

We must look at the site geology, the geomorphic evidence, the outcrop evidence, and subsurface exploration.

And we must know something about the Quaternary history of the site in order to know the soil stratigraphy and tell something about the age and amount of offsets that we see on the shears at the site.

Now we have divided this presentation up, as Bob Darmitzel previously told you, in this manner. Douglas Hamilton will discuss the regional geologic and tectonic setting and Doug Yadon will discuss the site geology and Roy Shlemon the Quaternary history, and I'll come back and try

eb9

to put this all together and tell you what our interpretations of this data are and what our conclusions are.

I know there are going to be numerous questions on the data. I would suggest, though, we keep in mind that some of the questions that you may have may be answered by subsequent speakers, so in order to expedite matters, it may be better to hold most of the questions until the end, unless you have some question on a specific piece of evidence.

With that brief introduction then I would like to turn it over to Douglas Hamilton who will discussion the regional geologic and tectonic setting.

DR. OKRENT: Before you proceed, I would like to request something if I may.

In reviewing the file for this, I've observed a considerable difference of opinion among various experts so I have to assume there is some degree of interpretation.

It would be helpful I think if all of the succeeding speakers, if practical, could indicate that they think is fact, what is interpretation, and where there are matters of judgment and this sort of thing.

I realize that's not an easy thing to do but it would I think assist us if General Electric and its consultants could do that, and if the Staff in turn could do it when they tell us what they think. But I suspect we don't have something quite as precise as Newton's law.

eb10

MR. HARDING: We'll attempt to do that.

MR. HAMILTON: My name is Doug Hamilton, and I'd like to briefly discuss the regional setting of the General Electric Test Reactor site.

First slide, please.

(Slide.)

I'd like to lead off with a slide showing the regional setting in the central part of California where the GETR site is located. On this slide we have shown the major faults that define the major geologic features in the San Francisco Bay region. These represent the plate boundary transform fault system that relate to the San Andreas Fault chiefly, and the San Andreas FAult is shown proceeding diagonally across this slide.

The other major faults that are recognized in this area include the San Gregorio Fault lying west of the San Andreas and a system of faults including the Calaveras, the Hayward and lesser faults, and including the Greenville and the Riggs Canyon that lie east of the San Andreas.

The Test Reactor site is located just south of the Livermore Valley immediately east of the Calaveras Fault.

In general, the geologic relationship in this area here is that the North American plate is moving in a generally southward direction and the Pacific plate in a northern direction in a generally right lateral strain system that corresponds

eb11

apparently to a general north-south compression regime.

I'd like now to focus more in the area between the Hayward Fault, the Calaveras and Greenville FAult and the Livermore Valley, and this is an area that we think can be illustrated as a general tectonic form by a model that John Crowell has proposed, and I think that's on the next slide.

(Slide.)

This is from the paper published by Crowell in which he makes a diagrammatic representation of a region of a transform regime such as the one we had in the Central Bay region here, showing what he calls pull-apart basins and tipped fault wedges where right-slip faults converge or diverge.

On this he indicates a number of faults that would be part of a right-slip transform system. He shows areas where the ground between these faults is relatively higher or lower because either of their being squeezed apart or dropped down because of the movement on the fault.

The "L" indicates the lower areas, the "H" the higher areas, and the hatchured lines show the areas of the pull-apart basins.

And I think, although I don't know just what he had as a model for this other than just illustrating the theoretical concept, that one can fairly well pick out the setting of the Central Bay area here if you imagine that the Calaveras Fault is perhaps this one, and east of that would lie the Livermore

eb12

Valley, the Diablo Range. The Berkeley Hills would lie west of the Calaveras Fault, and then you might imagine the Hayward Fault bein g the most westerly one on this diagram.

Now to go to a map that shows the actual area, we can see how that compares with this theoretical diagram.

(Slide.)

Here we have shown, on a larger scale than the first map, the principal faults again that exist in the area of the Livermore Valley: tne Hayward, the Calaveras, the Greenville and the Riggs Canyon system, the entire system in a regime of right-slip transform faulting.

And we see h?re that betwee the Calaveras and the Greenville-Riggs Canyon system we do have an uplifting region, the Diablo Range.

We have a down-dropped, a down-warped region, the Livermore Valley, near the converging faults.

We again have a substantial uplift in the Mount Diablo region and across the fault we have an uplifted area in the Mission Hills.

So this shows that the region around Livermore Valley corresponds rather closely with the kind of theoretical presentation that John Crowell made for tipped fault wedges and the pull-apart basins.

Next slide, please.

1462 017

(Slide.)

eb13

This is simply a listing of the principal sources that were used in compiling the map that we used to show the regional and aerial geology around the Test Reactor site. You see the sources go back to around 1948 and up to as recently as 1979. We've tried to keep this map current with the most recent interpretations and the data, and also take into account all the body of data that was known previously.

This represents studies of many different kinds, of the structural and stratographic geology, work by the Department of Water Resources in studying groundwater and the study of seismology, and just the general field of geologic research has all contributed to the understanding of the geology of the Livermore Valley.

Next slide, please.

(Slide.)

This map is the compilation that we made from the sources that we previously showed, and I would like to just go through and show you basically what's on this map. It represents the aerial geology in the center of the Livermore Valley and on it we show three kinds of structural features and six stratigraphic units.

The stratigraphic units are represented by different colors. They range from oldest to youngest age. The purple unit, which is the Franciscan and serpentinite body that constitute the local basement rock; the dark green are the

eb14

sedimentary rocks of the Great Valley sequence of Cretaceous age. The blue units are Pre-Pliocene Tertiary rocks, mostly sedimentary rocks. The brown unit here includes the Tassajara Orindo formations and mainly continental clastic deposits of Pliocene age.

The yellow unit represents the Plio-Pleistocene Livermore gravels which are the rocks that directly underlie the Test Reactor site located here in the Vallecitos Valley.

And finally the uncolored areas are the edge of, by and large, alluvial kinds of deposits that are the youngest sequence.

The three kinds of structural features that we show are faults indicated by solid or dashed or dotted lines, also fold axes indicated by lines with arrows indicating the fold away from or toward the axis, and the anticlines or synclines, and finally the areas where the rocks have a prevailing inclination or dip such as in the Vallecitos Hills, and we show these by strike and dip symbols and they represent the general attitude of rocks over a fairly wide-spread area.

The main features on this map are first of all the faults that bound the wedge of ground or structural block where Vallecitos and the Livermore Valleys are located. These are, on the left side, the Calaveras Fault Zone, on the right or northeast side the system of faults including the Greenville-Riggs Canyon, and an unnamed fault extending south from the

eb15

Greenville.

Between these faults the structural block that includes the Livermore Valley has a prevailing structural grain that is subparallel to that of the bounding faults. It's generally northwesterly aligned faults and folds in the rock ranging from Mesozoic through Pleistocene age.

The major features within this block are, first, the general down-warped area of the Livermore Valley and secondly, the faults, most of which have a trend that parallels that of the boundary faults and include the Livermore Fault Zone and the series of lesser faults that lie mainly between that and the Greenville and Riggs Canyon Fault.

The GETR is identified as being located in the alluvial area of --

PROF. KERR: I think our system is dead.

MR. HAMILTON: If I can be heard I'll just continue.

The Test Reactor site, as I indicated, is here in the Vallecitos Valley, and the geology in the immediate vicinity of that is defined by the structure of the Livermore gravels which here form a thick northeast to east dipping sequence of rocks with moderate dip toward the Livermore Valley.

The structure here is derived mainly from surface mapping but one can also look at the evidence that governs the fact of this structure from other means, including geophysical means, and evidence that can be developed from subsurface

eb16

borings including those for oil exploration and for groundwater.

(Slide.)

This slide will show one of the kinds of geophysical evidence that seems to corroborate the structural pattern that one gets in developing the surface geology.

This is an overlay of the Bougere gravity values superimposed on the geologic structural map. Again you can see a very strong correspondence between the gravity value or gravity anomaly pattern and the mapped geology with a very prominent gradient that follows the Calaveras Fault along the southwest side of the valley.

Another prominent gradient follows the boundary fault to the Greenville-Riggs Canyon system on the east side of the valley, a very pronounced gravity low corresponding to the Livermore Valley itself and a jog in the gradient which represents the rise from the down-warp of Livermore Valley to the structural high of basement rock of the Diablo Range south of the valley. The main jog here corresponds to the Livermore Fault Zone which runs across the floor of the valley parallel to the Calaveras-Riggs Canyon Faults.

Next slide, please.

(Slide.)

This slide shows the location of wells that were drilled for oil and gas exploration which were examined in the course of developing this map and making the study, and these

eh17

include a series of wells that were over in the southeast corner of the valley around an area of gas exploration some years ago, and a couple of more wildcat like exploratory wells including the wells here that are in the vicinity of the reactor site, to define the structure or help define it between the reactor site and the central part of the Livermore Valley.

The red line indicated in the middle of the map area is the line of a cross-section which I'll turn to next that shows the structure essentially across the regional grain between the Calaveras Fault and the Livermore Fault Zone.

(Slide.)

This is the cross-section looking northwest. It runs between the major Calaveras Fault Zone from the southwest and the central part of the Livermore Valley from the northeast, including the line across the Livermore Fault Zone.

The two wells that I showed, the Foley well and the Waggoner well, are located respectively in Vallecitos Valley just a little bit north of the reactor site and in the area of the Livermore Fault Zone out in the valley.

The Vallecitos Hills are in the mid-part of this section. The Vallecitos Valley and the low hills that surround it are in the left side and the reactor site projects to an area just a little bit west of the Foley well.

The features that can be seen on this are the very thick section of Livermore gravels down-warped in an easterly

eb18

direction through the axis of the Livermore Valley. These rest over the rheonus of Tert: ary formation that underlies at some d· pth the ground between the reactor site and the Calaveras Fault.

This is a natural scale and you see there's a very substantial accumulation of these Pleistocene sediments and continuing the pattern of down-dropping and down-warping there is also a substantial alluvial valley at the surface over the Plio-Pleistocene section.

The Calaveras Fault here is shown as being west up, and it is-- Additionally of course it has a predominant movement as a strike-slip fault. This apparently is true also of the Livermore Fault which, in this area, has an over-all west-down but is also apparently a right-slip fault.

The ground on the east side of the Livermore Fault generally is -- rather, the geology is higher although the topography isn't, and that fault also constitutes a very significant groundwater barrier.

I would like now to go on to --

MR. MAXWELL: What's the red?

MR. HAMILTON: That is interpreted as Franciscan basement rock that was in the bottom of the Waggoner well. It probably has the form of a sliver of overrock that is contained within the fault zone.

I would like now to look at some of the details of

eb19

the younger basin of alluvial deposits tc .ow how this structural pattern apparently continues even into the late Pleistocene and Holocene.

(Slide.)

This is a representation of contours on what is called the pre-alluvial surface. It was developed by, in effect, stripping off the alluvial deposits over the Livermore gravels and old rocks in the ground between the Calaveras Fault, which again lies along the southwest side, and the easterly part of the Livermore Valley at the right side of the slide area here.

This was developed by the Department of Water Resources by interpreting the data from a very large number of water wells for which logs were available, and these are shown in the red dots here showing that there's a very large amount of control.

The structural features that we have added into this are the Calaveras Fault and the Livermore Fault and the Parks Fault and the line along the north side of the valley.

Next slide.

(Slide.)

This is simply the same map with the data points removed. The Test Reactor site lies near the lower southwest corner of the map, and this is of interest because it shows that the basin of the Livermore Valley that can 'e seen in the

eb20

pre-alluvial surface corresponds to the major basin that is defined by the previous unit, the Livermore gravel.

It shows that this basin is very strongly controlled by boundary faults, and it shows very clearly the Livermore Fault coming across the basin, dropping the ground on the southwest side relative to that on the northeast side. The continuity of the Livermore Fault Zone can be seen where it extends down into the valleys of Mocho and Valle Canyon south of the valley.

MR. MAXWELL: What's the contour interval?

MR. HAMILTON: I'll have to ask Dick Harding that.

MR. HARDING: I'm not sure I recall at this point what the contour interval is, but the depth of the alluvium in the valley is on the order of three to four hundred feet.

MR. HAMILTON: So this would represent roughly 300 feet below ground surface in the central part of the valley at the deepest alluvial fill.

Next slide, please.

(Slide.)

Another way of looking at the structure and also of the tectonic regime in the region of the Livermore Valley is by looking at the focal mechanism solutions that have been derived for earthquakes in that area, and this is some work that I think was done by John Blume, or at least this was in a report that he prepared for the Lawrence Livermore Radiation

eb21

Lab.

This shows that there were four earthquakes that were plotted up. Three have predominantly or almost wholly strike-slip mechanisms, one located over near the Tesla Fault in the eastern part of the Livermore Valley. It has a combined strike-slip and probably a reverse component of movement so the focal mechanisms apparently do agree with the kind of concept of a right-slip environment corresponding to north-south compression generally.

Next slide, please.

(Slide.)

I'd just like to summarize the discussion of the aerial geology around the reactor site, pointing out again that it does lie within a structural block which is bounded by the Calaveras Fault on the southwest, by the Greenville-Riggs Canyon Fault, both of them right-slip faults, on the northeast. The block within which the Livermore Valley is located includes higher ground in the south part to the Diablo Range, higher ground to the north, and intense deformation to the north around the Diablo uplift, a pronounced down-warped valley which has obviously been a feature for a long time because it has this very thick accumulation of Plio-Pleistocene sediments as well as the present down-warp shown in the pre-alluvial surface of the gravels.

The structural pattern within this block of ground

eb22

is predominantly northwest and southeast folds and fault structures.

I'd like now to focus in on o..e particular aspect of the geology here which relates more to some specific structural interpretations at points away from the reactor site. This has to do with the Las Positas Fault which is shown on this map here as a northeast-southwest aligned break that lies between Arroyo Seco and Arroyo Rancho in the southeast corner of the Livermore Valley.

This is a fault that was first mapped by Harold Herd of the USGS and much has recently been exposed in a series of trenches right in the area around Arroyo Seco south of the Lawrence Livermore reactor or Radiation Lab which is in the southeast corner of the Livermore Valley.

This is a fault, in the surface expression at least as seen in bulldozer cuts, that seems to be a very high-angle, probably southeast-side-up fault that we've determined as being probably a reverse oblique type movement.

The significance of that fault can be seen I think in the next slide.

(Slide.)

Here you see we have superimposed the interpretation of the fault pattern that is presented by Dr. Herb of the USGS. That is superimposed in the red lines overlying the basic geology that we have compiled from other sources.

eb23

You can see that the Las Positas FAult, as it is recognized through trenching, is located there. The fault as it was originally mapped prior to the trenches having been excavated is shown in red, and that position is pretty closely corresponding to the mapped location here near Arroyo Seco.

The interpretation though follows that the Las Positas Fault is actually a quite major structure that defines the whole southeasterly end of the Livermore Valley, and it continues on and corresponds approximately to the contact between Livermore gravels and Tertiary formations and continues on nearly to the area between Vallecitos Valley and La Costa Valley.

The other major structures that are shown on this interpretation include the pattern of shearing that exists near the Vallecitos Valley, identified here also as a fault structure, and some refinement of the mapping of the Greenville and Riggs Canyon Faults.

Now I focus on this aspect because it's away from the area of detailed site studies that we made near the GETR but it is an important part of the theory that a fault located here at the Vallecitos Hills is part of an essentially previously unrecognized structure that extends across the southeast side of the Livermore Valley and represents a very much more substantial tectonic system than one could associate simply with a fault located just in the Vallecitos Hills area.

eb24

Now as I said, the Las Positas Fault is certainly a very real feature at the point where it is recognized here but we feel that the evidence that would allow extending that as a continuous fault from the area where it is known to exist all the way across the valley bears some further examination.

We would like first to point out that this fault would lie at right angles to the Livermore Fault Zone which is a major feature and is recognized through a number of different kinds of evidence, including the gravity gradient that I showed on an earlier slide, the contouring of the pre-alluvial surface, the existence of pronounced groundwater anomalies in the Livermore Valley, and most recently, a study by the Department of Water Resources in assessing the seismic environment from Del Valle Dam, located in Valle Canyon here, which included doing some trenching that verified the existence of the Livermore Fault coming down into the Valle Canyon along the northeast side.

So with that in mind I would like now to just review some of the evidence from the Las Positas Fault and go to the next slide.

MR. MAXWELL: Where is the Tesla Fault?

MR. HAMILTON: The Tesla Fault is mapped as coming out of the Diablo Range and trending down in the direction of the Livermore Valley. It's supposed to be identified from interpretation of some of the oil well holes in this area, and

c2

eb25

a search for that fault was one of the objectives of the Livermore program of excavating the trenches which did expose the Las Positas Fault in this area here.

So far as I'm aware, they haven't found any specific evidence of the existence of this as a fault that reaches the surface.

MR. MAXWELL: Where was that fault plane solution that appeared to be on the Tesla Fault?

MR. HAMILTON: I believe right in this area here, very, very close to here anyway.

That fault plane solution can either be associated spatially with the Tesla or, for that matter, with the Las Positas. It's within that general region.

MR. THOMPSON: May I raise a question of interpretation at this point?

I'm Thompson, ACRS consultant.

You have mentioned the Livermore Fault Zone in connection with the gravity anomaly as bounding the deep basin and yet it looks to me on the gravity map as though the Livermore Fault Zone is almost on the axis of the negative anomaly.

MR. HAMILTON: Could we run back to the previous slide here? I'd like to have Dick Willingham come up and address that issue.

Dick?

eb26

As I understand it now, the question has to do with the character of the anomaly that we feel is influenced by the location of the Livermore Fault Zone.

MR. THOMPSON: Yes. I don't see any evidence of the existence of the Livermore Fault Zone in the gravity model.

MR. HAMILTON: Our feeling has been that the pattern of the interruption of this anomaly in the area south of the Livermore Valley would correspond to arrayed basement rock, a situation that would correspond approximately to the higher ground along the Livermore Fault.

I don't think we see any evidence in the central part of the valley for the fault zone.

MR. THOMPSON: I think that answers my question.

(Slide.)

MR. HAMILTON: Okay, let's back up one slide. What I propose to do now is to follow the slip map that is published by Herd of the USGS that takes us along the line of the Las Positas Fault and for reasons of scale we have shown this in three segments which correspond to the easterly, the central, and the westerly mapped parts of the Las Positas Fault, and simply comment on some of the evidence as we see it and we interpret it that bears on the existence or lack of it in the Las Positas Fault.

The first of these strip maps covers the easterly part between essentially Arroyo Mocho and the easterly end of e-27

the valley.

(Slide.)

On this map are shown the Las Positas Fault indicated red, or the different elements of that Las Positas system. The stratigraphic units shown on here include the Livermore gravels and older terrace deposits of the series of ages in the generally reddish and bluish colors, and the younger alluvial and terrace deposits of Arroyo Mocho and Arroyo Valle that are shown in different shades of yellow.

The Lawrence Livermore Facility is the series of buildings that are shown just downstream from the Arroyo Seco area. An area that was trenched is over toward the east end of the Las Positas zone where a stream bank exposure that some of you have seen recently was cleaned off and some other trenches were excavated in the upper terrace deposits.

These certainly showed a positive expression of faulting along several different strands which seemed to show successively younger ages of faulting along the strands as you went northward.

I would like first to show a couple of slides for those who were not out in the field yesterday to show what those exposures look like.

(Slide.)

This is a view southwesterly looking down the strike of the Las Positas Fault on the east. This was a cleaned-off

eb28

exposure along the west side of Arroyo Seco Creek. The fault plane shows very clearly, and I can point it out here, running up the course of this slide.

It apparently has a rather large offset of overlying either gravely soil or terrace deposits but where it reaches close to the ground surface seen in the upper part of the slide, the rock on either side that is cut by this fault is part of the Livermore gravels. It is a north-dipping sequence.

The bedding is shown by the streaks of differing color on the slide.

The stratigraphic sequence on one side does not match that of the other so the movement is in excess of that that is represented by the height of the cut here.

The fault zone has slickensides on the surface that plunge about 25 degrees out of the slide toward the floor and that shows that the last movement of the slide here -- on the fault was an oblique kind of sense of movement.

The actual offset, according to our observations, was rather less than that that is suggested by the apparent offset of dark material against the Livermore gravels at the top of the slide. The material at the bottom of this apparently is some kind of an infill against the fault and the actual offset I think was on the order of perhaps six or eight inches, as I was able to observe it at least.

That is, the surface of the Livermore gravels here

eb29

project behind this infill but there is a distinct offset. The soil profile at the top, so far as we can tell, was not offset.

Next slide, please.

(Slide.)

This is the other strand that's located a few ten's of feet farther north from the one I just showed. The fault here is less distinct but it has more contrasting materials across it. A general zone of fault plane can be seen with Livermore gravels on the left and a rubbly kind of terrace material on the right, a rather interesting shear pattern, the fault dipping steeply to the south end of the hill.

Here the fault comes nearly to the surface. The surface is disturbed so it is not really clear whether it offsets the soil profile but it is clear that it does offset the terrace deposits against the Livermore gravel.

The apparent sense of movement in our judgment is probably south of the steep reverse in this particular plane here.

MR. PHILBRICK: Did you consider that stuff along the right of the fault as terrace gravel?

MR. HAMILTON: We thought that was the most likely way of describing it, although I'm not really competent to say that it's not also Livermore gravel.

MR. PHILBRICK: Does it have a lot of --

PROF. KERR: Can you get to a mike maybe?

eb30

MR. PHILBRICK: Does it have a lot of well-rounded material in it?

MR. HAMILTON: Can I ask Dick Harding to comment on that? He spent more time looking at it than I did.

MR. HARDING: I would say that there is some well-rounded material in that, yes. There are also a few angular. blocks.

VOICE: Do you find the angular blocks constant or common to the gravels?

MR. HARDING: In the Livermore gravels?

VOICE: Yes.

MR. HARDING: Most of the ones we see in there are more rounded than angular.

MR. PHILBRICK: What I'm looking at is this thing. It seems to me we have two different ages of materials in faulting. We have a difference in degree of disruption and deformation in these two structures that you have just shown pictures of.

Yesterday afternoon that white area that lies below and to the left of the hammerhead was pretty well broken up as if it had failed on a series of fracture patterns that lie in what is then the left side or maybe the hang wall of that fault. It's a different type of structure than you had in the first picture.

I'm looking for this situation and what we have here

eh31

is a much older, much more deformed fracture than what you have on the left.

Now is there any degree of agreement in that interpretation of these two pictures?

MR. HARDING: I don't think I can really argue with that, no.

MR. PHILBRICK: All right.

Now what we get from that, if I can make the next deduction, and this is an assumption, Dave, that along-- This is the Las Positas FAult?

MR. HARDING: Yes.

MR. PHILBRICK: Along this fault there has been motion on separate failure planes and fresh, unbroken material has broken in the left-hand fault. The stress pattern has caused a strain to move from a prior zone of weakness to a new failure plane.

Now is that in agreement with what you see,Doug?

MR. HAMILTON: Yes, I think that is illustrated both in the cut that these two photographs were taken from and also by the trench you may have seen that is farther up the hill.

MR. PHILBRICK: I didn't see that.

MR. HAMILTON: In that trench another break was found that cut only Livermore gravels but was clearly overlain by unbroken terrace deposits, so we have a series of different ages of breaks in different places in the zone. All the strands

eb22

generally seem to be parallel.

MR. PHILBRICK: What I'm trying to point out is you get new faulting along the Law Positas.

MR. HAMILTON: That certainly happened at several different times in past history.

MR. PHILBRICK: Do you know whether there was a land-slide there at that point?

MR. HARDING: There is a slump scarp up near the surface.

MR. PHILBRICK: That's right.

MR. HARDING: Yes.

MR. PHILBRICK: Okay. And had you considered that as affecting the depth of the gravel adjacent to the fault?

MR. HARDING: Well, when we first observed this feature before it was cleaned off, all we could see was the upper horizons and we had assumed that because of that scarp we saw that the feature was related to the slumping rather than to tectonics.

It wasn't until this was cleaned off and we actually saw the fault down below that we recognized that there were tectonic movements below that landslide feature.

MR. PHILBRICK: But what you have there, as I say it yesterday -- and I wish you would tell me whether I am right or not -- was a continuity of gravels across the top of both of those faults. Is that correct? -- the top gravel itself

eb33

at the ground surface itself at the position of each of these faults was undisturbed.

MR. HARDING: I didn't see any direct disturbance of those, but I wouldn't go too far to argue that they were not disturbed, given that you had the slump feature at the top.

MR. PHILBRICK: The slump feature lies a little bit extreme from the plane in this section. What I'm trying to bring out, in my opinion at the present time, the top gravels are undisturbed by either of those faults.

MR. HARDING: I'll accept that.

MR. PHILBRICK: Thank you.

MR. HAMILTON: My own observation of this was fairly brief. It was my view that at least the gravelly soil at the top was disturbed -- was not disturbed and that there was certainly no topographic expression at all of either of the two breaks that we've seen here, nor of the one that was capped by unbroken gravels lying a little bit farther south of here.

Next slide, please.

(Slide.)

We have now gone to the central segment of the Las Positas Fault. The area we were just looking at is over at the extreme right end of the map here with the Lawrence Livermore Radiation Lab to the right of that. And we now come to the segment that goes as a solid line to Arroyo Mocho. It's mapped as an approximately located fault to the west of that with

eb34

an en echelon continuation, again as an approximately located segment continuing across the Arroyo Valle.

The well-defined part of the fault is in the area at the extreme right of the map. We think it is significant that as you go west, the evidence that we are aware of seems to not really provide much support for a westerly continuation of the fault that we recognize in the east.

In particular, this fault which we saw did offset some kinds of terrace gravels at the far east as well as a little more gravel now is completely not expressed in a series of terraces that lie at the boundary of the Arroyo Mocho or the terraces which are quite well developed that lie on the Arroyo Valle, and further, that the course of these streams are not significantly deviated in a sense that would suggest lateral movement.

So we see evidence for neither vertical nor lateral movement of a fault where this fault is supposed to cross the streams, nor are we aware that there is any kind of an outcrop that would support the existence of the fault through these two areas here.

2.170

MR. PHILBRICK: Your thought is then that the fault doesn't exist there?

MR. HAMILTON: We con't see any basis for extending the fault as far as Arroyo Mocho.

Now when you get to Arroyo Valle you are now in the

eb35

area where we in general and the Department of Water Resources in particular extended the Livermore Fault extending from well south of this map area across this trend at essentially right angles to where the Las Positas Fault is supposed to be, and out into the valley while the Livermore Fault is defined by mapping and trenching down around the dam site and then forms the distinct break in the pre-alluvial surface and the very distinct groundwater barrier farther out in the Livermore Valley.

Next slide, please. --

(Slide.)

Excuse me, let's return to the previous one.

(Slide.)

The last part of this that I would like to address relates to the evidence for the Las Positas Fault along its southwesterly end where it's mapped as being the boundary between the Livermore gravels on the north and the Tertiary Cierbo formation on the south. This is an area where Clarence Hall did his Ph. D. thesis mapping.

He provi ed quite a detailed map and he mapped the boundary or the contact here as being one of a depositional overlap of north-dipping Livermore gravels over Cierbo formation following a sinuous course that would be appropriate for a northerly dipping contact.

The next photograph was taken in this area looking

eb36

west along that contact.

(Slide.)

We're now looking southwesterly along the mapped trace that is described as the Los Positas Fault and as mapped as a depositional contact by Hall. The two units that can be seen here are the Livermore gravels to the north. You can see the bedding of the Livermore gravels in this particular gravel outcrop, and the structure is generally dipping off to the right side of the slide.

These overlie the Cierbo formation which gives rise to a more darker, weathering soil. You can trace the Livermore gravels back over the Cierbo by the color of the soil, and you can find outcrops of the Cierbo back in this canyon in the middle distance as showing that the contact is indeed a sinuous one.

And you have Livermore gravels that can be found in a patch up the dip slope in this area in the intermediate distance corresponding to the dip of the contact.

Now contrasting with this, the Las Positas Fault is mapped as a high angle fault and drops Livermore against Cierbo in this same area.

Next slide, please.

(Slide.)

MR. PHILBRICK: Do you have any drilling there?

MR. HAMILTON: We do not.

eb37

This is a geological section that corresponds to that same slide view. It shows an irregular erosional unconformity contact between the Livermore gravels which also comes up and is preserved on the top of the hill overlying the Cierbo formation. And as I pointed out, you trace the sinuosity of this contact by Livermore outcrops up on the Cierbo area and Cierbo outcrops and re-entrace into the Livermore area.

This is essentially a prime consideration in our view that the Las Positas Fault does not exist in the area that lies along the southwesterly part of its proposed trace.

MR. PHILBRICK: That yellow-blue contact right of the arrow is an interpretation?

MR. HAMILTON: After you get past, say, down here in the subsurface part of that contact it's an interpretation.

Could we go back to the preceding slide?

(Slide.)

The fact that Cierbo formation underlies this slope here and can be identified in this valley bottom in a re-entrant, going back into the area of Livermore outcrop we feel is an observation, and the continuity of the Livermore gravels back into a re-entrant within the area of the Cierbo formation and also existing on the updip projection of that contact would go also as an observation.

MR. PHILBRICK: If it was a fault following the outcrop patterns you have shown now, what would be the tip of

eb38

the fault?

MR. HAMILTON: It would have to be essentially a bedding plane thrust on the sole of the Livermore gravels which would dip perhaps 20 degrees.

MR. PHILBRICK: How is it mapped? At the present time on a map what does it show?

MR. HAMILTON: The Las Positas Fault?

MR. PHILBRICK: Right.

MR. HAMILTON: Let's go to the preceding slide.

(Slide.)

It is in this area here and it is shown as generally a rather high-angle fault which of course is what we also observed for the Las Positas Fault at the point where it was trenched.

MR. PHILBRICK: Then your observations do not agree with the map.

MR. HAMILTON: That's our view.

MR. PHILBRICK: Thank you.

(Slide.)

MR. HAMILTON: I'd just like to summarize this discussion of the regional geology. I again point out the general structure form that we perceive for the region of the Livermore Valley, the Vallecitos Hills, and the Test Reactor site.

We certainly recognize the clear existence of the

eb39

Las Positas FAult, probably as a steep reverse fault which would also correspond to the general north-south compressive regime and would represent an expectable kind of tectonic failure, I think, in that orientation of a stress-strain system in the place was recognized.

We feel that that fault cannot be shown to exist across Arroyo Mocho and particularly across the Arroyo Valle. We have not shown it on this map and we feel that the contact between Livermore gravels and the Cierbo formation essentially precludes its existence in points farther southwest.

Next slide.

(Slide.)

I would just like to conclude with these views of the over-all interpretation. They are:

First, that faults, folds, and rock units are predominantly northwest trending structures in the general region of the structural block between the Calaveras and the Greenville-Riggs Canyon Faults, including Livermore Valley and the Vallecitos area.

The regional stress pattern is one of right transform shear along the San Andreas system which corresponds to north-south compression.

Geologic, geophysic and well data all indicate the Livermore Valley has been a subsiding basin since at least Pliocene time when the various thick sections of Livermore

eb40

gravels began to accumulate and including the recent geologic past when the alluvial basin that is documented by the pre-alluvial surface map defined by the numerous water wells was formed.

The Las Positas Fault we feel is a relatively minor cross-structure which is confined to the southeast corner of the Livermore Valley. It has an orientation and a sense of movement that is consistent with the same kind of compressive stress regime that should have given rise to the predominantly northwest trending strike-slip structures and folds.

We find there to be no evidence to extent the Las Positas Fault to the southwest across the Livermore Fault. In particular the evidence indicates that the Cierbo-Livermore gravels contact is an onlap unconformity that was originally mapped by Hall in 1958.

2.300

That concludes my presentation.

MR. MAXWELL: Has anybody calculated the thickness of sediments from the gravity?

MR. HAMILTON: I guess I would turn that question to Dick Willingham.

The question was: Has anyone calculated the thickness of sediments from the gravity map?

MR. WILLINGHAM: We made a preliminary calculation, and I don't have those numbers with me, but it was something on the order -- in excess of 10,000 feet I believe to the

eh41

basement.

MR. MAXWELL: You don't remember your assumed density for the sediments?

MR. WILLINGHAM: No, I'm sorry, I don't.

MR. HAMILTON: That section would include the several hundred feet of alluvium in the center of the valley, then the several thousand feet of Livermore gravels and then essentially an undocumented thickness o. Tertiary sedimentary rock down to the denser grade valley and particularly Franciscan rocks.

MR. MAXWELL: Let me turn the question around. That's a blocking anomaly.

What is the minimum thickness of the stuff lighter than the Franciscan that you would have to have in there, assuming any reasonable density for the gravels?

MR. WILLINGHAM: Could you repeat that, please?

MR. MAXWELL: Assuming a reasonable density for the gravels and associated sediments, what's the minimum thickness you would need to account for that anomaly?

MR. WILLINGHAM: I'm afraid I couldn't answer off the top of my head. First, the exact extent of the anomaly depends on the slope of the region there. It is not great but the residual anomaly would be less than 50 milligals.

I can try to work up something in the next few minutes if you would like.

eb42

PROF. KERR: Why don't you do that?

MR. HAMILTON: It's clear I think anyway that there's a whole lot of lighter sedimentary rock that is concentrated in that Livermore Valley region, and that must represent a rather long-standing kind of pattern of accumulation in that area there.

DR. MARK: You may have said and I missed it. The Livermore Fault system which is trending toward the northwest I guess --

MR. HAMILTON: Yes, sir.

DR. MARK: -- what was the sense of displacement of the east and west sides?

MR. HAMILTON: Could we go back to the regional map, to the preceding slide?

(Slide.)

The sense of displacement as it is indicated out in the valley would be northeast side up relative to the central part.

The sense of displacement, on the other hand, around the Arroyo Valle area, just based on stratigraphy, would be the opposite of that with the northeast side down because of the younger Livermore gravel units against older Tertiary and Great Valley units.

So there is something of an anomaly between the surface mapping and the Arroyo Valle area, and the evidence from

eb43

the well data out in the valley, also the evidence that I mentioned to Dr. Thompson, that the gravity seems to be higher suggesting shallower bedrock in the northeast side of the fault and yet in this area.

We feel that this probably is partly a function of there being substantial strike-slip movement as well as simply a vertical movement along the Livermore Fault.

DR. MARK: This could perhaps understandably terminate the westward extension of the Las Positas sort of disturbance?

MR. HAMILTON: Excuse me. Was that a question?

DR. MARK: I'm asking if my feeling about that is similar to yours.

MR. HAMILTON: Well, that's our view that the Livermore is a major structure that is traceable well down into the area from south to north of where the Las Positas is mapped as crossing it, and there is no interruption of the mapping between the area around Arroyo Valle and the area where it is defined by all the many water wells out in the valley.

We see no way that the Las Positas could get across that.

PROF. KERR: Any other questions or comments?

MR. JACKSON: Bob Jackson with the Staff.

I would like to offer a couple of comments at this point in time if I could.

eb44

The discussion of the Las Positas pro's and con's could go on for two days without any problem at all. I just wanted to offer that from the Staff point of view, we have never really concentrated on the necessity of having a Las Positas Fault existing in our interpretation of our findings at the site.

I think you'll note that in looking at the Safety Evaluation Report that was prepared. I just wanted you to keep that in mind while you're looking at this information.

The second item is that the handout provided by GE for this proceeding here has about -- I've looked at it briefly -- has -- about 50 percent of it is new information which has not previously been provided or compiled or submitted for Staff review, so we are seeing some of this for the first time in this form. Some of it may have been provided in different forms, or in widely dispersed areas.

In fact, some of the information provided is what we've been asking for for about two years.

A third thing which I can't let go by is that -- and it's very important -- that Earth Sciences Associates' map, latest map of the site area includes numerous east-west trending folds within the Livermore sediments, and those fold axes trend east-west, indicating north-south compression in the Livermore sequences younger than these other features.

So it is definitely untrue that there is nothing

eb45

other than northwest trending structures in this area.

Thank you.

MR. HAMILTON: If I can make a final comment, I think that perhaps Dr. Jackson has misinterpreted my view that there are no northeast-southwest trending structures. Clearly the Las Positas Fault is an example of that.

We find that the major structures that define the regional structural grain are predominantly northwest-southeast trending and the discussion that I was presenting was one of the general regional pattern rather than one of smaller-scale features.

We certainly agree that the northeast-southwest trending structures would be perfectly compatible with the same kind of north-south compression that presumably gives rise to the northwest-southeast trending shear regime. So I don't think that we have any disagreement on that point.

MR. HARDING: I would like to make a comment also on the subject of new information.

The slides that we're showing you today are in a different form to try to make things more clear to the Committee. However, to my knowledge there is very little new information in them; it has all been presented in the submittals that we have made over the last two years, either in a text discussion or in maps or in reference to material which is available in the literature.

eb46

PROF. KERR: But you won't feel bad if it also makes things clearer to the Staff, will you?

MR. HARDING: Not at all.

We'd like to continue now our discussion of the site geology with Doug Yadon.

MR.YADON: If I could I would like to try to get away without the microphone so that if anyone has any trouble hearing me at all --

PROF. KERR: Don't try.

MR. YADON: Okay.

My name is Doug Yadon. I'm with Earth Sciences Associates.

May I have the first slide?

(Slide.)

I'm going to be discussing the site geology investigations that we've conducted over the last two years at the GETR site, and I'm going to structure my presentation in chronologic sequence of the course of those investigations, and as I do that I'll be trying to point out some of the more important elements of what we found and what we interpret to be the geologic setting of the site area.

I'd like to start out with this slide and tell you that you'll be seeing this basic slide as a base for a number of the slides in my presentation, and I'm hoping that by doing that we'll keep everyone oriented as to where we are, and in

agbl
f1 eb46

which direction we're looking at things.

Basically, the GETR is represented by the symbol here. The heavy band is the Vallecitos Nuclear Center site boundary. Some names which I'll be using to describe geographic locations, I'll go ahead and go through those just briefly now so that when you hear them you'll have an idea where we're speaking about.

The area here is Vallecitos Valley, high ground to the northeast of the GETR Vallecitos hills. The valley that's represented in this area is known as Happy Valley. We'll be discussing that in some detail.

And we informally refer to the area to the southeast of Highway 84 as the pass area, or Highway 84 pass area. That involves some re-entrant ground that interrupts the main ridge of the Vallecitos hills and hills extending on to the southeast.

Okay. Can I have the next slide, please?

(Slide.)

To begin with, the investigations we'd conducted at GETR had been a phased program, and I'll start with Phase I.

The initial objective when we began our work at Vallecitos was specifically to investigate the mapped Verona Fault as mapped by several workers in the past and associated photolineaments in that general zone.

And the initial scope of work consisted of the three listed features: first, review of existing published and

agb2

unpublished literature; initial photo interpretation for orientation to the site and trying to assimilate the literature review and a program of limited trenching, both to investigate certain of the mapped traces and to give us a handle on whether or not the materials at the site would be amenable to further investigation, particularly in the area of age dating in the offsets that we might find.

PROFESSOR KERR: Is a photolineament a line that one observes on a photograph?

MR. YADON: Yes, it is.

PROFESSOR KERR: Thank you.

MR. YADON: May I have the next slide, please?

(Slide.)

Okay. To begin with, I would just like to go through a brief little history of the previous mapping of the Verona Fault through this general area. And I summarize some of the traces here and we'll go through those consecutively.

The first interpretation of any kind of structural trend through this general area was back in the 1920's, I believe, or 30's by Vickery and some of the earlier workers. The scale of mapping which is available from their work is not appropriate to actually showing the trace.

Their interpretation seemed, from reviewing their work, to indicate that they recognized bedrock structures in the Diablo Range to the southeast, and on grossly topographic

agb3

bases, suggested an extension of that bedrock structure to the northwest through the general Vallecitos area.

The first real detailed work out in this area, as far as we're aware, was that done by Hall and published in 1958 but conducted prior to that as part of his Ph.D. dissertation. Hall's traces on this map, or his trace, is shown as a light blue line right down through here.

And Hall interpreted the presence of a normal fault in the locations shown based primarily on geomorphic expression with the northeast side up, with the hills relative to the valley southwest side down. And he noted what he felt was an alignment of springs and a concentration of various types of landsliding generally along the trace that he mapped.

Essentially at the same time that Hall was completing his work in that area, Byerly and Everden were contracted by General Electric Company to provide some geologic input for the initial construction of the Vallecitos site.

And basically Byerly and Everden reached a similar conclusion to Hall that on geomorphic bases they interpreted the existence of a Verona Fault at a slightly different location than Hall, but essentially the same evidence was cited.

In addition, Byerly and Everden pointed out what they felt was a -- what they regarded as a photolineament of unspecified origin along the hillfront land break between the Vallecitos hills and valley, and that is shown -- you probably

agb4

can't see it in the back, but there is a dashed green line in this area, and their map only extended to the boundaries of the nuclear center.

It's important, I think, to point out that Byerly and Everden specifically indicate that they found no field geologic evidence to interpret this photolineament as a fault.

In a later study, John R. Blume and associates conducted some review geologic studies for General Electric Company in 1973. And the conclusions of their review were, again, some aerial photo interpretation, imagery interpretation similar to those presented by Byerly and Everden. Blume chose to accept Hall's trace, the blue line here, as the Verona Fault, and they also identified the photolineament which is shown in the dashed yellow line extending farther than what Byerly and Everden had mapped but essentially along the same trace, and they again indicated that they found no geologic field evidence to interpret that as a fault.

The next mapping in that area was Dr. Herd of the USGS, 1977. And his interpretation of the Verona Fault is shown as the orange line. The evidence presented by Dr. Herd for interpreting the existence of the Verona Fault was similar in type to that presented by the earlier workers, essentially geomorphic, hills up, valley down. He noted what he felt were alignment of springs along his fault trace and cites what he interprets as some geomorphic evidence in terms of truncated

agb5

spurs and similar features along the trace that he mapped.

The initial interpretation -- this was described as a high angle normal fault, again north side up -- northeast side up, southeast side down.

That gets us up to about where we began our investigations, and I'll go on to the next slide.

MR. MAXWELL: May I make a comment, please? This illustrates what I would propose to be a new law, that faults and earthquake epicenters always migrate toward the nuclear sites.

(Laughter.)

MR. YADON: I think it was effectively demonstrated in the previous slide.

(Laughter.)

(Slide.)

If you'll recall from the earlier slide where I described our initial scope and our Phase I investigations, it included some limited trenching in addition to the literature review which I just summarized for you and some imagery interpretation from available air photos.

The first two trenches that were cited were designated G-1 and G-2 shown on the slide. And two things, we tried to accomplish two things with those initial trenches: first off, of course, we wanted to look at the fault trace mapped nearest to the GETR site. We selected locations along that trace based

agb6

on the map trace and our initial imagery interpretation where we felt we could kind of confine geomorphically the location of any faulting along that trend.

For example, down here -- although it certainly doesn't show at this scale -- there is a saddle, a topographic saddle feature along the trace of the fault. And we decided to trench all the way across that assuming that if its origin was due to faulting, we should find something there.

And similar considerations at T-2, we also attempted as well as we could, given map scale problems, to trench in the area of the intersection of Hall's trace and Herd's 1977 trace.

As I said before, we were also looking for at least a general handle on the stratigraphy we had on-site to determine whether or not certain other techniques would be feasible if we had to do more investigation.

When these trenches were excavated, we found no evidence of any kind of normal faulting in any of the exposure we developed. However, after the trenches had dried sufficiently and the walls were adequately cleaned, there did appear low angle shears both in Trench T-2 and T-1 with a thrust sense of offset with a dip northeast underneath the hills, so that the apparent offset of some of the units which are encountered in these trenches suggested hills coming up and overriding the valley in this area.

1462 057

aqb7

At that point, just on the basis of the trench exposure and our review of previous work, we notified General Electric Company that we could not preclude from that information that there may be thrust faulting on the site. General Electric Company immediately notified NRC to that effect, and we continued our investigations from that point.

Okay.

Once we encountered these low angle shears, which were not expected on the basis of the literature review, we began to go back and consider some possible alternative hypotheses that might account for the presence of those features. And as a part of that review, we went back and re-examined our imagery and particularly with the thought in mind that we have low angle shearing with a reverse sense of offset in those trenches.

If I could have the next slide, we'll show what came out of that effort.

(Slide.)

This is a high altitude false color infrared image which was photographed for a transparency. This is Highway 84, Highway 680, Vallecitos Valley down here and Vallecitos Hills. The test reactor site is right about there.

And upon re-examining this imagery in particular, because of the scale advantage backing way off from the site and remembering that approximately in this location up here --

agb8

at somewhere down about in here we had low angle shearing dipping back into the hills, one thing that became apparent was that we have a gross geomorphic feature which was suggestive to us of the possibility of very large scale old landsliding throughout this slope.

And that was based on what I hope you can all see from this slide. It's a topographic scarp, an ampitheater-like shape up in this area. You can't see in the third dimension here, but there are permanent ridges extending from that scarp area and a relatively flat bench there and then kind of a bulging and toe form at the base of the hill slope.

xxxxx

That suggested to us at least that large scale landsliding was a possible origin we would have to consider for the features we saw in the trenches, as well as considering the tectonic thrust fault origin. Those were the two hypotheses we felt were viable on the basis of what we knew at that time.

Okay. If I could have the next slide, please.

(Slide.)

So at that point, I might also point out, that Salem Rice of the California Division of Mines and Geology was invited by the Nuclear Regulatory Commission to provide some input as a local expert on landsliding in the coast ranges of California.

And based on the initial trench exposures and his

agh9

review of the imagery, he came to a similar conclusion that there certainly appears to be evidence for large scale landsliding in the hills which may account for the shearing we saw in the trenches, the first two trenches.

To follow this up, we expanded our Phase I investigations and they were two-pronged: both to look at a possible landslide origin anc thrust origin. For the landslide origin, we did some additional subsurface investigation in Trench T-3. An anomalous swell which was up in the hills, it appeared, may represent a local pullaway or head scarp area for a block within the landslide complex and might be related to toe shearing at T-1.

We had relatively little good natural exposure in the area of Trench T-2, and because of the complexity of some of the structure we saw there, we developed some additional exposure in a canyon nearby so we could see a little bit more of the nature of the structure and a little bit more of the gravels near that trench.

We also excavated a series of three large diameter bucket auger borings in the vicinity of Trench T-1. And the idea of these was to determine whether or not we could get a handle on the downdip character of the shear and shear features that were exposed in Trench T-1, the idea being that if we could follow that shear downdip far enough in a thrust fault origin, one would expect that at some point the shearing would

agb10

tend to steepen. As it got deeper in a landslide hypothesis, the opposite would presumably be true. And this was an attempt to test those hypotheses with subsurface exploration.

Can I have the next slide?

(Slide.)

This shows a cross-section through that Trench T-1 and the borings that were developed. As I mentioned, these are very large diameter holes, they were logged in situ by geologists in considerable detail to depths of near 100 feet, and that was about the practical limit of exploration that is possible with this technique.

The heavy black shear shown in the trench area here was the main shear feature exposed in that trench. There were a series of lesser shears, the hanging wall of that feature, and it was initially this particular shear that we are going to attempt to find, going to the upslope projection or downdip projection of the feature.

What we encountered were several lines of evidence which we interpreted as indicating that this main shear and another fairly prominent subsidiary shear in this part of the trench, at least within the limited downdip exposure, tended to flatten back underneath the hills, the Vallecitos hills continued to rise up to the right there. And that was based on several lines of evidence.

Basically, that both the section above the shear in aqb11

Bore Hole 1 between these shears here and above shears in Bore Hole 3 between the shears were similar lithologically. The positions in which the shears were interpreted in all four locations were oxidized soft reddish clay units on the order of several inches thick with shear fabric well developed where the exposure was wet enough to see it.

In this particular bore hole, where the wall conditions were a little bit drier, it's more difficult to see well developed shear fabric, but the character otherwise was very similar. And these were not common features in the section anywhere else in the bore hole, they are very distinctive mainly because of how clean they were and how fat the clays were. Most of them were gravels -- contained quite a bit more sand and gravel than those units did.

endTape 1

1462 062

Tape 2
agbl

The third point that suggested that correlation to us was the fact that the section above these shears, in both bore holes one and three, was distinctive from that below and that the materials above were generally reduced and greenish in color and there is concentrations of carbonate kind of indicating above those shears that over geologic time it was common for groundwater to pond there. The sections underneath were more typical reddish colors of the Livermore gravels.

Next slide, please.

(Slide.)

MR. PHILBRICK: Did you have any photographs of those things?

MR. YADON: We have a few, but it was kind of tough down there with water coming in and not real effective conditions for photographing.

MR. PHILBRICK: Did you have a change in fabric at the horizontal planes that are shown?

MR. YADON: There was certainly shear fabric developed in those shear zones of several inches.

MR. PHILBRICK: What do you mean by shear fabric?

MR. YADON: Okay, many anastomosing near-horizontal shear planes with slickensiding and stretched nodules of calege which were stretched in an algon-like fashion in a sense consistent with the planar fabric of shearing seen in those zones.

xxxx

agb2

MR. PHILBRICK: What was the strike of the striations?

MR. YADON: Variable.

MR. PHILBRICK: Variable in what direction?

MR. YADON: In a gross sense, I'd say from something on the order of north-south to northwest-southeast.

MR. PHILBRICK: You hold those things to represent soles of slides?

MR. YADON: That was our initial interpretation, on looking at these, we felt that was a reasonable hypothesis.

MR. P"ILBRICK: And what was the motion of the slide itself, if they represent the sole of the slide?

MR. YADON: Well what we would have been looking at was the toe overriding, in that particular position near-horizontally from northeast to southwest, and then as the shears dip -- come into the southeast, the shears begin to climb and we would be looking at the overthrust toe of that feature.

MR. PHILBRICK: Why would you have a strike that would change away from the direction of gravity?

MR. YADON: I'm not clear on the question.

MR. PHILBRICK: Since the strike of the slide map is roughly northwest, what's the direction of --

MR. YADON: I'm sorry, when I indicated the strike as being from northwest to northeast, that was the strike of the

agb3

plane representing -- the section of the plane --

MR. PHILBRICK: I'm not asking about that, I'm asking about the striations on the plane.

MR. YADON: Okay. Those were highly variable and, in general, as a result of the exposures down there, I just couldn't confidently say which direction those were trending, the exposure was not sufficient. We were looking at a small hole in the casing and were unable to confidently determine a gross direction for the striation itself.

MR. PHILBRICK: Did you have layering in the sheared zone?

MR. YADON: Yes.

MR. PHILBRICK: Did the striations parallel each other in the different layers?

MR. YADON: Where striations were present, they were in the plane of the shearing that was observed.

MR. PHILBRICK: I don't care about the plane, I want to know the direction, the pitch, the plunge.

MR. YADON: Maybe what I could do is have someone take our phase II report--our phase I report and those slickensided striation directions are indicated on the logs and that would help my memory.

MR. PHILBRICK: Could you do that so that it could be established that the slickensides were in a position and a given direction?

agb4

MR. YADON: Yes, I'll go through and supply that information for you before we break today.

MR. PHILBRICK: Could we have that later, Bill?

PROFESSOR KERR: We certainly can present it later.

You said in an earlier response to a question that your initial interpretation was that these were due to slides. I didn't understand what the significance of that -- of your initial interpretation, has your interpretation changed?

MR. YADON: Let's go back to the other slide, back one slide, please.

(Slide.)

Essentially the interpretation that this data was supporting of the landslide hypothesis, which is the fact that these shears seemed--in the exposure we had, tend to flatten, which is consistent with the fact that at some point they have to begin to climb upslope. And that's about as far as this data would take you in that interpretation. We see nothing in that -- we have seen nothing in the additional exploration which suggests that that's an unreasonable interpretation of the data.

PROFESSOR KERR: Thank you.

(Slide.)

MR. YADON: Okay. So recognizing that there is at least a viable hypothesis that we have large-scale landsliding in the hills northeast of the GETR has decided that in order to

agb5

continue to address the alternative hypothesis that there may be tectonic faulting either associated with that or that features may be faulting, we decided that we would have to begin to look outside of the area of possible influence of the slide mass which is shown bounded in red here from our photo-

3.120

interpretive efforts in phase I.

And the first place we began to look was to the southeast, essentially along the projection of the strike of the features that had been mapped as photolineaments by earlier workers and the general strike of the shear features we encountered, and along Dr. Herd's trace of the Verona Fault.

(Slide.)

This will focus in on the area. This is a colored version of the Herd's 1977 report.

Basically what we felt was that since this area in here may be involved in landsliding, that to look for evidence of faulting away from that, influence of that, we would go to the southeast, to an area where we had -- in Herd's

xxxx

interpretation -- an alluvial unit, QOA-4, which appeared to cover the extension of the Verona Fault to the southeast. And our hope was that we might be able to find some stable materials or stratigraphy in that area to help establish controls on any possible faulting in the area.

May I have the next slide?

agb6

(Slide.)

This slide is a photograph looking from southwest to the northeast of some prominent drainages that are developed within this general area across the strike of that feature.

What we found, when we began our detailed mapping in that area, was a very well exposed marker horizon which kind of guided our mapping in the area subsequently. This was kind of a reflight exposure.

The cemented conglomerate seen in close-up here which can be traced in the outcrop for very long distances, up this drainage and the subparallel one to the southeast and this marker horizon was in the units mapped as QOA-4 by Herd and it was in units mapped earlier by Hall as Livermore gravels.

Next slide, please.

(Slide.)

Our interpretation from field mapping was that it's fairly clearly established that this particular unit, which can be traced in near-continuous outcrop for long distances up and down these valleys, was clearly overlain stratigraphically by the Livermore gravels section in the Vallecitos hills to the northwest, and we also encountered a generally finer grain section of sedimentary rock which was stratigraphically underlying this conglomerate unit, and the dip of this conglomerate unit was shallow to the

agb7

northwest in this general area.

It was our interpretation after continuing mapping out there that rather than being a younger alluvial unit, that this whole sequence was all part of the Livermore gravels section. And we informally named these units for mapping purposes -- the classic Livermore gravels exposed in the hills were known as the upper unit, stratigraphically underlying conglomerate was known as the middle conglomerate unit and we referred to the underlying section stratigraphically -- the middle unit as the lower unit of the Livermore gravels.

Next slide, please.

(Slide.)

This is a map that was generated during Phase I and represents the extension of mapping efforts that began in this general area here.

This is looking at this particular drainage. And what we found is we were able to map a fairly well exposed outcrop, this middle conglomerate unit in light brown from here to the southwest.

We follow the surface outcrops in this area through interpretation of a pipeline log in this area and surface exposure to the west of the GETR.

In the next picture, I will show a photograph of an exposure of this conglomerate unit on the west side of the Vallecitos Valley and attempt to show you the similarity

agb8

of that distinctive rock unit in that area.

Next slide, please.

(Slide.)

xxxxxx

This is another reflight exposure: the cemented conglomerate with topography and class similar to what we've seen in the southeast. The GETR is to the east in the background.

If I could go back one slide, please.

(Slide.)

We felt we had fairly effectively mapped this unit going all the way around essentially surrounding the Vallecitos Valley and the exposure was clear enough and well developed enough throughout this area, particularly in this area and in several arroyos that were eroded into the hills over here, to indicate to us that there was no obvious faulting offset of that marker horizon on a projection to the southeast of faults previously mapped along the hillfront.

And we felt we were looking at an unbroken sequence of the very shallowly-dipping middle conglomerate unit in here up to about 1500 feet of the hillfront up this arroyo and this one over here.

Another thing that we looked at -- if I can go two slides, please.

(Slide.)

After having mapped this middle unit of the

agb9

Livermore gravels, we went back and examined in some detail the electric log and the sample information that was available for the Foley Number One wildcat oil well log which is located in this position.

And by projecting downdip, directly downdip from surface outcrop of the middle conglomerate unit exposed here through the Foley well. we encountered in the Foley well what we interpreted as the presence of the middle conglomerate unit on that downdip projection on a basis of our interpretation of the resistivity character and spontaneous potential character in the Foley well. And I don't have a picture of that log in this part of the presentation but Dick Harding will be bringing that point up again and show you that log, and we can discuss our interpretation of the presence of middle conglomerate in that well at that time. I just want to show you what the section looks like looking toward the northwest on the next slide.

(Slide.)

These are the surface outcrops, fairly shallowly-dipping middle conglomerate; the middle conglomerate is actually a fairly thin unit in between the darker and the lighter browns here. Downdip projection intercepts Foley well at about this area and right where we interpreted its presence on the basis of the E-line.

Next slide, please.

agb10

(Slide.)

As part of Phase I, we continued mapping the general area shown here and were able to eventually follow the middle conglomerate outcrop in our interpretation unfaulted to a point to the northeast about here, where the section thinned and apparently pinched out.

And continuing mapping in that area, an examination of aerial photos revealed the presence of a sequence of alternating, generally coarse- to fine-grained units in the upper unit of the Livermore gravels in patterns approximately as shown in this stippled pattern on the slide.

These were not meant particularly at this scale to represent individual beds of gravel versus individual beds of finer-grained material, but rather the fact that we have a bedded sequence. And the map projection of those generally fine- and coarse-grained sequences kind of follows that trend shown here. And I will discuss what might be the significance of that a little bit later on.

In addition, we did detailed mapping to the northwest where again we felt we were outside the influence of possible landsliding along the hillfront in this area. And there was a difference of interpretation of several points of evidence as to whether or not there was evidence for faulting along the hillfront in this area. I'm going to show you an aerial photograph -- well, it is an oblique photograph agbll

from light aircraft looking to the northwest at this general area and discuss those issues next.

MR. PHILBRICK: What is the yellow stuff there?

MR. YADON: This is the interpreted landslide debris limits, gross limits of the landslide complex that was inferred on the basis of the photointerpretation and the presence of shearing at the base.

MR. PHILBRICK: Do you want to discuss that now?

MR. YADON: I prefer to hold off on that a little bit.

MR. PHILBRICK: Okay.

MR. YADON: Can I have the next slide, please?

(Slide.)

We're looking toward the northwest. This is the general limit of the hillfront, the Vallecitos hills are on that side, the valley here is Happy Valley, which I pointed out before.

The main basis for mapping faulting through this area was cited as the linear hillfront along here and the presence of springs and seeps along that trace and, in addition, there was an outcrop at Sycamore Canyon up in this area, where an attitude on the Livermore gravels was mapped by us as being vertical and that was cited as being compatible with faulting along the hillfront.

Our interpretation during Phase I, having done

agb12

the mapping out here and the imagery interpretation, was that all of these things could be explained by geologic factors other than faulting, in other words, there was nothing in those lines of evidence that we felt really pointed that strongly toward a fault interpretation: First of all those were that in details that, at least in our view, the hillfront is really not a particularly linear feature; broad re-entrance where the drainages come out and certainly we felt that simple erosional processes could develop a hillfront and hill valley juncture which we are looking at here.

Secondly, in the matter of springs, we know that the Livermore gravels section of these hills is dipping shallowly to moderately to the northeast down beneath these hills.

And we interpret the springs or wet spots -- I think maybe you can kind of see them in this photograph, the little bit darker area here coming up in this area, it's kind of a seepage in here and a wet spot here -- all of these spring areas, to our view, pretty clearly follow up into the canyons and back out along the hillfront and they were not aligned specifically at the hillfront but climbed into the valleys. And it was our interpretation that these were due to impoundment of groundwater on finer-grained clay beds or sections in the Livermore gravels with water percolating down to those impermeable horizons and then approaching ground

agb13

surface because they were unable to continue percolating downward under gravity.

And this was a relationship that was seen fairly commonly up in the hills, and apparently led some of the earlier workers to interpret springs up in the hills as also evidence of faulting, but we felt that that was -- that that could be stratigraphic control without any problem.

So far as the issue of the vertical bed in Sycamore Canyon, that was only one of quite a number of attitudes in that area. The attitudes there were generally a little bit steeper than some that we had seen further to the southeast, and I will address that a little bit later on in my talk here.

Next slide, please.

(Slide.)

Basically the field mapping, the initial trenching and excavations that I discussed concluded the Phase I investigations and on the basis of that work, a series of conclusions were reached.

Just briefly, these were that we felt that the Livermore gravels could be mapped as three distinct units, and you've seen those on the map that I've been talking around. At this point, the low-angle hillfront shears were thought to delineate the toe of an ancient landslide complex; the stratigraphic relations of the middle conglomerate in surface

agb14

outcrop and where we interpret it in the Foley well, in our interpretation, precluded post-Livermore gravel faulting through that Foley well.

An unbroken middle conglomerate unit was mapped across the southeastern extension of strike of many of the map fault traces from the Vallecitos hills, evidence that was initially cited for the existence of faulting northwest of the interpreted landslide complex we felt could be explained by other geologic conditions.

Finally, if we can go to the next slide....

(Slide.)

In the Highway 84 pass area -- Highway 84 comes down generally through here -- we felt that the presence of unfaulted middle unit conglomerates throughout this area up to this point and the at least gross structural continuity of Livermore, upper unit Livermore gravels represented by the alternating sequence shown here would put some limits on any possible faulting that might occur through this pass area either related to an extension of perhaps the Williams trend which is shown in this area or the postulated northeast faulting along the Las Positas. And I'll be discussing that area in more detail later on here.

Next slide, please.

(Slide.)

After Phase I, a Phase I report was submitted to

agb15

NRC and was reviewed, and on the basis of that review the NRC Staff and their consultants identified four areas which they felt required additional investigation. And those were to further investigate the northwest end of the mapped Verona Fault outside the area of possible influence of landsliding; to also further investigate the apparent thinning and apparent stratigraphic discordance in the Highway 84 pass area in the area where the middle conglomerate pinches out and the gravel horizons are present.

So wet spots and photolineaments were interpreted southwest of the GETR out in Vallecitos Valley, and NRC requested that those be explored, and finally they requested that more detailed information be developed to try and characterize and place some more confident limits on the interpreted landslide complex.

Now what I'm going to do here is --

PROFESSOR KERR: Excuse me, Mr. Yadon, is this a logical place for us to take about a 10-minute break?

MR. YADON: Yes, this would be a good place for that.

PROFESSOR KERR: I declare a 10-minute break.

(Recess.)

PROFESSOR KERR: Will you please be seated so we can continue?

Mr. Yadon, you may continue, please.

agb16

(Slide.)

MR. YADON: A brief recap. At the end of Phase I, we had review and request for additional investigationsand I'll be addressing each of these points in this sequence for the remainder of my presentation.

Next slide, please.

(Slide.)

This again is our index map, just to get you oriented. All the features shown in green superimposed on the base are the Phase II exploration features that were performed in order to address the four points listed previously. And we'll be going through those in order of the E series first, northwest, A series second, to the northeast, the B series in the area of photolineaments southwest of GETR and finally the F, G and D series in the landslide complex area.

(Slide.)

The first area we're going to look at is one highlighted in red. And not the next slide, but the one subsequent to that will be a map that is bounded by that area, and it will be north up. The next slide is a low altitude air photograph of the general area, just to give you a feel for what we're looking at.

(Slide.)

Here is the oblique aerial view, Happy Valley is in the foreground. Just for later orientation, this road here b17

is Alical Street, that is the remains of Trench E on the ridge in the center of the slide.

Next slide, please.

(Slide.)

This map is a geologic map of the same area looking north.

After a Phase I investigation, we had done detailed geologic mapping of all the available outcrop exposure in the general area outside the landslide complex but along the postulated northwest extension of the fault, and that's shown by the colored units. Again, the upper unit and the middle unit of Livermore gravels, the attitudes are shown and also are the north side.

In addition to having done that mapping during Phase II, we went in and did some surface exploration in this area to further refine our understanding of the geology here.

DR. MARK: Excuse me, when did Phase I end and Phase II begin?

MR. YADON: Timewise?

PROFESSOR KERR: About 10 minutes ago, Carson.

(Laughter.)

MR. YADON: Do you mean chronologically? Would you like a date?

DR. MARK: A date, yes.

MR. YADON: Does anybody recall? February, 1978, agbl8

I think, was when the Phase I report was formally submitted.

Okay. So, just to recap, the postulation of faulting in this area was mainly on the basis of a grossly linear hillfront represented by this contact of Livermores and alluvium.

PROFESSOR KERR: I'm sorry, what were the descriptive adjectives before "hillfront?"

MR. YADON: Grossly linear.

PROFESSOR KERR: What's the difference between grossly linear and linear?

MR. YADON: Well in my view, although this isn't topographic, I think the geologic contact reflects the topography fairly well in this view and, if you were to change scales on this map and get way back from it, you might describe that general contact between the tan and the white as a linear feature. The more closely you look at it in detail, though, it begins to look less linear.

So I'm kind of compromising and agreeing that there is some indication of linearity along that hillfront.

PROFESSOR KERR: Thank you.

MR. YADON: So we had a mapped fault trace based primarily on that geomorphic evidence. We also, during our Phase I and early-Phase II mapping, identified several photolineaments shown in the red here which we wanted to investigate in this area. They represent features in the general

agb19

zone mapped for the northward extension of the Verona.

The solid green line here is what we call our Trench E, and that's about a thousand-foot long trench that was excavated across the mapped trace of the Verona Fault which passes through this area and all three of the photolineaments which we defined in that general area.

And the conclusion from the detailed logging in that trench was that there was no evidence of any shearing of the low angle thrust character that was seen to the southeast along the base of the hillfront anywhere in the trench, there was no direct correspondence of shearing with any of the photolineaments of the mapped trace.

There were two minor shear features which were near-vertical, as I recall, with somewhat of a west-dipping attitude that, in the Livermore gravels, showed indeterminate offset, and those two shears were in the more northeasterly and in the trench, and they are both capped by an unfaulted Paleosol horizon, which will be discussed in the next presentation, and were interpreted to be on the order of 70- to 125,000 years old.

So we felt it is fairly clear that along the hillfront in the area of the mapped photolineaments and across the mapped trace of the fault that there was no evidence for such faulting at Trench E.

These two lines are shallow seismic refraction aab20

lines which were put in before the trench in order to gauge trenching conditions, groundwater conditions, and interpreting those, there was also no suggestion of faulting in our interpretation of those.

In addition to the trenching and the outcrop evidence, we ran a reflection line, essentially this green dashed outline, along Alisal Street. And the interpretation of the geologic structure across Happy Valley that I'll show you on the next slide in cross-section view looking west, was based primarily on the seismic reflection line, and that interpretation was refined and corroborated by the outcrop evidenced by exposures in Trench E and by some available water well logs of particularly P-10 and F-3 water well. If I could have the next slide, we'll look at that section.

(Slide.)

Again we're looking west with north to your right and south on this side. Basically what we encountered in Reflection Line 1, which is from here over to here, were a series of acoustic units of variable definition along the trend of the -- along the length of the line.

And on the basis of the character of those acoustic units, we interpreted lithologic equivalents for those, and those are shown here.

The upper area here is interpreted as upper unit Livermore gravels, it also shows an outcrop at this end of the

agb21

line and in Trench E in the hills to the north.

This middle unit, which showed fairly well defined acoustic boundaries on its top and bottom, was interpreted as the middle conglomerate unit. That is, again, a projection of surface outcrops which is shown here and it's also corroborated by indications in the well logs for P-10 and F-3 as encountering a cemented gravel unit, which we interpreted as middle conglomerate here and here.

And beneath that unit, on the basis of the character of the seismic reflection profile, we interpret that we have the presence of a section of lower unit, generally finer-grain, Livermore gravels. And beneath that, from outcrop evidence and interpretation, again, of the reflection record, that we have browny sandstone.

In this particular area, from about Station 150 to about 105 right through here, the continuity and definition of this particular acoustic unit was obscured, data dropped out some -- if I can go back one slide -- that was attributed to the presence of a veneer of unconsolidated alluvial fan material from -- debouched from Sycamore Canyon coming out generally in this area.

And we can have the geophysicist discuss the significance of that feature in terms of producing the data dropout, but it's our interpretation that the data dropout there was due to the presence of that thin loose unconsolidated

agb22

material in that portion of the line. Regardless of the area, a photolineament passes through that part of the reflection profile. The extension northwest of that trend, again, crosses Trench E, no indication of faulting. We see no basis to interpret the data dropout as being related to faulting.

Next slide, please.

(Slide.)

The character of the acoustic unit over in this area was very similar and there was at least some suggestion of continuity through here, so we interpreted the middle conglomerate to extend completely across the section.

We saw no evidence in the interpreted section for faulting, other than a fairly minor intra-formational fault, primarily tertiary, possibly disturbing the base of the lower unit of Livermore gravels and certainly no gross offset or significant offset we can see in the middle conglomerate across this section.

Next slide, please.

(Slide.)

So it was our interpretation that on the basis of all the exploration we did here, that there was no evidence for northwest extension of faulting of the mapped Verona trend.

The next point that was brought up in the review concerned relationships in the pass area. And a few slides

agb23

down the line here again we will have a mapped view encompassed by this red line. Just to forewarn you and try to keep this in line, when we get there, the view will be to the northwest. This will be the top boundary, so you will have to tilt yourself when you're looking at that one.

Next slide, please.

(Slide.)

This is a reminder of the exploration that was conducted in this area. It was an attempt to develop additional exposure, additional information, particularly in the area between where these gravel sequences, alternating sequences seemed to die out and, additionally, between the area where we pick up the middle conglomerate unit, the unbroken section through here looking at that area. Just to give you a couple of aerial views of the subsurface trenching exploration we did in that general area to give you a feel for the ground up there.

Next slide.

(Slide.)

This is one view of it. The gravel horizons are off to the right here. This end of the trench intercepted that sequence and followed along.

Next slide.

(Slide.)

It continued along the ridge crest until it acb24

encountered the northeasternmost outcrop of middle conglomerate used right there.

Next slide, please.

(Slide.)

This is the map view that was bound in the red box you saw previously and again northwest up rather than north.

Okay, basically this is just an orthophoto map that covers that area and the brown unit designated here is the outcrop pattern of the middle conglomerate. There's fairly large scale landsliding in that area. I'm not sure you can see too well on this slide, but we kind of highlighted where best exposed the more gravelly sections along the ridge crest in this area.

These correspond in detail to the general pattern of alternating sequences that you have seen in the other map. The green here is Trench A, that's the one you just saw an aerial view, the first shot we had was this part and the second one there.

The initial excavation of Trench A showed what we interpreted as fairly typical upper unit Livermore gravels in a sequence of both alternating fine- and coarse-grained units. And except in one place in the trench, we saw no indications of any faulting. The one exception was right about here at this bend.

agh25

In the initial trench, what we encountered there was a kind of an anomalously deep accumulation of alluvial soil. And the initial trench excavation, we were not able to get to the bottom of that to try and interpret why it was there, so additional exploration was done. And if I could have the next slide, I'll show you the trench right in that area, the subsidiary trench that was put in.

(Slide.)

This was designated Trench A-2, and it was about a third or fourth attempt to get to the bottom of the soil accumulation and try and understand what was happening there.

The soil accumulation is shown in the dark-brownish units here, a bolt-shaped depression. And in the base of this trench, particularly down in this part of the exposure, we exposed Livermore gravels continuously all along the trench, which is what we were attempting to do.

Next slide, please.

(Slide.)

This is just an indication of the detail to which the trench exposure was logged. The wall you were just looking at was the northwest wall of the trench, it's that one. This is the opposite wall, and that's the southeast end of the trench over there.

Next slide, please.

(Slide.)

agb26

This is a simplified version of the previous one, they show you some general relationships which were interpreted in that trench.

First of all, there is a major structural element encountered when we finally got into bedrock beneath this dark brown soil material which is represented particularly by this heavy black main shear zone and this red area which further defines the very complex shearing in this part of the trench. And I'll be talking about that more in just a minute.

To the northeast, we had kind of an alternating sequence of both finer-grain, coarser, brown, Livermore gravels.

On this side of the fault, we had essentially a gravelly sequence of Livermore gravels which were distinctive in the fact that they didn't have the interbedded finer units such as this section, and they were generally reduced greenish colors, as opposed to the more reddish oxidized colors that were here. Similar relationships in the other trench.

In the next two slides that I'm going to show you are going to be views of the actual trench wall looking right at this general zone, this feature, and the next one, the southeast wall at the same feature.

(Slide.)

Basically what we have here is that very heavy black line, the main shear zone -- I'll describe a little bit

agb27

later here -- is this sharp contact. It comes right up through here. You can't see it too well on this, but it was cleaned and traced across the trench here and it was followed, wrapping up through here and extending some distance into the colluvial soils and then died out. We were unable to trace it any further.

To the southwest of that main bo-.ding shear zone, there was a clayey section of Livermore gravels, it was very highly deformed, very, very complex manner. And the southwest boundary of that was another shear contact with that greenish-gravel to the southwest of this main zone of shearing.

The definition of this main shear zone which is exposed over here is on the basis of the continuity of the thin, maybe one- or two-inch wide series of essentially parallel shearing planes, very well developed shear fabric and the continuity with which that single thin zone of deformation could be followed.

There was also well developed mullion structure or kind of large-scale slickensides or route hike shear fabric with the axes, the long axes of the rods essentially horizontal and sometimes with a slight northwest dip to those.

In addition, slickensides also followed that same pattern striking along the fault which, on this particular plane, was about a north 65 to 75 degree west strike. Slickensides and mullion structure were in the plane of that shearing

1462 089

agb28

and dipping or plunging a little bit to the northwest.

And the shear fabric in this fine-grained unit which was caught up in the fault zone was a complex of variables. There were shears at every attitude, slickensides at every attitude.

Next slide, please.

(Slide.)

This is just looking at the opposite wall again. There's a well-defined main shear zone in this particular part of the trench. There's a preference for the walls to cave right along that zone. Again, very highly disturbed fine-grain unit essentially near vertical on an overall average of about 70 to 75 degree northeast dip on this main zone of shearing.

Next slide.

(Slide.)

This is an indication that -- I'm sorry, go back one slide for a moment, please.

(Slide.)

I have indicated in our view clearly the main structural element here shows at least less major displacement in a predominantly lateral sense based on the well developed mullion shear structure, slickenside and grooves along this main zone of shearing. And yet we also have a multiplicity of other shear attitudes and shear fabrics, some of them in

agb29

thrust sense in this direction, others in thrust sense this way. And just to indicate that that's not an uncommon occurrence along a structural element that is primarily lateral, I'll show you the next slide.

(Slide.)

This is a picture of a recent excavation of the 1906 break of the San Andreas Fault, and this feature here is the surface scarp associated with that faulting. San Andreas is clearly a right lateral transform fault, and yet the very complex shearing doesn't show up quite as well on this slide. But the main break in this particular feature is dipping about in this direction, shearing here, complex deformation. There are pieces of thrust in all directions from the strike-slip fault.

Next slide.

(Slide.)

We have a little close-up here. This is looking at a trench at the base of that previous exposure again, actually a fairly low angle thrust feature along that major strike-slip fault.

Next slide.

(Slide.)

Back to the kind of simplified trench log.

Basically what we interpret is that again the main structural element is this shear feature here with the

agb30

accompanying shear zone, this wedge of fine-grained sediments showing predominantly lateral movement. The sense of that offset, whether right lateral or left lateral, is not apparent in trench exposure. It is clearly lateral. And further, if there is an overall sense of normal offset in addition to the lateral that we see, it's fairly clear that that normal sense of offset would be east down, cumulating the very thick sequence of soils seen here and obviously west up, where we have not only the absence of those soils but a topographic ridge on the southwest end of that trench.

Next slide, please.

(Slide.)

Back to this slide, the trench we were just looking at is right in this area here. That's where this north 65 to 75 degree west faulting is most clearly shown.

Going back and re-examining trench exposures here at the main trench and the side trench here and also following the location of that thick low-velocity accumulation of soils with seismic refraction techniques, we were confidently able to extend that faulting we saw in Trench A at least to these limits, that's as far as we carried it with the seismic refraction at the time.

So far as where this northwest trending structure extends beyond this, we don't have an interpretation on that with any great confidence right now.

agb31

(Slide.)

This slide will indicate that in our interpretation there are fairly clear photolineaments that extend in the same general trend as this mapped fault in this area also defined by this ridgeline in here, and there appears to be an interruption with another photolineament found up in this area.

Additionally, across that general boundary we are seeing that ridgecrests to the northeast of that boundary are generally trending to the northwest. Across it we go almost into southwest trending ridgecrests. There clearly seems to be some structural control along that trend.

Next slide.

(Slide.)

Okay. So we discussed both the first two points. The third point brought up in that review was to investigate further photolineaments that various workers or reviewers had recognized southwest of the GETR and the B series 1, 2 and 3 H series trenches were excavated for that purpose. And the next will be an aerial overview of this, and then we'll look at the map bounded by red.

Next slide.

(Slide.)

Okay. Here we're just looking at the GETR, the Vallecitos hills. This is Trench B-1. There's a small break

agb32

in the trench to maintain the road here. Trench B-2. A series of side trenches on B-2. Trench H series, actually three trenches down in that area.

Slide.

(Slide.)

This summarizes the information we developed in that area. First of all, the photolineaments that were of initial concern and caused us to do this exploration in the first place are shown by green lines. And this one in particular was associated with wet spots generally along its trend.

What we found after excavating these various extensive trenches was, first of all, there was no faulting associated with this photolineament at all. Rather, the photolineament coincided with a buried gravel channel beneath the present geomorphic surface in that area. And we just presumed on that basis and geomorphic evidence that it accounts for the rest of that lineament, a long trend.

Again, the further southwest photolineament shown here, no evidence of any shearing or faulting directly associated with it. However, upslope we did encounter another low angle shear. It's known as the B-2 shear. That's where it was encountered. It's generally similar in character to what we call the B-1/B-3 shear or hillfront shear, which was originally encountered in the Phase I investigations.

Dr. Shlemon is going to be describing this general

7b33

area and some of the details of what was encountered in these trenches that offset relationships, ages of units offset, in detail, so I'm not going to spend too much more time at this point on this.

I just want to show you a cross-section view so you get a third-dimensional view of the situation here, and this will be looking northwest the section through the trenches.

Next slide, please.

(Slide.)

Again the Vallecitos hills to the right, the yellow indicates the trench. This is to scale, at least approximately. The B-2 shear encountered here. The B-1/B-3 shear there. The projection of the GETR.

Next slide.

(Slide.)

Okay, the fourth area of concern in the NRC review was characterizing and defining better the limits of the interpreted landslide complex in the hills northeast of GETR. The initial scope of investigations for Phase II called for excavating Trench D, which is shown right here in the area that we originally interpreted as representing the eroded head scarp of this massive landslide complex in here.

Upon excavating that, we found that in general there was fairly continuous stratigraphy across that break

agb34

with Livermore gravels dipping back into the hills in similar fashion that we determined in field mapping before.

And at the time that was uncovered, we were also developing quite a bit of information on soil stratigraphy and the age relationships for shearing at the base of the hills based on Dr. Shlemon's work, and that caused us -- this work caused us to modify our concept as to how well present geomorphic form in this interpreted landslide complex matched the geomorphic form of the feature when it actually originally failed.

Essentially this information led us to conclude that if there were landsliding in those hills, that it was very, very much older than we had originally interpreted and it was very highly modified.

And we felt what we might be looking at up here, rather than the actual head scarp area of the landslide, was something more analogous to a fault line scarp, in other words, an eroded reflection of a head scarp area that would now then be stripped away and would then represent a downslope, some amount.

To check that interpretation and also to provide more exposure for control on the character of the Livermore gravels above the shears exposed at the base of the hills, both the G series trenches and F series trenches were excavated.

We also tried to do some seismic reflection work

agb35

up on the hills. But the quality of the information received from that wasn't usable, there weren't enough reasonably valid geologic reflectors to do any interpretations. So that technique was abandoned in that particular area.

The basic finding of the trench exposures here was that although we didn't find any large polely infilling zone which might be expected at the top of a large landslide complex such as we were interpreting here, we did find several high angle breaks in G trenches, and one in F, that had a normal sense of offset on them, looked to be tensional features and which we felt might say something about a landslide interpretation, and I'll 'iscuss those in just a moment.

The same features were examined by the NRC reviewers and site visits, and it was suggested those high angle features in an alternative interpretation were related to recent shallow landsliding which is fairly common in the hills up here.

(Slide.)

This slide explains why we think that is not a valid interpretation for the origin of those high angle features. Here is the G trench here running up the ridge crest.

And the area where we first encountered one of these -- two of these high angle breaks, as a matter of fact -- in the G trench area, we put in a series of smaller backhoe trenches to follow those features along their strike and try and get better definition of them.

agb36

Fairly clearly this is a landslide which is active, a head scarp which offsets modern soil and grass here, actively moving downslope. And it's our understanding that this type of sliding was postulated for the origin of these features.

Clearly the fact that these trends cross the ridgeline and also the fact that in all the trench exposures these shears are capped by unbroken soil units, presumably Holocene age, we don't have a firm age date on those but of the same order of age or probably even older than soils that were clearly disrupted in the head scarp, we felt that another origin had to be invoked.

I might also mention that these were not common features in the general section here. Because the section was folded, there were bedding contacts that appear to have undergone some degree of slip, there were some clay units in Trench F that, again, seemed to show some internal bedding plane or at least bedding parallel slip in the clay units.

But these features in Trench G and a similar one in F were the only ones we encountered in very detailed logging of these which were high angle normal offset on the southeast side down relative to this.

Next slide, please.

(Slide.)

What I want to show you next is just a geologic cross-section extending through the G series trenches and then

agb37

down across the hillfront and out into the Vallecitos Valley, and at least propose a possible explanation for these high angle shear features we're seeing in Trench G and F and seeing how we might relate that to what we see at the base of the hills.

Next slide.

(Slide.)

This is that profile, the GETR projects to about there. This is the shear feature exposed in the Trench B-1/B-3. There's the one we encountered out at B-2. And these are projections either directly at the trench or -- of the high angle breaks that we are seeing.

C

The bedding in the Livermore gravel section exposed in Trench G is shown by the little short dashed lines here. The brown is predominantly coarse-grained units, sandy gravels, coarse sand. And the grey areas are predominantly fine-grained units, generally silts, clay silts, sandy silts, those type of things. And these breaks shown here are the actual attitudes of those breaks projected onto this section.

And it seemed to us not completely untenable that maybe what we're looking at was that these high angle features up in the hills may represent -- if I could have the next slide --

(Slide.)

-- the deep-seated remnants of what might have

agb38

been the pull-away zone on a postulated earlier land surface which in some fashion or another connect with the toe shears down in here.

And since possibly -- or at least many tens, possibly hundreds of thousands of years since the main movement on this interpreted landslide concept, we have since then stripped that portion of the sliding to end up with what we see out there.

Next slide, please.

MR. PHILBRICK: Have you made any slip circle analyses of that slide?

MR. YADON: As a matter of fact, we have not on this particular section. On an earlier interpretation or section, essentially the same orientation, down across the Vallecitos hills, some simplified Bishop analysis method of slices, static analyses were run. The remaining condition -- we did not run analyses on a presumed prior condition of a land surface somewhere in this area. And using strength values which were developed from some other consultants' strength tests on materials encountered beneath the GETR, the foundation area of the GETR and using some very conservative interpretations of groundwater conditions, a series of cases -- for instance, essentially a fully saturated case with the groundwater at the surface and then a couple of other groundwater levels in the slide mass, the static stability analyses

1462 100

agb39

indicated that the remaining slide material in this profile were stable. Factors of safety in that analysis on the order of two, two and a half, something like that.

We tried to gauge what the effect of a seismic input on top of that static condition would be. Because of the data available at the time, that was kind of more of an exercise of interest than a formal analysis.

But the conclusion of that was that it would take something on the order of many tens of g, five tenths up to eight-tenths the range of g values considered at the site to cause reactivation on that.

And in a most conservative analysis, a most simple a conservative analysis using a Newmark-type approach, we might expect to get on the order of inches to maybe a foot of movement of those landslide masses, given the section we looked at.

MR. PHILBRICK: That's a section that goes through the noses?

MR. YADON: Yes, it's a section similar -- one case that was analyzed was similar to this.

MR. PHILBRICK: No, but I mean the surface, the surface profile that you're taking as a profile looking down a nose.

MR. YADON: Right.

MR. PHILBRICK: Did you make any analysis of

agb40

anything going up the hollows?

MR. YADON: No, they did not.

MR. PHILBRICK: What would be your impression with respect to relative stability?

MR. YADON: Given the range of conditions that we tried to model on the section through the noses, it's just my judgment that there would not be a very significant difference in the answer. I think without seismic loading, probably end up again with factors of safety --

MR. PHILBRICK: What's the difference in elevation between the head scarp on the present scarp that you showed us, that present active slide. What's the difference in elevation between the head scarp on that present active slide and the toe of that slide?

MR. YADON: Okay, let me make sure that I'm reading you right.

MR. PHILBRICK: All I'm asking you really is how deep are the gullies going back into the hills.

MR. YADON: I think the relief between ridges and adjoining gullies is, what, on the order of maybe 50 or 100 feet local relief, something like that.

MR. HARDING: I think so.

MR. PHILBRICK: I think you are way short, I think they're closer to 200 feet or more. If this has stabled under these present conditions, then the gully profile will be stable

agb41

by a great deal more. If that's the case, have you investigated the offset in the stone line opposite the mouth of any gully?

MR. YADON: Not directly, no. The trenches are --

MR. PHILBRICK: The trenches have been on the noses?

MR. YADON: Right.

I'm not sure that we would encounter the stone lines in gullies because of erosional considerations, I don't think that part of the section is preserved there.

MR. WHITE: Before you go on, on your diagram you show some small black lines near the surface, near the ground surface. Do those represent bedding planes?

MR. YADON: These are the apparent dips of bedding planes exposed in the G series trenches. They have just been extended slightly downdip from their surface expression.

MR. WHITE: How old would they -- would they be old discontinuities?

MR. YADON: These are actual bedding contacts between units of the Livermore gravels, and, just as a ballpark figure, these particular rock units exposed on the hills are on the order of at least a half a million to several billion years old. So these contacts here were developed during deposition of this rock sequence during that time span.

MR. WHITE: If those were slides, there would be rotation and those bedding planes would not be horizontal,

agb42

they would suffer the same rotation as the whole mass of earth.

MR. YADON: Yes, if this is a completely accurate representation and if -- the possibility is this is not a completely accurate representation, we might have had a fairly significant component of block sliding and lateral translation in this part of the slide and getting the toe thrusting down here.

MR. WHITE: I guess what I'm saying is, or asking is this any kind of useful evidence that would either help or hurt your hypothesis?

MR. HARDING: We have looked at that to try to determine if we could see some disruption in the bedding that would definitely be related to sliding, and it turns out that because we have folding of the Livermore gravels prior to any sliding, that really doesn't help you. Also, if you assume circles of this size, the center of those circles are going to be several hundred feet above the existing landscape, the center of rotation. And with that large of a circle, you could get several hundred feet of slip along those planes, assuming a purely rotational slip with only changing the dip of those beds on the order of about five degrees.

So given the variations in attitude down that slope, we didn't figure there was any direct evidence one way or the other.

agb43

MR. WHITE: Thanks.

MR. YADON: If I could have the next slide, please.

(Slide.)

This is just a brief summary again in the order of the original NRC concerns and the results of our Phase II investigations of those concerns.

Just to remind you, first of all, we encountered an unfaulted stratigraphic sequence of Livermore gravels competely across Happy Valley in trenching.

In particular, there was an unbroken stage five paleosol which Roy will discuss next on the order of 70- to 125,000 years old across the map trace of the Verona Fault and all the photolineaments projected along the hill front, and that paleosol extended unbroken.

Secondly, in the Highway 84 pass area to the northeast, we encountered a previously unmapped fault. It appears to be a fairly significant structural element based on the degree of shearing that we see there.

The pertinent features of that are that it strikes north 65 to 70 degrees west, it dips deeply to the northeast, the last movement was predominantly strike-slip based on the mullion structures, the grooves and striations. And if there is a normal component of offset, it's apparent that it is east down.

The third point, we encountered two additional

agb44

low angle shears in the Vallecitos Valley southwest of GETR. I discussed one of those a little bit in detail in Trench B-2.

There was a third similar shear encountered in Trench H which was shown on a previous slide. And the extent of that shea laterally was not determined.

Finally, in regard to exploration up in the Vallecitos hills, we encountered several high angle tensional breaks and ind rminate offsets. And we feel that the evidence we developed to date still leaves open the interpretation that those are related to very ancient landsliding in the Vallecitos hills which is related to the low angle shears below, and that we're looking at very highly erosionally modified result of that old landsliding.

The last slide, please.

MR. THOMPSON: Before you leave that and with regard to your first point, is it clear beyond any doubt that those are Livermore gravels there or could they be younger gravels?

MR. YADON: Well it's our interpretation based on the mapping and continuity of structures and outcrop patterns in that area that those are Livermore gravels. The lithologies of class, the degree of weathering, the consolidation of the unit in our view is similar to what we have seen all throughout the Vallecitos hills.

There does seem to be a little more predominance

agb45

of sandstone fragments, perhaps a greater contribution from the great valley, maybe even tertiary rocks at that end of the section as opposed to more Franciscan-dominated debris in the Livermores at the other end. But we interpret them as Livermore gravels.

MR. THOMPSON: Did the NRC people who studied the trench agree with that?

MR. YADON: I'm not really sure what their final interpretation of that was.

MR. JACKSON: If I could comment a little bit. We never came to an agreement on what the age of the material was in Trench E, we had several problems with it.

Just to show the difficulty there, not very far away -- and I don't have a map in front of me, but not very far distant to the east of this trench there are vertical Livermore gravels standing vertically on end. And in this trench they are a very low dip, if not horizontal, the gravels.

The problem we see is one of if you take a Livermore gravel, which is a really big pile of sand and gravel, and you rework it by stream erosion and you deposit it, the characteristics of that more recently deposited material looked just like the original source material, it's very hard to discriminate.

One of the problems that we looked at was the topographic -- if you look at a topographic map, the material

agb46

in this area is flat-lying, it is very low in topography compared to the Livermore gravels just immediately to the east of it. So in general we did not make a conclusion on what the age of Trench E was, but we highlighted that there was some problems in the age interpretation.

MR. YADON: It is clearly older than about 100,000 years, though, on the basis of the presence of the paleosol.

MR. PHILBRICK: Are we basically through talking about landslides or not?

MR. YADON: Dick Harding in not the next talk, but the one subsequent to that, will address that interpretation and fault interpretation in an interpretive, conclusionary way, so we'll be back to it.

Maybe if we could find the Pleasanton or Happy Valley area map we might just point out one thing Bob brought up.

(Slide.)

I might just point out the generally steeper dips that I think Bob is referring to in the Livermore gravels southeast of Trench E are the ones exposed in Sycamore Canyon here. You can see there's quite a range on these dips, like 35 to the vertical dips shown here.

A couple of the other gentlemen were the ones who actually mapped that exposure, I'm not intimately familiar

agb47

with it myself. But I might point out that in this area where dips are, in general, somewhat steeper than what we see. Further to the southeast, if you will look in Trench E, I mentioned that there were two minor shears which were steep and generally west dipping encountered beneath the Paleosol in that trench.

In the areas where those shears occur, we have local steepening of dips in what we interpret as the Livermore gravels, they are at least grossly along the bedding trend of these attitudes. And the apparent dips, at least on those, are at least up to 45 degrees in some places.

So I don't think that in a general sense these are really all that anomalous in comparison to these. We interpret that at least a major component of that dip probably relates to drag effects along the Calaveras Fault which is pretty close by, right over in here.

MR. JACKSON: Doug, just if you could, your next slide shows a section across that area. Could you go to the next slide to point out some of the problems?

(Slide.)

The flat-lying QTLGU there and LGM.

Now, if we can go back to the other slide.

(Slide.)

Now those dips were measured in that ravine -- slightly displaced from that cross-section are dips of

agb48

35 to vertical, so the cross-section doesn't really represent very well. If you're on the axis of a fold, you have some problems. This is why we requested initially a trench -- Trench C, could you point to that, because of property axis difficulties.

We believe, in general, if we're going to trench, we should not trench in the vicinity of a fold axis if at all possible but go to an area where we had well exposed Livermore gravels in this ravine and then try to trench at Trench C.

So Trench E was clearly a secondary alternative and not recommended in the initial phases, but unfortunately GE could not get in there.

MR. HARDING: Bob, I would just like to point out that that fold axis does trend toward the upper end of Trench E, and we did see some steepening of beds in there close to those shears as well as the flattening out. So where there is a synclinal axis which can be followed up the canyon off the slide to the right, it apparently flattens out as we approach Trench E.

I might also point out that on the next slide --

(Slide.)

Here you can see some evidence of folding in our acoustic unit, in the QTLGM, which is over toward the right-hand side of that section there, and it's right along the

agb49

projected axis of that syncline.

So as we interpret the structure there, what happens is you have at the bottom of the slide a rather gently to moderately northeast dipping section of Livermore gravels which flattens out as it hits the valley and then steepens up again as it approaches this synclinal trough up to the north end of that structure, and I think that's perfectly compatible with both the outcrop pattern and what we see on the seismic reflection line, what is seen in the bore holes and what is seen in Trench E.

MR. JACKSON: Just to continue this debate a little bit because it's an interesting area, and one of our problems in projecting to the northwest, at the road intersection right near the 35 degree mark, that ravine, the beds are clearly steeply dipping.

Immediately where the first red line intersects that road, the bedding is apparently horizontal. There is a clear disruption, and this is usually good evidence of faulting in this kind of terrain, it is a cross-cutting of bedding. This is why we looked in this area as a problem.

I noticed in one of the earlier oblique photographs of this area, and which I had not seen previously -- I looked at in the same light as I did this morning -- if you stand to the southeast, it's clear -- there is one thing I want to stress. I think photolineaments have been grossly stressed,

agb50

I guess, if I can use that term. When we as geologists talk about lineaments, we're talking about them in a gross sense.

The case here is that you have a hillfront which is basically linear, but the outcrop pattern, the sinuous outcrop pattern is exactly what one would anticipate if you were to get an intersection of erosional streams with a shallow-dipping fault.

So where a valley crosscuts a shallow-dipping fault, you would get a V pointing upstream. And as you can see there, at the road, this is exactly the kind of outcrop pattern you get in this location.

MR. HARDING: Isn't the outcrop pattern also consistent with the stratigraphy dipping in that direction as well as the fault dipping in that direction?

x. xx

MR. JACKSON: Agreed. And that's why we have bedding plane faults.

MR. HARDING: But that configuration is not unique to faulting, and that needs to be pointed out.

4.240

I think we need to move on now to Dr. Roy Shlemon. And in order to be able to interpret some of these shear features in terms of their age and the amount of offset that has occurred on them as relates to either hypothesis, in interpreting the origin of these shears we need to know something about the Quaternary history and the soil stratigraphy that has been offset at the GETR site, and Dr. Shlemon will try to address that.

agb51

DR. OKRENT: Before we get into the next detailed presentation, could I understand, are we behind, on or ahead of your estimated schedule?

MR. DARMITZEL: We're just about on schedule.

DR. OKRENT: I must confess for myself I feel somewhat immersed in detail. It's not completely clear to me which of the details are most important for the decisionmaking process.

When I look at the agenda, at least, it's not clear to me whether we're going to have a discussion only on the faulting question in detal or whether there is to be discussion of the seismic design basis and what the probability is of a Point Five or a Point A or a 1.0 or whatever you're talking about. I don't know whether there is intended to be some kind of a similar look at the question of landsliding. I'm a little not sure about what I'm going to have had covered -- perspective by the end of the day.

And just to add one perspective which I will mention now to the Staff, I'll be interested in hearing from them sometime before I make up my own mind what they think are the probabilities of the various things they suggested might occur at the site, how this relates to what they think are the probabilities of seismic design bases of other sites.

I am not, at the moment, willing to think only in terms of the seismic and geological design criteria on some kind of non-probabilistic basis. And I'll be interested in knowing how what you do here relates to other places where you have used probabilistic bases in arriving at a decision like the San Joaquin site, where there was something of this sort introduced into your rationale.

So let me just indicate an interest in having a broader perspective somewhere, I'm a little worried that we may not get there before everybody will have to leave the room.

MR. JACKSON: I would like to comment from the Staff point of view on several things you mentioned.

We do plan to discuss briefly the amount of fault offset that we postulated as a design consideration and point out we have had done some rough probabilistic exceedence probabilities based on a data set which, in all honesty, no one I talked to would endorse as useful for even drawing a line through the data. But we'll show you that figure in my presentation.

Dr. Shlemon will present some overviews on the probabilistic approach from a geological judgment point of view.

We have not in any way approached this site from a seismic basis, from a seismology basis on a probabilistic approach. Our approach to the decision was made two years ago in which we would, in the Geosciences Branch, which is represented

agb53

here today, would make an estimate of peaks of strong ground motion that would be estimated to be at the site.

Dr. Newmark has been contracted by the Structural Engineering Branch to come up with acceleration for design purposes for this particular site. We've done that for several reasons. Three of the seismologists assigned to this review during the past two years have resigned and left the NRC. We do not at the present time have an NRC Staff seismologist assigned to this review.

Jim Devine of the USGS has worked with us on developing the Safety Evaluation Report input we have, and its base clearly is not developed on the probabilistic scheme. We have not addressed any of the questions that you raised from a probability point of view, and I doubt very much if we can make a comparison of this site to San Joaquin.

As a matter of fact, the probability approach submitted to the Staff for review was done basically at the last minute. It's a minor addition to the total review which we have undertaken for the site.

And we, indeed -- we, in turn, have reviewed it in that context. And our review on the probability assessment of the surface offset, you will hear from a seismologist -- seismology and geology viewpoint, a judgment of the acceptability of using those kind of techniques for establishing the location and the amount of surface offset. We plan to touch on those

agb54

things.

We particularly have wrestled with this problem considerably. We see no easy answers to them. And we think that the discussion would be better based on discussing that rather than the presence or absence or arguments over landslide or faulting at this particular site.

DR. OKRENT: I have no reason to be less interested in what I stated for my interest. And just to make the case more specific, as you know, there are questions raised about sites in the middle west and so forth, about seismic design basis.

And I find it a little bit difficult to understand the Staff rationale, as I look at different places around the country, why 0.8g is right in one place and 0.2g is right in another and so forth. So I need to have some kind of relative perspective as to what is being implied.

You're using probabilistic ideas whether you say so or not, because if you say you're using the historic intensity in some zone in the middle west and you're not going one step beyond, you've made a probabilistic judgment. Don't tell me otherwise.

MR. JACKSON: I agree, and I think we can provide some comments on that. But in the context of this particular review, I think when we get into areas of ground motion and near-field ground motion, we have not in the past adhered

agb55

strongly to or even endorsed the use of probabilistic methods. And we're approaching that on several reviews in great detail, principally San Onofre 1 in the SEP methodology approach and it's much better addressed in that context and not necessarily with the people available here today.

PROFESSOR KERR: Please proceed.

MR. HARDING: I might just answer the first part of your question there. Hopefully, when I get around to my conclusions section, we'll try to bring all these details together into what our final conclusions are.

I realize you are inundated with details, but many of these details have been points of disagreement along the way and I think we need to bring them out in order to set a basis for our final conclusions. So I hope those questions will be answered shortly.

MR. SHLEMON: Getting back to the GETR site for the moment, and to put it in context, I have a presentation regarding the regional geologic setting, another presentation dealing with the site geology.

And the overriding purpose of the soil stratigraphic investigations dealing with the Quaternary of the immediate region, the site region, the overriding purpose is to date the last displacement of the shears that you have heard much about, whether in fact they have been engendered by mass wasting or by tectonism.

agb56

In that regard, there are four principal objectives of the soil stracigraphic investigations that have been spelled out in some detail: namely, in Appendices A and B of the ESA Phase II report and, secondly, in Appendix B of the EDAC report.

And these four major objectives of the Quaternary and geology, geomorphology soil stratigraphic investigations are first, one, to determine the presence: are there any Quaternary units, soil stratigraphic or otherwise, and geomorphic units at the GETR site which could be dated using these particular techniques.

Secondly, the age: if they do exist, what is the age of these particular units? Perhaps we can date them by some wonderful volcanic ash which blankets the area -- it doesn't, of course -- by fission tract or some other absolute dating technique. But more realistically by dating by relative methods, geological rate processes ar specifically, by rates of soil formation related to Quaternary geomorphic associations and, of course, in all these studies, changes of Quaternary climate and vegetation.

The third objective, a major one, in fact, was to determine the displacement history if, in fact, we can find any Quaternary units. That is, are these units displaced and, if so, by what amount.

Finally, as a fourth objective and it came out

agb57

later in the study in response to a specific question, what is the age of the sediments that essentially underlie the GETR, particularly as exposed in Trench B-1 and B-2.

And another objective, therefore -- and this is presented, as I mentioned, in Appendix B of the EDAC probability analysis -- is essentially to identify the soil stratigraphic units at the GETR and come up with their approximate age.

May I have the first slide, please?

(Slide.)

To put it in context, it's a slide you've seen before. This is the GETR site indicated diagramatically and here is the large Trench B-1 and B-2. The red lines again the B-1/B-3 shear at the base of the hill slope and this is designated here the B-2 shear.

The soil stratigraphic investigations were concentrated mainly in four trenches: particularly in B-1; secondly in B-2; thirdly in Trench E, which is off this particular slide but you'll see in just a moment; and then also in a smaller trench called Trench H which reveals a very significant and important Quaternary stratigraphy for the region.

In addition, there were some investigations of the side trenches to trace down the geometry and the amount of displacement of the Quat nary units in the B-2 shear system.

agb58

The procedures used and described in the various appendices are essentially soil stratigraphic techniques. The terminology is typically that employed by the Soil Conservation Service,and soil here means pantological profiles not soils in the engineering standpoint. The terminology used, then, can be presumably used and the area can be replicated in terms of mapping and logging.

And, Mr. Chairman, as an aside, you had asked earlier whether -- what is judgment and what is, if you will, fact. Well everything we do, of course, is judgment out here, but in this case -- I'm sure the other speakers will say this as well -- the judgment is tempered by field evidence presumably in the form of trench exposures and logging and mapping.

Next slide.

(Slide.)

Here then is a slide showing the GETR. Here is the hillfront. A number of trenches you can see extending up and across the hills, and here is Trench B-1. Here is the exposure of the shear called B-1, the B-3 trench would be to the right as you face the slide or -- correction, the screen here -- Trench B-1 comes through this area. And we have some detailed soil stratigraphy logged in this area and I'll show you that in just a moment.

Next slide, please.

agb59

(Slide.)

This is from that same locality, turned right around looking down Trench B-1. The GETR is off to the left as you face the screen. This trench goes to the crest of the little hill, and then on the other side it's designated Trench B-2.

Now these are wonderful, as you know, localities for Quaternary geologists, we never have enough data, we always need more trenches and if we have our way, of course, we would wipe out the entire Coast Ranges.

But we have a magnificent exposure here, at least if these trenches are still open at the GETR site.

With regard to the B-1 shear system, right at this locality where these various plastic bags and detritus are strewn about is the locality where a detailed soil stratigraphic section was described.

And in fact where these red flags which you'll see in just a moment is where samples were collected for possible radiocarbon dates from the modern soil, fraught with difficulty but nevertheless we took all kinds of techniques and applied them here.

Next,please.

(Slide.)

Illustrated diagramatically here and spelled out in great detail, including the physical and chemical

agb60

characteristics of each of these in the appendix, Appendix A of the Earth Sciences Phase II report are the typical soil horizons of the GETR area, particularly within Trenches B-1 and B-2.

I won't go into great detail on all of them but note, please, that there exists a distinct unconformity in this section. The modern solum, the modern soil has several distinct horizons that have been mapped and described in detail.

These are mainly the A horizon, usually very dark and called the mollic epipedon, dark in color; the AE horizon or the albic or eluvial horizon is a tricky one but it's a very useful one here because it's distinctive, it's light-colored and it can be traced and recognized in the field in a number of the trenches, particularly because it contrasts dramatically in color with respect to the overlying dark-colored mollic horizon.

There is also in the modern solum the cambic horizon, slightly oxidized, an incipient B horizon and in many places a BT or argellic horizon, it's moderately developed.

In this area right below, not shown on this diagram, is a typical widespread regional unconformity often represented not only by the base of the modern solum but by a stone line, a geomorphic marker as well.

Typically below that and truncating the underlying

agb61

unit, one finds a very distinct and obvious buried Paleosol. This is a very useful regional and widespread stratigraphic marker. The buried Paleosol can be identified mainly by its red color. It is one cell notation generally in the range and it can be subdivided again in the field based on a number of physical and chemical characteristics spelled out in some detail in the reports by its argellic horizon, argellatious clay accumulations of B-2-1, B-2-2, et cetera, the lower case b, of course, indicating buried and here, of course is the parent material.

cxxxx

In brief then with respect to some of the stratigraphic markers, they do exist at the GETR site, particularly in Trenches B-1 and B-2. They are namely the modern solum, secondly the buried Paleosol and often -- although not that continuous at least in some areas -- can be a distinct stone line, a geomorphic marker.

4.500

Next, please.

(Slide.)

Diagrammatic, here is a geological log, a simplified geological log also reproduced in the report, and this is of Trench B-1. This is the engineering log and what I've superimposed on it, indicated on the right side as you face this particular screen is a soil profile, the description again in the report.

The idea here is to identify the soils away from the particular shears indicated by red, and then move those

agb62

soil horizons laterally toward the shear to see which, if any, are displaced.

Also indicated on this slide are these large black dots here, and these represent the area at that particular shear where three bulk samples, roughly 2500 to 3000 grams of organic material, very low in organic content to be sure. These samples were collected for mean residence time dates radiocarbon, MRT dates, and shipped off to commercial laboratories.

What can be seen also in this slide -- you see the base of the modern solum, here's the buried Paleosol and here represented diagrammatically is a distinct geomorphic marker to help date the age of the last movement of these shears, in this case, the stone line indicated diagrammatically because it is a discontinuous unit. Those clasts are derived mainly from the adjacent Livermore gravels in the adjacent hillfront.

What shows up also, by the way, on this particular slide, you can see that the shear, the principal shear in the B-1 near the hillfront, no doubt about it, completely displaces the buried Paleosol. It displaces the buried B horizon, the argellic horizon. Further it displaces -- not much, but nevertheless it does, into the stone line and, in fact, can be traced up into the argellic horizon or the modern solum.

The question we now face is how old are these

agb63

particular units and, secondly, by what amount is the displacement.

I'll point out here also the second red line, which indicates not only the B-1 shear but, in the others, that in fact there are smaller units that can be seen, that shears, if you will, whether they connect or not, it's apparently this one that dies out, however elsewhere they appear to connect with the principal shear, so there's a multiplicity of smaller units here.

These numbers here refer to the laboratory numbers for the radiocarbon dates, the mean residence time dates, the MRT dates. In this case, they are -- Geochron is the commercial laboratory.

Next, please.

(Slide.)

This is at that same locality. These red arrows here indicate those sites where those bulk samples were taken for mean residence times. The yellow flags here indicate the shear.

And this, although it is perhaps a little messy slide at the moment and it had rained and so we lost some of the structure at the time that slide was taken, nevertheless this is the top of the argellic horizon, this is the B-2-1 of the buried Paleosol and it is cleanly displaced.

The dates we would obtain by mean residence time --

agb64

because unfortunately, as typical, one seldom finds nice large chunks of detrital charcoal to yield unequivocal -- and they are always equivocal -- dates.

Nevertheless, the second thing one could do is try to get a bulk sample. Note, however, roots coming all the way through, and we are in the modern soil, no question about it. And hence we would expect the dates, whatever they are, even though they are mean residence times, dates to be on the young side because of contamination.

I had anticipated -- well, I can't leave that. I expect a date out of that of about 2000 years, just based on the mean residence times of the modern soil because soils, as you perhaps well know, really are weathering profiles and they only -- they date, in essence, a surface of landscape stability and therefore provide minimum ages for the underlying sediments which they are forming.

Next, please.

(Slide.)

Here's a closeup of that same area. And again you can see by the yellow flags here with the shear projecting in this area. I point this slide out also for the following reason and it shows up in another one rather well, I think, namely, that there's a blocky structure with a strong contrast in color between the underlying buried Paleosol, the overlying mollic epipedon and the argellic horizon.

agb65

A few of the stones -- a weak stone line did show up laterally in the trench. But also the fact that many of these clays are probably smectites, montmorillonites and expansive clays and therefore it is often fortuitous -- namely, one has to work in the spring and through the summer in order to see some of the structures in here, and by wintertime with the first rains they would tend to expand and we tend to lose those things.

These flags here identify the base of the particular horizons. The BT here is the modern, the base of the modern argellic horizon.

endTape 2

Next, please.

1462 127

#3 ebl

(Slide.)

This slide again is in the report and it's a typical family of curves taken from the literature to show the relative amount of contamination that yields the dates, apparent dates versus the true dates.

For example, if we have a true date, we'll say, of 20,000 years and we have taken off, say, 20 percent we end up moving down the family of curves of modern younger carbon, we would end up with something on the order of about 8,000 years approximately.

c5

So we have two lines of evidence to date the upper, the modern soil; three lines in fact. One is associated with the stone line, when did that form on a regional basis, presumably related to regional climatic change. Secondly of course is the relative profile of the development. It does take time for soils to form and we can calibrate those in the Mediterranean climate based on soil profile development elsewhere in California.

And thirdly of course interpretation of the amount of contamination. I point this out because we're dealing with mean residence times and of course contamination.

Next, please.

(Slide.)

Referring back now to the general location, the slide you saw, we were right up here in trench B-1 in the hill

eb2

front shear. Next we'll go over here to trench B-2 to see which units are displaced, and you can see already there are Quaternary markers. The question is how old are they and what is the amount of displacement.

Next, please.

(Slide.)

Here it is. This photograph is reproduced in the report. Right where the geologist has his left hand here is a bench break and slope. There was no question there is a shear, a major shear.

This has been called the B-2 shear system slip service displacement and another slide coming up in a moment will show you the details of this particular area where it goes up toward the surface. But perhaps even at this scale you can see this light-colored unit. This is the AE horizon, the albic horizon.

There is also a stone line very well developed in this area right up at the base of the modern colluvium, at the base of the modern soil. It comes right around, neatly wraps around and can be traced right here and extends off in this particular direction.

At depth, the shear passes deep into the trench and this is displayed on the logs prepared by Earth Sciences. There is no question there is displacement. That is the buried Paleosol. It is the argellic horizon.

eb3

You will note also there isn't a buried A horizon. We seldom find preserved, at least in California, anything much older than Holocene age, buried organic horizons, but typically we find the argellic or the buried B horizon, and here it is.

It is cleanly displaced and so is the stone line.

Next slide, please.

(Slide.)

However, this shear when traced up in some detail -- and these little pink flags identify the details of that particular shear system at the B-2, and you can see there are a number of these.

This was-- Although it looks like it was a nice clear day, shortly thereafter it started to rain. This was taken about a year ago, and that was the end. These smaller shear systems could not be seen until the next year, next spring.

However, displacements were measured from their maximum point, worst case situations assuming that all displacement occurred in one event and with respect to the buried Paleosol, here's the stone line and the albic horizon coming right around. A point was taken from here to the nose, way out to this point. And in fact this is the B-2 trench indicated on the flag here, and Station 115, and this turned out to be on the order of one meter or slightly more than three feet.

eb4

And it was only in this particular trench, on this wall of the trench in fact that yielded this much displacement but nonetheless there is displacement.

Next, please.

(Slide.)

Here's a diagram of that same area you just saw. Again note here the black dots that indicate areas where bulk samples were taken for mean residence time carbon-14 dating from the modern solum in most cases because that's where the organic matter is.

The red lines again indicate the shear plane and indicated diagrammatically are some of those smaller ones.

Again indicated is the stone line neatly displaced and wrapped around, and you can see however that with respect to the modern solum over here, the cambic horizon, the AE, the alluvium B-1 are apparently continuous across.

Samples were taken above and below the apparent shears in order to see what kind of information we would get from that with respect to MRT dates.

Next, please.

(Slide.)

This table, again reproduced in the report, illustrates some of the typical difficulties one has if one accepts blindly numbers that come from a laboratory without some interpretation.

eb5

First of all, six samples were taken. These samples were split and hence we have 12 numbers because the samples were fractionated. We had hoped of course to get both folic and umic acid dates. There are essentially two commercial labs in the U. S. that provide relatively fast service, and this turns out to be Geochron and Teledyne.

Although not indicated on this particular diagram but pointed out in the report, all of these samples yielded-- By the way, from 3,000 grams we ended up with less than one gram of organic matters eventually recovered because these are bulk samples, we're in a Mediterranean climate, so it's not 30, 40, 50 grams of pure organic carbon.

Note, however, without going into great detail, here are the soil horizons indicated by their symbols. We were able to get some alkali solubles and insolubility of this technique would give us a little better dates. I had anticipated dates on the order of about 2,000 years in trench B-1; actually we were getting dates from the 4,000 year olds up to 4600 years old.

And what's intriguing about that, you'll notice these are inverted and they're reversed and essentially they're meaningless other than to tell us that the modern solum, the accumulation of organic matter, that has been going on for at least 4,000 years, including some of the units that aren't displaced. However, some units that are truly displaced also

eb6

yield the same age.

So what does this tell us? Simply that there's been weathering; the superposition has been going on for at least 4,000. If we convert, make some simple assumptions using the geomorphic relationship as well as soil profile development, then we take 4600 here. If you want to play the game you can double it because it's the mean residence time and make it 8,000, maybe a little more. And you can add another factor for contamination.

In general the MRT dates are not the most definitive things in the world to use and we seldom would use them, but in the absence of anything else radiometrically, they do support, if one interprets them, that the last displacement, the last displacement of the B-1, B-2 shears --

Can I have the next one, please?

And that's right in the system. Again here's B-1 and B-2 and here's the hill front and there's GETR. The last displacement was probably on the order of -- well, certainly post-20,000 years, stage 2 in the isotope stage, but it could be as young as 8,000 years. In other words, it's Early Holocene. Based on soil stratigraphy, regardless of the origin of the shears, whether it be mass wasting, whether it be tectonic, possibly Early Holocene, probably slightly older but nevertheless conceivably that young.

MR. PHILBRICK: Were your samples only taken in the

eb7

B-1 trench?

MR. SCHLEMON: B-1 and B-2.

MR. PHILBRICK: Did they take any samples in those satellite trenches that ring the end of B-1?

MR. SCHLEMON: Not for radiocarbon dates.

MR. PHILBRICK: Did you find the same offsets --

MR. SCHLEMON: Yes.

MR. PHILBRICK: -- in those rings?

MR. SCHLEMON: Yes, a little less in fact. And I point that up. It's coming up in the next three slides.

MR. PHILBRICK: Okay.

(Slide.)

MR. SCHLEMON: Another trench that was quite instructive with respect to its regional soil stratigraphic relationships was Trench E. Now that's way over here. There was some concern about projecting the lineament through it.

Next slide, please.

(Slide.)

Trenching is a very-- It's unfortunate this trench is covered because it's academically of interest as well as perhaps has some bearing on the particular problem.

We're looking across the Vallecitos Valley. The Calaveras Fault would be on the range of the hills over here. And expressed here by this red line is that regional buried Paleosol, again just the argellic, the B horizon.

eb8

Not well displayed here but it certainly can be seen and shows up on the detailed logging is the stone line at the base of the modern colluvium and alluvium. I say "modern." Obviously post-stone line, with the modern soil not only developing through that colluvial-alluvial unit but in fact now becoming superimposed on the underlying buried Paleosol.

Of particular interest there, and especially when you can date this buried Paleosol relatively, not by absolute dating but by association with the geomorphic and Quaternary climatic changes, is the fact you can see there's a very close correspondence on the surface of the ancient surface with the modern surface, with some diversion, as it were, as one goes downstream, and it looks like there has been sort of a migration of the axis of the little valley here farther downstream.

In a broad sense it appears then we're looking at regional climatic change and because this is miles away from the GETR site but can be traced all over in a number of trenchs, that gave rise to, if you will, epochs of landscape instability as sediment production separated regionally by times of landscape stability, if you will, or surface stability, slope stability, soil formation terminated again by the landscape instability, and more colluvial sediment production.

Now this applies obviously only to certain localities. There is always sediment production in the valley; there is always erosion up in the hills. But here then from a

eb9

geomorphic Quaternary standpoint we can see -- reconstruct the Quaternary history of the area and hence get an idea of the amount of displacement and if there are markers in the area.

Next, please.

(Slide.)

Here's that same Trench E. Smaller shears were found. You notice in contrast to B-1 and B-2, they do not have the same sense of displacement. They are indicated here by the little red flags. This photograph is also in the reports.

A Munsell color chart here for notation. The flags you see on this side represent depth markers in feet, and here are the horizon markers here.

Next slide, please.

(Slide.)

Notice the shears, and here you see it indicated diagrammatically. Here then indicated diagrammatically is the buried soil, the argellic horizon, a number of crotovenas or old burl fills here.

Here is the weak stone line. Clearly this shear does not go into the B-3 horizon of the buried Paleosol. Regardless of the origin of the thing it is old, and I'll give you the evidence for its age.

But in brief, that buried Paleosol on a regional basis probably relates to stage 5 in the oxygen isotope chronology and therefore is in the range, not absolute age but

eb10

range of about 70,000 to 125,000 years B.P.

Again we have the modern solum superimposed on the buried Paleosol.

Next, please.

(Slide.)

Here's a closeup of that same one. Again the flags indicate the horizon. The horizon markers are at the base. I point this one out for the following reasons.

There is the contact, the erosional unconformity right here at the base. Note the roots. Here is modern pedogenesis superimposed on the older Paleosol. I point this out because along the ped faces, along the strong blockey structure, columnar prismatic in some cases with a lot of clay films or cutans along the faces, it is possible to find little flecks of charcoal. And it's almost futile to get a date on those because there's a very strong probability or, if you will judgment, that a radiocarbon date from that would be a 2,000 year old date from a 100,000 year old buried Paleosol, simply the modern organic material coming all the way through.

Unless one can find detrital charcoal then sometimes radiocarbon dates lead to more problems than really one needs.

Next, please.

(Slide.)

A closeup of the same thing showing some of these roots coming through. Here again you can see a couple of clasts

eb11

here that represent the buried stone line. It's discontinuous but again it's on a regional basis. The clasts are derived mainly from the Livermore gravels.

Again the markers indicate in this case in feet, and here is the argellic as a distinct marker.

Next, please.

(Slide.)

Okay. We then wanted to look at some of these side trenches for trench B-2 to come up with the amount of displacement and to anticipate, referring I guess to Dr. Philbrick's question here.

So we measured displacements as seen in these trenches. Here are the data points now. We just had a couple of markers here and here, so that's called the B-1/B-3 system. Here's the B-2 system, and a number of side trenches were put in.

Where you see the red line indicates that the soil stratigraphic markers were truly displaced.

A few odd features here suggesting that this thing just sort of curves right back around and could not be traced in any of these trenches over here. There are 36 of them in fact. Most of the examination, however, concentrated where there was clearly displacement.

Here for some reason Trench 12 -- and there's a photo coming up next -- was not displaced. We found a few minor shears and the attitudes were slightly different. These

eb12

were essentially all the low angle reverses that you saw in the previous slides, and of course they were not encountered here.

Next, please.

(Slide.)

MR. PHILBRICK: What's the difference in elevation between the B-2 trench at the red line or at the top elevation of that point with respect to 12 and 20? Are you on contour?

MR. SCHLEMON: Yes, that's the rationale. We're following the contour as well as the photo. That's correct, it's awfully close.

MR. JACKSON: I would like to comment on that, Mr. Philbrick.

With a low-angle flyover with Dr. Schlemon and myself and Bob Morris at the site it was absolutely clear that those trenches are not on projection of the spring line and they differ from the contour to some extent, so those trenches are probably all well to the northeast of where the most likely chances of encountering a low-angle thrust would be.

MR. PHILBRICK: I'm asking specifically with respect to elevation and the question I want to know is:

Do these trenches follow the contour around so that you should find this thing in the bottom of those trenches?

MR. SCHLEMON: That was the intent of putting them in, to find them. We see them here, we do not see them here.

MR. PHILBRICK: In other words you don't find them

eb13

in the bottom of trenches?

MR. SCHLEMON: Not in here, that's correct.

MR. PHILBRICK: All right.

Is that set of trenches from 25 to 12 then along the side of a gully?

MR. SCHLEMON: Not a gully. It's a low slope.

MR. PHILBRICK: But it's a lower point of elevation --

MR. SCHLEMON: Yes.

MR. PHILBRICK: -- than the trench which is marked in yellow?

MR. SCHLEMON: The B-2 trench here?

MR. PHILBRICK: Right.

MR. SCHLEMON: Slightly lower.

How many feet do you think?

MR. YADON: A few feet.

MR. PHILBRICK: Would you want to hazard a guess as to the age of that gully with respect to the surface of the B-2 trench?

MR. SCHLEMON: This surface here? It's Holocene.

MR. PHILBRICK: I don't mean that. I mean relative age.

MR. SCHLEMON: Relative age of the gully with respect to the surface here? Essentially the same.

MR. PHILBRICK: The same age.

MR. SCHLEMON: Approximately. There could be a thin

eb14

veneer of modern slope wash and colluvium on it. It's a little lower in slope.

MR. PHILBRICK: But you would expect that thing to be in those trenches.

MR. SCHLEMON: That's correct.

MR. PHILBRICK: Then why isn't it?

MR. SCHLEMON: <u>Je ne sais pas</u>.

(Laughter.)

Here it is. It's field evidence. That's all I can report, and what's there and what's displaced and what isn't, unless somebody else who examined this in detail has an additional bit of information. It was not traced in 12 here and could not be seen right around the side trenches.

MR. PHILBRICK: Then can we assume that this is related to the fact that it's on a nose and not in a hollow?

MR. SCHLEMON: The hollow is a few feet lower apparently; that's correct.

MR. PHILBRICK: And how deep are your trenches?

MR. SCHLEMON: The trench is -- what? -- 20 feet here perhaps.

MR. YADON: The deepest part of that was about 40 feet.

MR. SCHLEMON: These were 5 to 20 feet, and 40 feet in the main trench according to the geologist who logged them, but we do have the same markers exposed here. In fact the

eb15

next slide will show you one, a typical side trench.

MR. PHILBRICK: Okay.

(Slide.)

MR. SCHLEMON: Here's a side trench, looking for that same set of shears that you saw on B-2. And what shows up here, by the way, there's simply no displacement. Here you're looking down at it. Here's the buried Paleosol, the argellic horizon again in that 70 to 125 thousand year old range. Here is a weak stone line at the base. It's also very distinct of course by this albic horizon or AE horizon.

This AE horizon in this particular case is perched on the impermeable unit, namely the clay of the buried soil. However, up in this direction it is not down that far. It represents a depth of wetting and hence a lateral movement of water, an eluvial or bleached zone.

This then is the latest colluvial unit and with the modern soil superimposed.

A typical example of the side trenches, the typical depth, 5 to 6 feet, looking for the shears that would cut the same markers. In this case there was no shear. The markers are there; the stratigraphy is there, but no shear.

Next, please.

(Slide.)

This slide summarizes some of those side trench data and here then they're lumped into two general groups.

eb16

First of all the B-2 shears, the same trench you just saw on the last slide.

Secondly, with fewer data points of course, namely the B-1/B-3 shears.

The red dots here indicate the amount of displacement here expressed in feet. This, by the way, is spelled out in much more detail in the report. It's been simplified here.

The red dots indicate the latest geomorphic marker of soil data for the maximum amount of displacement measured in the trenches. Thus, for example, if we take that stone line -- and I'll spell this out in some detail in just a few minutes -- as being essentially equivalent to the last major, if you will, fluvial and/or climatic vegetation geomorphic change in the region, put it approximately, being reasonably conservative, in the order of, say, isotope stage 2, make it somewhat time-transgressive but not over 10 or 20 thousand years, certainly in the order of several thousand years at the very most, then we end up with something, with displacement in the last, say, 15, 14, 10, 12 thousand years, maximum displacement assuming it's one event -- it could be multiple events -- and that point right there was just about one meter, and that's the one you saw in the Trench B-2.

All the other measurements of the 12 or 15, all the others were in the order of about one and a half feet, two, about two feet.

eb17

We get comparable amounts of displacement on the two localities, the B-1 and the B-3, and shears near the hill front.

If we go down to the buried Paleosol of course there's less resolution because we don't have a distinctive marker, that is geomorphic marker, but we are measuring our pieces of argellic horizons that appear to be displaced. That's indicated on this slide by the blue dots.

The lines indicate ranges and in the worst-case situation, and this is piling conservatism on top of conservatism, with respect to the B-2 shears, the maximum if we add these together, we end up with about 11 feet or so.

The same thing with the B-1/B-3 system, a little less. We have fewer data points here. This then is a displacement of the buried Paleosol at a maximum, say, of 11 feet or so. Most of the other measurements where we could see it are less than that.

Next, please.

(Slide.)

Finally let me take you into Trench H. This was not investigated in great detail; it was a reconnaissance. But it's a very instructive trench because it does show a whole multiplicity of buried profiles, buried soils that apparently relate to the late Quaternary history of the region and shed some light on the age of the Livermore gravel and hence on the

eb18

age of the sediments that are underlying the -- that underlie the GETR site.

Next, please.

(Slide.)

Here they are, just a few slides. Trench H was not on the main trace of those shears that you've seen, indicated here as the same malach epipedon, the dark colored horizon.

These flags, one, two and a whole series of these things, and to make the long story short, down here at the base of the trench 15 feet below, this trench to my knowledge is probably unique in the Coast Ranges of California because it exposes four strongly developed superimposed buried soils, each truncated, terminated by a stone line, an overlying packet of colluvium, in other words landscape instability, time of soil formation, very strong developed profiles. The whole sequence is repeated at least four times.

Next, please.

(Slide.)

An example, not only collected samples of course but actually to test the age of this, an independent method to see how old. I would have speculated that we were probably in the order of 350 to 400 thousand years by association with isotope stage numbers and Paleomagnetic samples were collected, samples run by the University of California at Davis.

They all yielded -- in fact, 13 samples, normal

eb19

polarity, essentially Brunhes. At least we know that line of evidence also suggests post-700,000 years. That is Brunhes Matuyama boundary.

Here's the top of the buried soil. It hasn't been cleaned off in the trench. And there's a very weak stone line that can be traced laterally.

Next, please.

(Slide.)

A typical example, a closeup of some of the argellic horizon with modern organic material coming right down the ped faces, again just to show you the very strong development, based on relative profile development with other soils in comparable sediments in similar climatic regimes within California. We know we are dealing with a very strong profile, relatively speaking, and certainly those that likely formed in general interglacial intervals.

Next, please.

(Slide.)

And finally indicated diagrammatically here are those four soils. Here is the modern solum, the BT indicated by the dark color. Here are the argellic horizons of these four multiple buried soils.

Indicated diagrammatically also is the basic stone line, the basal pocket of alluvium and colluvium, and there are four of these things here indicated as you see here by

eb20

simply one, two, three and four, and by the argellic horizon.

I might point out, however, in Trench H clearly all four of those buried soils are displaced. That displacement, however, which is described in the Earth Sciences report, the same units, however, are all displaced in the same thing. The uppermost stone line similar to the B-1, B-2 is also displaced and roughly by the same amount as indicated before.

From that, just as an interpretation on a regional basis, it would appear that the amount of displacement, B-1, B-2, probably even in Trench H here, probably is the same amount.

Next, please.

(Slide.)

We referred a lot to ages and where do they come from. Now in the absence of multiple widespread volcanic ash to get potassium argon dates, the Quaternary geologist typically has to resort to something a little indirect. But the most or the best, I should say, the best stratigraphy chronology framework to fit all this in, plus tying it into other radiometric dates in the region -- it's a strange place to go but nevertheless it's the isotope chronology, and this is taken, simplified, from Shackelton and Updyke in 1973 in a Quaternary research paper.

The work, as you undoubtedly are acquainted, stems from Ameliorani and others over 15 or 20 years ago, but this paper by Shackelton and Updyke is a nice synthesis. This

eb21

diagram is reproduced in Appendix A of the ESA Phase 2 report.

What I point out here are the stage numbers. These are the oxygen isotope, O-18, O-16 stage numbers. An indicated interpretation here is relative sea level. Now what we're interested in here are relative high stands and relative low stands, and these are presumably glacial eustatic, if you will, probably in mid-latitudes although somewhat out of phase, probably equated to -- quote -- "fluvial" -- unquote -- phases of landscape stability and instability.

Note Stage 17 or -- correction -- 18 over here. That's 700,000 years. That's the Brunhes MatuyamaPaleomagnetic boundary.

Of interest here are Stages 1, which is the Holocene, 3, if you will, using mid-Western terms, mid-Wisconsin, Stage 5, which is essentially late Sangamon, and 7, 9, et cetera. Note the odd numbers refer to relative high stands, relative interglacials, the low stands, relative low stands and hence glacials.

Some of the dates we have based of course not only from this area but from all over the world, the last major low stand in the order of about 17 to 20 thousand, referring to mid-latitudes, essentially the Late Wisconsin.

Stage 5, and there's a blowup on the next slide, can be subdivided readily into Stage 5-A and 5-E in the sub-stages. That's roughly indicated here 80,000 but to be very

eb22

conservative we've moved it over to the boundary and made it roughly 70,000.

So this is a critical one, the Late Sangamon or the last major interglacial from roughly 70,000 to 125,000 years before present appears to be the last time available, length of time as well as presumably climatic change and influences that are likely to give rise to times of landscape stability and soil formation for that uppermost, strongly developed Paleosol which is displaced.

Next, please.

(Slide.)

Here's a blowup of that same thing again taken from the Shackelton and Updyke curves. Note 5-A, 5-E. The "NG" here refers to New Guinea and the Barbados in the calibration.

There's roughly 80,000; there's 125,000. And that's present sea level. And 5-E is of interest because it's apparently the only time for ten's of thousands' -- hundred's of thousand's of years that glacial eustatic sea levels were truly higher than the present, in the order of six to ten meters.

Note, however, that here is Stage 3, the Mid-Wisconsin, if you will, an interstadial and also a time of soil formation. But generally throughout California under the same climate, Mediterranian -- interior Mediterranian climate and also related to geomorphic surfaces, the soils that have formed

eb23

on the Stage 4 sediments are only moderately developed at best. It's at Stage 5, 5-A through 5-E, that gives rise to the strong developed soils.

Here is Stage 2. We make that stone line, that production, the last major epoch of production of colluvium and alluvium in the 20,000 -- 17,000 to 20,000 year old range. The stone line, the overlying sediments, the soils then, the modern colum has to be post that. It has development. It does have an argellic horizon.

So to be very conservative for making the youngest colluvial epoch in the order of, say, 15 -- even younger than that, 15 to essentially about 6 or-- Well, let me go back and say 10 to 11 thousand years, because there had to be a time of landscape stability to allow that soil to form.

In other words, there's very little, if any, movement deposition going on at the GETR site at this moment at B-1, B-2 because those soils aren't forming. There's a little bit of colluviation, cumilic profiles near the mountain front, the hill front. So then again we have 2, 4, and of course Stage 5 and that's important to come up with the age of the sediments underlying the GETR site.

Next, please.

(Slide.)

Now with that question in mind, there's the GETR site again, a secondary question came up, or another question:

eb24

How old are those sediments under the GETR site?

Next, please.

(Slide.)

And that's the trench B-1 where we can see it and indicated diagrammatically. This diagram appears in the Appendix A of the Edak report.

There is the GETR indicated diagrammatically and upon an interpretation of the engineering log plus a field inspection, the following came out:

First of all, here's the B-2 shear that you saw displaced, the very Paleosol, the 70 to 125 thousand.

Here's the B-1/B-3 system at the hill front, also displaced. No question.

However, in the middle -- and we can trace this Paleosol as a marker -- we begin to lose it, its distinct identity, its blocky structure.

It turns out, however, there are little younger channel fills in here, including one almost directly opposite the GETR, and they in turn are capped by a very weak buried Paleosol, and here's the regional stone line, the last one we're making is younger, say, 15, 17, even 20 thousand B.P. and it goes all the way across.

I have indicated it diagrammatically here; it's not that continuous. This diagram is instructive for the following:

It would appear, using the oxygen isotope numbers

eb25

and stage numbers as a chronology to work with to determine the amount of displacement that we have the entire sequence here to at least perhaps Stage 6.

Just going through it briefly, here is Stage 1, essentially the modern solum. That's Holocene.

Here is essentially Stage 2, slightly younger, the basal :one line, the production of colluvium on which the modern soil is forming.

Let me skip then to Stage 4, presumably in the order of say 60,000 PB approximately where we had younger channels that were cut.

And Stage 3, Mid-Wisconsin, using Midwestern terminology, soils, Paleosols, and they in turn were truncated.

These then are-- Underlying that is the older, if you will, Illinoisian, using Midwestern terminology, Stage 6, basla alluvium on which develops Stage 5 interglacial soil.

With that in mind, at the GETR site expressed in Trench B-1 it would appear -- and there's GETR -- at least at that particular area that there has been no displacement right at the GETR site, certainly into Stage 5 time which is 125,000 years at the old side, and if we take this as being Stage 6, then it's conceivably up to roughly Stage 6 or 7 boundary which is on the order of 195,000 years.

So to be very conservative you make the youngest part of Stage 6 on the order of 125, 128 thousand years BP and

eb26

there's no displacement there. However, there is displacement of the buried Paleosol at B-1 and B-3 --

MR. PHILBRICK: How come there is no displacement under GETR if you have displacement down here from that?

MR. SCHLEMON: It may be only in the depth of this trench. That's all you see. Speculation doesn't go beyond that.

MR. PHILBRICK: The whole mass between the upper break and the lower break is moved.

MR. SCHLEMON: Correct.

MR. PHILBRICK: Okay.

MR. SCHLEMON: Not necessarily as one unit --

MR. PHILBRICK: So there has been motion under the GETR.

MR. SCHLEMON: If this is traced underneath here, that's correct.

MR. PHILBRICK: Now you haven't found out whether the upper failure plane is visible to the north or the south in the adjacent gullies.

MR. SCHLEMON: Which upper failure plane? This one?

MR. PHILBRICK: No, the right-hand one.

MR. SCHLEMON: That's in B-3. And again, those who did the regional mapping can point that out to you

MR. PHILBRICK: I mean actual excavations.

MR. SCHLEMON: That's in the B-3 trench. The people

eb27

who logged it can tell you about that.

PROF. KERR: Do you understand the question?

MR. HARDING: Yes, I think so.

For the most part, although our trenches were put on the noses or the ridges because we figured that would be the place where we could get into the Livermore gravels easiest without being masked by alluvial fine materials coming out of the gullies, there is one exception to that and that is the canyon excavation north of Trench T-2 in which we actually went up into the canyon and scraped off the walls.

In that particular case we did see what we are calling the B-1 shear going across that gully uphill like you would expect, dipping in that direction from the T-1 trench.

MR. PHILBRICK: So the B-1 then runs underneath the main mass of the hill.

MR. HARDING: Correct.

MR. PHILBRICK: B-2 does?

MR. HARDING: Well, --

MR. PHILBRICK: Because you couldn't find it going north, you couldn't find it going south.

MR. HARDING: That's correct.

MR. PHILBRICK: So it's to a limited extent, but the B-1 is one which may run along the hill for some distance.

MR. HARDING: That's correct.

MR. PHILBRICK: Well, then, do you want to make a

eb28

comment on the relative age of the B-1 versus the B-2?

MR. HARDING: Well, I think what we're seeing here in terms of our offsets or our profiles are that the relative ages are somewhat similar throughout at least the Quaternary history here.

MR. PHILBRICK: I would say they weren't because if they were, then you ought to get the B-2 running parallel to the B-1 all the way through. But when it doesn't show it means it's limited only to the nose that stuck out from the hill.

MR. HARDING: So you're saying then the B-2 is older?

MR. PHILBRICK: I'm saying B-1 is the original one. B-2 didn't form until after the topography was developed. It produces the nose that produces the load.

Your trouble with this whole damn thing on the landslide business is that you're dealing with a dissected mass of material in which the major part of the stress-producing forces have been removed.

MR. HARDING: That's correct.

MR. PHILBRICK: Okay. Now the result of that is that you see that the thing is -- that the ground is essentially stable in the hollows where the load has been taken off, and B-2 only developed in the nose where the high head still remains on the soil mass.

MR. HARDING: All right.

eb29

MR. PHILBRICK: So B-2 is a landslide shear for sure and B-1 is probably one.

MR. HARDING: Okay. Can we put this off until we get to the next-- I have another table which sort of goes into the ages of these various shears, and maybe that may bear on this question.

MR. SCHLEMON: I have about two slides to summarize the whole thing here.

Next one, please.

(Slide.)

Here is one of them. First of all then, essentially the information in tabular form that was given on the various dots in the various diagrams, that roughly between -- and there has been displacement perhaps up to as young as 8,000 years and most of it is probably older and within the last 8,000 years approximately, based on the three lines of evidence I indicate there has been no, at least that we can measure in the B-1/B-2 system, displacement.

However, the stone line and the overlying colluvium is displaced. Here is the maximum amount, and I expressed here in feet now three feet at one and about perhaps two feet at the other.

With respect to-- Getting down here to the bottom one here, to the Paleosol -- correction -- down here, 70,000 to 125,000 year old, very Paleosol, the uppermost soil and

eb30

stage 5. Here is maximum displacements that are measured indicated her e in feet.

And finally if we go on beyond Stage 6, conceivably based on interpretations by Earth Sciences of their information in the logs, there conceivably has been movement in the order of at least 80 feet or more than 80 feet, and with respect to B-1/B-3, greater than 40 feet.

Can I have the next one, please?

(Slide.)

Here then with respect to the Quaternary stratigraphy of the region and dating mainly from the four trenches, B-1, B-2, Trench E and Trench H.

First of all, the basic question: Are there any Quaternary markers to use to date the last displacement of the shears? Yes. What are they? Widespread stone lines on a regional basis, not only the major one in B-1/B-2 but also showing up in Trench E and H.

Secondly, there's at least one distinctive buried Paleosol in the order of 70 to 125 thousand years.

Secondly, with respect to the age of the markers, the next basic question was asked: How old are they?

Again we made the stone line and the colluvium be very conservative, roughly Stage 2 and shortly -- and younger a little bit, and that would be less than, say, 20,000 years, slightly younger.

eb31

The strontly developed Paleosol is 70 to 125, but there are also, as indicated by Item C here, multiple buried Paleosols included in Trench H, and if we plug those in to the oxygen isotope curve as a first approximation and make those, for example, Stages 5, 7, 9 and 11 respectively, at least they are all younger than 700,000.

On that basis that conceivably put the age of those buried Paleosols back into the 400,000 year range. They are all displaced but it also means therefore that the Livermore gravels underlying have to be older than the order of, say, 400,000 years.

And another point to note with respect to the -- right at the GETR site, based on Trench B-1, there has been no displacement conceivably of Stage 6, in fact conceivably more likely it's Stage 6 age, but we'll make it very young and say no displacement for at least 125,000 years.

With respect to the third one, the third major question, displacement of markers, one of the prime things I think that came out of that soil stratigraphic investigation is that there has been repeated or multiple movements on the same shear planes. And here it is: There have been multiple movements on the same slip surfaces, particularly the B-1, the B-2, and that shows up by having increasing displacement on the older marker, namely the buried Paleosol, lesser displacement on the younger ones.

eb32

Here they are. Maximum of about three feet, early Holocene time, and a maximum of 12 feet on the buried Paleosol.

And finally-- This one I think is the last to summarize the whole thing because that would go to the next speaker.

With respect to the Quaternary history of the region, we always need more data. We'd love to have more trenches but somewhere a judgment has to be made. With respect to the B-1, B-2 trenches and H, in particular, at GETR, that multiplicity of buried Paleosols in the GETR trenches probably exposes the -- well, what is now the best known late Quaternary stratigraphy in the Coast Ranges of California. I'd love to have a few more trenches but we have a tremendous amount of information at the moment.

Thank you.

PROF. KERR: Are there questions or comments?

(No response.)

I believe we agreed that this would be a good time to break the presentation, and I'm going to call on Mrs. Hubbard if she will now to make the presentation she requested.

Mrs. Hubbard, would you mind coming to a place where you can use a microphone, please? You may sit or stand as you like.

MRS. HUBBARD: In the midst of all this expertise I feel a wee bit out of my depth, in fact a whole lot out of

eb33

my depth.

My name is Helen Hubbard and I live with my family at 3401 Little Valley Road, Sunno, California, and we've lived there for 14 years.

I'm also a member of a grassroots energy advocacy organization called Citizens for Total Energy based in Alameda and Santa Clara Counties.

I really don't know why I'm here but I guess I have two reasons. One, probably more than anyone else in this room, I have the best information of how it is to live next to an operating reactor. The Vallecitos west boundary is my Little Valley Road.

And two, because I guess nobody is representing me or my neighbors or my community. We aren't part of the people that the Friends of the Earth claim to represent. We come from all walks of life and we do not represent the company either.

From my back door as the crow flies, I can walk to the control room of the GETR in 15 minutes. One Little Valley Road there are 11 families, 32 adults and five children under 12 years of age. Each of them can make the same walk in approximately the same time. Five of the families had purchased property and built homes while the reactor was operating and no one is planning to move even if GE is allowed to operate the site again.

On November 19th, 1977, we sent a petition bearing

eb34

more than 500 names to Mr. Case, then Acting Director of NRC, which stated:

"We, the undersigned, residents of Sunno, Pleasanton and Livermore communities, support the General Electric Vallecitos Nuclear Center. We do not believe that the research being carried on there in any way contaminates our environment. We are not unduly concerned with earthquake speculation or obviously we would be the protestors.

"If and when hearings are held for relicensing the site, we ask that they be held in Sunno so that the people most closely affected may easily attend."

I guess that request wasn't granted, and it was probably terribly naive. However, if there are other hearings possibly they could be held at least in our valley so that some of us could be there.

It is not difficult to be frightened. It is difficult to be logical and reasonable when you are being barraged by the horror of a killer you cannot see, smell or taste.

Over the past two years we have listened to the enumeration of every possible disaster that could occur, and we still support Vallecitos because we are logical and reasonable people.

We're beginning to wonder, and we wonder a lot, if

eb35

Vallecitos and the controversy surrounding it is only a part of the total attack on every nuclear installation in this country. In California that includes the Lawrence Livermore Laboratory, Sandia, Rancho Seco, San Onofre, and the yet unlicensed Diablo Canyon facility, plus any others that haven't so far surfaced.

If these shutdowns were to happen, it would affect every facet of our lives from nuclear medicine to national defense and to the electricity that flow into our homes. California is a very shakey state. That's earthquake-wise, and if we were to be completely safe from the havoc of a large earthquake, we should move the people out of the cities, drain the dams, stop all storage and transportation of volatile gases and toxic chemicals, and we could go on and on and on.

We care very much about our environment and we care very much about our children and their children. However, nothing in life is without risk and those of us who live in close proximity to the Vallecitos Nuclear Center are willing to accept what we consider to be the small risk the facility represents.

In light of the studies and modifications that General Electric has done to insure the public safety, we strongly urge you to recommend that the GETR be relicensed and restarted as soon as possible.

Thank you.

PROF. KERR: Thank you, Mrs. Hubbard.

eb36

MRS. HUBBARD: You're very welcome.

PROF. KERR: I think we do perhaps owe you an explanation for the location of the meeting. We do try to hold meetings near where the people are who live near the reactor and who are concerned. The logistics of arranging the meeting are difficult and we were unable to get that close for this meeting.

MRS. HUBBARD: If you're worried about logistics we'd be glad to provide the housing and the transportation.

PROF. KERR: I was just going to request the use of your-- You don't have a basement probably.

MRS. HUBBARD: Oh, yes, I do.

(Laughter.)

PROF. KERR: I would simply say further that although we may do it imperfectly I believe it was the intent of Congress that both the Nuclear Regulatory Commission and this Committee represent the people of the country.

MRS. HUBBARD: I know that. I just feel a little lonesome out there.

PROF. KERR: We next hear from Mr. Baldwin.

MR. BALDWIN: Good afternoon. My name is Andrew Baldwin. I represent Congressman Dellems of Oakland, Alameda County Planning Commissioner Robert Shockly, and Friends of the Earth.

I have four brief comments for the ACRS.

eb37

The first is that we urge the ACRS and the NRC, should they decide or start to consider the relicensing of the GETR, to hold the reactor to all the standards now required of nuclear power plants, including Part 100, Appendix A of 10 CFR, the general design criteria of Part 50, and all Regulatory Guides as applicable.

Secondly, General Electric's consultants this morning have so far pretty much skipped over Trench H. They mentioned that it was there, and we would like to hear some more discussion of Trench H because there is a very dramatic fault-type offset in Trench H as well. It is very close to the Plutonium Labs at Vallecitos, within a couple of hundred yards, and some discussion ought to come up some time about whether those labs can remain open.

The most important point I have to make is a very unfortunate point, and it shouldn't -- this type of thing should not come up in the United States in a democratic system, but unfortunately it has, and it must be brought out, and this concerns the credibility of the General Electric Company.

If I refer to the ACRS transcript of February 10th, 1978, in there the story of how the GETR was closed down was told to the ACRS, and on page 295 of that transcript the NRC Staff told the ACRS that following the submission of the General Electric relicensing application for the GETR, General Electric was told that their seismic investigation was inadequate and

eb38

that they would have to redo it.

The trigger of that comment was apparently the reference in the 1977 license application to a report from 1973 by John Blume and Associates. The report is entitled "Seismic and Geologic Investigations for the General Electric Test Reactor Facility," dated July '73. This report was prepared for the General Electric Company, and I am going to give a copy to you now, Mr. Chairman.

The report was withheld by General Electric for four years. It did not reach the NRC until 1977, and the NRC Staff is currently investigating the withholding of this report and is apparently mulling over the possibility that the whole matter should be referred to the Justice Department.

That report, the 1973 Blume report, maps the Verona Fault in the location of Hall, in other words 2,000 feet away, but it contains an extensive discussion of the possibility that the fault is only 200 feet away, and the NRC didn't see that for four years, and when it did, they ordered GE to do some more investigations.

Another incident arose in the course of this case which we believe, again very unfortunately, reflects on the credibility of the General Electric Company. It was revealed in the spring of 1978 that General Electric had been reporting to the State of California that there was negligible radiation contamination of groundwater near the GETR. This was based on

eb39

something called the General Electric Cross-Monitoring Program.

An employee of the California Regional Water Quality Control Board became somewhat suspicious of the adequacy of the program and investigated and he determined that all monitoring points for groundwater near Vallecitos, in other words, all the data that General Electric was giving to the Regional Water Board, were from water sources upstream of where they dumped their water, and not surprisingly, they showed negligible water contamination.

When measurements were made, apparently for the first time, downstream, they found extensive contamination, at some locations in excess of EPA-acceptable standards for municipal water supplies. And I have a copy of that report prepared by that staff member for the Regional Water Quality Control Board, and I'll give you a copy of that.

We have learned, in other words, that General Electric was told in '73 that the fault was theirs or was likely to be theirs, and withheld the information from NRC. We learned that they report groundwater measurements upstream from where the contaminants are dumped.

And the lesson of these incidents is that we should be very careful about believing anything else that they have to say.

The interventions are-- There are actually five parties in the Atomic Safety and Licensing Board case.

eb40

Congressman Ronald Dellems is one. Alameda County Planning Commissioner Shockly is another. Friends of the Earth is a third. The interventions-- Two other members of Congress have intervened in the case, Philip Burton and John Burton. And the position of all five Intervenors with respect to the General Electric Test Reactor is as follows:

The earthquake hazards at the Vallecitos site are well documented by the U. S. Geological Survey and the Nuclear Regulatory Commission, and the consultants hired by General Electric. Sufficient data exists to warrant a permanent shut-down because of the threat of earthquake damage leading to harm to the public health and safety.

Thank you.

PROF. KERR: Thank you, Mr. Baldwin.

Mr. Okrent.

DR. OKRENT: I wonder whether either you or the groups or individuals that you are representing have some quantification of what level of risk they would consider to be acceptable or what level of risk unacceptable from this specific facility because what I've heard from you, and I must say also in general from others, is just a qualitative comment, and I guess we all know there isn't anything such as zero risk.

So I generally press everybody as now I will try to press you, if I may, can you help me quantify what you would consider to be either acceptable or unacceptable? And you can

eb41

put it in any framework that you like, if you are so willing. There is no obligation, of course, though it would be helpful.

MR. BALDWIN: In the context of this particular case we have a reactor of substantial size. It's a significant fraction of the size of some nuclear power plants. It's within 50 miles of 4.5 million people and they are very few large reactors in the country that are sited as close to major metropolitan centers.

It is more than 20 years old and the engineering that went into the reactor is primitive. The containment systems are primitive. The control systems are primitive. The safety systems generally are primitive. And it's within 200 feet of an active earthquake fault.

PROF. KERR: Did you understand Mr. Okrent's question?

MR. BALDWIN: Yes.

And that is an indication of the level of work. All of those things each builds on the other.

PROF. KERR: No, I think he was asking what level of risk you would be willing to accept, not what level of risk you felt existed.

Am I mistaken?

DR. OKRENT: That's correct. In other words if I can state it specifically, presumably the people living closest to the facility are likely to be at highest risk. Would you

eb42

consider a risk to them of, for example, a lethal dose of radiation one in a million per year to be acceptable or unacceptable, one in 10,000 per year, one in a billion per year? Can you quantify it in that sense?

MR. BALDWIN: Well, obviously not. No one could.

DR. OKRENT: Excuse me. People do.

MR. BALDWIN: You'd have to put a value, for example, what is the value if you wanted to use dollars, what is the value of a future of a child born in the future with a defective heart structure or a defective bone structure or stillborn. It is not an acceptable technique, to try and put a dollar value on birth defects occurring in the future, or mutations occurring to people 100 or 1,000 years from now. It simply can't be done.

And the level of risk acceptable depends on the evaluation of those kinds of things.

PROF. KERR: I think the answer is that Mr. Baldwin feels that he cannot. Don't you?

DR. OKRENT: Well, if I may just continue it for a minute, it's not an unimportant subject I think.

Certainly in this same part of California there are other technological facilities that impose the risk of accidental death to people living within their facility and a decision is being made by the various responsible authorities, whether they are state, federal or local, that these facilities can or cannot run, and they are therefore making a decision,

eb43

implicitly or explicitly, that some risk is acceptable for the people living in the vicinity of these facilities.

So I don't know that they are doing it in terms of some dollar value, but they're doing it. So I'm trying to ascertain, since there are responsible individuals, members of Congress here in particular, who have a concern that this facility may be imposing undue risk, whether they can quantify what they consider to be undue risk so I, for example, might get guidance in that regard and I can compare it to other things in their own district to see whether this is something that they would want to be applied to all technological affairs in their district, and so forth.

MR. BALDWIN: I could propose -- perhaps propose an answer following a famous rule from tort law which was developed in this century in tort law but many centuries ago in mathematics. Every gambler knows it.

You multiply the risk of an accident times its total loss if it occurs to get an expected loss, and compare that to the benefit of the facility. In the case of the GE Test Reactor the benefits have proved to be minimal. It was a major producer of medical isotopes up until the time it was shut down. The lesson of the last two years when it has been shut down is that it has not been a critically important facility for that purpose.

The other thing they did in the reactor was, as far

eb44

as we can tell, was research into the development of advanced reactor designs. This research-- There is no evidence we know of that this research has come to a halt, assuming it has any value, and therefore, if you really want to use the old tort law rule, there's a great benefit of this facility to the General Electric Company, they make millions of dollars, or did, every year, on that operation of the reactor.

But as far as the United States is concerned or the world is concerned, or most importantly in our view, unborn generations of Americans is concerned, the facility has minimal benefits and therefore if you want to quantify it, the quantifications on the benefit side is going to be close to zero, and therefore the risk level is going to outweigh it.

DR. OKRENT: Well, that's an interesting point of view. It's not quite the question I posed, but if that's the way you wish to express the answer I will accept it.

PROF. KERR: Thank you, Mr. Baldwin.

Mr. Mark.

DR. MARK: A similar question. I don't expect you would be able to be in a position to answer it, yet it would be a relevant one.

The risk has been described as being unacceptable, period, because there are earthquakes, because the reactor, which is probably one of the less threatening reactors in the country from the point of view of power level and complexity --

eb45

it's very close to the San Antonio reservoir. It's subject to about the same possible influence from earthquakes. The risk from one exists; the risk from the other exists.

It would not be a bad idea to compare them, and I'm not asking you to do that. I suspect the reactor might seem like a good neighbor in that context.

But I would indeed like to know that someone who complains of it was able to tell me that it is worse than anything else we have around and therefore, something on which we are most entitled to move on.

MR. BALDWIN: Well, the San Antonio reservoir is not something that the Nuclear Regulatory Commission has any concern with.

DR. MARK: Only the public.

MR. BALDWIN: Only the public indeed, and that's what we're doing today, examining this one facility, the GE Test Reactor, and there has been substantial concern about seismic safety underneath dams in California. And in fact one of the biggest of them all, the Auburn Dam, was cancelled for that reason, or is in the process of being cancelled for that reason.

PROF. KERR: Are there questions or comments?

(No response.)

Thank you, Mr. Baldwin.

We will recess for lunch and will reconvene at five

eb46

minutes after two.

(Whereupon, at 1:05 p.m., the meeting of the Subcommittee was recessed to reconvene at 2:05 p.m. the same day.)

1462 173

Tape3contd

agbl

5.2

AFTERNOON SESSION

(1:05 p.m.)

PROFESSOR KERR: We will reconvene.

Mr. Harding, my agenda seems to show that you're up.

MR. HARDING: Okay.

Well we got through an awful lot of data concerning the regional geologic tectonic setting, the conditions we found at the site from our explorations and the Quaternary history of the GETR site as well as the offsets and ages of offsets on the shears.

Hopefully now I can try to bring it all together for some of you who may have gotten lost and try to reach some interpretations and conclusions.

What we have tried to do up 'till now was present primarily facts where possible. What I'm going to do is mainly interpretation.

So if I can have that first slide?

(Slide.)

There is no real hard evidence on the GETR site in terms of the shears that we see in those trenches that enable us to determine definitely one way or the other whether they are of tectonic origin or of landslide origin.

So what we must do then is to try to look at both of these hypotheses and see how they fit in relation to all the other information which we have presented today.

agb2

If we look first at the landslide hypothesis, we can say that if this hillfront here next to the GETR where we have our shears down at the bottom represents the landslide, then it really has no relationship to the regional structural geology because it's essentially a surficial feature, it may be related to the seismicity of the area, but we really don't care what the regional geology looks like in terms of that.

Next slide.

(Slide.)

If we look at some of the features that we see in the trenches, for example, this is Trench B-1, a cartoon of it. We can see from the attitude of the shears, particularly this one, that it tends to flatten out with depth, as determined in Trench B-1 where it becomes nearly horizontal before it hits the bottom of the trench, as well as in Trench T-1 which Doug Yadon showed you where we drilled the borings down and were able to trace it out and it, in turn, becomes horizontal. This is exactly what you might expect at the toe of a large landslide.

The B-2 shear, we dug down as deep as 45 feet in Trench B-2 and it continued to dip downstrike and we were not able to determine if that one did actually flatten out.

There are some other features in the trenches which, if you look at these shears, you would, for example, assume that it was tectonic and this was an active fault which has

agb3

continued to move through time.

You would expect to find, for example, a surface scarp associated with these shear features. In no case did we ever find a surface scarp actually associated directly with the feature at the surface of the ground.

Another thing you might expect to find if it was a fault would be a rubble zone downslope from the fault. As this block moves up it gets eroded off and you get rubble deposited, the kind of thing you find very often in many fault exposures. We don't see anything like that here.

Next slide.

(Slide.)

In terms of the age of the offsets now as determined by Dr. Shlemon, how does our landslide fit with these various offsets and the age relationships?

If you'll recall, he talked about these oxygen isotope stages representing relative high and low stands of sealevel, the high stands presumably interglacial stages, relatively dry climate and periods of land stability.

The low sealevel associated with glacial stages, presumably a period in which the climate was much wetter than it is today and a period of landscape instability.

These ages here represent sort of the boundaries between these various stages. If you look at our offsets over here from both the B-1/B-3, B-2 and H shears, we find that

agb4

clear down in the bottom of the trenches we had as much as 40 feet on the B-1, as much as 80 feet and as much as 20 feet of offset within the Livermore gravels below the Paleosol, which is represented by this Stage Five. Therefore, the major movement on these shears occurred some time in this period (indicating).

We can see there several low stands representing periods of wetter climate during which this could have occurred.

As we move up, then, closer to the Holocene, we find that this particular Paleosol is offset this amount in each of the trenches. This could have corresponded to this particular low stand of Stage Four.

Our stone line, which is a Stage Two stone line, has then been offset this amount. And within the last 8000 years or so we've had zero offset.

So in terms of a landslide, then, we can see a major amount of movement occurred prior to 70- to 130,000 years ago, presumably at a time of wetter climate and also likely accompanied by some sort of a seismic event, in other words, a combination of climatic factors and seismic activity triggering this thing.

Since that time, we have seen renewed movement again occurring in a wetter climate and a time probably of seismic activity.

agb5

Now we are next to the Calaveras Fault, which has a recurrence interval -- it has been estimated for earthquakes to be varying from 10 years to 100 years, something like that.

So we can see that in these long periods, there were hundreds of earthquakes occurring during those times. And it seems likely that at least one or two of them could have occurred fairly close enough to the GETR to give us a seismic input and cause repeated movement on the landslide.

Next slide, please.

(Slide.)

Now is it unusual to find landslides of this age? Of course not, there's no reason to think landslides are only recent phenomena.

This is an excerpt from a table presented in one of our reports on landsliding, and it shows a number of slides which have been dated in California ranging all the way from 800,000 years -- and I might point out that this is new information, this is an update which we recently got ahold of -- all the way down 40,000 years, 95,000 years and so on throughout the Pleistocene.

Next slide.

(Slide.)

What about landslides in the area, is it unusual? Here is a picture looking up looking north along the trace of the Calaveras Fault. This is Highway 680, the GETR site is

agb6

over to your right. And we see Pleasanton ridge here, and this whole ridge side has been mapped as a large landslide complex in a recent paper given at the recently passed GSA meeting in San Diego by Dresson and Cummings in which they investigated these slides, and they suggest that there has been at least three periods of repeated movement probably related to seismic events on the Calaveras Fault.

Next slide.

(Slide.)

This is a little far afield, but this is landslide on a flat marine terrace surface up near Point Arena, California that we had investigated at one point. And we were -- in this case because there has been little erosion, were able to delineate this slide by digging some 42 trenches across the thing and drilling some 25 bucket auger holes and nine core borings across it.

This slide is over a thousand meters long and over 350 meters wide, and I would like to just show you the next slide, which is the cross-sections longitudinally and laterally through this feature.

Next slide.

(Slide.)

The upper slide is the lateral cross-section, this is the longitudinal cross-section, and you can see the kind of slope we're talking about.

agb7

We ran a stability analysis using strength values measured on the slip plane which was a clay bed within this syncline. And the only way we could see to move this block, which shows evidence of repeated movement throughout the Pleistocene, was by imparting a seismic acceleration to that.

Next slide.

(Slide.)

If we go back to this cross-section which Doug Yadon showed you then, we can see that certainly in terms of age this proposed landslide is certainly old enough to have removed a considerable amount of material from the upper surface of it--much of its driving force in the process completely modifying the slope and modifying some of the features that you would expect to find in a modern recent landslide.

Next slide.

(Slide.)

MR. PHILBRICK: That terrace directly under the words "Trench G-1," is that found elsewhere in that area?

MR. HARDING: Pardon me?

MR. PHILBRICK: The terrace directly below Trench G-1, is that level found elsewhere in that area?

MR. HARDING: You mean this bench here?

MR. PHILBRICK: Yes.

MR. HARDING: Yes, that's characteristic of our agb 8

slide. If you'll recall the picture I showed when I started out, we have characteristically a high area, a bench area and a low big toe.

MR. PHILBRICK: Does the bench area appear away from this area as to the south?

MR. HARDING: Not as characteristically as it does here. If you go across Highway 84 --

MR. PHILBRICK: That's right.

MR. HARDING: -- to the southeast, we find a very flat surface on top of our middle conglomerate which is pretty level and uniform, however, that surface is lower than this one.

MR. PHILBRICK: Okay.

MR. HARDING: Is it unusual then to have the landslide so eroded that you can no longer find the pull away, head scarp and those kind of features? Apparently not, because this is one which was investigated in southern California by Michael Hart, and this landslide was investigated over a period of two years, they kept extending back its limits as they did investigation, and it wasn't until they had completely exhumed this thing in the process of development for a large tract that they were able to determine that the head scarp was actually in back of the hillfront and there actually was an example where erosion had, at least in this area, created an inversion of topographic relief.

Next slide.

agb9

(Slide.)

In summary, then, of our landslide: This hypothesis has no conflict with the regional geologic setting, the number, attitude and character of shears are consistent with the relationships expected in a large landslide complex. The age of the landslide is sufficient to allow significant erosion of the head scarp area. Pleistocene landslides are certainly not uncommon in this area and renewed movements resulting from a combination of different climatic conditions and seismic events are also common.

Next slide.

(Slide.)

Now let's examine the thrust fault hypothesis. This is a section which you should be familiar with by now: GETR is sitting here, our hillfront and the locations approximately where our shears are.

And we see if we were to project the shears downslope or downdip, we'd find that they end up out here in the middle of the Livermore Valley, so that the root zone of our thrust zone is a deep basin or what you would expect would be the root zone to show an uplift of basement rock in this area. We actually have a depressed basin.

On the other hand, if you tried to steepen the dip of this fault, which essentially you have to do if you want to connect up a relatively flat-lying B-1 shear with the

agb10

moderately dipping B-2 shear, then we would expect to see some evidence of a repeated section crossing the middle conglomerate unit and bringing that up into the hills, and also evidence of repeated sections of the Foley Number One Well, the next slide will show that relationship.

(Slide.)

This is what you expect, then, if you were to steepen the dip on that fault.

We see no evidence of any repeated section and outcrop in the hills, and an examination of the Foley E log shows no evidence of repeated section within the upper 3000 or 4000 feet of that log.

Next slide.

(Slide.)

This is the Foley Number One E log, it was logged from 500 feet to much deeper than the section we're interested in. It is approximately from about 1000 feet to say 2000 feet. This is what we're interpreting as our middle conglomerate unit, and we see then no repeat of any section in this part of the log.

Next slide.

(Slide.)

Now based on the attitude and strike of this shear then which is over here, our three GETR shears, in relationship to the Calaveras Fault Zone, you can see that this portion

agb11

of the fault is nearly parallel to the Calaveras Fault. So we would expect, then, assuming a north-south compression, that this fault should have a significant component of lateral slip.

If we look at the slip directions, though, that were actually measured in the trenches -- Next slide.

(Slide.)

This is what we see. The black lines here with the arrows are the strike and dip of the shear zone to the south. The arrows, the green arrows which here may be hard to distinguish, show the plunge and direction of slickensides and the double-headed red arrow then shows the direction of that slip on the shears.

What do we see, is there a consistent pattern here?

Well here it looks like right oblique slip, here it looks like left oblique slip, here it is nearly pure dip-slip as well as up here.

So in terms of the direction of slip then, our shears fit better the landslide case than they do what we would expect to see if it was a fault.

Next slide.

(Slide.)

Well then to cap the thrust fault origin, the thrust fault is difficult to fit into the geologic setting and make it fit what we know about the regional geology, and the direction of slip is not what one would expect given its orientation

agb12

within this tectonic setting.

Next slide.

(Slide.)

There is one other alternative explanation for these things, and that is that these shears up here represent some sort of a bedding plane slip or detatchment structure which results from the uplift that we see and the drag folding adjacent to the Calaveras Fault.

In other words, we are wrapping these sediments around and we are pushing up the tertiary formations here to the point where we are developing adjustments within that. In that case, these shears then would have really no essential root zone and would be non-seismogenic.

Next slide.

(Slide.)

Our conclusions, then, on the origin of our shears are that the landslide is most reasonable, if not the conclusive interpretation. However, in order to be conservative, we have to assume that the shears are part of a zone of thrust faulting.

We must go on then and try to characterize that thrust fault zone on a basis of the known geologic data to establish a design criteria. And we need to characterize that in terms of the length of the fault, in terms of the average slip rate, in terms of the recurrence interval of expected

agb13

offsets and in terms of the amount of historic offset on those shears.

Now, it has been proposed by the reviewers of our report that this shear represents a fault which can be extended to the northwest and connect with the previously mapped Pleasanton Fault up here in the Pleasanton area.

As Doug Yadon has told you, our Trench E up in this area as well as the investigations we did in this area, appear at least in our estimation to preclude that kind of extension along the hillfront.

Now several other investigations have been done in the Pleasanton area looking for the Pleasanton Fault. And we have gathered up that information and submitted it to the Staff. And they cite some of those reports as indicating that the Pleasanton Fault is there, and therefore, as evidence that you can make a connection between our GETR shear and the Pleasanton Fault.

So the next slide will be looking in this area near Pleasanton, our hillfront here, to examine some of this data.

(Slide.)

You can see here the linear hillfront which has been discussed before. This is our Trench E. This is our seismic reflection profile or borings. There are a number of other green lines on here. First I had better talk about the

agb14

black lines.

The black lines represent previously mapped traces of faults in the area. This was one mapped by the Department of Water Resources back in 1966. These black lines represent the California Division of Mines and Geology Special Study Zones Maps. And this, of course, is our Verona Fault trace on the basis of Bill Herd's work.

The green lines on there represent explorations. The solid green lines, such as our Trench E, represent actual exposures, either trench exposures or, in some cases, the cleaning off and mapping of incised channels out here in the flood plane.

The dashed green lines represent primarily geophysical traverses, either resistivity magnotometer or gravity, in some cases, seismic refraction profiles.

Of all of these investigations, all of those in green indicate that a fault does not exist up here at the Pleasanton Fault. There are only three out of those that suggested the possibility these existed, and these were cited in the SER.

One is a report by Alan F. McKay which is done here, it's a report based entirely on photointerpretation, and it merely pointed out the same lineament we've been looking at plus another lineament here which is questionable.

This report here was a report done by Judd, Hall

agb15

and Associates in which they had I believe it was four borings across a line here and had run several seismic refraction surveys across that.

And what they found was that in three of their borings on one end of the line they had a gravel unit down about 20 feet. And then they moved over about 500 feet and drilled another boring and didn't find that gravel unit. And so, based on that information plus the seismic refraction profile, they suggested that there was a fault there.

(Slide.)

The upper diagram of this slide is taken from that report and it's their interpreation of the subsurface profile based on their seismic refraction work, and they postulate a fault in this area (indicating).

However, if we take a look at their time-distance diagram up here, this is not the relationship that you would expect to find if you had this kind of configuration of fault boundary. What you would expect is what we see down below marked by the red lines.

So the next slide shows our interpretation of the subsurface conditions using their data. And we see that you can easily explain this kind of condition by just assuming a relatively shallow dip in the refraction surface here, which is just exactly what you would expect to find out there in flood plane materials.

agb16

Even Judd, Hall apparently wasn't too impressed with the data that they had, because they concluded in their report -- The next slide.

(Slide.)

"It was our opinion that insufficient data exist to definitely establish the existence of the fault and its activity."

Next slide.

(Slide.)

The other piece of information which was cited as showing that there could be a possible connection between our Verona Fault and the Pleasanton Fault was the Radum gravity profile which was done by Andy Griscom. And in that profile there are a number of anomalies, one of which occurs right here. And there are several others, I'll show you that profile in a minute.

But you can see that the Livermore gravels here dips beneath the valley alluvium at this point. And so if we look at that profile --

(Slide.)

-- that anomaly occurs right here. And Griscom said that there could be a fault there.

But it could just as easily be explained -- in fact, it is more reasonable to explain it as resulting merely from stratigraphy.

agb17

The rest of the profile, I think, shows just exactly what we have been seeing before, and that is that on a regional basis our bedrock surface is dropping off down into the basin as we go out toward the Livermore Valley.

Next slide.

(Slide.)

Griscom prefaced his interpretations with this comment:

"Because of ambiguity, it is nearly impossible to prove that local gravity anomalies on detailed profiles across unconsolidated sediments are definitely related to faulting. Several closely-spaced profiles would be necessary.

"Even if the same local gravity features are present on each profile and even if the features are co-linear and located along a proposed fault trace then the relationship, though rather compelling, is still not proven.

"In general, detailed gravity profiles are only one piece of evidence which must be evaluated in conjunction with all other evidence when searching for proposed faults in unconsolidated sediments."

I'd say we would have to agree with that last sentence.

agb18

Now even if we assume, after looking at this data which has been cited as evidence for the Pleasanton Fault, that it is good data, all of this, all of these investigations show the fault to occur along a trend which passes directly through our Trench E area. The Judd, Hall report, the Alan F. McKay report.

7.050

So all of the evidence cited to date passes along this trend, and we know from exposures here in our Trench E that there has been no faulting in this area for at least the last 70- to 125,000 years.

Next slide.

(Slide.)

This is just a recap of the Trench E area.

Next slide.

(Slide.)

Okay. This is a model of the tectonic framework of the Livermore Valley as proposed by the USGS reviewers. And what it shows then is our Verona Fault on the hillfront of the GETR connecting up in some complex juncture here with the southwest trending Las Positas Fault.

Now as Doug Hamilton pointed out, we believe that there is significance evidence to support the existence of the Livermore Fault which crosses this trend, and very little evidence to support the existence of the Las Positas Fault down into the southwest area.

agb19

Even so, assuming this model, what it requires then is that this whole entire Livermore Valley block is moving westward, requiring that it move down into the basin and then back upward into this Verona thrust fault zone.

Let's go then and examine what this juncture should look like if this is actually a true model. What would we expect to find then if we had a fault here connecting these, some sort of an echelon or complex juncture would be a thrust fault with the east side up.

Next slide.

(Slide.)

We would have to connect up our shears where we last saw them on the Verona Fault, go northeastward, make a nearly right angle, go through our Trench A area, come down here and somehow connect up with the Las Positas trend.

And I think you can see that kind of a model which -- it is restrained to do that -- would require that this be a thrust fault with the east side up.

Next slide.

(Slide.)

You'll recall this diagram of what we actually saw in Trench A. We saw not a thrust fault but a high angle strike-slip fault where the east side was definitely down as demonstrated by this deep soil zone and this ridge of bedrock off on the west.

gb20

Next slide.

(Slide.)

So in conclusion as to the length of the fault then we think that from the last places that we actually see the shear zone, we can project to the northwest only as far as Trench E where it is terminated. We can project to the northeast then only as far as the area of Trench A, even though in these areas we have seen no actual evidence that these shears exist there.

Next slide.

(Slide.)

Now in terms of the average rate of strain relief, we can see from this table which lists our various soil horizons on the Livermore gravels and the amount of offset -- and this is the maximum offset measured in any of the trenches and the age of those offsets, that we have an average rate of strain relief on the order of 0.002 of an inch per year.

Our recurrence interval turns out to be, if we assume that we have a series of three foot or one meter offsets, one here and several here and several here, it turns out to be on the order or 17- to 20,000 years, something like that.

MR. JACKSON: Earlier in the day, Dr. Shlemon, who is your consultant, indicated that the movements here were clearly Holocene. He said it several times. And your submittals

ebl

indicate that it is prior to 17,000 years. Now that position has changed considerably since the last review meetings we've had on this.

I would like to know what the position is of yourself and your consultant.

MR. HARDING: I don't think the position has changed one whit. Holocene would include 8,000 years -- anything between 8,000 years and 10 or 11 thousand years.

MR. JACKSON: The submittal in the response to Dr. Schlemon's report indicates that there is no movement post-17,000.

MR. HARDING: No movement post-17,000?

MR. JACKSON: Right.

MR. HARDING: I would disagree with that, Bob, unless it's a typographical error because we've always said that the stone line, which is in the age of 17 to 20 thousand years, has been offset three feet.

MR. JACKSON: Okay.

Is there Holocene movement on these fault features at the site?

MR. HARDING: I don't care what you call it.

MR. JACKSON: I really would like an answer to that question because it's extremely important to the landslide versus faulting issue.

MR. HARDING: All right. What we are saying, if

eb2

you will let me answer it, is that there has been movement before 8,000 years ago. Now if you want to assume that's Holocene, that's fine, I'll call it Holocene. I don't care. But that's the magic number.

There has been no movement since, or within the last 8,000 years.

MR. JACKSON: Which is corrected from actual age dates from 1600 to 3,000 years. Is that correct?

MR. HARDING: I think 4600 was one of the --

MR. JACKSON: 16 to 46. Okay.

MR. HARDING: Okay.

Getting back then to this table in terms of the --

PROF. KERR: Excuse me. I need some explanation. I don't know what is meant by "actual age dates."

Mr. Jackson?

MR. JACKSON: Dr. Roy Slemon talked for a long time about the actual dates which were obtained from the radiometric dating house that they were sent to, and he explained very well why there should be a correction factor applied to that because of modern movement contamination which tends to make the ages too young.

PROF. KERR: I followed that but I thought his conclusion at least, whether you agree with it or not, was that the actual age should be not less than about 8,000 years.

MR. JACKSON: That's correct, but the point that I

eb3

was trying to make is that there is Holocene movement here which --

PROF. KERR: I'm just trying to --

MR. JACKSON: And that it is an assumption to go from those dates, and the correction factor you apply to those actual dates from the radiometric dating firm is a correction factor which is applied with judgment, so it could be as young as 1600 years.

MR. HARDING: It could also be as old as 15,000.

MR. JACKSON: I agree with that.

The problem that I think we will discuss later is that there is a very important point here in that the -- Mr. Yadon spoke earlier about the non-offset Holocene material at the ridge crests at the back scarp fractures of this landslide and at the toe of this landslide we have Holocene movement so there is a discrepancy, and this has been a problem from the very early days here in equating the two.

PROF. KERR: And this difference of opinion is fairly crucial to the difference between the position you have and the difference being advocated by GE, or is this one of detail? Is this a key item?

MR. JACKSON: It's one of four or five or six or seven items that we think are key.

PROF. KERR: It's a significant item but not necessarily the most significant?

eb4

MR. JACKSON: It's very important to the landslide argument because in the very first days of the review, the landslide argument was entertained heavily by us, but it was argued by GE and Earth Sciences very strongly that this was a very ancient feature, it had very -- it was very old. It was morphologically very old, and that the importance of that was well lost at the time that the young age dates were found.

At the toe there is a discrepancy between the morphology where the material has gone that has been eroded from this amphitheater. We will discuss it at length when we get into it.

MR. HARDING: There are several answers to your questions, Bob, and I don't think I should go into them at this time. We have been providing them all along in terms of discussions, letters, answering your questions in terms of reports.

PROF. KERR: Gentlemen, I recognize that this is an important discussion for both of you. I would prefer if we can that we avoid acrimony and try to talk about facts and opinions.

MR. HARDING: Okay.

We're discussing here the rate of strain relief on an assumed fault. We're not discussing the landslide at this point. We've gone beyond that.

The point I was trying to make with this slide is that this rate of strain relief, determined by the actual

eb5

measured offsets of those shears, is an extremely low rate, on the order of a couple of orders of magnitude less.--

If I could have the last slide?

(Slide.)

(Continuing) -- than what we see in other faults in the area in California.

We see, for example, the nearby Hayward and Calaveras Faults have prebreaks and this was measured pre only and does not include what movement would occur during earthquakes on the order of 5 to 7 or 6 to 12 millimeters per year.

The White Wolf Fault, which is a thrust fault similar to the assumed fault at GETR, has a slip rate on the order of 4 millimeters per year, and the Sierra Madre Fault down near the San Gabriel Mountains, on the order of 8 millimeters a year compared with a .05 millimeters per year at the GETR site.

You can also see that the average earthquake or average offset interval then on the faults range from 10 to 100 to the order of 2,000 generally, and down here we're talking about on the order of 20,000 years.

Next slide.

(Slide.)

Now it has been proposed that in order to set up a model to determine what the offset to be expected on the GETR

eb6

shear should be we should look at the data from the San Fernando Fault. This is a physiographic diagram of the area of the Transverse Ranges in Southern California.

Los Angeles is here, the Mojave Desert out here, and the San Andreas Fault shown in the background providing the boundary between the north-moving Pacific plate against the south-moving American plate.

This is also the area of what is referred to as the Big Bend of the San Andreas Fault which goes from a generally northwest direction to a more nearly east-west direction and then back over here, returning again to its northwest direction.

Given then that we have this bend then in the San Andreas Fault and we have this plate moving against that bend, we can see that tremendous compressional forces are developed in this area and that is attested to by the large mountain range, the Transverse Ranges which rise to elevations of over 10,000 feet.

Along the front of that fault then we have a relatively long range front fault system which is more than 170 kilometers or so in length, in which in some places as much as 1200 feet of movement has been measured in the last 500,000 years.

Of that fault zone, a small segment approximately 12 kilometers long, broke in 1971. That's our San Fernando earthquake.

eb7

We're going to look at a cross-section across the San Fernando zone on the next slide.

(Slide.)

On the bottom is a cross-section across the San Fernando Fault compared at the same scale with our cross-section across the GETR area, the Vallecitos Hills and Livermore Valley, and what we see in comparing these becomes quite obvious I think.

We have obviously a much different topographic relief in the Transverse Ranges. We have three or four thousand feet of relief here versus 1200 feet. We see that at the root zone of our San Fernando Fault we have crystalline basement rocks, Cretaceous, even Pre-Cambrian raised up considerably in this mountain range.

We see that we have a tremendous down-warp section of Plio-Pleistocene materials here that have been thrust underneath this overriding block.

You compare all that to this section here and I think that by any comparison you want to make, whether it be topographic, uplife, or whether it be deformation of the rocks or whatever, we can see that this has obviously been an area of much higher tectonic activity, much greater rates of movement.

The maximum offset --

MR. JACKSON: Could I comment for a minute? I think

eb8

it's a good time since this is basically our position and not Earth Sciences'.

It will take one minute.

PROF. KERR: Okay.

MR. JACKSON: The attempt at wrestling with the amount of surface offset that a fault in this locale could generate -- well, I will discuss with some figures a little bit later, but just so it is not misrepresented, there was never an attempt to make a one-on-one comparison of this feature at the GETR site with the San Fernando. It was used only as an analogy because it is a thrust fault, it is in California, it's in close proximity to the San Andreas -- quote -- "system" -- quote -- and that's as far as we went with the comparison.

H

Part of the problem is there just isn't anything else to look at that is similar in terms of going to a comparison, and in that context, that's what I wanted to comment on.

PROF. KERR: But given the data and insofar as it is objective, you would not object to Mr. Harding commenting on it?

MR. JACKSON: Absolutely not. I would agree with him on most of these comparisons.

PROF. KERR: All right.

Please proceed.

MR. HARDING: I'll go ahead and make the comparison then.

eb9

It appears to me that quite obviously if you have a fault in this area the amount of offset you can expect is going to be different than this. Much less.

Did you have a question?

PROF. KERR: I was just going to comment that in this area it may be obvious to you but I have not seen many things that looked all that obvious today.

MR. HARDING: Well, I've been through the reasons. I guess it it's not obvious it's not obvious. Let's move on.

(Slide.)

To get back to our GETR site then, what we have left are two shears bracketing the GETR, and the historical data indicate to us that for at least the past 125,000 years and more likely for a much longer period than that, movement has been occurring primarily on these shears and these shears only, and no movement has occurred in here between these shears for at least the past 125,000 years.

So if I can have the next slide, --

(Slide.)

-- to summarize then, we believe that the ancient landslide is still the most reasonable origin of the shears at the GETR site.

PROF. KERR: And "ancient" means 1,000, 10,000, 100,000, or more?

MR. HARDING: In this case it could mean as much as

eb10

two or three hundred thousand years for the principal movement, and the last movement would have occurred --

PROF. KERR: But it could mean as little as how much?

MR. HARDING: 8,000.

To be conservative, however, we have assumed that a tectonic origin is the cause of these shears out there, and then based on the observed geologic data, the assumed fault zone has the following characteristics:

Its length is limited to eight kilometers;

The maximum amount of offset we would expect, based on the historical data and based on comparing it with San Fernando, is one meter.

Future offsets, just on the basis of what we have observed there, are more likely to occur on the existing shears.

That concludes my presentation.

PROF. KERR: Thank you, Mr. Harding.

Are there questions?

Mr. Okrent.

DR. OKRENT: First, from the point of view of information, are we going to hear more on landslides in the GE presentation?

MR. HARDING: I don't believe so, although I don't know what some of the other presenters are going to present.

DR. OKRENT: And are we going to hear more on the probability of .5g or .8g or this sort of thing from GE, that

eb11

is, the degree of shaking that would occur?

MR. DARMITZEL: Dr. Richter is going to discuss that.

DR. OKRENT: That's later in the program on seismology?

MR. DARMITZEL: That's correct.

DR. OKRENT: With regard to the landslide question, I see that on page 10 of the Staff document dated September 6, 1979, under Item 9 they have a sentence which says:

"In the absence of a definitive evaluation we must make the conservative conclusion that the GETR could be impacted by a landslide. The dimensions of such a slide cannot be estimated at this time."

Have you provided information in some other way that, in your opinion, provides the dimensions of the landslide that might occur?

MR. DARMITZEL: We have done a very brief analysis of the soil stability above the reactor which showed that there would not be a risk to GETR from landsliding. However, we have been trying to get resolved for nearly two years now the hazard to the reactor due to surface offset and ground acceleration from the Calaveras Fault. We have not been able to get beyond those two points with the NRC, and that's the purpose for this meeting today.

We would like to get resolved the surface offset

eb12

value where that surface offset would occur, and the review could then go forward.

If we can't reach agreement on surface offset as is documented in their Safety Evaluation Report input, it is near to impossible to evaluate a reactor to 2.5 meters breaking the surface beneath the reactor. So the primary purpose is to resolve the extent of surface offset and where that offset would occur, and we will have a probability analysis description on that point in just a few moments.

DR. OKRENT: Thank you.

MR. JACKSON: A comment from the Staff point of view on that.

We included a landslide interpretation here because GE has argued that it is not part of the over-all seismic and geologic design basis. That's the reason why they have not entertained it. We have included it in here because we do believe it is part of the geologic and seismic design basis which is the issue in the Show Cause proceeding issue that we are trying to respond to in this SER, so we included that in there.

John Greeves of the Staff will make a brief presentation on the landslide aspect when we make our presentation.

PROF. KERR: Are there other questions or comments?

Mr. Harding, I gather from one of the comments you made when you started your presentation that there was

eb13

no conclusive evidence that the shear observed was either due to seismic or to landslide but that you felt perhaps the logic was on the side of the landslide.

MR. HARDING: I think that's correct.

PROF. KERR: If we had time I would be interested in hearing you argue the other side of the issue. Do you think you could make a good case for it being seismic in origin?

MR. HARDING: We have always had trouble doing that. I think, as I pointed out, and that was one of the reasons for weighting it toward the landslide in our opinion. It's more the negative evidence against the tectonic origin, I think, rather than the supportive evidence for the landslide.

PROF. KERR: But you would not necessarily-- But you can see how it might be possible for an honest individual with considerable professional competence to conclude that maybe it was of seismic origin, or do you think that --

MR. HARDING: Oh, of course.

PROF. KERR: So it is in your view still somewhat equivocal but with the weight of the evidence being on the side of the landslide? I don't want to put words in your mouth. I'm trying to explore --

MR. HARDING: I think that's our interpretation. I'm not suggesting that that would be other people's interpretation. Obviously it isn't.

PROF. KERR: I'm asking for yours, not anybody

eb14

else's.

Thank you.

Now as I look at this agenda and at my watch I do think we want to leave the Staff some time to talk.

MR. JACKSON: In fact, we were going to request extra time because at the time we agreed to this agenda I had been told briefly that GE's presentation would be approximately three hours. We cannot do justice to let's say a rebuttal in the time allowed on all the issues, but we will attempt to do so in the time frame. But we would request as much time as possible.

PROF. KERR: Well, since it would be helpful to you to know to what you are responding, it seems to me we ought to give GE a chance at least to say what they want to say. If we need to hold a later Subcommittee meeting, I'm sure we can't schedule one tomorrow but we could schedule one within the fairly near future perhaps.

MR. JACKSON: I think our argument is presented in the written text and we'll just highlight those as best possible in the time available, but I would request as much time as we can have.

PROF. KERR: I would hope that GE can maintain a schedule so that we can complete the GE presentation by 4:00. Is that going to be possible?

MR. DARMITZEL: We will skip then to the part of the

eb15

Tape 4 fls.

presentation that deals with the application of probability methods to a problem such as surface offset underneath the reactor, and go to Dr. Jack Benjamin for the next presentation.

MR. BENJAMIN: Thank you.

1462 208

Tape4
agb1
7.380

May I have the first slide, please?

(Slide.)

I would like to change the topic just a little bit and very briefly make a few statements about probabilistic methods, after which John Reed will present an evaluation of the probability of no offset beneath GETR.

MR. JACKSON: Dr. Kerr, could I ask that Dr. Benjamin address -- indicate who he is employed by?

MR. BENJAMIN: I'm Jack Benjamin --

MR. JACKSON: And his background in seismology and probability?

MR. BENJAMIN: I'm Jack Benjamin with Jack Benjamin Associates, and not being a geologist, I am an applied probability, I suppose, authority with books, papers, this type of stuff. I'm not a geologist. So the geologist -- in effect I will make this point later on -- but the geologist testified the information and reading the model, and then we perform the operations from this point on.

So I'm simply going to say the probability of a new offset intersecting existing structure can be reliably forecasted.

Next slide, please.

(Slide.)

And I will discuss the basis for this type of argument.

agb2

First, as a general approach which might be based on faith and growth and applied probabilistic methods, it says that probability methods are useful, reliable and their use is growing exponentially.

The first real application engineering for probability methods goes back to about 1940 to civil engineering, about 30 years. In geotechnical work, it's about 15 years old.

Thus far there aren't any textbooks in geotechnical, but they'll be coming along shortly. And there are many, many papers in the field. Actually, probabilistic methods are the accepted way of performing most investigations today that deal with real data.

PROFESSOR KERR: Mr. Benjamin, I don't want to appear rude, but I don't believe that you have to convince us that probability is useful in certain situations and that it can be applied to physical systems.

MR. BENJAMIN: Thank you.

Let me move on, then, to the second point.

PROFESSOR KERR: I would hope that we could be substantive.

MR. BENJAMIN: Fine. Thank you.

Of course, the basis for this is that probability methods are universal rather than subject-related, that is, you don't deal with individual draws of cards, this is not the

agb3

basis but, rather, it's the general models with which you're working.

Next slide, please.

(Slide.)

Now most of the criticisms of probabilistic methods have been related to, first, the levels of information, I've heard some of that at this meeting.

I say if the model fits, if the geologist or seismologist will provide a model, or the general characteristics of the model and these characteristics are known, then the forecasts are reliable with any level of information, whether it is one experiment, no experiments or a thousand experiments, it doesn't make any difference. We have theories that handle this.

Secondly, uncertainty between the model and reality does not invalidate the forecasts. Because what we do with such problems is we will make our forecasts and then we will take the second step and look as to how reliable the information and the model and so on.

Third, some people have said that geology does not use probabilistic methods. It certainly seems to me that they do. They may not do it formally, but they do it informally.

Next slide, please.

(Slide.)

The world is probabilistic but not deterministic.

And, Mr. Chairman, I will pass on to the final slide.

(Slide.)

So to repeat, the probability of a new offset intersecting an existing structure can be reliably forecasted is a statement of my introduction. And I hope to turn this over to John Reed, and he can show you exactly how it can be done.

PROFESSOR KERR: Would you add "convincingly" to "reliably?"

MR. BENJAMIN: It's a matter of which side of the fence you're standing on, some people don't like probabilistic methods.

PROFESSOR KERR: So it may not be possible to do it convincingly?

MR. BENJAMIN: It may not be possible to do this. Certainly the problem of acceptable risk is one of the areas that remains to be resolved.

MR. JACKSON: Could I question how important -- for just my own understanding, if I could --

PROFESSOR KERR: Mr. Jackson, please.

MR. JACKSON: I'm sorry.

PROFESSOR KERR: Continue.

MR. REED: May I have the first slide, please?

(Slide.)

agb5

The purpose of the probabilistic analysis that was performed was, first, to determine the probability of occurrence of a future surface rupture offset of any size greater than zero beneath the reactor building foundation. And once this was done, the second purpose was to determine whether the probability of occurrence is sufficiently low so that surface rupture offset should not be considered as a design basis event.

Now, there are a couple of points that I want to make very clear here at the beginning as to exactly what probability we are computing. We are computing the probability of occurrence of an offset beneath the reactor building, not on existing shears but beneath the reactor building.

The second point is this probability is for any size surface rupture offset, whether it is an inch, a foot or a meter, we're looking at the whole family of potential offsets.

And I think you can appreciate that if we were focusing on, say, a three foot or larger offset, we would find that that probability would be somewhat lower than the one that we are computing here. So we're focusing on a conservative value, namely, the one for all future surface offsets.

Another reason for formulating the problem in this manner is it allows us to be less restrictive with the interpretation of the data, and I think that will become

agb6

clearer as I get into the presentation.

The second point, determining whether the probability of occurrence is sufficiently low so that surface rupture offset should not be considered as a design basis event leads to the need for a criterion.

Next slide, please.

(Slide.)

This is the criterion that we used in our analysis to determine whether our computer probability was sufficiently low. I can read it to you:

> "A conservative calculation showing that the probability of occurrence of potential exposures in excess of the 10 CFR Part 100 Guidelines is approximately 10^{-6} per year is acceptable if, when combined with reasonable qualitative arguments, the realistic probability can be shown to be lower."

This is from the U.S. NRC Standard Review Plan Section 2.2.3.

Now there are several points in this criterion that I think we should be clear about. First, we're making a conservative calculation. At each point in the analysis we pick conservative values and comparing that to the 10^{-6} number. This implies that if we were to pick more realistic values that, in fact, we would find our probability would be ab7

even lower.

I'll show as I go through the presentation the conservative elements that are part of the analysis that was performed.

I want also to point out that in this criterion the event that they're talking about is potential exposures The probability that we computed was the probability of an offset not included in the calculation where the potential and probability for damage and given damage release, radioactive material or the dispersion or finally the exposure, namely, that people there are to receive, the dose of radioactive material.

So if you added those probabilities on top of what we have already computed, the probability would be even lower.

There's an example of this criterion being applied in the nuclear power plant context for Hope Creek 1 and 2 recently. This criterion was used as a basis for eliminating a flammable gas cloud from an LNG tanker accident --

PROFESSOR KERR: Mr. Reed, excuse me, let me urge in light of the time we have that you not try to convince us that the criterion is reasonable. You have convinced us that at least in one place it has been written down. I would urge that you spend the time convincing us that you can achieve it, this number, and then we'll discuss the ad8

acceptability if we are convinced.

MR. REED: All right.

If we can move on to the next slide, please.

(Slide.)

If I may, I would like to state to you the results and the conclusions of the analysis so that we know what we're aiming for here.

First the results are as follows: the calculated probability of occurrence of a future surface rupture offset of any size greater than zero beneath the reactor building complies with the criterion. That's the first result.

The second result is that the probability analysis is conservative. Based on these results, the conclusion is surface rupture offsets should not be considered as a design basis event for designing the reactor building.

These are what we are aiming for here.

The point, again, is that we're talking about any size offset.

The second point I would like to make here is that the analysis that was performed is independent of whether you consider this to be a landslide or of seismic origin.

Next slide.

(Slide.)

I'd like to give you quickly the outline of my presentation here since we're going through it so you can kind

agb9

of keep track where we're headed. I'm going to show you three approaches. These approaches start with a simplified and become increasingly complex. The reason for doing this is as follows:

In the simplified approach I think we can see quickly the results of the analysis and be able to see how it depends on the data. The other approaches are a little more mathematically rigorous and they give essentially the same values.

One of the points -- or, rather, one of the criticisms that has been made of the analysis that has been performed is that somehow the model that has been selected -- particularly the more detailed model -- somehow it doesn't reflect that there could be some sort of strain rate growth on the existing shears, that it's like a trigger waiting to go off.

The point that I wish to make, the simplified and the confidence level probability approach is that it doesn't depend on this, it doesn't matter what the strain rate occurrence model is on the existing shears in terms of computing the probability beneath the reactor building.

I might add as an aside here it would make a heck of a lot of difference if we were computing the probability of an offset on the shears but not beneath the reactor building.

agb10

Second, the analysis doesn't depend on whether in the past offsets have occurred simultaneously on the two shears, in other words, two of them at once or one at a time, the analysis doesn't depend on that.

By looking at the simplified approach and the confidence level approach we can see this.

Next slide, please.

(Slide.)

Very quickly -- we've seen this today. We're talking about two shears, namely the B-1/B-3 shear. The B-2 shear -- you remember the trenches and you remember the location of the GETR. We are taking a slice out of this model, a two-dimensional slice, length and depth, and saying that this is representative of the situation. This is conservative since, in fact, it could be possible that you could have an offset that would affect some area out in here and not here (indicating).

Next slide, please.

(Slide.)

Again this is the cross-section. I present this because I want you to remember a couple of numbers as we get into the actual analysis.

Again here's our B-1/B-3 and our B-2 shears. The distance between the two shears is 1320 feet, a number you should remember. The second is the width of the reactor

agb11

building, namely, 72 feet.

Note also that in the analysis we have assumed that there are two types of events that can occur. If in fact an offset occurs at all, it will either occur on the existing shears or in between the shears.

There is also another event that's possible, namely, the next one might occur outside. We neglected this. We said that we'll be conservative and consider only the possibility between the two existing shears.

Next slide, please.

(Slide.)

This is the data. This is the data that you have seen before in several presentations. This is the same data that we're going to use to compute the probability of an offset beneath the reactor building.

Now it's important to realize that in this probabilistic analysis the only time number that really becomes important is the last number, namely, 128- to 195,000 years. In this period of time, all offsets have occurred in the existing shears. Between the existing shears over that 1320 feet there have been no offsets for the last 128- to 195,000 years.

Now you might keep in mind when we get into the detailed model that there are four time periods. Onepoint that was brought up earlier was concerning this earlier date,

agb12

and it's important. This number is not important to the analysis, it's the last number that's important.

In the detailed model, there is another mechanism that is used. Rather than working with the total displacement, we're going to assume that somehow in that total displacement, say at five feet, we know how many offsets occur.

It turns out that because of the fact that we're looking at the probability of any size offset or greater, that we can use this mechanism to perform the probability calculation and that, in fact, the number of offsets in this 128- to 195,000 years can be handled and dealt with and shown that the results are conservative.

C8

Next slide, please.

(Slide.)

Now there are some basic probability parameters that we need to have in hand here that are common to all three methods. What we are computing here is the probability P, annual probability of an occurrence of an offset beneath the reactor building foundation, and this is equal to the product of two terms.

The first term, P-1, is the annual probability that an offset will occur between the two shears. And as you might expect, this number is fairly small because for the last 128,000 to 195,000 years, the offsets have been occurring on the shears.

agbl3

Now given that somehow an offset occurs between the shears, P-2 tells us the probability that an offset will come up underneath the reactor building. If you remember, there is that 1320 foot distance between the two, so it's possible if an offset occurred between them it might miss the reactor building. In general, it would be more probable then that they would miss it.

There's also a point that these definitions bring to mind in that there is a difference between surface rupture offset and vibratory ground motion, particularly in the presence of a nearby fault.

If an earthquake occurs, for example, you're going to sense the vibratory ground motion, you cannot get away from it. You can argue how big it's going to be but you cannot escape it.

This is not true of an offset. First of all, an offset has got to occur. Second, it's got to occur off the existing shear where it has been doing its thing for the last 128- to 195,000 years. Then in addition, coming up between them, it's got to get the reactor building.

So there's a very great chance that it won't hit underneath your reactor building, and I think this will become clearer as we get into the numbers.

Next slide, please. 1462 221

(Slide.)

agb14

The simplified approach. This is the first one of the three approaches that we're going to look at here.

I've broken this slide into two pieces, I've assumed two values for the amount of time that we have not observed offsets between the existing shears: the lower value of 128,000 and the higher value of 195,000.

And we need an estimate of our P-1. Remember, P-1 is the probability that an offset will occur between the existing shears, the annual probability. What we have observed is zero events in the last 128,000 years, and one might be tempted to use that as a value. But to be conservative what one might visualize here is that we dug a little bit deeper. And as we dug deeper, we eventually found a shear underneath the reactor building. But as you dig deeper, the soil gets older. And so, when you did find that, you would find the age might be, what, 300,000 years. Then one would use 1/300,000.

So to be conservative we'll say we'll dig one inch deeper and we'll find it there, we'll say it's 128,000 years, so we used 1/128,000 as our P-1 value.

Now given that the offset has occurred, what is our P-2 value. The offset has occurred between the shears. We have a distance of 1320 feet. And we have a width of the reactor building of 72 feet.

We'll assume that it's equally likely that it could

agb15

come up anywhere between. And this again is conservative because the reactor building is located out near the quarter-point or a little bit farther and it is more likely that if the shear did occur it would probably occur closer to the shear, the offsets would occur closer to the shears.

So picking like one card out of 52, the analogy here is for P-2, the width of the reactor building divided by the distance between the shears. We have 72/1320. If you multiplied those two together to get P, the number you come up with is a probability, an annual probability of a future surface rupture offset beneath the reactor building of 4.3×10^{-7}.

Playing the same game with the 195,000 years, P-1 is 1/195,000, P-2 is the same, the number you come up with is slightly smaller, 2.8×10^{-7}.

Now this approach is intuitively appealing. I think you can kind of see the elements here. The interesting advantage of looking at it in this manner is you can see this doesn't have anything to do with existing shears, it doesn't matter whether an offset is ready to be triggered on the existing shears and it also doesn't matter whether the past offsets have occurred simultaneously on both shears or whether they've gone off one at a time.

Next slide, please.

(Slide.)

1462 223

agb16

The second approach is the confidence level probability analysis, and this is a classical statistical approach and it's used to determine our probability value for P-1.

What we have is the same data: we have zero offsets in the last 128,000 years. The question, the classical statistician is: I want a value of P-1 so that I have a very high confidence that the true value of P-1 is less than this assumed value, and that's exactly what this equation is giving you.

And this comes about by the fact that the underlying model leads to a confidence level distribution called chi^2. Because we have zero offsets in our length of time, T, we would use chi^2 with two degrees of freedom.

And if one uses that, one can find an equation value of that. And transforming that equation value appropriately, one comes up with the estimate of P-1 as follows:

As a function of the confidence level probability, you pick like 0.5 or 0.9 or 0.95 and the length of time that you've observed the zero offsets in the 128,000 years.

Now our P-2 is slightly more complicated than the previous one that we had. In P-2 before, if you remember, we divided the width of the reactor building by the length or distance between the two existing shears.

The difference here is we've added to the numerator

agb17

and the denominator the width of the offset of the ground surface, it has a finite width.

And the geologists have looked at the data out on the site and determined that the width of the offset, if it did occur in the future, would be on the order of two to four feet. So I picked the largest value, the four foot, and used that in computing the P-2 value.

Again, this model, before I show you some of the results, does not depend on the occurrence model in the existing shears, it does not depend on whether offsets in the past have occurred in pairs or individually.

The next slide, please.

(Slide.)

Now multiplying the P-1 and the P-2 together to get the total probability, these are the values that you get.

We have here three different confidence level probabilities, and these are the values of the probability, annual probability of the future surface rupture offset beneath the reactor building both for a T value of 128,000 years and for 125,000 years.

Notice that corresponding to the 90 percent, or 0.90 confidence level probability, annual probability of an offset beneath the reactor building is 10^{-6} for the conservative 128,000 year interpretation of the data.

agb18

Next slid., please.

(Slide.)

The third approach, the detailed model analysis, is an approach where we do make an assumption of what the occurrence rate for offsets is and we assume that the poisson distribution, this is a common distribution that has been used in the past for determining the occurrence probabilities of earthquakes.

As I've shown in the first two models, the effect of this assumption drops out, and this also is true when you look at the detailed model analysis.

For P-1 it's composed of two terms. The second term here, the lambda, E to the minus lambda, is the poisson probability of an offset in one year based on a mean rate of occurrence of offsets.

This is offsets anywhere, on the shears, off the shears. So we need the second term, the phi term. This is the term that gives you the fraction of offsets that will occur between the two shears.

So phi of the offsets will occur between the shears. And one minus phi will occur on the shears.

Our P-2 is exactly the same as it was for the confidence level probability, and our final probability is again P-1 times P-2.

Now the problem is that at this point we don't know

agb19

what phi is and what lambda is. We have to use our data. This detailed model analysis is using the Bayesian approach, and what we need is a probability distribution on lambda and phi.

Next slide, please.

(Slide.)

Now I'm going to go through this quickly, it's involved and I'm not sure it's important to the argument that we understand the mathematics shown here, but there are several points that I would like to make to kind of tie this thing together.

What we're looking for is a probability distribution on lambda and phi. And in the Bayesian context, this is composed of three terms:

First, there's a normalizing term that makes this a regular probability distribution, one in which the area under the curve is equal to one.

The second term is a likelihood function, which is a function of the data, and I'll talk about that in a little bit.

The third term is a prior distribution. And in our case, we assumed the diffuse prior distribution. However we looked at the alternative distributions and convinced ourselves that the prior distribution is a conservative one, so let's focus on this likelihood function because this is the kernel of the argument here and it is in it that the data

agb20

exists.

Now if you'll remember we had four time periods. And what we are assuming here -- or asking ourselves the question, rather, is what is the probability of observing the data as a function of, say, some lambda and phi values.

And the data that we have observed is the following: we have four time periods, and each one of those time periods we're going to pretend for a moment that we know how many offsets occurred in that time period. What we really to know is the total displacement, we really don't know the number of offsets. But that isn't constrictive, as we'll be seeing.

And so the first term here is just the poisson probability of observing NI offsets in time period I. And the second term here is the probability for that time period of observing the offsets on the shear, because that's where we did observe them, we didn't observe them off the shear and that's why you have to use the one minus phi value for four time periods.

So cranking this together, solving for our normalizing concept, we finally come up with the equation in this form.

The important point to realize here is that what happens is that it is not important what the individual offsets or time periods are, it's the total or the total times like our

agb21

128- to 195,000 years. And we're interested not in the individual offsets in each of the fourtime periods but the total number of offsets.

So our probability distribution on lambda and phi is a function, of course, of lambda and phi, but then the other two terms it's a function of is the N value, which is the total number of offsets and the T value,which is the total amount of time.

Next slide, please.

(Slide.)

Now there are two ways that we can use this probability distribution on lambda and phi to obtain an estimate for our P-1. If you remember, that's what this is all about, we're trying to get an estimate of P-1.

The first way is we could obtain a weighted estimate, this is kind of like an average value. We take our value for P-1, the equation -- if you remember that from a couple of slides ago, the phi, lambda, et cetera and we weighted over the probability distributions for lambda and phi, and that gives us our weighted estimate. And if you crank through the mathematics, this is the result that you come up with.

Now let's look at this result for a second.

First of all, as you can see for large values of N, there is very little influence that N has on this equation.

agb22

Remember, T is like 128,000 years, so T plus one is like 128,001, this thing is almost unity.

Similarly, for a large value of N, this is almost unity.

But the nice thing that we can see about this equation is we know each one of these two terms is going to be less than one no matter what value of N you assume, so we can conservatively say that our weighted estimate value of P-1 is less than 1/T. Well if you remember, that's exactly the value that we used in the simplified approach.

The second way that we could use this probability distribution for lambda and phi is to obtain confidence limits, and we use this loosely here in that what we are really doing is finding probability units -- probability limits. But for decision purposes, it's quite proper to call them confidence limits in the same context that a classical statistician might.

If you would kind of visualize for a second -- none of this is here on the slide, but just picture that you have these two axes, the phi axis and the lambda axis. And if you were to plot on this thing this probability distribution for lambda and phi, it would be kind of like a mountain coming out of the slide at you here.

And if you superimposed on that this equation for P-1, and then if one just integrated that mountain just in

agb23

the region where you are, say, greater than the assumed value of P-1, then one could compute the probability of exceeding P-1.

In terms of the confidence limit, what we really want is the other area, the area down in her namely, the probability of being sure that we don't exceed P-1.

So anyway we have the two approaches, the weighted and the confidence limit approach in which to use our data.

So if I could have the next slide, please.

(Slide.)

These are the results. This is for this detailed model that we just looked at. And I'm showing up here for comparison the confidence level probability analysis results that you saw a few minutes ago. I have here the three confidence level probabilities, the 50 percent, 90 percent, 95 and the weighted estimate.

Now I need to comment to you that in the confidence level approach, using the detailed model, one has to assume that he knows N. Well, what turns out is that if you start with low values of N like three or four and you compute the confidence level probabilities, then you go to higher values of N, you find that the probability number creeps up a little bit, but by the time it hits an N value of 10 it stabilizes, and by the time you hit 15 it's flat.

So what I'm showing here is just the computation

agb24

for the highest value, one based on N is equal to 15.

Notice that the confidence level values are almost identical to the detailed model values, the confidence level and the detail. The weighted estimate value, if you remember, is very close to the simplified model. There we had a value, I think, of 4.3 and 2.8. And the only difference is in the P-2 term, in the weighted estimate value we used here we included the width of the offset, that's why there's a slight difference.

Now another way to look at this data is let's ask ourselves a question: what is the confidence levels corresponding to our criterion, our 10^{-6}?

Next slide, please.

(Slide.)

So here we have the four numbers corresponding to the detailed model, to the confidence level model, for 128,000 years and 195,000 years.

Notice that the smallest confidence level is a 0.89, which is essentially 0.9.

So what this says is based on this analysis we have a 90 percent confidence that we have met our criterion.

Next slide, please.

(Slide.)

Now let's summarize some of the conservatisms, because this was part of the ground rules in using this

agh25

criterion.

First keep in mind that that probability is for an offset when that criterion was really for potential consequences. If you added on top of it the probability of damage, of release, of dispersion, finally of exposure, you would find that the total probability of exposure would be a magnitude lower than the probability of an offset.

The second item of conservatism is that we considered only that offsets could come up between the shears. In fact, it's entirely physically possible that it would come up outside the two shears.

Our conclusion is based on the 128,000 years. It would be more appropriate to probably use a value between 128- and 195,000 for the age of the material is probably even older than 195,000 years.

In our detailed model, we used a conservative distribution for lambda and phi. We investigated other distributions and found that the diffuse was conservative.

Second, or the last conclusion is -- the last element of conservatism is that we used a two-dimensional model when, in fact, if you went to the sides of the two trenches, in fact it's possible, physically possible, an offset could occur out there and not affect the reactor building.

Next slide.

agb26

(Slide.)

In summary, the weighted estimate probability value is less than 10^{-6}. The 90 percent confidence level value is essentially equal to 10^{-6}. The probabilistic analysis was performed in a conservative fashion. The analysis and the results comply with the criterion. Hence, surface rupture offset of any size should not be considered as a design basis event.

That completes my presentation.

PROFESSOR KERR: Thank you, Mr. Reed.

Are there questions?

(No response.)

You have presented this to the Staff before this?

MR. REED: Yes, it is contained in the EDAC report.

PROFESSOR KERR: Is it your view that the Staff finds your presentation on this convincing?

MR. REED: No, I don't think so.

PROFESSOR KERR: Can you give me any idea why maybe?

MR. REED: Well I tried to make some points in my presentation. There was a review of the report by David Shlemons of the University of Nevada and several points that he made were that this approach was not appropriate because of the assumption that the offsets--using a poisson

agb27

distribution on the existing shears, if I'm correct, in fact that an offset could be just waiting there to happen.

The point that I tried to make in my presentation here is that it does not matter in terms of computing the offset beneath the reactor building.

The second point that he made was that somehow the fact that offsets could have occurred in the past simultaneously on both shears somehow invalidates the model.

And the point I tried to make in the presentation was that this does not make a difference.

There's another third area that I don't wish to address, and my understanding is that they do not accept the age dates.

PROFESSOR KERR: Any other questions or comments?

Did you want to make a comment, Mr. Jackson?

MR. JACKSON: I was going to comment on that one issue. From the geoscientists' point of view, we reviewed the probability analysis and basically reviewed it from an approach which, and the way we were asked to review it, was to review the geologic and seismologic input assumptions and, second, provide an overall judgment on the use of this approach based on our best judgment and based on observations that have been made by geologists and seismologists, not into the -- at least from the people available here, not into the pure probabilistic aspects of it, it's just the basically --

agb28

and what you will hear is from Dr. Slemmons, who is a geologist, his review from that perspective.

PROFESSOR KERR: What I was trying to determine was whether the Staff found it a convincing argument <u>en toto</u>. My impression is that you did not find it a convincing argument.

MR. JACKSON: From the geoscience point of view again we don't find it convincing because of the observations of fault behavior as we observed them in the field.

PROFESSOR KERR: Okay. Thank you.

A friend of mine always had some reservations about using a poisson distribution because he said it always looked rather fishy to him, but I won't repeat that.

(Laughter.)

DR. OKRENT: I did ask a question right at the beginning of the Subcommittee meeting about was there some delay in the ACRS getting a copy of this probabilistic study? When did the Staff get it?

MR. NELSON: Chris Nelson, GETR Project Manager for the Staff. The Staff received the report shortly after it was issued, I guess, in April. And that should normally go to the ACRS as all incoming reports do on the distribution.

But then when it was noted to us that either you hadn't gotten copies or needed copies, then they were given to Mr. Igne which was some time later, I understand.

agb29

PROFESSOR KERR: Yes, like last week.

DR. OKRENT: Thank you.

MR. DARMITZEL: We had two other speakers on the agenda, but for the sake of meeting the 4:00 deadline, I will now summarize the --

DR. OKRENT: Excuse me. I must say, if you have any comments that you could give me in five minutes on the probability of different degrees of shaking at the site due to the Calaveras Fault, which I assume is the dominant source, I'd find it of interest. If you can't do it in five minutes, I'll have to forego it, I guess.

But I will offer a comment. At the moment, what I see is a statement that you could have an earthquake on that fault with a frequency of one in 10 to 1 in 100 per year. I don't know that it would always necessarily be closest to the site, but let me assume for the moment that at one in 100 per year it is. That then could represent a rather large challenge to the facility.

And so I'm a little bit interested in knowing how one gets from that kind of earthquake to different degrees of shaking at the facility.

MR. DARMITZEL: I don't know that I could answer adequately in five minutes, but I'll make an attempt.

Prior to the trenching that was done on the site, it was felt that the Calaveras Fault represented the greatest

agb30

hazard to the facility in terms of earthquake potential. And we have had several studies conducted to determine how much ground motion would occur as a result of an event on the Calaveras.

We've had a couple of reports issued, one by Engineering Decision Analysis Company which specified a 0.56g effective ground acceleration value to be used in analyzing the GETR. And we have a report from

And we have a report from Dr. Charles Richter, a recognized authority, that stated a horizontal ground acceleration value of 0.5g when correctly applied is an appropriately conservative value for the evaluation of the GETR facility.

During the course of this work, for the sake of expediting the review process we agreed to analyze the structure to 0.8g tied to response spectra described in Regulatory Guide 1.60, strictly for the purpose of expediting the process. There was no technical basis that we had in hand for using anything greater than 0.56g. That was the highest value that we had received from any consultant.

But we did analyze the structures, as I say, to the 0.8g value. And that submittal has been made to the NRC for some time. We suggested the 0.56g almost two years ago in a formal report. We have not at this time received from the NRC a value for analysis for the ground motion value.

agb31

As Dr. Jackson stated, he gave a peak value to another branch of the NRC and they were to get that worked on and come up with an effective ground acceleration value for analysis.

That's how it stands right now.

Our report showed 0.56g ground motion. We agreed to analyze the structures to 0.8g.

DR. OKRENT: Unfortunately I can't tell what it means if a consultant recommends 0.5g or 0.58g, what the basis of his recommendation is or what probability of non-exceedence he is seeking and so forth and so on.

So I was trying to understand whether General Electric thought it had submitted a study which in its opinion was a reasonably plausible and maybe even defensible analysis of the probability of different degrees of shaking at the site or the frequency per year or the recurrence, or whichever way you wish to state it. I can't tell from your answer whether you think you have submitted what in your opinion is a semi-definitive document on this subject.

MR. DARMITZEL: We have submitted what we thought was a definitive document on that, and the value submitted was 0.56g.

DR. OKRENT: But 0.56g presumably corresponds to some return interval, and 0.4 would be another one and 0.7g would be still another one.

agb32

MR. DARMITZEL: A 500 year return interval for the 0.56g.

DR. OKRENT: What's the return interval for 0.7g, does the report say, or for 0.9g?

MR. REED: It peaks out at 0.6.

DR. OKRENT: I don't know what that means.

MR. REED: If you look at the return period acceleration curve, the curve becomes vertical at 0.6g. It's very steep at 0.56, where it corresponds to 475 years. At 0.6, in terms of the curve, it's infinity.

DR. OKRENT: So you have made some assumption in this that you can't get higher than a certain shaking?

MR. REED: That's correct.

DR. OKRENT: What's your confidence in the assumption? Is that in the report?

MR. REED: Yes. Well, I think it is exemplified by the approach that was used. It was basically a replay of the record, of the historic record.

DR. OKRENT: That is a limited confidence study, I would say.

MR. REED: At the time the study was done, the approach that was being used was that the GETR would be analyzed as a good engineered structure, as would be a hospital or other critical facility, and that was the basis on which it was done at that time.

1462 240

agb33

DR. OKRENT: Just one other question. In your analysis at 0.8g, you mention looking at the structures. Did you look at the instrumentation and the piping and everything you would need to accomplish safe shutdown on a continuing basis?

MR. DARMITZEL: Yes, the analysis looked at the reactor structure and all safety-related equipment.

DR. OKRENT: Then presumably non-safety-related equipment --

MR. DARMITZEL: That might have an impact on non-safety-related equipment, yes.

MR. PHILBRICK: If there's a question whether it be 0.56 or 0.8 or 0.5, would it be wise to hear Professor Richter, since he's in the room?

PROFESSOR KERR: If you feel it will assist you in your responsibilities, it would be wise.

MR. PHILBRICK: I don't know about the responsibilities, but I would like to hear what he has to say.

PROFESSOR KERR: You have a considerable responsibility as a consultant to this august group. If you didn't know that before, I want you to know it.

(Laughter.)

Would you like to hear from Professor Richter?

MR. PHILBRICK: Yes, I would.

PROFESSOR KERR: We shall hear from Professor Richter

agb34

if he's willing to speak to us.

DR. RICHTER: I'm not quite sure what you wish at this moment. I had prepared a somewhat extensive memorandum covering a number of points, largely independent of the material which you have had so far presented.

Now you are, I gather, raising a specific point at this time. Do you wish me to speak to that only and preserve the rest of the memorandum for some later moment?

PROFESSOR KERR: Is the memorandum such that we can get copies of it?

DR. RICHTER: Yes.

PROFESSOR KERR: Did you have a specific question in mind, Mr. Philbrick?

MR. PHILBRICK: I just thought people were talking about 0.56 and not knowing whether they were talking about 0.56 or 0.10. It would seem to me the man who produced the number was sitting here and if he had something to say, he might perhaps offer it for us.

PROFESSOR KERR: That seems not unusual, but I want to defer a little bit to GE and see how they want to use their remaining 10 minutes. I'm sorry we don't have more time, but we really almost don't.

I would hope, in any event, whether the presentation is made or not, that we can get a copy of the memorandum.

DR. RICHTER: My name is Charles Richter. I'm

agb35

here as part representative of Lindvoll, Richter and Associates, and what I'm about to read to you is not merely in my personal statement but conclusions I included in a report of Lindvoll, Richter and Associates. It is not part of the personal memorandum I mentioned a moment ago.

I will read this:

"Conclusions: For the Calaveras Fault, two earthquakes have been postulated, one a maximum expectable event of magnitude 7.0 with a peak horizontal ground acceleration at the GETR site of 0.6g; the other a maximum credible event, a magnitude 7.5 with a peak horizontal ground acceleration of 0.7g. A mean effective acceleration for engineering purposes of 0.4g for the maximum expectable earthquake and 0.5g for the maximum credible earthquake is also specified for the site.

Now to that I would like to append personally that we give these peak accelerations and these maximum effective accelerations somewhat under protest, because we are constantly being asked for them. We do not believe this is the proper approach to the problem which consists of the presentation of a design earthquake and a complete computer analysis in view of the time history thus presented.

Thank you.

agb36

PROFESSOR KERR: Thank you, sir.

Shaler, do you have any questions?

MR. PHILBRICK: No, thank you.

PROFESSOR KERR: Mr. Okrent?

DR. OKRENT: Nothing.

DR. MARK: Do you associate any recurrence intervals with these events, Professor Richter?

DR. RICHTER: Pardon me, would you please repeat that?

DR. MARK: I was wondering if you associate a recurrence interval with events of this sort. You said a peak credible event of which you would expect it would happen once in 1000 years or once in a year.

DR. RICHTER: That is a peak vibrational oscillation and represented by the time history of what we consider the maximum credible design earthquake. Those time histories are obtained by study of the known recordings of earthquakes of various magnitudes and by various dates in the area. And we feel that this is the proper basis for safe design at the site in question.

And while perhaps a conclusion might be reached by setting up a recurrence interval, we're more interested in what might be expectable or credible within the reasonable life of the installation in question.

DR. MARK: Thank you, sir.

agb37

PROFESSOR KERR: Any other questions or comments?

Mr. Darmitzel, does that conclude your presentation?

MR. DARMITZEL: I have one summary slide that I would like to show.

(Slide.)

The recommended seismic values for the GETR structural evaluation that we have made are the following:

No offset which breaks the surface beneath the GETR. The basis for this position is the probability analysis that was performed and described by John Reed.

One meter of offset on the observed shears, 0.56g effective ground acceleration was the value we submitted to the NRC. However, as I said, we have nalyzed the structure and safety-related equipment to 0.8g.

We believe that the geology program has been thorough and responsive. Prior to embarking on the program, we discussed it in detail with the NRC and their consultants and agreed on the trenching and other examinations that were performed, and all questions that were raised have been answered in detail.

We believe that our position on the geology/ seismology is supported by the evidence. We feel that the most reasonable explanation for these shears is landslide. And for the sake of expediting the review, we have agreed to

agb38

analyze as though it were earthquake-caused and to the seismic values stated above.

We feel that it would be appropriate for the NRC Staff to prepare a value impact study which compares the value of the negligible reduction in risk to the public against the impact of the loss of fuel testing capability which can enhance the safety of all reactors, and the loss of the capability in the United States to produce the needed medical isotopes.

To elaborate on that, those isotopes no longer being produced at General Electric are now being produced by a foreign government and shipped to the United States.

Finally, we believe that our investigation and studies support the GE position and an independent body, the California Division of Mines and Geology, support our view that the most reasonable explanation for these shears is landsliding. One of the authors of that report is present today, Salem Rice.

And we urge that this matter be sent to the Full ACRS with the recommendation that the NRC Staff reconsider their seismic value input.

And that is the conclusion of our presentation.

PROFESSOR KERR: Thank you, Mr. Darmitzel.

Are there questions from members of the Subcommittee or from our consultants?

agb39

(No response.)

PROFESSOR KERR: I shall declare a 10-minute recess, after which the NRC Staff will begin its presentation.

(Recess.)

C9

PROFESSOR KERR: May we reconvene, please?

Mr. Nelson, I turn things over to you.

MR. NELSON: Thank you.

(Slide.)

My name is Chris Nelson, I'm Project Manager for the NRC Staff for the GETR, or General Electric Test Reactor.

I will minimize time spent on introduction, since we've been on the topic all morning. I would just like to point out that the Staff is today prepared to discuss its conclusions with regard to the first issue of the October, 1977 Order to Show Cause, which is right in the middle of the viewgraph shown up here.

(Slide.)

That is what the proper seismic and geologic design basis for the GETR facility should be. And if there are no questions at this point, then I will turn it right over to Dr. Jackson, who's Chief of Geosciences Branch of the NRC.

PROFESSOR KERR: Does anyone have any questions for Mr. Nelson?

(No response.)

agb40

Please proceed.

MR. JACKSON: I'd like to take a few minutes to give a brief overview of our geologic and seismologic review of the GETR site and how we approached it from our review within the NRC.

Since the submittal of the report, the so-called Blume report of 1973, the Staff identified linears in close proximity to the GETR building and, as a result, looked into available information. Part of this looked -- letters to USGS and Darrell Herd, who was mapping, doing a geologic mapping program in the Livermore region.

Since the show cause order was issued, we have tried to keep up with all the information that's been submitted by GE. There were two major programs called Phase I and Phase II investigations separated in time by about one year. There was substantial new information as a result of the Phase II investigations.

As a result of the difficult questions to be addressed here, we requested USGS to assist us. And they assigned to this review Bob Morris and Jim Devine of the Reston office, Dr. Earl Brabb of Menlo Park and Dr. Darrell Herd of Menlo Park.

And about one year ago, because of accusations as to the bias of the USGS, we asked for an independent consultant to work for us, Dr. Bert Slemmons from the

agb41

University of Nevada, who is a recognized expert in fault evaluation and has done a great deal of work with assessing our knowledge of the existing data on fault and fault behavior.

Our Safety Evaluation Report discusses the results of our review of information. We disagree with GE's interpretation on a number of points. We feel that the data and the evidence strongly support tectonic origin for the shears at the GETR site.

The geologic setting of the region, in a very brief fashion, is that we are three kilometers from the Calaveras, the third -- an active splay of the San Andreas Fault system.

We are located between two -- the GETR is located between two faults which have Holocene movement on them.

And there is incomplete mapping and not a thorough understanding of the overall tectonic development of the Livermore Valley. This is acknowledged in part by an in excess of $1 million study just undertaken by Lawrence Livermore Labs to assess the faulting and the origin of features in the Livermore Valley.

I'm going to ask Dr. Brabb to give a presentation of the regional interpretation of the geology as it is interpreted by the USGS, and then Darrell Herd will follow. Dr. Slemmons will comment on his analysis of the probabilistic

agb42

methodology and then some comments on the amount of offset. Jim Devine will make some comments on the earthquakes on the Calaveras, and I will follow up with a final comment on the surface offset.

Dr. Brabb.

DR. BRABB: Obviously, for the last six hours you've heard a great deal of information. Someone here asked for a separation between fact and inference a long time ago.

Suffice it to say that there is very little of the information that was presented in a semi-factual character to you that we accept. We take exception to almost all of their major points. Specifically, we do not believe --

PROFESSOR KERR: Excuse me. Who is "we," you, USGS or NRC?

DR. BRABB: I'm speaking solely for the USGS at this stage.

PROFESSOR KERR: Thank you.

DR. BRABB: We do not agree that the origin of the features in the trenches is landsliding. We do not agree that the Verona is only eight kilometers long and that it doesn't have to be considered in conjunction with other faults.

We do not believe that there is no offset younger than 8000 years.

We don't think it has been established that there is no offsets beneath GETR. We don't accept the average rates,

agb43

we don't accept the one meter of offset as being a conservative value.

PROFESSOR KERR: One could almost get the impression that you disagree with them.

(Laughter.)

DR. BRABB: Yes, sir.

I've tried hard to understand the landslide idea. I went over to the consultants to look at their data. In the early stages, they talked about an ampitheater that could be seen only on infrared photography. And I studied that photography with them and we talked over the origin of the features that they saw, and we still disagree.

This is a picture of the GETR site, and at one time, we debated considerably where these particular features could be seen on the chart, because at that time all that could be seen was on aerial photographs, according to them.

I could not see this feature. And particularly for me, a landslide has to have what I call a movement pattern, I have to be able to envision it in three dimensions: I have to be able to envision it moving down the hill, I have to be able to envision it back in its original place, and I cannot do that with this feature.

Finally, we had a feature in the ground that we could investigate and determine what the facts were.

(Chart.)

agb44

This is the feature shown on their map in blue -- Did they pass out the copies of this? I think you have a colored copy of this in the material you were just handed. That's enlarged of this particular area. It should be exactly this map (indicating).

This is the map prepared by the consultants in their reports, all I have done is to color it in a special way.

The feature that we're talking about is the blue one shown here. At one time, that was envisioned as a head wall scarp. That has been trenched in three places, and no significant movement for large scale dislocation such as might be associated with the landslide that they envision was encountered there.

Furthermore, this crescent-shaped ampitheater that they talked about is common in the area. And it's clear from a study of the topography that it has nothing to do with landsliding, at least I haven't been able to determine any relation.

If you take a line along the crest of the ridges, as I've done with these black squiggles that you see on here, there are several places that have a concentric shape to them.

PROFESSOR KERR: May I suggest that in our discussions of this, that we avoid when possible it is clear or it is obvious. It seems to me if there is one thing that

agb45

is clear, it is that a number of professionals have been involved in this and that they have reached different conclusions. And surely it is possible for professionals to reach different conclusions, can't we?

DR. BRABB: Let me try a different way.

If you look at these three nested ampitheaters, for example, there is no suggestion in the terrain that there is any landsliding associated with them.

Similarly, if you look at the topography in the vicinity of the other features, there's no suggestion of landsliding. Moreover, no one has mapped landsliding in those areas, so there is no disagreement that there is no landsliding. Therefore, concentric shapes by themselves are not an indication of landsliding.

9.140

And the particular concentric shape that was associated with this feature has been trenched in three places, and they admit that there is no movement there.

The other feature that was talked about early was the attitude of bedding. The assertion was made that the bedding within the landslide area was more disrupted than the bedding outside the landslide area. And today several points were made with respect to bedding, the direction of bedding, the amount of the dip and so on.

(Chart.)

During one of our field trips, we had a chance

agb46

to evaluate the bedding along with the consultants. This is this map which is in your folio and, again, this is the map that was prepared by the consultants. For your orientation, the GETR facility is right here.

The first attitude that we looked at together was approximately 90 degrees perpendicular to the regional strike. The regional strike-and-dip, if you'll remember, is in a northwesterly direction. The regional dip is to the northeast. This particular attitude was approximately 90 degrees to that.

The consultant admitted that the attitude was improperly plotted as being in the Livermore gravels, and it more likely represents an original dip in the very young materials along the creek.

The next two attitudes, shown in the green dots, were places where we agreed that this was indeed a good attitude. In both of these places, the attitude is parallel to the regional strike-and-dip.

The next four places, the attitudes could not be found. We don't know the explanation for that, other than this again is an area of disagreement.

Therefore, in the seven attitudes that were seen along the creek, there were only two that we ag. on, and both of these were parallel to the regional strike-and-dip.

The other problem that bothers me with the

agb47

landsliding is this matter of timing. Basically they want to start this landslide with its maximum movement some time prior to 40,000 years ago. And yet the features that we see in the trenches show recurrent movements of about the same amount extending into the modern period, Holocene.

It's very difficult for me to conceive of how a feature like that could form -- mostly removed by erosion and still be operative as a process into the present time, I can't conceive how that can be done.

(Chart.)

Perhaps we could admit that landsliding is a reasonable hypothesis if it weren't for another feature. I have colored in orange here the middle conglomerate unit they have talked about.

Notice that as you come around GETR, to the south of GETR -- the reactor is located right here -- they show on their map several fold axes and clear indications of bedding extending in this direction.

This purple line on their map is a place where they think that there are conglomerate beds with more or less continuous aspect to them.

If you'll take the stratigraphic thickness of the distance between this middle conglomerate and approximately where this conglomerate projects to this point, it's somewhere on the order of 4000 feet. But when you get over here,

agb48

it's only somewhere on the order of 500 feet.

Therefore, there are two anomalies to explain. One of them is that there's a tremendous amount of section, geologic section missing here. The other is that there's a 90-degree discordance in the strike of the beds. For us, that still has to be explained.

And the Williams Fault, which they project up here in this light blue line, doesn't explain it. There is no other candidate to explain it. For us, it is a part of the Verona Fault system, very close to its juxtaposition with the Las Positas Fault.

Now we thought we were going to have to defend the Las Positas Fault because, up until now, we had differed from the consultants in the interpretation of the exposure near the Sandia facility on the Las Positas Fault. But today they recognize that that indeed is due to tectonic forces.

I'm a little surprised that they admitted compression in this area to form the folds that they talked about over here because, up to now, they had talked about the structural regime as being entirely tensional.

We feel there is evidence for lateral movement on the Las Positas Fault, that the Verona and Las Positas Fault systems must be considered together and, therefore, that the eight kilometer length that they have is not appropriate.

MR. THOMPSON: May I ask at this point how you

agb49

explain the disappearance of the conglomerate beds in the northwest?

DR. BRABB: Up here?

MR. THOMPSON: No, the purple beds you have ending in both directions. You have an explanation of the Las Positas Fault for the southeast. Do you have an explanation for the other end?

DR. BRABB: This is the consultants' map, but --

MR. THOMPSON: I'm asking you.

DR. BRABB: I've accepted, George, what they have on the map. I have no explanation other than I presume it could be a stratigraphic lensing out of the conglomerates in this direction. There is no evidence of any structure in here.

MR. THOMPSON: Thank you.

DR. OKRENT: Can I ask, is it important whether or not the Verona and the other fault --

DR. BRABB: Las Positas.

DR. OKRENT: Las Positas, whether these two are connected or not with regard to your overall conclusions?

DR. BRABB: Yes.

DR. OKRENT: Could you tell me how it affects them?

DR. BRABB: We're not sure exactly because there are very few analogies in the world. But apparently there are

1462 257

agb50

some.

DR. OKRENT: No, how would your conclusions be different: (a), if you think these two are connected; (b), if the Verona Fault is short with regard to your overall recommendations for the GETR site.

DR. BRABB: The length of the fault is very commonly taken as an indication of earthquake magnitude. Therefore, if the fault is much larger, the expected earthquakes might be much larger and, correspondingly, the amount of ground breakage beneath GETR might be much larger.

DR. OKRENT: So it would affect your possible consideration of the ground breakage under GETR, but not the seismic shaking?

DR. BRABB: Both.

DR. OKRENT: But does the Calaveras still dominate?

DR. BRABB: We're not sure. But I think there is at least the possibility that, with this longer fault system, that the amount of shaking from that fault system would be greater than the plant would experience from the Calaveras at a greater distance.

MR. MAXWELL: May I ask a question, please?

If you have the Las Positas as primarily a strike-slip fault and it passes into a thrust fault, then the length of fault really is not a simple matter.

agb51

DR. BRABB: That's correct. It's a very complicated, difficult problem.

And, as you know, our function in this is largely as a review committee, if you will, to advise the Nuclear Regulatory Commission on our view of this. We have not investigated many of the critical factors with relation to the faulting, simply because that hasn't been our role to play on this. We don't know how complicated it makes it, all we're saying is that there is an element there that we don't feel has been adequately addressed.

MR. MAXWELL: Thank you.

DR. OKRENT: Let's see. When you say that the evidence to date, with regard to the connection or not of these two faults, is no better or better than the evidence that was available to the USGS at Diablo Canyon where there was a question of how many faults in a row could be interconnected -- and I think they took a somewhat intermediate position, not the longest interconnection that one could possibly.....

MR. DEVINE: I'm Jim Devine, USGS. Are you referring to San Onofre's connection of the offshore zone of deformation?

DR. OKRENT: No, at the moment, I'm thinking of Diablo Canyon.

MR. DEVINE: Because the geologic evidence for the

agb52

Hosgri being a continuous structure was pretty obvious. I'm not sure I gather the analogy you're drawing with Diablo.

DR. OKRENT: Well there there was a question to how the overall length might relate to the earthquake.

MR. DEVINE: The San Gregorio, the San Simeon, et cetera.

DR. OKRENT: Yes. And you feel you're in a better position or a worse position here, or what is the information situation?

MR. DEVINE: My own estimate -- and speaking only for myself because I've not talked with the others on it -- is that we have had greater confidence at Diablo on how they did or did not connect than we have here. There is a gap here where there is no data.

MR. JACKSON: I'd like to add to that. The problem of the connection of the Verona with the Las Positas has been one of the most difficult aspects of this.

Trench A was put in in an attempt to cross a connection. The connection of a thrust with a strike-slip fault, if that indeed is the true model here, is probably more likely an en echelon fault set. In other words, you go in separate steps and pieces, and it would not be one continuous fault line. And I think that that is a really important concept to have in mind.

PROFESSOR KERR: You left me hanging. What's

1462 260

agb53

important about it, that it's worse or better or --

MR. JACKSON: It's extremely difficult without bulldozing the whole hillside to find all the splays.

Now in one area which was selected based on a tunnel log, probably the largest single fault exposure was discovered in that, which had not been mapped previously at that location where we had estimated. More trenches in the same location may show more faults in a step-like sequence, that's the only point I'm trying to make.

PROFESSOR KERR: Thank you.

DR. OKRENT: But if I could continue, if I remember correctly, it was the en echelon argument that led USGS to decide that they didn't have to link all the possible faults at Diablo.

MR. DEVINE: That, combined with the fact that you were getting farther and farther from the site. It became more and more academic as to whether you needed it.

So the question was largely subdued because of the fact that it was not critical that we have the entire ground, the only critical aspect was whether it then became actually part of the San Andreas system through the San Gregorio.

9 10

So it was aimed not as strictly determining the earthquake magnitude-fault length relationship as much as to its inherent relationship with the San Andreas itself.

agb54

Here I think it is important that there be an understanding as to how the two must be considered together or separately, because it greatly impacts the estimate of the magnitude of the earthquake that you would put on the Verona. By itself, obviously, the Verona is only one-third as long as it would be when you tie it to the Las Positas.

So it's more important here for fault length magnitude estimates than it was at Diablo.

MR. PHILBRICK: When you compare the Las Positas, when we saw it yesterday afternoon, with what you have drawn at the site, I don't find a comparison at all.

MR. DEVINE: Comparison of what?

MR. PHILBRICK: Comparison in deformation. The result of the motion on the Las Positas site over there by Sandia is entirely different than what you've got showing in the sections. I don't see how you tie them together at all.

MR. JACKSON: If I might comment on that. Trench A we did not see yesterday, nor did we discuss at any great length, but Trench A is probably the most complicated fault exposure in this whole study and you can interpret it pretty much to support any hypothesis you'd like.

MR. PHILBRICK: Let me ask you this, if Trench A is comparable to what we saw yesterday afternoon, then the difference lies between Trench A and what you had in B and

agb55

those at the site. Because what you had at the site, so far as the picture were concerned, are simple shears. What you had at Las Positas over there at Sandia was a mess. It was all broken up.

MR. JACKSON: Why don't we discuss that when Dr. Herd gets up. If you could hold it, he's going to talk about the Las Positas to some length.

MR. PHILBRICK: My only concern is can you tie these together rationally, do you have the information to tie them together or don't you?

PROFESSOR KERR: My impression was the point you were trying to make was that you don't have enough information to know whether they should be tied together or not and, hence, in order to be conservative, you have to assume that they are tied together.

Now did I miss the point? I'm not trying to put words in your mouth.

DR. BRABB: I think that's a fair statement.

MR. MAXWELL: I think there's another point that's very important, and that is -- this is a question on my part whether the fact that you tie a strike-slip fault to a thrust fault brings you into the realm of more continuity or not, because you're simply taking up the movement on the thrust fault -- of the strike-slip fault on the thrust fault, and so probably the length of the fault is not enough.

agb56

MR. DEVINE: Yes, to refer to t only as fault length-earthquake magnitude relationship is extremely naive, and it's obviously much more than that.

PROFESSOR KERR: Now you're confusing me even more, which is not hard to do in this area.

I think you said that it was important to decide whether to connect them or not, because that determined this magnitude of earthquake that you're predicting.

MR. DEVINE: It is important in the overall aspect of the earthquake estimate, but that is not defined simply by fault length versus earthquake magnitude, that was my reference.

For example, our final lines suggest the similarity of this combined feature with San Fernando, 1971. If indeed the Verona is an independent feature of a different birth and genesis than Las Positas, that analogy is reduced considerably.

Consequently, whether you just define it as fault length or the overall picture of a complicated thrust-strike-slip system as opposed to -- of some 24 miles long as opposed to a thrust feature of eight miles long, it's a very complicated relationship and clearly does not fit the possibility of an associated with San Fernando, if we talk only of VErona by itself.

And that's our punch line with reference to the

ebl

San Fernando event of 1971, and the similarities of the thrust type features there.

DR. OKRENT: If I think for a moment about the seismic shaking and not the surface displacement question, it's my impression that the Calaveras is a much more active fault, that you would expect a large earthquake with a substantially higher frequency there than on the Las Positas, the Verona.

MR. DEVINE: I agree fully.

DR. OKRENT: Since that's the case and if I understand correctly, the factor may be 100 or something like this in probability, and since there is a tenuous connection in any event between the Verona and Las Positas, it seems to me that one's judgment with regard to the seismic shaking is on a firmer basis if I can -- less shakey --

(Laughter.)

-- if you relate it to the Calaveras.

So that was the reason at least earlier I was asking if that wasn't the dominating fault with regard to seismic shaking, and that the other one wasn't likely to be an important thing in that consideration.

MR. JACKSON: There are two items I would like to comment on on that issue.

The magnitude estimated for the Calaveras is magnitude 7 to 7.5. The magnitude estimate based on all the

eb2

people who have worked on this, the seismologists, is something between 6 and 6.5.

Now the USGS does not like to give the numbers usually but there's a very important point to point out here:

The Calaveras is a strike-slip fault and it can be argued whether it has some oblique movement or not.

The Verona Fault is a thrust fault.

The GETR sits either right on the toe of it or in very close proximity to this shear zone. The behavior and the ground motion content from a thrust fault is very different from the ground motion from a strike-slip fault, and I think Jim will comment on this if we can get through with Earl and Darrell.

MR. DEVINE: I will hold off until it is my turn.

DR. BRABB: I really don't have too much more other than to point out that there's a problem on the north end as well. We don't think that Trench E shows that the Verona Fault is limited to the north. For one thing, in terms of the interpretation of the geophysics, you're dealing with materials of a similar character on opposite sides of this fault so it's very difficult to interpret what may have happened at depth.

Also the geomorphic features in the vicinity of Trench E that were discovered late after the Trench E was opened suggest that because of the sinuous nature of this

eb3

thrust that there may be a component of it just to the west of Trench E.

So we don't think that Trench E limits the Verona Fault to the north. Moreoever, if you take the projection that we originally made, it is right on line with something that's mapped by the California Division of Mines and Geology as a possible active fault zone, the Pleasanton Fault. It's been talked about previously.

The consultants disagree and have picked out a particular sentence of Judd Hall and Associates who made the study in this area and said that they cannot conclusively prove that there's faulting there. Nevertheless, they do show on their map a fault shown by this dashed orange line in here, and they talk about such things as offset streams, photolineation, seismic anomalies, and offset water table,so there is some indication of faulting in here.

As you go to the north there are other indications to the extent that the California Division of Mines and Geology put it in their Alchas Priella Zone. The boundary on their map ends at the quadrangle boundary and the zone ends there but nevertheless, that is right along the production of the Verona Fault.

Therefore, there is independent observations that there is faulting in this area and we think that that may well be the extension of the Verona Fault. Therefore,

eb4

regardless of the issue of how it connects to the Las Positas Fault and the issue of what the addition of a fault in that direction would mean, we think there's a problem to the north as well.

Moreoever, since this fault has a strike that is --

PROF. KERR: I'm sorry, you think there is or that there could be?

DR. BRABB: We think there could be. We think it has not been adequately investigated.

MR. PHILBRICK: What's the sense of motion of that northern fault?

MR. HERD: The Pleasanton Fault is known from groundwater differences and also is visible on old aerial photography of the area around Camp Parks which is just north of the area of this picture. The apparent displacements are vertical along the fault zone although its orientation and en enchelon character would suggest that it has a strike-slip character like the Calaveras which it parallels to the east approximately a half kilometer.

MR. PHILBRICK: If it is strike-slip, what's the sense of motion?

MR. HERD: If the Pleasanton Fault is a strike-slip fault?

MR. PHILBRICK: Yes.

MR. HERD: Well, then the block west of it is

eb5

moving northwesterly relative to the block to the east.

In other words, Earl is asking, and what we're not certain of is, is it not possible to change from a strike-slip character along the Las Positas Fault Zone into a thrust as we turn more perpendicular to the principal compressive stress direction back to one which is more strike-slip in character as we turn back to the northwest vector in line with and parallel with the Calaveras Fault Zone.

DR. BRABB: One other area of disagreement pertains to the age of youngest fault movement in the trenches. The consultants have admitted that it might be as young as 8,000 years. We see no basis for distinguishing between the bottom part of the soil that they are calling 8,000 years old and the modern soil. In other words, we think this is part of a continuous monitoring soil development and therefore, the faulting may be considerably younger than 8,000 years.

This was true not only in the trenches that we discussed today but it was true in all the trenches that we examined at the base of the hill where faulting was observed. Especially in Trench A, it's not shown, in our opinion, correctly on the cross-sections, also in Trench B-1 and the unlabeled trench that was dug just southeast of P-1.

Therefore, we think that the fault movement and therefore all the probabilistic figures that relate to it are using a number that is not correct. We think that the faulting

eb6

is younger than that.

DR. OKRENT: Can I ask a question that relates to this?

If I recall correctly you indicated an uncertainty as to whether or not there might not be faulting directly under the reactor containment. Could you tell me more about why you don't find the existing evidence to the depth that it has been taken convincing?

DR. BRABB: Sure. I'll let Darrell Herd answer that for me.

MR. HERD: May I defer that as part of my larger presentation?

DR. OKRENT: Sure.

MR. HERD: Thank you.

I'd like a minute to get my slides here.

(Pause.)

MR. JACKSON: I've just made a rough estimate of how long we need. It seems like Dr. Herd would like 20 to 25 minutes, Dr. Slemmons about 15 minutes, and then Jim Devine and I will finish up in the following 15 minutes. That's about 55 minutes from now, to give you a rough estimate, depending on interruptions and questions.

PROF. KERR: We have to be out of the room at 6:30, so that should give us some margin.

MR. JACKSON: Okay, fine. We'll try to adhere to

eb7

that.

MR. HERD: I am Darrell Herd of the U. S. Geological Survey, and I will try to provide an overview of the geologic setting of Livermore Valley, specifically addressing ourselves to the Verona Fault, the evidence for it, just briefly touching on some of the things Earl has mentioned, --

(Chart.)

-- and then discuss the Las Positas Fault Zone, the evidence for it, its continuation, its importance, with its relationship to the Verona Fault, and then let the rest of the Staff continue with the various aspects of the rest of the survey discussion.

Specifically we have heard presented today a number of differing interpretations of the geology than you see represented in your colored geologic map which represents the Geological Survey's interpretation of it, including the depiction of the Verona Fault as a thrust fault with its continuation and link-up, more or less, with a minor gap with the Las Positas Fault Zone.

The Verona Fault, as was reviewed this morning, was identified first because of the recognition that there's a high-standing set of hills of Livermore gravel, a section of which appears to stand and tower several hundred feet above the rest of the Livermore gravel to the southwest.

You have heard discussions as to the fact that

eb8

Middle Conglomerate has been demonstrated to exist at the front of this hill front and that it wraps around and climbs upwards to the east and that relative to this Middle Conglomerate and to eastward dipping beds in the back of this hill which, as far as we know, are unbroken and were so reported by the consultants, that there is a dramatic thinning of sections through a very short distance between Trench T-1 here and Trench A which is at this point.

Specifically the approach that the USGS and the Nuclear Regulatory Commission asked the consultants to pursue was to, one, prove or disprove the existence of the Verona Fault by trenching it and two, provide documentation for the landslide by exposures because up until that time we had only circumstantial evidence for landslide.

You have heard Earl discuss the geologic evidence that was provided by the consultants for this large arcuate, bowl-shaped amphitheater from which a large landslide was supposed to have descended to explain the thrust faulting seen in Trenches T-1 and T-2.

(Chart.)

But we haven't discussed the fact as to why there are other shears here, addressed your question as to how does that relate to to the Las Positas in terms of width of faulting and timing of faulting.

The trenches that were excavated in the head wall

eb9

scarp indeed did not find any pull-away structures or any shear planes which would be coincident and fit a landslide explanation for the thrust faulting tha: was seen at the front.

And note, we have not only the principal main zone and you will look at the consultants' log and you will see that the thrust faulting at the base of the main hill front is quite extensive and very gouged like many, many plains so that it isn't a single plain. We have encountered a second fault plain in B-2 and then one which we haven't even heard discussion of today, Trench H, which is outboard of all faults of the same age, Holocene, faults which have the same character, thrust, which dip northeast. Okay.

In the area of the head wall scarp, the consultants have provided us cross-sections of landslides which are supposed to originate somewhere in this area to the south of the presumed head wall scarp of this landslide, yet when you examine the cross-sections they provided, the strikes of these faults are not coincident with -- are not aligned with a shear surface which would fit a landslide with a bowl facing in this direction.

In fact those faults, if I remember correctly, fit better landsliding into adjacent drainages which are perpendicular to the hill front rather than parallel to the hill front.

So we have faults that don't fit the thrust

eb10

faulting we see out to the front and as Earl has told you, there is faulting which is of different age in this area, whatever orientation you accept for those faults. We heard reported this morning that these faults do not offset Holocene soils, yet in the front trenches, B-1, B-3, T-1, T-2, the B-2 and the H trenches, we have seen evidence of Holocene displacement.

And this displacement is not of one age in the front; it is repeated ages of offset because there is differential progressive offset of the older units.

Okay.

So we are left then with a high-standing series of Livermore gravel which do not seem to be landslid yet which are bounded by a thrust fault which most readily explains the rapid thinning of the section as we approach the southeast. Okay.

We asked that Trench A be placed here to find a continuation of the Verona Fault. We said that the Las Positas Fault is here, and that if there was a connection that fault, the Verona Fault, would have to intersect the Las Positas someplace, as has been reported, between the top of the Middle Conglomerate and the base of these continuous, unbroken northeast-dipping Livermore gravel sections.

Trench A was dug there and indeed we did find extensive thrust faulting, plus strike-slip faulting. If

eb11

you'll notice the trench logs carefully in subsequent hours you'll find that there is a considerable thrust component which is generally ignored in discussions of that trench.

(Chart.)

We would point out that a discovery of both thrust as well as strike-slip character here would fit very well the geologic evidence. We talked about the fact that we've got rapid thinning and as a thrust fault would turn toward an intersection with the Las Positas Fault, we would see a change from a strike-slip character to one of more -- excuse me, a thrust character to one of more of a strike-slip character but we would have components of both. And that's indeed what we do see in the trench, in Trench A, that is, strike-slip as well as thrust.

Now if we can, I would like to discuss point by point about the Las Positas Fault Zone and what evidence for it is, and how it relates and why we do not believe that there has been evidence presented to demonstrate that the fault zone does not exist.

I would like to do this, if I can, by bouncing awkwardly back and forth between my geologic map for reference to aerial photographs which we have had flown which will show up points of geology.

(Chart.)

#5 ebl

9.680

First things first. In the first report of 1978 the General Electric position was that Livermore Valley is a valley in extension and that there were only northwest trending faults in the valley site, that there were no northeast trending faults, which is contrary to my mapping which shows a northeast trending fault and no northwest trending faults through the area in the valley. Okay.

Since that time the General Electric position has changed from one of extension to now one of compression.

PROF. KERR: Excuse me. You said "my mapping." You had done mapping before this or --

DR. HERD: I am the source of the original geologic map in here in 1977.

PROF. KERR: When did you do the work that led you to conclude that there was a fault there?

DR. HERD: The work was carried on in the years '75 through '77 and published in August of 1977, almost a month following the initiation, as I understand it, of that GETR license review.

PROF. KERR: Thank you.

DR. HERD: The Livermore gravel section is exposed to the South in a line of hills which end abruptly in North Basin escarpments which are also associated with escarpments in terraces which are broken by the fault zone. There apparently is now no contention about the existence of the Las Positas

eb2

Zone at Lawrence Livermore Laboratories.

Those of you who have visited the outcrop know that there is a fault in the terrace exposed along Arroyo Seco where we see a number of parallel to subparallel faults northeast trending which are vertical and are part of the zone which was mapped here in 1977.

According to the consultants' map, this fault is limited by the northwest trending faults which preclude its continuation to the southwest.

If I recall correctly, and you can look at their report, they show also a number of northwest trending faults through the -- even the trace of the Las Positas northeast of Arroyo Mocho in this area next to the Laboratory. These northwest trending faults have been searched for by Lawrence Livermore Laboratories during the last several weeks to the extent of a million-dollar-plus trenching excavation and no evidence of these northwest trending faults has been found in this area.

What has been found is the Las Positas Fault Zone, confirmation of its existence.

Now there is supposed to be a northwest trending fault, an Arroyo Mocho Fault, which comes through this area of the drainage of the alluvial plain next to the saddle of Arroyo Mocho. In fact what you see there, rather than a northwest trending fault, is a rather spectacular groundwater barrier

eb3

associated with the Las Positas Fault, if I can find it here quickly.

(Chart.)

This is adjacent to Arroyo Mocho where we're supposed to have a northwest trending fault. We have a spectacular northeast trending groundwater barrier in alluvium of Latest Pleistocene age overlooking to the south --

PROF. KERR: Dr. Herd, what I'm seeing is a spectacular blur.

DR. HERD: It's in your handout.

MR. JACKSON: All the photos are in the handout but I'm afraid that the overhead lights have been glaring on this.

DR. HERD: This is the area of the Wente Brothers Vineyard just north of Tesla Road. We are looking to the south and the Las Positas Fault Zone here apparently impounds northward flowing groundwater along a sharp, but no scarp associated with it, break which traps the water on the south side of the fault.

Okay, so we have physical evidence for the existence of the fault at a point where it's supposed to be truncated by a northwest trending fault, the Arroyo Mocho Fault, for which we see no surficial evidence.

Okay, the next point. The fault, the Las Positas Fault, is not supposed to continue southwest beyond Arroyo Mocho. In fact you find northward shallow-dipping Livermore

eb4

gravels here which have dips of ten to eight degrees to the north which are truncated rather spectacularly, I think it is in this side, and a north-facing escarpment which is aligned with and on strike with the Las Positas Fault Zone to the northeast. We're looking at this one here from the north facing to the south.

This is the very spot where the Livermore Fault is supposed to cross the Las Positas Fault, violating its possible existence. In fact, the evidence for a Livermore Fault, if you would, is based on points of evidence which are on either side of the Las Positas Fault and this escarpment we see.

The Livermore Fault is supposedly inferred from three points rather, a fault which is supposedly exposed in Oak Knoll in Livermore gravel here, a groundwater level difference of more than 100 feet north of Livermore, and then the next point of evidence is a piece of data provided by the Department of Water Resources relative to Del Valle Dam which is south of this escarpment.

A line has been drawn to connect these three widely scattered pieces of evidence to draw a fault which precludes the existence of it, the existence of the Las Positas.

There is one, no surficial outcrop of any fault through this area. The groundwater difference can be explained

eb5

by channeling because there are a number of channels in this sector and there is no address made, so far as I'm aware, or have been aware by any of the other parties, as to the explanation for the north termination of the Livermore section here abruptly in this escarpment.

I propose that the logical explanation is that it simply is the continuation of the Las Positas Fault to the southwest.

The next point. Supposedly the area of the GETR that is to the east of it has no evidence for the Las Positas Fault Zone yet if you will examine the mapping of the consultants versus that which I provided in 1977 you will find that there is practically no difference between their map pattern of the contract between Cierbo and Livermore in this section, and it's interpreted by them as an onlap uncon-

785

formity.

Yet when you visit the area-- And I'm afraid that there is no reproduced copy of this, but I would like to show it and we can certainly provide you with one should the desire exist on your part.

(Chart.)

Okay, we're looking now at an aerial view to the northwest of along the strike of the Las Positas Fault Zone and this is the very area of the gully that we were shown as an irregular course through which the Las Positas Fault

eb6

Zone was questioned to pass.

You will notice that in the distance is the escarpment of the Livermore gravel which appears to be terminated by the Las Positas Fault Zone. This contact between the Cierbo and the Livermore is along this valley and this is where we put the Las Positas Fault, not over here in the area of Trench A where it has been sometimes inferred to have been looked for, but it certainly never was examined by trenching along this line.

You will notice that there is quite a physical elevational drop across this contact between the edge of the Cierbo and the Livermore. If this was an onlap unconformity how can it be explained as it being such a severe and really, if you'll examine it, a more lineal course through this area than simply as an onlap unconformity.

Within attitudes of the section we have beds of Cierbo which are striking into this area and are not continued in attitudes on this side of the fault or the contact in whatever fashion.

(Chart.)

As a result, as far as I'm aware, no conclusive evidence has been presented through outcrop or trenching to preclude the existence of the Las Positas Fault here, nor any explanation for the termination of the gravels which fit very nicely with the picture of the Las Positas Fault coming

eb7

across in this fashion, that is, extending from the Greenville Fault Zone to the northeast, ending to the southwest near San Antonio Reservoir. Okay.

If the Las Positas Fault is a continuous strike-slip fault its termination or connection with a thrust fault at its west end is not unreasonable in that motion along the north side of the fault is with the block moving to the west accompanying or not necessarily being similarly timed with thrust-faulting events which occur along the Verona Fault Zone. A turn, as we say, of this Verona Fault into the Las Posita at this point is not unreasonable.

Okay.

MR. PHILBRICK: How do you take a horizontal motion and take it into a vertical motion? You have to have a change somewhere between the two.

DR. BRABB: That in fact is what Trench A tells us because we're seeing a fault which is trending almost -- it's what? North 65 to 70 degrees west, which is this orientation, not this northerly orientation. And that's a good point I would like to go back to in terms of the Williams Fault which I forgot to mention.

In changing from a strike-slip to a pure thrust you would expect components of strike-slip, and in the trench log there is a strike-slip component as well as a thrust component onlapping with the section thrown out over the block

eb8

on the west side.

As far as I'm aware there is no way that one can understand or determine a sense of up or down across that trench in that that is simply a swale of accumulation of Livermore -- of colluvium and there is no continuity or match of any of the section in Livermore across the contact.

MR. PHILBRICK: So you have a fault there. Is the fault at right angle to the Las Positas or --

MR. HERD: It's a fault that trends north 70 degrees west, which is the trend that we have shown here.

DR. MARK: I wanted to ask I think the same question. Is it not possible that Las Positas just keeps on going down what I guess might be about Niles Canyon there and that the Verona simply comes up and intersects it in the same way Las Positas in the picture merely intersects whatever it is called, the Greenville Fault?

MR. HERD: Indeed that's possible. In fact, that must be true because we can find evidence for the continuation of the Las Positas Fault beyond its point of intersection with the Verona. It is not the termination of the Las Positas and I hope I haven't miscast that.

DR. MARK: Does it make a difference if two faults merely cross or if, as you were saying earlier, they are one continuous something-or-other. You know, they are quite discontinuous in their nature.

eb9 c 10

MR. HERD: For the calculation of the earthquake at tha site, we have to consider the area of surface on the fault. A thrust fault, because of its low angle dip, of course has a greater surface area for motion.

DR. MARK: You mean Livermore Labs can be stuck with -- I keep forgetting the name of that -- they'll have to add the Greenville to the Las Positas and ask how they made out?

MR. HERD: No, I'm sorry for leading you on in that direction. The Greenville Fault terminates at the northeast end of the Las Positas Fault Zone. As far as I know, they have no motion which is compatible for sympathetic or simultaneous displacement although I guess there is some evidence for faults of conjugate character and moving simultaneously in earthquakes in China, I believe, in terms of the interpretation of the seismic record, but I do not know well enough personally to comment on that.

But what can and must be addressed here is a fault that, as has been pointed out, if it is a strike-slip fault which turns into a thrust fault through a change in dip and strike, then how do you differentiate and separate one from the other in terms of the determination of the fault length for the calculation of the earthquake and how do the two behave independently or together.

That's the question, the change in the continuity

eb10

of ~~ strike-slip into a thrust motion in the area of the GETR.

MR. PHILBRICK: Without breaking hell out of it at the joint.

MR. HERD: Without breaking hell out of it at the joint.

MR. PHILBRICK: Sure, you can't change the direction of motion without breaking the rocks all to pieces.

MR. HERD: And indeed we find that very thing here. The La Costa tunnel which is referenced on the consultants' map, it's a tunnel that was dug through the area right adjacent to Trench A which gives us a cross-section through the Livermore gravels at this point. And you will notice in their report of 1979 there are a series of -- there's a wide zone of thrust faulting encountered in that trench which allows us to establish that the zone for thrusting and faulting in this area is quite broad and it is not simply confined to the A trench.

In fact, if you look in the area of Trench A relative to the tunnel there are thrusts which actually lie west of an outcrop with projected surface intersection, than the major fault zone which we saw at Trench A.

Can I just digress for one more point?

MR. PHILBRICK: Go ahead.

MR. HERD: Also very important is the explanation

eb11

of this faulting. If we did entertain that this would be the Williams Fault, the continuation of it, we have to violate several geologic facts if we can in this area. The Williams Fault is supposed to have a trend that is primarily north 36 degrees west through this segment. It's supposed to cross through, as far as we know, continuous northeast-trending Livermore gravels which, by the report of the consultants in 1978, was supposedly unbroken. And this fault has a trend which is quite off of that which was seen in the fault exposure in Trench A.

This fault is supposed to trend north 36 degrees west. The faulting we saw in the trench was north 70 degrees west.

So to summarize if I can, this exposure has the type of motion, a combination strike-slip and thrust, and a direction of plunge as well as outcrop strike which is consistent not with the Williams Fault but with a Verona Fault which is changing in character from a thrust at this point to one of strike-slip as it approaches Las Positas.

MR. PHILBRICK: Do you find any more motion there at the junction point than you do down there at the GETR?

MR. HERD: Do we find more motion down there?

MR. PHILBRICK: Do you find more motion at the junction than you do at GETR?

MR. HERD: I don't believe we can even assess that

eb12

from the data we have at hand. The section at Trench A does not match across it. There is similar age of offset; that is, the soils show a motion which is Holocene, just like we see in the main zone of faulting as well as the ones outboard of it to the front.

So there is no way, so far as I'm aware, to evaluate the amount of slip here versus what we could calculate from the available exposures at this point. Certainly, as I say, there is no continuity in section across the fault at A, and we have no way to compare it to there.

MR. PHILBRICK: Well, at the junction it's all broken up.

MR. HERD: Correct.

MR. PHILBRICK: But GETR is not all broken up.

MR. HERD: It is not all broken up. Come again, will you explain that?

MR. PHILBRICK: Not in the pictures I saw.

MR. HERD: Not in the pictures that you saw.

MR. PHILBRICK: No.

MR. HERD: Will you explain?

MR. PHILBRICK: It has a couple of joints like that but you don't have it all fractured. What you described there at the joint is a fracturing.

MR. HERD: What I am describing at the joint here is, as far as I know, a series of imbricate thrusts or a sort

eb13

of thrust faulting and partial strike-slip faulting, a combination of the two which lies in the area of the La Costa tunnel and Trench A.

At the GETR we see a very large, very extensively sheared zone at the foot of the scarp, and we see --

MR. PHILBRICK: Have you got pictures of what's in that Trench A?

MR. HERD: In Trench A, do I have pictures of it? The consultants showed you a slide of it today. I don't have a picture of it at this point. I would have to refer to their logs.

MR. JACKSON: What you would like to see is a comparison of Trench A photograph with Trench B-1.

MR. HERD: Okay. If I might, could I borrow your picture of Trench A?

May I request the Board that --

PROF. KERR: Don't we have it in the material that was handed out?

MR. HERD: Do you? Okay.

MR. MAXWELL: Darrell, looking at this relationship of Las Positas to the Greenville Fault, the obvious solution is to say the Las Positas is older and has been chopped off by the Greenville and the Las Positas ought to be dead. What can you do to dissuade me of that opinion?

MR. HERD: Okay. Certainly in my report of 1977

eb14

I entertained that possibility that the Las Positas and other faults to the east, the Carnegie and Corral Hollow Faults might once have been continuous, having been separated by right lateral movement along the Greenville Fault Zone.

Well, I have no understanding as to-- Well, there is indication of current activity along both the Greenville and the Las Positas but there is no indication of continuation along the Carnegie to the east.

The evidence for movement along the Greenville is offset of alluvial fans and the like in this area, just to the north of the intersection of the two faults.

I would only propose as a possible explanation -- I certainly don't have a ready valid airtight one -- is that the orientation of a fault in a northeasterly direction which lies at a 60 degree angle to the trend of the Calaveras, which is an active strike-slip fault zone, is a direction which is in mechanical orientation -- it's a mechanical orientation that's permissive of a strike-slip character of motion, which is exactly like which we see in the exposures at Lawrence Livermore in this area, such that perhaps this is an older zone of structural weakness, i.e., an older fault which, because of its orientation in the present stress regime, has now been reactivated or continues to be active in a strike-slip character as opposed to a high angle reverse character which the Carnegie Fault is to the east of the

agbl
flwseb14

Greenville Fault zone.

MR. MAXWELL: You can see the problem with the junction with the Greenville Fault where the arrows are drawn.

MR. HERD: Indeed, there is some question as to the character of motion along the Greenville Fault at this point, there is evidence for normal faulting in that there are graben-like structures in late-Pleistocene alluvium over in this part and no apparent right lateral jogs in any streams over here to evidence continued right slip motion.

And activity along the Greenville Fault, in terms of morphological expression, dies both to the north toward Mount Diablo and to the south toward the center part of the Diablo Range, such that it could be argued that the motion that we see along the Greenville Fault Zone is just normal, i.e., part of an extensional faulting accompanying movement of this block away from the Greenville Fault. And we have discussed this in meetings before.

MR. JACKSON: I think if you could discuss the items we could move on.

MR. HERD: Okay.

DR. MARK: Could I ask one short question? The flap at Livermore in which they have managed to spend a million dollars in a week or something, they must be forced surely to follow this Las Positas Fault all the way up around

agb2

the corner into the Verona and into the Pleasanton, because that will determine how big an earthquake hits the land.

MR. HERD: That's correct.

DR. MARK: Good. Then they'll work very fast.

(Laughter.)

MR. HERD: It's my understanding that Lawrence Livermore is entertaining the problems of trenching the Las Positas Fault in this sector.

DR. OKRENT: I thought they would remove it.

(Laughter.)

MR. HERD: That would be magic.

DR. OKRENT: Before you leave, I thought while you were standing you were going to tell me about why there might be faulting or have been faulting under GETR. Did you tell me that?

MR. HERD: No, but I will certainly try that right now.

Okay. The GETR site has been trenched at spots shown by the black dots. We have a Trench B-1 and B-2 continuation which was dug on the north side of the GETR. Okay? And there have been no trenches excavated in a similar full length past the GETR on the south side.

Because of the fact that we are seeing discontinuous thrust faults outboard of the main zone, it appears to me that we have no evidence to preclude the existence of a thrust

agb3

fault which would intersect and perhaps even lie beneath the GETR vessel which does not continue to the north side where we would have encountered it in the trench that we had, that there could be a fault to the south and parallel to the front.

In other words, we have only one data point line on the north side to preclude faulting through and beneath the GETR vessel. And that does not, as far as I know, preclude other faults which just do not make it into the area of that trench on that side.

DR. OKRENT: How far is the trench north of the GETR?

MR. HERD: Please correct me from the audience, I think it was about 80 or 90 feet north.

MR. JACKSON: 300 feet in the projection to the nearest perpendicular strike of the fault.

DR. OKRENT: Are you suggesting that there could be a fault that is parallel to the trench and therefore you don't see it? Or it's perpendicular and it doesn't reach it?

MR. HERD: The latter, that it is perpendicular to the trench and doesn't reach it. That is, that it is another shear surface like those we've seen before which is discontinuous in length which is parallel to the hillfront and does not continue in its northwestward projection into the area of Trench B-1 and B-2.

1462 292

agb4

MR. PHILBRICK: Now how long would that fault be?

MR. HERD: The Verona Fault?

MR. PHILBRICK: No, the one you're talking about.

MR. HERD: Oh, under the GETR site if there were one?

MR. PHILBRICK: Yes, if there were one.

MR. HERD: If there were one, it could be limited in terms of length by the point where the middle conglomerate is unbroken to the southeast or to the east and to the trench on the north. So what is that X number of kilometers? I haven't measured it off.

MR. PHILBRICK: Where's the age trench?

MR. HERD: The age trench is next to the plutonium facility which is built against a hill up in the Livermore gravel outboard and in front of the GETR.

MR. PHILBRICK: It's down the hill from the other?

MR. HERD: Correct, it's down the hill. So we have seen three zones of shear -- of thrust faulting nested within each other progressively to the east.

MR. JACKSON: I'd like to make a comment.

To show that even we can disagree when we work together, I don't believe there is good evidence of faulting beneath the GETR reactor itself, based on the observations of

agb5

what we have in the trenches, based on some old photographs of the reactor excavation itself.

I think what led us to postulate the locations for where we requested the trenches initially were air photolineaments out in front of the hillfront. Two out of the three proved out to be false, and the other one a channel fill. And I'm in agreement with that.

The problem, I think, that Darrell -- just to highlight a little bit -- is discussing is that the age of the material beneath the GETR facility is important and could you have other splays under there which could project between the two existing breaks.

Dr. Slemmons, I think, will comment on that a little bit on what he terms sharp-cut faulting.

Dr. Slemmons.

DR. SLEMMONS: I might point out that I came into the study at a very late stage and did not have the opportunity to look at one or two of the earlier trenches but I have seen all the more recently developed trenches in the area.

The conclusions that I presented here were arrived at very slowly because I wanted to make a very objective appraisal of the evidence and consider both sides of evidence of which you have heard presented today.

First of all, I would like to make a few comments

agb6

with regard to the probabilistic approach. I've written a brief report that suggests that I am rather uncomfortable at this stage with the probabilistic approach for the evaluation of seismic risk at this site.

10.190

And I think, although I'm not an expert on probability analysis, particularly the mathematical relationships, I feel much more comfortable with a deterministic type of approach in that it is easier to obtain geological data that you could infer groundmotion and other -- magnitude and other relations from directly.

The problems that I see with the probabilistic approach at this point in time is that I feel that much of the geological data is inadequate for the analysis, and that some of the numbers that have been used have been used with three significant figures when the quality or the verification of the data does not warrant that precision.

Areas that have given me concern are, first of all, the age or timing is a double inference: first of all, it's based on a marine sequence of age as based on the Caribbean work of Shackleton and Opdike using oxygen isotope ratios to correlate changes in sealevel.

This then has been extrapolated to the continental areas. And I don't think the present state of the art allows a high degree of precision, although I think the general approach that was presented earlier by Roy Slemon is a

agb7

reasonable one.

For example, in the area to the east, Roger Morrison, who has been working with these methods in the Lake Bonneville area, finds that his results do not correlate very well with his own work in the Lake Lahonton area, so that here are two places within the Great Basin, rather conflicting results come up, at least when examined in detail.

PROFESSOR KERR: Could I summarize by saying you think the approach is reasonable but it is just wrong?

MR. SLEMMONS: No, I don't think it's wrong, I think it's reasonable, but I don't think it can be carried out with three significant figures.

I think in the analysis, for example, we saw 128,000 years used. And when the final probability was given, there was I think a greater degree of precision than was justified.

DR. OKRENT: But could I ask, do you think 100,000 years is a reasonable number?

MR. SLEMMONS: I think if you round it off in general periods of that sort, it's reasonable.

DR. OKRENT: Do you think the measurements are good enough that you could agree on 100,000 years?

MR. SLEMMONS: I think the thing that makes me uncomfortable is there are no hard dates, there's not a single

agb8

absolute date that has been used.

DR. OKRENT: I'm unable to tell the depth or the extent of your disagreement with -- I don't see any difference in the result they get whether they use 100,000 or 128,000, that's a small difference in the final result.

Now, if your uncertainty is whether it is 10,000 or 128,000, I would like you to say so. If you're r t sure whether it's 100,000 or 128 or 156, I would like you to say that. Because there's a difference between 20 percent and an order of magnitude.

MR. SLEMMONS: I do not believe there's a difference of an order of magnitude, something of 100 to the 128 range would be more reasonable, I believe.

A second factor has to do with, if I could present a section which comes from an experimental study and may not correlate directly.

(Slide.)

This is the work of Friedman and others at Texas A&M which shows some experiments that deal with reverse slip mechanisms. This is determined experimentally in the laboratory and the modeling may not at all approximate in detail the relationships in the Vallecitos area.

But during a sequence of stages of deformation, from tc to an intermediate stage to a more advanced stage, we find that faulting develops first on simple shears and then

agb9

branches develop.

Eventually in these experiments, you can get branches in some cases that extend into the footwall or the lower block, in other cases, short cut relationships are obtained which gives a greater probability depending upon experimental setup of faulting occurring on one side, branch faulting occurring on one side or the other of the earlier trace.

In New Zealand, for example, Lenzen, in a rather limited observation, has found that the renewed faulting usually involves a short cut, so your earlier faults are the ones that are lowest down on the slope.

In rather conflicting relationships Bill Bull and his workers in Southern California have found that the faulting has been first at the base of the range in the case of the Santa Suzannah system and then the branching later has been out on the Piedmont.

Here in the Vallecitos area we seem to have three synchronous zones, that is, three zones that have other similar types of timing. And I don't think that there are basic studies that are available or have been synthesized for this particular study that show which side of your main traces are likely to develop new or branch faults, and this relationship I think should be considered for the two faults to the south of the GETR, particularly the Trench H fault

agb10

zone and the one at B-2 in that short cutting relationships may take place or could take place on those zones and may in a non-linear way affect the potential for rupturing at GETR itself.

This factor then I think is one that should be considered and could use other field and laboratory relationships for the analysis.

In general, these problems reduce my confidence and ability to analytically approach the field relationships or the probability with the known field relationships for this area and for reverse fault mechanisms.

MR. THOMPSON: Before you leave Friedman's experiments, is it not true that he used a cut surface which was an unstable surface so that the fault plane was trying to find a mechanically stable direction?

MR. SLEMMONS: That's correct.

MR. THOMPSON: Whereas with an earthquake, that would not be the way it would originate?

MR. SLEMMONS: That's correct. So the modeling does not accurately display the kind of relationships that might occur at Vallecitos.

MR. THOMPSON: Thank you.

end A

1462 299

PROFESSOR KERR: Please continue.

MR. SLEMMONS: I would like to make a few comments with regard to the fault rupture length and maximum displacement relations.

(Slide.)

I recently completed a compilation that has involved a study of all the available worldwide examples of surface salting, and this relates rather to the Kanemori type of approach to multiple events we find in roughly 20 percent of the examples of historic surface rupturing. In 17 out of 87 examples more than one fault has been activated. This means the magnitude is the summation, actually, of a series of faulting events. It is necessary to consider the possibility of a complex system of branching faults, or separate splays, or even different fault zones rupturing at the same time.

The documentation of the data for reverse slip faults of the type that we have for Vallecitos is limited to only 11 examples, the ones that I've underlined here in red. So we find a very poor data base.

The correlation coefficient is apparently reduced somewhat, and the standard deviations are about .4 or .5 on the magnitude scale. That would mean that the values that you get by using fault lengths to magnitude, or the maximum displacement to magnitude can be off by the order of magnitude of .4 to .5.

wel 2

(Slide.)

The scattering of data is shown here on this diagram, and you can see a rather broad spread of the data points for the regression analysis.

Three approaches have been used by the Staff in order to estimate the magnitude, and I think they come up with reasonable values.

First of all, the fault maximum displacement, we used values between 1 -- or for that fault length, and from the field data you can come up with approximately 1 to 3 meters. That is the most recent offset has a maximum on all three zones between 2 and 3 feet. The previous event, or the previous soil is offset by up to 3 meters. And so this seems to give two examples. The more probable value would be the more recent one in either event. But the possibility of the 3 meters being a single event cannot be precluded.

So this gives us sort of a ballpark figure for this zone of from one to three meters.

The Staff has used the San Fernando earthquake for an analogous relationship, and they're using the 12 to 15 kilometer length, which is crudely similar to the Vallecitos zone, assuming that it extends to either the Pleasonton or to the Calaveras fault zone, and is truncated or terminated by the Los Positas zone. You would come up with values of approximately 1-1/2 or 2 meters -- correction -- you would come up with similar values to the 2.4 meters that was observed in San Fernando.

A second case,which has not been used due to it being new data, would be the example of one recent event that I have uncovered of faulting in Algeria, where a magnitude 6.7 earthquake produced 1 meter of offset. So the general data for the 6.5 magnitude range seems to give values in the range of about 1 to about 2.5 meters.

In general I concur, then, with the Staff position. I think it is warranted on both the correlation of analogous relationships and in other parts of the world. It is compatible with the total worldwide data set, and it is also appropriate for the observations obtained in the trenches at the site.

PROF. KERR: And what you purport to calculate is the largest possible offset, or the offset that will exist 50 percent of the time if you have the largest possible earthquake, or what? I'm not sure . . .

MR. SLEMMONS: I'll let Bob comment on that, but --

PROF. KERR: I thought you were the one who did the calculations.

MR. SLEMMONS: Only in part. Bob also made his analysis, and it's actually his analysis that came up with the 2.5 value. But that is consistent with my data as well.

PROF. KERR: You collected the data, he did the wei 4

calculations of the offset?

MR. SLEMMONS: Yes.

Bob, do you want to comment?

MR. JACKSON: Dr. Slemmons compiled a large number of fault offsets versus magnitude observations that have been made for many years. That's available and in the figure he used there.

I have drafted a figure which shows you available data that is applicable to this site, and I will comment on that.

We did calculate some, and the Branch had some calculation of exceedance probabilities of those maximum offsets, and I will comment on them.

PROF. KERR: I was asking him, because I thought he had done a calculation and had come up with the 2.5 meters. That's not the case?

MR. JACKSON: No. I will explain how we got to the 2.5.

PROF. KERR: Thank you.

MR. JACKSON: Any other questions for Dr. Slemmons?

PROF. KERR: I see none.

Thank you.

MR. JACKSON: I'm Bob Jackson. I'm with the Geosciences Branch at NRC. I had a number of comments about the landslide versus fault origin of these features at the site, but I think you have heard about as much as is available to hear on that issue.

I will restrict my comments, then, to the estimates of the kind of offset that you might have at the site.

It's clear, I think everyone will agree, that there has been in Holocene time, or somewhat older, depending upon interpretation, one meter of offset on three separate splays in close proximity to the GETR site.

The B-1 zone has a shear zone, multiple movements -- I can't remember the exact number, but at least four splays over a 55 foot wide zone. Trench B-2 zone has a fairly clean shear, and I think this is what Dr. Philbrick was referring to. And Trench H has a fairly clean singular fault break, all of which have one meter of movement, of the youngest movement.

There are recurrent movements of the order of about one meter. There have been estimates by USGS in one of the earlier trenches of offset units as much as three meters.

In order to approach how much offset to estimate, which has never been done before for a nuclear facility, is one of the most difficult questions that NRC has wrestled with. In a number of hearings over the past 15 years or so relating to sites such as Bodega Bay, Mendocino, Corral Canyon, a site called Davenport, and several others which don't come into my mind right now, the NRC took the position that you can design for surface faulting.

In every one of those cases the hearing board overruled the Staff and found in favor of considering surface faulting and estimating amounts of offset should be considered, just to give a little historic perspective on it.

DR. OKRENT: Excuse me. I think the only case where the hearing board overruled the staff was at Malibu, Corral Canyon.

MR. JACKSON: Mendocino also.

DR. OKRENT: Thank you. Did Mendocino go to a hearing board?

MR. JACKSON: It was not a hearing board? It was ACRS?

DR. OKRENT: I think it was, but I could be wrong.

MR. JACKSON: I'm sorry. I'm not trying to be specific here. I'm just trying to give a general frame of reference.

It's a difficult issue. The surface faulting issue is difficult.

PROF. KERR: But be careful, by giving a general frame of reference without being specific, that's difficult.

(Laughter.)

MR. JACKSON: Thank you.

One of the problems we always have as geologists and seismologists is no data, and this is no different case.

(Slide.)

Based on data which was assembled by Dr. Slemmons on reverse slip movement fault observations around the wor..., we have the rupture length plotted versus amount of surface offset, and that is maximum surface offset observed during event. That is the data we have which we can go to to draw a line and calculate probabilities on the kind of estimate of offset we might ascertain at this site.

Dr. Bonilla, USGS, plotted a line through that, and it has a reverse slip which tells you that the longer the fault, the less the offset.

PROF. KERR: Excuse me. 2, 4, 6, 7 data points?

MR. JACKSON: Yes.

Now, in order to add to that a little bit, you can now look at reverse -- add to that data set reverse oblique slip, the movements which essentially are perpendicular to the strike of the fault, and then those that have some component of movement parallel to the strike of the fault.

If we add that data set --

DR. POMEROY: May I just ask a quick question here?

Of that original data set, how many of those points are in the western United States and in California?

MR. JACKSON: I'd have to go back to the chart and look at them.

I'm told there is one only in California.

PROF: KERR: I conclude that there has been no

wel 8

earthquake with an offset less than .4 meters. Is that correct?

MR. JACKSON: For a reverse type fault offset movement, and assuming this is the total available data set, that would be correct.

We can add to that reverse oblique slip movement, which broadens the data set a little bit, and those are the triangles. And that gives you a little better data set. I think it's a total of 13 or 14 points now.

I twisted the arm of a seismologist in the Branch to calculate some best fit lines to this data, and some exceedance probabilities. Just for accuracy, if I can do it here, there are two data points which are off the graph . They're well off it. And they do influence this. I don't have them drawn on here.

The calculation of --

(Slide.)

I've compiled these three graphs on one, and we've calculated a 50 percent exceedance probability, a 30 percent exceedance probability, and a 22 percent exceedance probability. I'm not an expert in probability. The gentleman who did this did it at gun point, and the estimate of the San Fernando event, if you knew the total rupture length was going to be 12 kilometers, would have given you a maximum surface offset of 1.68 meters. The actual observed offset over a wide zone

wel 9

of breakage was 2.5 meters.

If you wanted to add an element of conservatism to that, 3.15 meters would be a value you could propose.

Now, I really am doing this to show that your total data set that we're arguing about here, the total maximum offsets we talk about range from 0 to 5 meters. We're talking about not a wide range of offset. We're trying to fine tune our estimate, so that a structural engineer can take that estimate and use it in his design calculations.

During several meetings I guess in the last two years we had many meetings as to how much we should increase the one meter observed offset at the site to account for our uncertainty in the data and our uncertainty in the geologic setting of the region. We decided to go with 2.5 meters, and I think in the text it says something approximately 2.5 meters. We're not trying to be at all precise. We expect it's something approximately in the 2 to 2.5 meter range.

We went to the San Fernando event as an analogy, because it is in California. It is a thrust fault. It is probably the best studied event that we've had in a long time. And it had a lot of offsets observed along the San Fernando.

I'd just like to make -- this is basically the logic we used. We wanted some -- since one meter offsets have occurred at the site, the possibility of three

wel 10

meter offsets during given events had been observed, we accounted for that by going to this approach.

In terms of the location of the surface offsets we asked Dr. Slemmons to look at the -- and the USGS -- to look at the probability approach for the initiation of new rupture underneath the GE test reactor site. We did not ask them to look at the mathematical probabilistic aspects. We're really looking for a geologist's and seismologist's judgment as to what we thought the likelihood of a ruture would be under the site.

At the many meetings and discussions we came to the resolution that although there would be a higher likelihood of movement along existing splays, there is a possibility of rupture between the splays. And based on compilations by USGS individuals and observations even of the recent El Centro earthquake, the location initiation of new ruptures is possible between two existing splays.

I'd like to let Jim -- are there any questions?

DR. MARK: Very simple. You mentioned a 12 kilometer surface fault, or surface crack, to go with the San Fernando. How long is the fault with which that was associated? Was this a 12-kilometer fault, splitting from end to end?

DR. SLEMMONS: That zone is part of the Santa Susanna zone, and Bill Bole of the University of Arizona has just finished some geomorphological offset studies along the front of that range and on the Piedmont faults. The zone is a couple of hundred kilometers in length, but it consists of a number of individual segments that seem to have short rupture lengths. That is,the most recent activity is confined to sections of 5, 10 and 15 kilometers.

So the zone, although very long, appears to be one that's segmented in its activity.

DR. MARK: Here the 12 kilometers is associated with the estimate of the possible length of Verona. Not by everyone. But that number appears. And the idea that a fault should show surface evidence right up to the last penetration of the crack seems a little strange to me.

MR. JACKSON: Indeed it does. And the general accepted rule of thumb used by geologists is approximately one half of the total fault length should be used as the rupture length.

DR. MARK: It actually could be less, but you can't guarantee? Like it would be half. That sounds --

MR. JACKSON: In fact, I think there's new evidence that indicates it could be substantially less.

DR. MARK: The other point was, all those points you had, all seven of them, are those all with earthquakes of approximately the same depth of focus, or is that an important parameter in surface faulting?

MR. JACKSON: They were all shallow events, 30 kilometers or less.

DR. MARK: Thank you.

MR. JACKSON: Jim Devine will now comment.

VOICE: If the epicenter is going to be 30 kilometers how far is that going to be from the GETR?

MR. JACKSON: Well, I'll take this opportunity to comment on Dr. Page's letter to the ACRS of several weeks ago.

There is a great question as to whether you ought to be concerned about the distance you are from the epicenter on a fracture zone, or the distance that one sits from the surface expression of the fracture zone.

The recent earthquake in El Centro gives excellent data which we've recently plotted up, and I don't have with me, which shows a very good relationship of distance from the fault break, rather than a not good relationship between the distance from the epicenter.

So I think a more important consideration is not distance to the epicenter, but the distance from the slip surface to the area of observation.

DR. THOMPSON: The El Centro is a vertical fault. It's a very major difference.

MR. DEVINE: Yes. I would like to try a little on that.

For the first time we've got a rather extensive set of data close in to the fault at Imperial Valley on October 15, and the evidence there is now overwhelming that the peak ground motion is controlled by how far you are from the fault trace, irrespective of where the epicenter was. And that evidence is just overwhelming.

PROF. KERR: You mean in a general sense, or for this one earthquake?

MR. DEVINE: Well, it's obviously for this one earthquake, because that's all the data we have. We have suspected this from other evidence.

PROF. KERR: You feel comfortable in generalizing from one --

MR. DEVINE: Well --

PROF. KERR: I mean, I don't know. I'm asking.

MR. DEVINE: There's been suspicion of this in the past. This is the first time we have real hard evidence. There was possibility that the evidence in the recent Tabaj earthquake in Iran reflected the same thing, but that evidence was equivocal because we were not able to complete an investigation of that. It suggested it.

But in El Centro, in that one earthquake, where we do have an extensive set of data close in, the evidence is just overwhelming that the peak motion is controlled by the distance to the fault trace, not to the epicenter. And that is a vertical fault. But I don't understand why that is so significant as to whether the peak ground motion is controlled by the epicenter versus the fault trace.

DR. THOMPSON: I'm not sure what we're debating here. But surely the elastic energy is released in the hard rock below the surface materials. And so I'm not at all surprised that this would be true at El Centro, because the hard rock lies vertically below the surface trace. And in the case of the thrust surfaces you're talking about here, it's quite a long way down to where you would get into hard rock that would be releasing elastic energy in an earthquake.

So I think the situation might be a little different than El Centro.

MR. DEVINE: But many researchers have attributed at least in part the high peak accelerations at Pacoima Dam from a thrust fault earthquake as the fact that it was only three kilometers above the trace, even though it was 20-some kilometers from the epicenter.

PROF. KERR: I think it's clear that there might be a difference of viewpoint here. You've made a point that I think is important, and I would urge that you continue.

MR. DEFINE: The discussion earlier on the Verona fault and the earthquake magnitude one might speculate for it, I would like to put in perspective quickly. And that is, in our letter to the NRC we indicated we thought there was not sufficient data to make a viable estimate of the magnitude of that earthquake. We don't know its fault length, we don't know its amount of displacement, and we don't know its tie-in with the Las Positas fault sufficiently. So we begged off on making a speculation of the magnitude of the earthquake that that fault could generate. Recognizing it's an important problem, because it is so very close to the site puts us into this near field question even more so than before.

However, we do recognize the possibility of a very large earthquake on the Calaveras fault, and in an effort to answer your question earlier, Dr. Okrent, about the probability of that, I have a couple points I need to make:

One is that, as you know, we've been very reluctant to make probability studies and use them as the basis for estimating maximum credible earthquake. We see very serious problems in that, and our position has not changed entirely on that.

However, as data are collected and our data source gets better, we're gradually easing into more and more reliance on it. But in this case we still do not offer numbers based on probability assessments.

The only estimate that I can determine that's been done by survey of probability events on the Calaveras fault is a study in progress which was handed to me over lunch, which offers the probability of an earthquake in the range of 5.5 to 7.7. That range of earthquake occurring approximately once every 25 years. But now that could be all 5.5's. So I do not have a probability estimate from any of our researchers that would confine it to just, say, the upper end of that range.

But we do believe that it's a very viable estimate for a maximum credible event, of something in the order of 7 to 7.5 on the Calaveras. The probability of it -- the frequency of its occurrence is, in our judgment, highly speculative. But to offer speculation, somewhere between 100 and 1000 year return interval. But that is recognized, I hope, as a highly speculative number, even though we do have a fair amount of earthquake data on the Calaveras fault.

The other point that I need to make references the peak acceleration and the effective acceleration and the acceleration that was used to anchor the broad band response spectrum, say, if you got 160.

1462 315

3c ebl

We have been offering advice to NRC on sites close in to the fault which is commonly called the near field, to limit our advice to discussing only ground motion or at least the judgment on our part that peak acceleration anchored to response spectra are not the same thing in the near field, and it's very, very difficult to get from one to the other, and it's not possible as a seismologist to do so because of the engineering influence that allows you to go from peak ground motion to "effective" -- in quotes -- g value for anchoring response spectra.

I think the recent earthquake in the Imperial Valley only adds to our confidence that this is where we need to stop as a seismologist in describing ground motion from an earthquake. The g values obtained from the Imperial Valley earthquake are very high. Of the nine stations within eleven kilometers of the fault trace, seven of them had peak g values in excess of .5. One of them reached 1.74g.

While it's obvious-- And the subsequent damage in the area, the maximum intensity we would assign to any of the damage in the area is of the order of intensity 8 and even those were in very limited, small pockets.

So it's obvious to us that peak ground motion and da ~~e don't equate very well, and it's to the point where it's not very useful to offer values to the design process, based on that philosophy.

1462 316

eb2

I think it then becomes extremely important to modify the peak ground motions as seismologists would record them and report them by some sort of engineering analysis, as I understand is in progress now by Dr. Newmark. I think it's impossible for us to sit here and speculate on whether .5g is a proper anchor for a response spectrum because of this very large difference we see between peak ground motion and any sort of a reasonable acceleration anchor for the response spectra.

Consequently, we did not do so in our report, even though it was written before the Imperial Valley earthquake.

That concludes my comments.

PROF. KERR: What was the magnitude of Central Valley?

MR. DEVINE: 6.4.

PROF. KERR: Thank you.

Are there questions for Mr. Devine?

DR. OKRENT: Let me try one or two questions, then I'm going to try to make a couple of comments, because I'm going to try to beat the crowd in order to make a plane.

It's my impression that at San Joaquin, at San Joaquin I thought USGS used some probabilistic considerations with regard to one of the faults. Am I wrong?

MR. DEVINE: Yes, we did, but --

DR. OKRENT: I'm not faulting you for it.

(Laughter.)

MR. DEVINE: I was trying to avoid being clever and

eb3

saying we did it in a qualitative way because in effect that's what we did.

The evidence was, in our judgments, there rather strong but because of the specific probability numbers but the orders of magnitude were so strong that we felt the magnitude estimates for the Pcnmosa was unreasonable.

DR. OKRENT: I just wanted to see if my memory wasn't valid.

MR. DEVINE: That is correct.

DR. OKRENT: If I can I would like to make some observations, at least how I see where things stand now. Is that okay?

PROF. KERR: I would like you to do that. Also, since you are leaving shortly, if there is anything specific that you would like the consultants to contribute later on, I wish you would comment on that.

DR. OKRENT: Well, first I would like to note I don't think the Staff had enough time to really present what they would have liked to in order to give their side. That's just the way the agenda was set up.

But my next comment is I think we would have to have another Subcommittee meeting before we brought anything in to the Full Committee so there will be more time in my opinion.

I would recommend that the Staff do a fairly good review of GE's probabilistic study with regard to the probability

mpbl
flws
3

of faulting. To me it's an important part of the picture, and it certainly either should be looked upon as numbers that are in the ballpark or numbers that are in great dispute for some reason.

But I haven't heard any reasons so far or in what I read in the Staff comment in that regard, so I think you ought to get either the probabilistic analysis staff or somebody to review this together with geologists and seismologists, whatever is appropriate.

And I think your subcommittee should do the same via the consultants we have here, and you could maybe get one or two mathematicians in if that's what you think, Mr. Chairman.

At the moment I expect if there's a resolution of this degree of faulting, it will come more from that end rather than 'is it two meters or one meter'.

I suggest that we ask our consultants to think about what they've heard today, and to write us, reviewing in particular what seemed to be important differences in the Staff position and GE's position as it would affect the various proposed design bases, and then provide their other thoughts that they think are relevant. But they have seen various differences and I think it would be useful to have that.

And I still think that the question of what is the appropriate seismic design basis for vibratory motion is important. If I understand correctly, the Staff doesn't have a recommended position in that regard. Dr. Newmark is supposed

1462 319

mpb2

to be looking at that is what I thought I heard, with regard to an effective g.

MR. NELSON: As pointed out in our September 27 letter, that, as well as the remainder of the engineering design, was deferred because of the differences in the parameters that GE had proposed and that our geologists and seismologists had arrived at.

DR. OKRENT: Well, I can only give you my own opinion. I think it would be useful to know what the Staff thinks is a suitable effective g because from what I have read, if I understand correctly, it is what USGS would give as an acceleration from the seismology point of view, but they weren't recommending it as an engineering design number.

I'll speculate that if one is going to look at the vibratory motion part, because you're talking about probabilities let's say of maybe one in 100 or one in 1000 per year of a large earthquake nearby, and maybe even nearer, that's a fairly frequent challenge.

And so I speculate there will be more than ordinary interest in knowing that the plant in fact can ride out an earthquake safely, if one gets to that point in this review. In other words, I think the kind of assurance that one wants if you're being challenged at that rate is different than if you think you're being challenged on one to the five per year, if I may put it that way.

mpb3

haven't heard enough today to tell me whether anybody really thinks landsliding is a problem. Maybe the consultants would tell us that and maybe the Staff at some future time, but I'm just making an observation.

The Staff wrote something and --

MR. JACKSON: Excuse me, Dr. Okrent.

In the handout we eliminated that for the sake of time, and John Greeves is sitting here and he was going to discuss it, and he'd be happy to if you would like.

PROF. KERR: We would not like.

DR. OKRENT: Not today.

But do you think there needs to be more information developed with regard to landsliding before you dismiss it?

MR. JACKSON: Absolutely.

DR. OKRENT: That, then, is something that perhaps needs to be explored.

I only want to make one final comment.

If I look at various reactors around the country and try to look at what the Staff accepts or doesn't accept with regard to overall seismic risk where I am able to either quantify it in some way myself or look at the Staff's own numbers, I find a very considerable disparity.

In other words, the kinds of numbers discussed at--for LACBWR, for example, with regard to liquifaction, or during the Diablo Canyon review, or the numbers today, the range is

mpb4

really several orders of magnitude.

I think you can't stay on this deterministic road because you're getting yourself into an untenable position. Let me leave it that way. And I think you had better get on with the probabilistic look for whatever insight it can give you.

I'm not urging that you use that as your only basis for decisionmaking, but right now I can't find a good rationale in the decisionmaking.

MR. JACKSON: Dr. Okrent, I would ask for clarification of that.

Do you mean for the GETR site or in a generic sense? If it's in the generic sense there are several large ongoing programs looking at probabilistic methodologies.

One is the systemmatic evaluation program which is being handled by Lawrence Livermore Laboratory and Terra Corporation.

DR. OKRENT: I'm familiar with that. I'm going to a two-day subcommittee meeting when I leave here. That's the reason why I'm going to leave here soon.

MR. JACKSON: That's not the same study.

DR. OKRENT: No, I'm also aware of the other.

But what I'm saying is what the Staff accepts or doesn't accept as you go from reactor to reactor I think shows a wide variance with regard to overall seismic risk. And I'm

mpb5

unwilling to sort of say, well, it should be 10^{-6} or 10^{-7} at one place and it's okay for it to be 10^{-3} or 10^{-4} at another place unless I know why or so forth.

MR. JACKSON: We agree in principle. We've tried to implement it. We have a large number of studies going on. It's a very difficult issue, as you well know.

Dr. Pomeroy has worked on aspects of it. We are exceedingly interested in it because of the problems with the tectonic province concept approach that we currently utilize in the eastern U.S.

PROF. KERR: Are there questions that the consultants or the other members of the subcommittee want to raise?

MR. WHITE: I would like to raise one thing.

I could add to what Dr. Okrent said. In considering the probability question you shouldn't -- the Staff shouldn't devote themselves, their effort, to showing what is wrong with what was done, but rather should have their own way of dealing with it.

MR. JACKSON: We clearly do not have the manpower resources to do that. We are in the review mode. I think we would like to do that most f the time, but we just cannot.

PROF. KERR: Let me thank all of you who have participated today. It has obviously been a long session, and one in which there was maybe more than the usual divergence of viewpoint. And I recognize some of the problems

mpb6

facing all of you.

It seems to me that this reactor and the situation is certainly unusual. It is not a power reactor, for example, in the usual sense. I don't know whether that makes the risk more or less, but at least it's different.

It's also a reactor that is there, and one that has been useful and would continue to be useful to not, I think, just the General Electric Company but to other parts of the country as well. I don't know how one takes it into consideration, but I think one cannot ignore that.

Now, Dave has said, and I think one can't help but emphasize that I think the issue is whether the reactor can operate with acceptable risk. There are other issues obviously being considered, but I believe that is the issue with which we are faced.

And because of that I think one can't avoid some comparison with other risks, and with risks of other reactors.

Now when I read what the Staff wrote in the September 27 letter, I find that I don't understand what the message is; and I don't mean to be critical here, but I think it's crucial. Maybe GE does understand the message, but I don't.

In the last paragraph I find:

"Furthermore, while you may propose to analyze the GETR using a seismic and geologic design basis and then close the

mpb7

report, we're not aware of any structure that has been analyzed or built to this type of seismic loading, and it is our current view that an analytical argument cannot be formulated which would conclusively support the ability of the structure such as GETR to withstand a 2.5 meter surface offset."

11.250

The part of this I don't understand is that one could assume the difference between the Staff and the Licensee is a meter and a half of surface offset.

It's also possible, though, that the Staff's position is that they do not think the reactor should operate anymore, and that since they consider that 2.5 meters of offset is unachieveable, this is the way of shutting it down.

I'm not trying to be critical, I'm trying to interpret.

If indeed this is the conclusion that the Staff has reached, then it seems to me it should be said to the Licensee in a less oblique way. I mean, the Staff may indeed have concluded that this reactor cannot, now that the Staff has reached some different conclusions about seismicity, be operated safely in that location.

Now you may have some difficulty finding what rules, regulations and laws which permit you to enforce the decision, but if that's the decision that has been reached, it seems to

mpb8

me that the Staff and the Commission should be candid about it.

If on the other hand the difference is a difference on whether one should design for -- really for 2.5 meters of offset versus GE apparently thinks they can design for one, it seems to me that there is basis at least for further discussion.

In the first place it certainly must be true that the probability of one meter offset is considerably higher than the probability of 2.5.

DR. OKRENT: It's smaller -- I'm sorry, I beg your pardon, I'm wrong.

PROF. KERR: It could be, but....

(Laughter.)

I don't know what the difference it is, but it seems to me that some explanation of this might put things in better perspective if indeed that is the issue. And Dave has spoken to this.

And it seems to me -- and maybe it's a comparative thing, I don't know.

I have also heard the statement on several occasions today that people are seeing things for the first time. IF indeed that is true and if some of the things that have been seen are substantive, again, maybe further discussion is in order.

I also -- well, I won't say any more about that.

MR. NELSON: If I could just comment on your comment

mpb9

on the cover letter, the Staff conclusions in the evaluation which was enclosed with this cover letter is that 2.5 meters is the proper design basis for offset, and the Staff also felt obligated to say -- and this is based not on a review of the facility or an analysis of the facility, but on an understanding that, I guess, of the state of the art of structural engineering that they didn't feel analytically that it could be demonstrated that the facility could withstand 2.5 meters; but it didn't preclude the Licensee from pursuing that using the Staff's design basis.

PROF. KERR: The point I was trying to make, Mr. Nelson, and I may not have made it very well, is that it is possible, but after further consideration -- in fact somebody said earlier that 2.5 was about 2.5, and I don't know what that means -- further consideration might bring the two parties closer together if indeed the issue is 2.5 versus one.

If the issue is that the Staff has concluded that under no circumstances can they be pursauded that the reactor can be operated safely, then there's no particular point in exploring whether it's 2.5 or one or somewhere in between.

That's the point I was trying to make. I don't know which is the case.

But I do think that the Staff ought to make it clear which is the case.

To the consultants, I would hope that you could write

mpb10

in your usual way something to the subcommittee and the committee that would be useful to us in our further consideration.

Dave has said that emphasis should be placed on the differences, and I think this is true. If there are areas of information which you think could be developed in some reasonable way and which would assist you and us in our further consideration, please mention that too as you think about it.

Now I have no idea whether you think the Staff's position is too conservative or not conservative enough, or whether you even want to comment on that directly or obliquely. But it seems to me such a comment maybe obliquely is appropriate. I certainly do not have a position at this point. I don't know.

It's clear that the positions are different, but I have not seen evidence that one is -- perhaps even the Staff is being not conservative enough, I'm not sure at this point.

I do think that we certainly must have probably another subcommittee meeting before we go to the full committee. That will not hold things up. It's perhaps unfortunate because we're scheduled tightly enough in December that even if we wanted to take this to the full committee in December we couldn't schedule it.

I will try to get in touch with the NRC people through Mr. Igne shortly after you've had a chance to consider this,

mpb11

and after GE has had a chance to consider it further and see what our next move should be.

I would assume that the next move is probably to schedule another subcommittee meeting, as much as I love meetings. But if this is to continue, I do think we need to develop some of the things we did not have a chance to develop today a bit more fully before we go to the full committee.

Those are the comments I have.

Does anybody have anything further?

MR. BALDWIN: Yes, Mr. Chairman.

Andrew Baldwin, representing Congressman Dellums.

First of all I would like to request that you request the various parties here, the ACRS consultants, the ACRS members, the NRC Staff, and General Electric and USGS to provide copies of the various filings up until now and in the future to the service list in the Licensing Board proceeding.

All of these documents are very important to that case, and the members of the Licensing Board and the parties in that case haven't had the opportunity to see them.

PROF. KERR: To what documents do you refer, Mr. Baldwin?

MR. BALDWIN: Well, I noted today that there is a brown volume provided by General Electric, there were filings made by USGS with pictures and all the rest.

PROF. KERR: Those become part of the minutes and go mpbl2

directly to the Public Document Room.

MR. BALDWIN: Well, the members of the -- What I'm asking is that that material be provided by the mail to the people on the service list in the Licensing Board proceeding.

PROF. KERR: If you will write me a letter, because I'm not sure what it is you want, if you will write me a letter I will certainly see that it gets to the ACRS executive director and to the committee.

I'm not trying to put you off, it's just that I'm not sure what it is you want.

MR. BALDWIN: I'll try again.

PROF. KERR: Would you be willing to write it?

MR. BALDWIN: Sure.

PROF. KERR: Okay. And we'll do what we can.

MR. BALDWIN: All right.

I would like to request also that any future subcommittee meetings concerning this reactor be held in this area.

Is that your current intention?

PROF. KERR: We have not scheduled the next meeting. And we will schedule again -- we always do -- with that as an important consideration. I don't know what the circumstances will be, so I can't say where the next committee meeting will be held. But we certainly will attempt to schedule it near the site.

mpb13

MR. BALDWIN: Thank you.

MR. DARMITZEL: One point of clarification:

We're to wait for word through Mr. Igne as to what General Electric should do to proceed with this matter now? You're going to get advice from your consultants, and that will have some kind of impact on this -- How we'll be notified?

PROF. KERR: I'm going to have him get advice from you on what you want to do next, as well as what we want to do next. It depends on what you want to do next to some extent.

But assuming that you want to pursue this further -- and I did -- then I think the next step, subject to advice from the rest of the committee, is to schedule another sub-committee meeting. And my point was, we couldn't in any event have scheduled a meeting with the full committee in December because December's agenda is filled at this point.

Did I respond to your question?

MR. DARMITZEL: Yes.

One last thing. I would like to respond briefly to the allegations made by Mr. Baldwin.

I don't think this is the proper forum to answer those allegations. There is no substance to them as far as we're concerned.

PROF. KERR: Please let's -- I don't think....

MR. DARMITZE: All right, sir.

PROF. KERR: We do, as you recognize, permit members

mpb14

of the public to appear before the committee and make statements, and I think this is in the tradition of free speech.

Are there other comments or questions?

(No response.)

Again may I thank all of you for your participation. I declare the meeting adjourned.

(Whereupon, at 6:30 p.m., the subcommittee meeting was adjourned.)

1462 32

AGENDA

Introduction and G.E. Position	R. W. Darmitzel	General Electric Co.
Geologic Investigation Scope	R. C. Harding	Earth Sciences Assoc.
Regional Tectonic Setting	D. H. Hamilton	Earth Sciences Assoc.
Site Geology	D. M. Yadon	Earth Sciences Assoc.
Soil Stratigraphy and Age Dating	R. J. Shlemon	Roy J. Shlemon and Associates
Geology Investigation Conclusions	R. C. Harding	Earth Sciences Assoc.
Geology Overview	R. H. Jahns	Stanford University
Application of Probability Methods	J. R. Benjamin	Jack R. Benjamin and Associates
Probability Risk Assessment for Surface Offset	J. W. Reed	Jack R. Benjamin and Associates
Site Seismology	C. F. Richter	Lindvall, Richter and Associates
Summary	R. W. Darmitzel	General Electric Co.

1462 333

GENERAL ELECTRIC POSITION

1. MOST PROBABLE ORIGIN OF SHEAR-LIKE STRUCTURES IS LARGE-SCALE LANDSLIDING.

2. POSTULATED VERONA FAULT LENGTH APPROXIMATELY 8 KM.

3. NO "OFFSET" IN PAST 8,000 YEARS.

4. NO OFFSET BENEATH THE GETR.

5. CONSERVATIVE VALUE FOR PROBABILITY OF FUTURE OFFSET LESS THAN 10^{-6} PER YEAR.

6. AVERAGE RATE OF STRAIN RELIEF EXTREMELY LOW.

7. 1 METER OF OFFSET ON THE OBSERVED SHEARS IS A CONSERVATIVE VALUE.

8. 0.56 G EFFECTIVE GROUND ACCELERATION IS A CONSERVATIVE VALUE.

SUMMARY

- o RECOMMENDED SEISMIC VALUES FOR GETR STRUCTURAL EVALUATION
  - NO OFFSET WHICH BREAKS THE SURFACE BENEATH THE GETR
  - 1 METER OFFSET ON OBSERVED SHEARS
  - 0.56 G EFFECTIVE GROUND ACCELERATION
- o GEOLOGY PROGRAM THOROUGH AND RESPONSIVE
- o GEOLOGY/SEISMOLOGY POSITION SUPPORTED BY EVIDENCE
- o VALUE IMPACT STUDY APPROPRIATE
- o REVIEW BY FULL ACRS COMMITTEE

1462 335

Presentation by Earth Sciences Associates

- Regional Geologic and Tectonic Setting
  Douglas Hamilton
- Site Geology
  Doug [illegible]
- Quaternary History
  Roy Shlemon
- Interpretations and Conclusions
  Richard Harding

1462 336

SCOPE OF INVESTIGATIONS

- Literature Review
- Aerial Photo Interpretation
- Aerial Reconnaissance
- Detailed Field Mapping
- Subsurface Exploration
- Soil Stratigraphy Studies
- Age Dating
- Geophysical Studies
- Geotechnical Engineering Studies
- Ground Water Studies

1462 337

INTERPRETATION OF THE ORIGIN OF SHEAR FEATURES AND CONCLUSIONS RELATIVE TO DESIGN CRITERIA MUST BE CONSISTENT WITH KNOWN GEOLOGIC RELATIONSHIPS:

- REGIONAL GEOLOGIC AND TECTONIC SETTING
- SITE GEOLOGY
  - o GEOMORPHIC EVIDENCE
  - o OUTCROP EVIDENCE
  - o SUBSURFACE EXPLORATION
- QUATERNARY HISTORY
  - o SOIL STRATIGRAPHY
  - o AGE AND AMOUNT OF SOIL OFFSETS

1462 338

- REGIONAL GEOLOGIC AND TECTONIC SETTING

  DOUGLAS HAMILTON

- SITE GEOLOGY

  DOUG YADON

- QUATERNARY HISTORY

  ROY SHLEMON

- INTERPRETATIONS AND CONCLUSIONS

  RICHARD HARDING

1462 339

POOR ORIGINAL

- REGIONAL GEOLOGIC AND TECTONIC SETTING

  DOUGLAS HAMILTON

- SITE GEOLOGY

  DOUG YADON

- QUATERNARY HISTORY

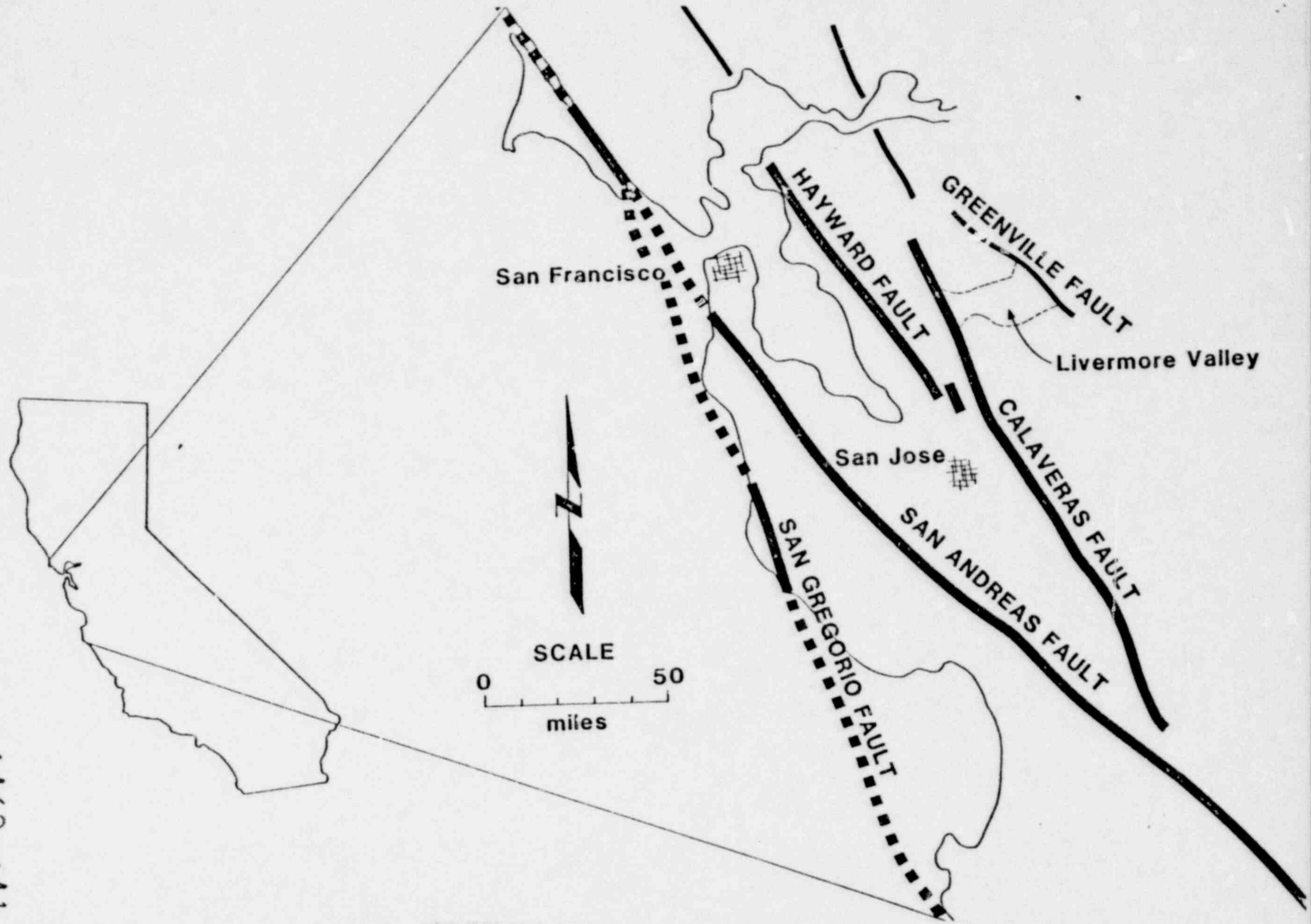
  ROY SHLEMON

- INTERPRETATIONS AND CONCLUSIONS

  RICHARD HARDING

1462 ‘40

8


San Francisco
HAYWARD FAULT
GREENVILLE FAULT
Livermore Valley
CALAVERAS FAULT
San Jose
SAN ANDREAS FAULT
SAN GREGORIO FAULT
N
SCALE
0
50
miles

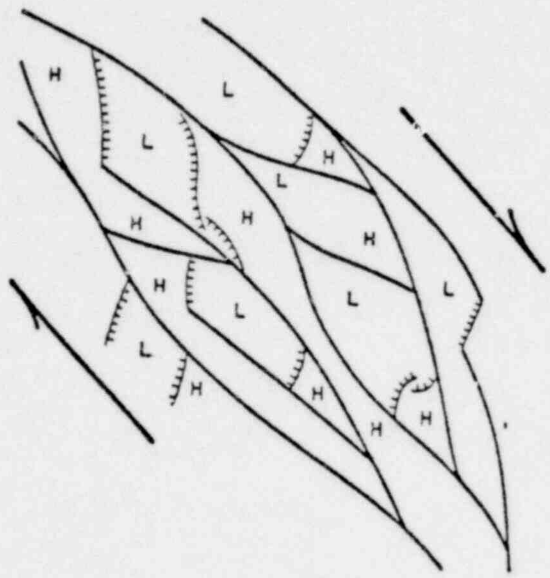
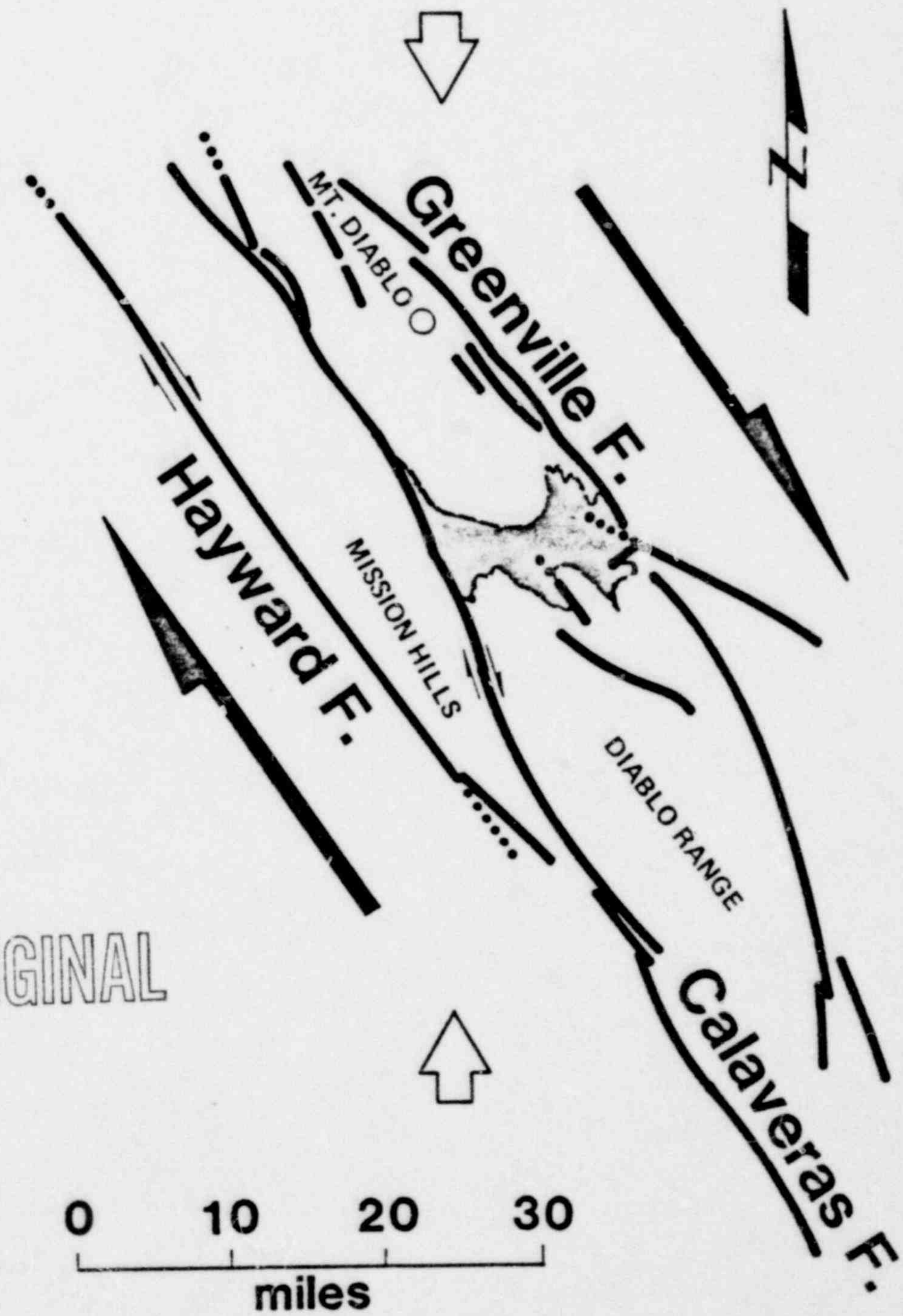


FIG. 12.—Sketch map of region in a transform regime, showing pull-apart basins and tipped fault wedges where right-slip faults converge or diverge

From Crowell (1974).



POOR ORIGINAL

1462 343

11

REGIONAL STRUCTURAL GEOLOGY MAP

Sources for ESA Compilation

Huey, 1948

Hall, 1958

DWR, 1966

CDMG, 1966

Brabb and others, 1971

Cotton, 1972

Helley and others, 1972

DWR, 1974

Wight, 1974

CDMG, 1975

Thorpe and Wight, 1976

CDMG, 1977

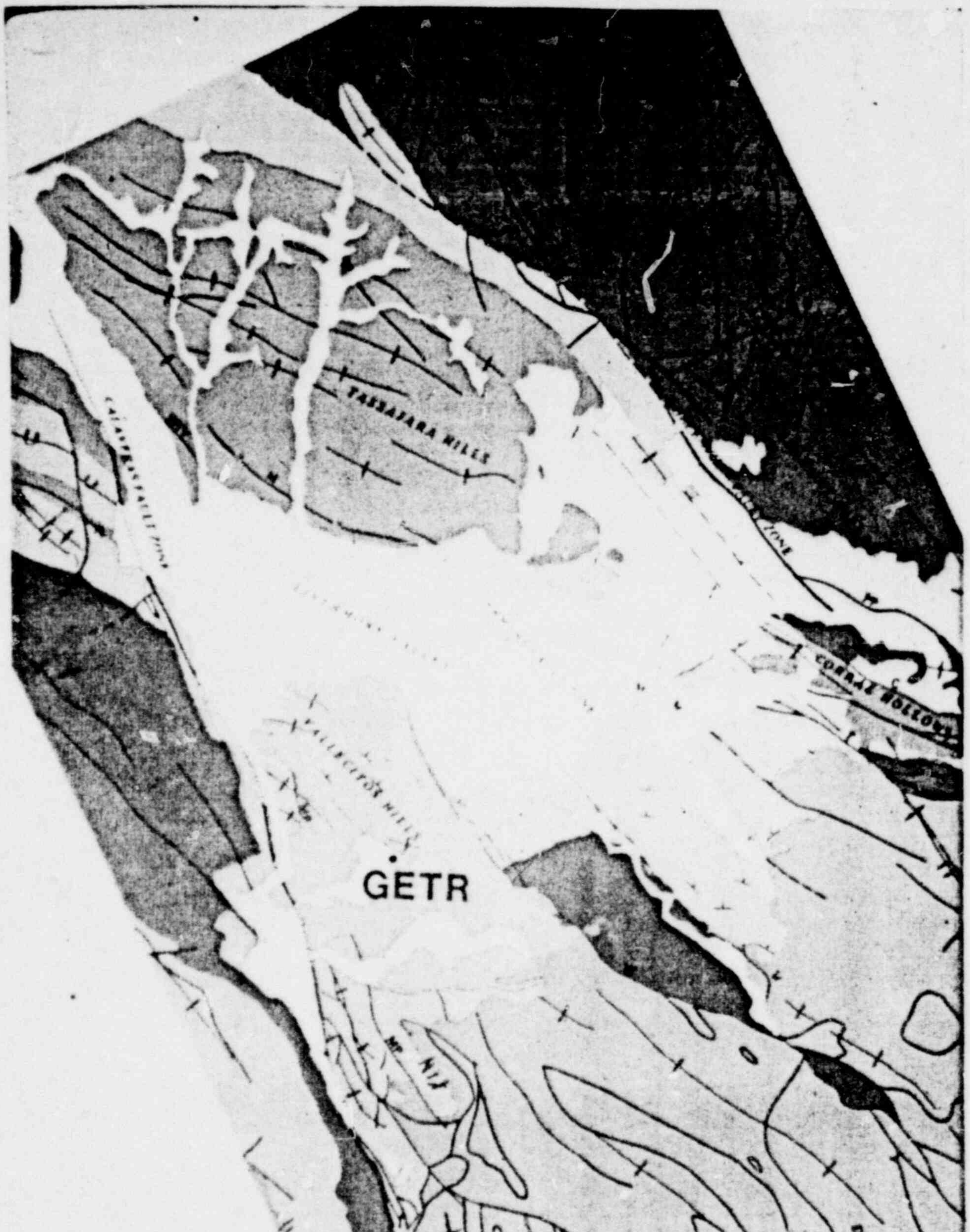
Herd, 1977

ESA, 1978 a, b

URS Blume, 1978

ESA, 1979

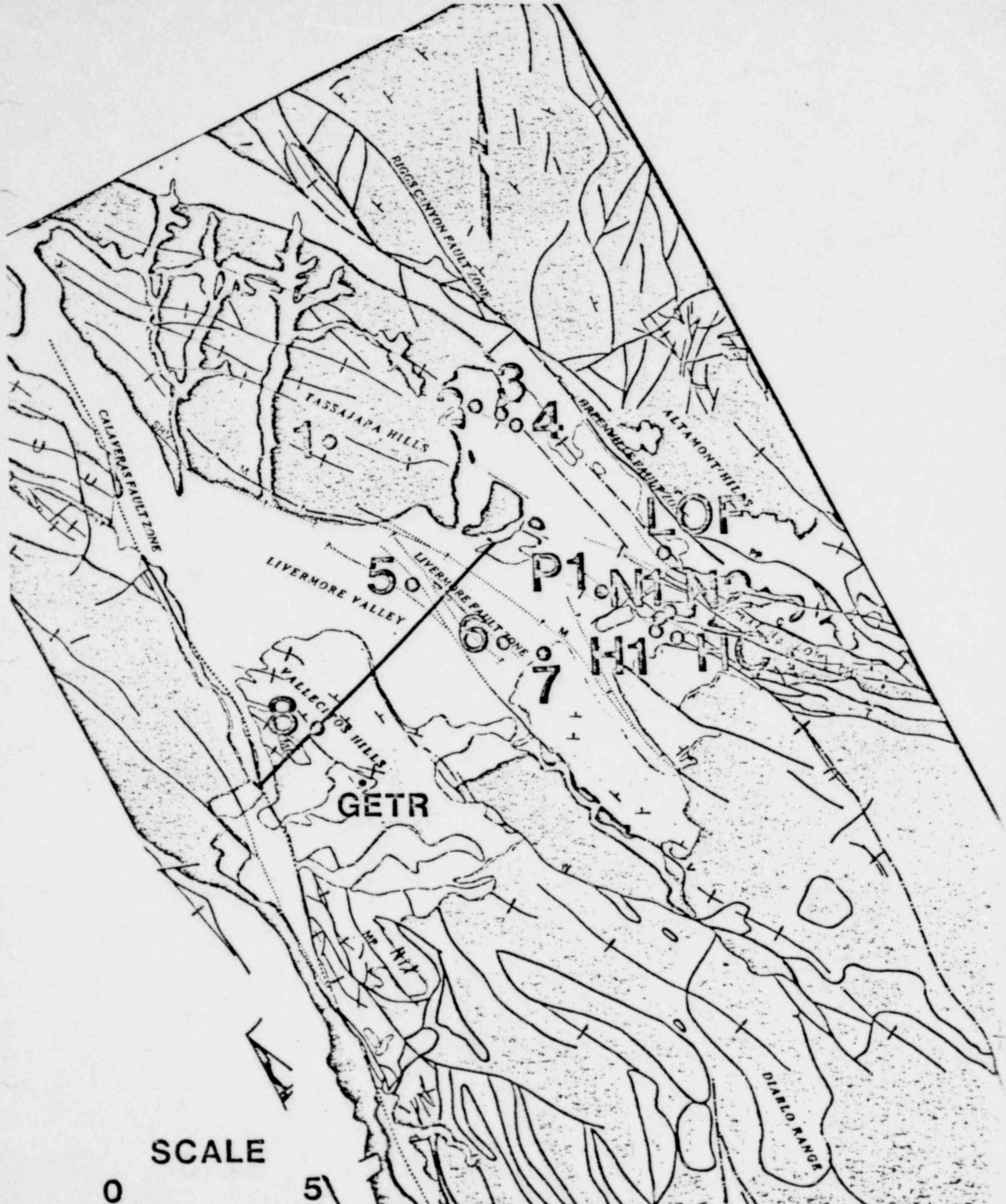
Haltenhoff, 1979


TASSAJARA HILLS
CORRAL HOLLOW
GETR
DIABLO RANGE
SCALE
0
5
miles

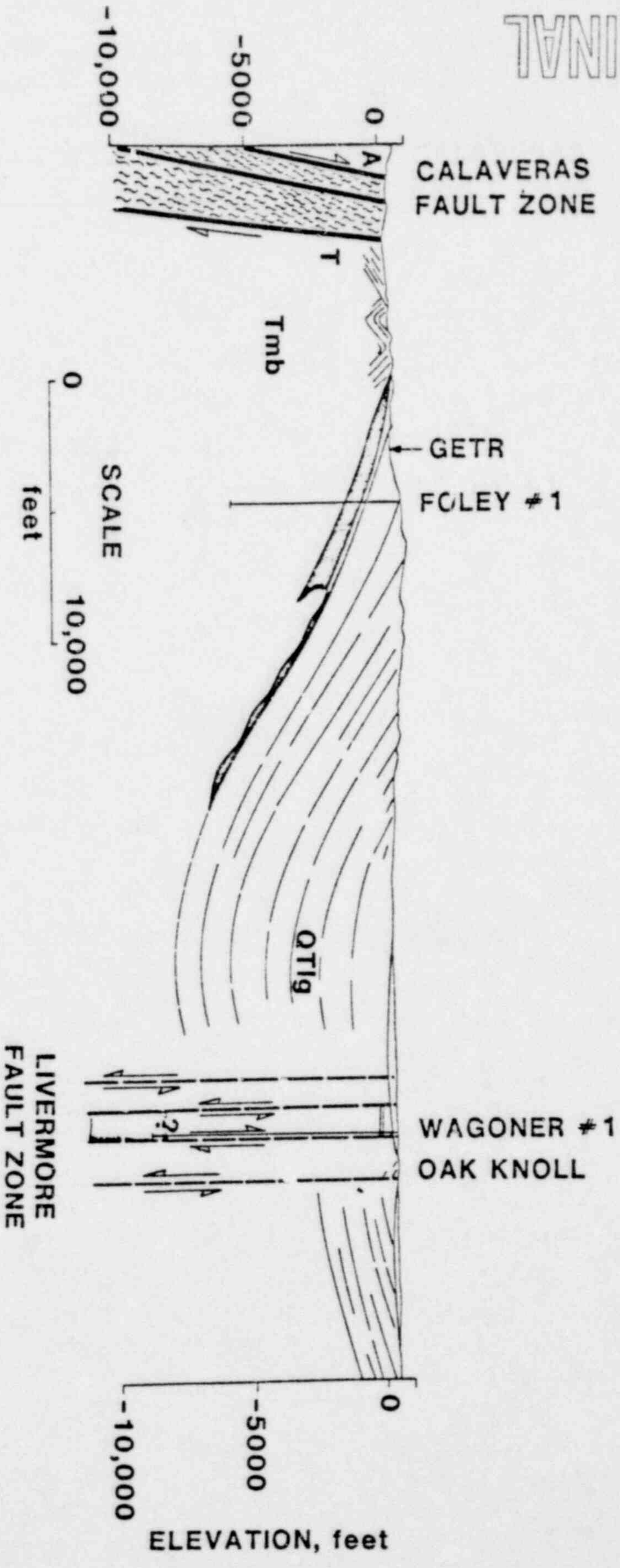

RIGGS CANYON FAULT ZONE
ALTAMONT HILLS
TASSAJARA HILLS
CALAVERAS FAULT ZONE
-50
LIVERMORE VALLEY
LIVERMORE FAULT ZONE
VALLECITOS HILLS
-40
GETR
-30
-20
DIABLO RANGE
SCALE
0
5
miles

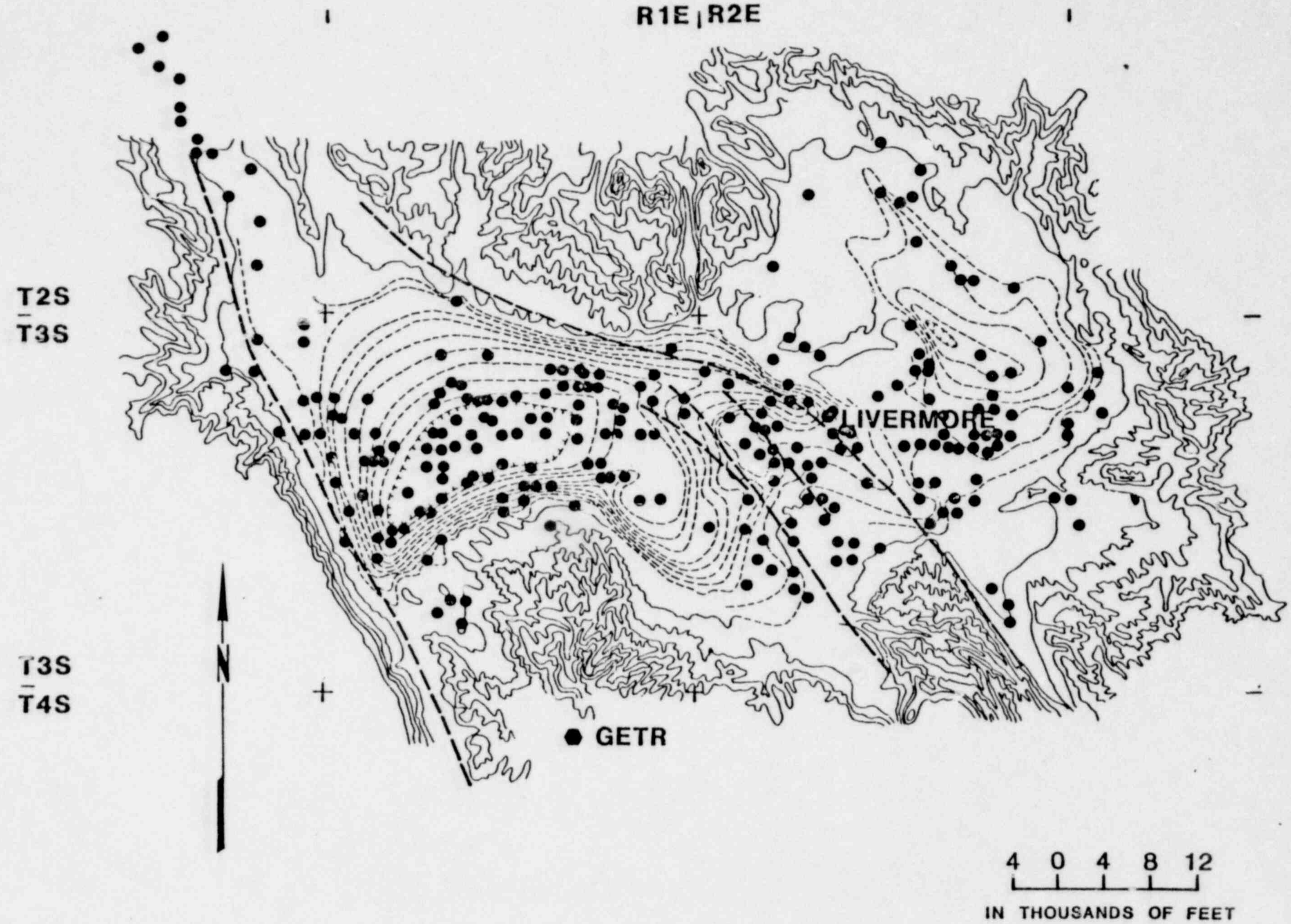
1462 346

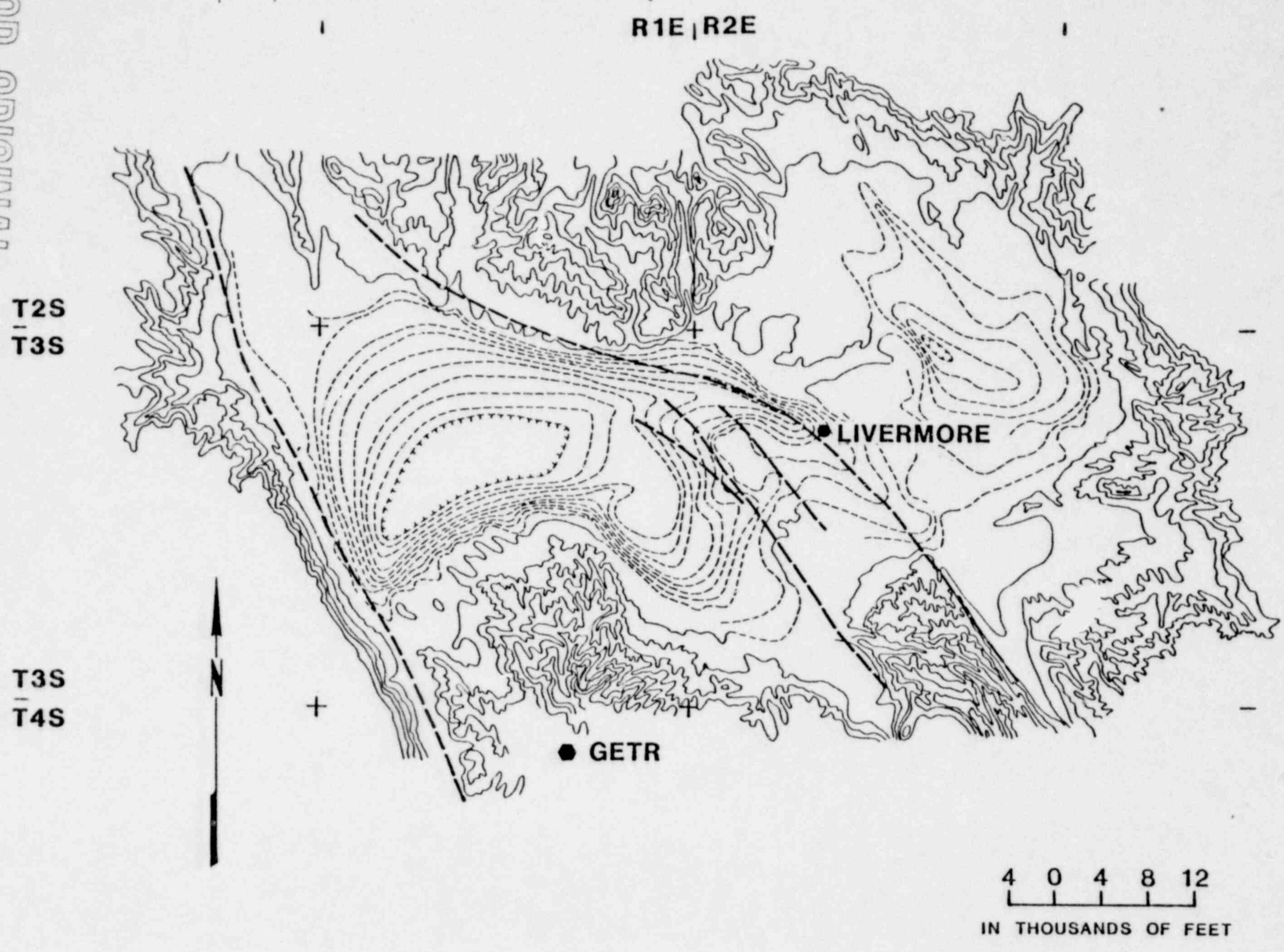
14


RIGGS CANYON FAULT ZONE
TASSAJARA HILLS
ALTAMONT HILLS
CALAVERAS FAULT ZONE
LIVERMORE VALLEY
LIVERMORE FAULT ZONE
VALLECITOS HILLS
DIABLO RANGE
1
2
3
4
5
6
7
8
LOF
P1
H1
GETR
SCALE
0
5
miles

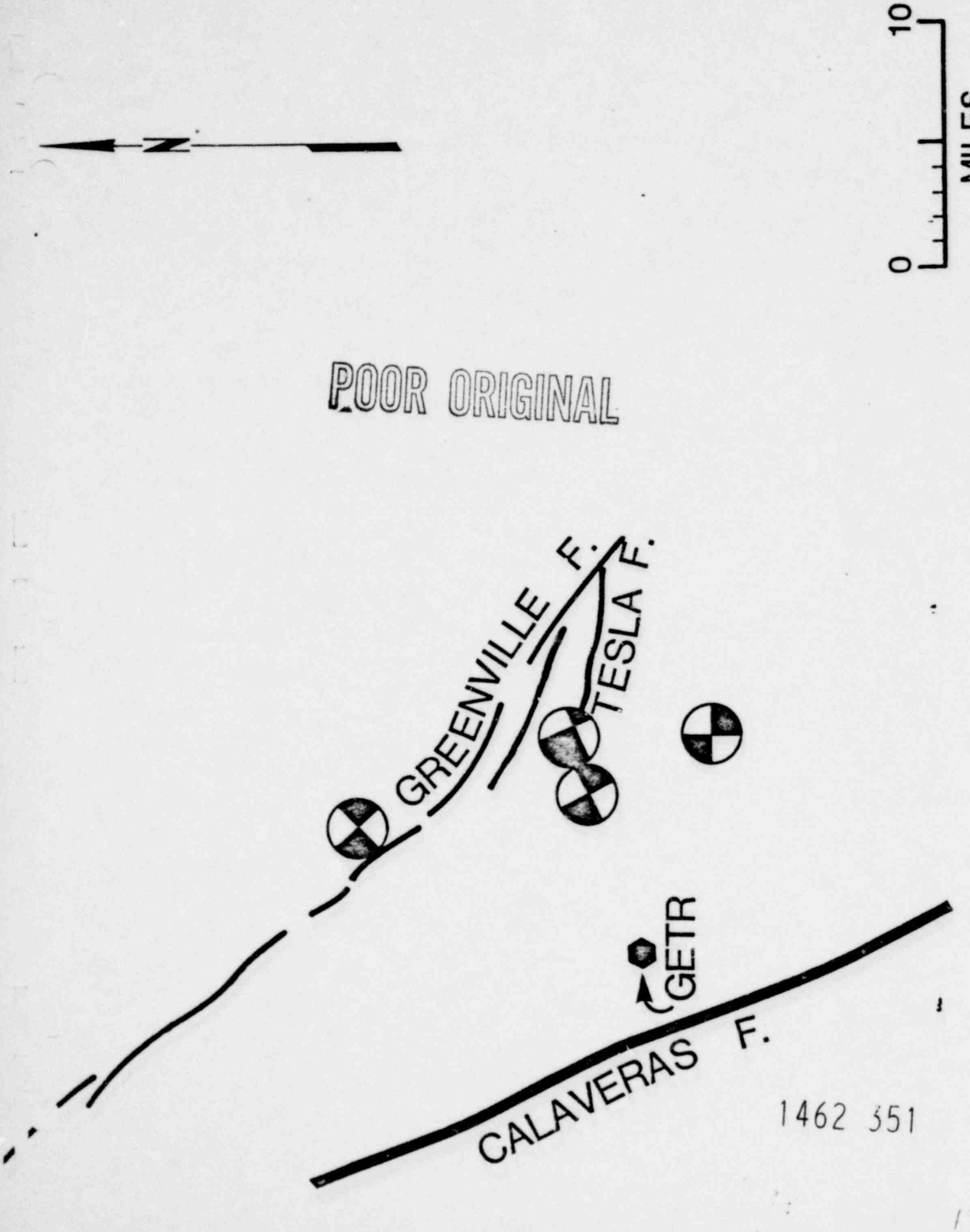
1462 347


CALAVERAS
FAULT ZONE
A
T
Tmb
GETR
FOLEY #1
QTlg
WAGONER #1
OAK KNOLL
LIVERMORE
FAULT ZONE
SCALE
0
10,000
feet
0
-5000
-10,000
ELEVATION, feet


R1E | R2E
T2S
T3S
T3S
T4S
N
LIVERMORE
GETR
4 0 4 8 12
IN THOUSANDS OF FEET

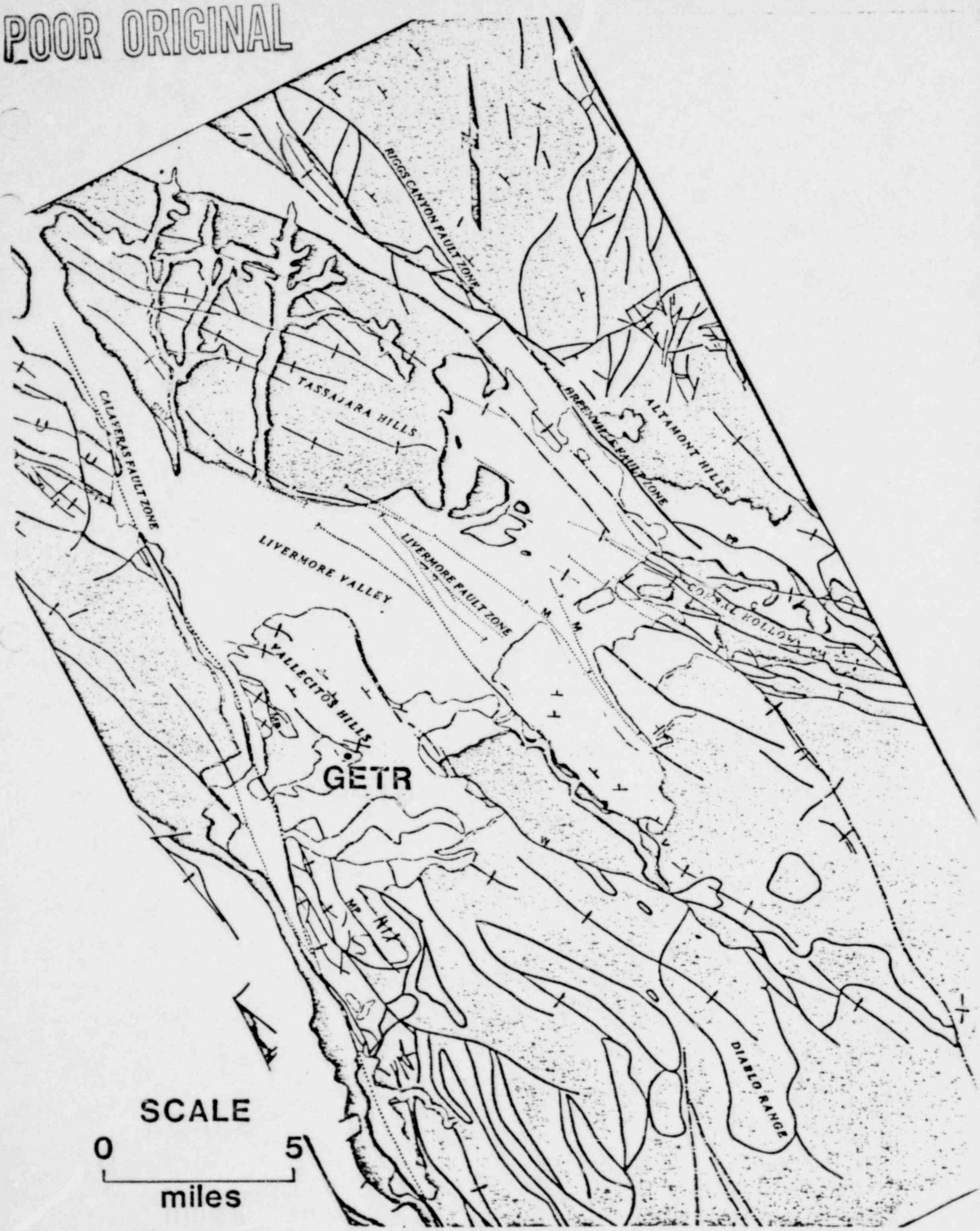

R1E | R2E
T2S
T3S
T3S
T4S
LIVERMORE
GETR
N
4 0 4 8 12
IN THOUSANDS OF FEET

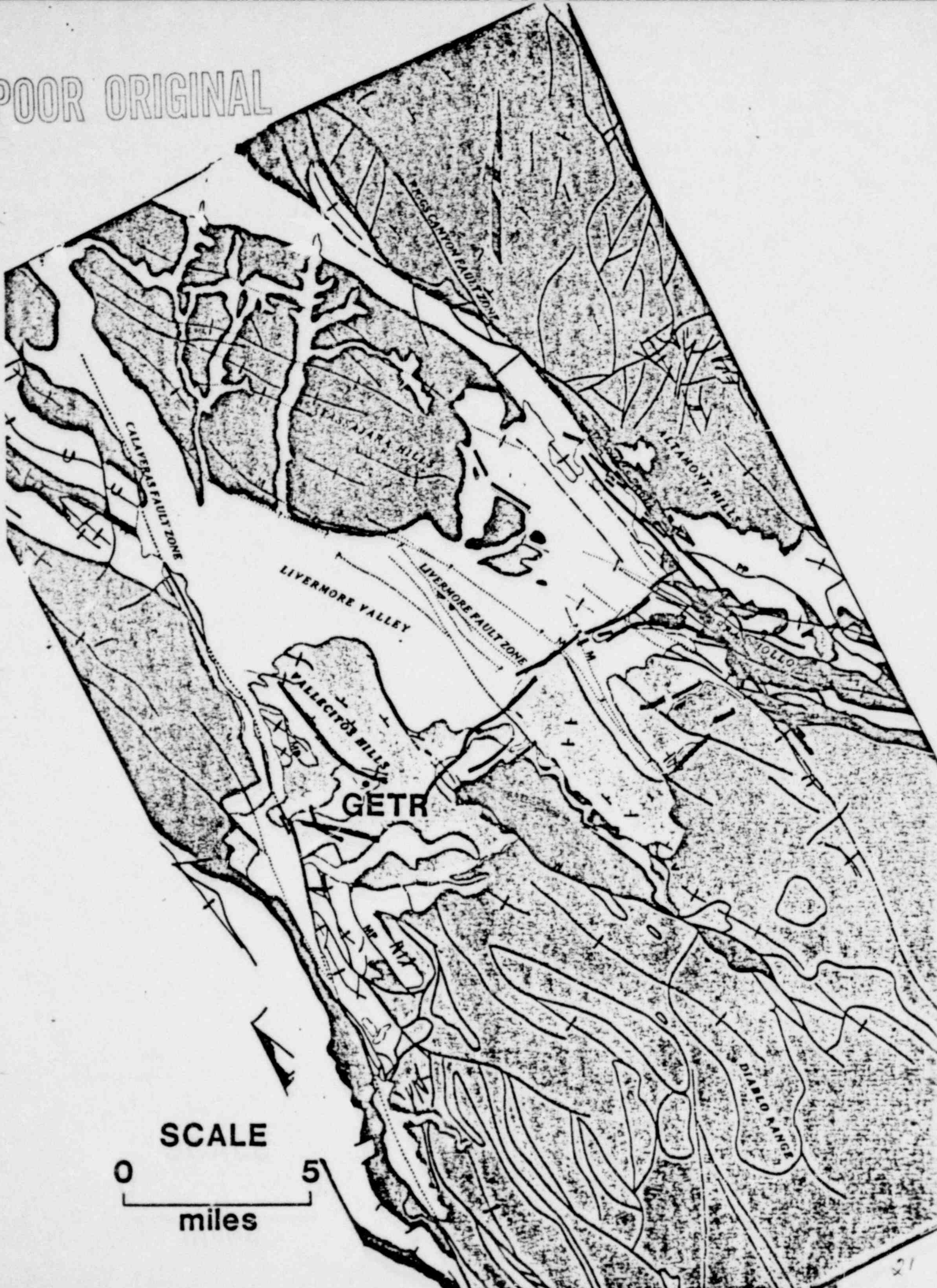
1462 350

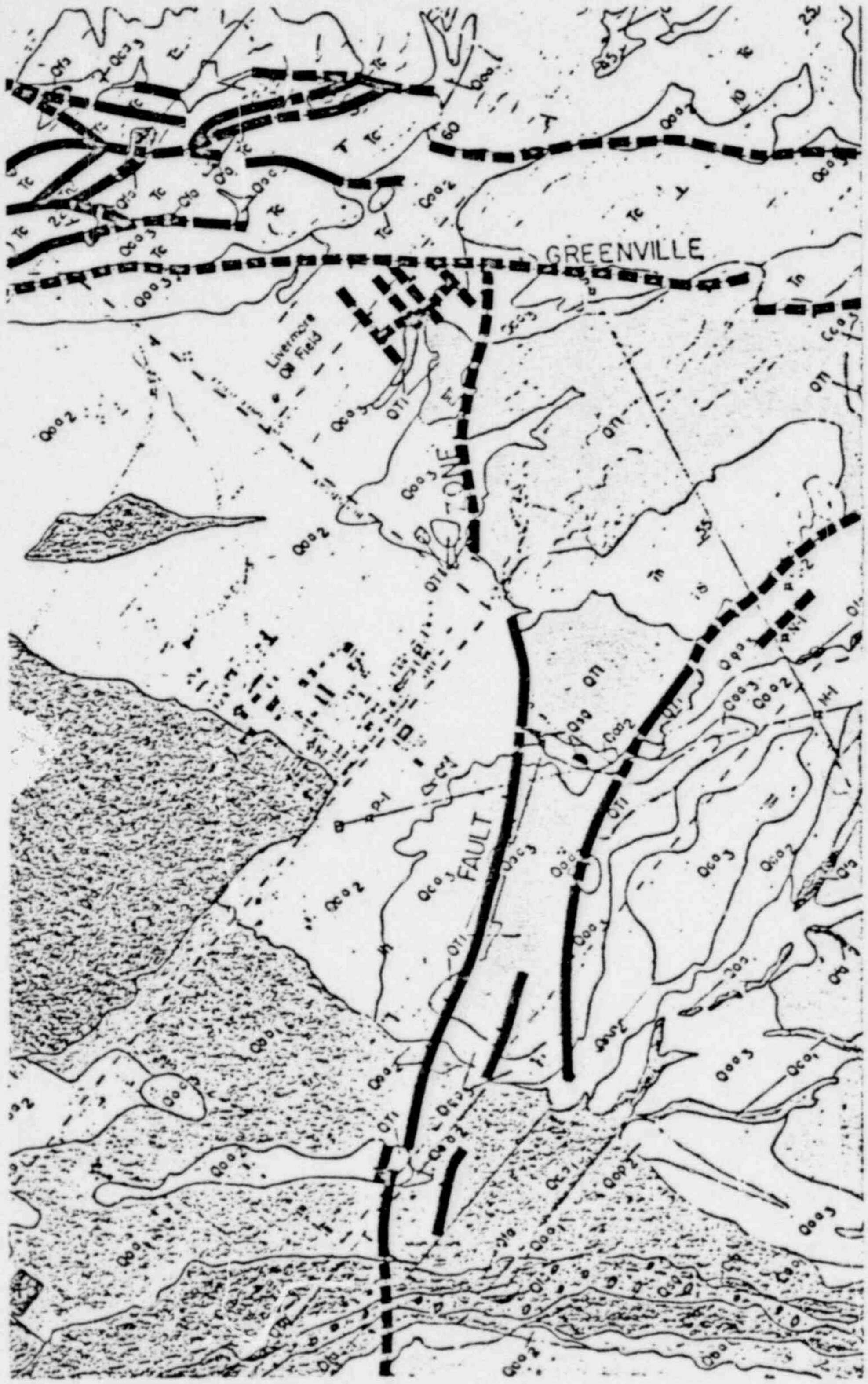


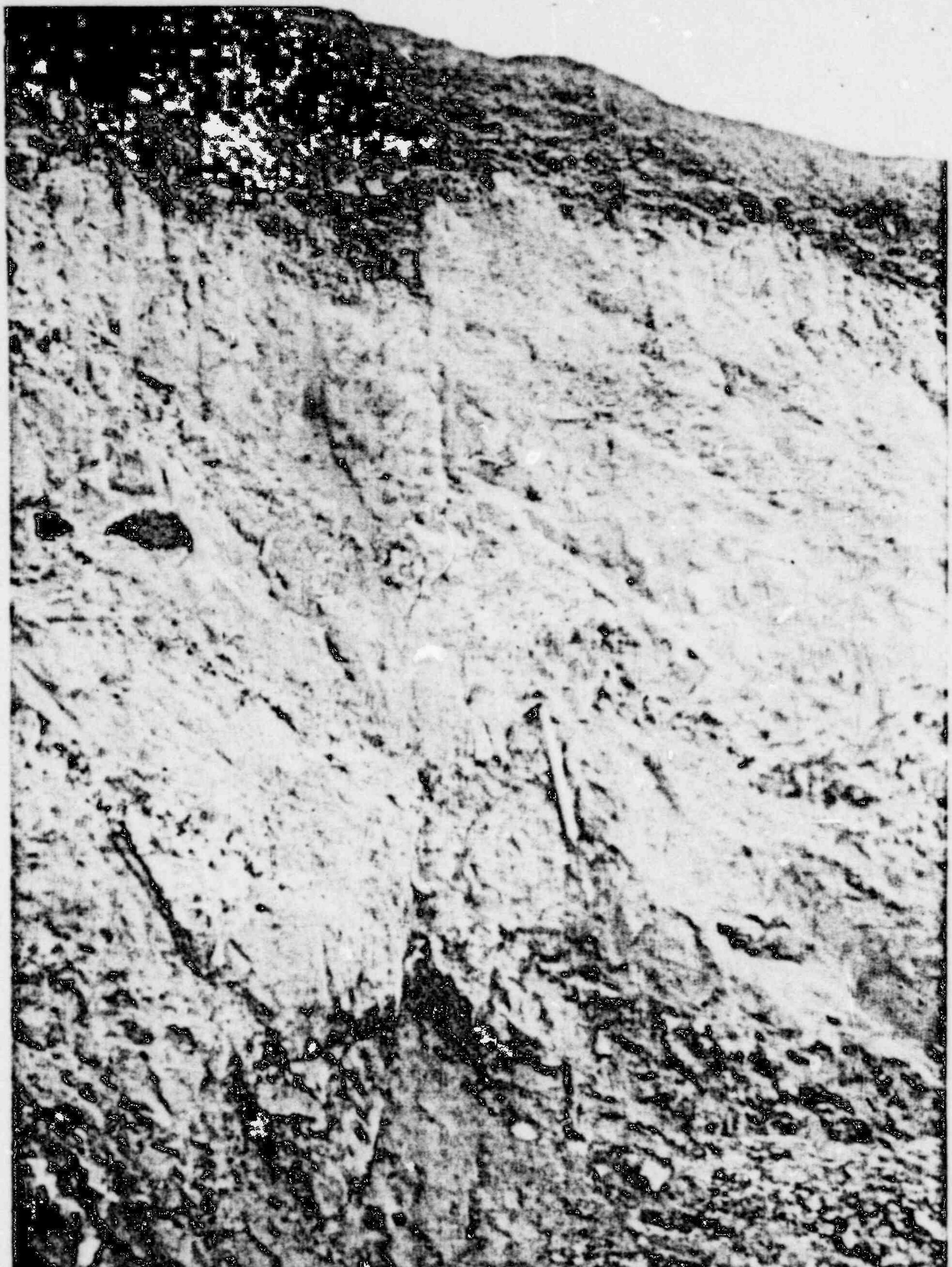
POOR ORIGINAL

1462 351


RIGGS CANYON FAULT ZONE
TASSAJARA HILLS
GREENVILLE FAULT ZONE
ALTAMONT HILLS
CALAVERAS FAULT ZONE
LIVERMORE VALLEY
LIVERMORE FAULT ZONE
VALLECITOS HILLS
GETR
DIABLO RANGE
SCALE
0
5
miles


TASSAJARA HILLS
CALAVERAS FAULT ZONE
LIVERMORE VALLEY
LIVERMORE FAULT ZONE
ALTAMONT HILLS
VALLECITOS HILLS
GETR
DIABLO RANGE
SCALE
0
5
miles

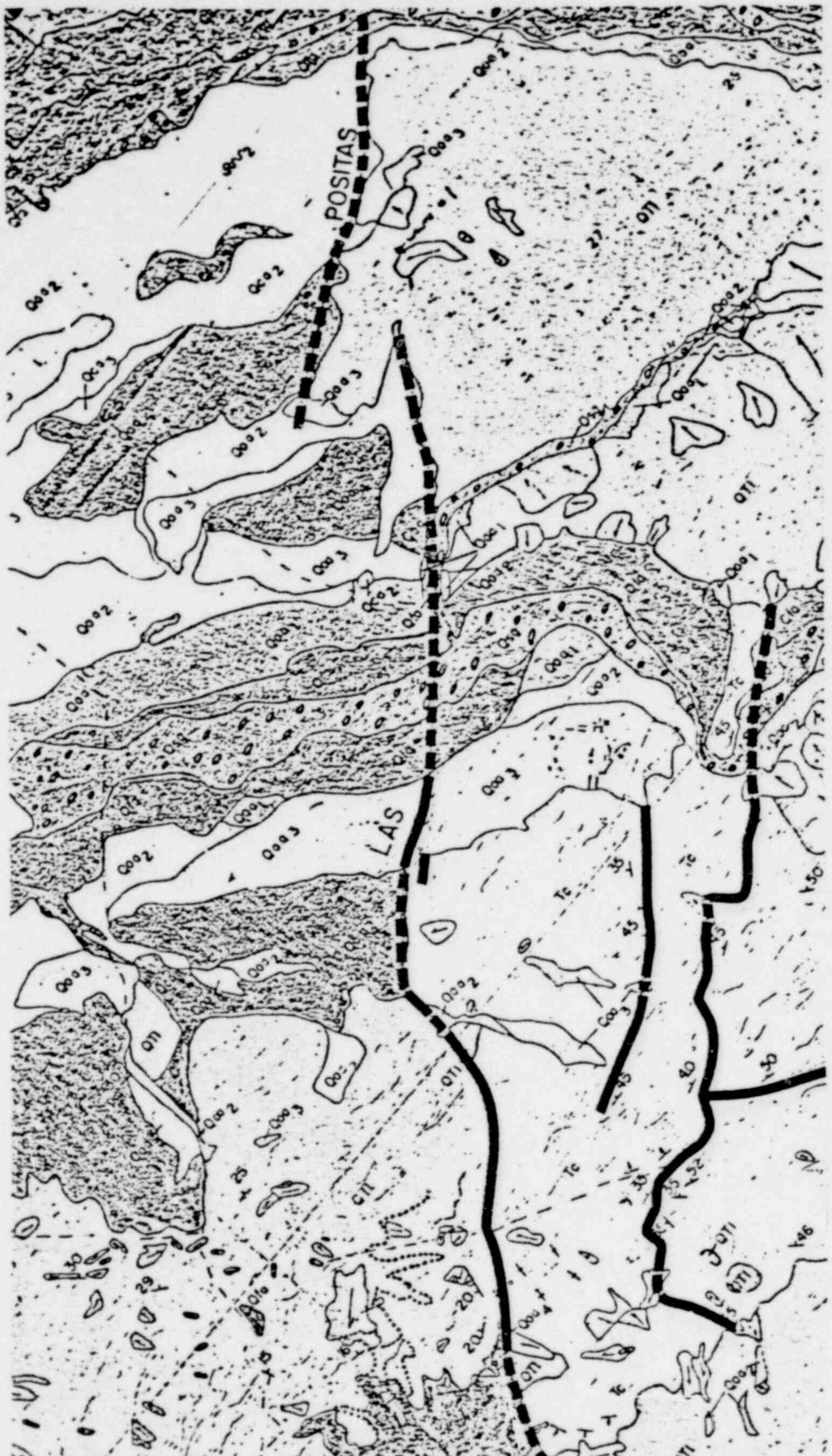

GREENVILLE
Livermore Oil Field
ZONE
FAULT

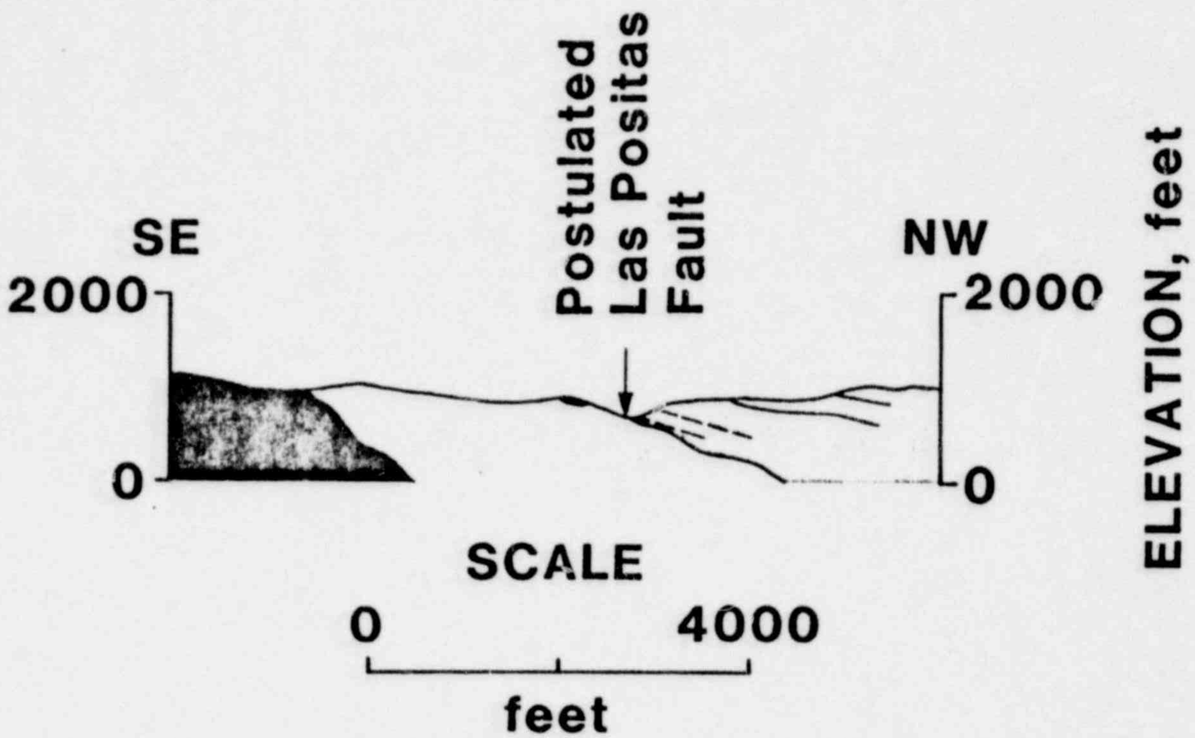


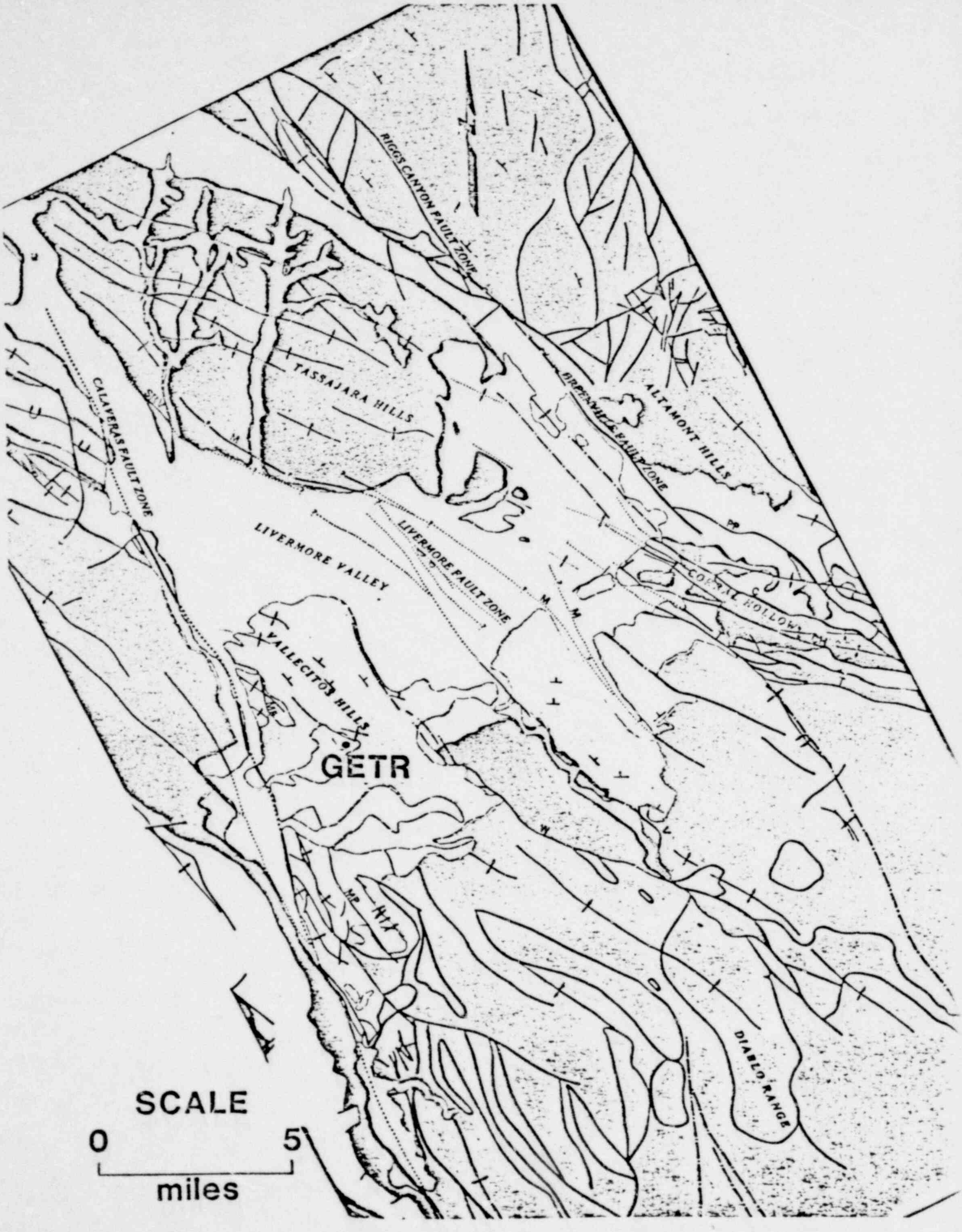
POOR ORIGINAL

1462 356

24

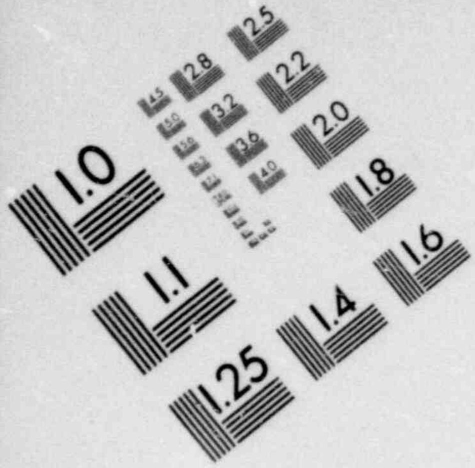

POSITAS
LAS


SE
2000
0
Postulated
Las Positas
Fault
NW
2000
0
ELEVATION, feet
SCALE
0
4000
feet



POOR ORIGINAL

1462 360



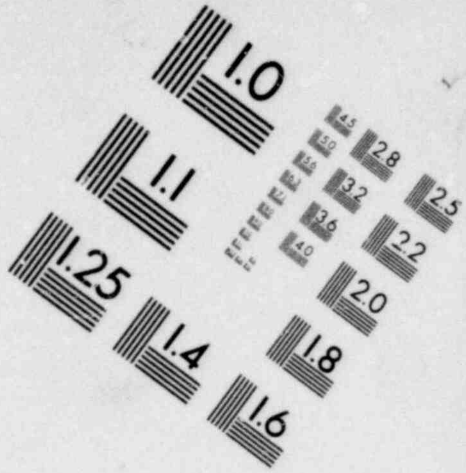
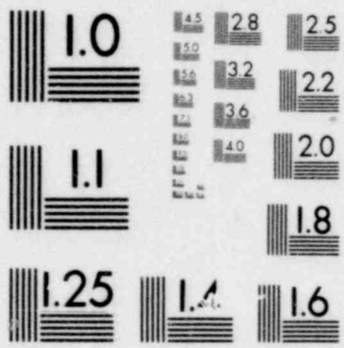
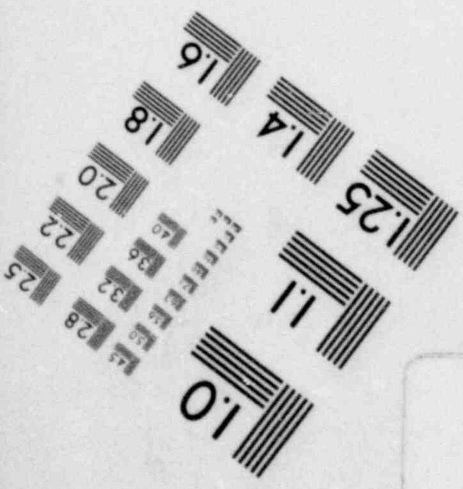


IMAGE EVALUATION TEST TARGET (MT-3)



6"



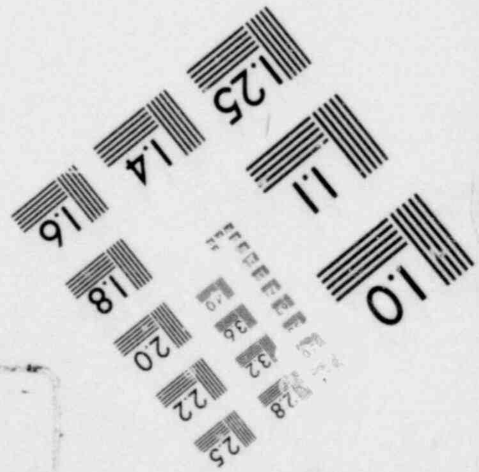


IMAGE EVALUATION
TEST TARGET (MT-3)

6"

**IMAGE EVALUATION
TEST TARGET (MT-3)**

6″

IMAGE EVALUATION
TEST TARGET (MT-3)

6"

REGIONAL GEOLOGIC AND TECTONIC SETTING

- FAULTS, FOLDS, ROCK UNITS PREDOMINANTLY NORTHWEST-TRENDING STRUCTURES
- REGIONAL STRESS PATTERN - RIGHT TRANSFORM SHEAR CORRESPONDING TO NORTH-SOUTH COMPRESSION
- GEOLOGIC, GEOPHYSICAL, AND WELL DATA INDICATE LIVERMORE VALLEY HAS BEEN A SUBSIDING BASIN SINCE AT LEAST PLIOCENE
- LAS POSITAS FAULT IS RELATIVELY MINOR CROSS STRUCTURE IN SOUTH-EAST CORNER OF LIVERMORE VALLEY
- NO EVIDENCE TO EXTEND LAS POSITAS FAULT TO SOUTHWEST ACROSS LIVERMORE FAULT; EVIDENCE INDICATES CIERBO-LIVERMORE GRAVELS CONTACT IS ONLAP UNCONFORMITY AS MAPPED BY HALL, 1958

1463 001

- REGIONAL GEOLOGIC AND TECTONIC SETTING

  DOUGLAS HAMILTON

- SITE GEOLOGY

  DOUG YADON

- QUATERNARY HISTORY

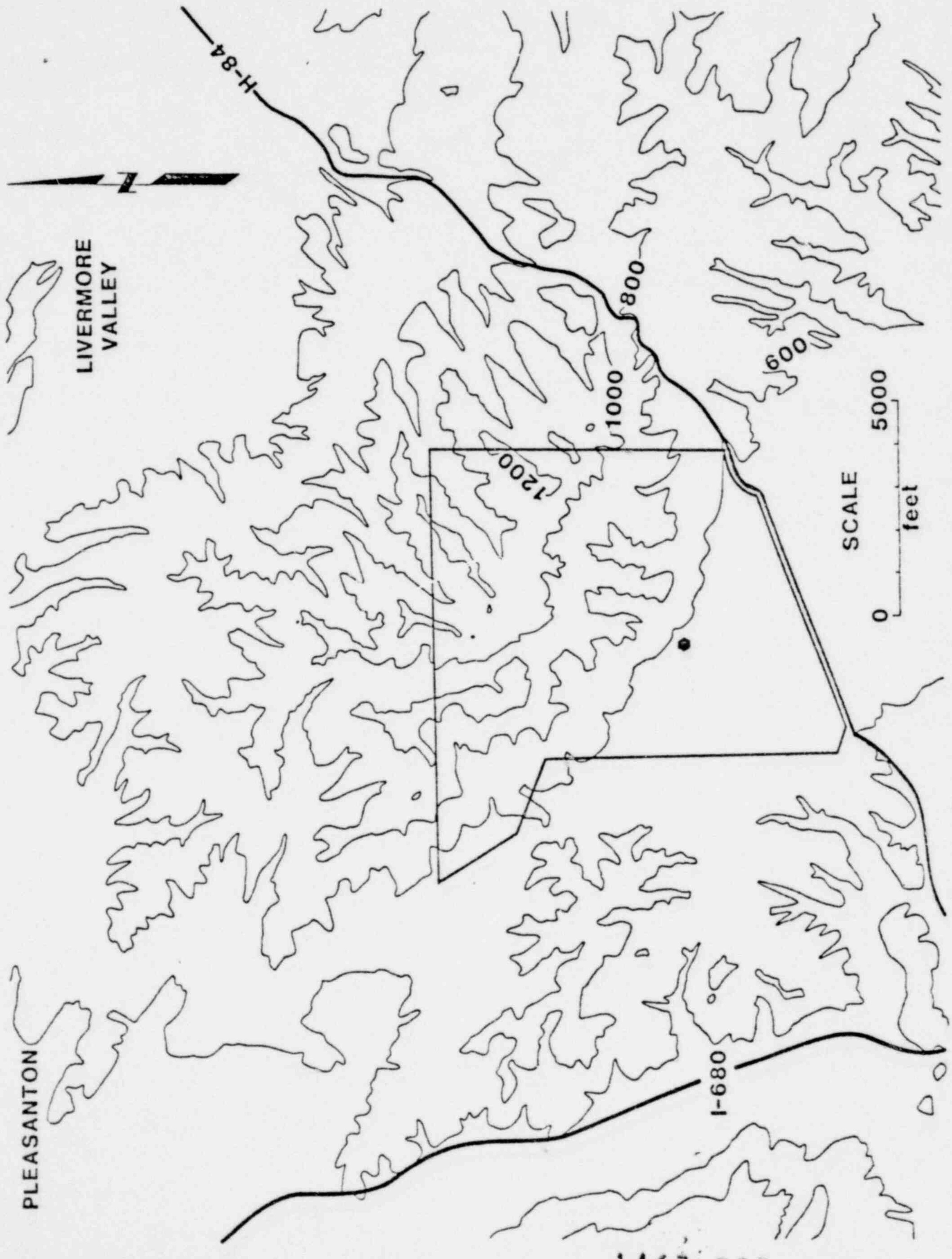
  ROY SHLEMON

- INTERPRETATIONS AND CONCLUSIONS

  RICHARD HARDING

POOR ORIGINAL

1463 002

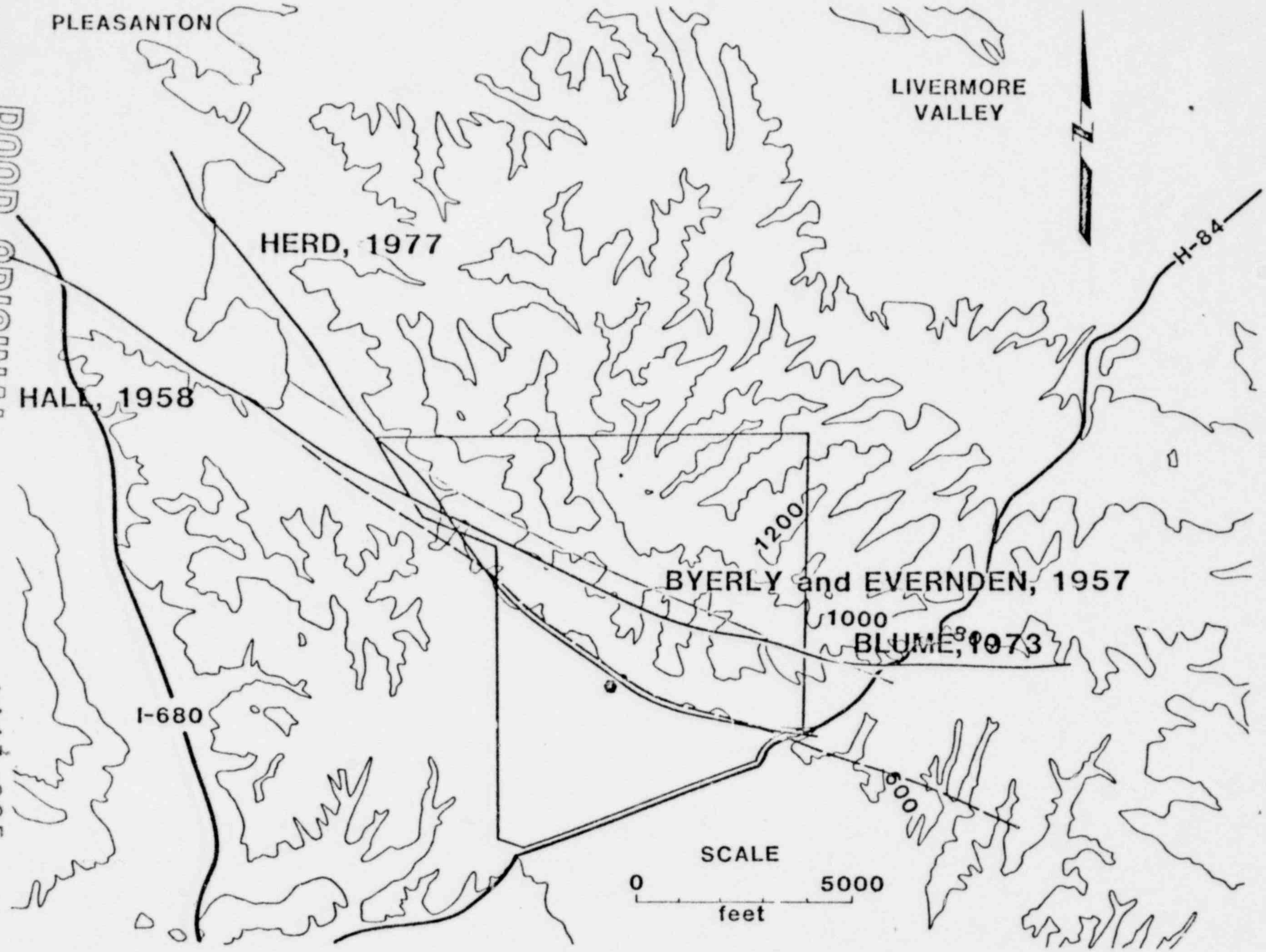

LIVERMORE VALLEY
PLEASANTON
H-84
I-680
800
600
1000
1200
SCALE
0
5000
feet

PHASE I GEOLOGIC INVESTIGATIONS

INITIAL OBJECTIVE -- INVESTIGATE MAPPED VERONA FAULT AND ASSOCIATED PHOTOLINEAMENTS

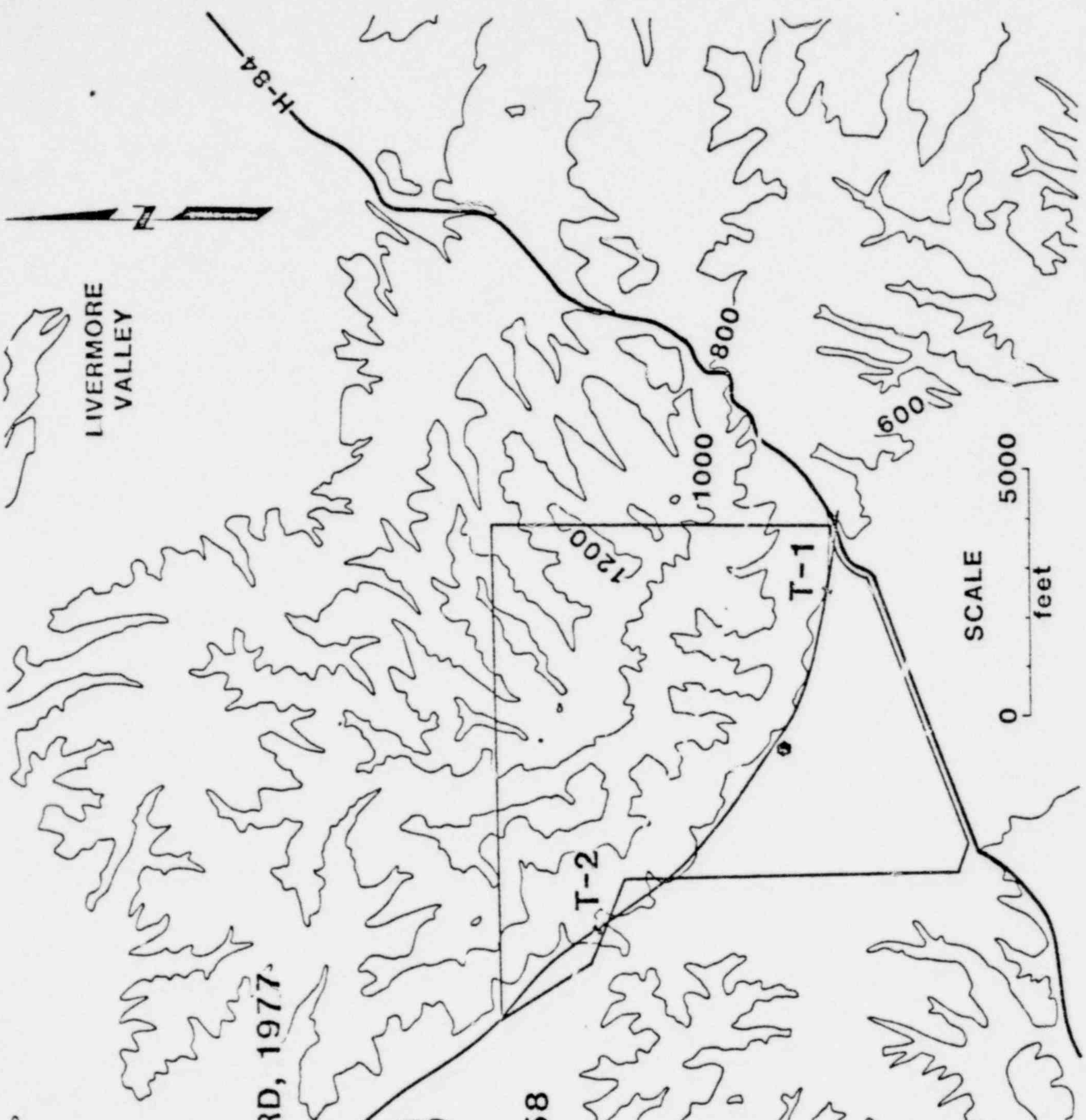
INITIAL SCOPE OF WORK -- REVIEW OF EXISTING LITERATURE
-- PHOTOINTERPRETATION
-- LIMITED TRENCHING

1463 004

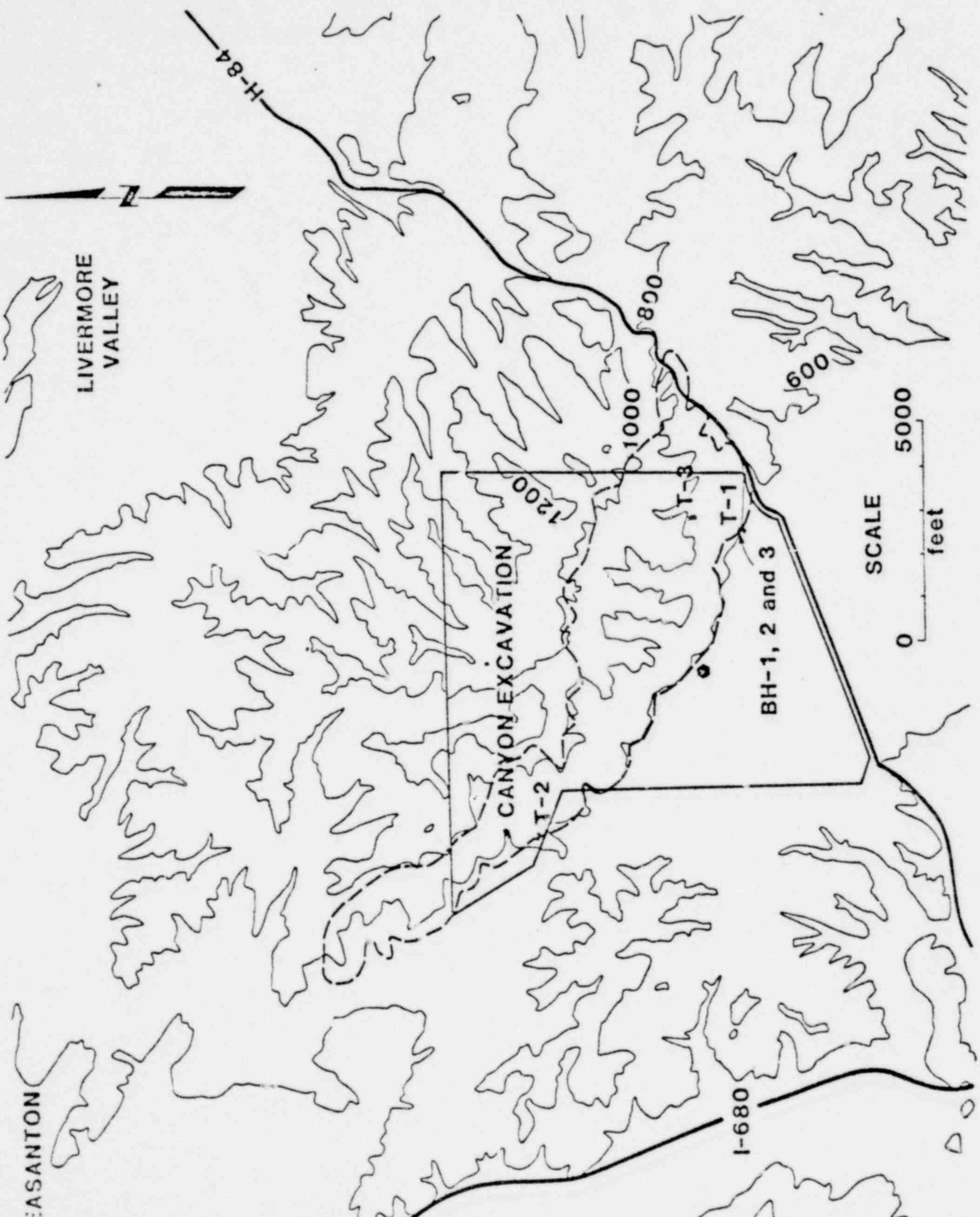

PLEASANTON
LIVERMORE
VALLEY
N
H-84
HERD, 1977
HALL, 1958
1200
BYERLY and EVERNDEN, 1957
1000
BLUME, 1973
I-680
600
SCALE
0
5000
feet

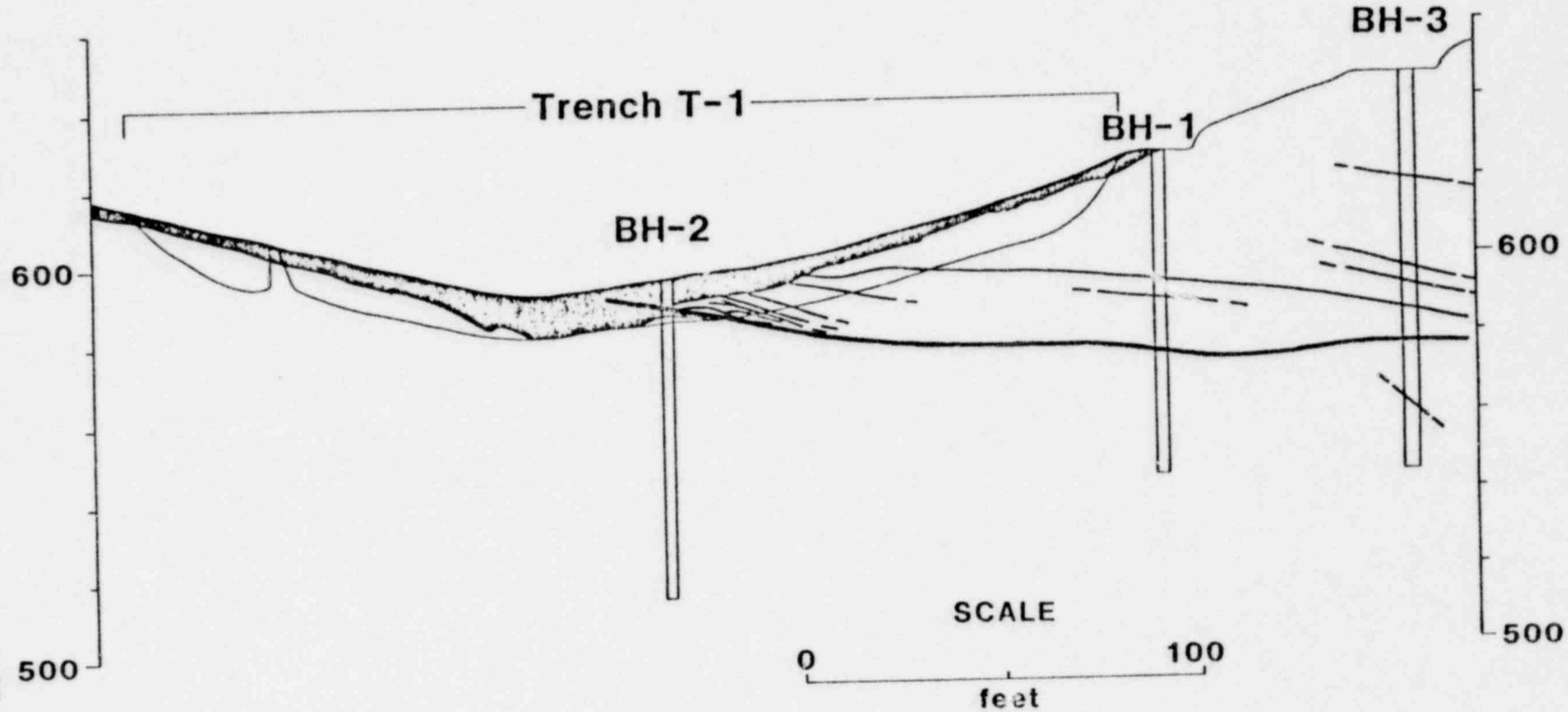
1463 005

POOR ORIGINAL

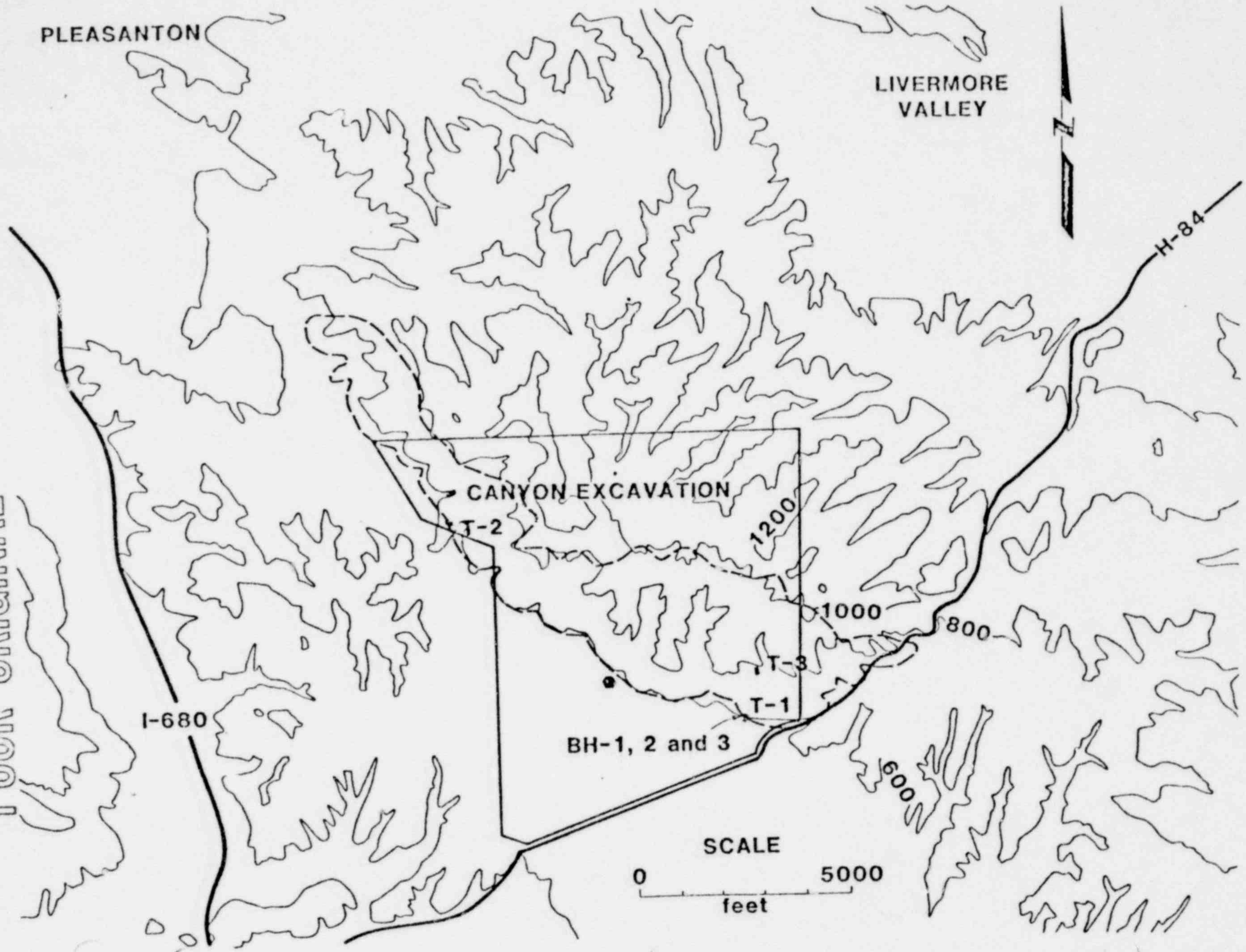


1463 007


LIVERMORE VALLEY
H-84
800
600
1000
1200
T-3
T-1
T-2
CANYON EXCAVATION
BH-1, 2 and 3
SCALE
0
5000
feet
I-680
PLEASANTON

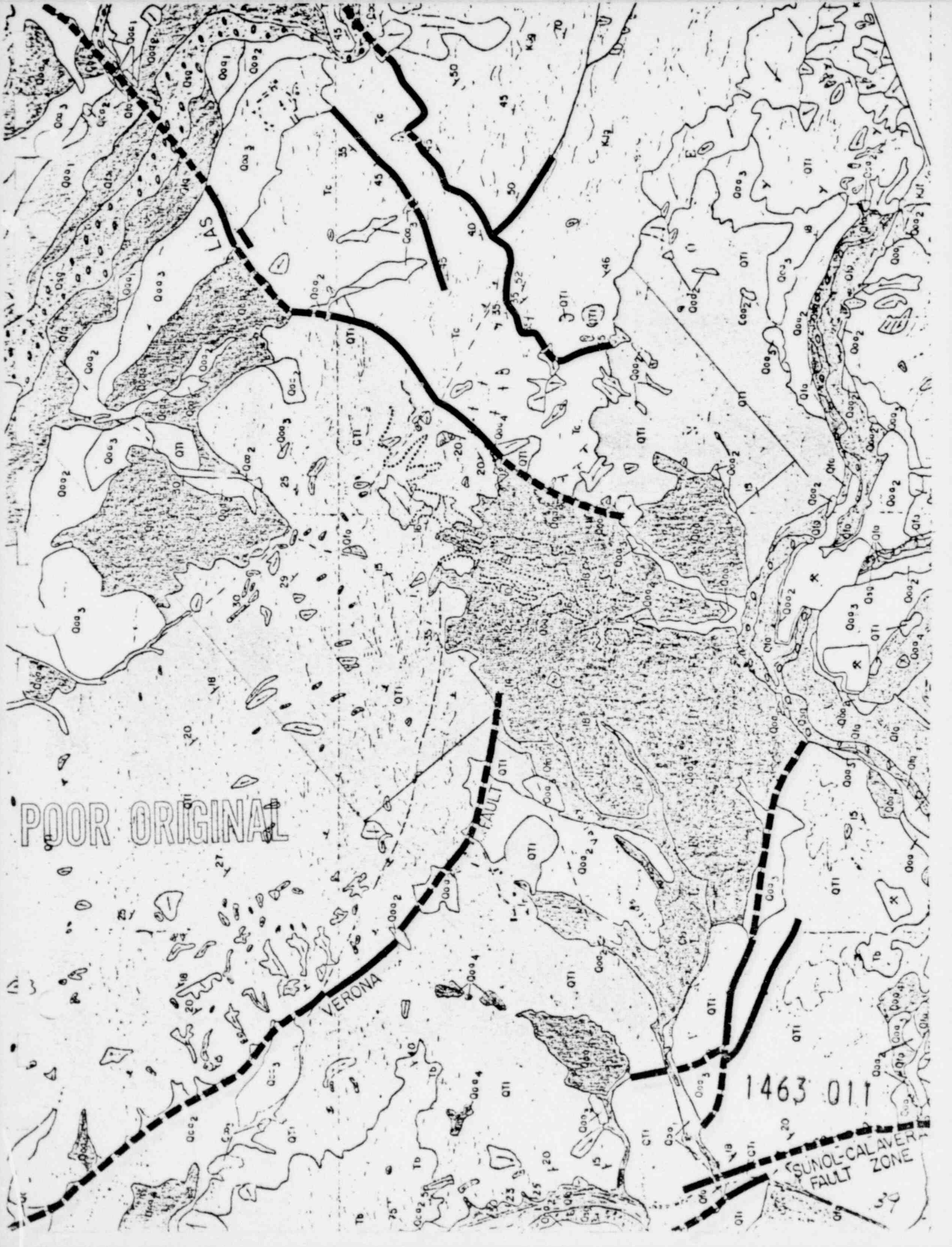

Trench T-1
BH-1
BH-2
BH-3
600
500
600
500
ELEVATION, feet
SCALE
0
100
feet

1463 009


PLEASANTON
LIVERMORE VALLEY
N
H-84
CANYON EXCAVATION
1200
1000
800
600
T-1
T-2
T-3
BH-1, 2 and 3
I-680
SCALE
0
5000
feet

1463 010

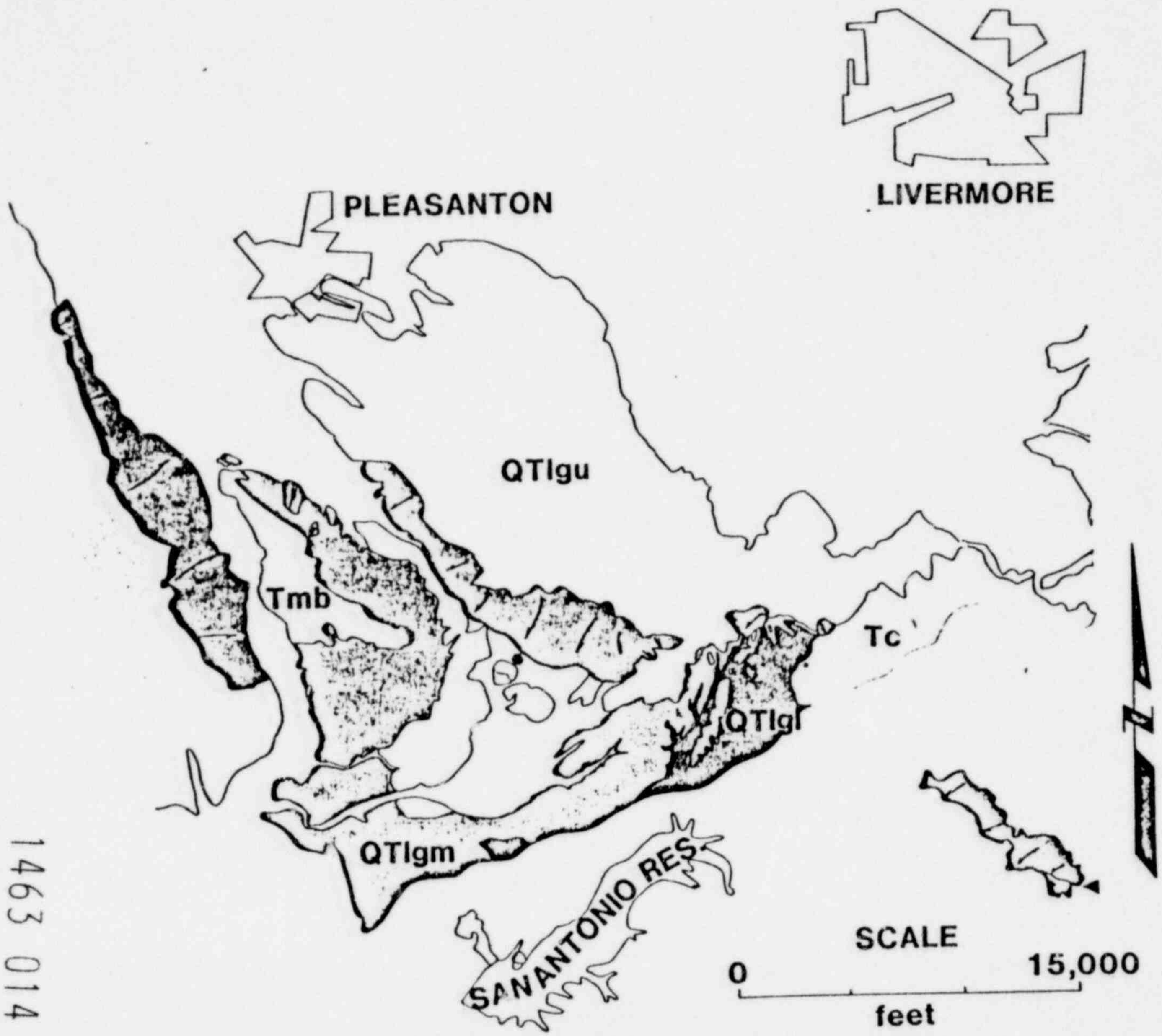
32



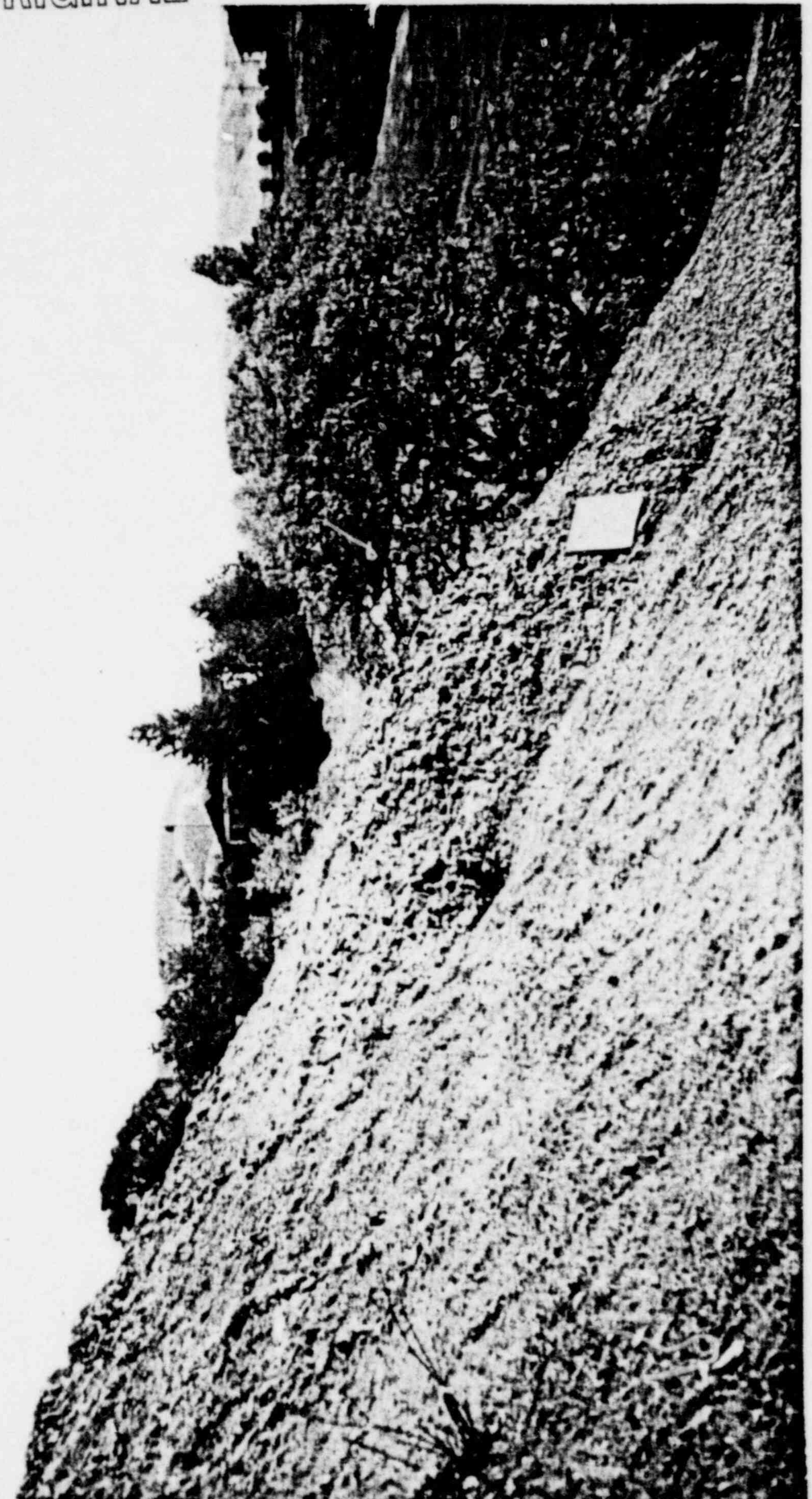
1463 011

1463 012

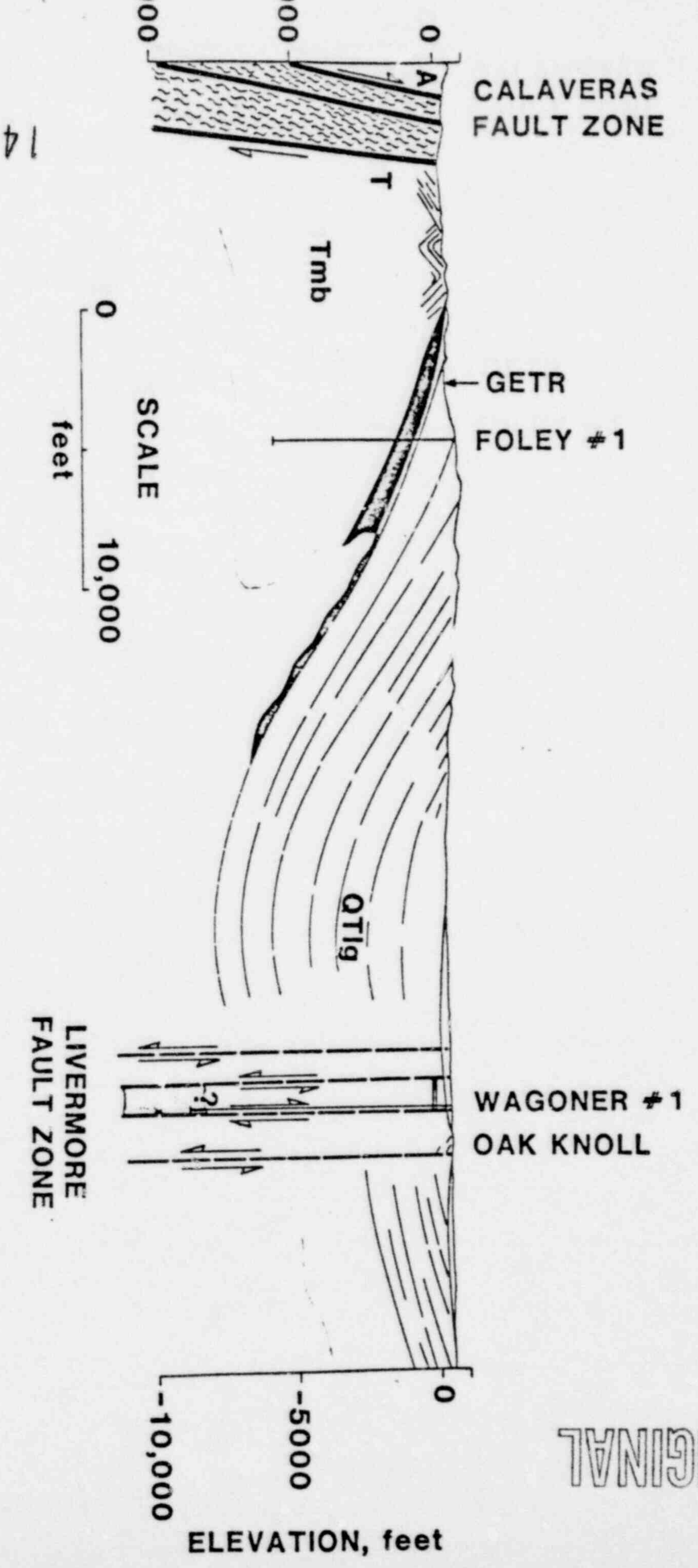
1463 013


LIVERMORE
PLEASANTON
QTlgu
Tmb
Tc
QTlgl
QTlgm
SAN ANTONIO RES
SCALE
0
15,000
feet

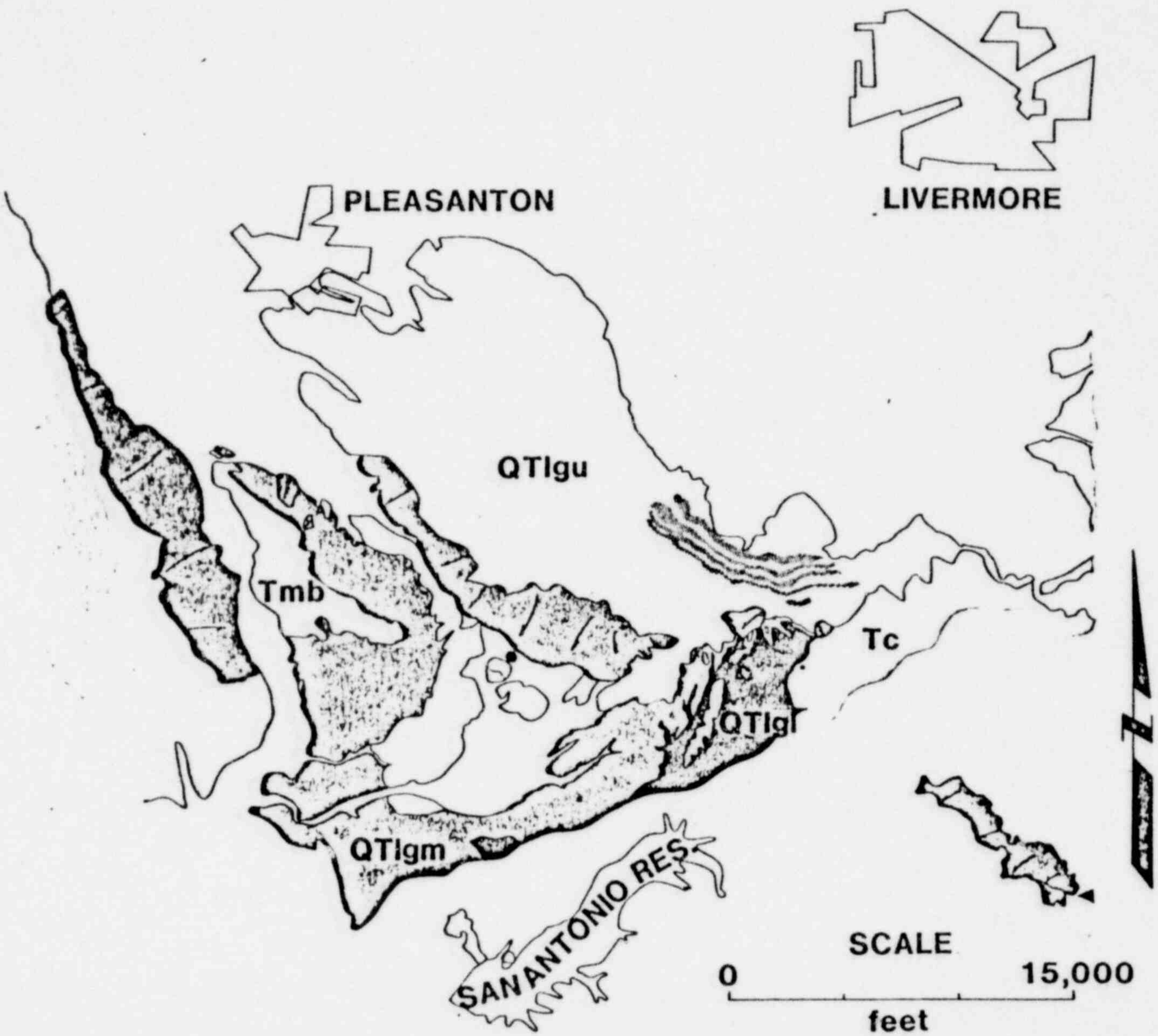
1463 014



1463 015 43 44



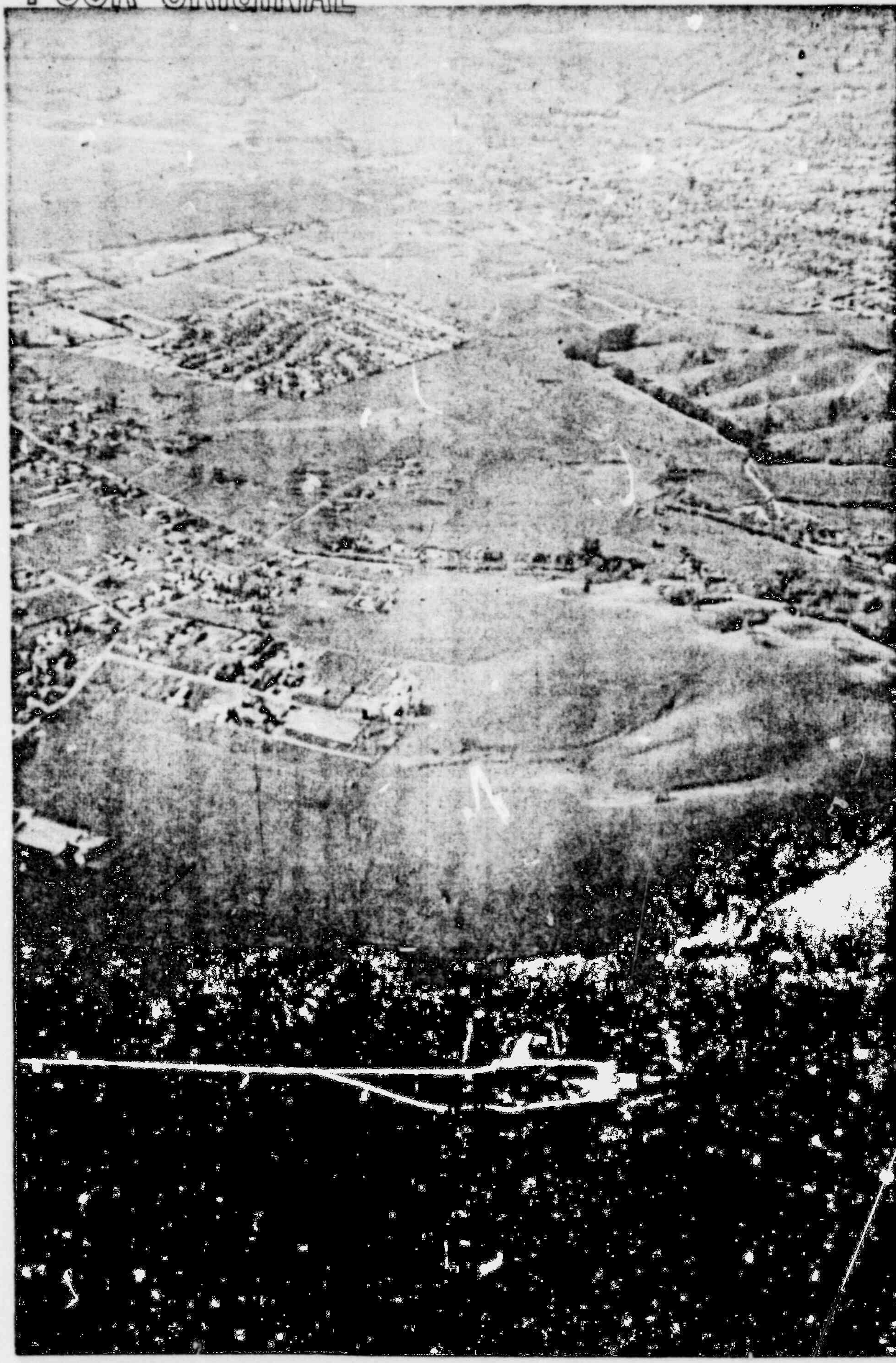
CALAVERAS
FAULT ZONE
A
T
Tmb
GETR
FOLEY #1
QTlg
WAGONER #1
OAK KNOLL
LIVERMORE
FAULT ZONE
SCALE
feet
0
10,000
0
-5000
-10,000
ELEVATION, feet



POOR ORIGINAL

1463 017

POOR ORIGINAL



1463 018 4?

CONCLUSIONS OF PHASE I INVESTIGATIONS

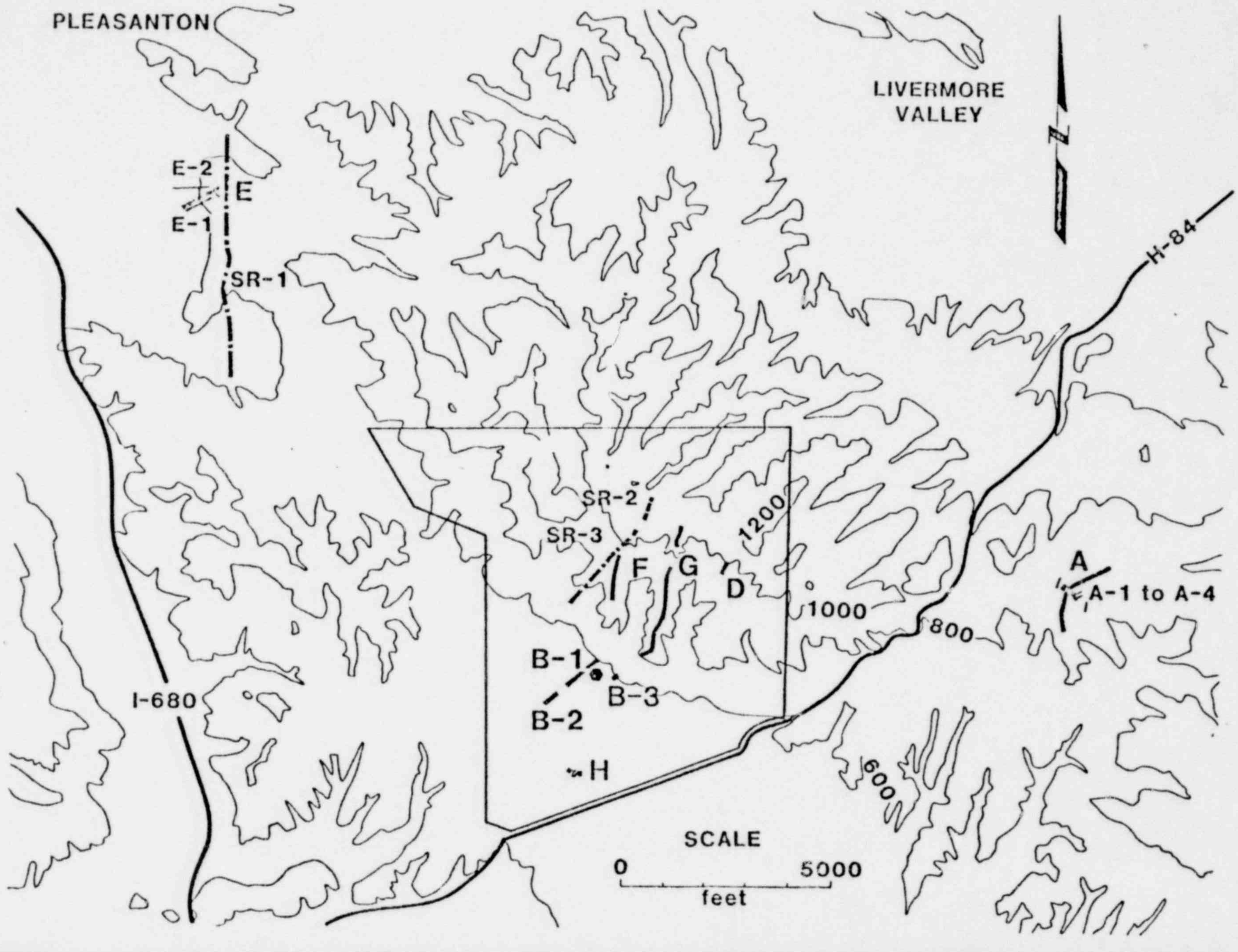
- LIVERMORE GRAVELS CONSIST OF THREE DISTINCT, MAPPABLE UNITS
- LOW ANGLE HILLFRONT SHEARS DELINEATE TOE OF LARGE, ANCIENT SLIDE COMPLEX
- STRATIGRAPHIC RELATIONSHIPS PRECLUDE POST-LIVERMORE GRAVELS FAULTING THROUGH FOLEY NO. 1 WELL
- UNBROKEN QT_{lgm} LIMITS EXTENSION OF MAPPED FAULT TRACES ALONG STRIKE TO SE
- EVIDENCE CITED FOR NW END OF VERONA FAULT MORE READILY EXPLAINED BY OTHER GEOLOGIC CONDITIONS
- POSTULATION OF FAULTING FROM HILLFRONT TO NE CONSTRAINED TO NARROW GAP IN HIGHWAY 84 PASS AREA

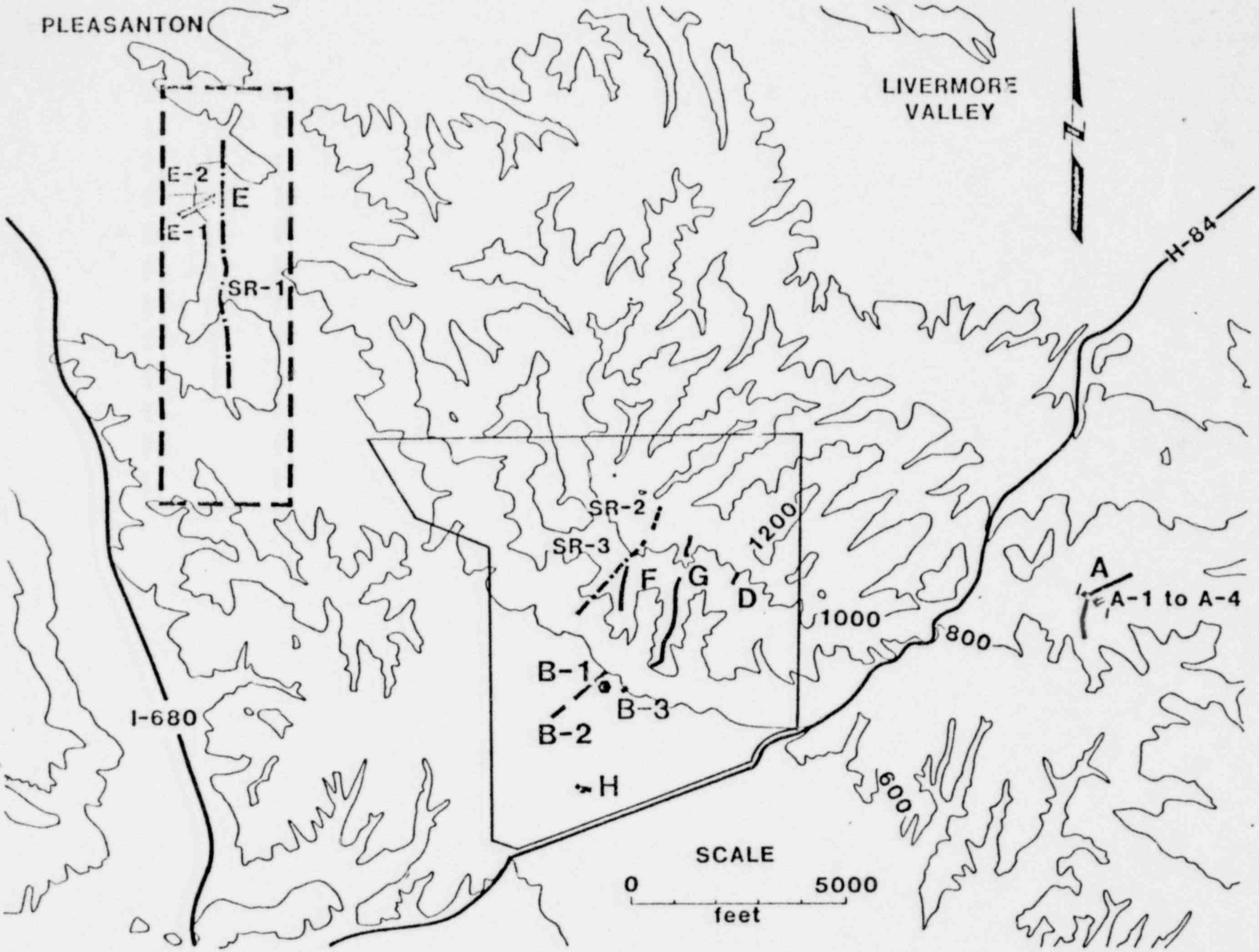
1463 019

NRC PHASE I REVIEW
REQUESTS FOR ADDITIONAL INVESTIGATIONS

- NW END OF MAPPED VERONA FAULT
- THINNING AND APPARENT STRATIGRAPHIC DISCORDANCE IN PASS AREA
- PHOTOLINEAMENTS / WET SPOTS SW OF GETR
- CHARACTER AND LIMITS OF ANCIENT LANDSLIDE COMPLEX

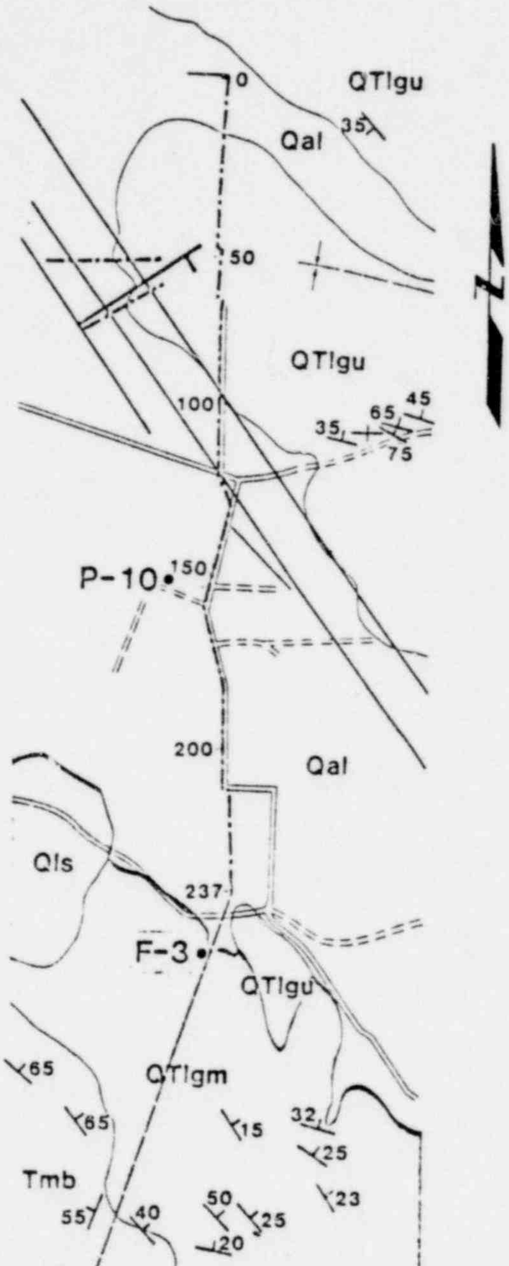
1463 020


PLEASANTON
LIVERMORE VALLEY
N
H-84
E-2
E
E-1
SR-1
SR-2
SR-3
F
G
D
1200
1000
800
600
A
A-1 to A-4
B-1
B-2
B-3
H
I-680
SCALE
0
5000
feet


PLEASANTON
LIVERMORE
VALLEY
N
H-84
I-680
E-2
E
E-1
SR-1
SR-2
SR-3
F
G
D
1200
1000
800
600
A
A-1 to A-4
B-1
B-2
B-3
H
SCALE
0
5000
feet

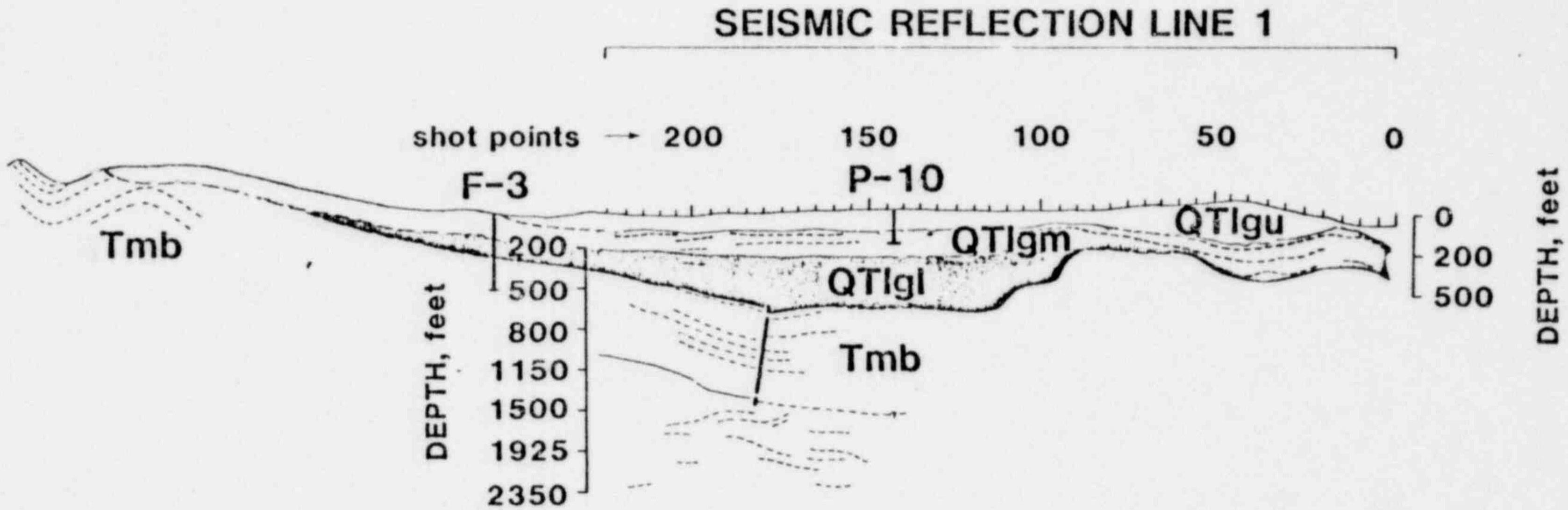
POOR ORIGINAL

1463 023


QTlgu
Qal
35
50
QTlgu
100
35
65
45
75
P-10
150
200
Qal
Qls
237
F-3
QTlgu
QTlgm
65
65
15
32
25
Tmb
55
40
50
25
23
20
SCALE
0
1000
feet

1463 024

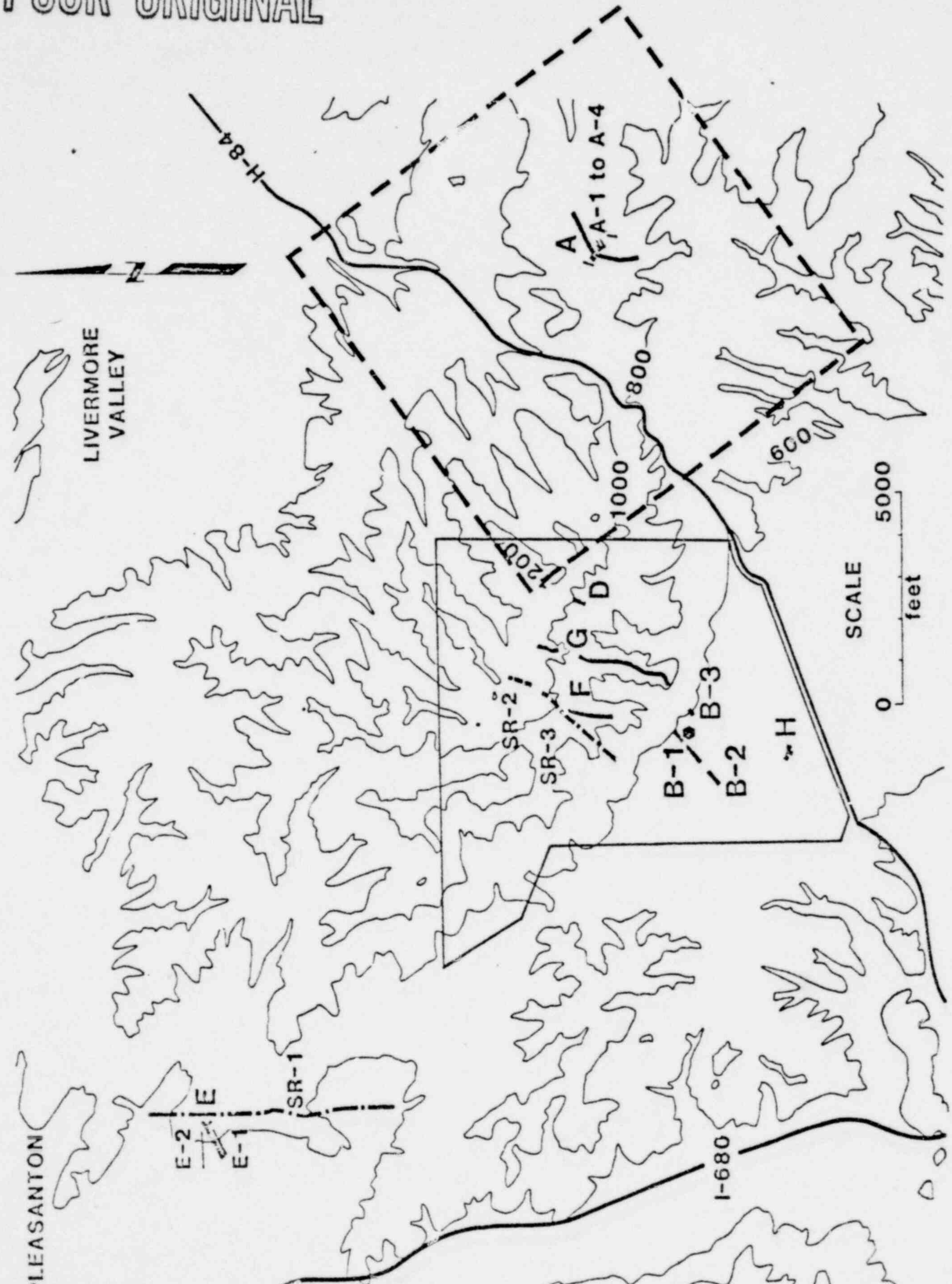
GEOLOGIC SECTION
HAPPY VALLEY



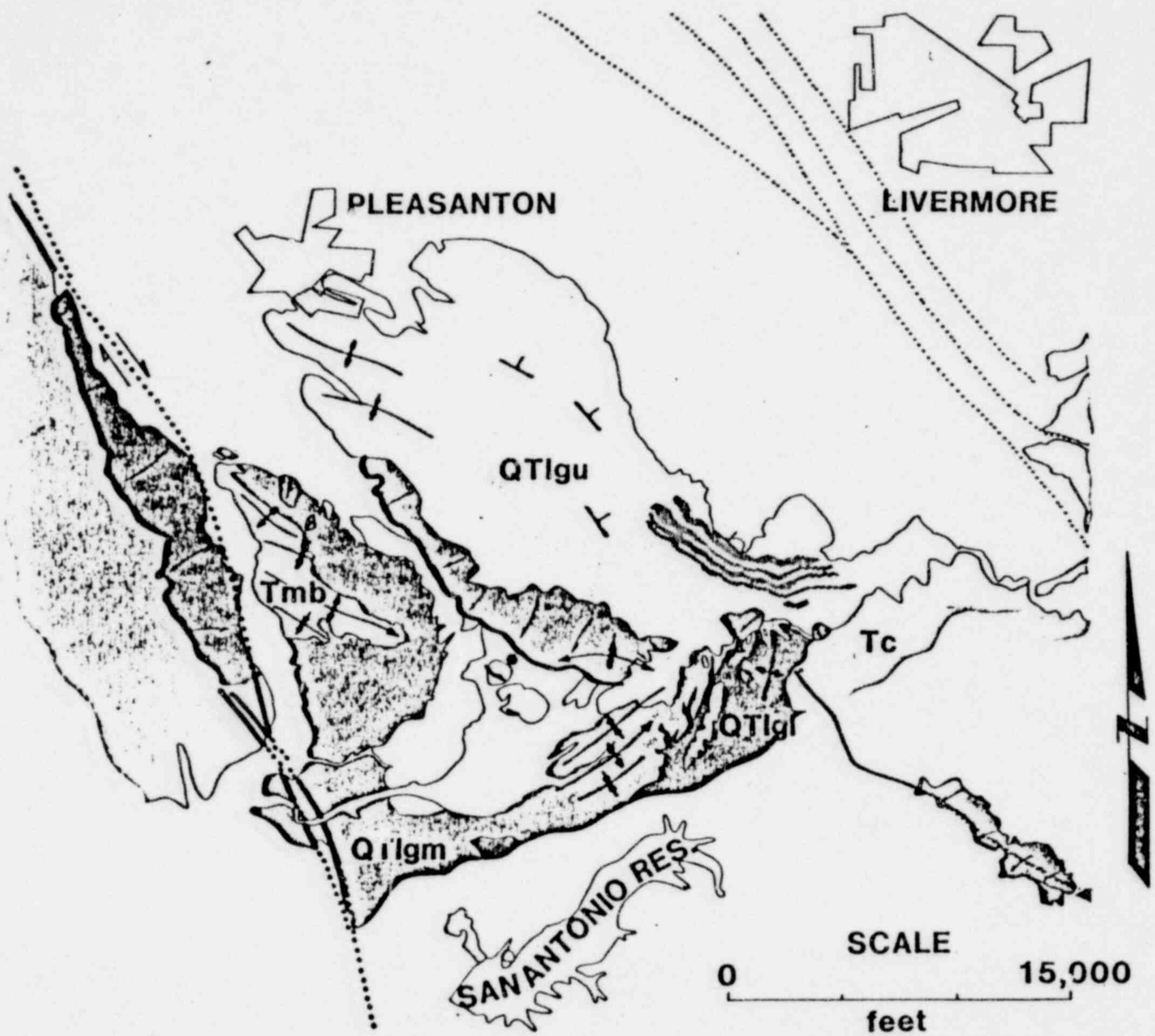
HORIZONTAL AND VERTICAL SCALES VARY

1463 025

POOR ORIGINAL



1463 026


PLEASANTON
LIVERMORE
QTlgu
Tmb
Tc
QTlgl
SAN ANTONIO RES
SCALE
0
15,000
feet

1463 027

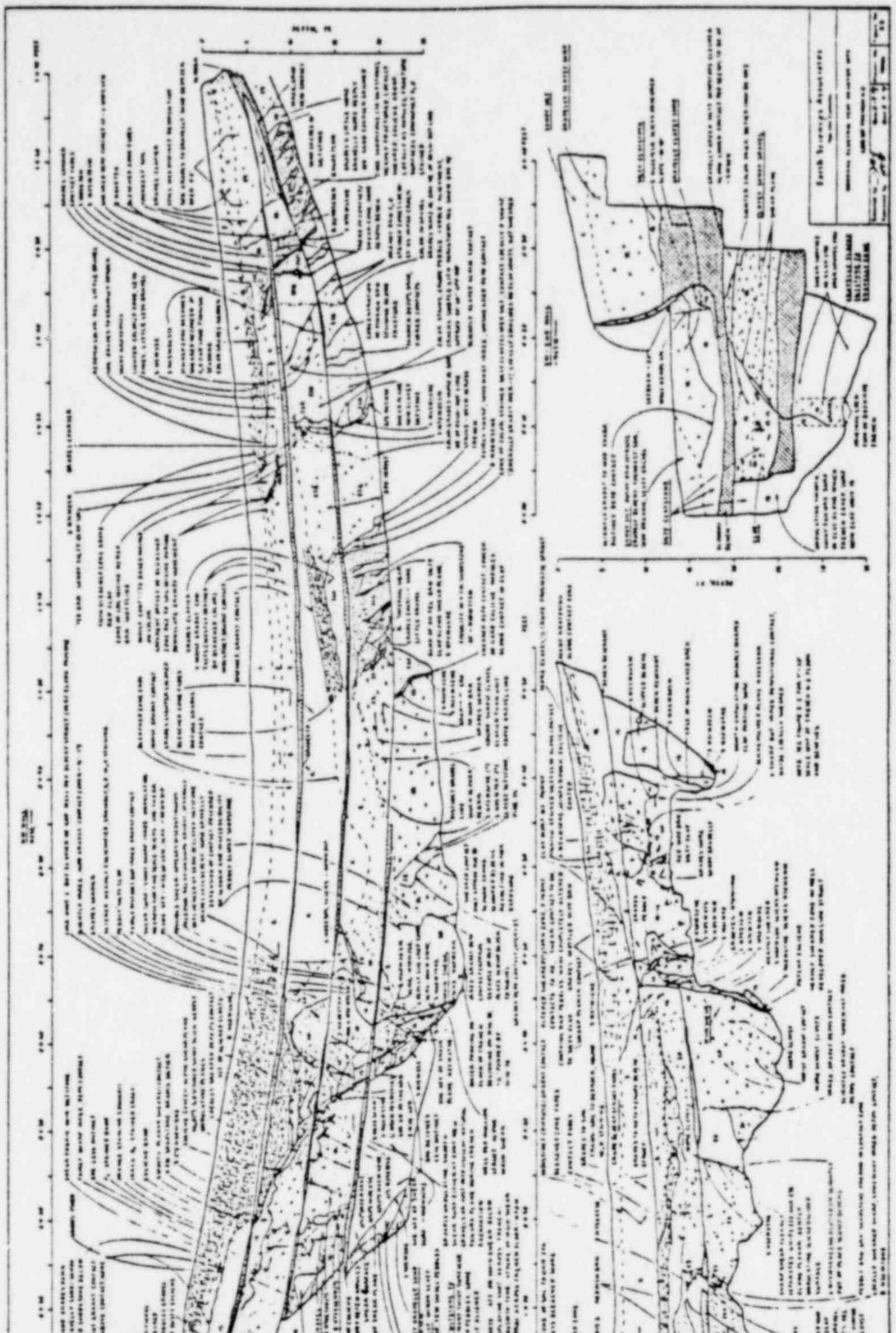
POOR ORIGINAL

1463 028

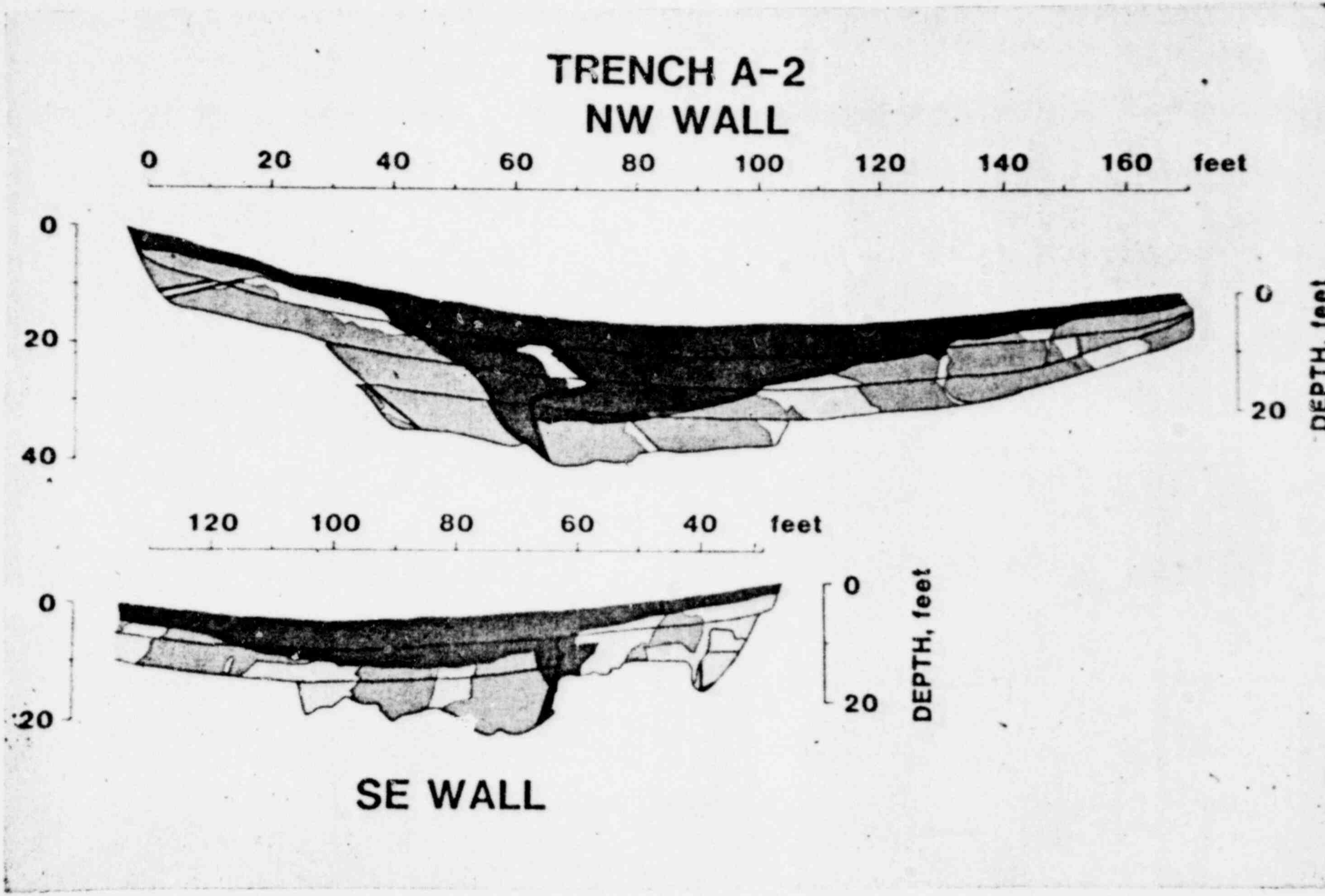
1463 030

1463 031

POOR ORIGINAL



1463 032

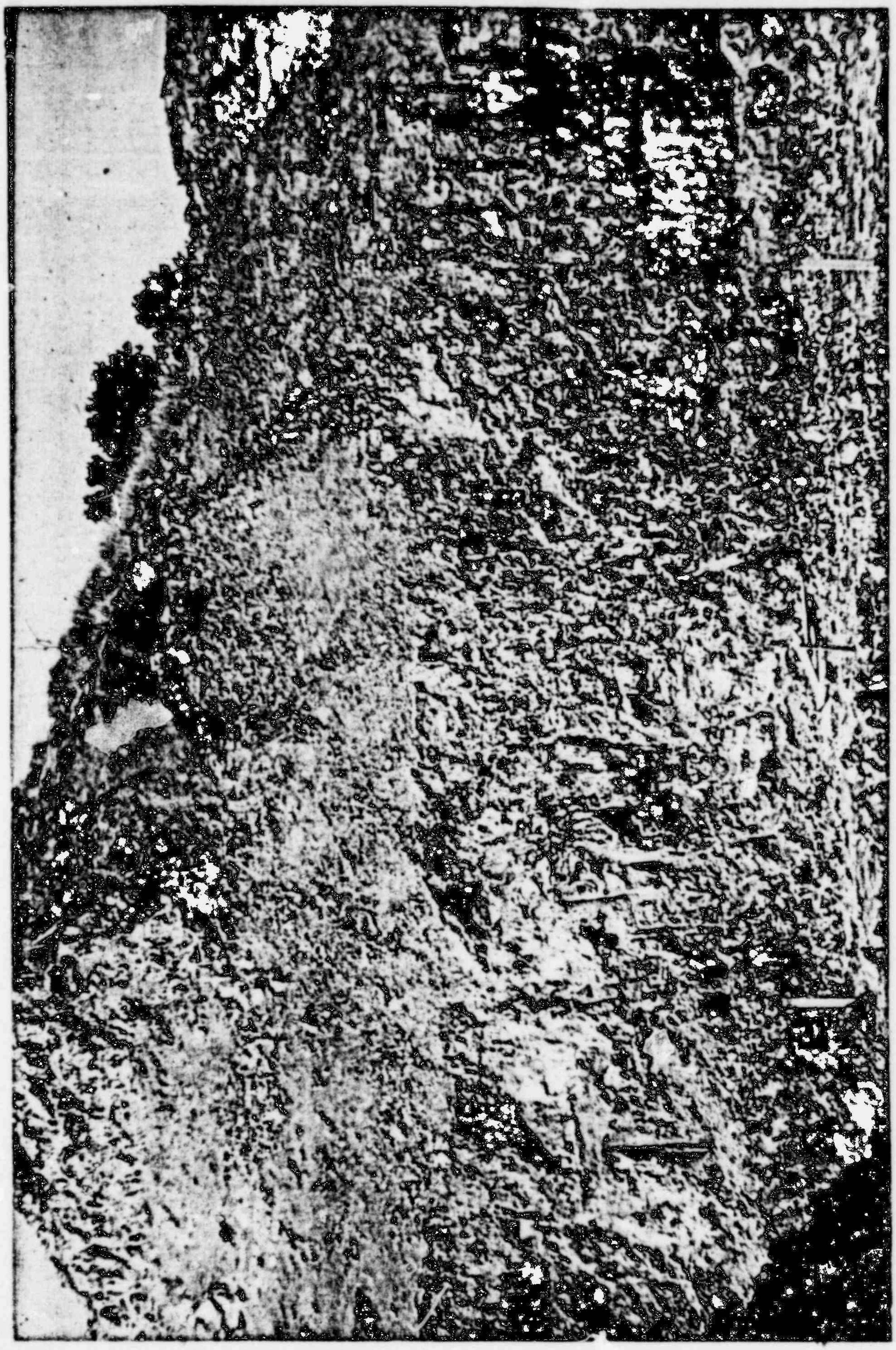

TRENCH A-2
NW WALL
0 20 40 60 80 100 120 140 160 feet
0
20
40
0
20
DEPTH, feet
120 100 80 60 40 feet
0
20
0
20
DEPTH, feet
SE WALL

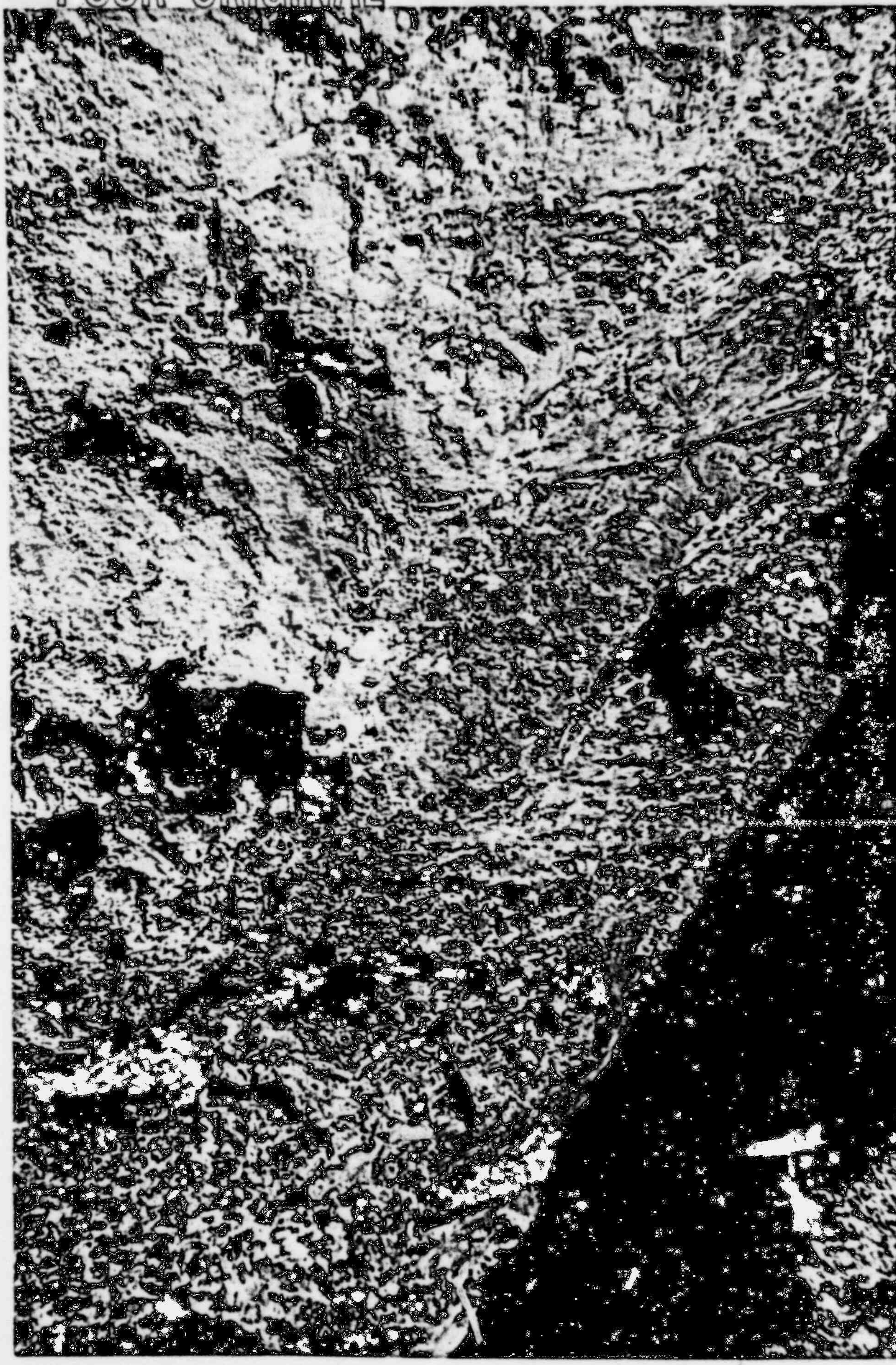
1463 033

POOR ORIGINAL

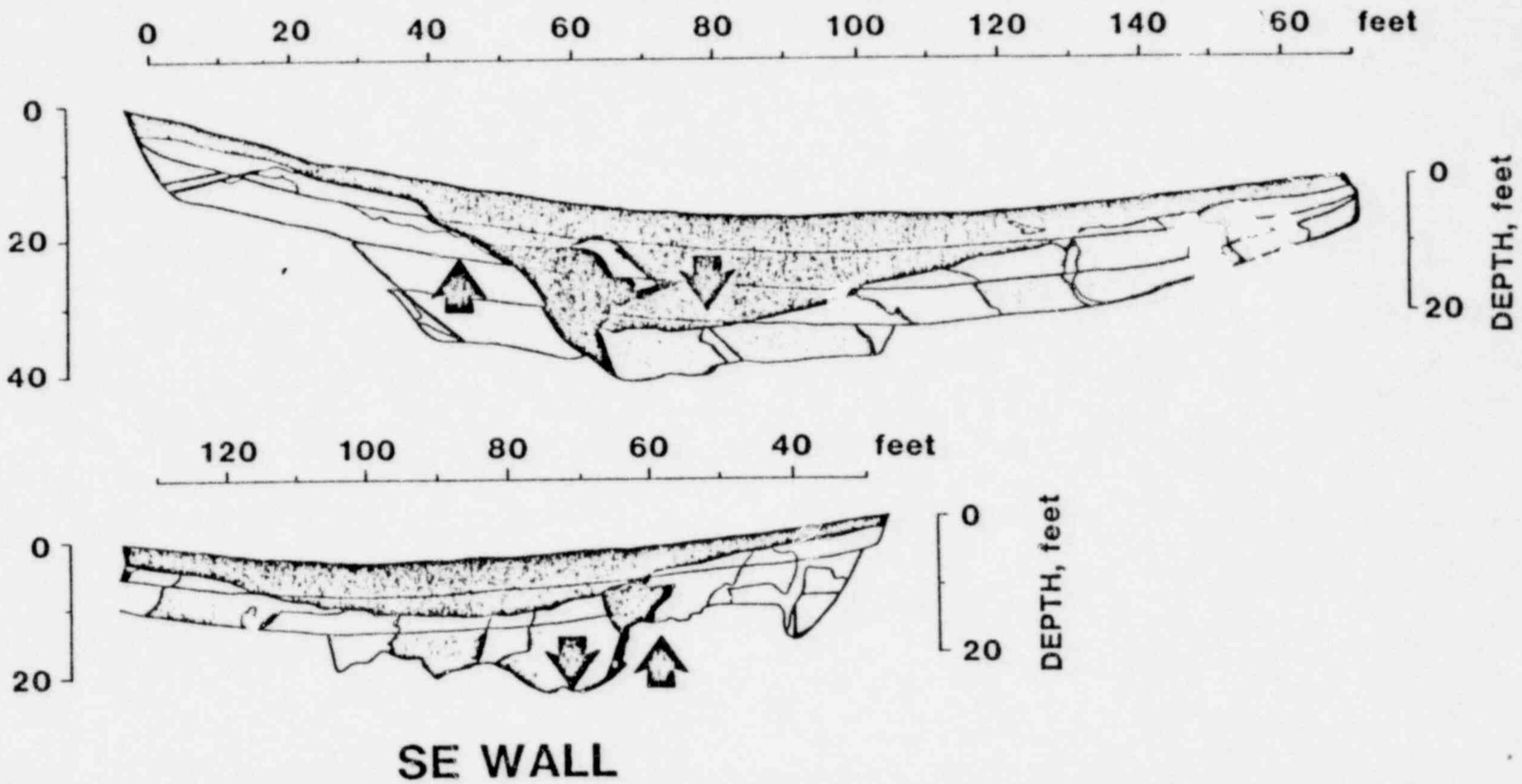
1463 034

POOR ORIGINAL



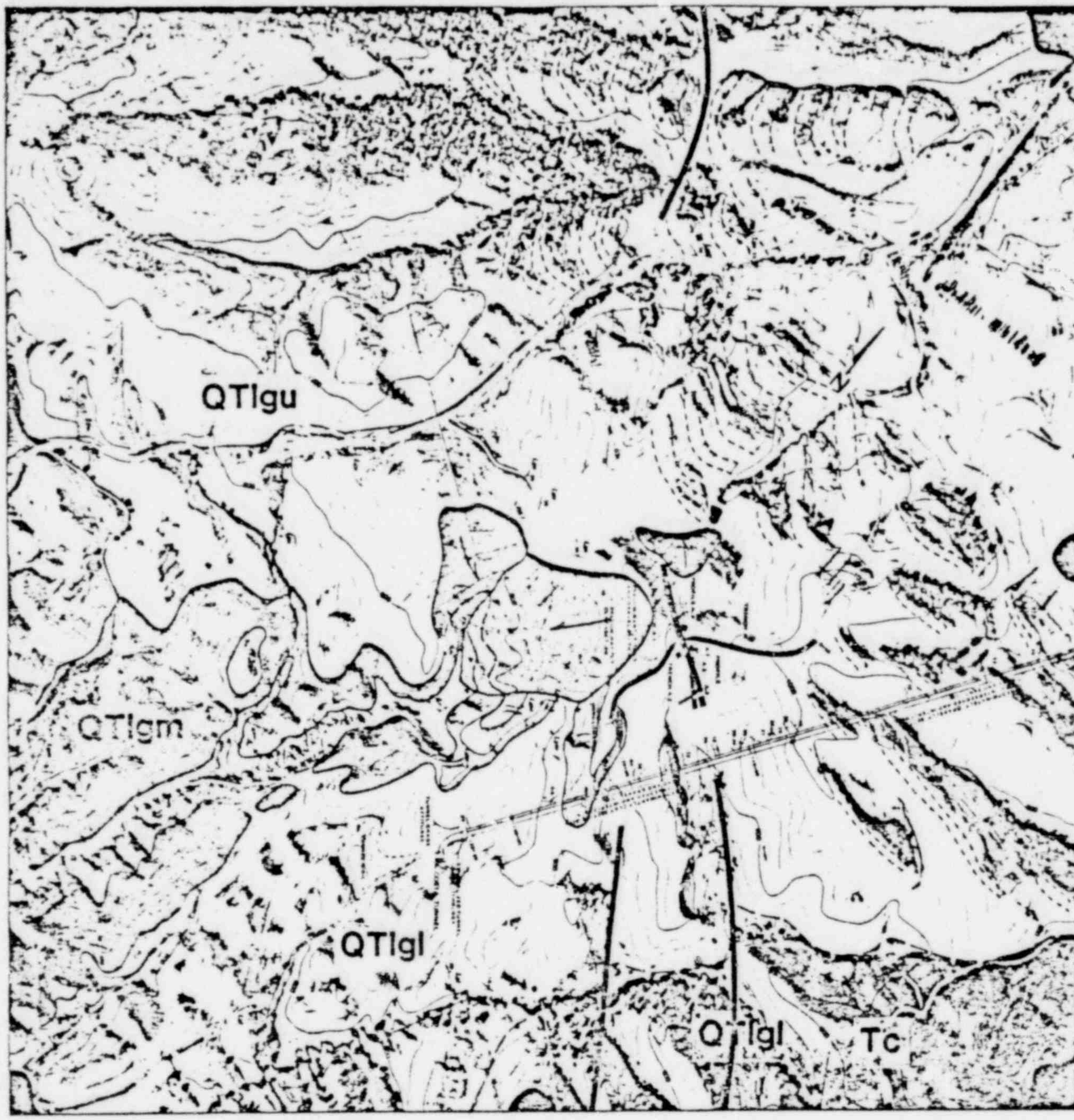


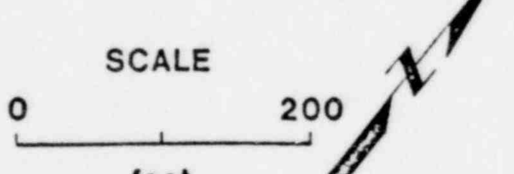
TRENCH A-2
NW WALL



1463 039

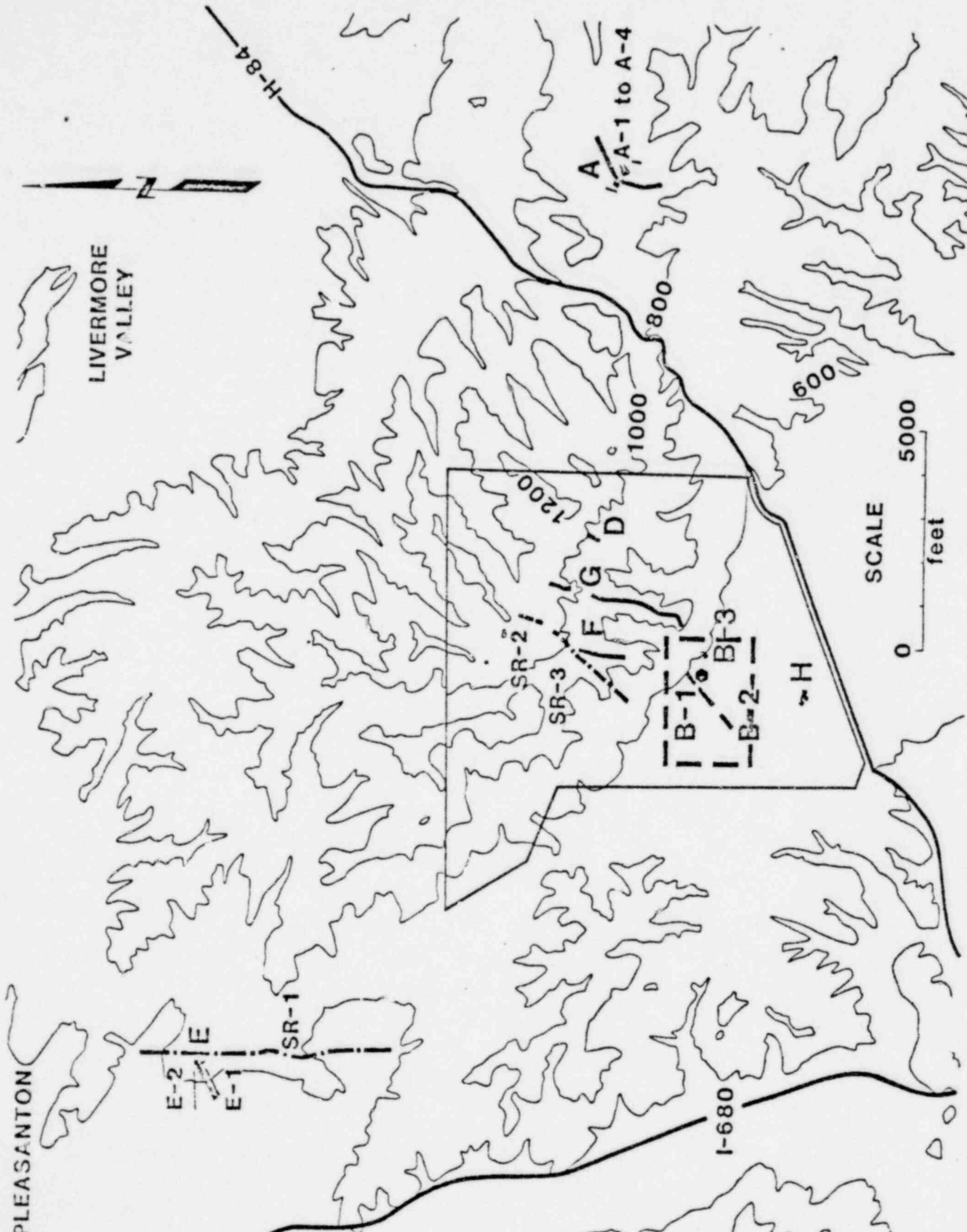
POOR ORIGINAL





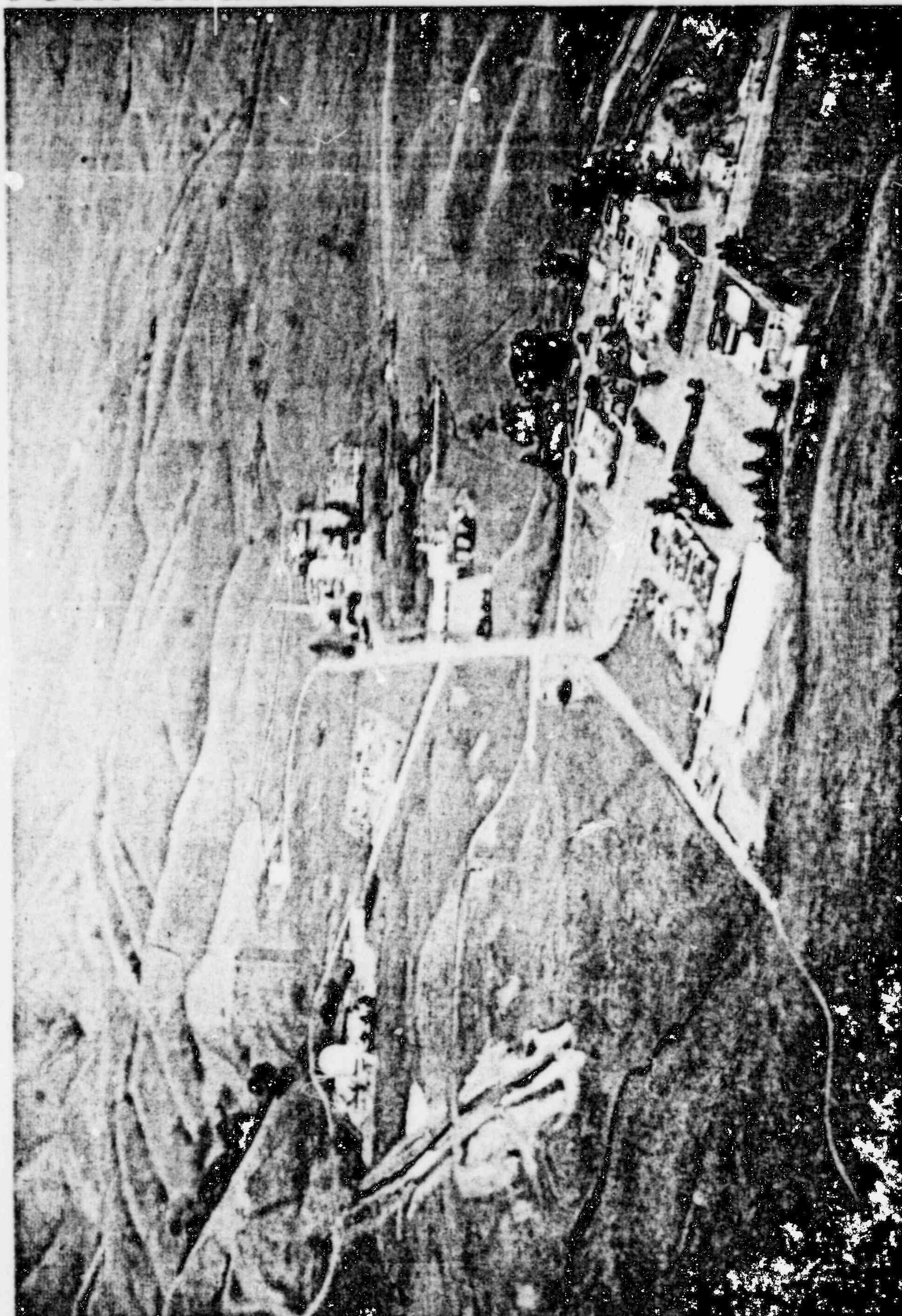
1463 040

POOR ORIGINAL

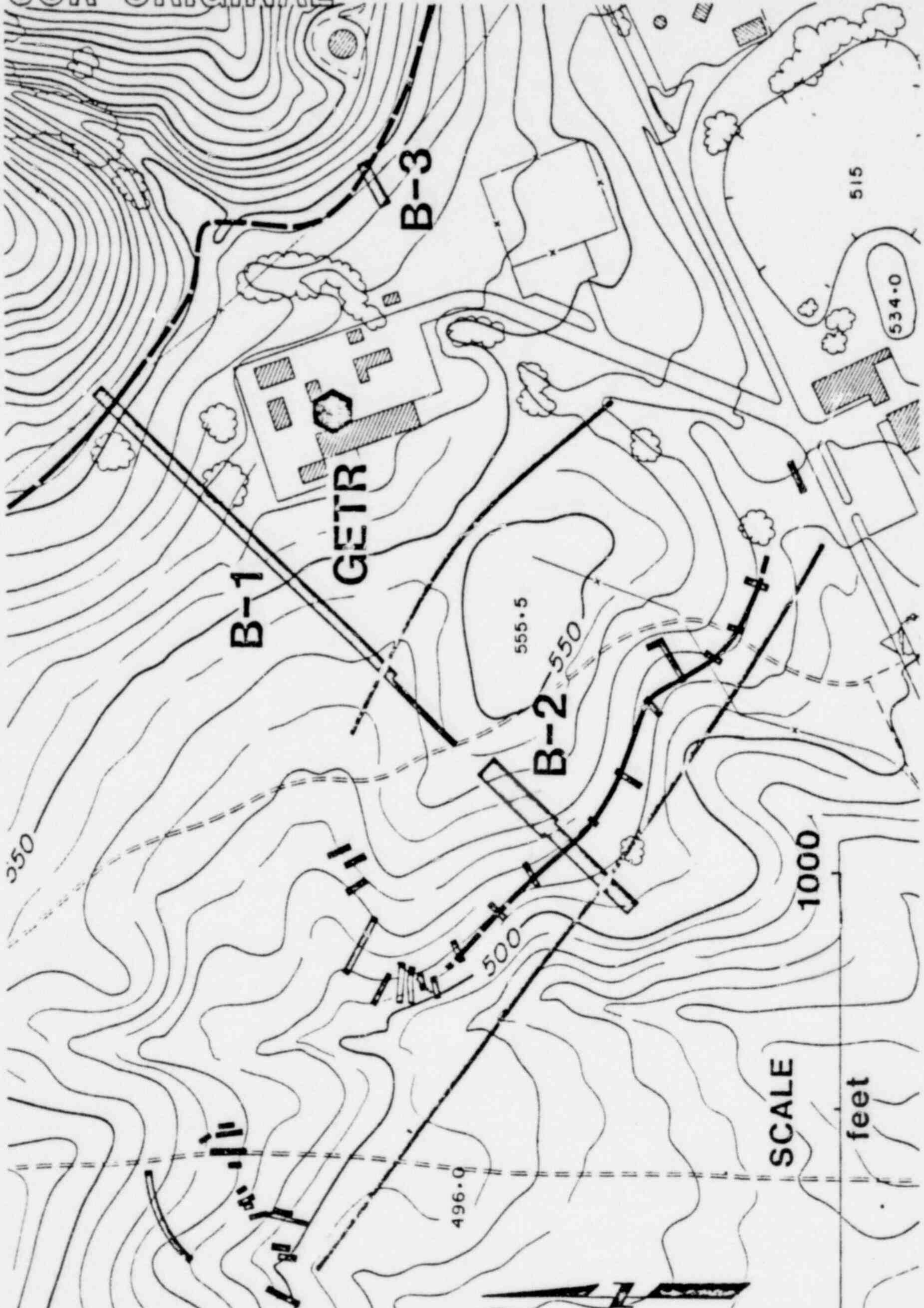


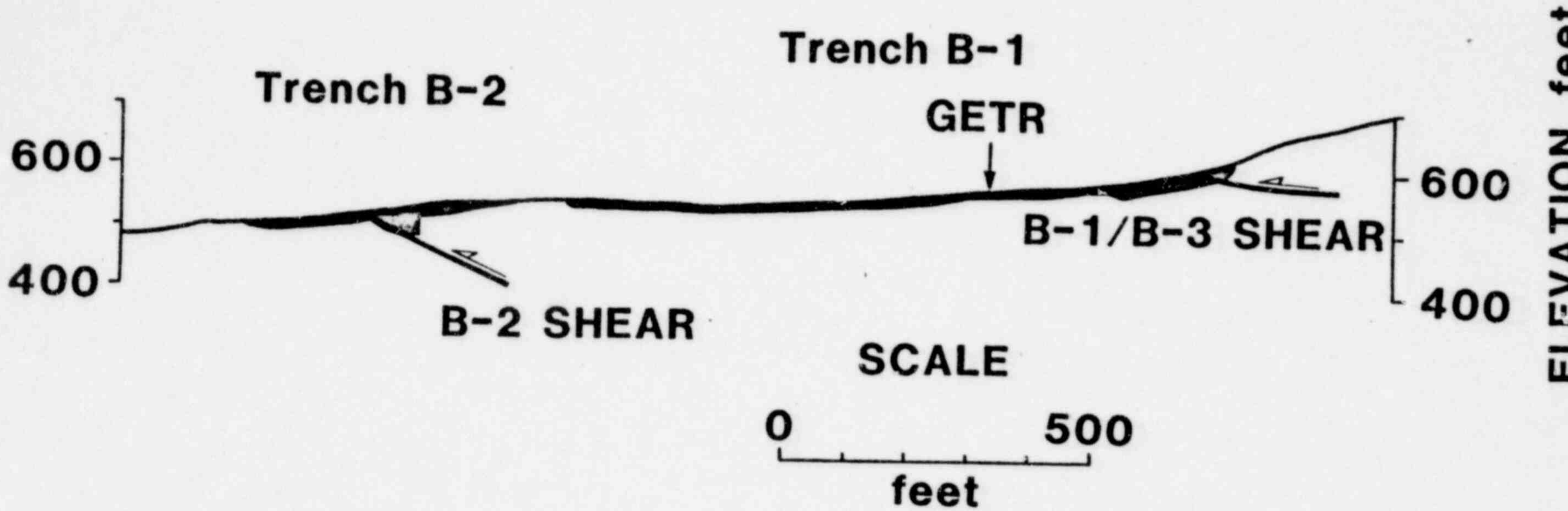
1463 041

POOR ORIGINAL



1463 042

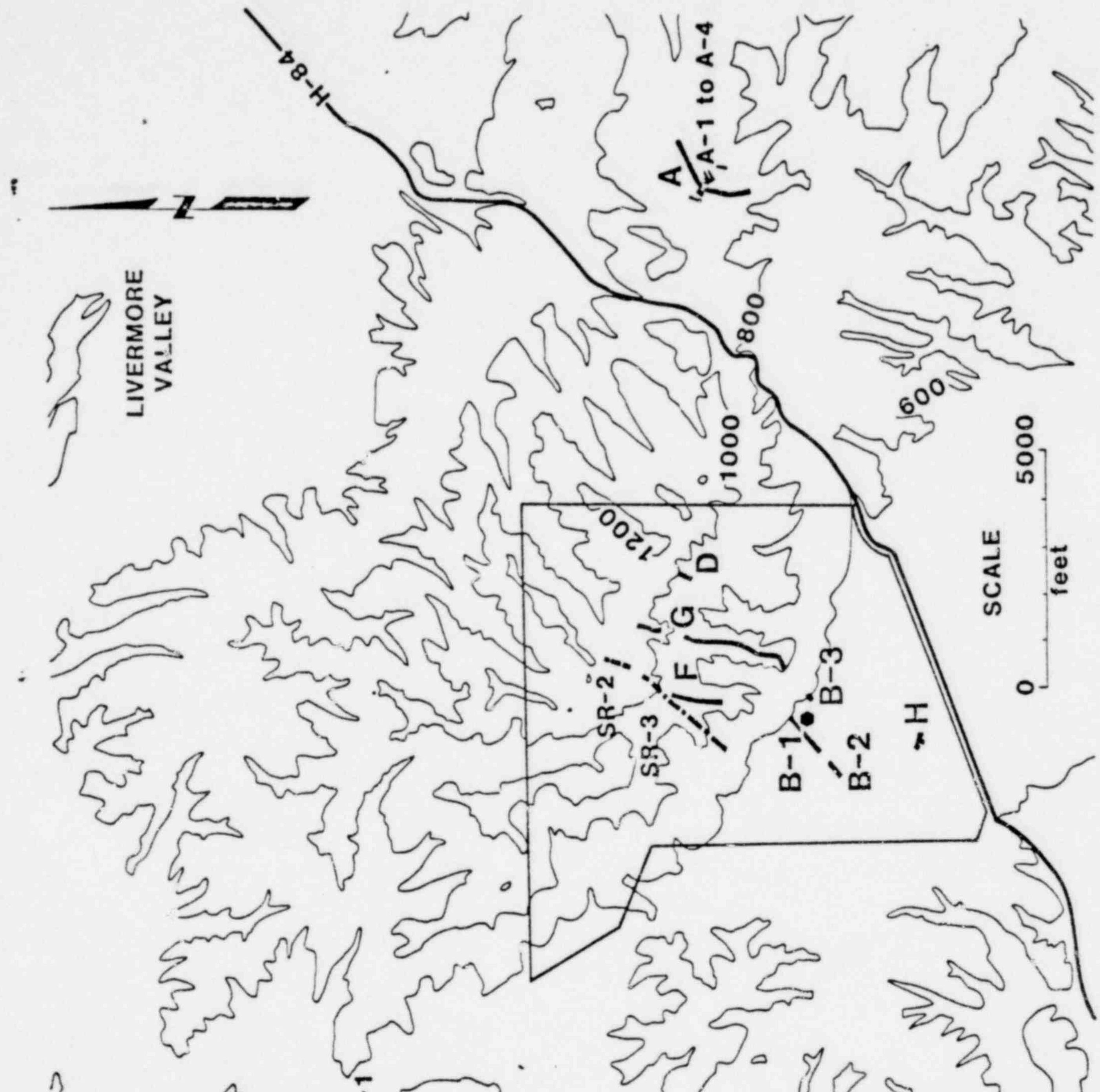

B-1
B-2
B-3
GETR
555.5
550
550
500
534.0
515
496.0
473.0
SCALE
0
1000
feet



Trench B-1
Trench B-2
GETR
600
400
B-2 SHEAR
B-1/B-3 SHEAR
600
400
ELEVATION, feet
SCALE
0
500
feet

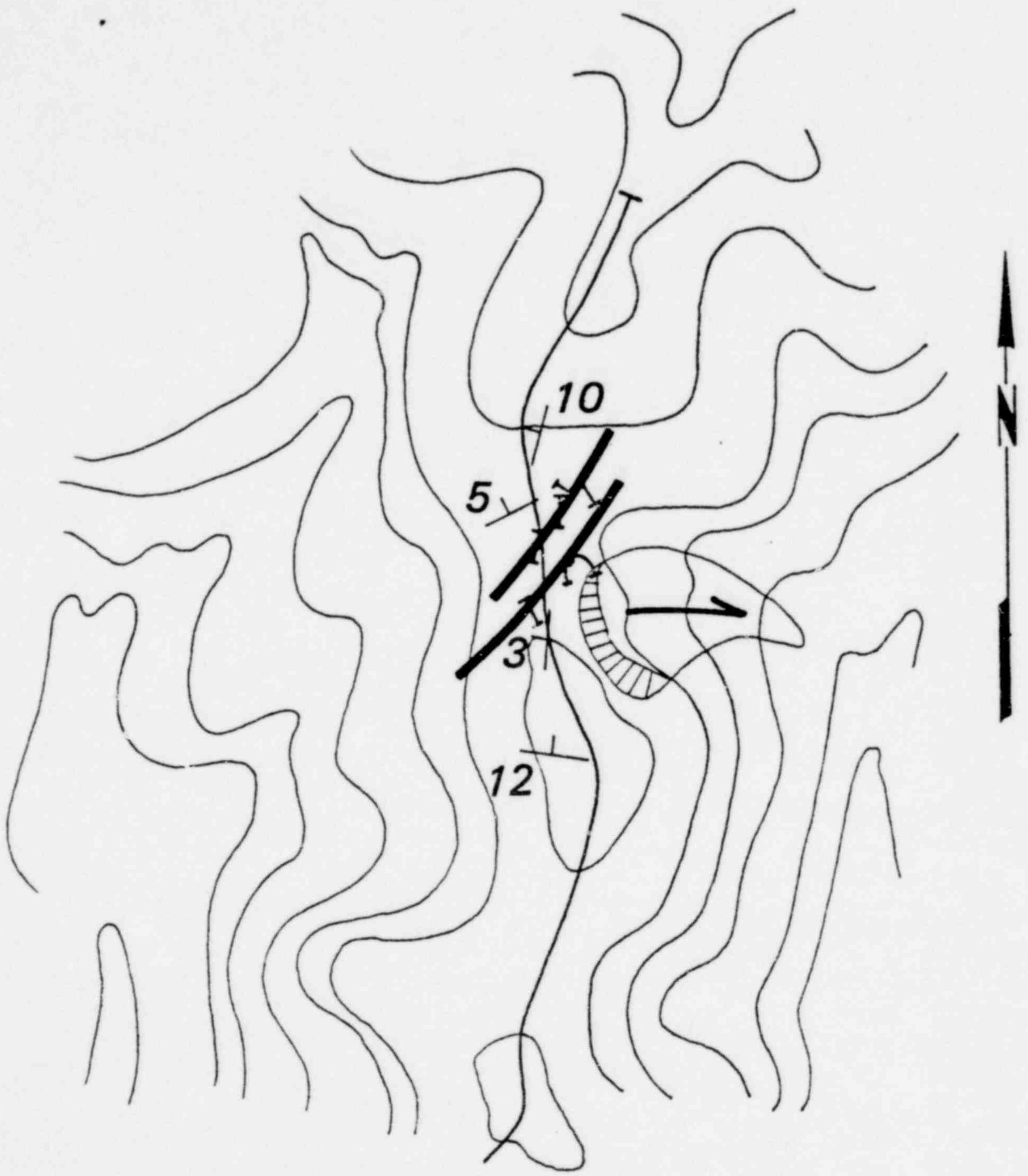
1463 044

POOR ORIGINAL



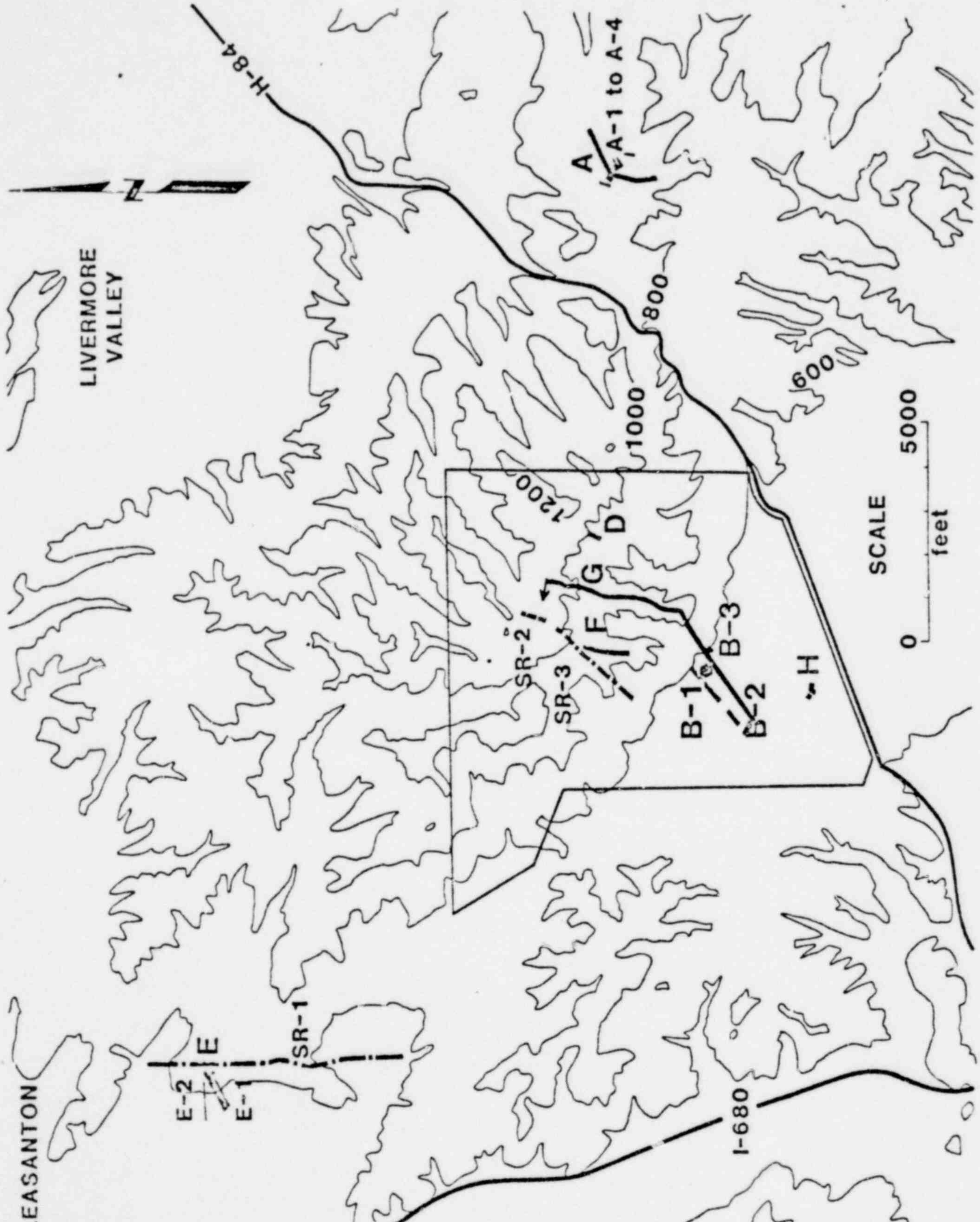
1463 045

POOR ORIGINAL



TRENCH G AREA

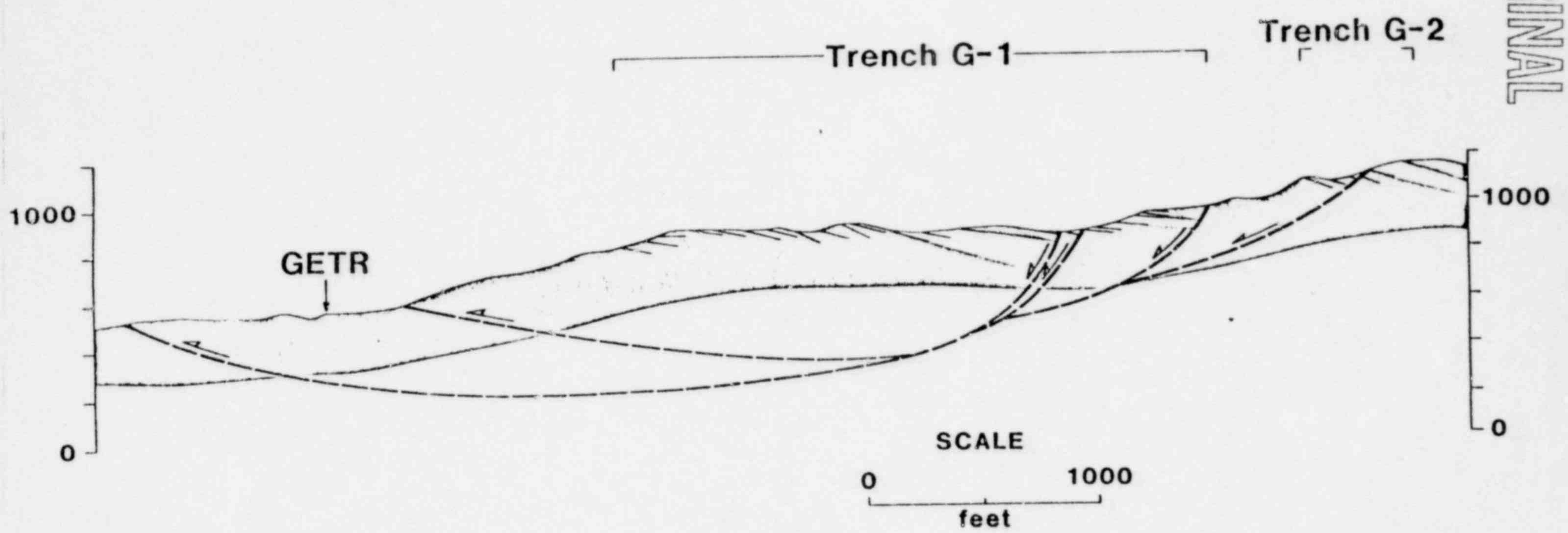
1463 046



N
LIVERMORE VALLEY
PLEASANTON
H-84
I-680
A
A-1 to A-4
800
600
1000
1200
D
G
F
SR-2
SR-3
B-1
B-2
B-3
H
E
E-1
E-2
SR-1
SCALE
0
5000
feet

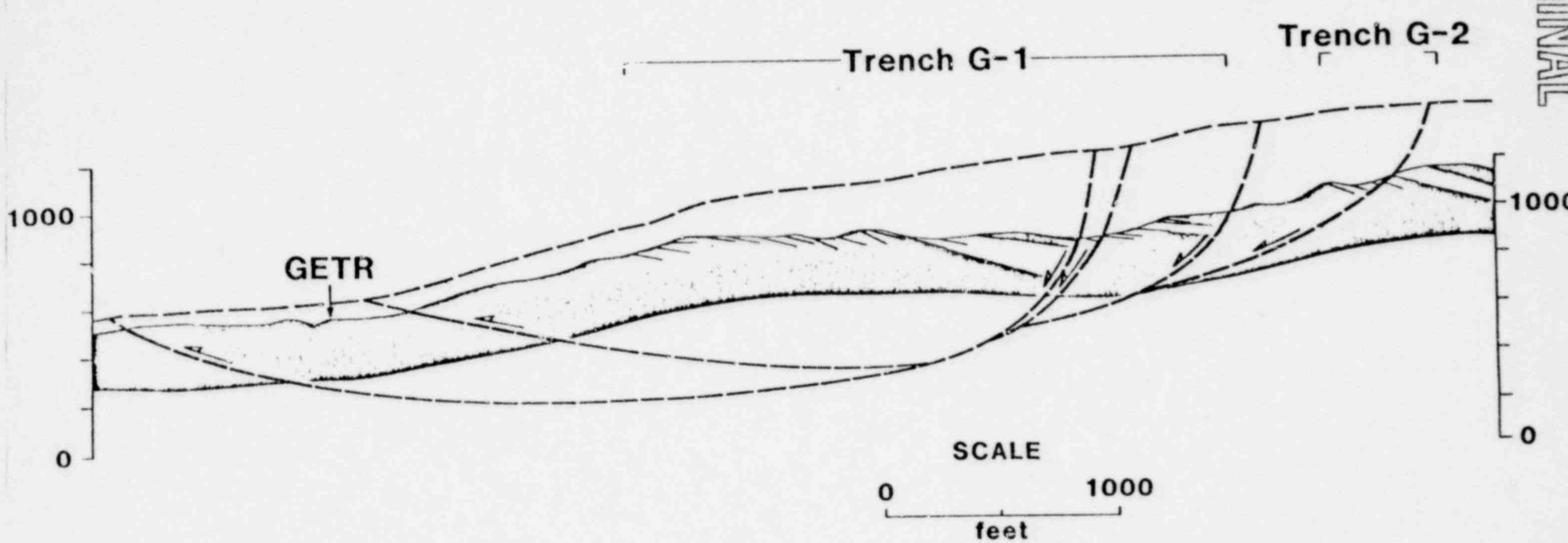
1463 047

75


Trench G-1
Trench G-2
1000
GETR
0
1000
ELEVATION, feet
0
SCALE
0
1000
feet

1463 048

POOR ORIGINAL



1463 049

RESULTS OF PHASE II INVESTIGATION

- UNFAULTED STRATIGRAPHIC SEQUENCE OF LIVERMORE GRAVELS ACROSS HAPPY VALLEY; UNBROKEN STAGE 5 PALEOSOL IN TRENCH E ACROSS MAPPED TRACE OF VERONA FAULT

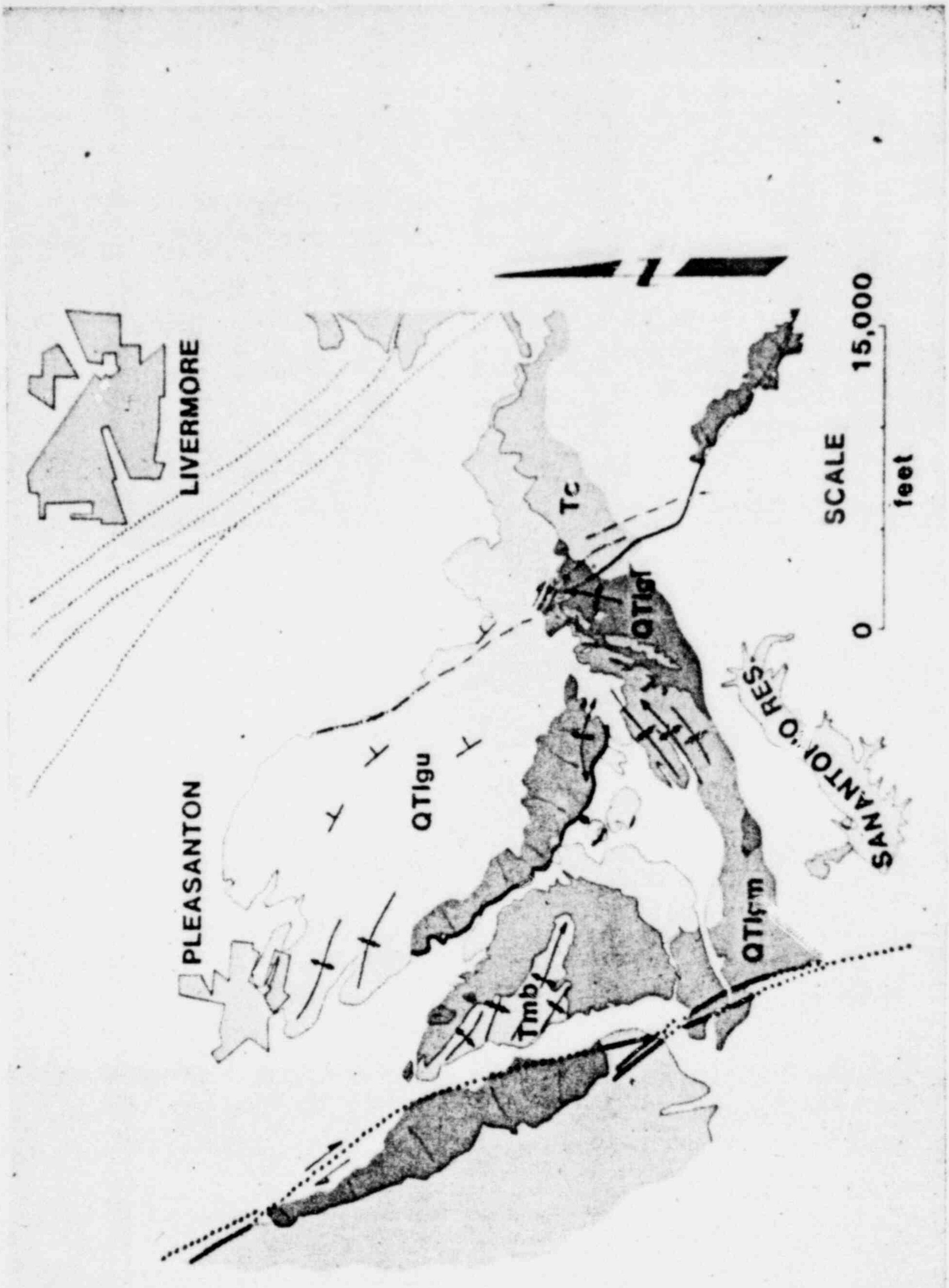
- PREVIOUSLY UNMAPPED FAULT IN PASS AREA
  - STRIKES N65-70°W, DIPS 70-75°NE
  - LAST MOVEMENT PREDOMINANTLY STRIKE-SLIP
  - COMPONENT OF APPARENT EAST-DOWN OFFSET

- TWO ADDITIONAL LOW-ANGLE SHEARS IN VALLECITOS VALLEY SW OF GETR

- SEVERAL HIGH-ANGLE TENSIONAL BREAKS OF INDETERMINATE OFFSET IN VALLECITOS HILLS

1463 050

POOR ORIGINAL



- REGIONAL GEOLOGIC AND TECTONIC SETTING

  DOUGLAS HAMILTON

- SITE GEOLOGY

  DOUG YADON

- QUATERNARY HISTORY

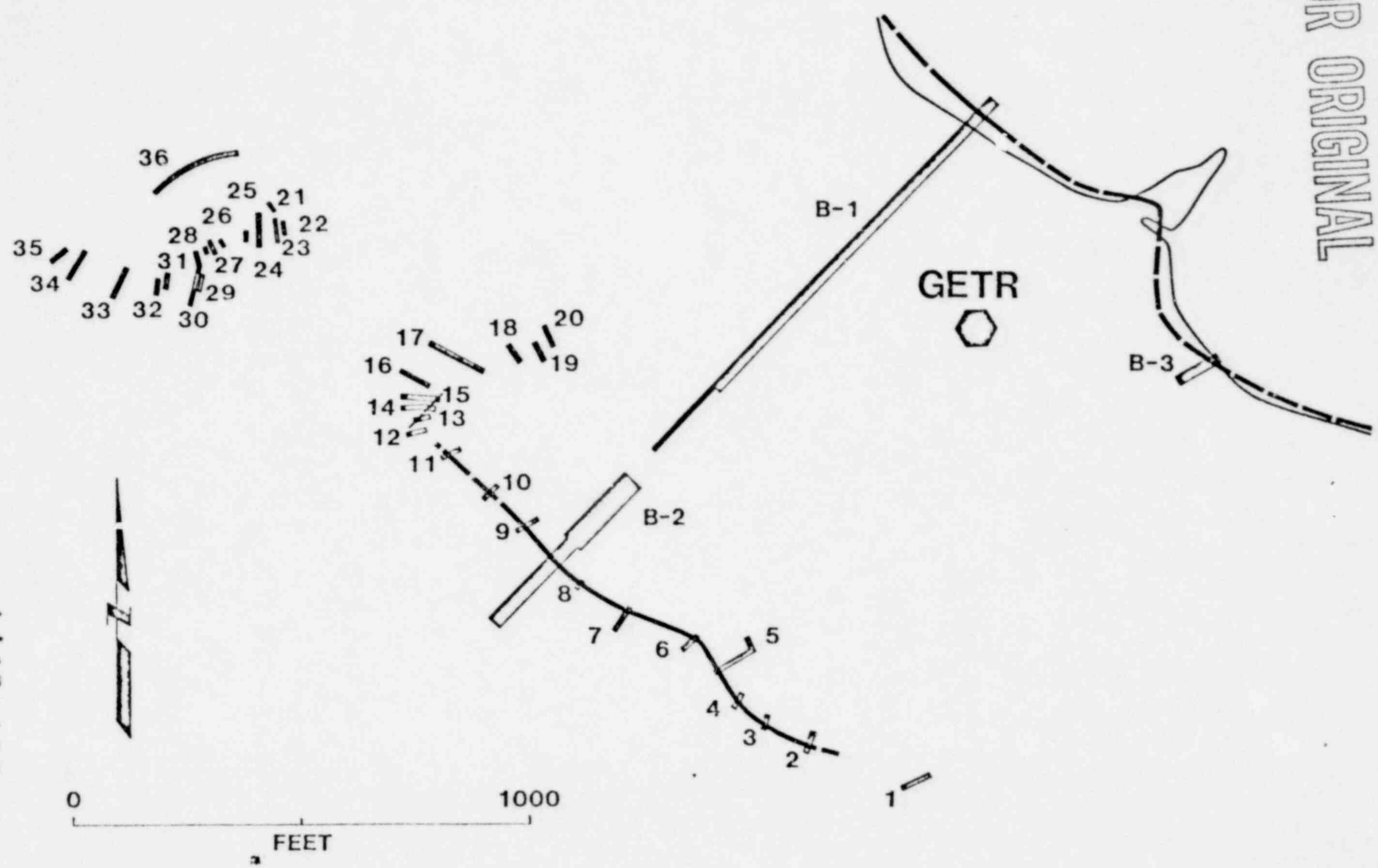
  ROY SHLEMON

- INTERPRETATIONS AND CONCLUSIONS

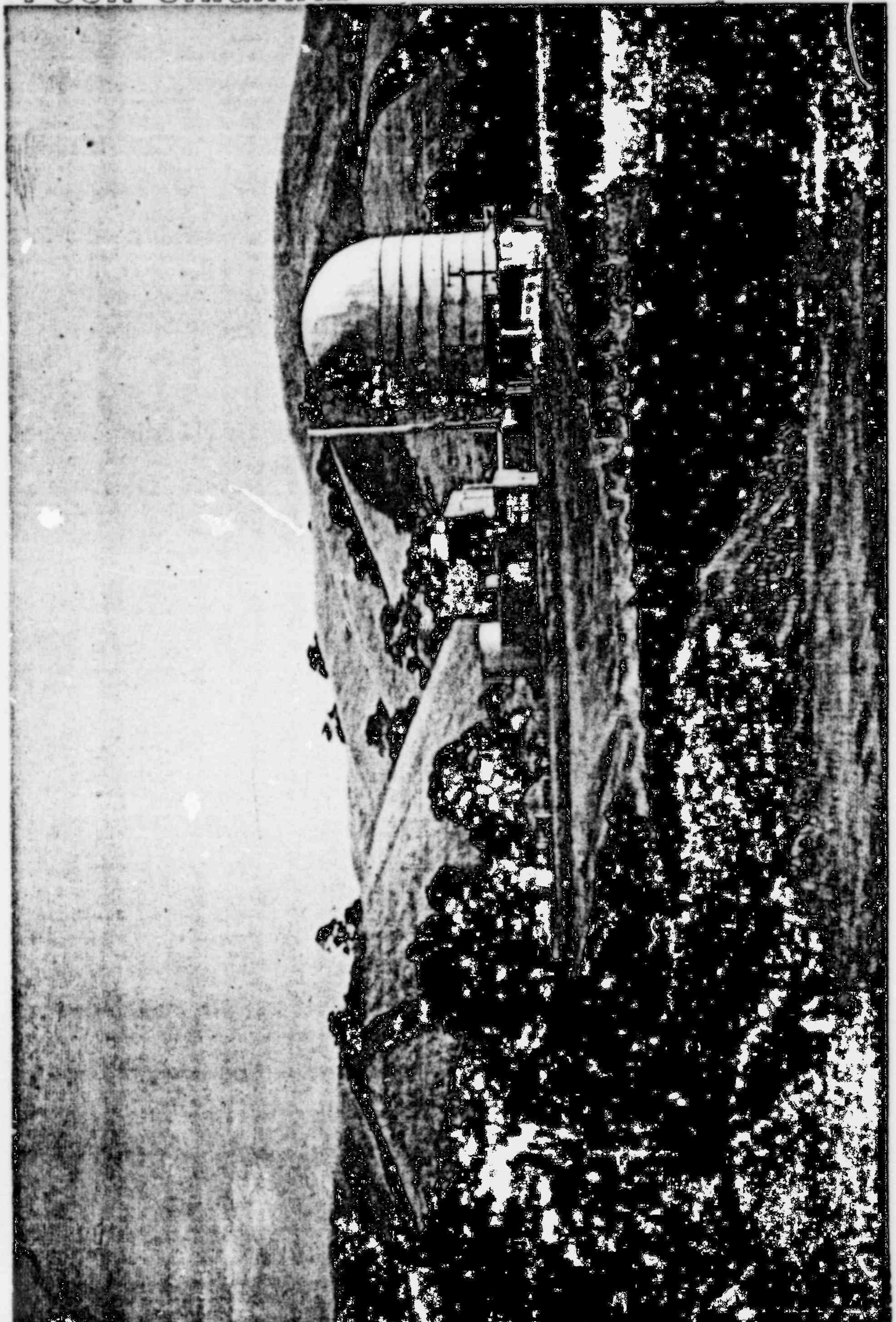
  RICHARD HARDING

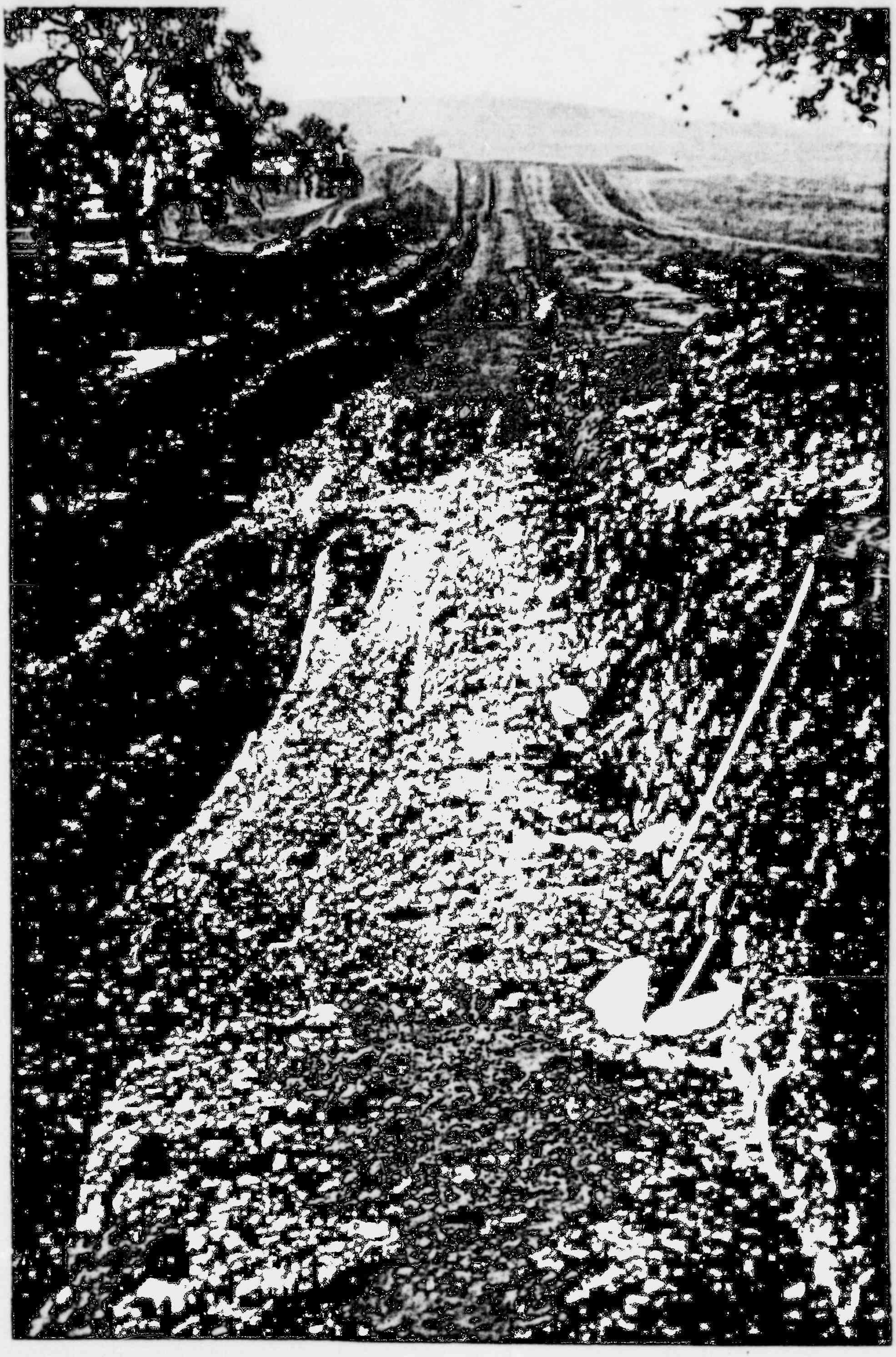
POOR ORIGINAL

80


GETR
B-1
B-2
B-3
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20
21
22
23
24
25
26
27
28
29
30
31
32
33
34
35
36
0
1000
FEET

1463 053





SOIL HORIZONS, GETR SITE AREA

MODERN SOLUM	0	
	A11	MOLLIC EPIPEDON
	A12	
	A13	
	A2(Ae)	ALBIC HORIZON
	B1	CAMBIC HORIZON
	Bt	ARGILLIC HORIZON
BURIED PALEOSOL		BURIED ARGILLIC HORIZON
	IIC1	WEATHERED PARENT MATERIAL
	IIC2	

1463 056

#3

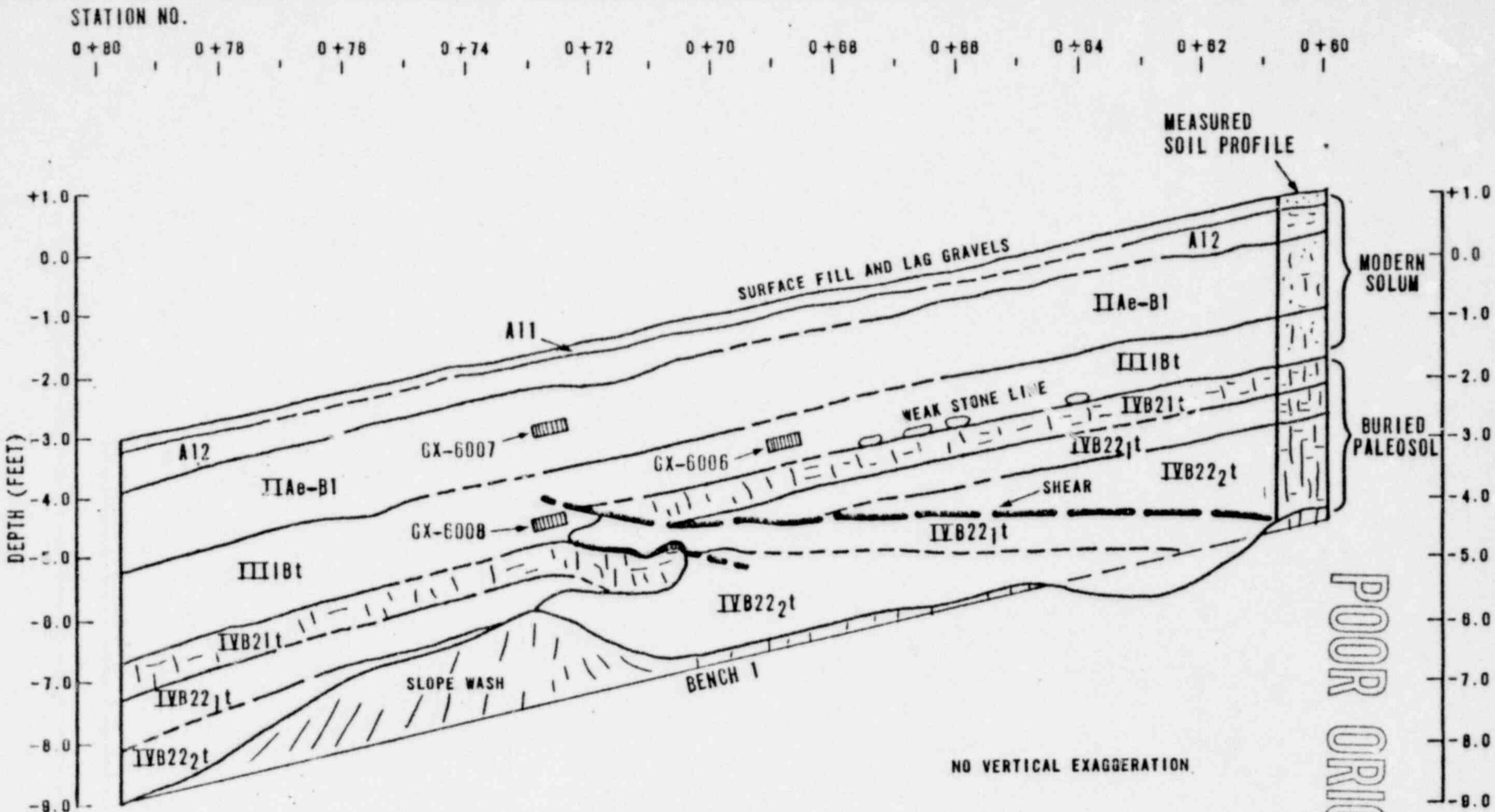
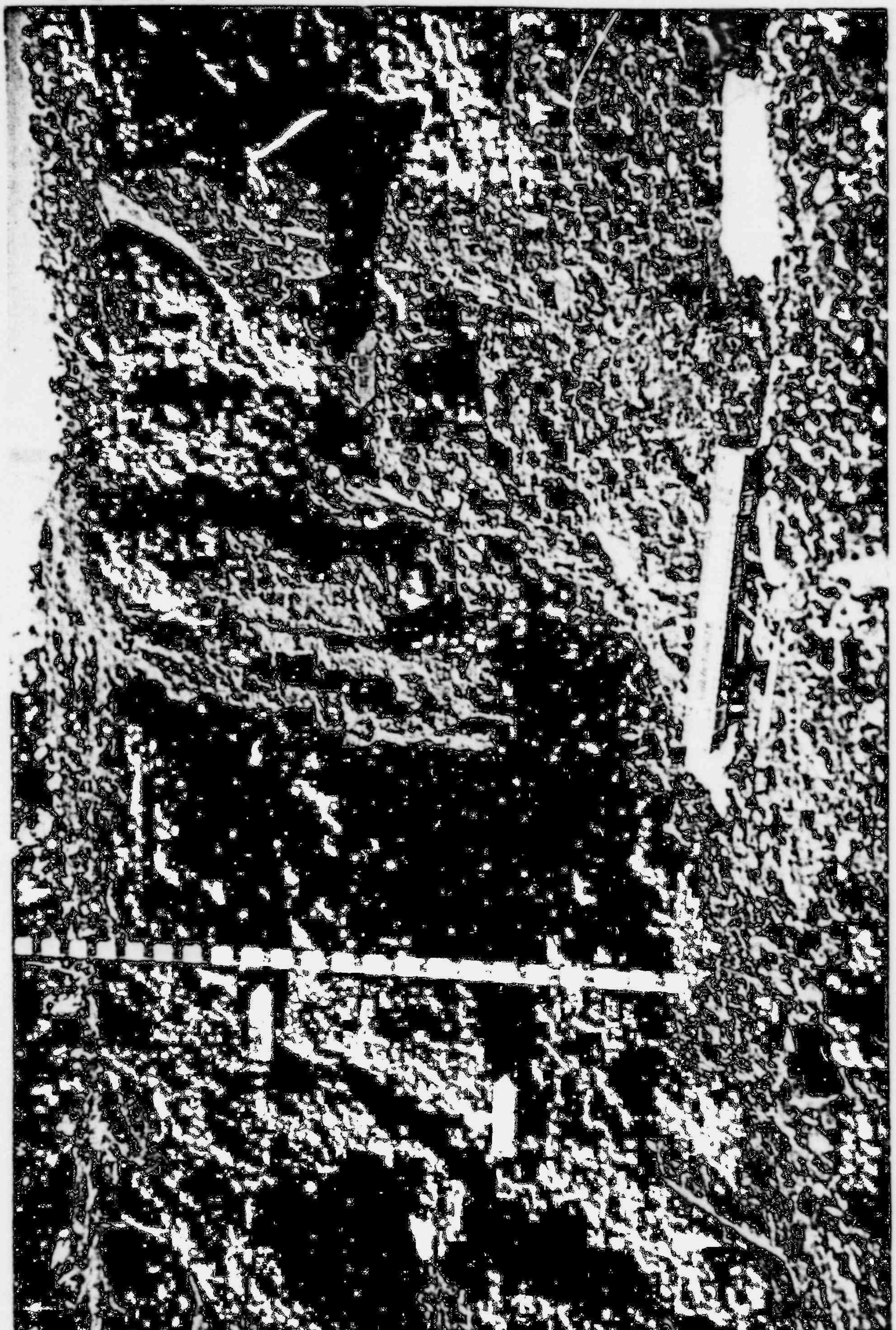
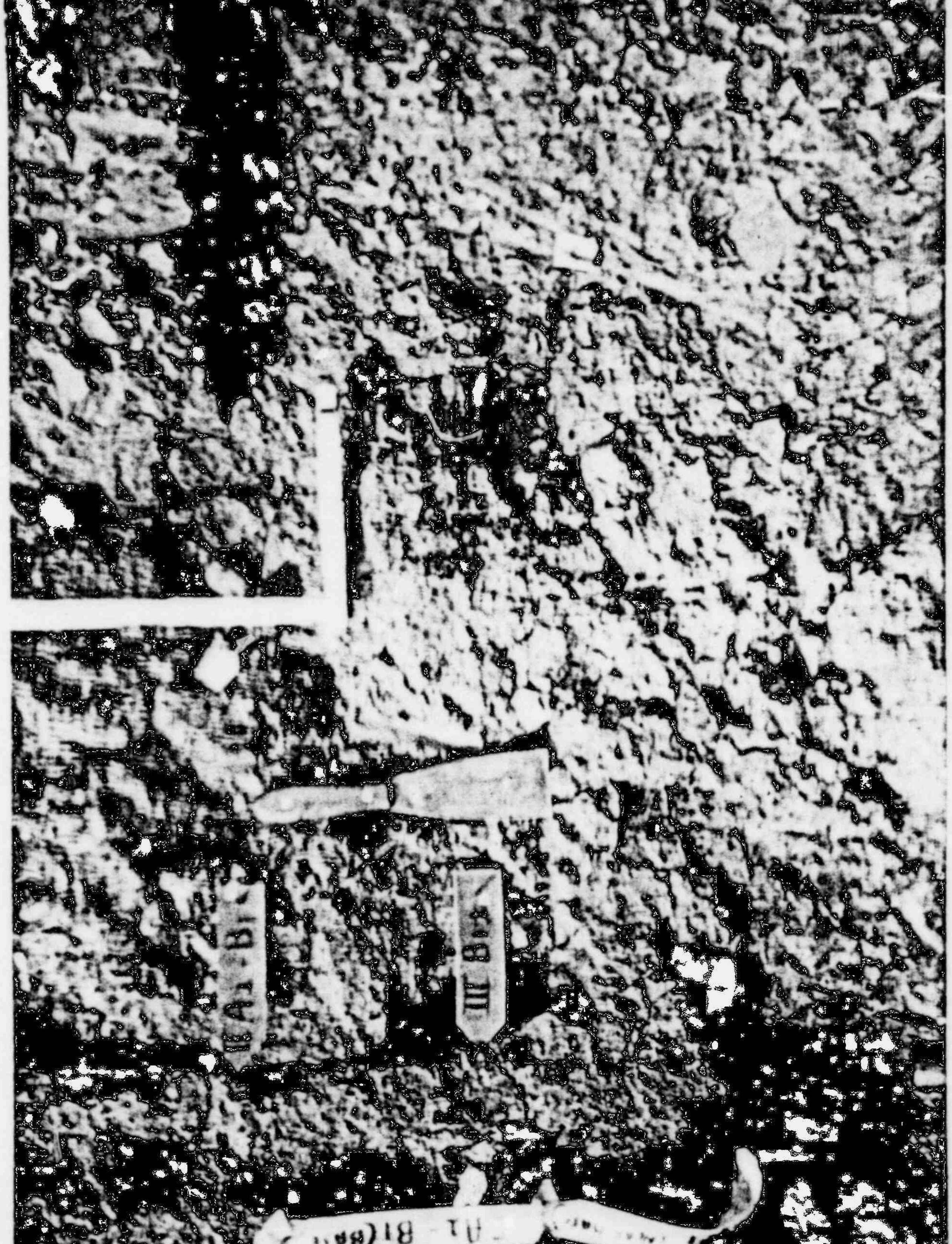


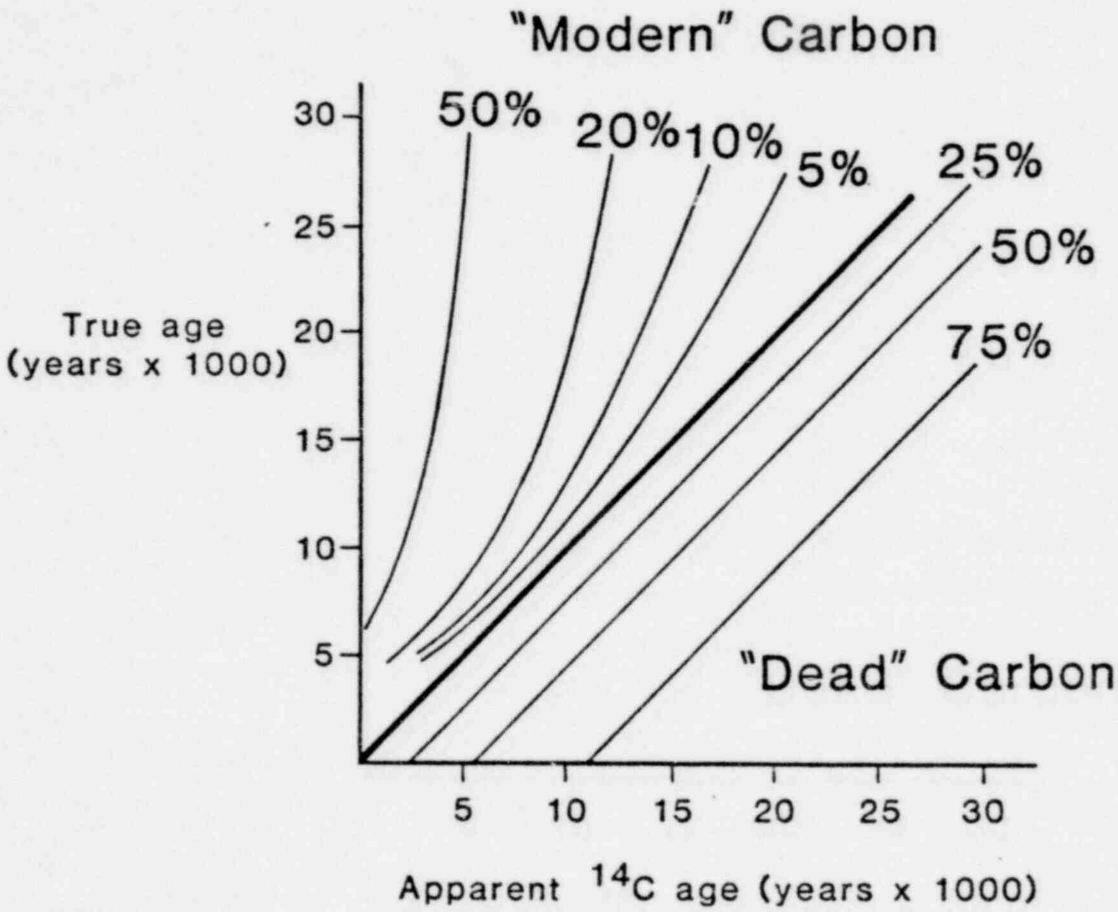
Figure A-8: Soil-stratigraphy, west wall, Trench B-1; ESA stations 0+60–0+80. Representative soil profile measured at station 0+60 (Table 2). Dominant shear extends into $IIIB_t$ horizon of modern solum. Radiocarbon sample localities indicated by laboratory number (e.g., GX-6008; see Table 1).

1463 057



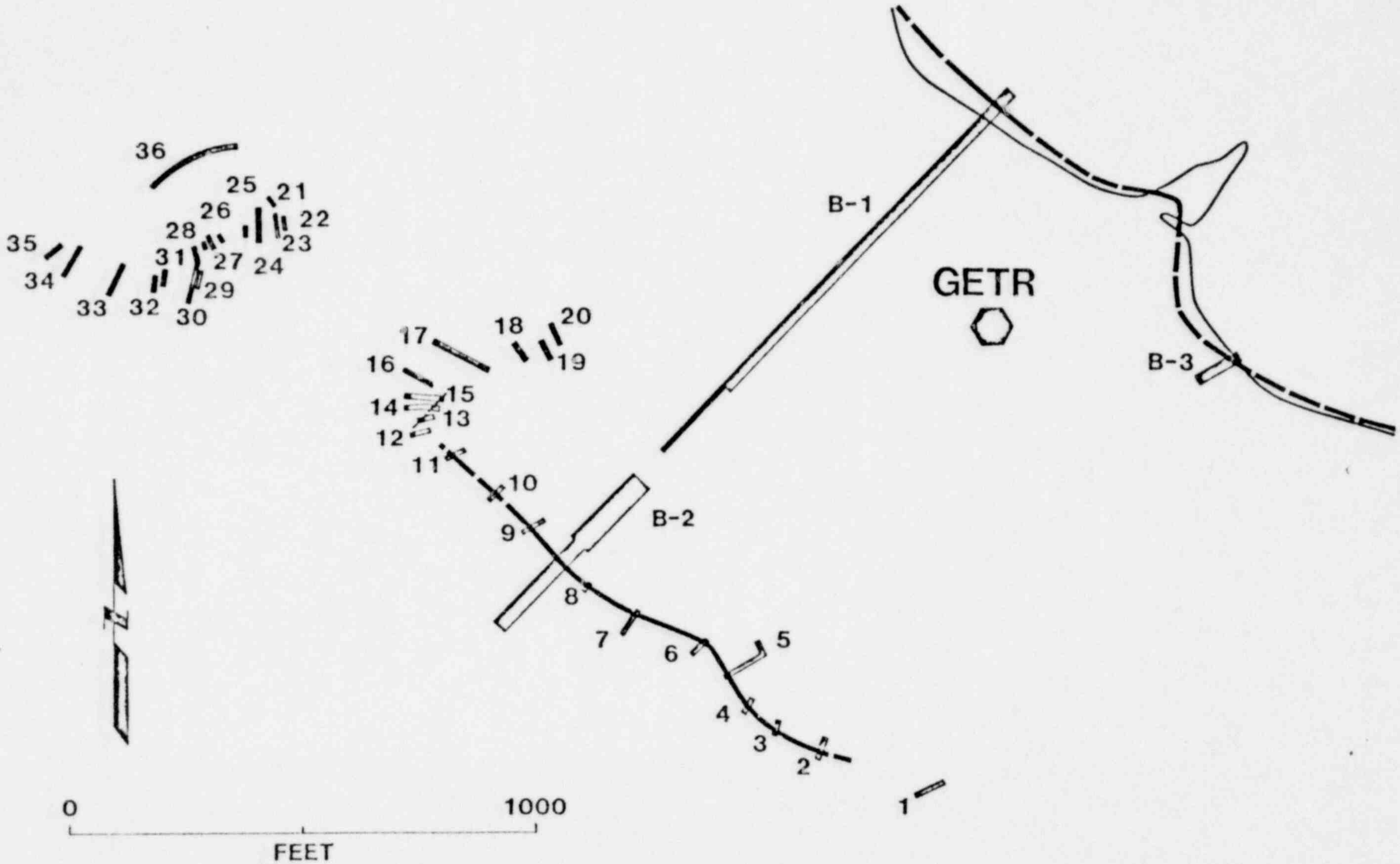
1463 058





"Modern" Carbon
50%
20%
10%
5%
25%
50%
75%
True age (years x 1000)
30
25
20
15
10
5
"Dead" Carbon
5
10
15
20
25
30
Apparent ^{14}C age (years x 1000)

1463 060


GETR
B-1
B-2
B-3
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20
21
22
23
24
25
26
27
28
29
30
31
32
33
34
35
36
0
1000
FEET

1463 061

POOR ORIGINAL

1463 062

90

POOR ORIGINAL

1463 063

91

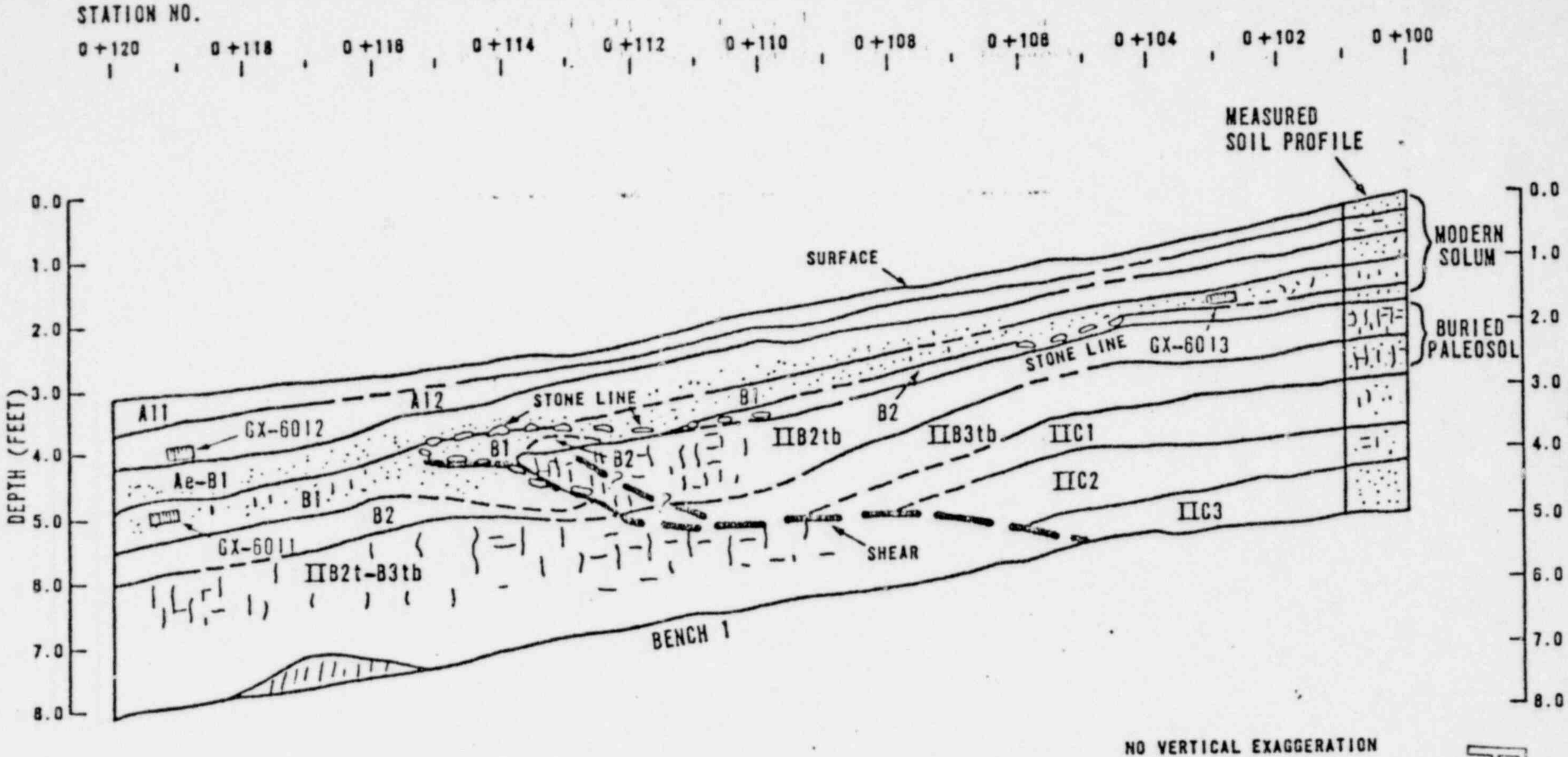


Figure A-9: Soil-stratigraphy, west wall, Trench B-2; ESA stations 1+00–1+20. Representative soil profile measured at station 1+00 (Table 3). Albic horizon (Ae) extends into lower B_1 of modern solum below shear (station 1+14). Radiocarbon sample localities indicated by laboratory number (e.g., GX-6011; see Table 1).

1463 064

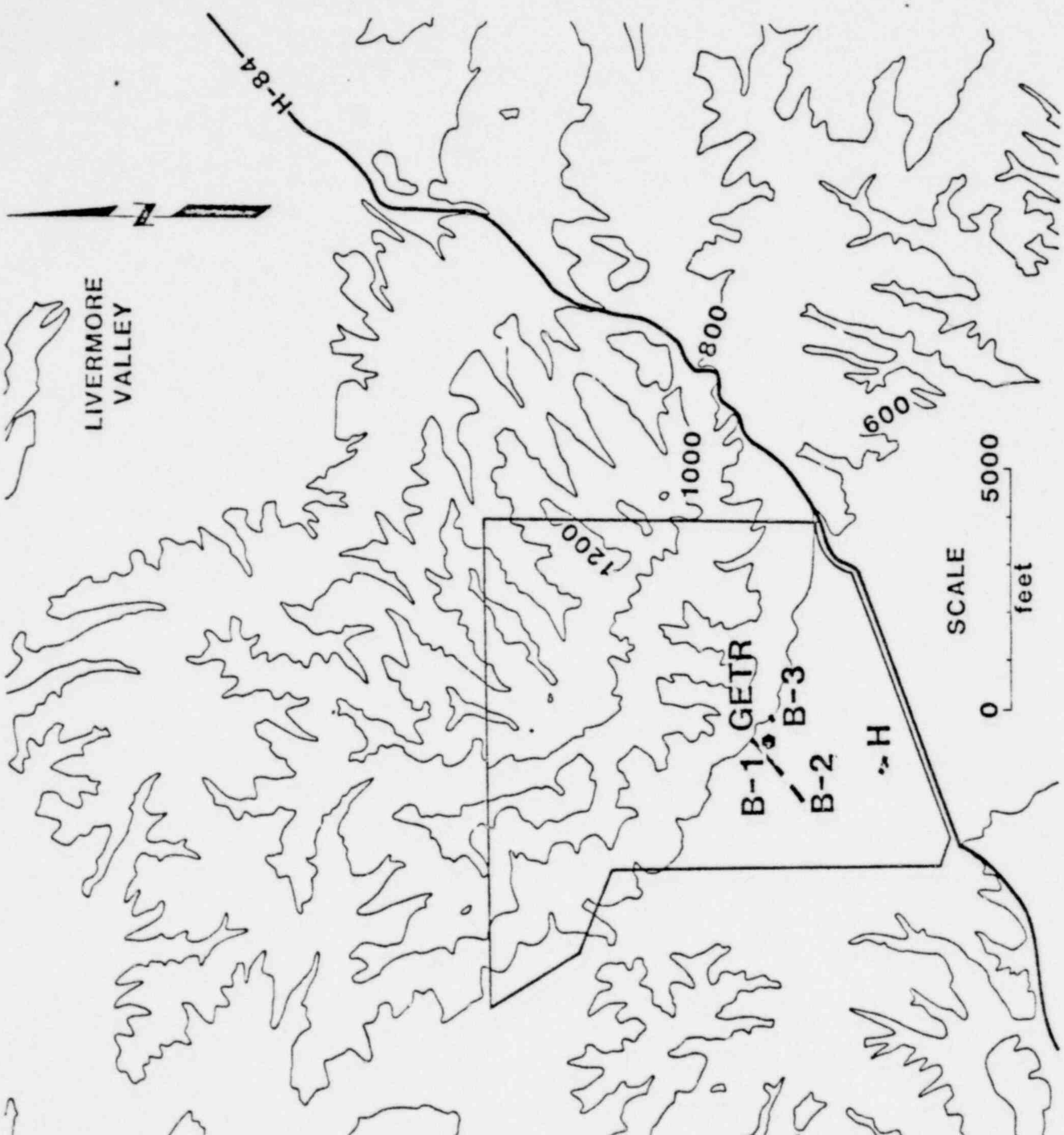
RADIOCARBON DATES, MODERN SOILS, TRENCH B-1/B-2, GETR

LAB NO.	SAMPLE LOCATION	AGE (MRT) ALKALI SOLUBLE	AGE (MRT) ALKALI INSOLUBLE	SOIL HORIZON
GX-6006	TRENCH B-1, STA. 69.5, DEPTH 2.3 FT.	4,310 ± 300	2,440 ± 160	IIBT
GX-6007	TRENCH B-1, STA. 73.0, DEPTH 1.6 FT.	3,045 ± 215	4,600 ± 500	IIAE-B_1
GX-6008	TRENCH B-1, STA. 73.0, DEPTH 3.3 FT.	4,240 ± 195	4,195 ± 195	IIBT
GX-6011	TRENCH B-2, STA. 119.0, DEPTH 2.0 FT.	2,160 ± 195	1,475 ± 210	B_1
GX-6012	TRENCH B-2, STA. 119.0, DEPTH 1.0 FT.	1,245 ± 115	1,565 ± 175	A_{12}
GX-6013	TRENCH B-2, STA. 103.0, DEPTH 1.2 FT.	1,240 ± 130	2,180 ± 195	B_1

1463 065

43

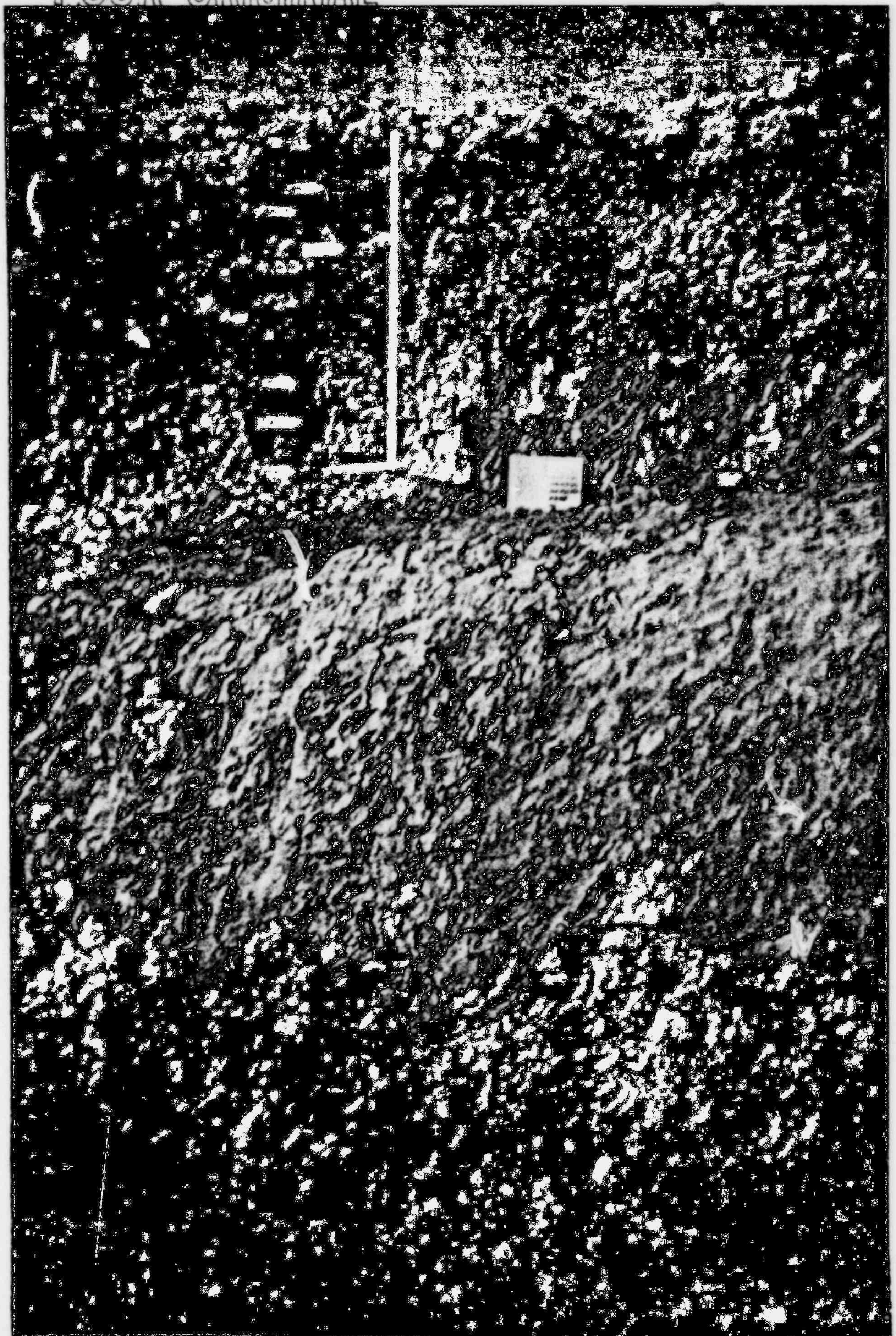
POOR ORIGINAL



1463 066 94

POOR ORIGINAL

1463 067



1463 068

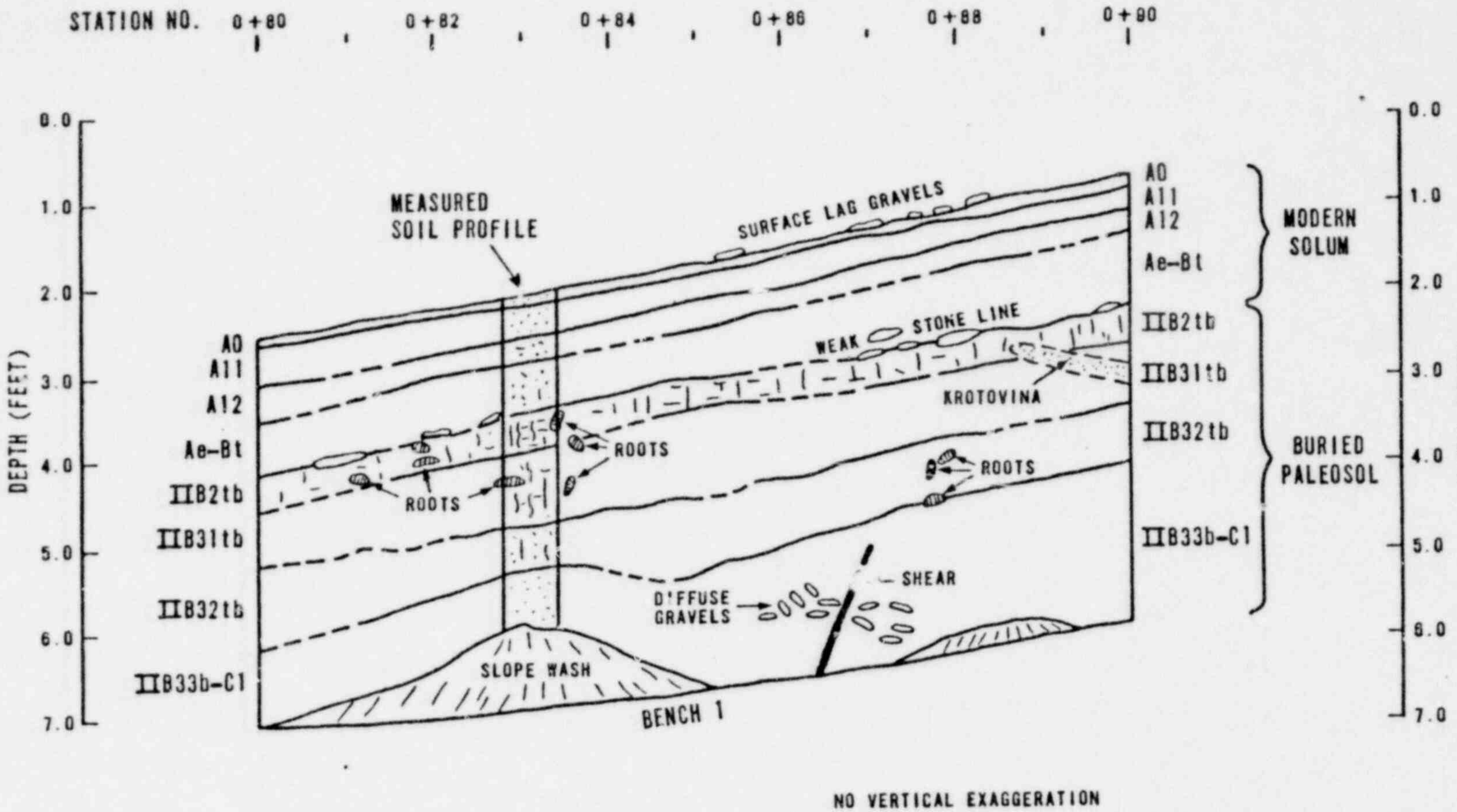
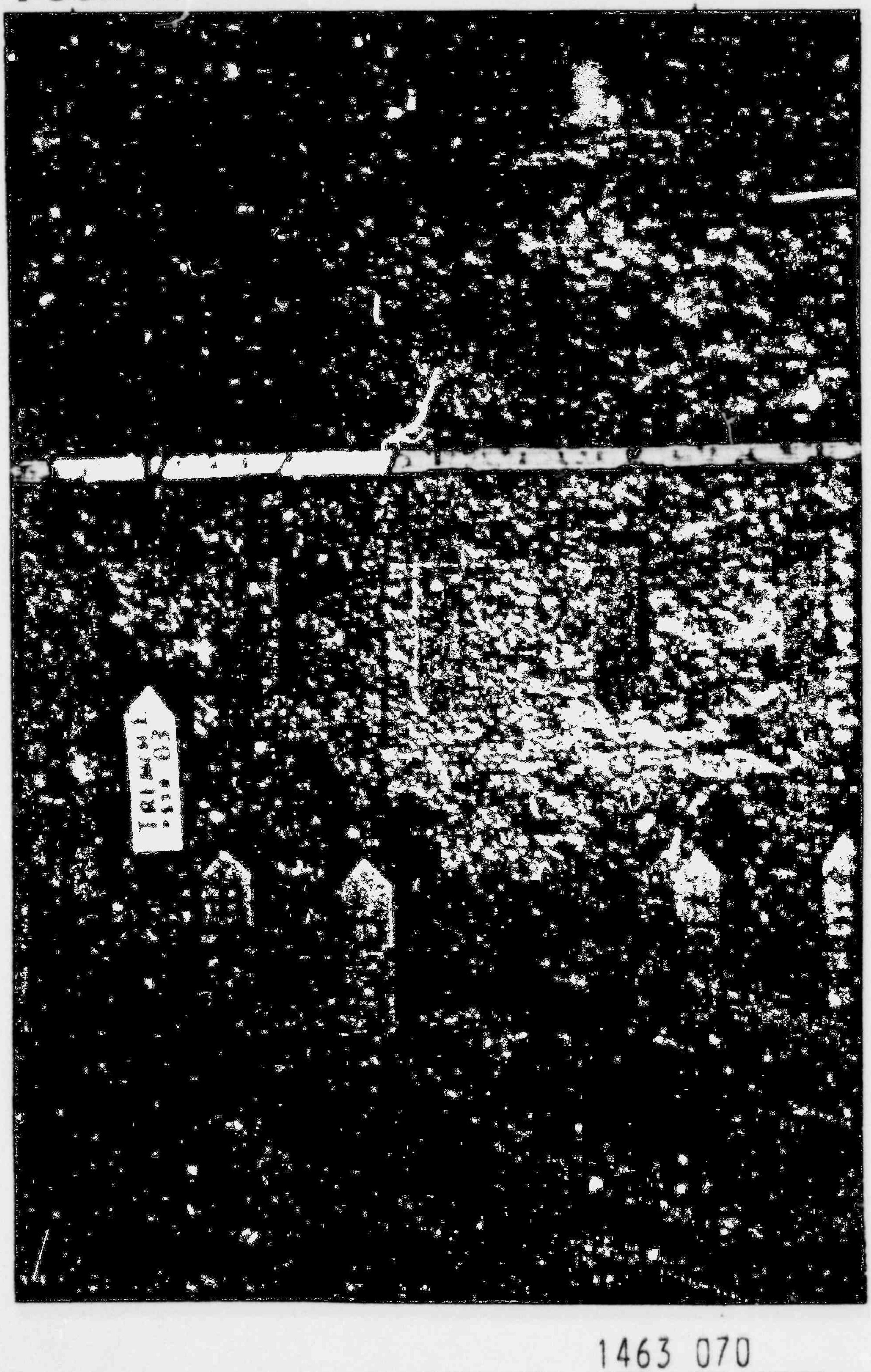


Figure A-12: Soil-stratigraphy, west wall, Trench E; ESA stations 0+80--0+90. Representative soil profile measured at station 0+83 (Table 4). Buried paleosol argillic horizon (IIB_{2tb} and IIB_{3tb}) penetrated by modern roots. Shear does not visibly penetrate nor displace paleosol argillic horizons. See also Figures A-5 and A-13

1463 069

97

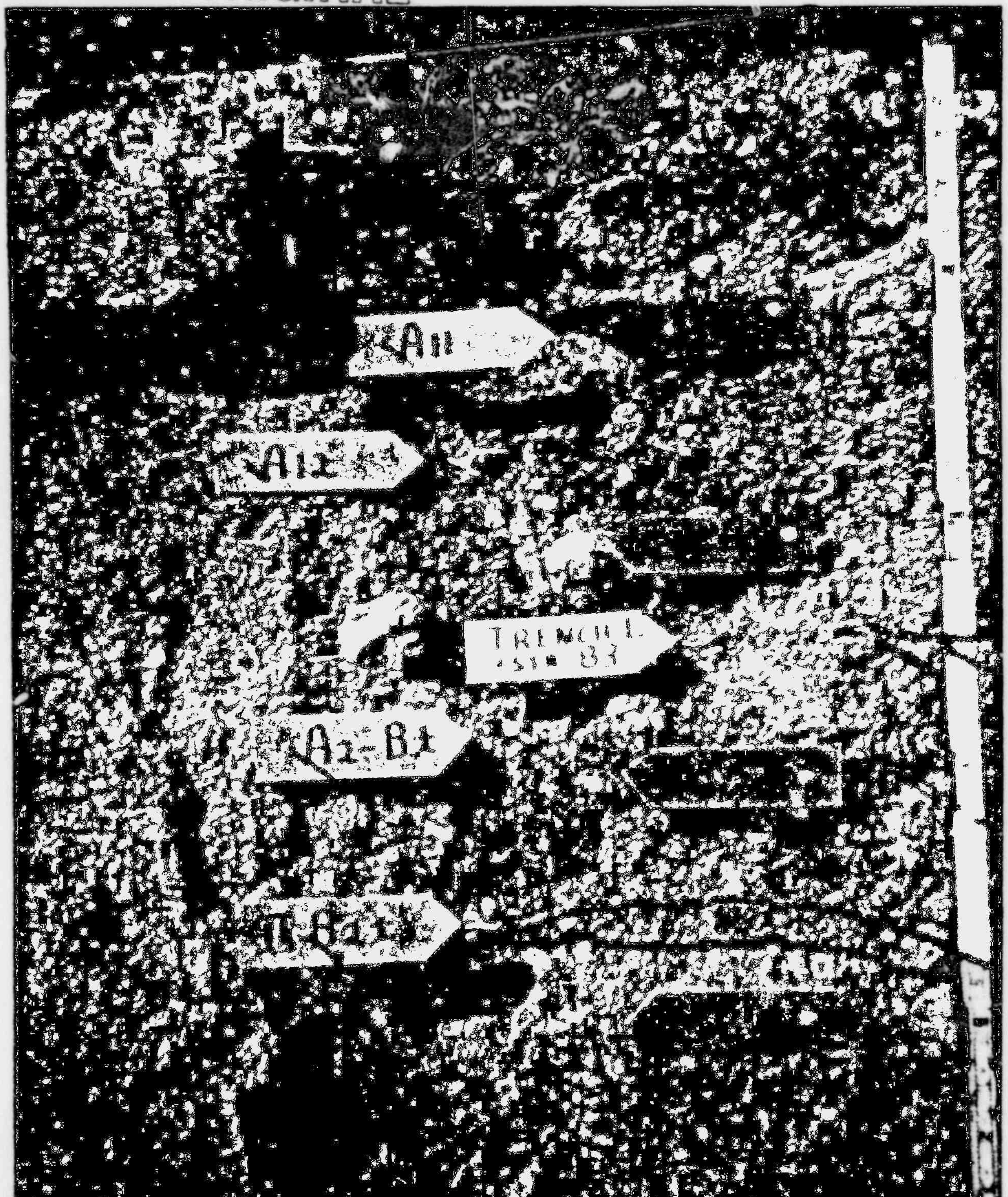
POOR ORIGINAL



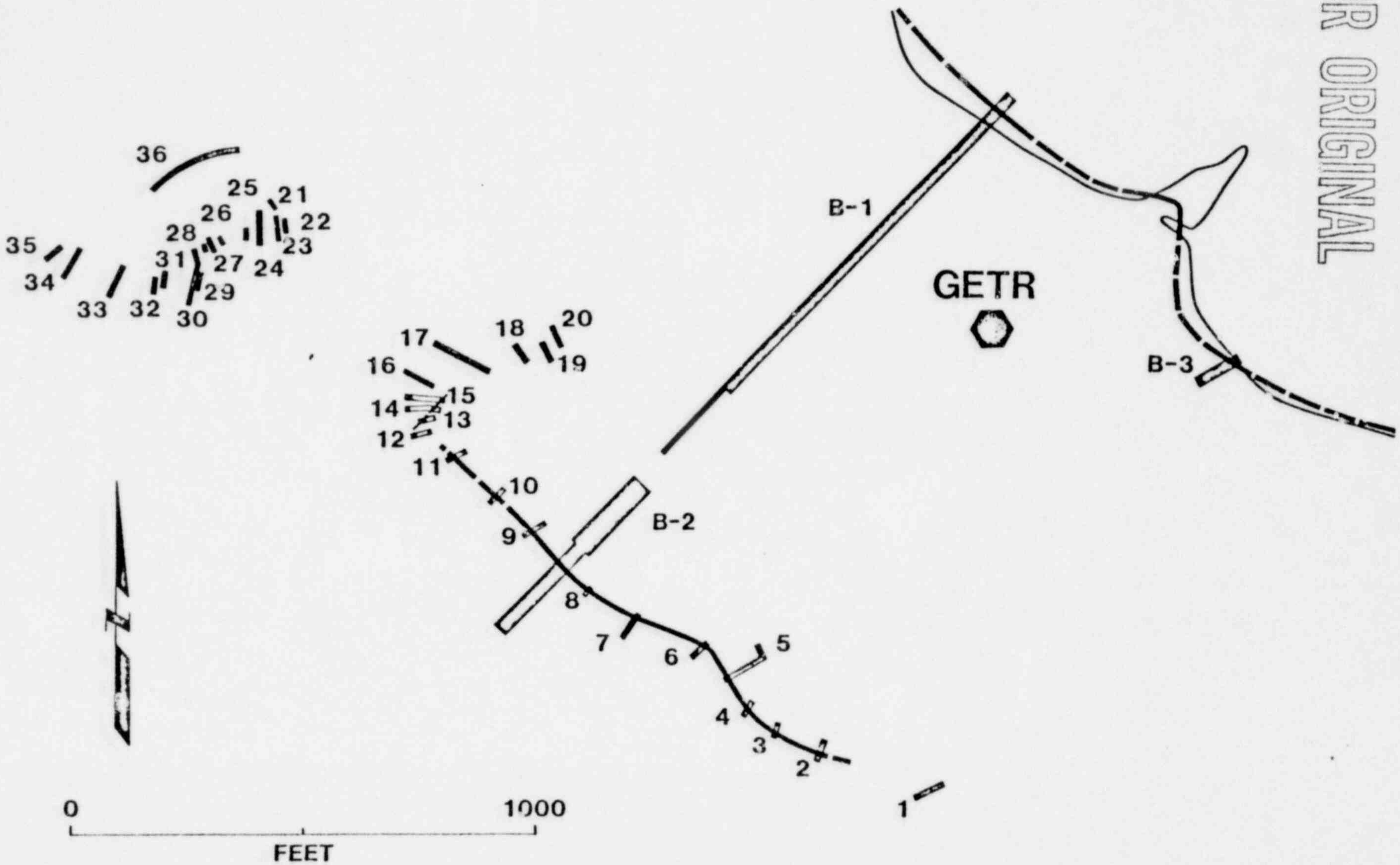
98

1463 070

POOR ORIGINAL



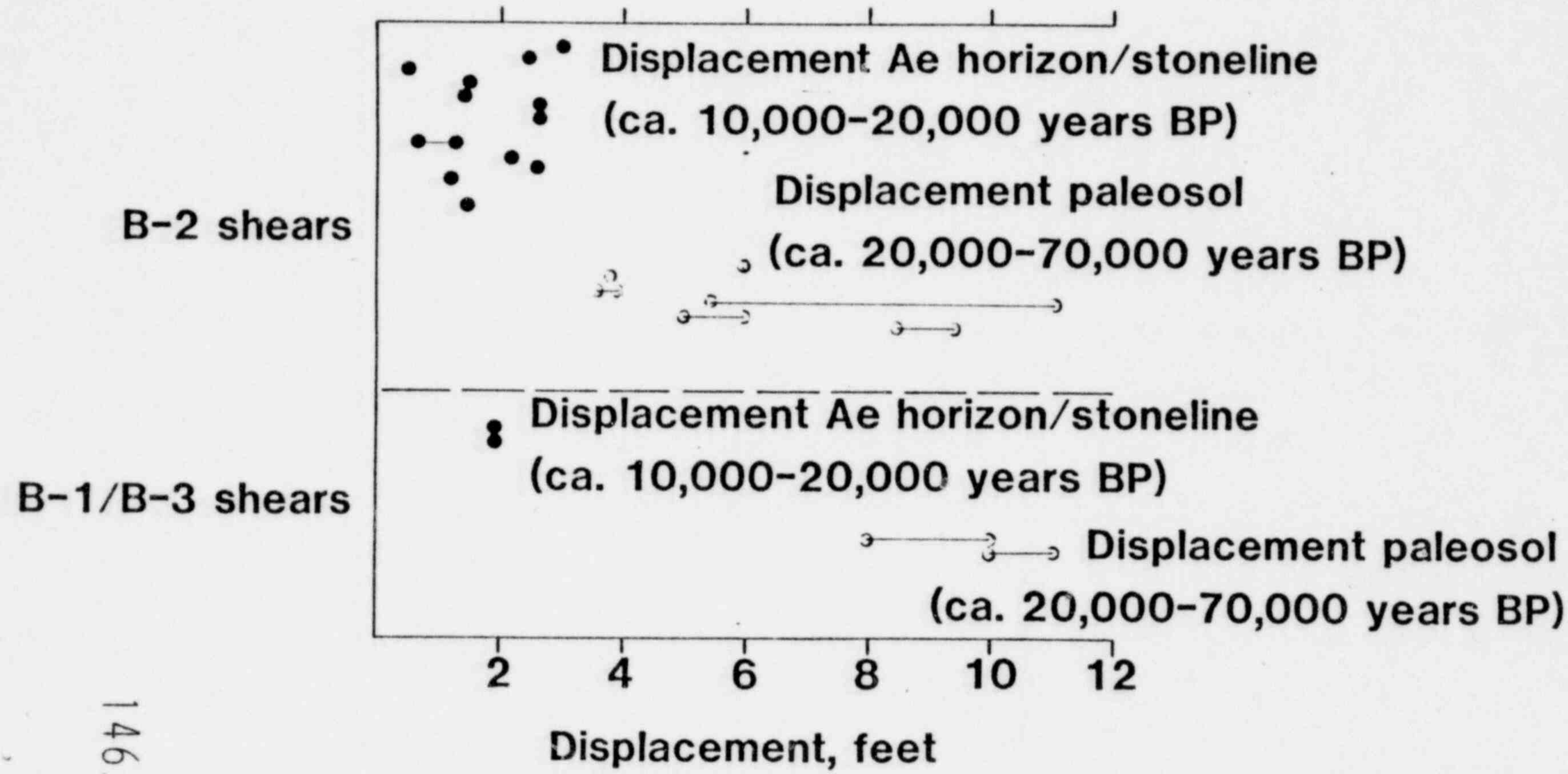
1463 071


GETR
B-1
B-2
B-3
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20
21
22
23
24
25
26
27
28
29
30
31
32
33
34
35
36
0
1000
FEET

1463 072

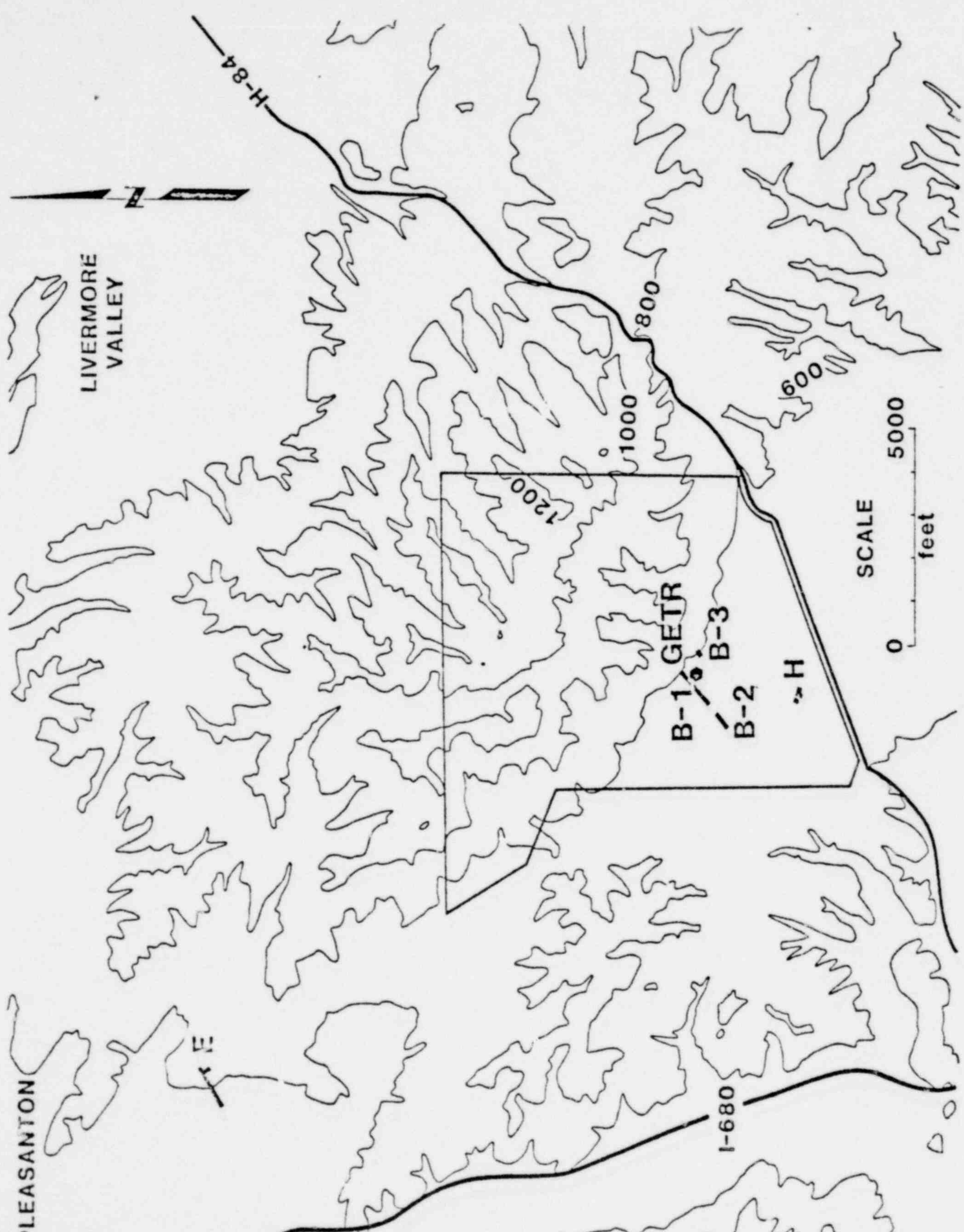
101

1463 073


Displacement Ae horizon/stoneline
(ca. 10,000-20,000 years BP)
Displacement paleosol
(ca. 20,000-70,000 years BP)
B-2 shears
Displacement Ae horizon/stoneline
(ca. 10,000-20,000 years BP)
B-1/B-3 shears
Displacement paleosol
(ca. 20,000-70,000 years BP)
2
4
6
8
10
12
Displacement, feet

1463 074

102


H-84
LIVERMORE VALLEY
800
600
1000
1200
5000
0
SCALE
feet
GETR
B-1
B-2
B-3
H
PLEASANTON
I-680

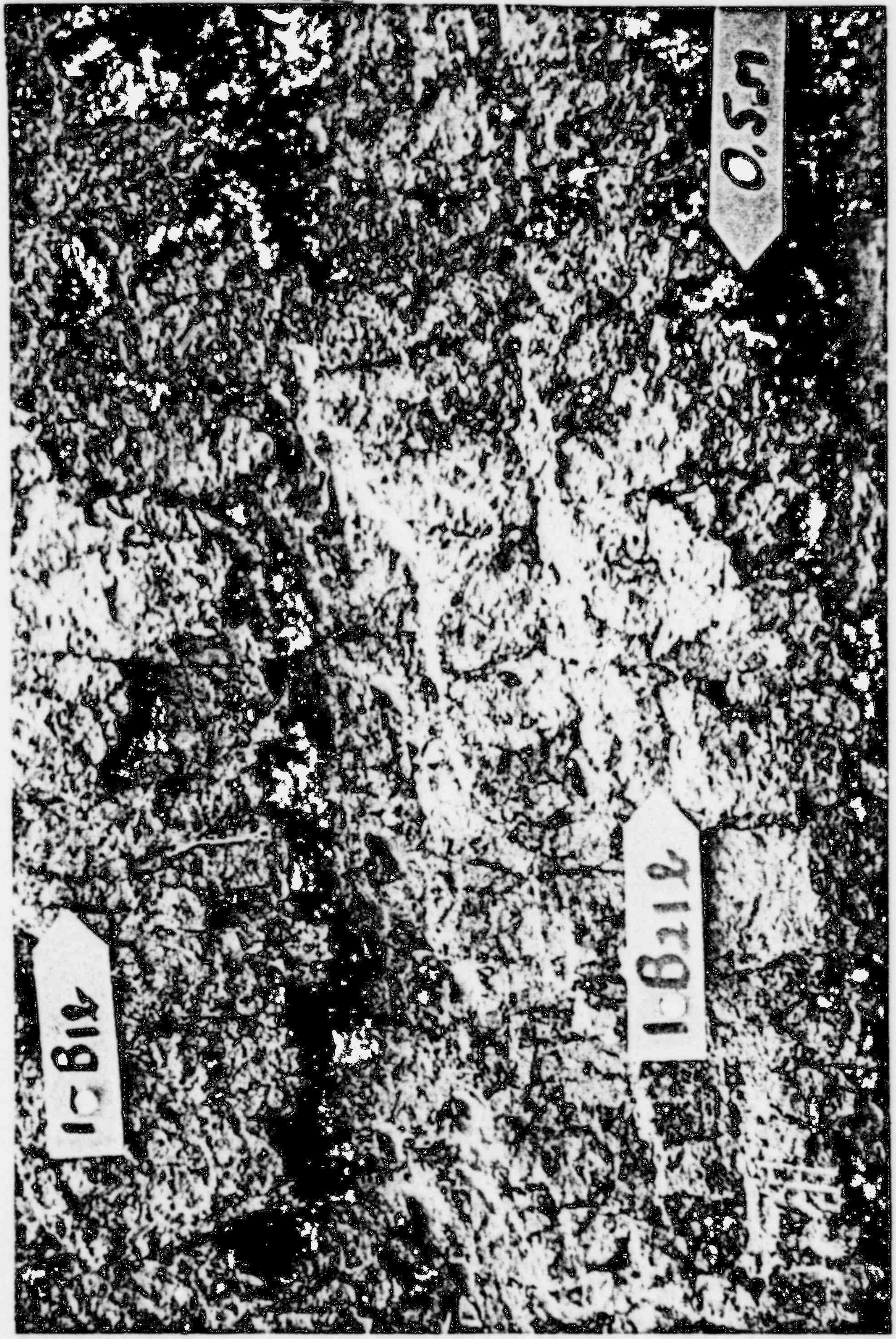
103

1463 075

POOR ORIGINAL

105

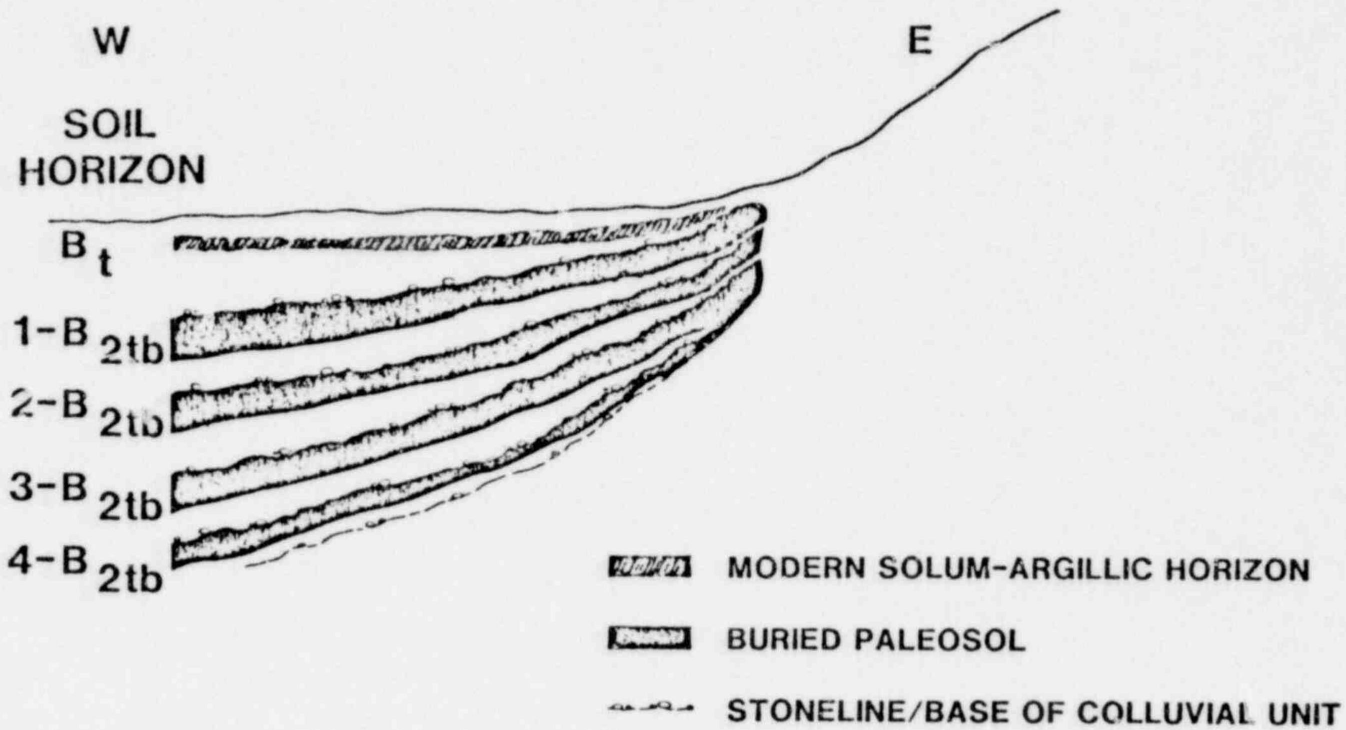
1463 077

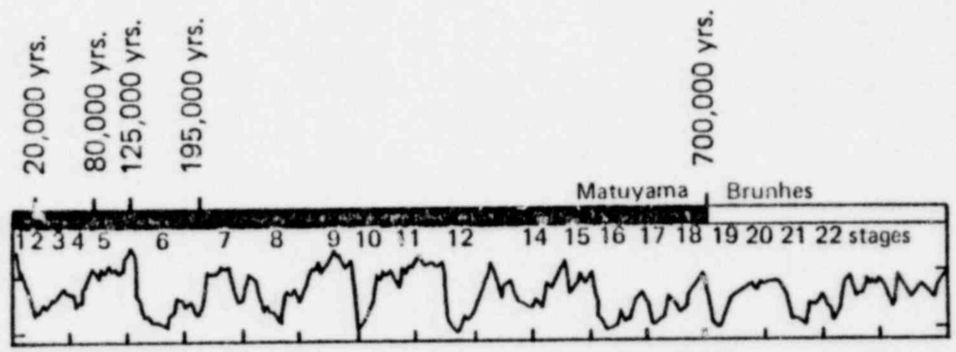


106

MULTIPLE BURIED PALEOSOLS

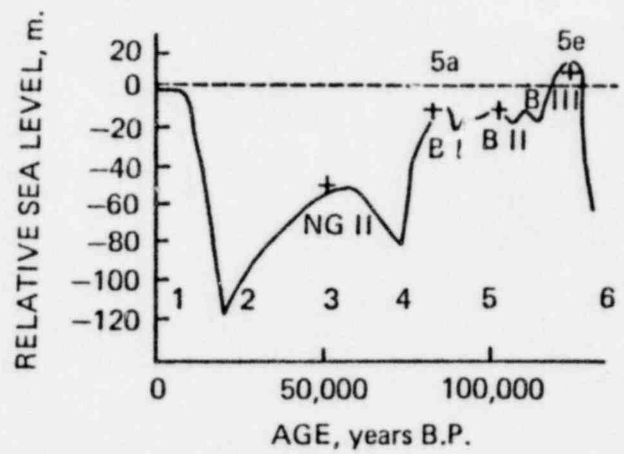
TRENCH H, GETR





QUATERNARY MARINE ISOTOPE - SEA LEVEL CHRONOLOGY

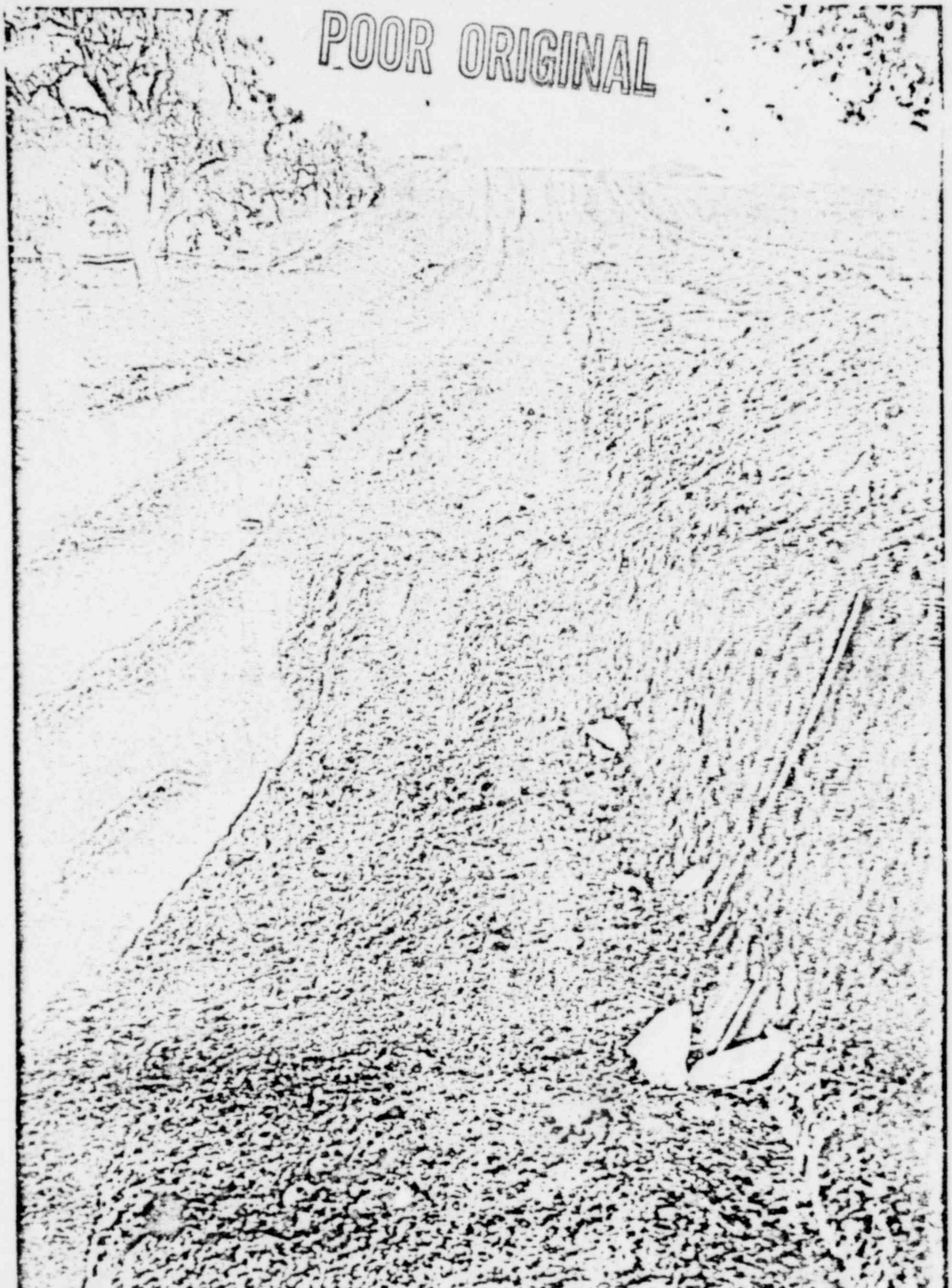
1463 080



LATE QUATERNARY

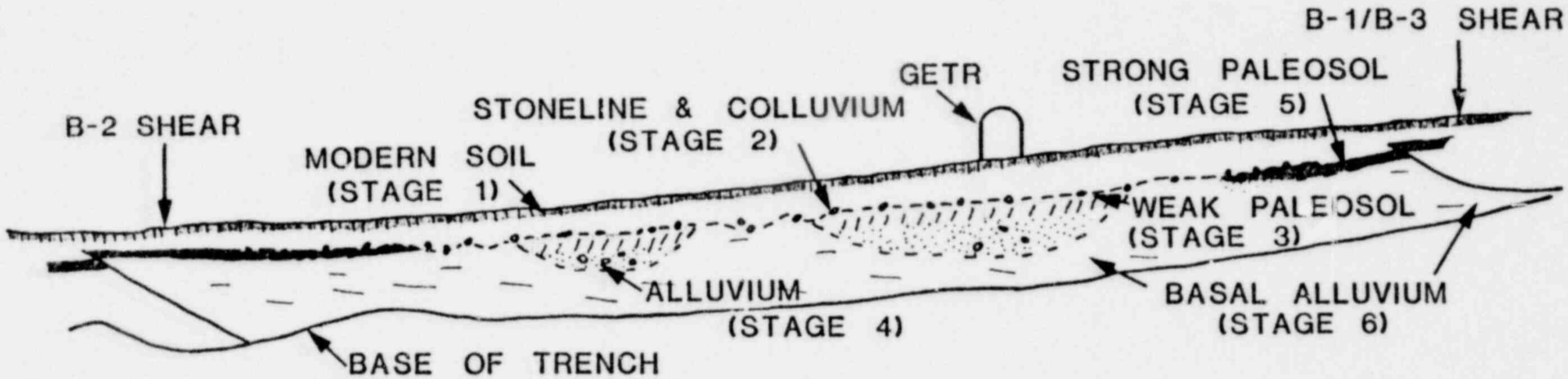
GLACIO-EUSTATIC SEA LEVELS

1463 081



110

LATE QUATERNARY SOILS AND SEDIMENTS AT GETR SITE



1463 083

TIME PERIOD (BEFORE PRESENT IN YEARS)	OFFSET DURING TIME PERIOD (FT)	
	SHEAR B-2	SHEAR B-1/B-3
0 -- 8,000 to 15,000	0	0
8,000 to 15,000 -- 17,000 to 20,000	3	2
17,000 to 20,000 -- 70,000 to 125,000	5	10
70,000 to 125,000 -- 128,000 to 195,000 or greater	80+	40+

1463 084

SUMMARY

PRESENCE OF QUATERNARY MARKERS

A) WIDESPREAD STONELINES
B) REGIONAL, DISTINCTIVE BURIED PALEOSOL

AGE OF MARKERS

A) LAST STONELINE/COLLUVIUM/MODERN SOLUM (<20,000 YRS)
B) STRONGLY DEVELOPED PALEOSOL (~70,000 – 125,000 YRS BP)
C) MULTIPLE BURIED PALEOSOLS, TRENCH H
D) > ~125,000 YRS AT GETR

DISPLACEMENT OF MARKERS

A) MULTIPLE MOVEMENTS ON SAME SLIP SURFACES
B) MAXIMUM ~3 FT – EARLY HOLOCENE
C) MAXIMUM ~12 FT OF 70,000 – 125,000 YR BP PALEOSOLS

1463 085

- REGIONAL GEOLOGIC AND TECTONIC SETTING

  DOUGLAS HAMILTON

- SITE GEOLOGY

  DOUG YADON

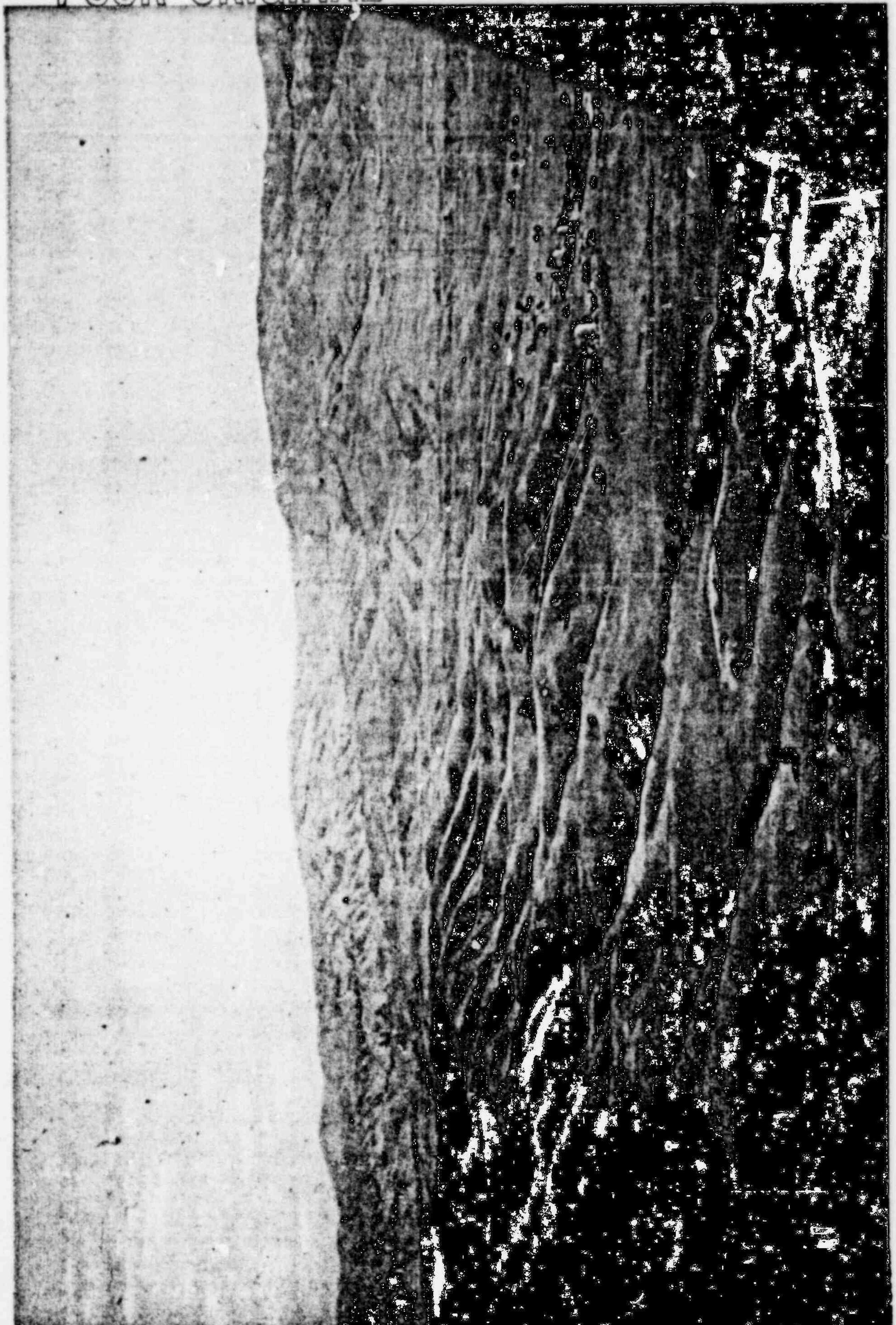
- QUATERNARY HISTORY

  ROY SHLEMON

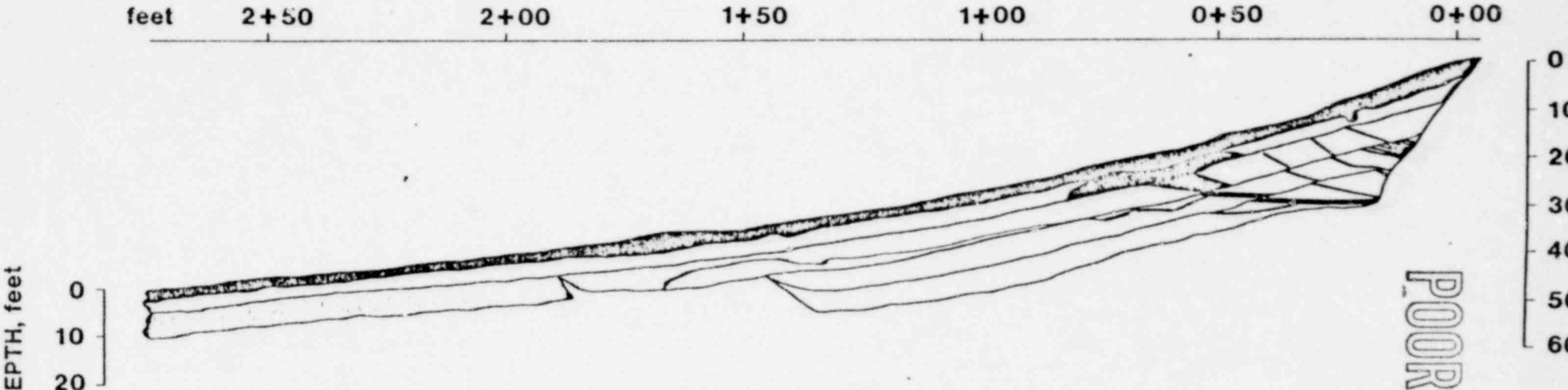
- ⇨ INTERPRETATIONS AND CONCLUSIONS

  RICHARD HARDING

1463 086



TRENCH B-1
VIEW NW



1463 088

116

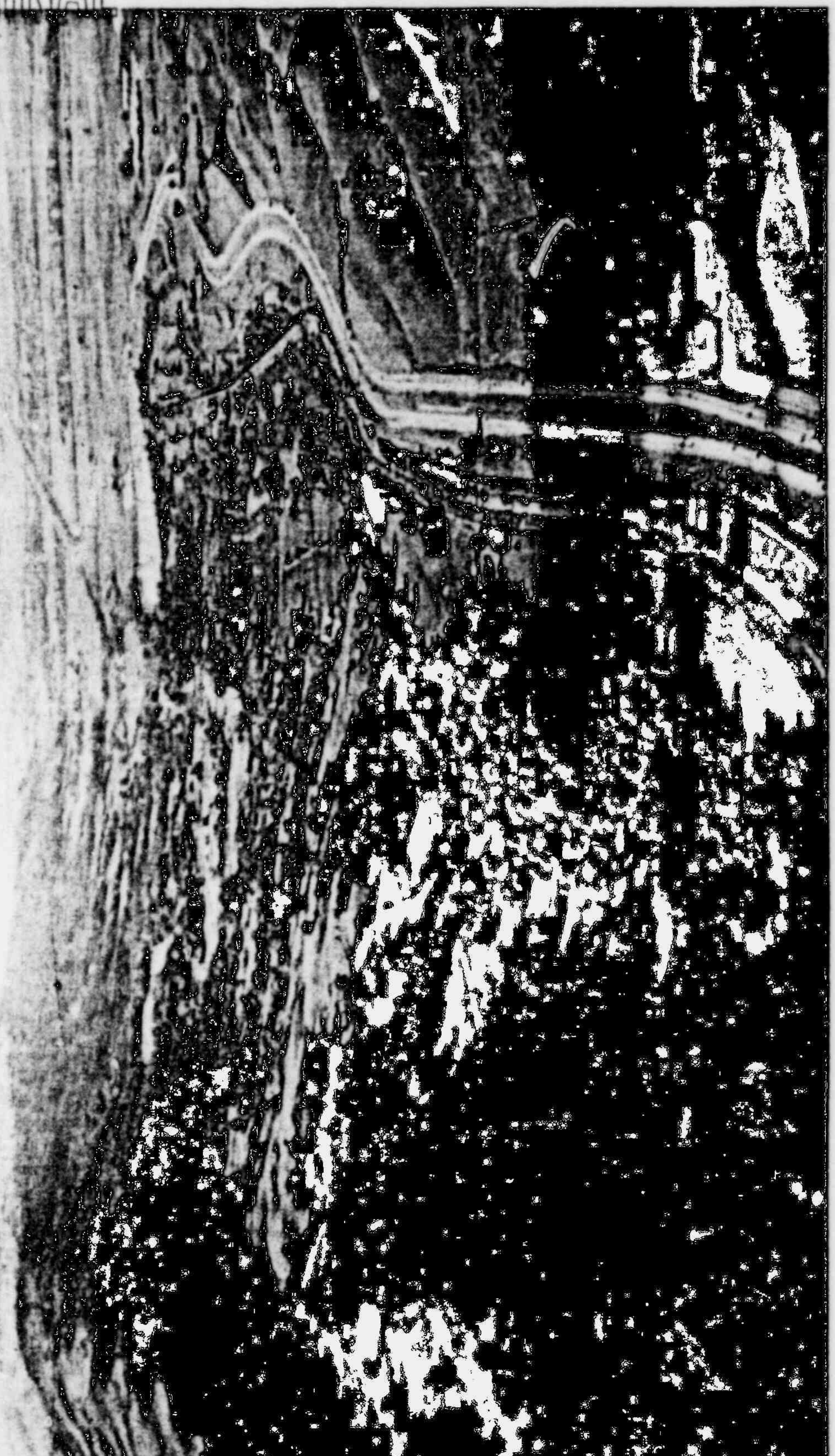
AGE OF OFFSETS

<table>
<tr><th rowspan="2">OXYGEN-ISOTOPE STAGE</th><th rowspan="2">RELATIVE SEA LEVEL</th><th rowspan="2">YEARS B.P.</th><th colspan="3">SHEAR OFFSETS, FEET</th></tr>
<tr><th>B-1/B-3</th><th>B-2</th><th>H</th></tr>
<tr><td>1</td><td>HIGH</td><td>0-10,000</td><td>0</td><td>0</td><td>0(?)</td></tr>
<tr><td>2</td><td>LOW</td><td>10,000-30,000</td><td>2</td><td>3</td><td>1-1/2(?)</td></tr>
<tr><td>3</td><td>HIGH</td><td>30,000-60,000</td><td>--</td><td>--</td><td>--</td></tr>
<tr><td>4</td><td>LOW</td><td>60,000-70,000</td><td>10</td><td>5</td><td>4(?)</td></tr>
<tr><td>5</td><td>HIGH</td><td>70,000-130,000</td><td>--</td><td>--</td><td>--</td></tr>
<tr><td>6</td><td>LOW</td><td>130,000-195,000</td><td rowspan="4">40+</td><td rowspan="4">80+</td><td rowspan="4">20+</td></tr>
<tr><td>7</td><td>HIGH</td><td>195,000-250,000</td></tr>
<tr><td>8</td><td>LOW</td><td>250,000-300,000</td></tr>
<tr><td>9</td><td>HIGH</td><td>300,000-350,000</td></tr>
</table>

1463 089

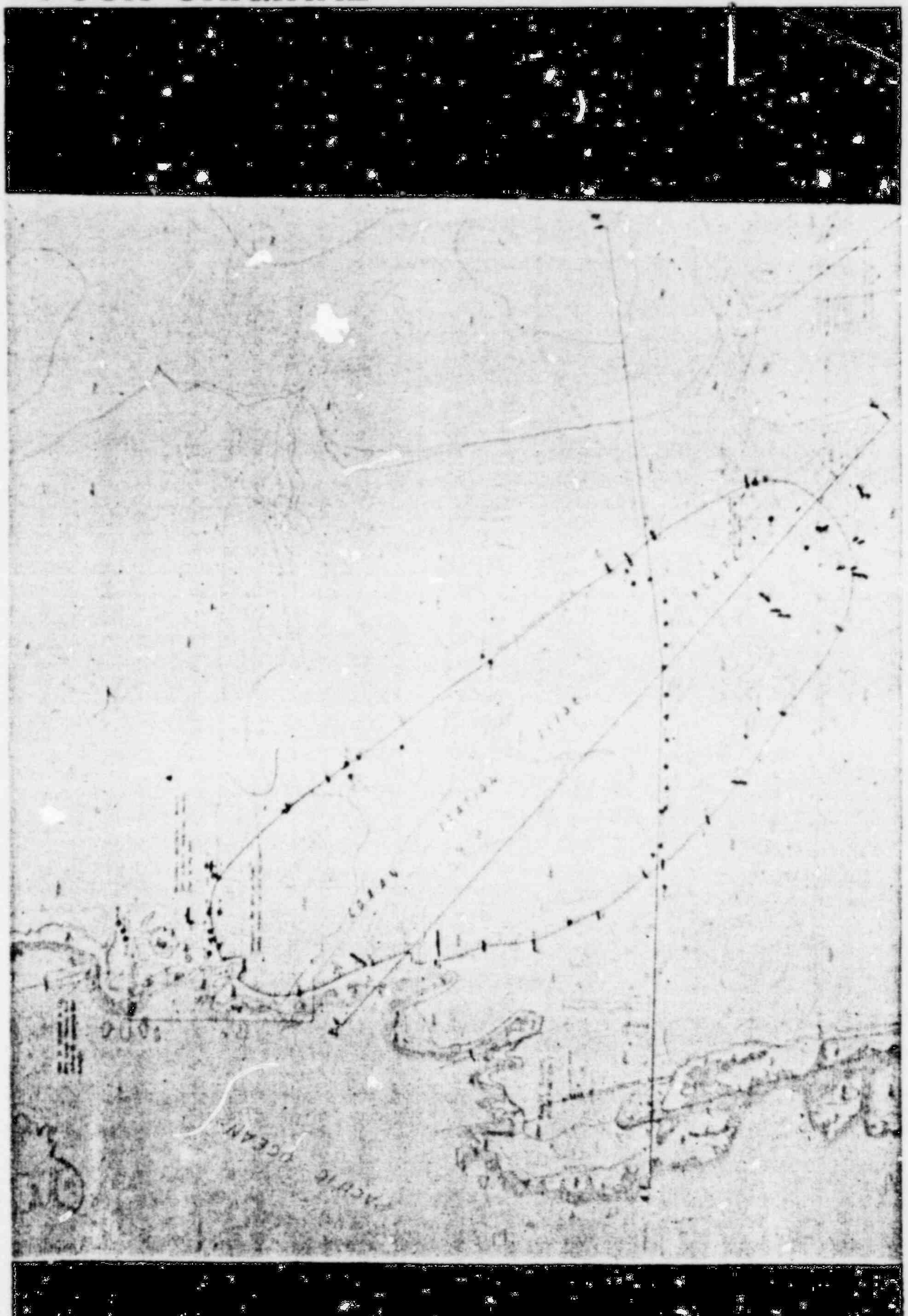
PLEISTOCENE LANDSLIDES IN CALIFORNIA

NAME	AGE (YEARS BP) (DATING METHOD)	APPROXIMATE SIZE
BARTON FLATS	16,000-20,000(?) (GEOMORPHIC/ STRATIGRAPHIC)	12 SQ. MI.
PALOS VERDES (OLDEST COMPLEX)	> 800,000 (U-SERIES)	500-200 WIDE, 400-122 400-1200' LONG, 400-500' THICK
PALOS VERDES-FILLORUM COMPLEX	>95,000 (STRATIGRAPHIC)	1200-1600' LONG, 40-300' THICK
MC CREARY'S MARSH	>15,080 190 (C_{14})	
"DIAMOND A"	>40,000 (C_{14}) 35,000±2100 (C_{14})	
PARSON'S LANDING	ABT. 17,000 (STRATIGRAPHIC)	200 ACRES
FLETCHER HILLS (WEST SLIDE)	18,000-24,000 (C_{14})	UP TO 4000' WIDE, 1200' LONG
UNION-PHELPS #1 DRILL SITE	13,200±160 (C_{14})	
BURDELL MOUNTAIN	30,000±2000 (C_{14})	2600' LONG, 1000' WIDE, >100' THICK

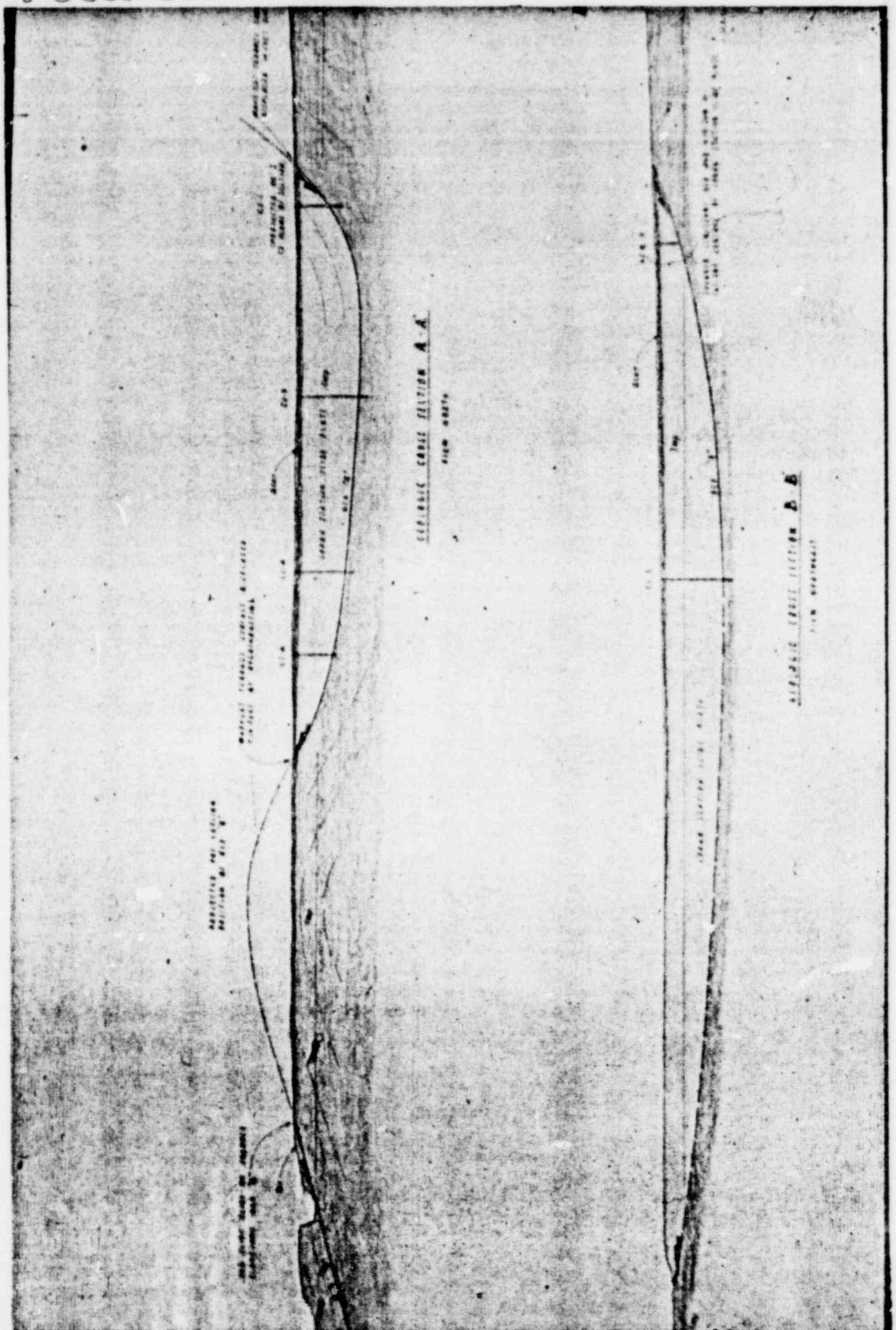


119

1463 091


PACIFIC OCEAN

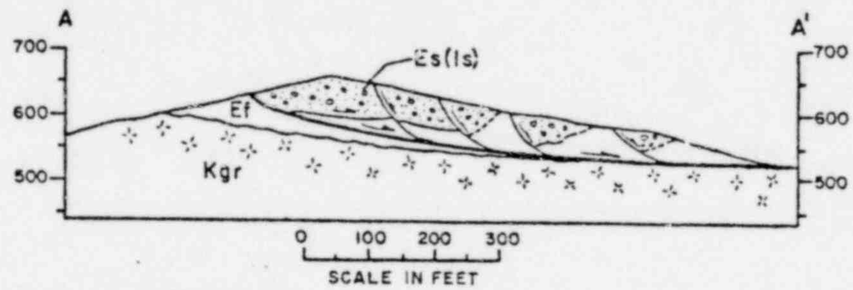
POOR ORIGINAL



1463 093

121

POOR ORIGINAL



EXPLANATION

Es Stadium Conglomerate
Ef Friars Formation
Kgr Granitic Rocks
Is Landslide Debris

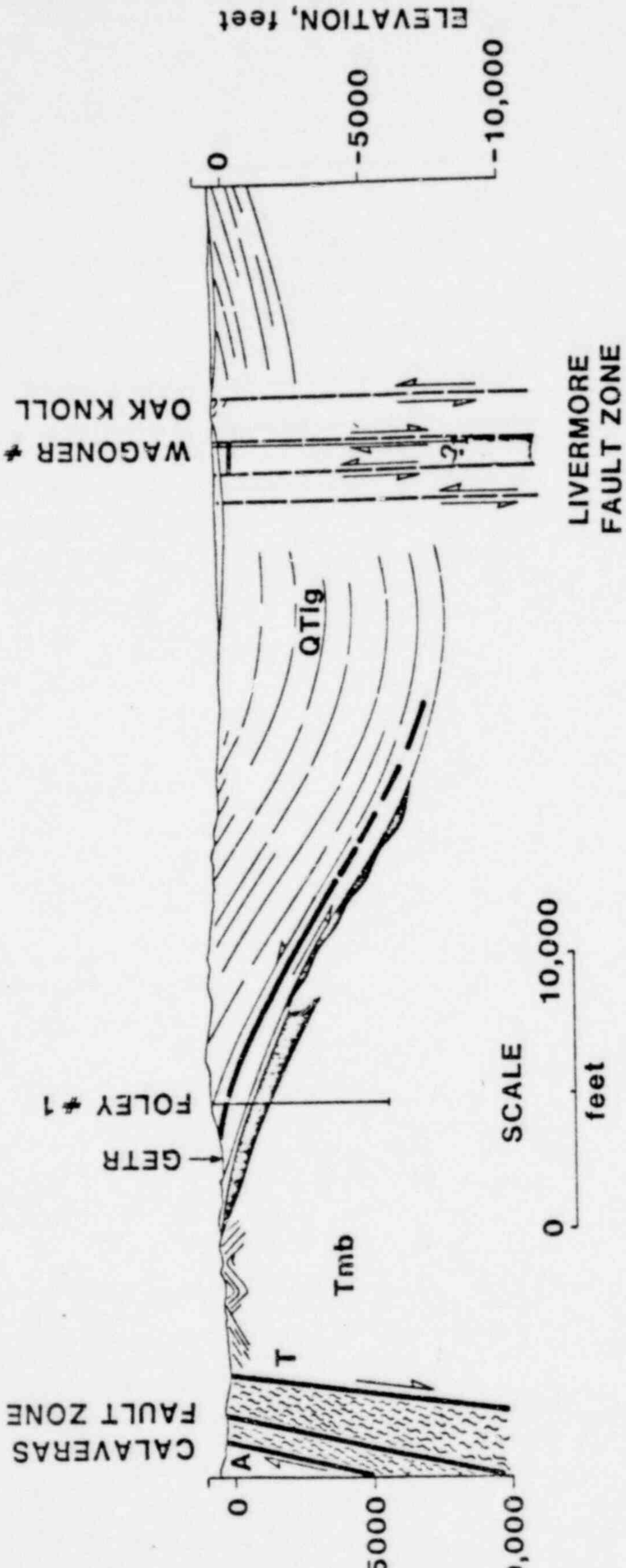
FIGURE 5. Structure of Poway Slic

1463 094

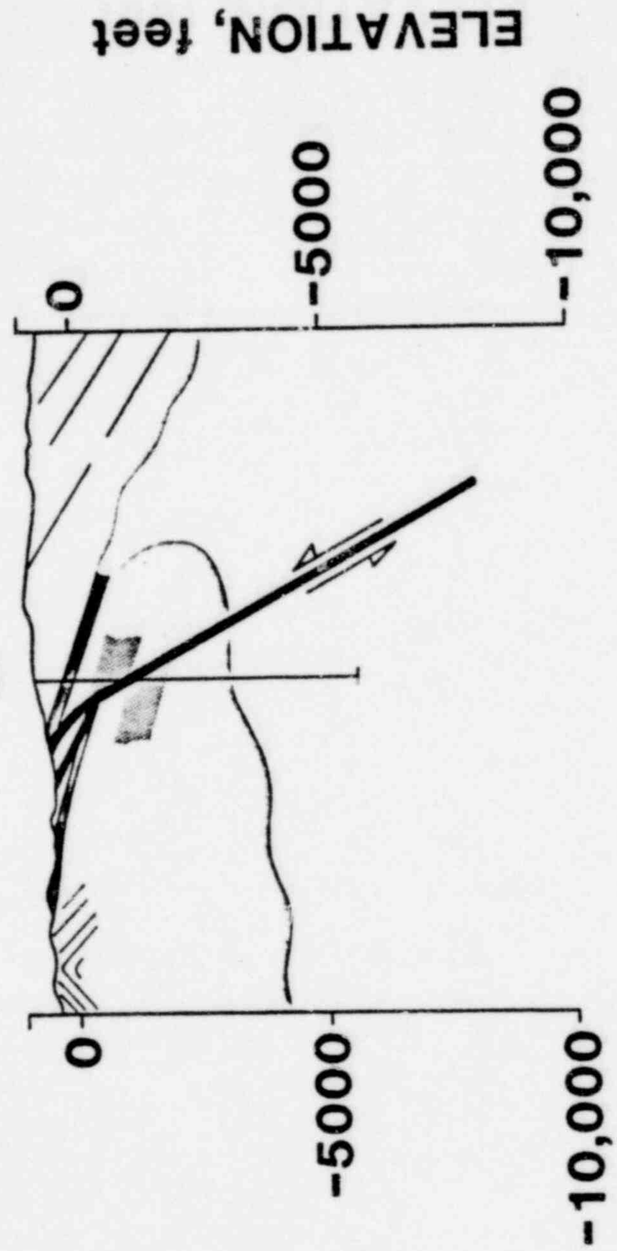
LANDSLIDE ORIGIN

- NO CONFLICT WITH REGIONAL TECTONIC SETTING
- NUMBER, ATTITUDE AND CHARACTER OF SHEARS CONSISTENT WITH RELATIONSHIPS EXPECTED IN LARGE LANDSLIDE COMPLEX
- AGE OF LANDSLIDE SUFFICIENT TO ALLOW SIGNIFICANT EROSION OF HEADSCARP
- PLEISTOCENE LANDSLIDES COMMON IN CALIFORNIA
- RENEWED MOVEMENTS OF PLEISTOCENE LANDSLIDES RESULTING FROM SEISMIC EVENTS ARE COMMON

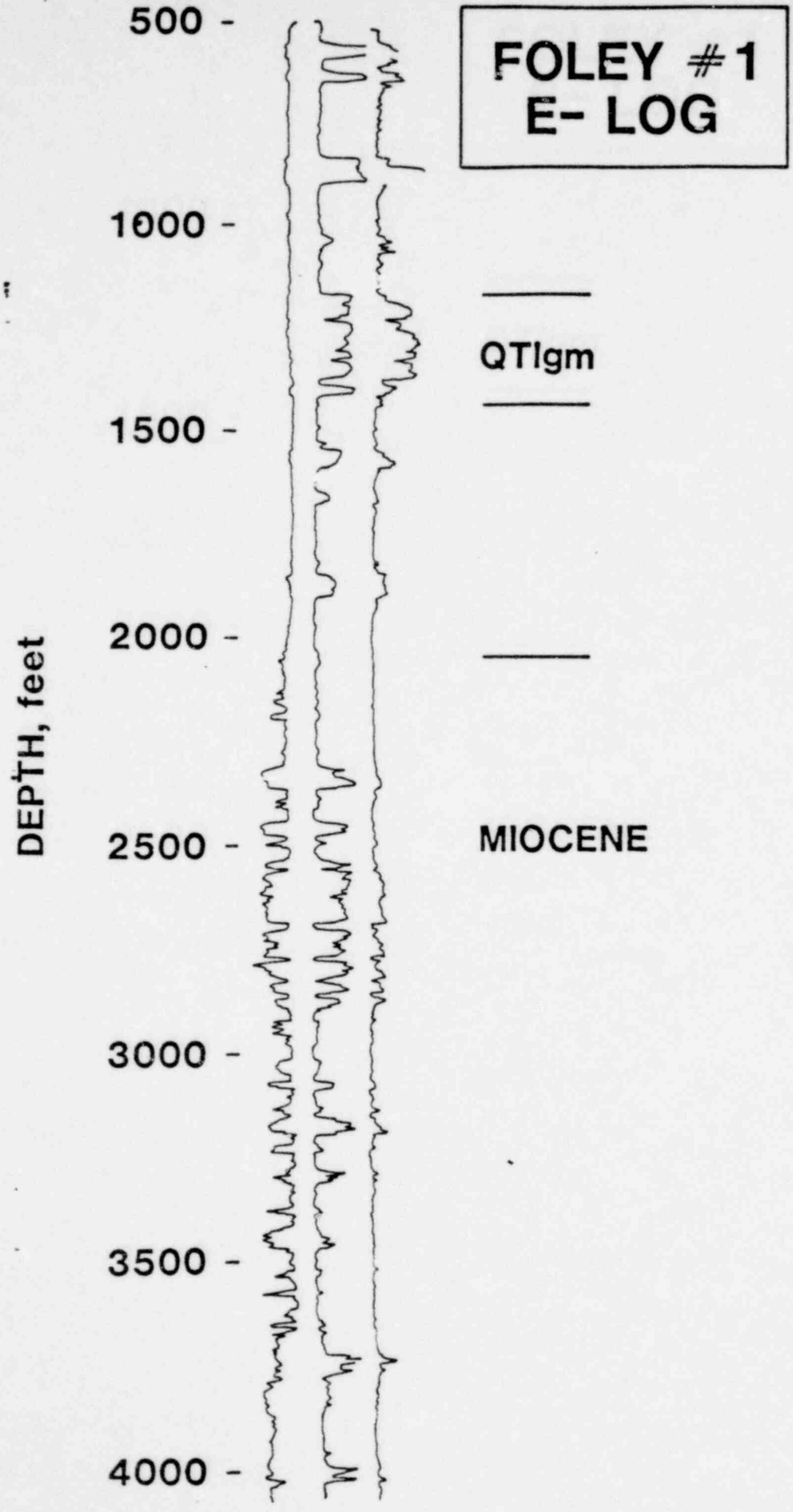
1463 095


ELEVATION, feet
0
-5000
-10,000
OAK KNOLL
WAGONER #1
LIVERMORE FAULT ZONE
QTlg
SCALE
0
10,000
feet
FOLEY #1
GETR
Tmb
T
A
CALAVERAS FAULT ZONE
0
-5000
-10,000

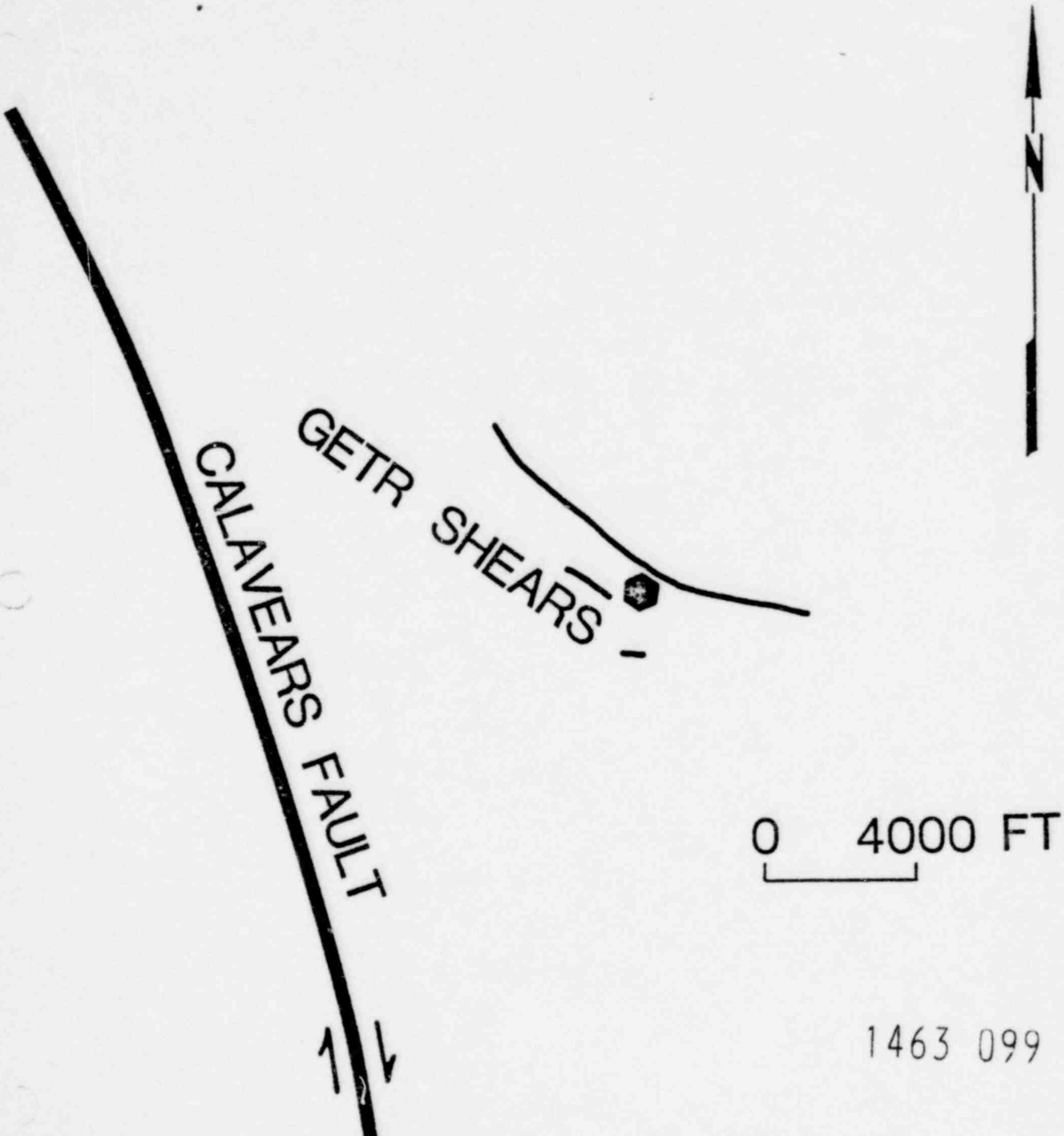
1463 096



ELEVATION, feet
0
-5000
-10,000
FOLEY #1
0
-5000
-10,000

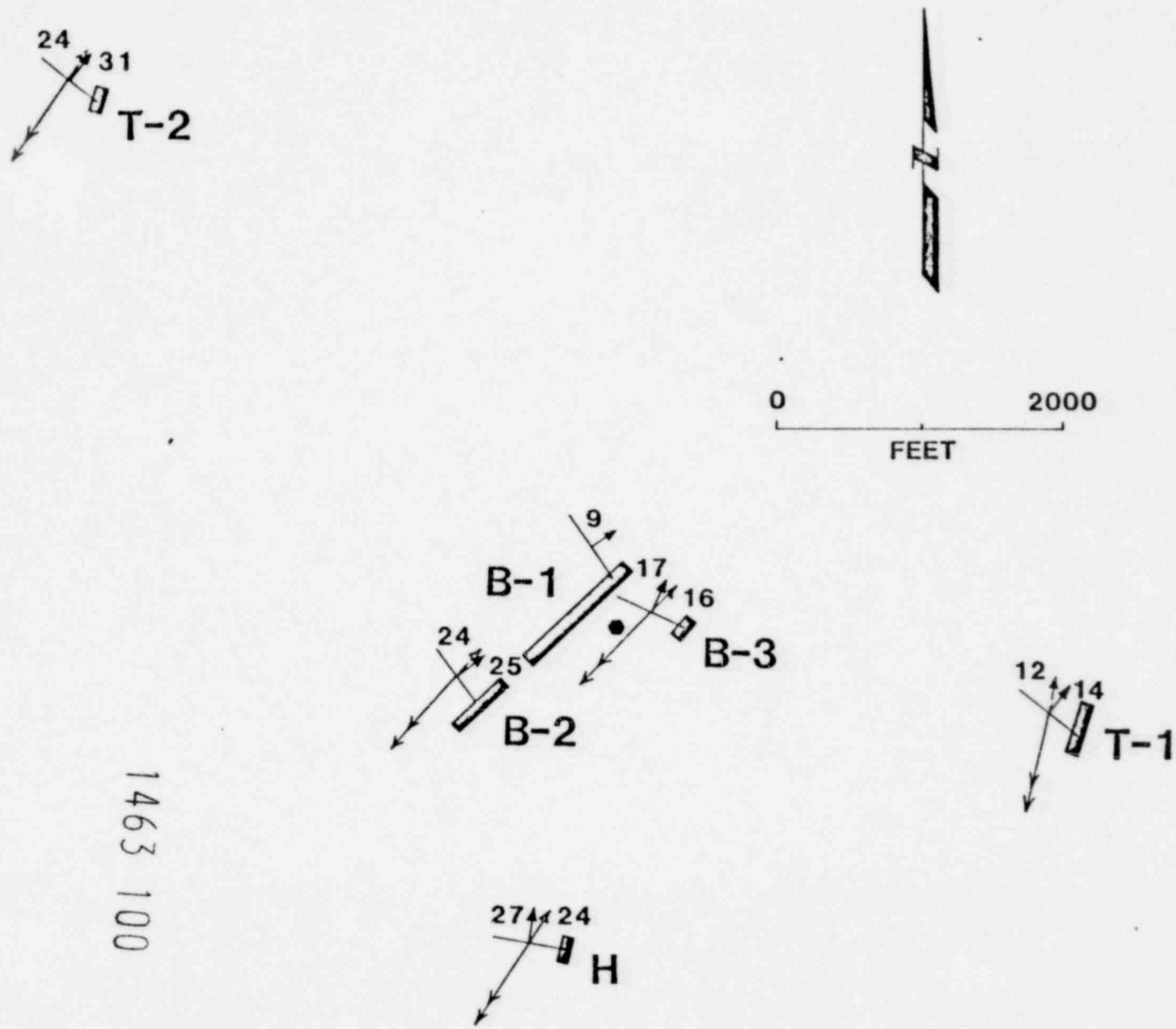

FOLEY #1
E- LOG
DEPTH, feet
500
1000
1500
2000
2500
3000
3500
4000
QTlgm
MIOCENE

1463 098



1463 099

127



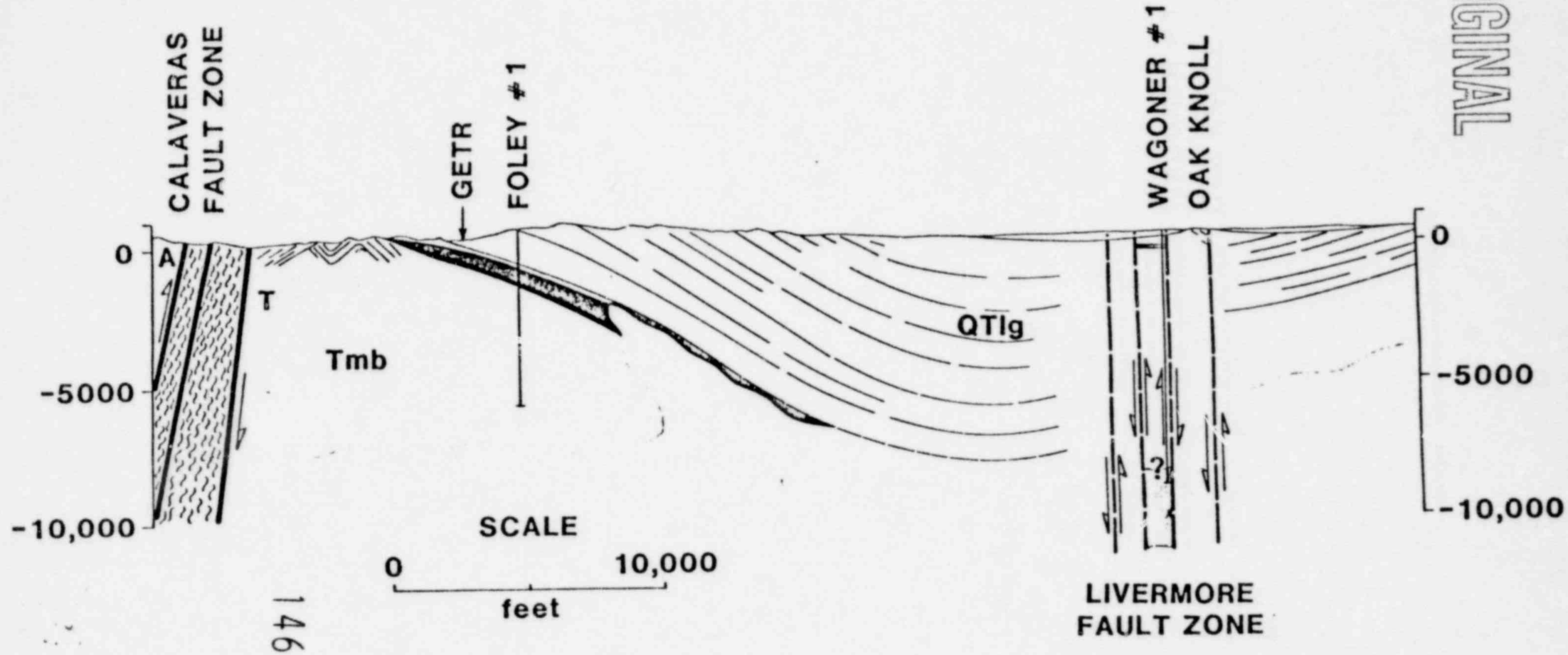
1463 100

128

THRUST FAULT ORIGIN

- THRUST FAULT DIFFICULT TO FIT INTO GEOLOGIC SETTING
- DIRECTIONS OF SLIP ON SHEARS INCONSISTENT WITH REGIONAL TECTONIC SETTING

1463 101

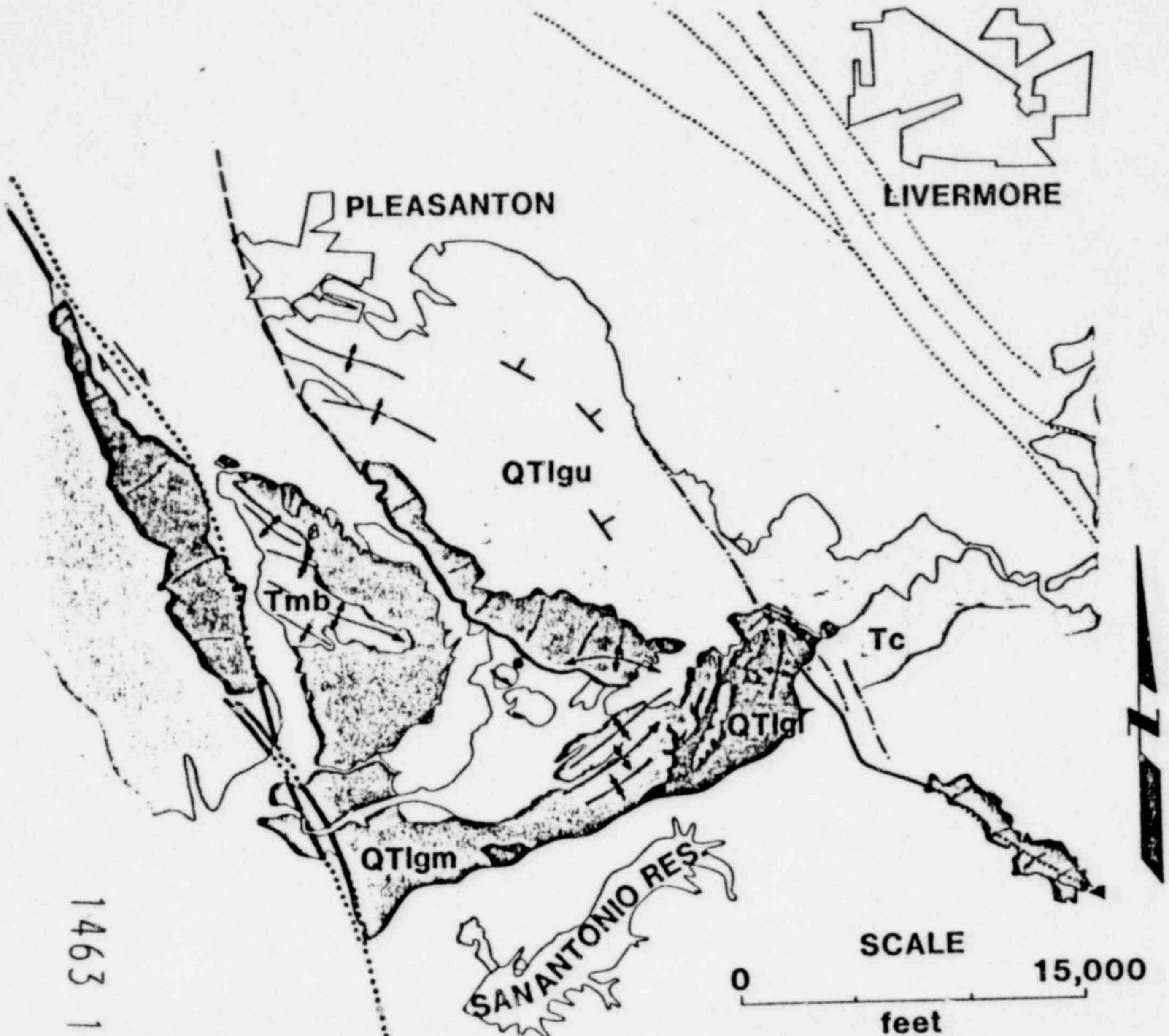

CALAVERAS
FAULT ZONE
GETR
FOLEY #1
WAGONER #1
OAK KNOLL
A
T
Tmb
QTlg
0
-5000
-10,000
ELEVATION, feet
SCALE
0
10,000
feet
LIVERMORE
FAULT ZONE

1463 102

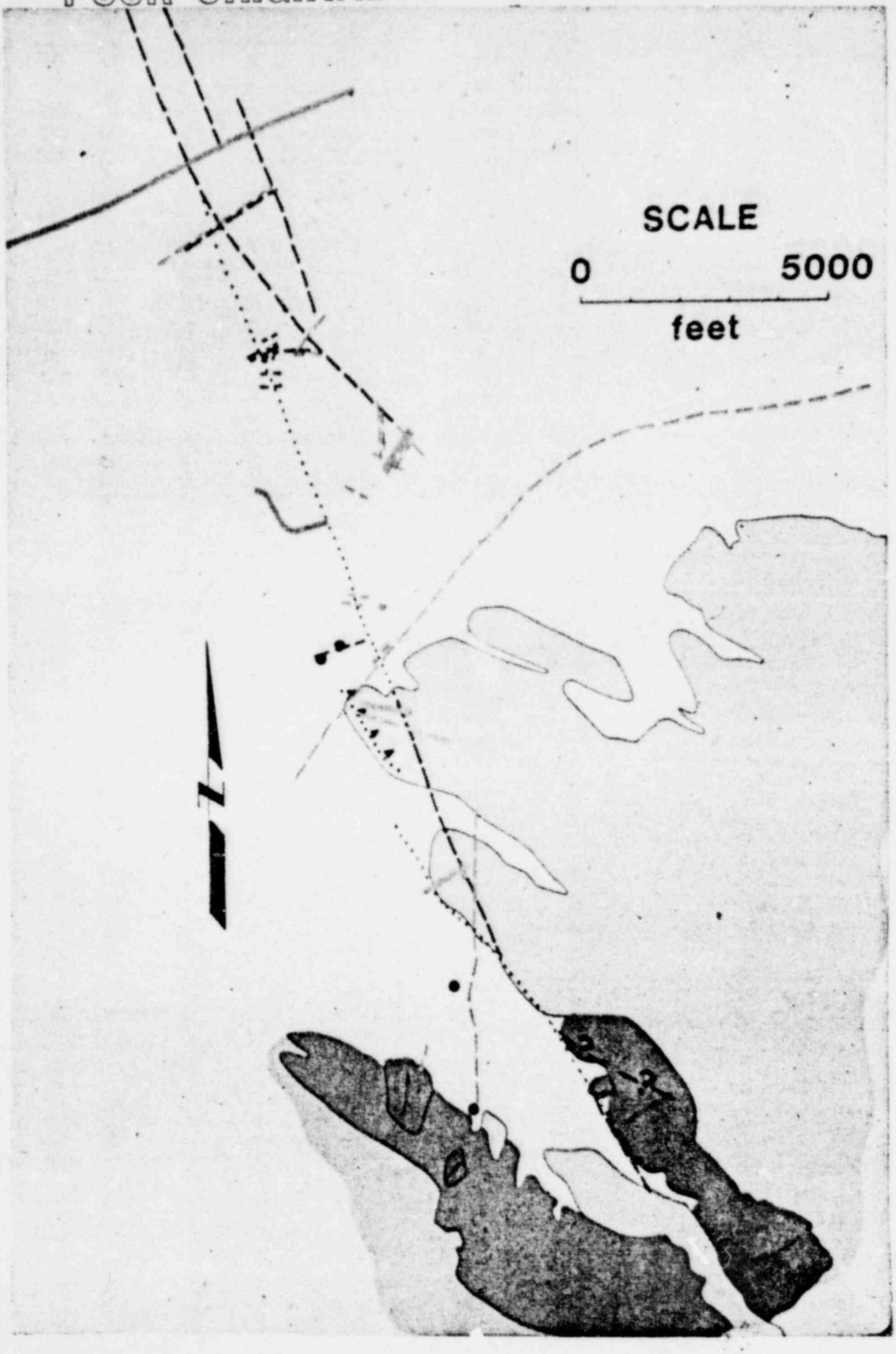
CONCLUSIONS ON ORIGIN OF SHEARS

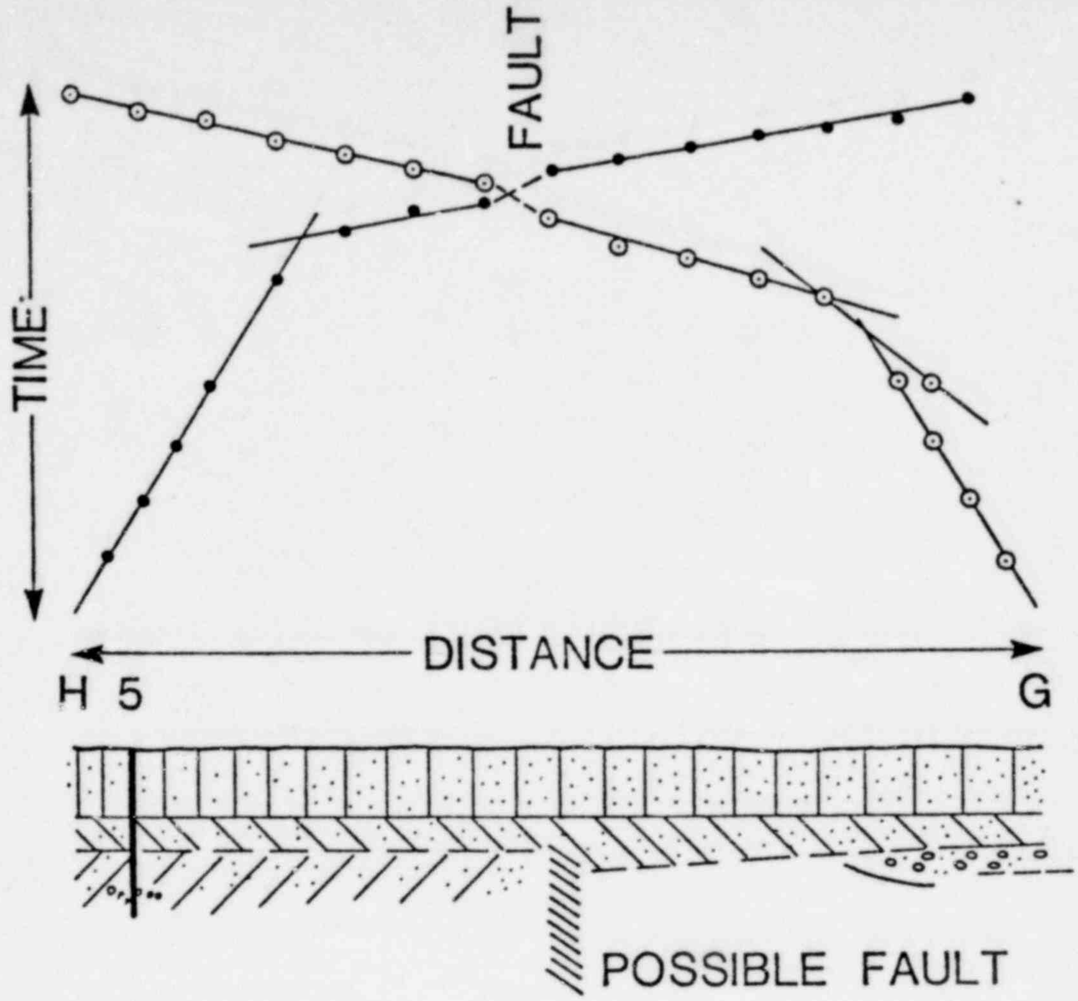
- LANDSLIDE IS MOST REASONABLE, IF NOT CONCLUSIVE, INTERPRETATION
- TO BE CONSERVATIVE, ASSUME SHEARS ARE PART OF A ZONE OF THRUST FAULTING
- CHARACTERIZE FAULT ZONE ON BASIS OF KNOWN GEOLOGIC DATA TO ESTABLISH DESIGN CRITERIA
  - LENGTH OF FAULT
  - AVERAGE SLIP RATE
  - RECURRENCE INTERVAL
  - AMOUNT OF HISTORIC OFFSET

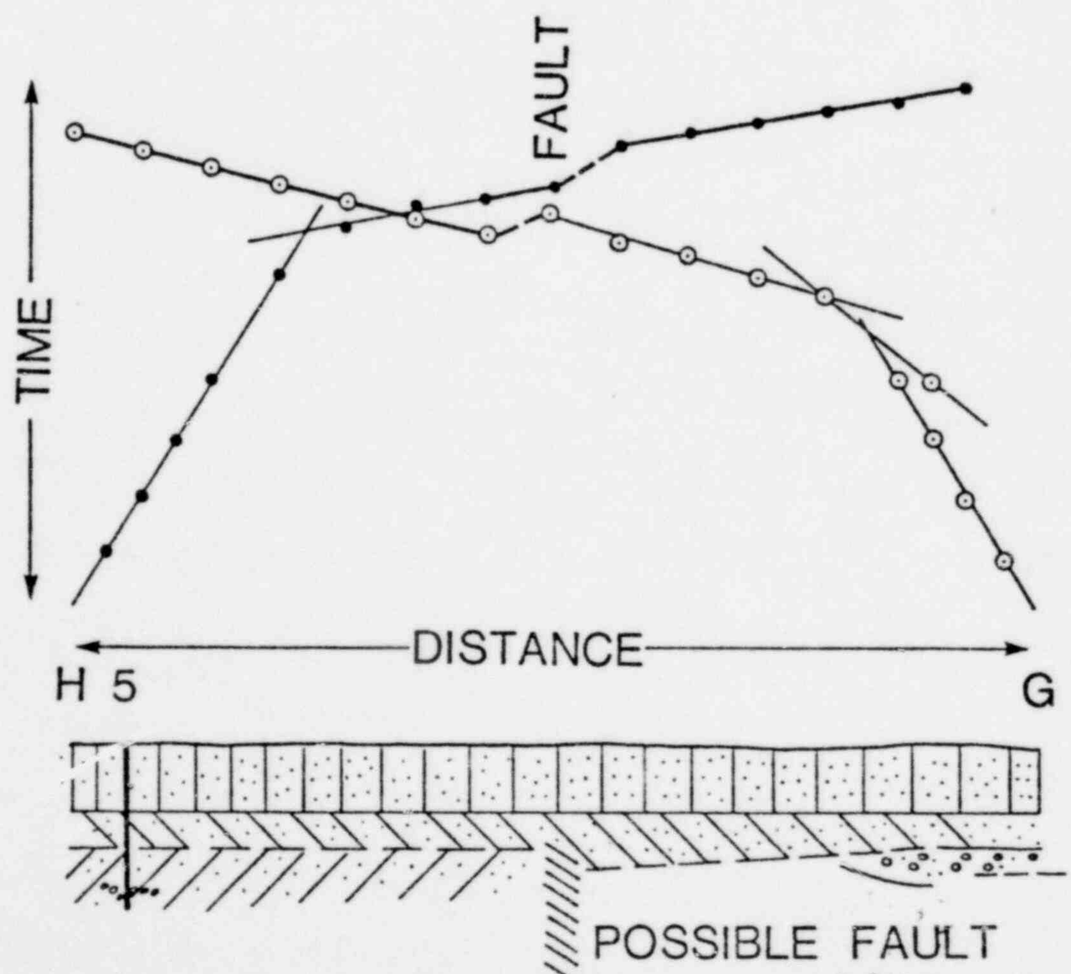
1463 103


PLEASANTON
LIVERMORE
QTlgu
Tmb
Tc
QTlgl
QTlgm
SAN ANTONIO RES.
SCALE
0
15,000
feet

1463 104

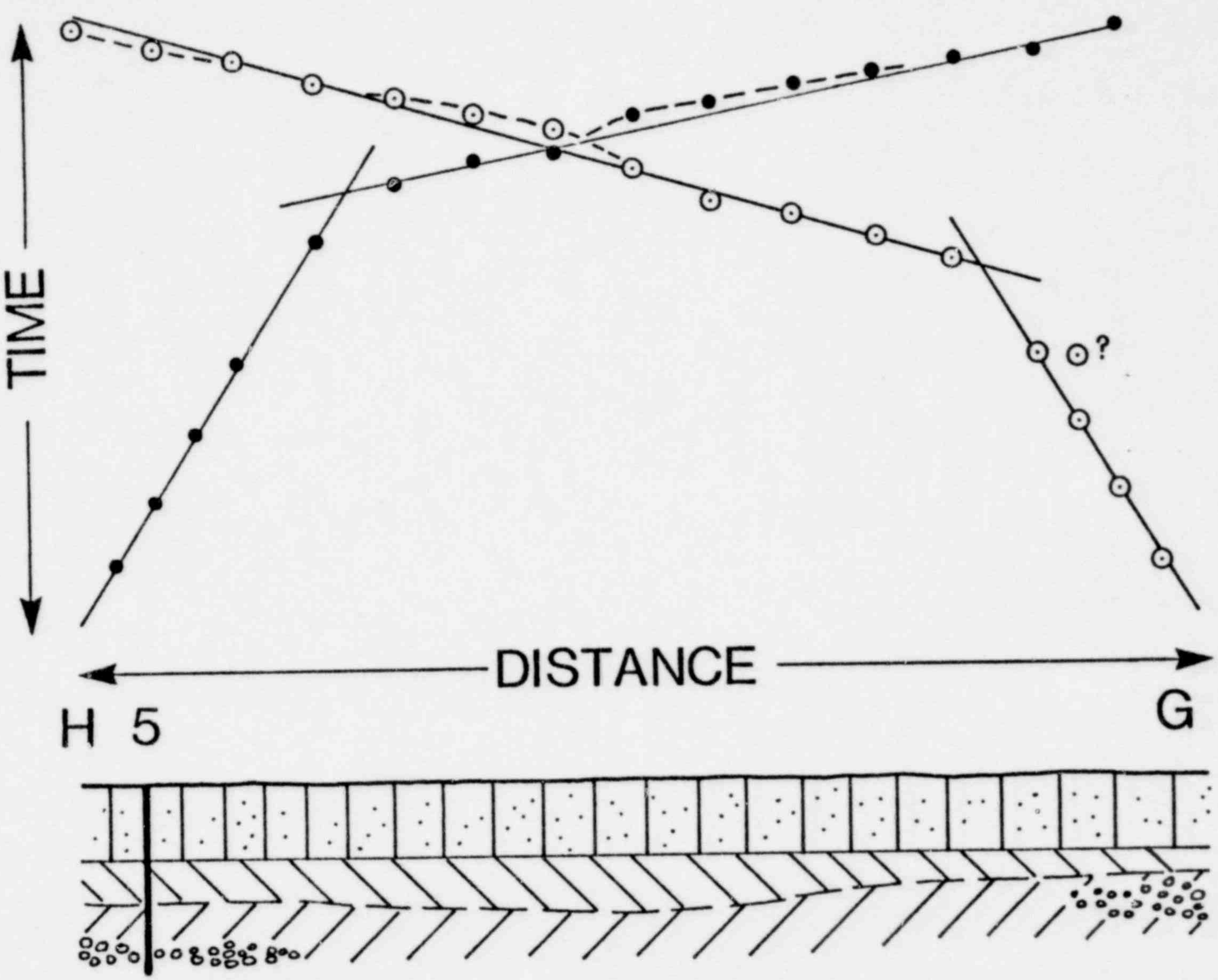

SCALE
0
5000
feet


FAULT
TIME
DISTANCE
H 5
G
POSSIBLE FAULT


FAULT
TIME
DISTANCE
H 5
G
POSSIBLE FAULT

134

1463 106



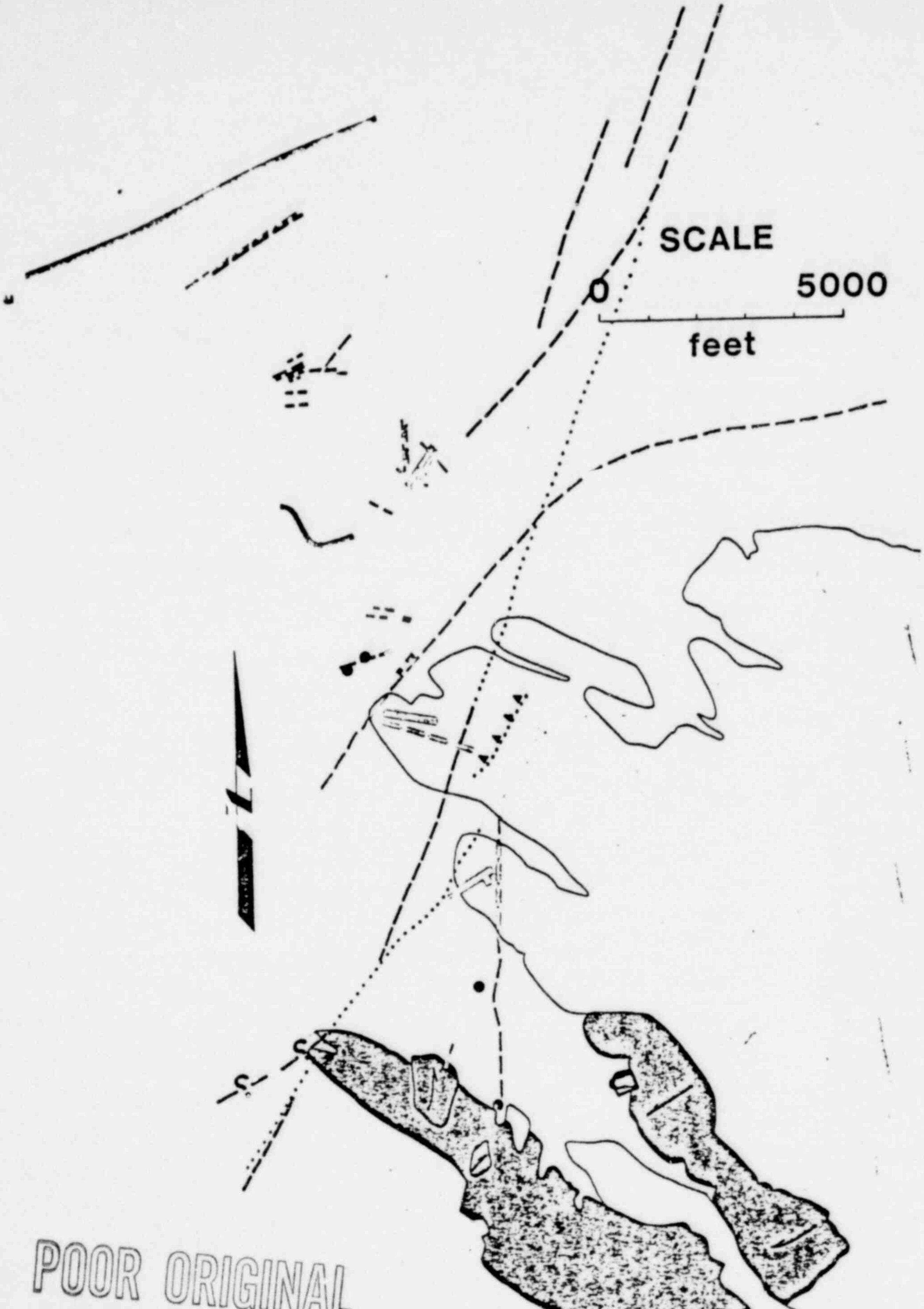
1463 107

135

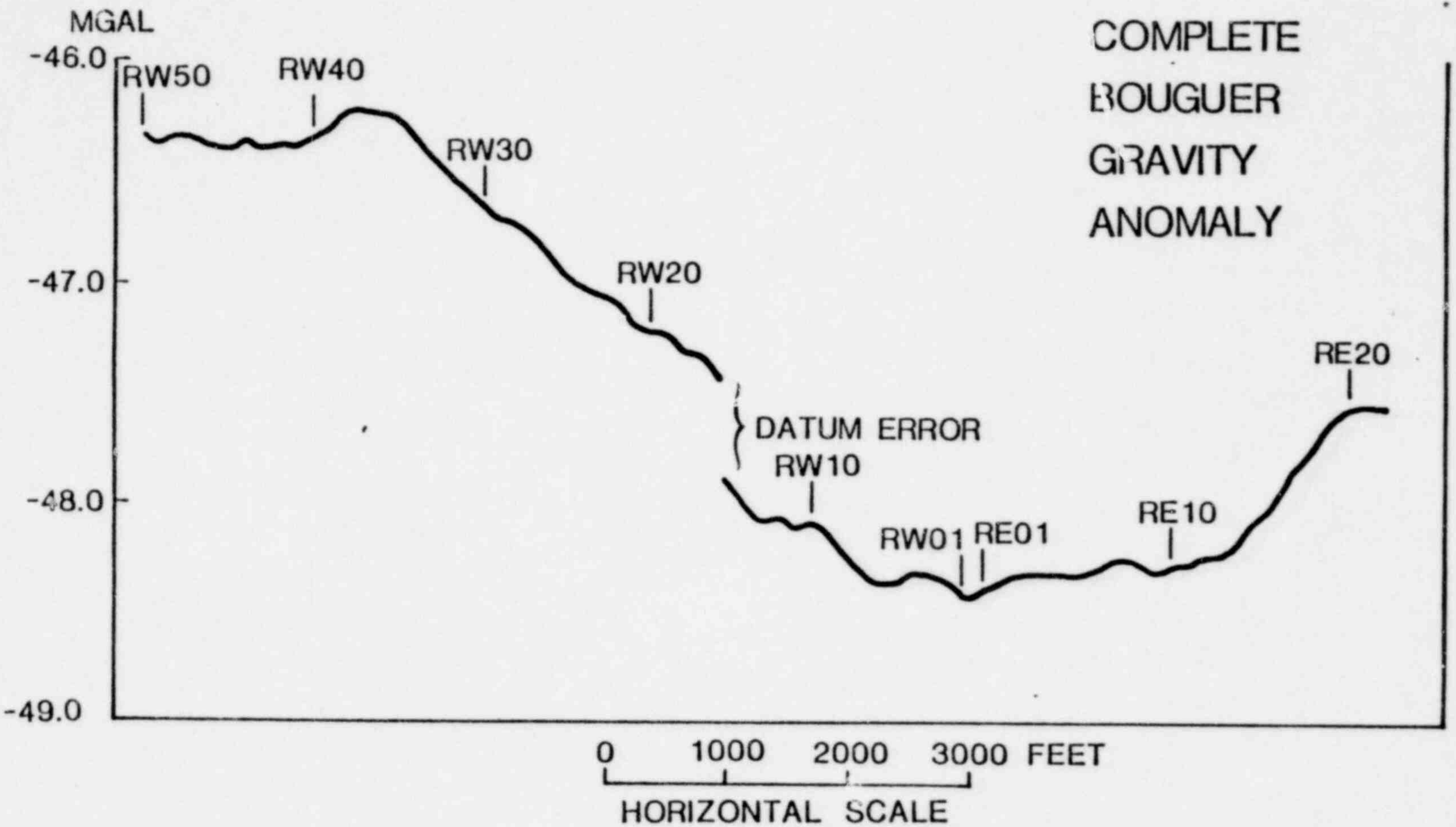
"IT IS OUR OPINION THAT INSUFFICIENT DATA EXIST TO DEFINITELY ESTABLISH THE EXISTENCE OF THE FAULT AND ITS ACTIVITY."

JUDD HULL AND ASSOCIATES, 1977, P. 7

1463 108



POOR ORIGINAL

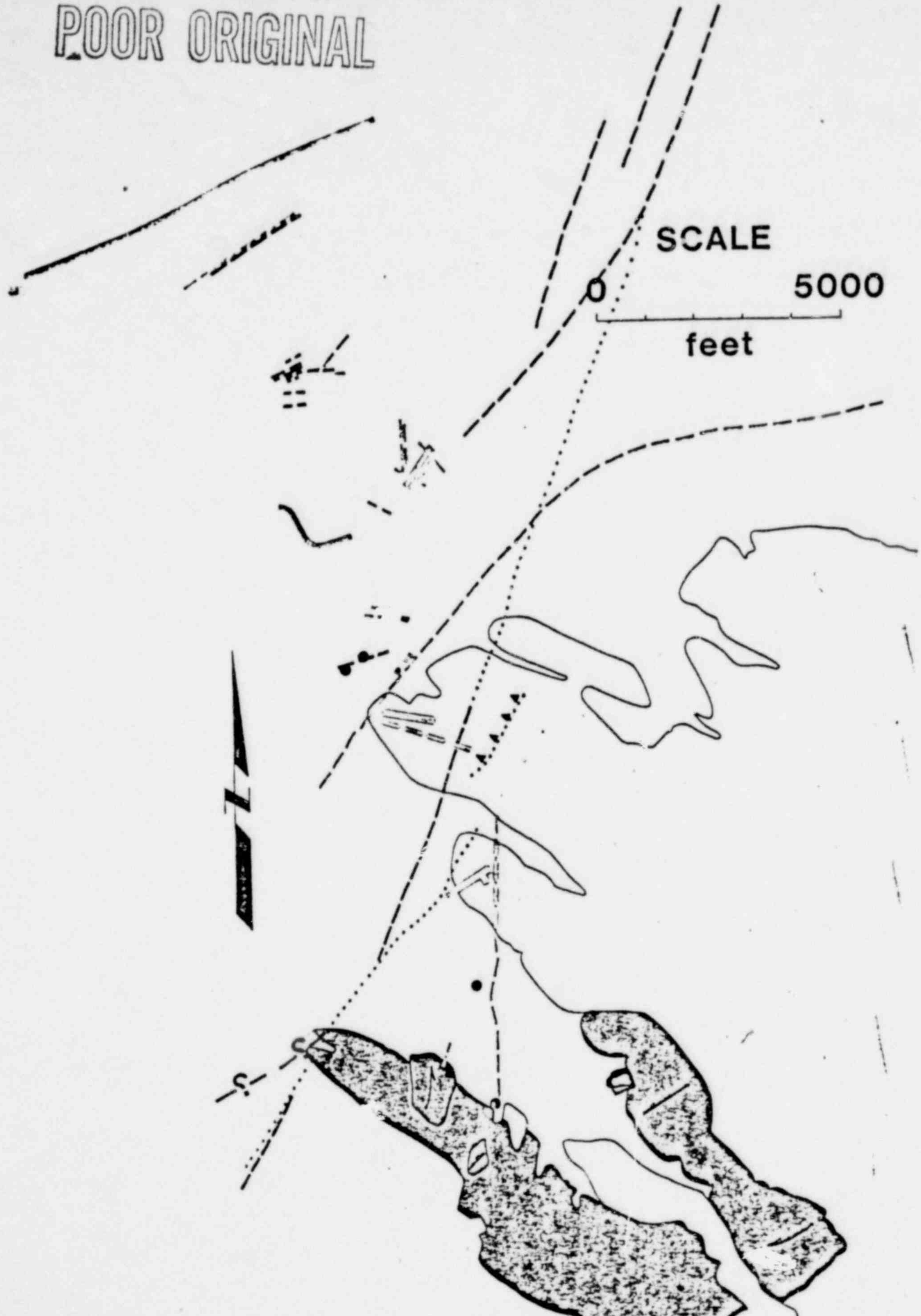


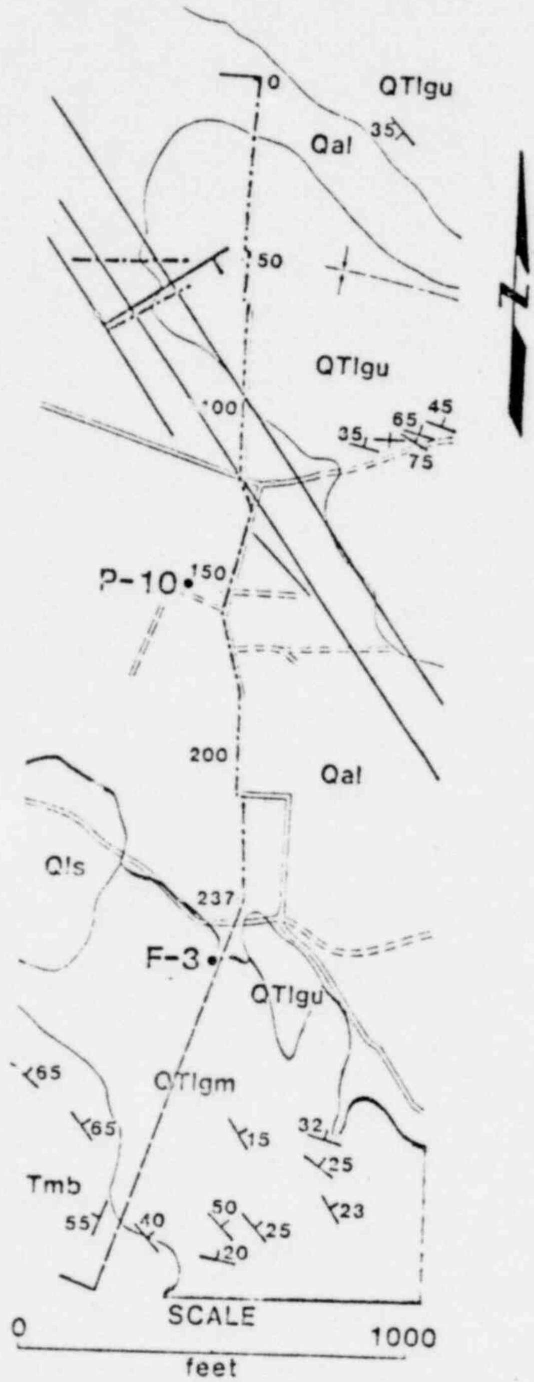
Note: Adapted from Plate 4. Radum Gravity Profile, Griscom and others (1979)

1463 110

"Because of ambiguity it is nearly impossible to prove that local gravity anomalies on detailed profiles across unconsolidated sediments are definitely related to faulting. Several closely spaced profiles will be necessary. Even if the same local gravity features are present on each profile and even if the features are colinear and located along a proposed fault trace, then the relationship, though rather compelling, is still not proven. In general detailed gravity profiles are only one piece of evidence which must be evaluated in conjunction with all other evidence when searching for proposed faults in unconsolidated sediments."

Griscom, Roberts, and Holden, O.F.R. 79-549, p. 3


SCALE
0
5000
feet

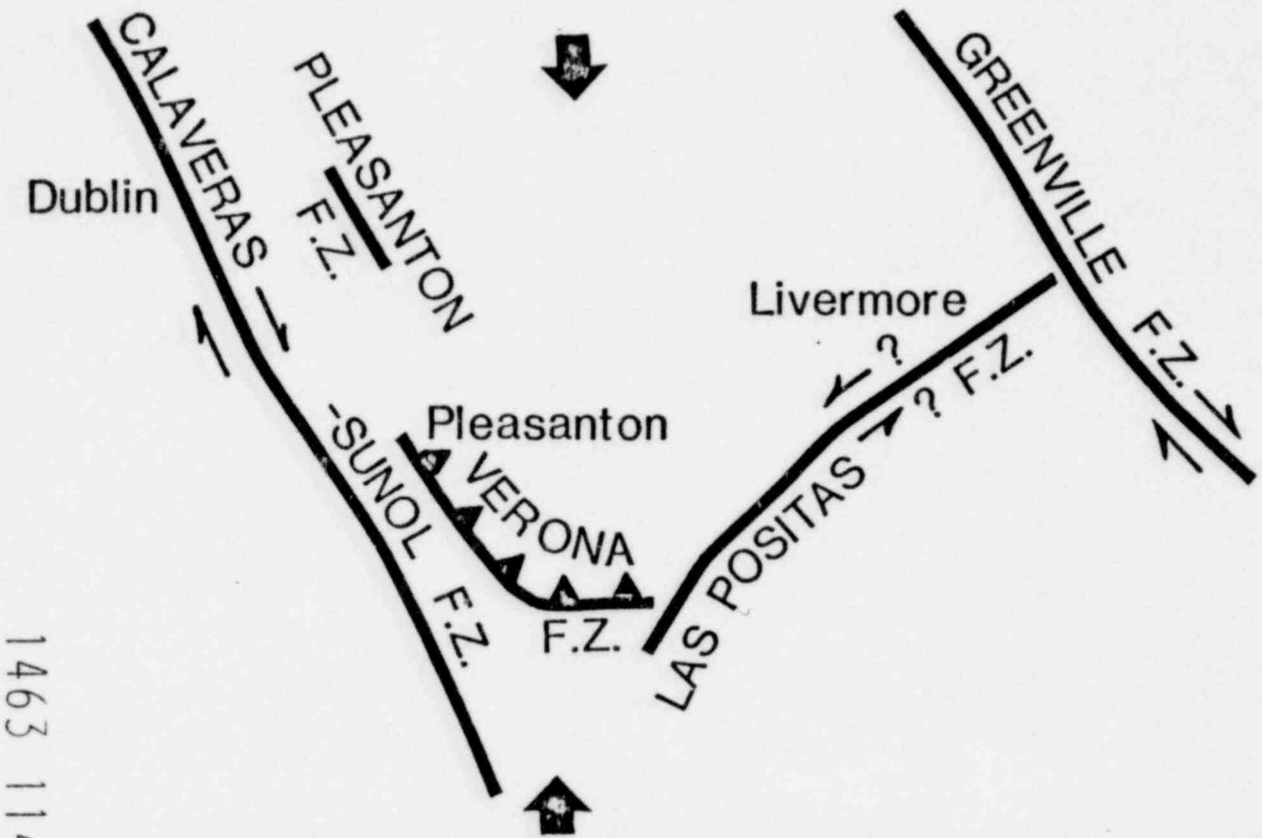


QTlgu
Qal
50
QTlgu
150
P-10
200
Qal
Qls
237
F-3
QTlgu
QTlgm
Tmb
SCALE
0
1000
feet

TECTONIC FRAMEWORK OF LIVERMORE VALLEY

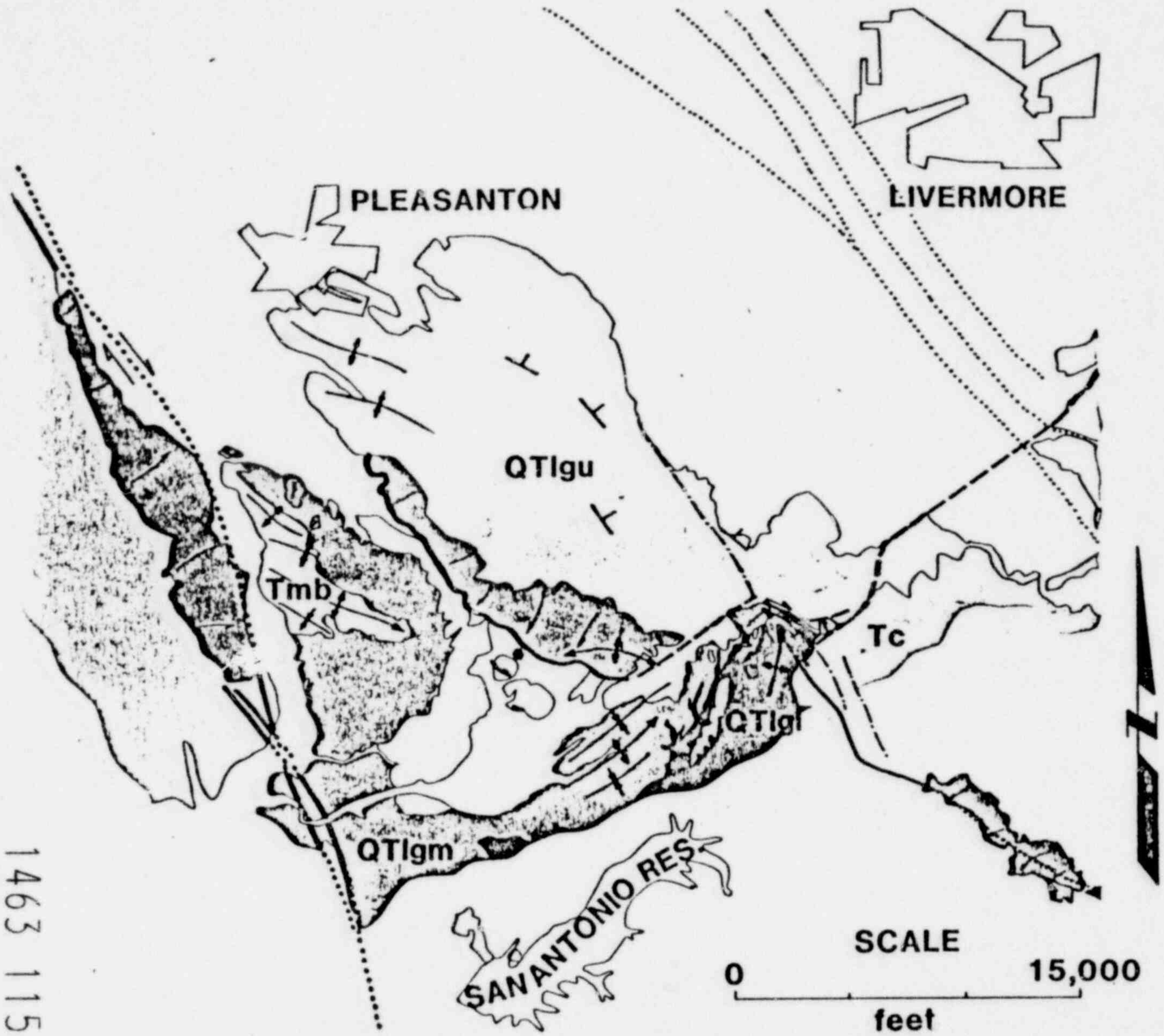
PRINCIPAL ACTIVE FAULTS ARE SHOWN

Maximum compressive stress axis represented by bold arrows



1463 114

142

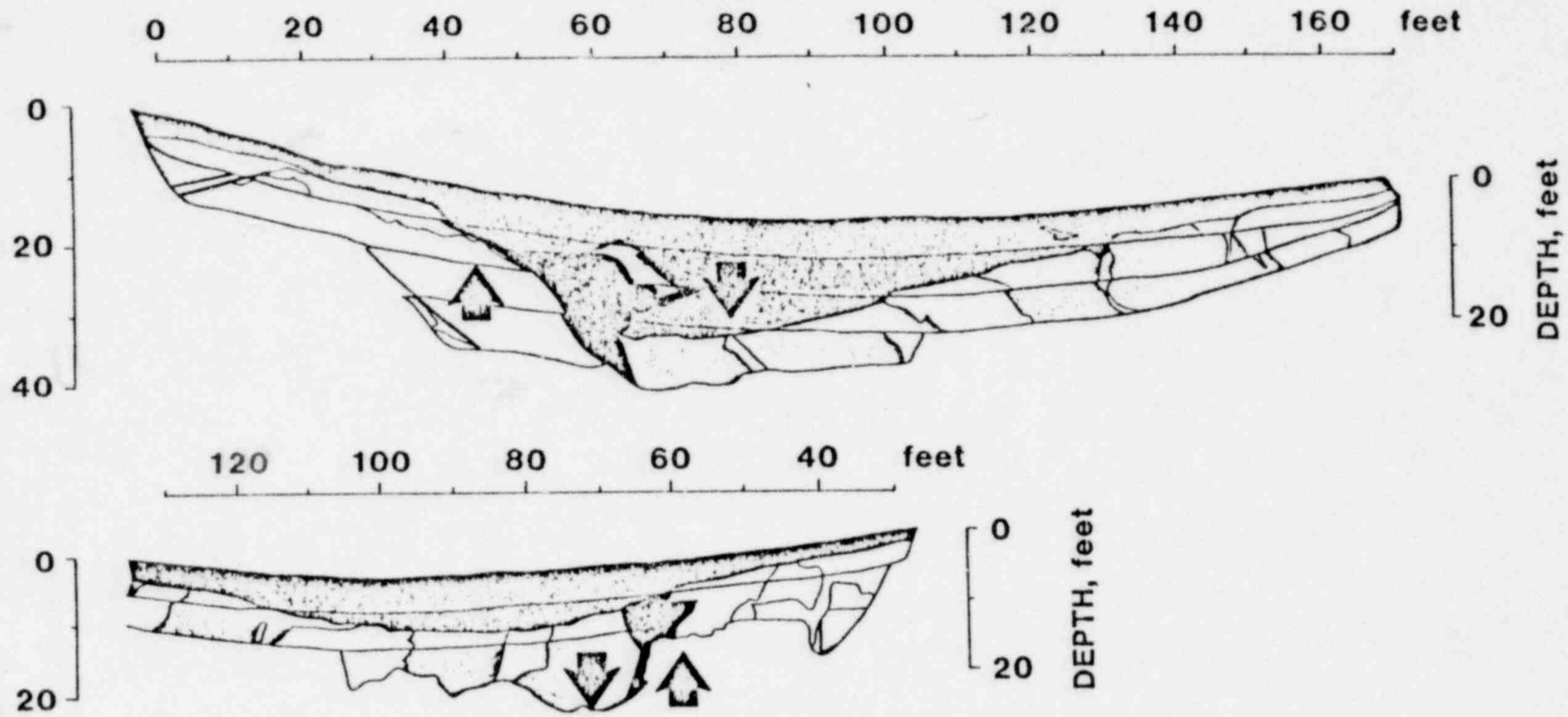


PLEASANTON
LIVERMORE
QTlgu
Tmb
Tc
QTlgl
QTlgm
SAN ANTONIO RES
SCALE
0
15,000
feet

1463 115

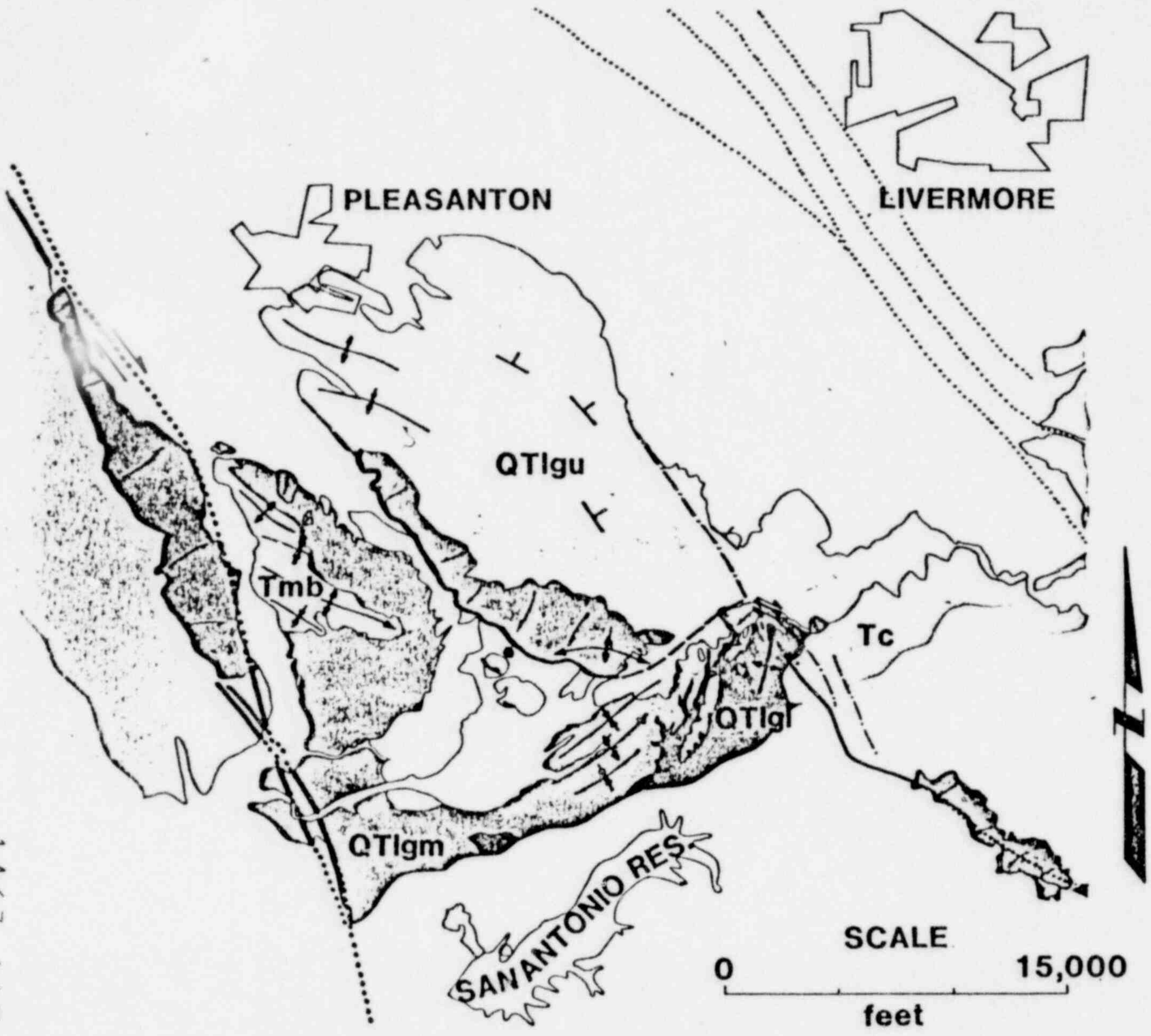
143

TRENCH A-2
NW WALL



SE WALL

1463 116


PLEASANTON
LIVERMORE
QTlgu
Tmb
Tc
QTlgl
QTlgm
SAN ANTONIO RES.
SCALE
0
15,000
feet

1463 117

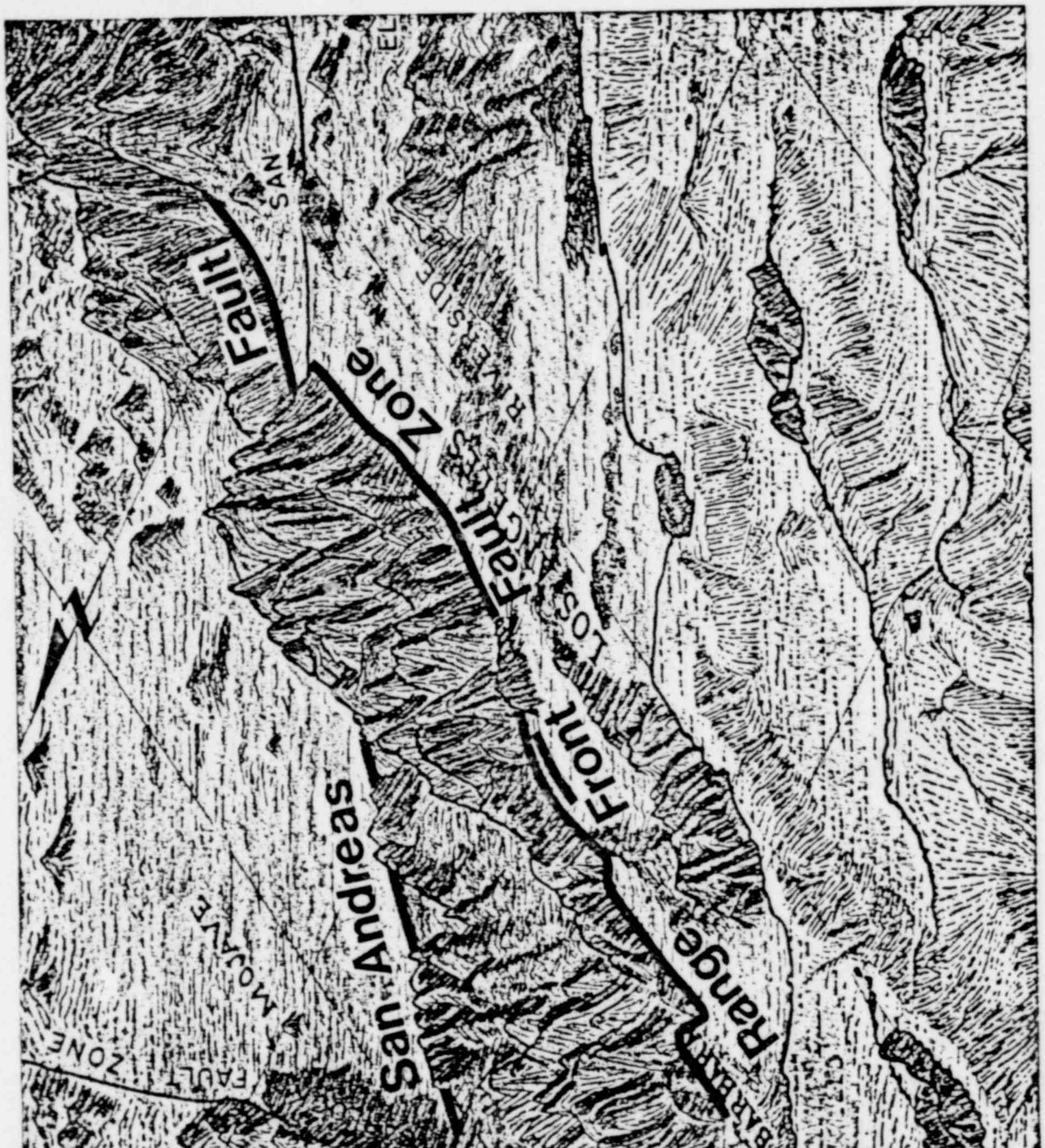
STRATIGRAPHIC HORIZON	AGE (1000's YRS BP)	MAXIMUM OFFSET (FT)	AVERAGE RATE OF STRAIN RELIEF IN./YR(MM/YR)	RECURRENCE INTERVAL FOR 3-FOOT OFFSET (1000 YRS)
CAMBIC HORIZONS	8	0	0	--
ALBIC HORIZON/STONELINE	17	3	.002(.05)	17
STAGE 5 PALEOSOL	70	12	.002(.05)	17
LIVERMORE GRAVELS	500(?)	80+	.002(.05)	19

1463 118

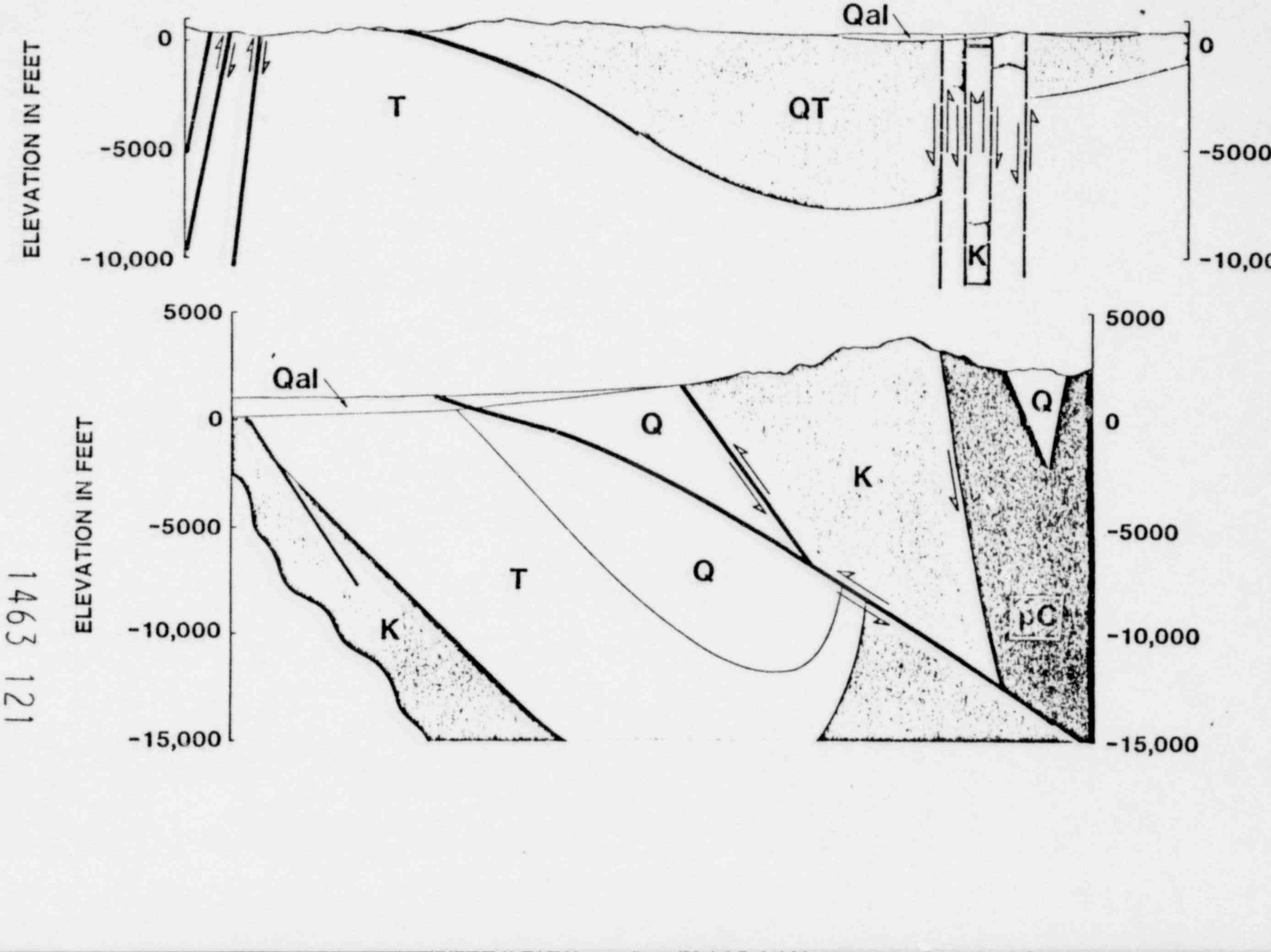
FAULT	AVERAGE SLIP RATE mm/yr	AVERAGE EARTHQUAKE RECURRENCE INTERVAL (years)
HAYWARD	5-7 (creep)	10-100
CALAVERAS	6-12 (creep)	10-100
WHITE WOLF	4	2,000
SIERRA MADRE	8	300
GETR SITE	.05	20,000

1463 119

147

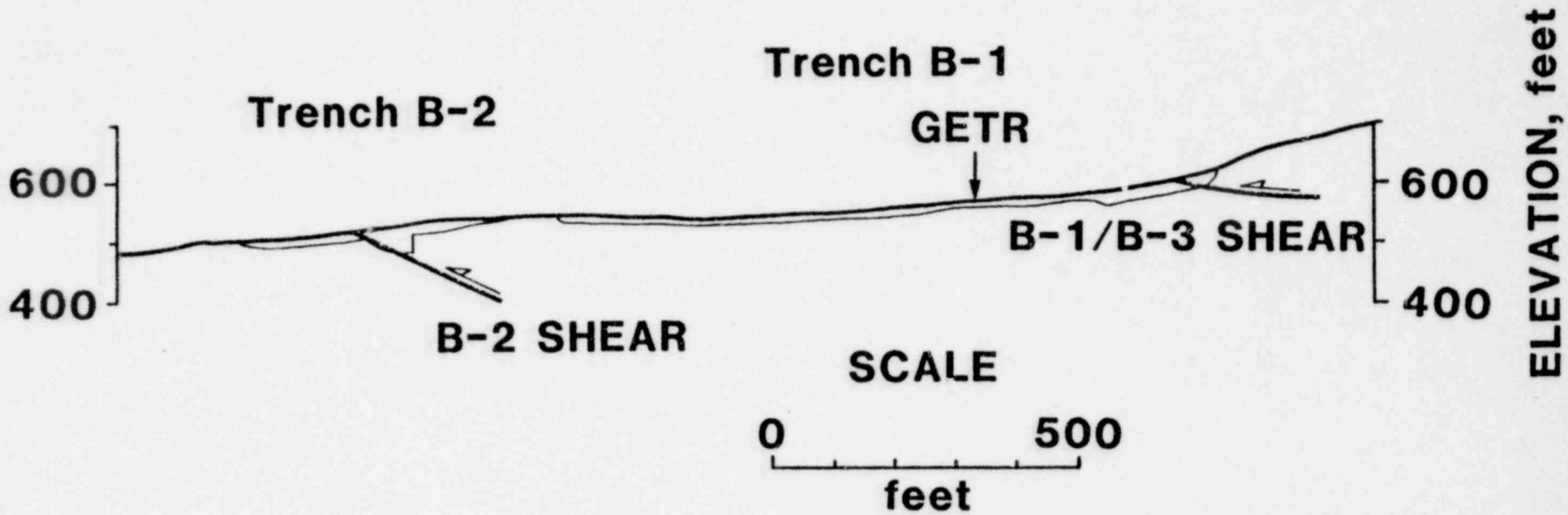

San Andreas
Fault
Zone
Fault
Front
Range
SAN
LOS
MOJAVE
FAULT ZONE

1463 120



ELEVATION IN FEET
0
-5000
-10,000
T
QT
Qal
K
0
-5000
-10,000
5000
0
-5000
-10,000
-15,000
Qal
Q
Q
K
Q
T
K
pC
5000
0
-5000
-10,000
-15,000

1463 121



Trench B-1
Trench B-2
GETR
600
400
600
400
B-1/B-3 SHEAR
B-2 SHEAR
ELEVATION, feet
SCALE
0
500
feet

1463 122

SUMMARY OF CONCLUSIONS

1. Ancient landslide most reasonable origin of shears at GETR site

2. To be conservative, a tectonic origin is assumed

3. Based on observed geologic data, the assumed fault zone has the following characteristics:

   - Length ~ 8 kilometers
   - Maximum expected offset ~ one meter
   - Future offsets most likely to occur on existing shears

3.

THE APPLICATION OF PROBABILITY IN ENGINEERING ANALYSIS

by Jack R. Benjamin

Jack R. Benjamin and Associates, Inc.

ACRS Subcommittee Hearing

General Electric Test Reactor

1463 124

November 14, 1979

153

Jack R. Benjamin & Associates, Inc.
Consulting Engineers
Court House Plaza Building, Suite 205
260 Sheridan Ave., Palo Alto, California 94306

The probability of a new offset intersecting an existing structure can be reliably forecasted.

1463 125

BASIS:

1. General: Probability methods are useful, reliable, and their use is growing exponentially.

2. Specific: Probability methods are universal rather than subject related.

1463 126

1. If probability model fits, forecasts are reliable with any level of information.

2. Uncertainty between model and reality does not invalidate forecasts.

3. Methods can be used formally or informally as in geology.

1463 127

The world is probabilistic not deterministic.

1463 128

The probability of a new offset intersecting an existing structure can be reliably forecasted.

1463 129

4

PROBABILITY ANALYSIS OF SURFACE RUPTURE OFFSET BENEATH GENERAL ELECTRIC TEST REACTOR REACTOR BUILDING

by John W. Reed

Jack R. Benjamin and Associates, Inc.

ACRS Subcommittee Meeting

General Electric Test Reactor

1463 130

November 14, 1979

159

Jack R. Benjamin & Associates, Inc.
Consulting Engineers
Court House Plaza Building, Suite 205
260 Sheridan Ave., Palo Alto, California 94306

PURPOSE OF PROBABILISTIC ANALYSIS

1. To determine the probability of occurrence of a future surface rupture offset of any size greater than zero beneath the Reactor Building foundation

2. Then to determine whether the probability of occurrence is sufficiently low so that surface rupture offset should not be considered as a design basis event

1463 131

Jack R. Benjamin & Associates, Inc.
Consulting Engineers

PROBABILITY ACCEPTANCE CRITERION

". . . . a conservative calculation showing that the probability of occurrence of potential exposures in excess of the 10CFR Part 100 guidelines is approximately 10^{-6} per year is acceptable if, when combined with reasonable qualitative arguments, the realistic probability can be shown to be lower."

USNRC Standard Review Plan
Section 2.2.3

1463 132

RESULTS AND CONCLUSION OF ANALYSIS

RESULTS

- Calculated probability of occurrence of a future surface rupture offset of any size greater than zero beneath the Reactor Building foundation complies with the criterion
- Probabilistic analysis is conservative

CONCLUSION

- Surface rupture offset should not be considered as a design basis event

1463 133

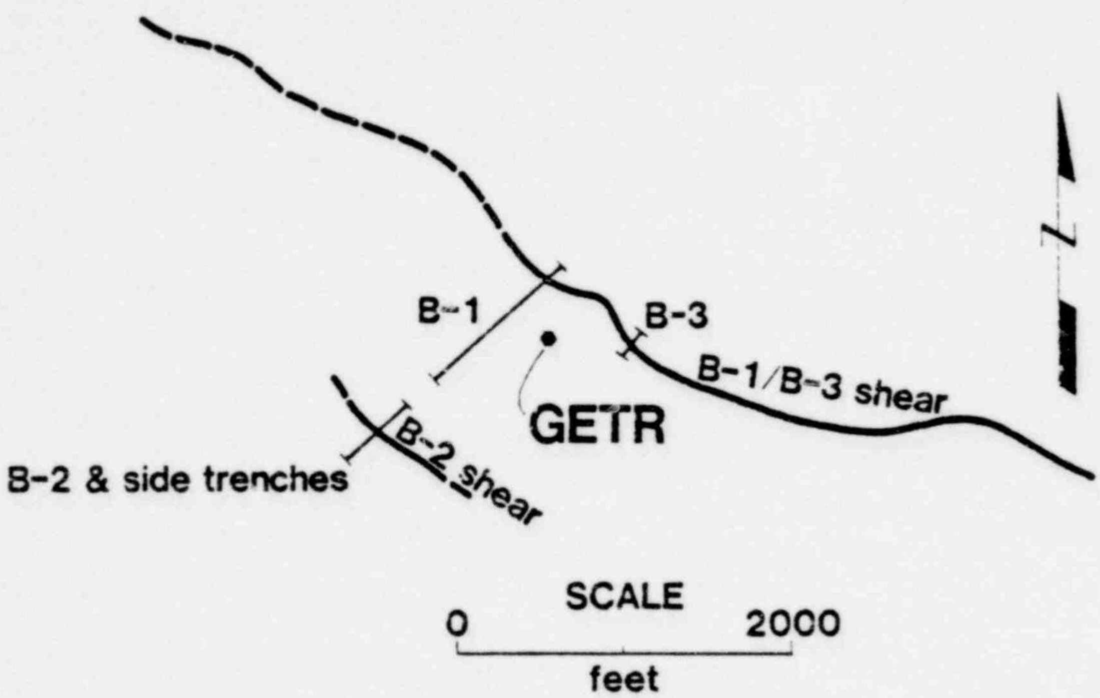
OUTLINE OF PRESENTATION OF PROBABILISTIC ANALYSIS

- Simplified approach
- Confidence level probability analysis
- Detailed model analysis

1463 134

163

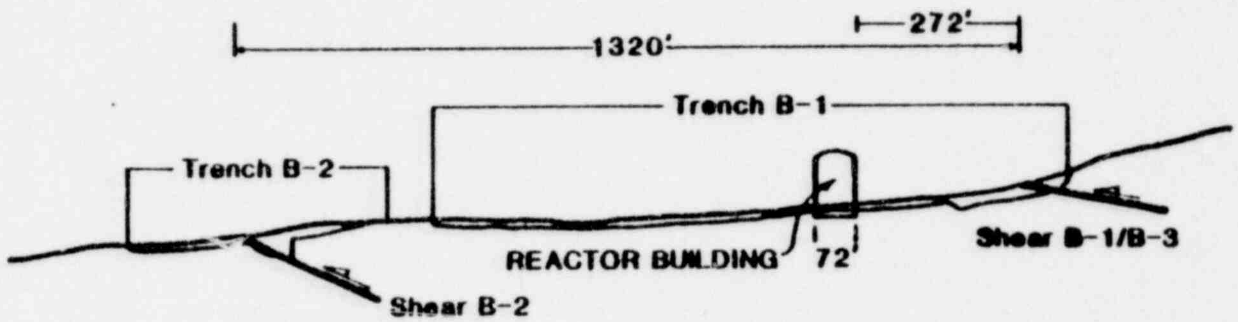
LOCATION OF SHEARS IN RELATION TO GETR



1463 135

164

CROSS-SECTION OF GETR SITE



SCALE: Horizontal = Vertical

0 200 400 600 800 1000

feet

1463 136

Jack R. Benjamin & Associates, Inc.
Consulting Engineers

165

OBSERVED OFFSET DATA

Time Period (Before Present in Years)	Maximum Offset During Time Period (ft)	
	Shear B-2	Shear B-1/B-3
0 – 8,000 to 15,000	0	0
8,000 to 15,000 – 17,000 to 20,000	3	2
17,000 to 20,000 – 70,000 to 125,000	5	10
70,000 to 125,000 – 128,000 to 195,000 or greater	80+	40+

1463 137

BASIC PROBABILITY PARAMETERS

Annual probability of occurrence of an offset beneath Reactor Building foundation, P:

$$P = P_1 \times P_2$$

Where:

P_1 = annual probability that an offset will occur between shears B-2 and B-1/B-3

P_2 = probability that an offset will occur beneath the Reactor Building foundation, given that an offset occurs between the shears

1463 138

SIMPLIFIED APPROACH

t = 128,000 years	t = 195,000 years
$P_1 \cong 1/128,000$	$P_1 \cong 1/195,000$
$P_2 \cong 72/1320$	$P_2 \cong 72/1320$

$$P = P_1 \times P_2$$

$P = 1/128,000 \times 72/1320$	$P = 1/195,000 \times 72/1320$
$P = 4.3 \times 10^{-7}$	$P = 2.8 \times 10^{-7}$

1463 139

Jack R. Benjamin & Associates, Inc.
Consulting Engineers

CONFIDENCE LEVEL PROBABILITY ANALYSIS

$$P_1 = -\ln(1-C)/t$$

Where:

C = Confidence level probability

t = Number of years without an offset between the shears

$$P_2 = (\ell + b)/(L - b)$$

Where:

ℓ = Width of Reactor Building

L = Distance between two existing shears

b = Width of offset at ground surface

$$P = P_1 \times P_2$$

1463 140

PROBABILITY OF OFFSET OCCURRING BENEATH REACTOR BUILDING FOUNDATION

Confidence Level Probability	No. of yrs. without an event	
	t = 128,000 yrs	t = 195,000 yrs
0.95	1.4×10^{-6}	8.9×10^{-7}
0.90	1.0×10^{-6}	6.8×10^{-7}
0.50	3.1×10^{-7}	2.1×10^{-7}

1463 141

Jack R. Benjamin & Associates, Inc.
Consulting Engineers

DETAILED MODEL ANALYSIS

$$P_1 = \phi \lambda e^{-\lambda}$$

Where:

λ = Mean time rate of occurrence of offsets

ϕ = Probability that an offset will occur between the two shears given that an offset occurs

$$P_2 = (\ell + b)/(L - b)$$

Where the parameters are the same as the confidence level probability analysis

$$P = P_1 \times P_2$$

1463 142

METHOD FOR OBTAINING PROBABILITY DENSITY FUNCTION FOR λ AND ϕ

$$p(\lambda,\phi) = \psi \, L(\lambda,\phi \mid \text{data}) \cdot p'(\lambda,\phi)$$

Where:

ψ = normalizing constant

$p'(\lambda,\phi)$ = prior probability density function

$$L(\lambda,\phi \mid \text{data}) = \prod_{i=1}^{4} \frac{(\lambda t_i)^{n_i} e^{-\lambda t_i}}{n_i!} (1-\phi)^{n_i}$$

t_i = time period (years)

n_i = number of events in time period t_i

$$p(\lambda,\phi) = \frac{t^{n+1} \lambda^n e^{-\lambda t}}{n!} (n+1)(1-\phi)^n \quad \text{for } 0 \leq \phi \leq 1, \; \lambda \geq 0$$

Where:

$$t = \sum_{i=1}^{4} t_i$$

$$n = \sum_{i=1}^{4} n_i$$

1463 143

ESTIMATED VALUES FOR PROBABILITY P_1

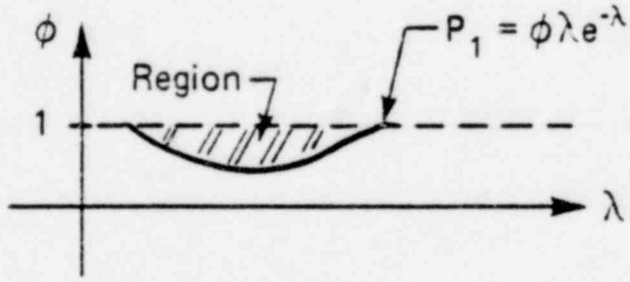
Weighted estimate

$$\check{P}_1 = \int_0^1 \int_0^\infty \phi \lambda e^{-\lambda} \, p(\lambda, \phi) \, d\lambda \, d\phi$$

$$\check{P}_1 = \left(\frac{t}{t+1}\right)^{n+2} \cdot \frac{n+1}{n+2} \cdot \frac{1}{t}$$

$$\boxed{\check{P}_1 < \frac{1}{t}}$$

Confidence limits



1463 144

PROBABILITIES OF OFFSET BENEATH REACTOR BUILDING FOUNDATION

Analysis Basis	Detailed Model*		Confidence Level Prob. Analysis	
	t = 128,000 yrs.	t = 195,000 yrs.	t = 128,000 yrs.	t = 195,000 yrs.
Weighted estimate	4.5×10^{-7}	3.0×10^{-7}	NA	NA
0.95 Confidence level	1.3×10^{-6}	8.4×10^{-7}	1.4×10^{-6}	8.9×10^{-7}
0.90 Confidence level	1.0×10^{-6}	6.7×10^{-7}	1.0×10^{-6}	6.8×10^{-7}
0.50 Confidence level	2.9×10^{-7}	1.9×10^{-7}	3.1×10^{-7}	2.1×10^{-7}

*Based on n = 15

1463 145

Jack R. Benjamin & Associates, Inc.
Consulting Engineers

CONFIDENCE LEVELS FOR OFFSET BENEATH REACTOR BUILDING FOUNDATION FOR 10^{-6} CRITERION PROBABILITY VALUE

Detailed Model*		Confidence Level Prob. Analysis	
t = 128,000 yrs	t = 195,000 yrs	t = 128,000 yrs	t = 195,000 yrs
0.91	0.97	0.89	0.96

*Based on n = 15

1463 146

175

EVALUATION OF CONSERVATISM

- Probability of potential consequences are at least one order of magnitude lower
- Offsets can occur outside of area between the two shears
- Conclusion is based on t = 128,000 years. An average value between 128,000 years and 195,000 years is more appropriate. Furthermore, the age of unfaulted soil material is probably older than 195,000 years
- Prior distribution for λ and ϕ was conservatively assumed in Detailed Model
- Two-dimensional geometric model is conservative

1463 147

Jack R. Benjamin & Associates, Inc.
Consulting Engineers

SUMMARY AND CONCLUSION

- Weighted estimate probability value is less than 10^{-6}
- 0.90 Confidence level value is essentially equal to 10^{-6}
- Probabilistic analysis is conservative
- Analysis and results comply with criterion

Hence

- Surface rupture offset of any size should not be considered as a design basis event

1463 148

177

GENERAL ELECTRIC TEST REACTOR

ACRS SUBCOMMITTEE MEETING

NOVEMBER 14, 1979

1463 149

178

INTRODUCTION

-- GETR IS A 50 MWT. LIGHT WATER COOLED REACTOR AT VNC NEAR PLEASANTON, CALIFORNIA

-- OL ISSUED	1 - 7 - 59
-- POWER INCREASE FROM 33 TO 50 MWT	10 - 6 - 66
-- REQUEST FOR LICENSE RENEWAL	10 - 21 - 75
-- LICENSE EXPIRATION	10 - 6 - 76

-- 1977 -- NRC STAFF EVALUATION OF GETR GEOLOGY/SEISMOLOGY INITIATED AS PART OF LICENSE RENEWAL REVIEW

-- 8/77 -- USGS OPEN-FILE REPORT NO. 77-689 INDICATED VERONA FAULT CLOSE TO GETR

-- 10/77 -- EVIDENCE OF FAULTING OBSERVED IN TRENCHES AT SITE

-- OCTOBER 24, 1977 -- ORDER TO SHOW CAUSE ISSUED

1463 150

ORDER TO SHOW CAUSE

-- REQUIRED THAT GETR BE PLACED AND MAINTAINED SAFELY IN A COLD SHUTDOWN CONDITION ON OCTOBER 27, 1979

-- REQUIRED GE SHOW CAUSE WHY SUSPENSION SHOULD NOT BE CONTINUED

-- ISSUES OF ORDER:

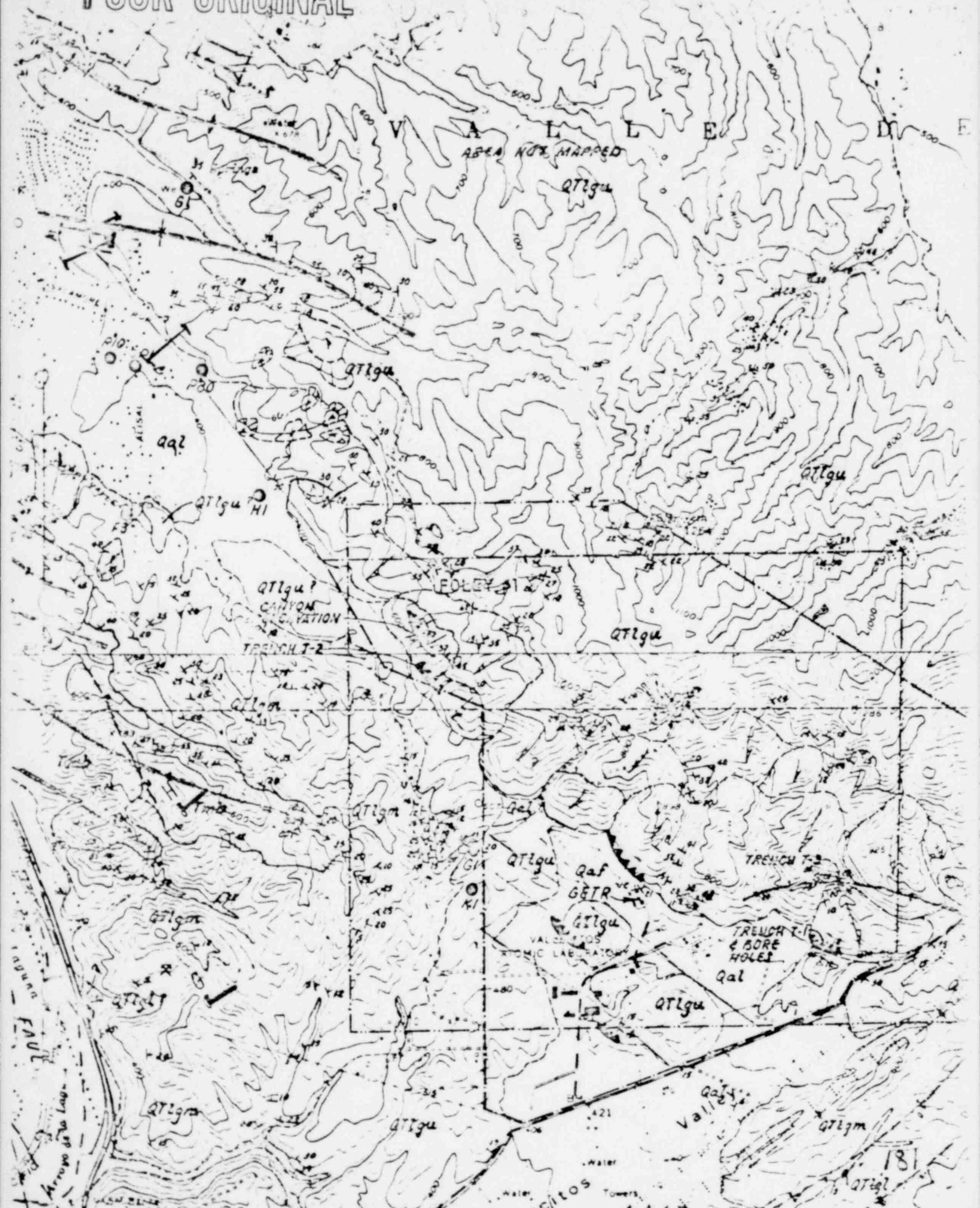
(1) WHAT THE PROPER SEISMIC AND GEOLOGIC DESIGN BASES FOR THE GETR FACILITY SHOULD BE;

(2) WHETHER THE DESIGN OF GETR STRUCTURES, SYSTEMS, AND COMPONENTS IMPORTANT TO SAFETY CAN BE MODIFIED SO AS TO REMAIN FUNCTIONAL CONSIDERING THE SEISMIC DESIGN BASES DETERMINED IN ISSUE (1) ABOVE;

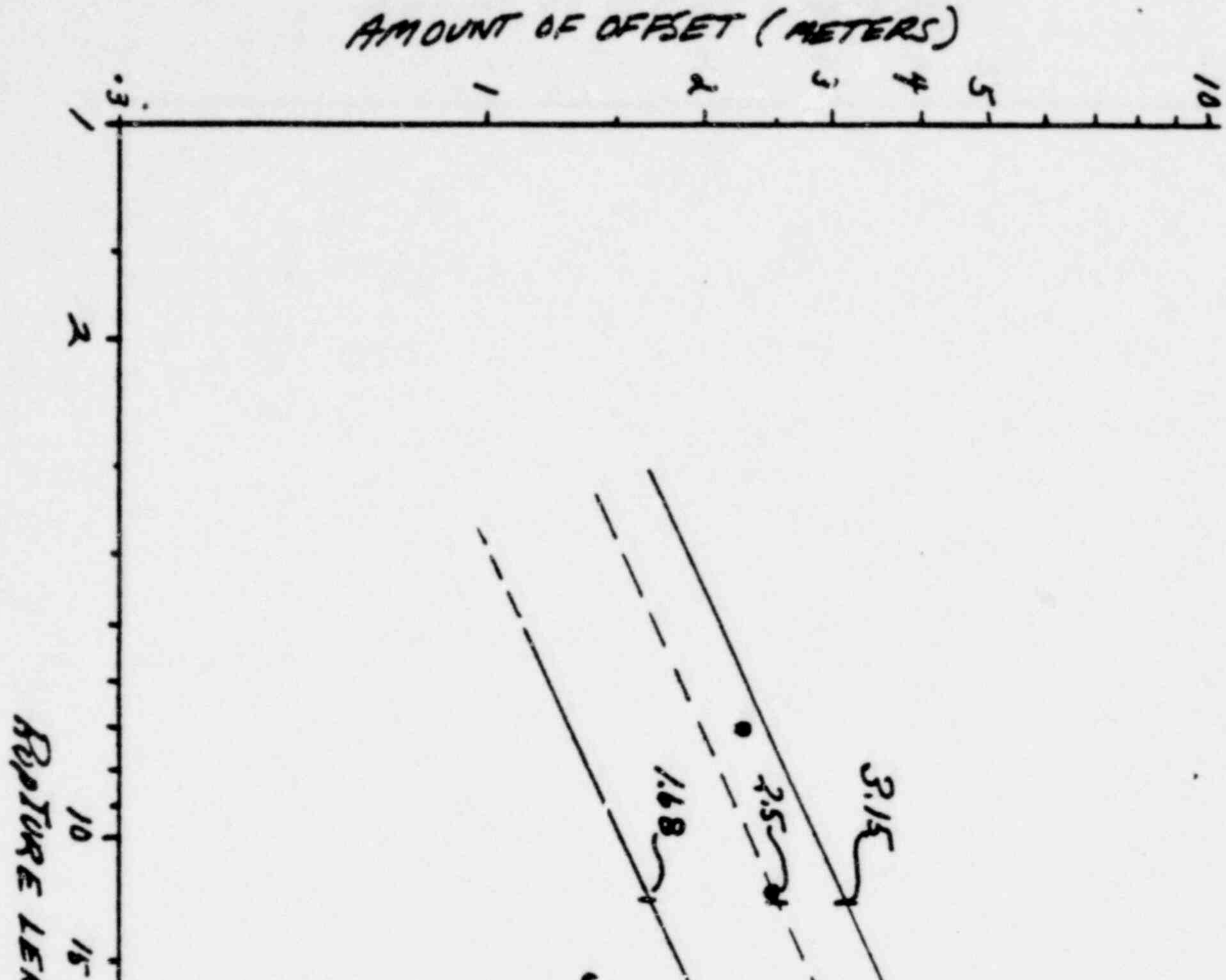
(3) WHETHER ACTIVITIES UNDER OPERATING LICENSE NO. TR-1 SHOULD BE SUSPENDED PENDING EVALUATION OF THE FOREGOING.

1463 151

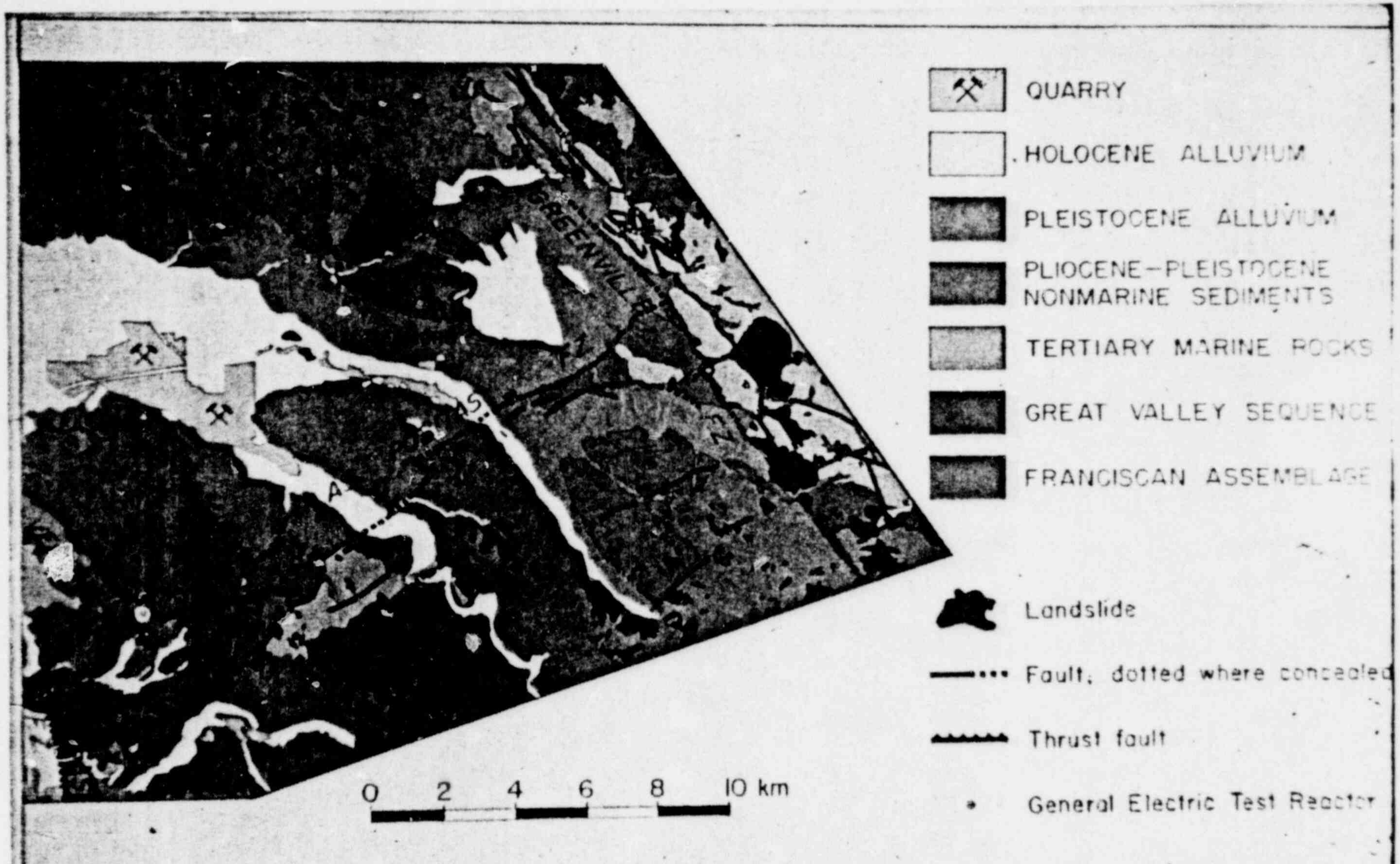
180


V A L L E D E
AREA NOT MAPPED
QTlgu
Qal
QTlgu?
CANYON EXCAVATION
TRENCH T-2
FOLEY
QTlgm
Qal
QTlgu
Qaf
GETR
TRENCH T-3
VALLECITOS ATOMIC LABORATORY
TRENCH T-1 & BORE HOLES
Qal
Water
Towers
181

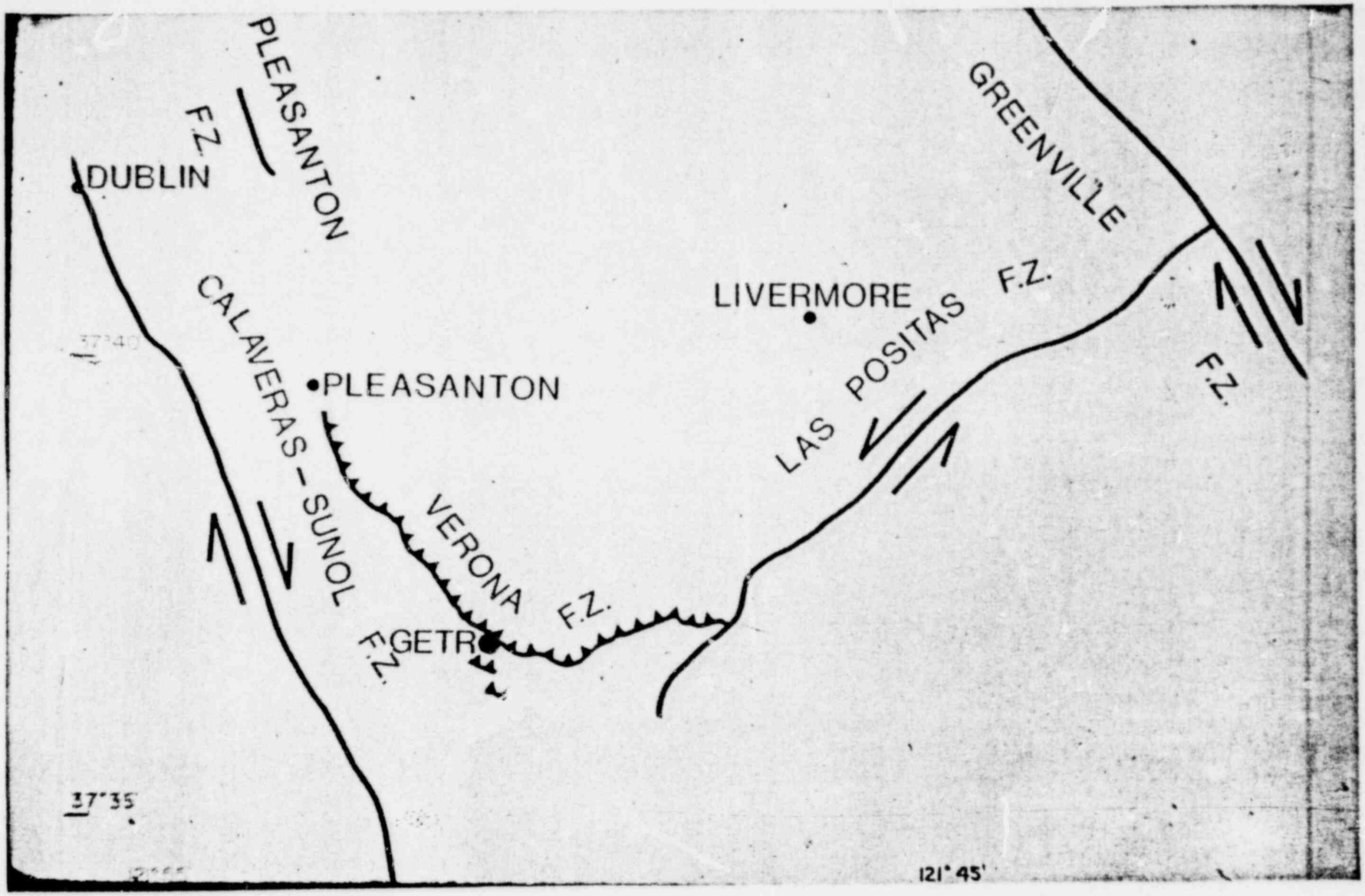
1463 152


AMOUNT OF OFFSET (METERS)
.3
1
2
3
4
5
10
RUPTURE LENGTH (KILOMETERS)
1
2
10
15
20
25
30
50
70
100
3.15
2.5
1.68
22% exceedance Prob.
30% exceedance Prob
50% exceedance Prob
●-REVERSE SLIP
▲-REVERSE-OBLIQUE SLIP
(after SLEMMONS, 1977)

1463 153

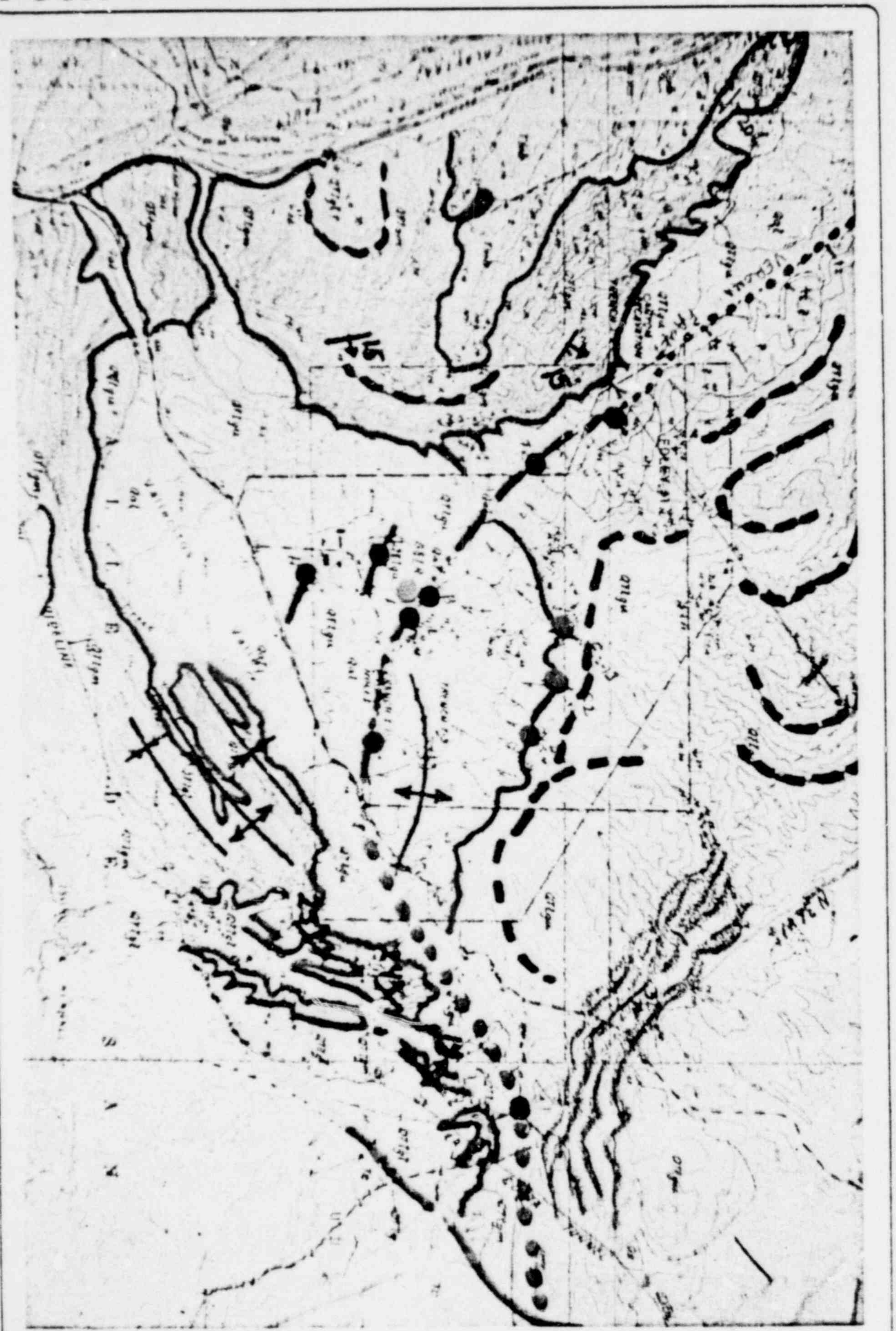


GEOLOGIC MAP OF LIVERMORE VALLEY, CALIFORNIA

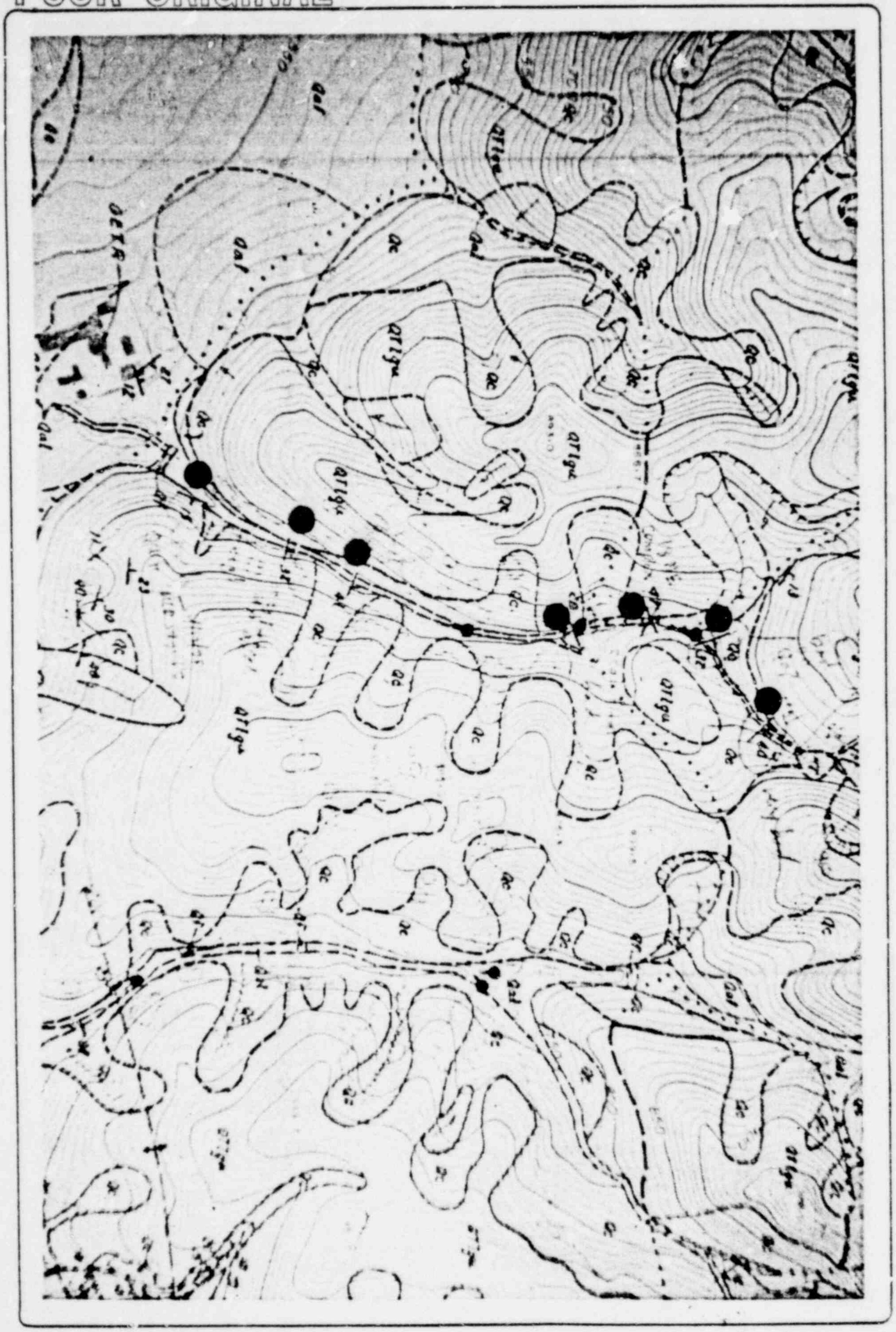

PLEASANTON
F.Z.
DUBLIN
GREENVILLE
F.Z.
LIVERMORE
LAS POSITAS F.Z.
37°40
PLEASANTON
CALAVERAS - SUNOL
F.Z.
VERONA
F.Z.
GETR
37°35
121°45'

1463 155

POOR ORIGINAL

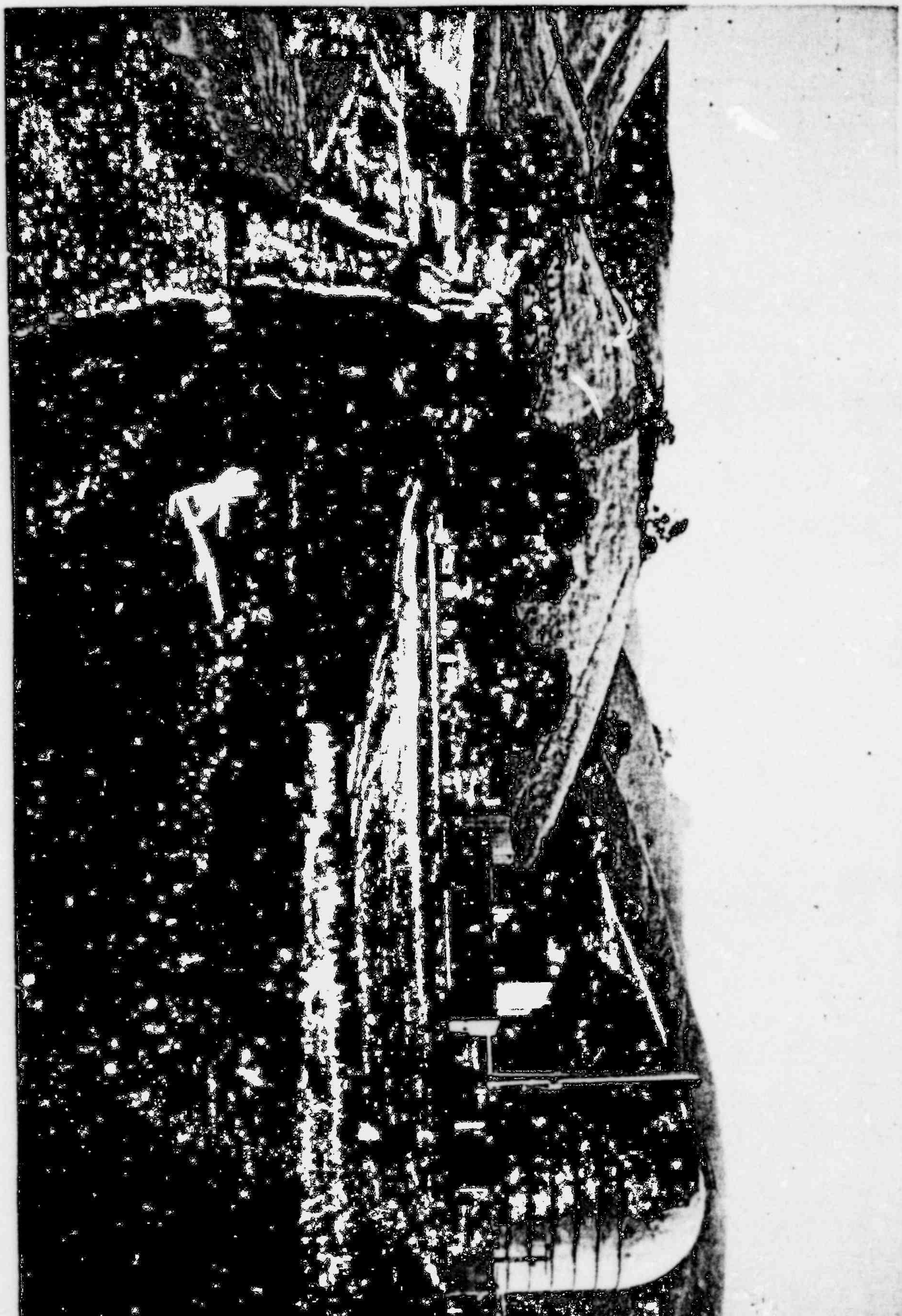


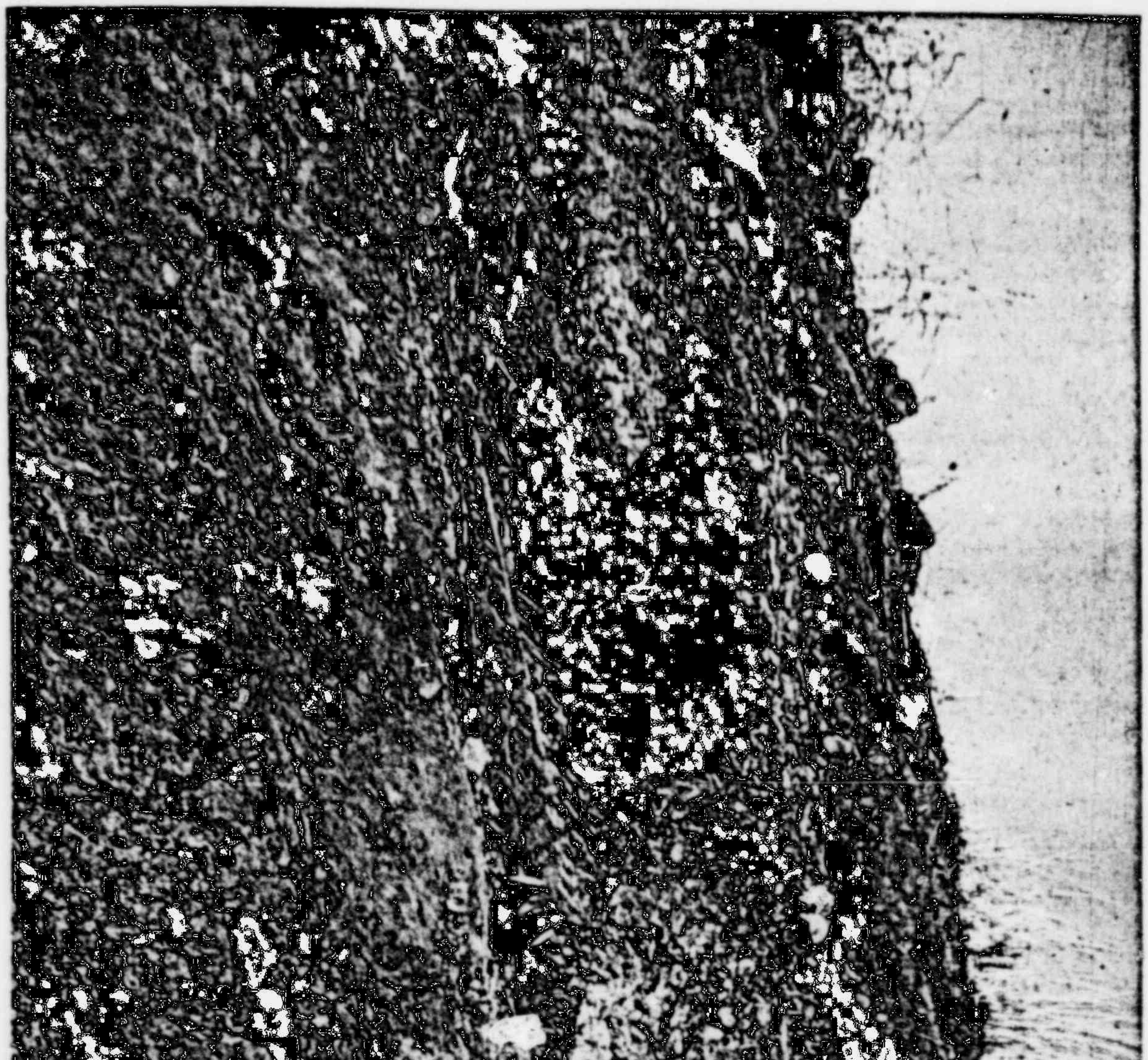
POOR ORIGINAL



1463 157

186





188

1463 159

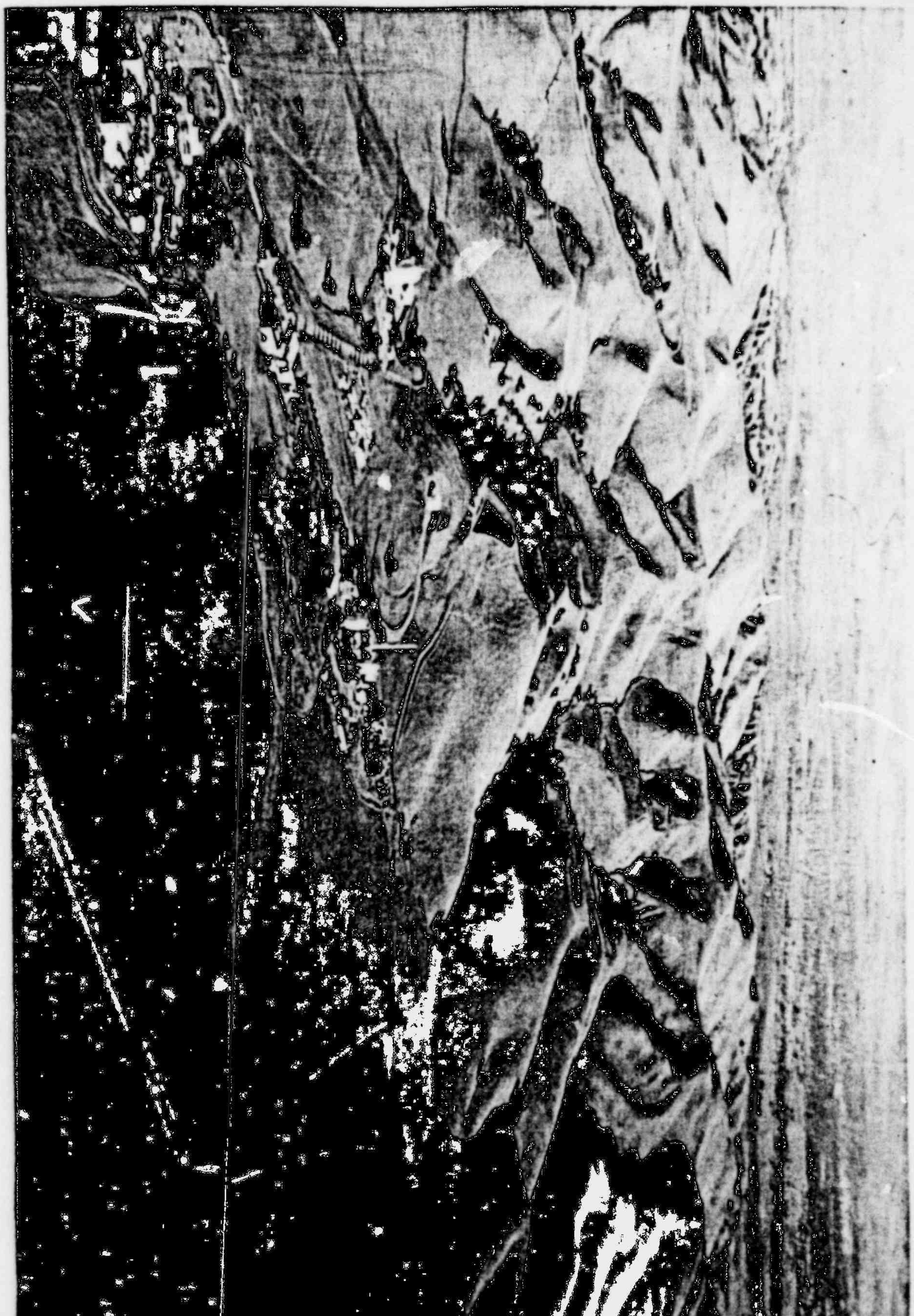
1463 160

187

190

191

1463 162



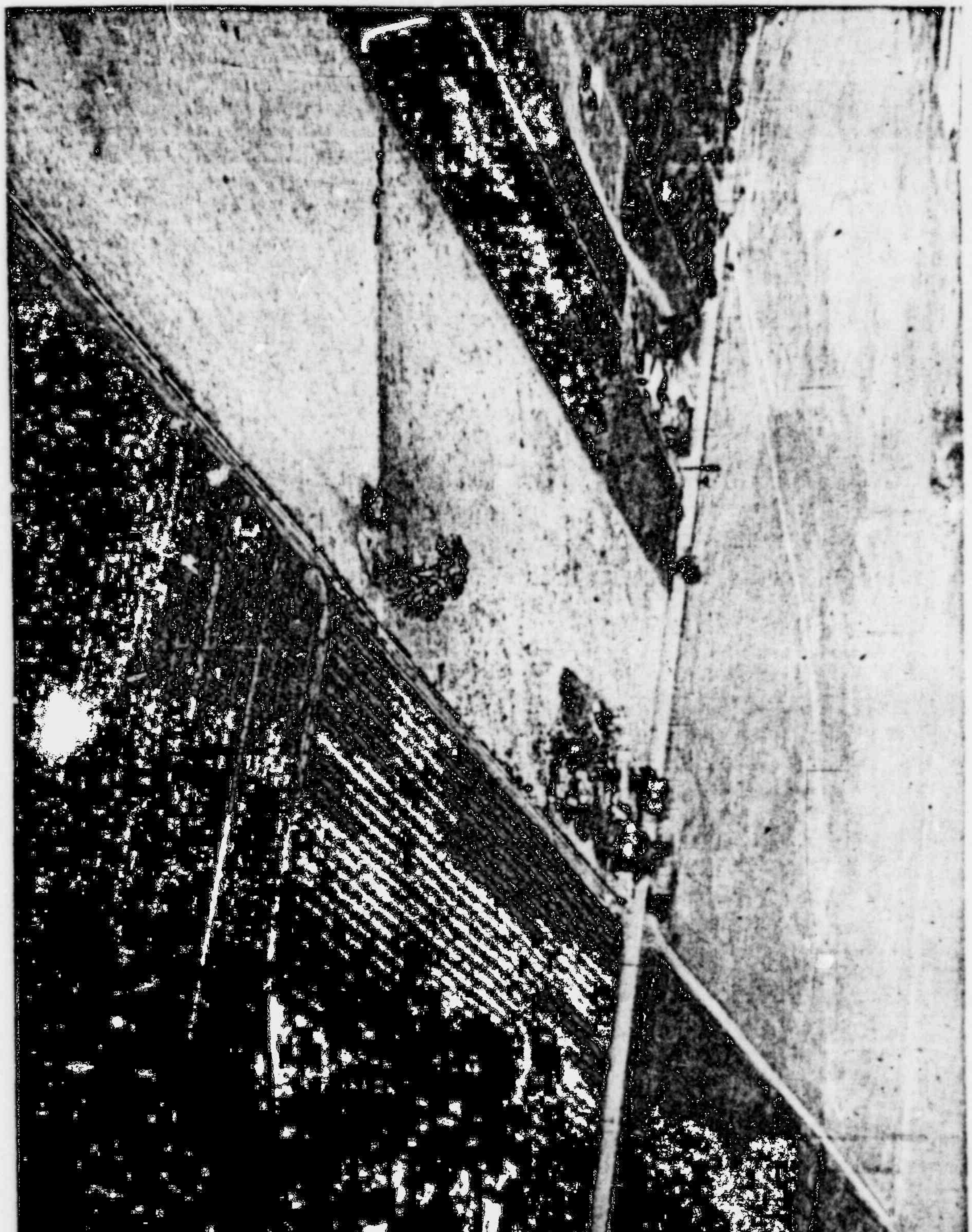
192

1463 163

193

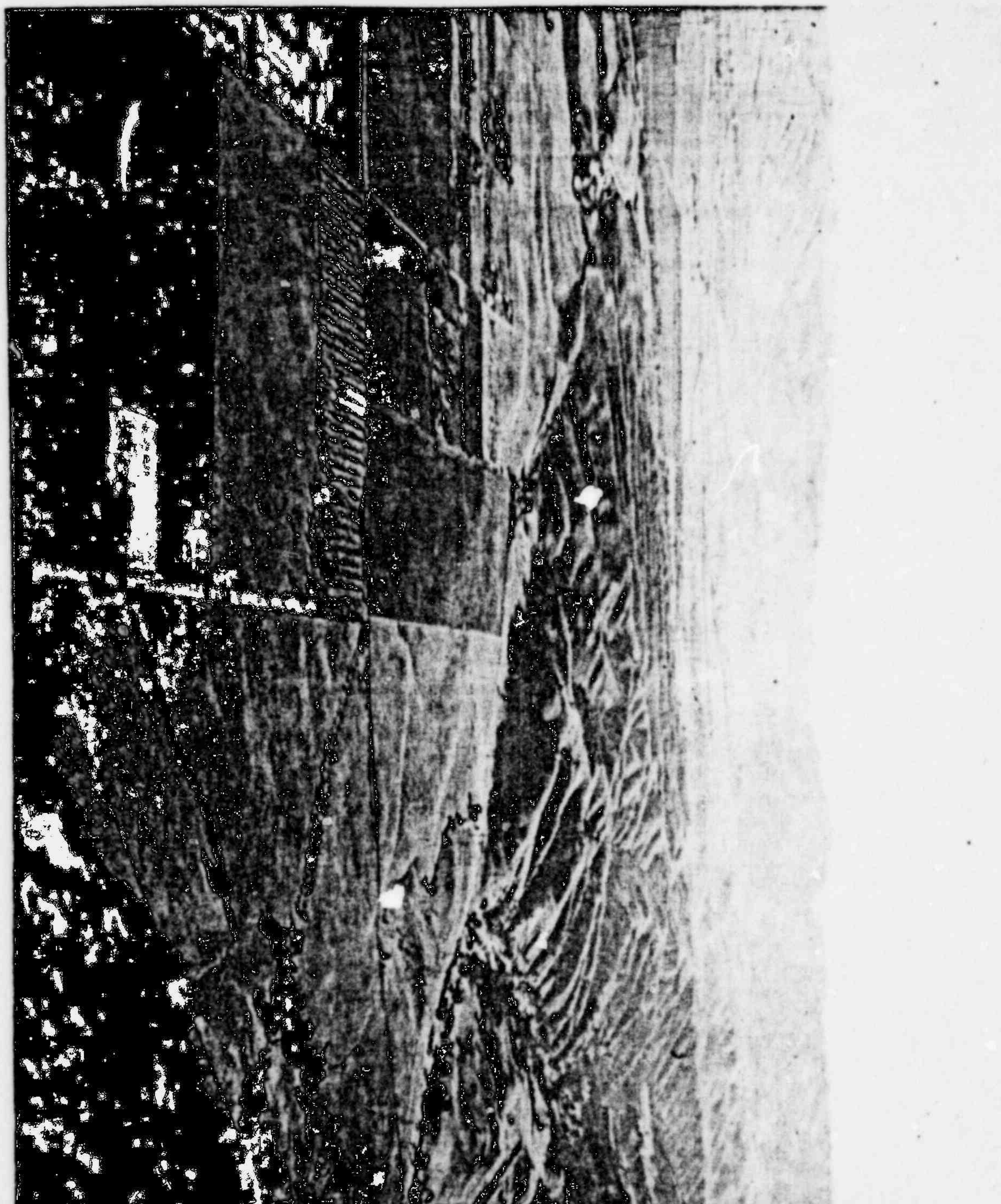
194

1463 165



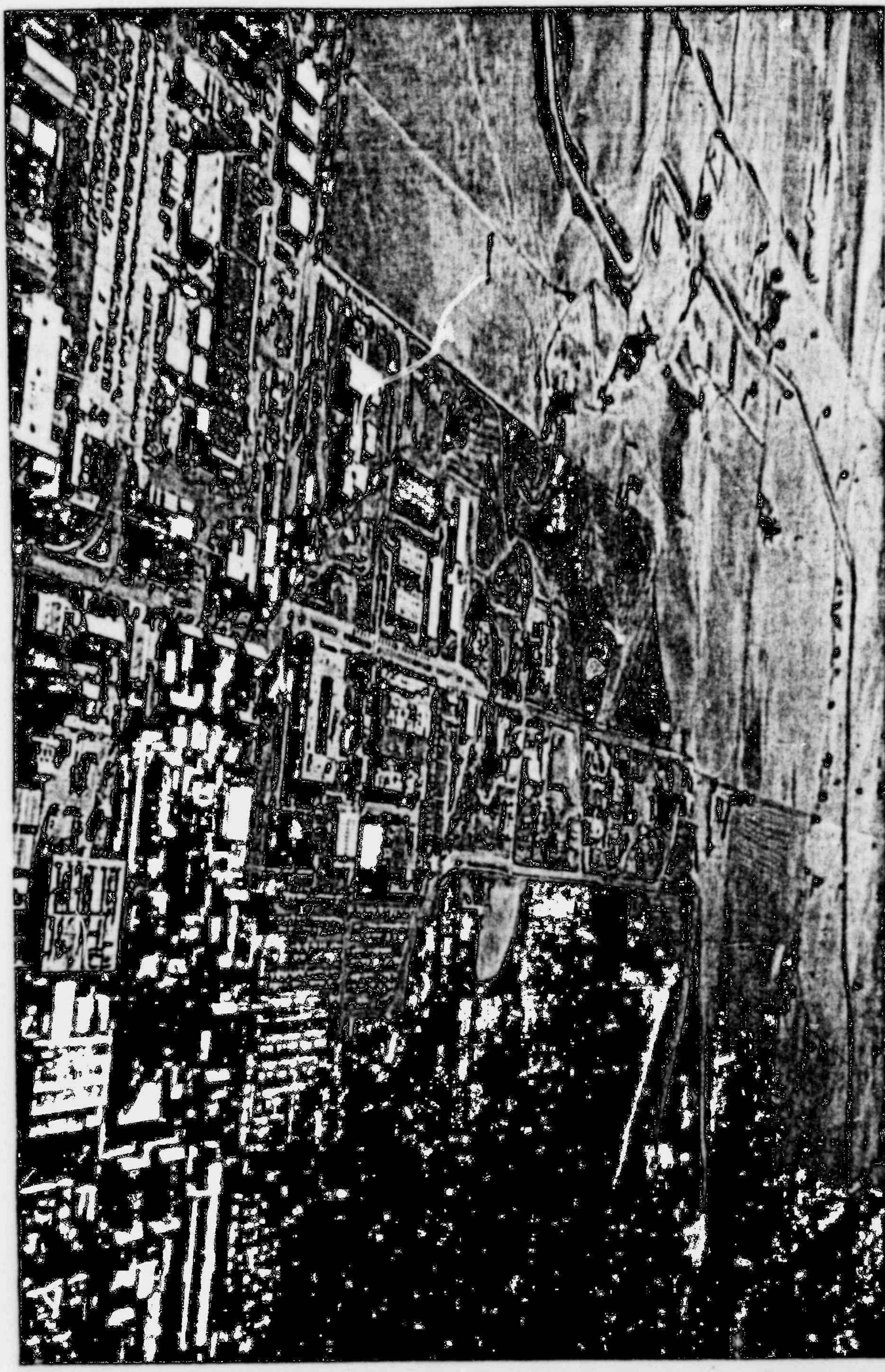
195

1463 166



1463 167

196



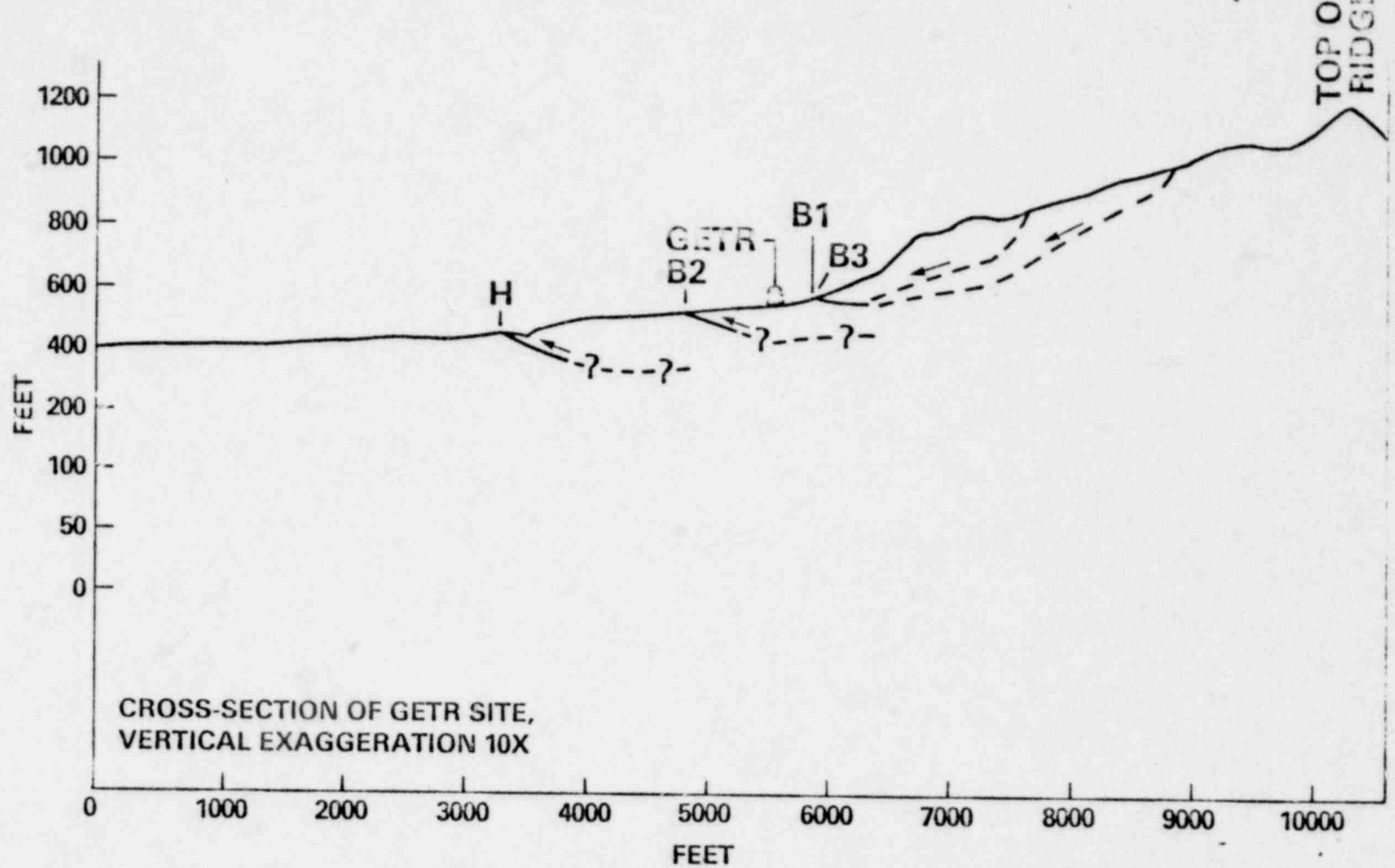
197

POOR ORIGINAL

192

1463 169

200



CROSS-SECTION OF GETR SITE,
VERTICAL EXAGGERATION 10X

1463 172

TABLE 1
LANDSLIDE
CAUSE FOR CONCERN

1. GETR LOCATED WITHIN A SHEAR ZONE
2. GETR LOCATED AT THE TOE OF A HILLSIDE INTERPRETED BY SOME OBSERVERS TO BE A LANDSLIDE COMPLEX
3. YOUNGEST OFFSET INTERPRETED TO BE DURING HOLOCENE
4. DISPLACEMENTS WERE REPEATED OVER A VERY LONG PERIOD OF TIME
5. POTENTIAL FOR STRONG SEISMIC FORCES ON HILLSIDE SLOPES

TABLE 2
LANDSLIDE ANALYSES

INFORMATION REQUIRED	GE ASSUMED
1. DETERMINATION OF LOCATION ORIENTATION AND SHAPE OF FAILURE PLANE	ARCS OF CIRCLES
2. DETERMINATION OF SHEAR STRENGTH PARAMETERS PARALLEL TO FAILURE SURFACE	Ø=16.5°,C = 1000 PSF
DISTRIBUTION OF PIEZOMETRIC LEVELS BENEATH SLIDE AND GENERAL GROUNDWATER LEVEL	NO SIGNIFICANT PRESSURES EXIST

TABLE 3
LANDSLIDE
UNRESOLVED ISSUES

SUBJECT	GE POSITION	STAFF POSITION
1. AGE OF YOUNGEST OFFSET	PRE HOLOCENE	DURING HOLOCENE
2. G.E. LANDSLIDE STABILITY REPORT, JULY 1978	DOCUMENTED LANDSLIDE INFORMATION IS SUFFICIENT	INADEQUATE - ALL IMPORTANT PARAMETERS ARE ASSUMED
3. ADDITIONAL INVESTIGATIONS AND ENGINEERING ANALYSES FOR LANDSLIDE CONCERN	UNNECESSARY FOR DEVELOPMENT OF SEISMIC DESIGN BASES FOR GETR	DETAILED INVESTIGATIONS AND COMPLETE ANALYSES ARE REQUIRED

1463 173

202