

OCT 15 1987

Docket Nos.: 50-275
and 50-323

Mr. J. D. Shiffer, Vice President
Nuclear Power Generation
c/o Nuclear Power Generation, Licensing
Pacific Gas and Electric Company
77 Beale Street, Room 1451
San Francisco, California 94106

DISTRIBUTION

Docket File	DJeng
NRC & LPDRs	HAshtar
CTrammell	RPichumani
GHolahan	NChokski
JLee	RMcMullen
OGC-Bethesda	LReiter
PDV Plant File	RRothman
EJordan	GBagchi
JPartlow	
ACRS (10)	

Dear Mr. Shiffer:

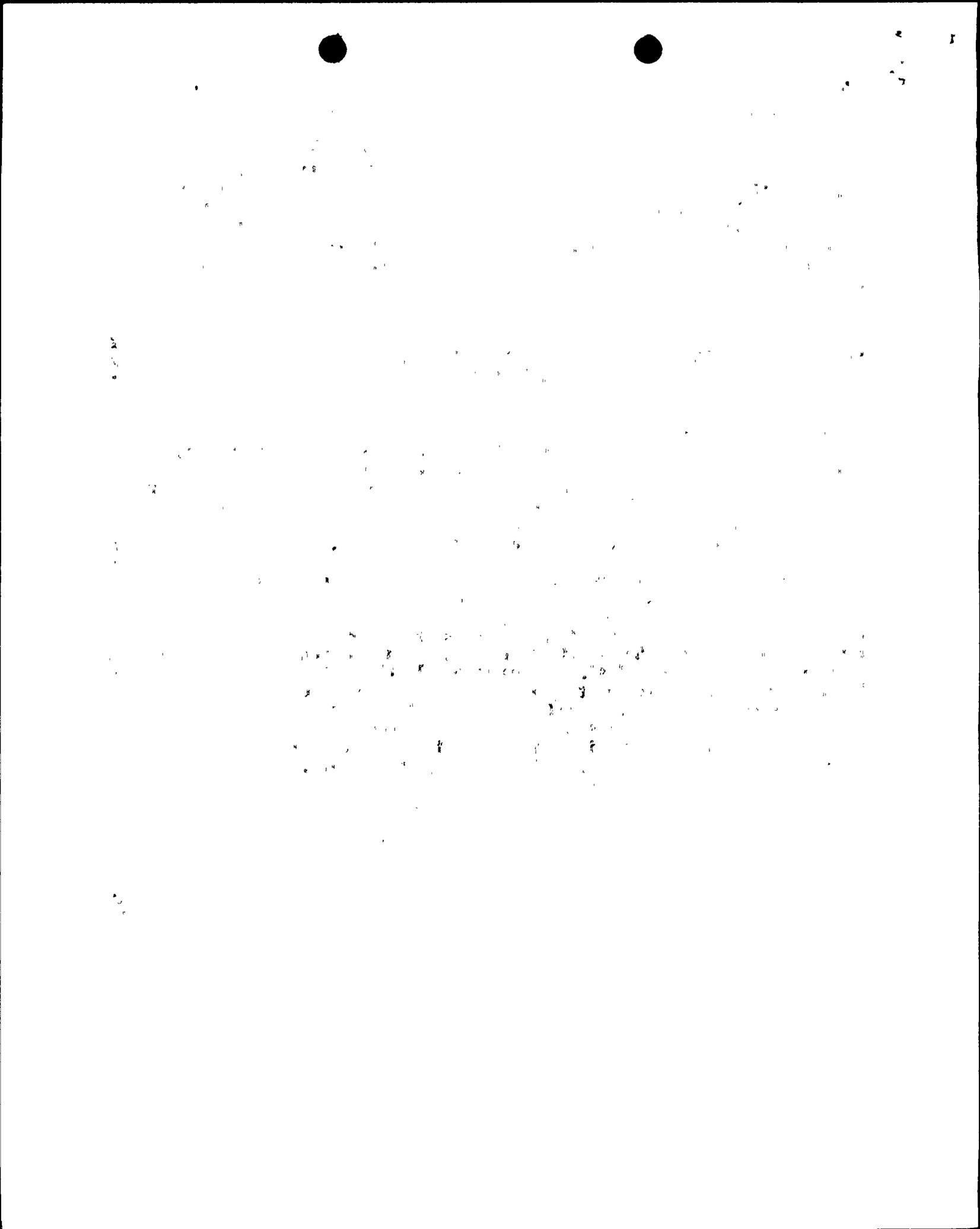
SUBJECT: COMMENTS RESULTING FROM JULY 15-16, 1987 GROUND
MOTION WORKSHOP (TAC NO. 55305)

Enclosed for your information is a revised summary of NRC staff and consultant comments made at the July 15-16, 1987 ground motion workshop. While there is some overlap between these comments and those presented in Enclosure 7 to our summary of this meeting dated July 30, 1987, we consider the enclosed summary to be a more complete version of the earlier summary. Please note also that it contains two new items (p.2, first two items) regarding hanging walls and fault segmentation analysis.

Also enclosed for your information are the comments of our consultants who attended this workshop.

As we have discussed, the staff and its consultants are still not clear about how PG&E plans to use the results of the ground motion studies in the soil structure interaction analysis and the probabilistic risk assessment. Because of the importance of this issue we believe that a detailed submittal and a subsequent meeting are needed to discuss the integration of the ground motion estimates into the rest of the analysis. Accordingly, we ask that you respond to this request within thirty days of receipt of this letter with a proposed schedule for such a submittal and related meeting.

8710230104 871015
PDR ADDCK 05000275
P PDR



The reporting and/or recordkeeping requirements contained in this letter affect fewer than ten respondents; therefore, OMB clearance is not required under Pub. L. 96-511.

Please contact us if you should have any questions regarding these matters.

Sincerely,

Original signed by

Charles M. Trammell, Project Manager
Project Directorate V
Division of Reactor Projects - III,
IV, V and Special Projects

Enclosure:

1. Summary of Staff and Consultant Comments
2. Letter, Aki to Savy dated July 20, 1987
3. Letter, Day to Savy dated August 20, 1987
4. Letter, Archuleta to Savy dated July 27, 1987
5. Letter, Campbell to Rothman dated August 5, 1987
6. Letter, Costantino to Reich dated October 4, 1987

cc w/enclosure:
See next page


DRSP/PDV
CTrammell:cd
10/15/87


SGEB
GBagchi
10/15/87


DRSP/PDV
GWKnighton
10/15/87

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

37

38

39

40

41

42

43

44

45

46

47

48

49

50

51

52

53

54

55

56

57

58

59

60

61

62

63

64

65

66

67

68

69

70

71

72

73

74

75

76

77

78

79

80

81

82

83

84

85

86

87

88

89

90

91

92

93

94

95

96

97

98

99

100

Mr. J. D. Shiffer
Pacific Gas and Electric Company

Diablo Canyon

CC:
Richard F. Locke, Esq.
Pacific Gas & Electric Company
Post Office Box 7442
San Francisco, California 94120

NRC Resident Inspector
Diablo Canyon Nuclear Power Plant
c/o U.S. Nuclear Regulatory Commission
P. O. Box 369
Avila Beach, California 93424

Janice E. Kerr, Esq.
California Public Utilities Commission
350 McAllister Street
San Francisco, California 94102

Mr. Dick Blakenburg
Editor & Co-Publisher
South County Publishing Company
P. O. Box 460
Arroyo Grande, California 93420

Ms. Sandra A. Silver
660 Granite Creek Road
Santa Cruz, California 95065

Bruce Norton, Esq.
c/o Richard F. Locke, Esq.
Pacific Gas and Electric Company
Post Office Box 7442
San Francisco, California 94120

Mr. W. C. Gangloff
Westinghouse Electric Corporation
P. O. Box 355
Pittsburgh, Pennsylvania 15230

Dr. R. B. Ferguson
Sierra Club - Santa Lucia Chapter
Rocky Canyon Star Route
Creston, California 93432

Managing Editor
San Luis Obispo County Telegram
Tribune
1321 Johnson Avenue
P. O. Box 112
San Luis Obispo, California 93406

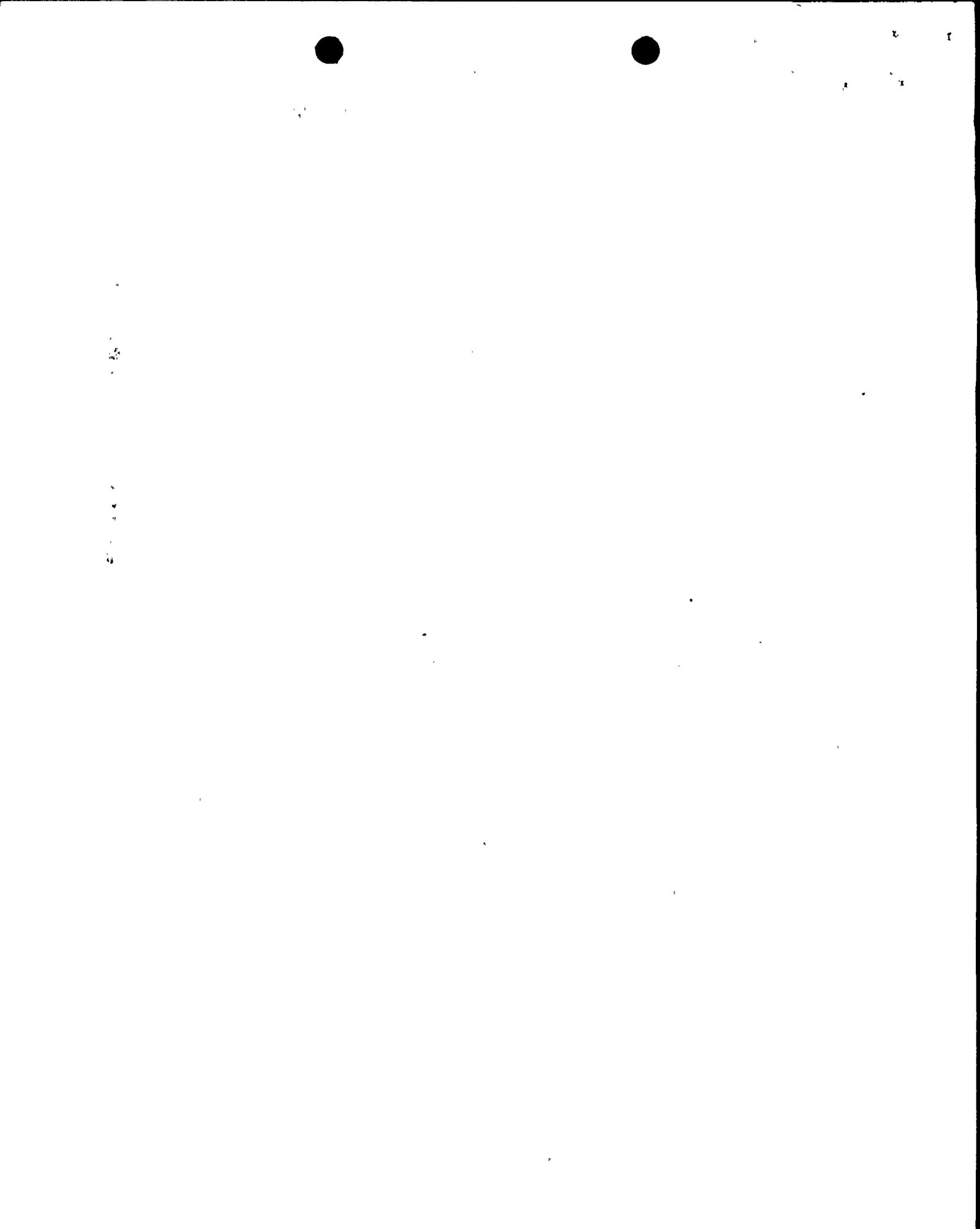
Chairman
San Luis Obispo County Board of
Supervisors
Room 220
County Courthouse Annex
San Luis Obispo, California 93401

Mr. Leland M. Gustafson, Manager
Federal Relations
Pacific Gas and Electric Company
1726 M Street, N. W.
Washington, DC 20036-4502

Director
Energy Facilities Siting Division
Energy Resources Conservation and
Development Commission
1516 9th Street
Sacramento, California 95814

Dian M. Grueneich
Marcia Preston
Law Office of Dian M. Grueneich
380 Hayes Street, Suite 4
San Francisco, California 94102

Ms. Jacquelyn Wheeler
2455 Leona Street
San Luis Obispo, California 93400



Pacific Gas & Electric Company

- 2 -

Diablo Canyon

cc:

Ms. Laurie McDermott, Coordinator
Consumers Organized for Defense
of Environmental Safety
731 Pacific Street, Suite 42
San Luis Obispo, California 93401

Ms. Nancy Culver
192 Luneta Street
San Luis Obispo, California 93401

Mr. Joseph O. Ward, Chief
Radiological Health Branch
State Department of Health
Services
714 P Street, Office Building #8
Sacramento, California 95814

President
California Public Utilities
Commission
California State Building
350 McAllister Street
San Francisco, California 94102

Regional Administrator, Region V
U.S. Nuclear Regulatory Commission
1450 Maria Lane
Suite 210
Walnut Creek, California 94596

Michael M. Strumwasser, Esq.
Special Assistant Attorney General
State of California
Department of Justice
3580 Wilshire Boulevard, Room 800
Los Angeles, California 90010



Y Y

J J

1
2
3

4
5
6

7
8

9

10

11

Pacific Gas and Electric Company
Long Term Seismic Program

- 3 -

Diablo Canyon

cc:

Dr. S. T. Algermissen
U.S. Geological Survey
P. O. Box 25046
Denver Federal Center-M.S. 966
Denver, Colorado 80225

Dr. C. J. Costantino
Building 129
Brookhaven National Laboratory
Upton, New York 11973

Dr. Keiiti Aki
Department of Geological Sciences
University of Southern California
Los Angeles, California 90089

State Geologist
California Division of Mines
and Geology
Room 1351
1416 Ninth Street
Sacramento, California 95814

Dr. Ralph J. Archuleta
Department of Geological Sciences
University of California Santa Barbara
Santa Barbara, California 93106

Dr. Steven M. Day
S-Cubed
P. O. Box 1620
La Jolla, California 92038

Mr. Don Bernreuter
Lawrence Livermore Laboratory
P. O. Box 808
Livermore, California 94550

Dr. George Gazetas
JEC 4049
Rensselaer Polytechnic Institute
Troy, New York 12180-3590

Mr. Donald A. Brand
Vice President, Power Generation
Pacific Gas and Electric Company
77 Beale Street, Room 2917
San Francisco, California 94106

Dr. James Davis
California Division of
Mines and Geology
1516 Ninth St. - Fourth Floor
Sacramento, CA 95814

Dr. Robert D. Brown, Jr.
U.S. Geological Survey
Mail Stop 977
345 Middlefield Road
Menlo Park, California 94025

Dr. Morris Reich
Structural Analysis Division
Building 129
Brookhaven National Laboratory
Upton, New York 11973

Mr. Lloyd S. Cluff
Pacific Gas and Electric Company
77 Beale Street, Room 2661
San Francisco, California 94106

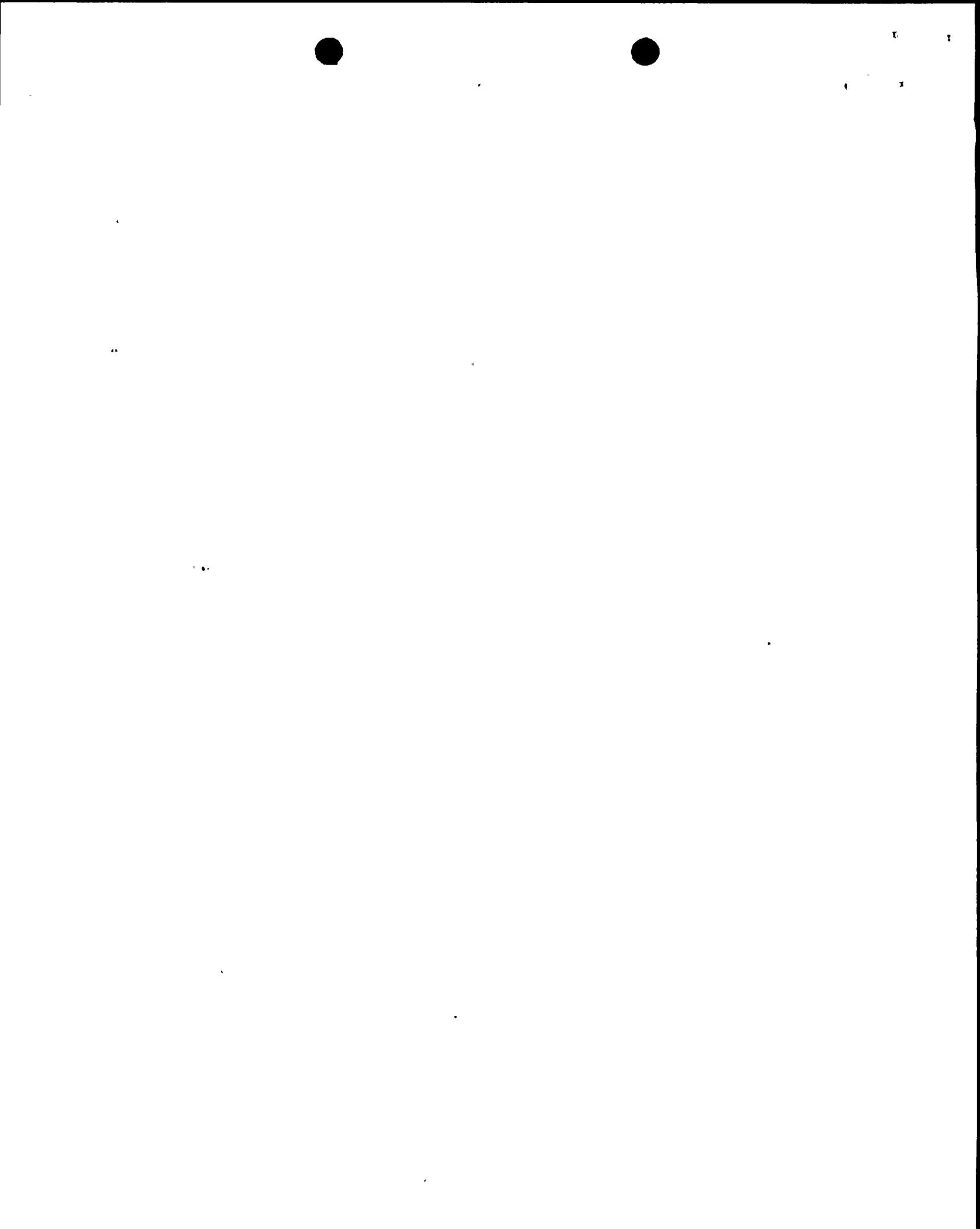
Dr. Jean Savy
Mail Stop L-196
Lawrence Livermore Laboratory
P. O. Box 808
Livermore, California 94550

Dr. David B. Slemmons
2995 Golden Valley Road
Reno, Nevada 89506

Dr. Anestis S. Veletsos
5211 Paisley Avenue
Houston, Texas 77096

Dr. Robert Fitzpatrick
Building 130
Brookhaven National Laboratory
Upton, New York 11973

Dr. Ken Campbell
U. S. Geological Survey
P. O. Box 25046, Mail Stop 966
Denver Federal Center
Denver, Colorado 80225



Pacific Gas and Electric Company
Long Term Seismic Program

- 3a -

Diablo Canyon

cc:

Dr. Michael Bohn
Sandia Lab. - Organization 6412
Post Office Box 5800
Albuquerque, New Mexico 87185

Mr. William H. Wallace
Vice President, Engineering
Pacific Gas and Electric Company
77 Beale Street, Room 2645
San Francisco, California 94106

Eddie Clark
613 Stanford Drive
San Luis Obispo, California 93401



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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

1

Enclosure

Summary of Staff and Consultant Comments made at the July 15-16, 1987
Ground Motion Workshop

The amount of work performed and the attention paid to the relevant issues in the numerical modeling studies is impressive.

In the numerical modeling studies, there is a need to make estimates of the uncertainty and to track the uncertainties in obtaining the hazard curves to be used as input for the Probabilistic Risk Assessment.

The numerical modeling methods should be applied to earthquakes other than those from which it was developed to see how well the ground motion can be predicted.

Coherency is very strongly non-stationary and assuming stationarity may not be correct. The effects of a propagating rupture should be considered in the coherency estimates. It is not clear how the spatial incoherency is to be used to derive a realistic input motion for use in the soil structure interaction analysis.

Consideration should be given to producing other ground motion estimates along with spectral acceleration. Also, the ground motion estimates seem to be concentrated on peak ground acceleration and spectral acceleration in the 3 to 8.5 Hertz, a broader range of frequencies seems appropriate.

Input parameters important to the ground motion estimates should be identified and sensitivity studies should be performed on them.

Site effects on recordings made in the plant area may be reflected as amplification in the 2 to 6 Hertz range and the recordings may also reflect the effect of the presence of the structures.

The change in the element size in the modeling study from 3 X 4 km to 1.5 X 1.5 km may overly reduce the level of the spectrum at frequencies of engineering importance.

The assumption in the modeling study that the slip distribution is the same as the 1979 Imperial Valley earthquake where the seismogenic zone is probably below 5 km is not appropriate for the Diablo Canyon site where there may be significant slip up to 2 km from the surface.

In using the generalized ray theory method the surface wave contribution is not seen and the duration of the records may be too short.

Attenuation should be examined and the effects of the differences in attenuation between Imperial Valley and Diablo Canyon should be addressed.

With respect to the empirical ground motion studies, the median and 84 percentile spectra over the entire frequency range are needed.



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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

The Diablo Canyon site could be on the hanging wall of a postulated Hosgri reverse or thrust fault. Both the empirical and numerical studies should take this into account since ground motion recordings on hanging walls appear to be systematically higher than those on the foot walls.

In the fault segmentation analysis, the probabilistic formulation appears questionable. The problem that is to be solved is; given the existence of an observed change in fault characteristics, what is the likelihood of it being the end of an earthquake rupture?

It is not clear how the results of the theoretical and numerical ground motion studies are to be used in the Soil Structure Interaction analysis and the Probabilistic Risk Assessment. The flow chart presented at the meeting did not have enough detail to allow evaluation.



2

Vertical text or markings on the left side of the page.

DEPARTMENT OF GEOLOGICAL SCIENCES
TELEPHONE (213) 743-2717



20 July 1987

Dr. Jean Savy
Lawrence Livermore Nat'l Laboratory
P.O. Box 808
Livermore, CA 94550

Dear Jean:

This is my letter report on the Diablo Canyon LTSP ground motion workshop held on 15-16 July 1987 in San Francisco.

Before commenting on the main subject of the workshop, namely, ground motion, I would like to mention that I was very much encouraged by Kevin Coppersmith's presentation on progress in seismic source characterization from the geology/seismology/geophysics data. I felt that he was pursuing a new promising direction by relating various features of a fault zone with potential earthquake source parameters. Combined with the numerical modeling approach for ground motion estimation, it may be revolutionizing the seismic hazard estimation, and the present Diablo Canyon LTSP may serve as a model in the future.

There are, however, several pit falls covered up in brilliant presentations given in the two-day workshop.

(1) Leon Reiter objected to Kevin's statement about the probability of a geologic fault-step becoming an end point of an earthquake fault, and stated that the "normalized" probability does not apply to the probability for a given fault-step. I agree with Leon, and let me explain why.

Let "A" be the existence of a geologic fault step, and "B" being an end point of an earthquake fault. Kevin's observation indicated that the occurrence of "A and B" and that of "A and non-B" are about the same. Writing these joint probabilities as $P(A,B)$ and $P(A,\bar{B})$, respectively, we have

$$P(A,B) \approx P(A,\bar{B}).$$

Defining the conditional probability $P(A/B)$ as the probability of "A" given the condition "B", we can write

$$P(A,B) = P(A)P(B/A) = P(B)P(A/B),$$

$$P(A,\bar{B}) = P(A)P(\bar{B}/A) = P(\bar{B})P(A/\bar{B}).$$

Thus, we have

$$P(B)P(A/B) \approx P(\bar{B})P(A/\bar{B})$$

or
$$P(A/B) \approx P(A/\bar{B}) \frac{P(\bar{B})}{P(B)}.$$



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

This is the "normalized" probability of Kevin, showing that, if $P(\tilde{B})$ is greater than $P(B)$, the conditional probability of finding fault-step at the end point is higher than that at the mid point.

On the other hand, if we want to find the conditional probability for a given fault step, we find

$$P(B/A) \approx P(\tilde{B}/A),$$

supporting Leon's statement in the meeting.

(2) With regard to the source aspect of the numerical modeling, I am most concerned with the deficiency in the intermediate frequency range by the Joyner-Boore scaling. Upon my question, Paul Sommerville agreed that the deficiency exists, and it will be greater if the sub-event size is reduced. This is an extremely serious problem, because if one makes the sub-event infinitesimally small, there will be no appreciable acceleration generated by the source. What concerns me most is that this most serious effect is completely disregarded in the ENCLOSURE B, where the problem with the Joyner-Boore scaling is addressed.

The preference of $1.5 \times 1.5 \text{ km}^2$ over $3 \times 4 \text{ km}^2$ sub-event was discussed in view of the Fraunhofer approximation by Paul, but his discussions are not at all founded on physical ground. He pretended that the Joyner-Boore model is equivalent to the specific barrier model, and tried to justify the sub-event size from the point of barrier-interval. As he agreed upon my question, the stress drop in each sub-event is reduced by the factor equal to the cube root of moment ratio. This implies the reduction of stress drop by several tens if one uses the $1.5 \times 1.5 \text{ km}^2$ sub-event, with a significant reduction of acceleration in the intermediate frequency range.

The need for the Fraunhofer approximation comes from the deficiency of their method of calculation used for Green's function. If the method is adapted to the case of a finite source, the need disappears. In any case, this need should not be considered as competing with the physical need for expressing the real earthquake process.

In fact, the Joyner-Boore model is only justified as a phenomenological model generating the ω^{-2} scaling approximately. There is no clear physical support to the model.

The preference of smaller sub-event by Paul on unfounded grounds makes me feel uncomfortable because it would underestimate the acceleration in a clever, clandestine manner.

(3) Paul Sommerville and Ben Tsai insisted that the decay of acceleration spectra with frequency is similar among Imperial Valley, Coalinga and Diablo Canyon. I noticed, however, an apparent amplification by a factor of 2 to 3 for the frequency range from 2 to 6 Hz observed for the Diablo Canyon relative to the interpolation of higher frequency trend. I was impressed because it agreed excellently with the result obtained by Scott Phillips (MIT, PhD thesis, 1965) for the USGS station at See Canyon located about 5 miles from the site on the same rock (Miocene marine).



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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

101
102
103
104
105
106
107
108
109
110
111
112
113
114
115
116
117
118
119
120
121
122
123
124
125
126
127
128
129
130
131
132
133
134
135
136
137
138
139
140
141
142
143
144
145
146
147
148
149
150
151
152
153
154
155
156
157
158
159
160
161
162
163
164
165
166
167
168
169
170
171
172
173
174
175
176
177
178
179
180
181
182
183
184
185
186
187
188
189
190
191
192
193
194
195
196
197
198
199
200

201
202
203
204
205
206
207
208
209
210
211
212
213
214
215
216
217
218
219
220
221
222
223
224
225
226
227
228
229
230
231
232
233
234
235
236
237
238
239
240
241
242
243
244
245
246
247
248
249
250
251
252
253
254
255
256
257
258
259
260
261
262
263
264
265
266
267
268
269
270
271
272
273
274
275
276
277
278
279
280
281
282
283
284
285
286
287
288
289
290
291
292
293
294
295
296
297
298
299
300

301
302
303
304
305
306
307
308
309
310
311
312
313
314
315
316
317
318
319
320
321
322
323
324
325
326
327
328
329
330
331
332
333
334
335
336
337
338
339
340
341
342
343
344
345
346
347
348
349
350
351
352
353
354
355
356
357
358
359
360
361
362
363
364
365
366
367
368
369
370
371
372
373
374
375
376
377
378
379
380
381
382
383
384
385
386
387
388
389
390
391
392
393
394
395
396
397
398
399
400

I am enclosing copies of relevant pages from the Phyllips thesis. Fig. 2.4a on p. 53 shows the USGS stations used in his thesis work. Fig. 2.4b on p. 54 shows the location of PSE (station near the Diablo Canyon). Table on p. 88 shows the rock type for the station PSE. The site amplification factor at PSE relative to the average of all stations studied is given below for various frequencies:

frequency in Hz	1.5	3	6	12
amplification factor	1.1	1.6	3.5	1.2

The above factor was obtained from the natural logarithm for power given in Table 3.2 of his thesis. Finally, the distribution of amplification factor for 6Hz for the central California is shown in Fig. 3.3c on p. 104. The Diablo Canyon area is of the highest amplification in the whole area for 6Hz.

I urge Paul and Ben to study the site amplification effect at the Diablo Canyon relative to some of the nearby USGS stations for which Phillips estimated the site effect.

(4) The final point I'd like to raise is the procedure used for estimating the coherency of seismic motion. It is well known (since my own work published in Bull. Earthq. Res. Inst. in the late 50's) that the spatial coherency of seismic motion due to local earthquakes is very high at the arrival of main P and S waves, but is decreased with time and becomes very low for the coda. The procedure used by Paul Sommerville is to calculate the coherence for a relatively long time window assuming that the statistical properties of waves are stationary in time. I imagine that the coherency must be higher if only early part is studied. It may be that the early part contribute more significantly to strong motion. It is, therefore, important to approach the problem without assuming the stationarity in time.

In summary, the above three issues concern me greatly, because, if unchecked, they tend to bias the ground motion estimate toward a lower value. All these matters are highly technical and therefore may be easily hidden from a layman. In order to secure the credibility of this innovative approach of numerical modeling, it is essential that all these issues be clearly exposed and fully addressed.

Sincerely yours,

Keiiti Aki

encl.

:;1

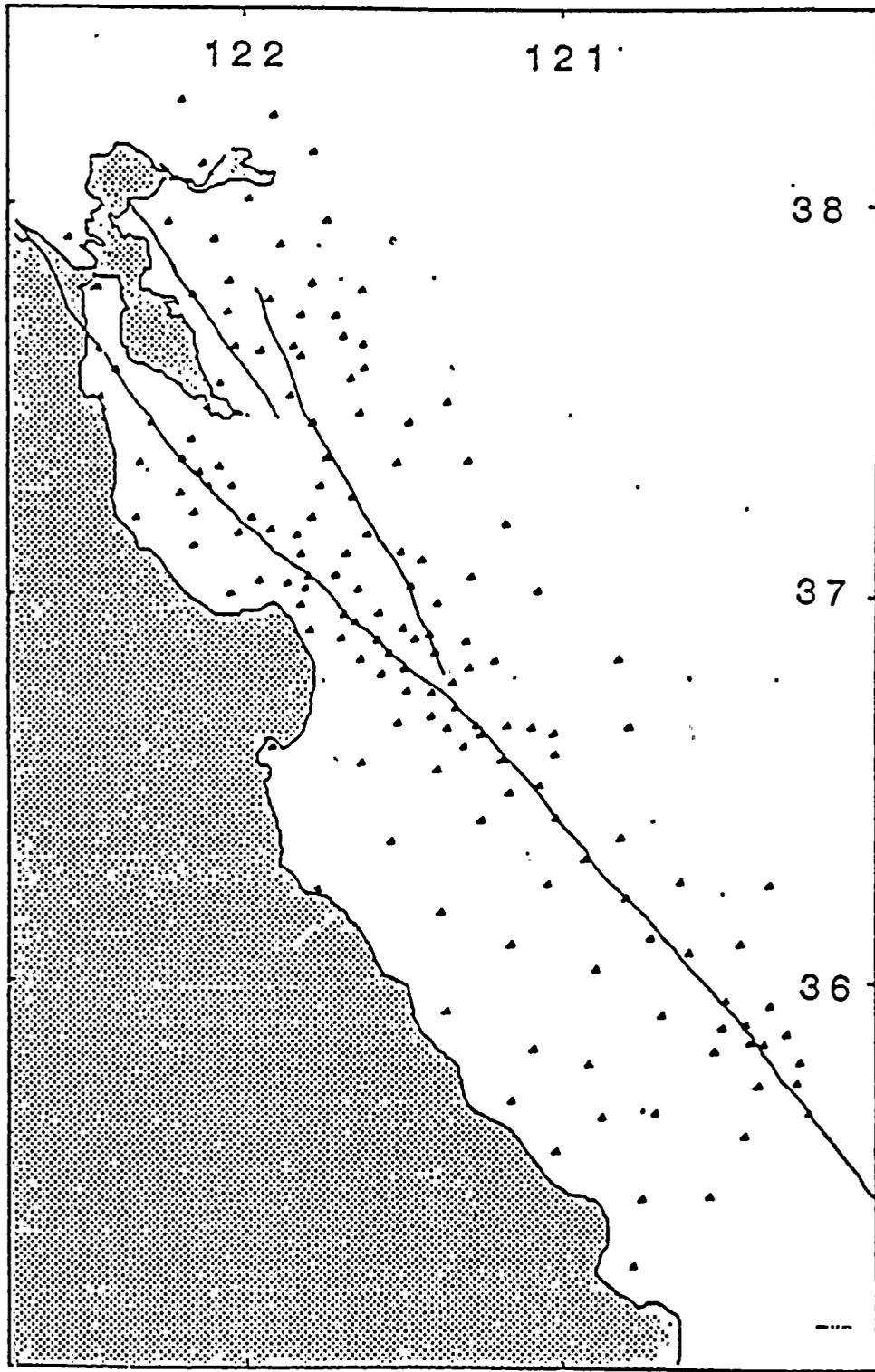
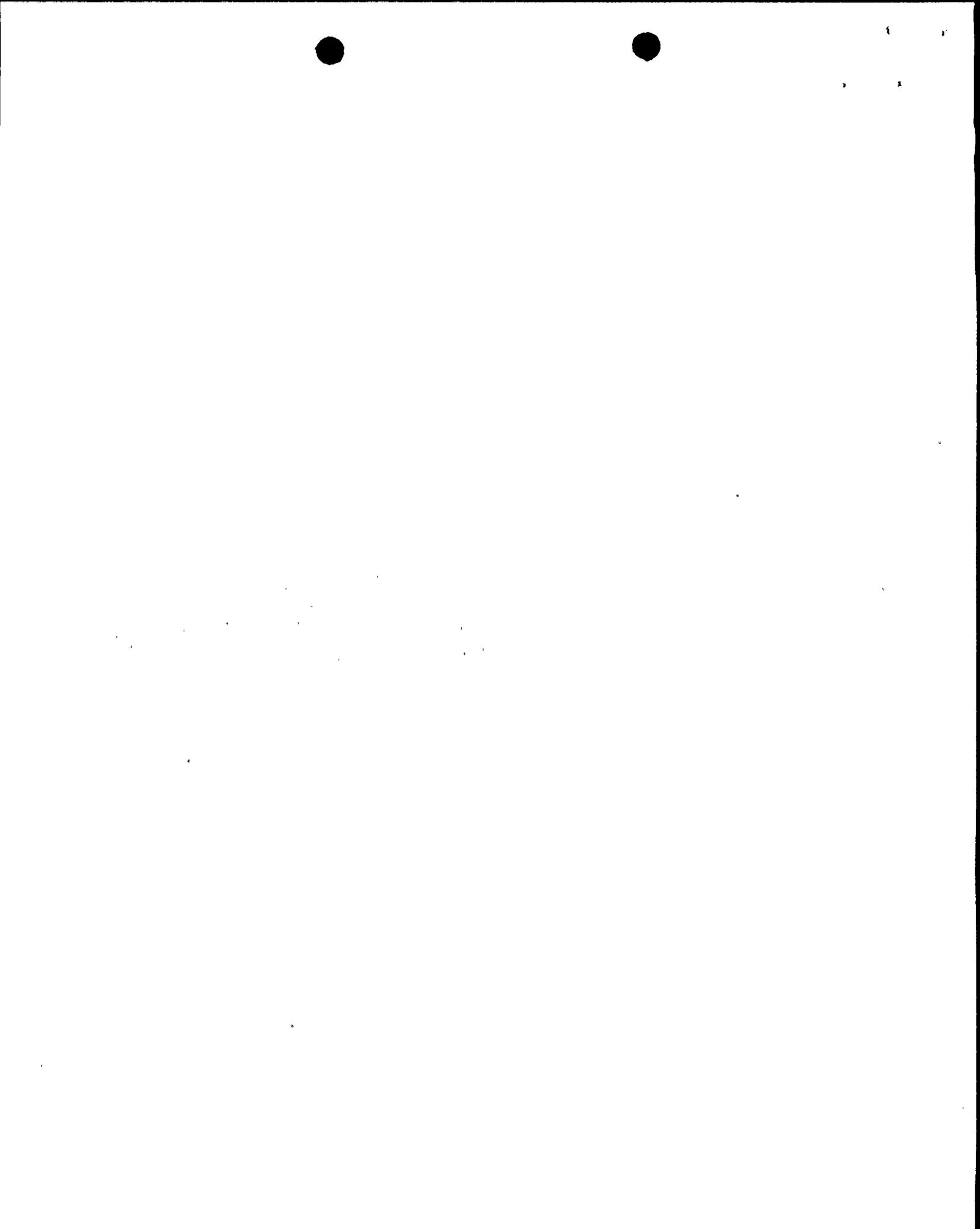


Figure 2.4a





JRRV	Redwood Retreat	KJf
JRWV	Ravenswood	KJf
JRXV		Mm
JSAV	San Andreas	KJfv
JSCV	Steven's Creek	Ml, KJf
JSFV	Stanford Telesc.	Mm
JSGV	Saratoga Golf	QP
JSJV	St. Joseph Sem.	QP
JSMV	Sawmill Rd.	φ
JSSV	Soda Springs Rd.	KJf
JSTV	Sta. Theresa Hill	ub, KJf
JTGV	Trout Gulch Rd.	Pml
JUCV	U.C. Sta. Cruz	ms
JWSV	Woodside	E
NGVV	Green Valley Rd.	
NMHV	Hamilton Ranch	
NLHV	Lake Herman	
NPRV	Point Reyes	
NTPV	Mt. Tamalpais	
PADV	Adelaida	Mm
PAGV	Antelope Grade	ub
PANV	San Antonio Res.	Mm
PAPV	Alder Peak	KJf
PARV	Alder Peak	KJf
PARV	Anticline Ridge	Pml
PB WV	Bitterwater Creek	Pml
PBYV	Bryson	Ku



PCGV	Cerro Alto Cmpgr.	Kl
PCRV	Curry Mtn.	Ku
PGHV	Gold Hill	bi
PHAV	Harlan Ranch	Pml
PHCV	Hearst Castle	KJf
PHGV	Hog Canyon	QP
PHRV	Hernandez Valley	KJf
PJLV	Jolon Rd.	Mm
PLOV	Lonoak Rd.	Pml
PMGV	Sta. Margarita	gr
PMPV	Monarch Peak	KJf
PMRV	Maxie Ranch	Ku
PPFV	Parkfield	Qt, QP
PPRV	Paso Robles	QP
PPTV	Peach Tree Valley	Pml
PRCV	Roach Canyon	Ku
PSAV	San Ardo	Pml
PSEV	See Canyon	Mm
PSHV	Shandon	Mm
PSRV	Scobie Ranch	Ml
PTRV	Twissleman Ranch	QP
PTYV	Taylor Ranch	Pml
PWKV	Work Ranch	QP

— Middle Miocene Marine



1 1

1

1

1

1

1

1

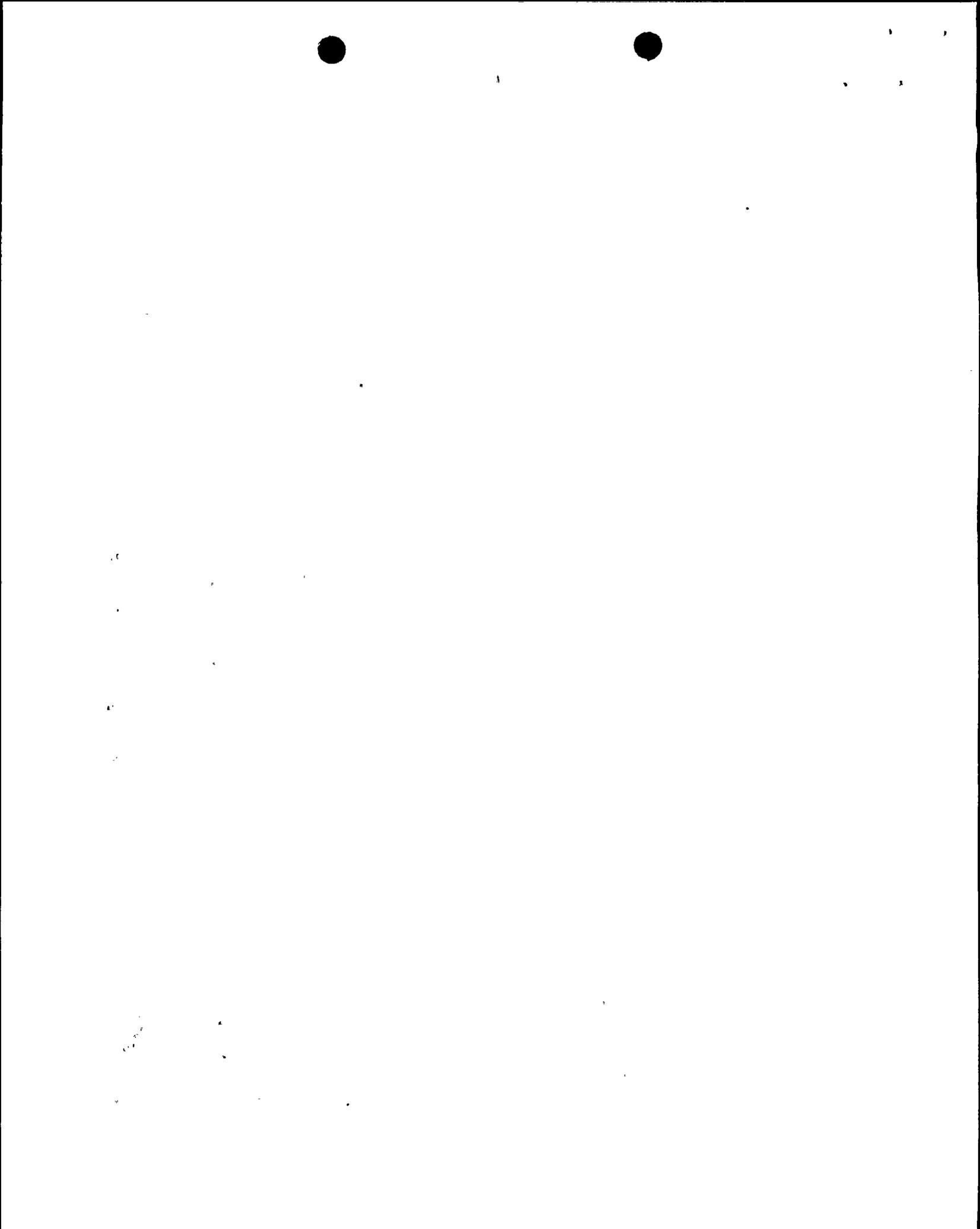
1

(Power Spectra)

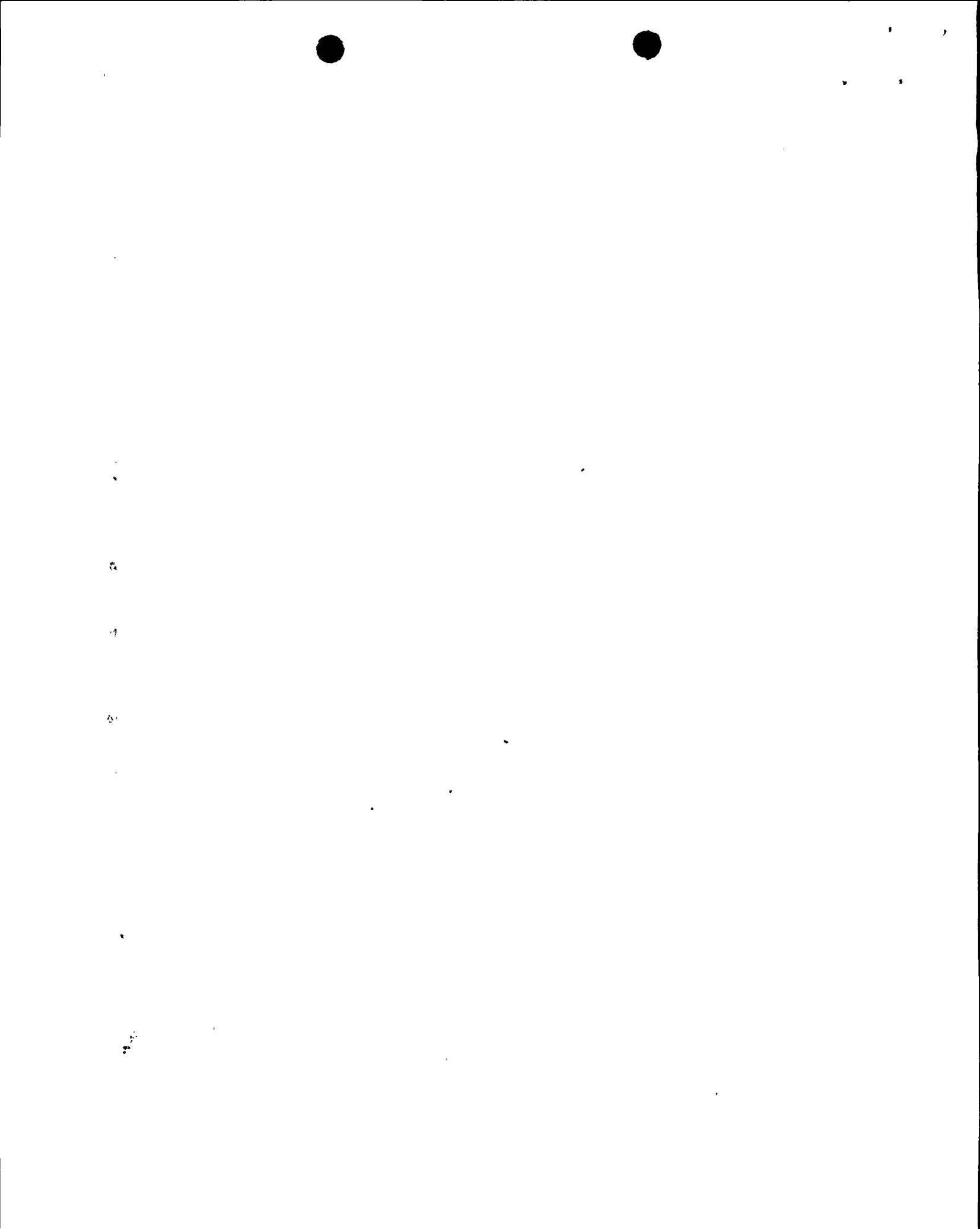
Table 3.2 Site Effect Results and Standard Errors

X = 2 ln ($\Lambda/\bar{\Lambda}$)
S = Standard Error (Napiers)

Station	1.5 Hz		3 Hz		6 Hz		12 Hz		24 Hz	
	<u>X</u>	<u>S</u>								
BAVV	-1.5	.1	-2.1	.0	-2.1	.0	-2.3	.1	-2.9	.5
BHGV	0.4	.1	0.4	.1	0.0	.0	-0.1	.1		
BHNV	0.2	.3	0.4	.2	-0.2	.3	-0.1	.2		
BCGV	0.5	.1	0.4	.0	1.0	.0	1.3	.0	1.7	.1
BEHV	1.6	.1	1.2	.1	1.0	.0	0.1	.1		
BEVV	-1.3	.1	0.6	.1	-0.7	.0	-1.5	.1	-2.2	.1
BHSV	-0.5	.1	0.6	.1	1.6	.1	1.9	.1		
BJCV	-2.7	.1	-2.6	.1	-1.8	.0	-0.1	.0	0.0	.1
BJOV	-2.5	.1	-1.6	.1	1.4	.0	2.0	.0	2.3	.1
BLRV	1.1	.1	0.5	.1	-0.2	.0	-0.6	.1	-1.9	.1
BMCV	-2.8	.1	-2.3	.1	-0.7	.0	0.6	.0	0.1	.1
BMHV	-2.3	.1	-1.5	.0	-1.2	.0	0.5	.0	0.8	.1
BMSV	0.1	.1	0.6	.1	0.3	.1	-0.5	.1		
BPCV	-2.2	.1	-1.7	.1	0.8	.0	3.0	.0	1.1	.1
BPFV	-0.8	.2	0.1	.1	0.5	.1	1.3	.1	0.	
BPIV	-2.0	.1	-1.7	.1	-0.9	.0	-0.4	.0	0.7	.1
BPPV	-1.8	.1	-0.4	.1	0.5	.1	0.9	.1		
BRMV	0.6	.1	1.5	.1	1.8	.1	1.2	.1		
BRVV	0.0	.1	-0.4	.1	-1.1	.1	-0.5	.1	-1.6	.2
BSUV	2.7	.1	2.0	.1	1.9	.1	1.9	.1		
BSCV	-0.3	.1	-0.9	.0	-1.2	.0	-1.0	.0	-0.7	.1
BSGV	-2.2	.1	-1.8	.1	-0.1	.0	2.0	.0	2.4	.1
BSLV	2.9	.1	2.7	.1	2.2	.1	0.9	.1		
BSRV	-2.0	.1	-1.1	.1	0.9	.1	1.9	.1	-0.0	.2
BVLV	0.7	.1	0.4	.1	-0.4	.0	0.0	.1	-1.0	.1



BVYV	-0.7	.1	-1.0	.1	-0.6	.0	0.6	.0	0.0	.1
CACV	1.9	.2	1.7	.1	1.2	.1	1.3	.2		
CADV	0.1	.1	0.4	.1	0.0	.1	-0.8	.2		
CAIV	-0.7	.2	-0.3	.2	0.9	.1	0.3	.1		
CALV	-2.0	.1	-1.8	.1	-1.8	.0	-2.3	.1		
CAOV	-1.3	.2	-1.1	.1	-1.7	.1				
CBRV	0.4	.1	0.2	.1	0.2	.1	-0.4	.1		
CBSV	1.2	.1	1.5	.1	2.2	.1	1.8	.2		
CBWV	0.5	.2	0.0	.1	0.1	.1	-0.1	.2		
CCOV	0.3	.1	-0.1	.1	-0.9	.1	-1.2	.1		
CCYV	-1.3	.1	-1.5	.1	-1.1	.1	-0.2	.1	1.2	.1
CDAV	0.7	.2	1.1	.2	0.1	.1				
CDOV	1.3	.1	1.7	.1	0.6	.1	-1.3	.2		
CDSV	0.5	.1	0.6	.1	1.2	.1	0.3	.2		
CDUV	3.1	.2	2.4	.1	1.9	.1				
CDVV	-1.1	.2	-1.1	.1	-0.1	.1	-1.0	.2		
CLCV	1.2	.2	1.4	.1	1.5	.1				
CMCV	2.4	.2	1.3	.1	0.6	.2	1.5	.2		
CMIV	-0.3	.1	-0.3	.1	-0.5	.1	-0.7	.1		
CMJV	-1.3	.1	-1.0	.1	-0.8	.1	-1.7	.1		
CMLV	-0.3	.2	-1.3	.1	-1.6	.1				
CMMV	-1.2	.2	-1.5	.1	-1.5	.1	-1.0	.2		
CMOV	-0.2	.2	0.8	.2	0.7	.1	0.2	.2		
CMPV	0.7	.1	0.4	.1	-0.1	.1	-1.3	.2		
CMRV	-1.5	.2	-1.6	.2	-2.3	.1	-2.3	.1		
COSV	-0.7	.2	-0.4	.1	-0.4	.1	-1.3	.1		
CPLV	-0.6	.1	-1.1	.1	-1.0	.1	-0.6	.1	-0.4	.3
CPNV	2.8	.3	2.0	.2	1.4	.2	1.3	.2		
CRAV	0.6	.2	-1.0	.2						
CRPV	0.3	.2	1.1	.2	0.2	.1	-0.3	.2		



CSAV	3.4	.2	2.6	.1	1.7	.1	0.4	.2		
CSCV	-0.1	.2	0.3	.1	-0.2	.1	-1.0	.2		
CSHV	-0.7	.1	-1.0	.1	-1.0	.1	0.8	.1	0.4	.3
CTLV	1.2	.1	0.3	.1	0.5	.1	-1.2	.1		
CVLV	0.3	.2	-0.1	.1	0.0	.1				
HAZV	-0.8	.1	-1.4	.1	-2.3	.0	-1.7	.0	-1.2	.2
HBTV	0.2	.1	-0.2	.1	-0.9	.1	-0.8	.1	-1.3	.3
HCAV	-0.1	.1	-0.6	.0	-0.9	.0	-0.8	.0	-2.5	.1
HCHV	0.4	.1	0.5	.0	0.6	.0	-0.6	.1		
HCOV	2.2	.1	1.7	.1	1.0	.1	1.0	.1		
HCPV	-0.9	.2	0.2	.1	0.4	.1	-0.9	.1		
HCRV	-1.3	.1	-1.8	.1	-1.1	.0	0.4	.0	-0.8	.1
HCZV	2.0	.1	1.2	.1	1.8	.1	0.7	.1	1.5	.2
HDLV	-1.0	.1	-0.8	.1	0.2	.0	0.5	.0	-0.3	.1
HFEV	-0.3	.1	-0.6	.0	-1.1	.0	-1.3	.0	-1.8	.1
HFIV	3.0	.1	2.2	.0	1.7	.0	-0.5	.1	3.1	.1
HFPV	-1.9	.1	-2.0	.1	-1.8	.0	-0.8	.0	-0.0	.1
HGSV	-1.3	.1	-1.7	.0	-1.6	.0	-1.4	.0	-2.1	.1
HGWV	-1.5	.1	-1.9	.1	-2.0	.0	-1.9	.0	-1.0	.1
HJGV	-1.5	.1	-1.7	.1	-1.4	.0	-0.8	.0	-0.3	.1
HJSV	-0.1	.1	-0.7	.1	-1.8	.0	-1.4	.1	-1.5	.1
HKRV	3.2	.1	2.6	.1	2.3	.0	1.2	.1	-0.1	.4
HJTV	-0.9	.1	-1.0	.1	-1.4	.0	-2.5	.1		
HMOV	-2.5	.2	-1.8	.1	-0.7	.1	0.0	.1	0.9	.1
HORV	2.8	.1	1.3	.1	0.4	.0	-0.9	.1	-2.0	.4
HPHV	2.9	.1	2.6	.1	2.4	.1	1.1	.3		
HPLV	-1.6	.1	-1.7	.1	-1.6	.0	-2.6	.1		
HPRV	2.1	.1	1.2	.1	1.2	.1	1.2	.1	1.1	.3
HQRV	-1.1	.1	-0.6	.0	-1.0	.0	-1.6	.1		
HSPV	1.1	.1	1.4	.0	0.6	.0	-1.1	.1		

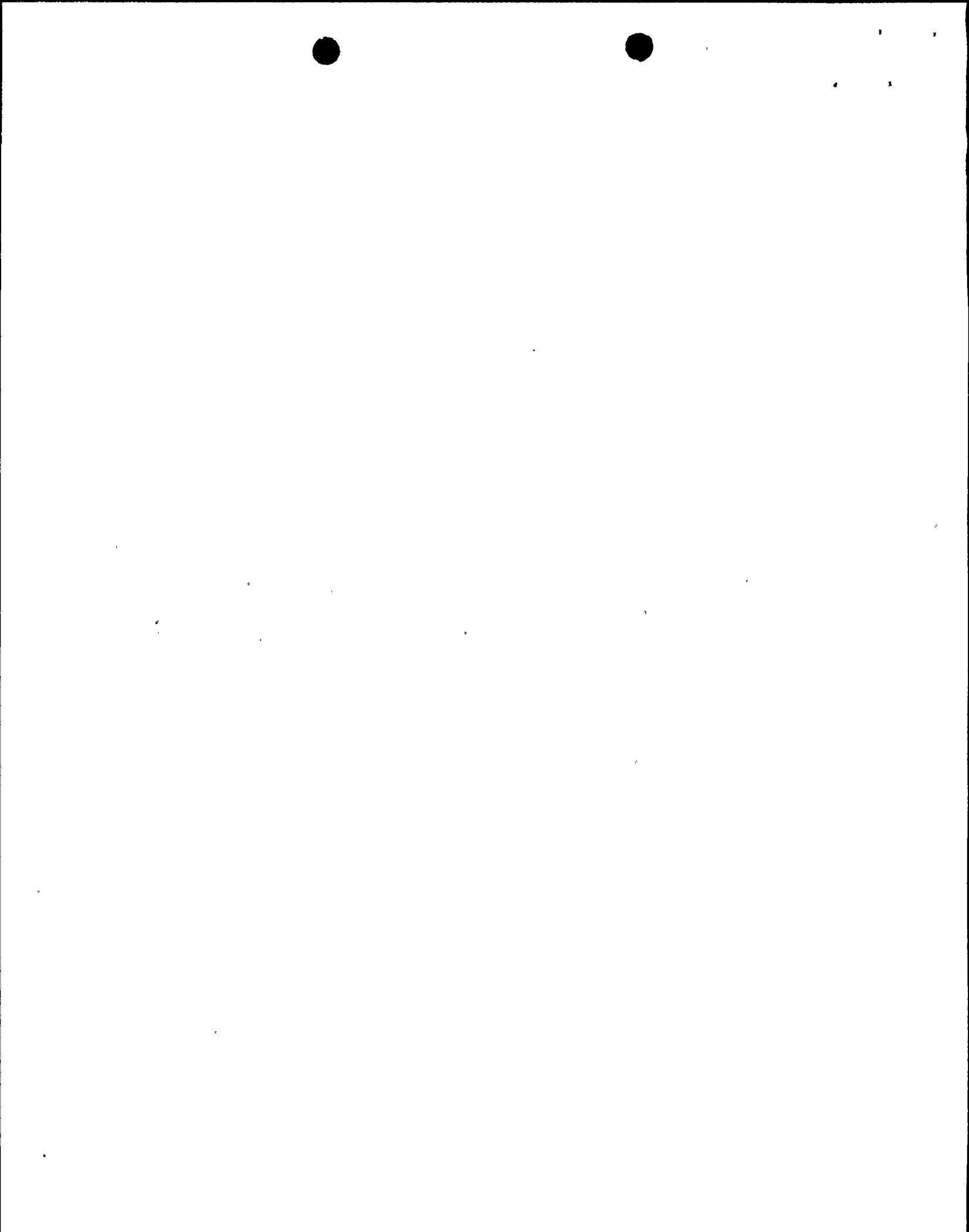


JSLV	0.7	.1	1.4	.1	1.4	.1	1.1	.2		
JSPV	-0.8	.2	-0.7	.1	0.2	.1	-0.2	.1		
JALV	-1.4	.1	-0.6	.1	0.2	.0	0.2	.1	1.1	.1
JBCV	-0.3	.3	-0.7	.2	-0.6	.1	-1.3	.2		
JJGV	1.2	.1	1.2	.1	1.1	.1	1.3	.1	0.2	.3
JBLV	-1.5	.1	-1.5	.1	-1.0	.1	-0.3	.1	2.6	.1
JBMV	-1.0	.1	-1.3	.1	-1.8	.1	-1.2	.1	-0.6	.1
JBZV	1.7	.1	1.8	.1	2.4	.1	2.6	.1	2.0	.1
JCBV	-0.7	.1	0.4	.1	0.1	.0	-0.2	.0	-0.1	.1
JECV	0.7	.1	1.2	.1	0.7	.1	-0.6	.2		
JEGV	0.1	.2	0.3	.1	1.0	.1	1.1	.1		
JHLV	-1.9	.2	-1.8	.1	-0.8	.1	0.3	.1		
JHPV	-0.0	.1	0.0	.1	-0.7	.1	-0.2	.1	-0.9	.2
JITV	0.4	.1	1.8	.1	1.7	.1	0.8	.1	-0.1	.2
JIXV	-0.9	.1	-0.7	.1	-0.3	.1	-0.1	.1	0.2	.1
JMGV	-0.8	.2	-0.5	.1	-0.3	.1	0.9	.1	-0.3	.4
JPLV	1.4	.1	1.1	.1	0.9	.1	0.9	.1		
JPPV	0.2	.1	-0.1	.1	-0.4	.1	0.4	.1	0.7	.1
JPRV	0.4	.2	0.7	.1	1.0	.1	0.8	.1		
JPSV	1.1	.1	0.9	.1	0.6	.1	-0.2	.1		
JRGV	1.9	.2	2.6	.1	2.8	.1	2.5	.1	2.2	.1
JRRV	-1.8	.1	-1.9	.1	-0.7	.0	-0.1	.0	0.1	.1
JRWV	-2.1	.2	-2.8	.1	-2.0	.1	-1.8	.1	-3.3	.2
JRXV	-2.3	.3	-3.9	.3	-4.7	.3				
JSAV	-0.9	.2	-0.6	.1	-0.7	.1	0.2	.1	0.6	.3
JSCV	-0.8	.1	-0.6	.1	-0.6	.1	-0.5	.1	-1.2	.1
JSFV	1.7	.1	1.1	.1	0.4	.1	-0.5	.1	0.1	.1
JSGV	1.1	.1	0.8	.1	0.9	.1	0.5	.1	0.1	.1
JSJV	0.9	.1	0.7	.1	0.9	.1	1.2	.1	1.0	.1
JSMV	0.8	.1	0.2	.1	0.5	.1	0.1	.1	0.8	.2



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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

JSSV	-0.6	.1	-0.4	.1	-1.0	.1	-1.0	.1	-0.3	.2
JSTV	-0.1	.1	-0.3	.1	-0.3	.1	0.5	.1	0.7	.1
JTGV	0.9	.1	1.0	.1	1.1	.1	1.5	.1	0.2	.2
JUCV	-1.6	.2	-1.1	.1	-0.6	.1	-0.4	.1	-0.9	.2
JWSV	1.7	.2	1.5	.2	1.1	.1	1.2	.1		
NGVV	0.4	.3	0.7	.2	0.5	.1	-0.1	.2		
NIMV	2.7	.3	1.8	.2	0.3	.2				
NIHV	1.0	.2	1.0	.1	0.6	.1	-0.2	.2		
NPRV	0.1	.3	0.3	.2	0.9	.2				
NTPV	-0.8	.3	-0.0	.2	0.1	.1	0.9	.1		
PADV	0.4	.2	0.6	.1	1.0	.1				
PACV	-0.3	.2	0.0	.1	0.3	.1	-0.3	.2		
PANV	0.0	.3	1.7	.2	1.9	.1				
PAIV	-1.9	.2	-0.9	.1	-0.5	.1	0.5	.1		
PARV	1.0	.2	0.5	.2	0.5	.2				
PIWV	1.2	.1	0.3	.1	0.5	.1	2.2	.1	1.8	.2
PIYV	-0.0	.2	0.1	.2	1.7	.1	1.3	.1		
PCGV	-1.8	.3	-1.6	.2	-1.4	.1	-0.8	.2		
PCRIV	-1.3	.1	-1.1	.1	-1.0	.1	-1.4	.2		
PGIV	-2.0	.1	-2.6	.1	-2.5	.1	-2.3	.1		
PIAV	1.4	.1	1.6	.1	0.9	.1	0.4	.1		
PHCV	-0.8	.2	-0.5	.1	-0.9	.1	-2.3	.2		
PHGV	1.1	.1	1.1	.1	0.6	.1	0.2	.1		
PHRV	2.3	.1	1.3	.1	0.2	.1	-0.5	.1		
PIVV	1.9	.1	1.6	.1	2.1	.1	1.4	.1		
PIOV	1.7	.1	2.3	.1	1.8	.1	1.9	.1	2.0	.2
PMGV	-2.2	.3	-1.8	.2	-1.2	.1	-0.1	.1		
PMPV	-0.1	.1	-0.5	.1	-1.0	.1	0.1	.1	-0.9	.2
PMRV	-2.1	.5	-1.9	.2	-1.6	.2	-0.1	.2		
PPIV	1.1	.1	1.1	.1	0.2	.1	-0.1	.1		



San Luis Obispo
 Santa Barbara
 Santa Maria
 San Jose

San Luis Obispo	PPRV	0.0	.3	0.4	.2	0.7	.1	1.9	.2		
	PRCV	-0.9	.1	-0.5	.1	0.3	.1	0.4	.1	-0.2	.4
	PSAV	2.7	.1	2.0	.1	1.1	.1	1.8	.2		
	PSEV	0.2	.3	1.0	.1	<u>2.5</u>	.1	0.3	.2		
	PSIV	1.3	.2	0.7	.1	1.3	.1	1.0	.2		
	PSMV	-0.0	.1	0.1	.1	-1.0	.1	-1.4	.1		
	PTRV	1.9	.2	2.2	.2						
	PTYV	-0.7	.1	-0.2	.1	-0.1	.1	-0.5	.1		
	PWKV	1.0	.1	0.4	.1	0.0	.1	0.5	.1		

13 45.88

20/22

2lu → 2.5

Walter
 Silva
 of Woodward-Clyde

3.5 times greater
 than average of
 Central Calif. St.
 at 6 Hz.

	Amp.
1.5 Hz	1.1
3.	1.6
6.	3.5
12	1.2



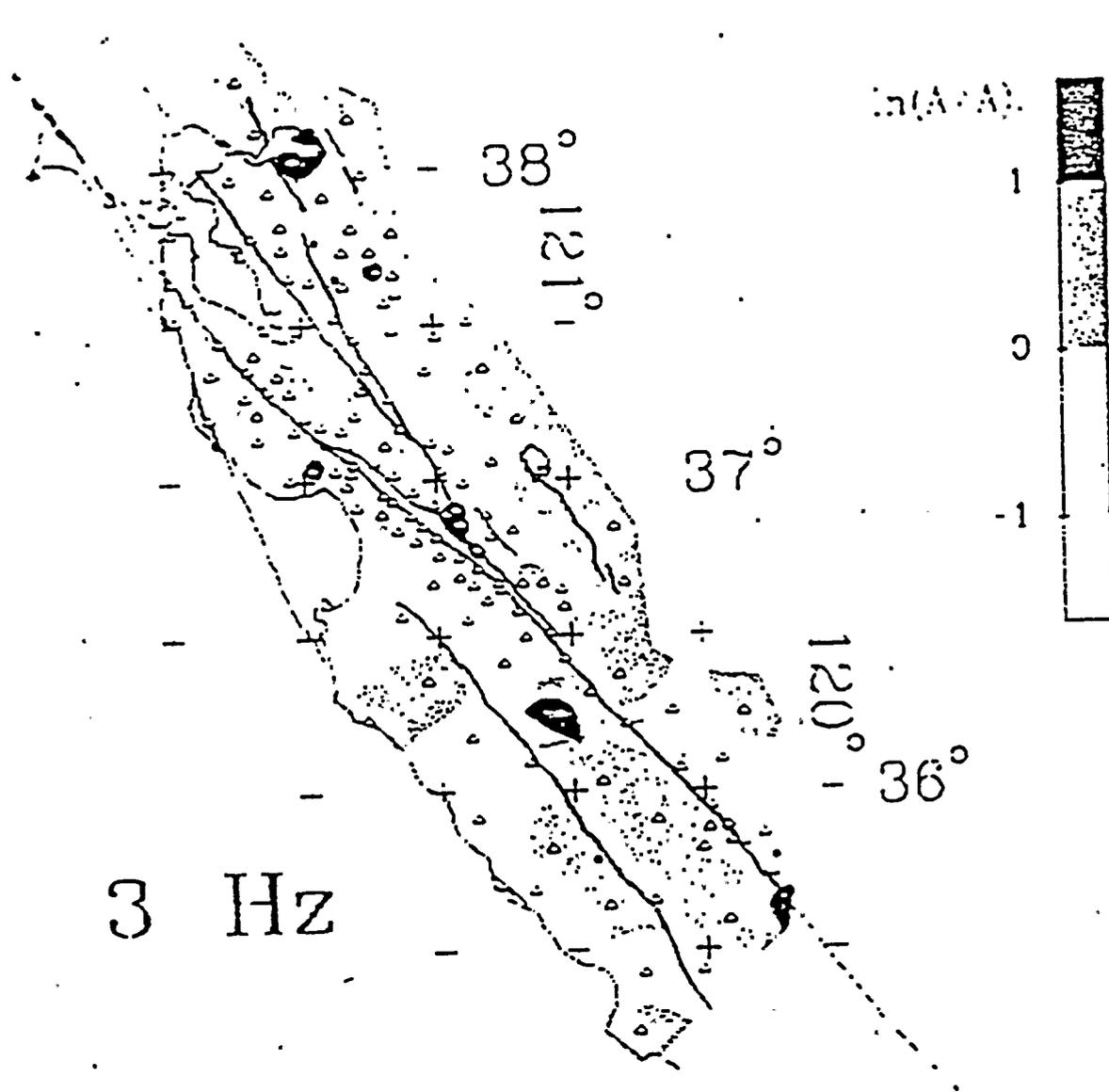


Figure 3.3b



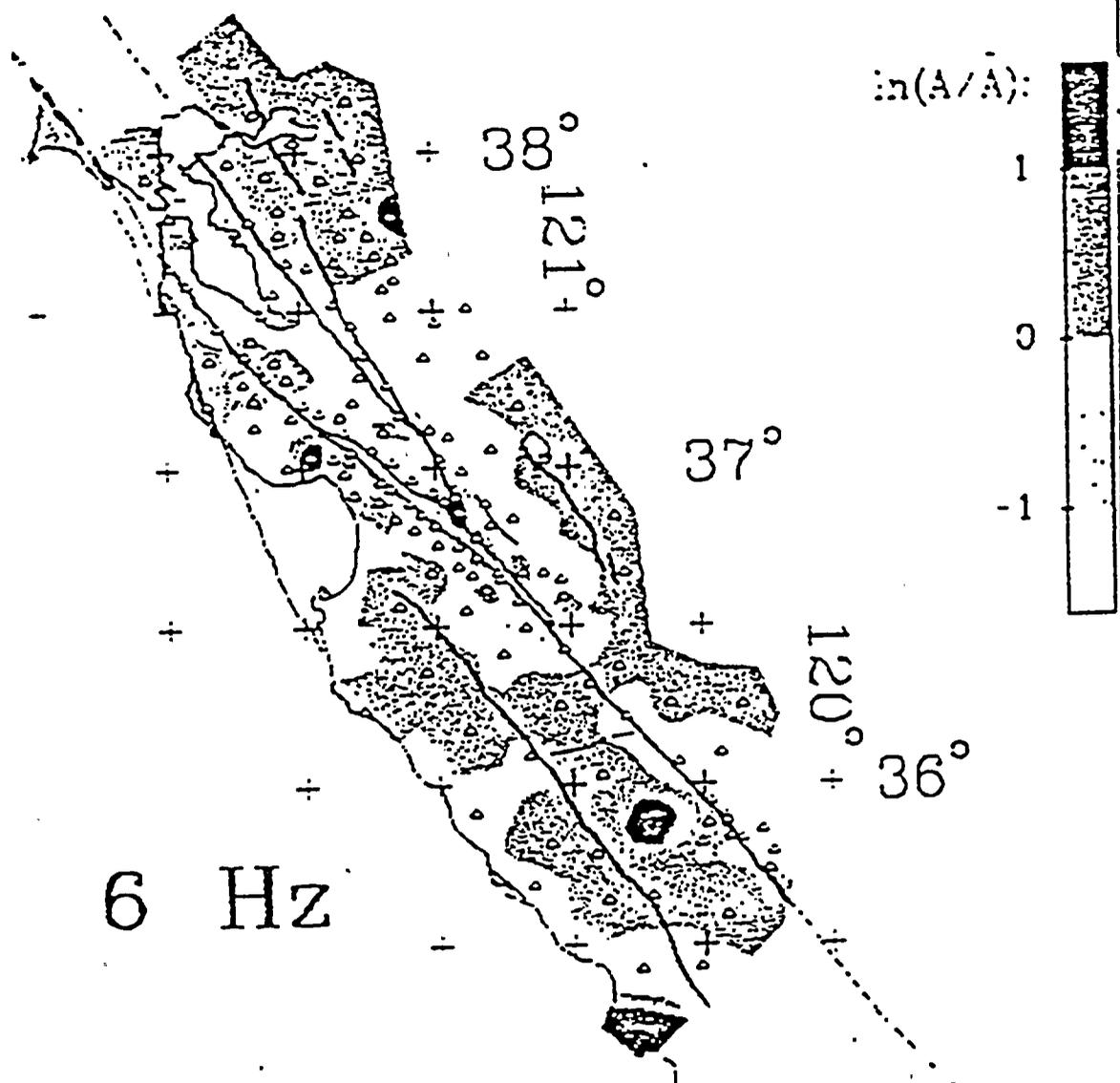
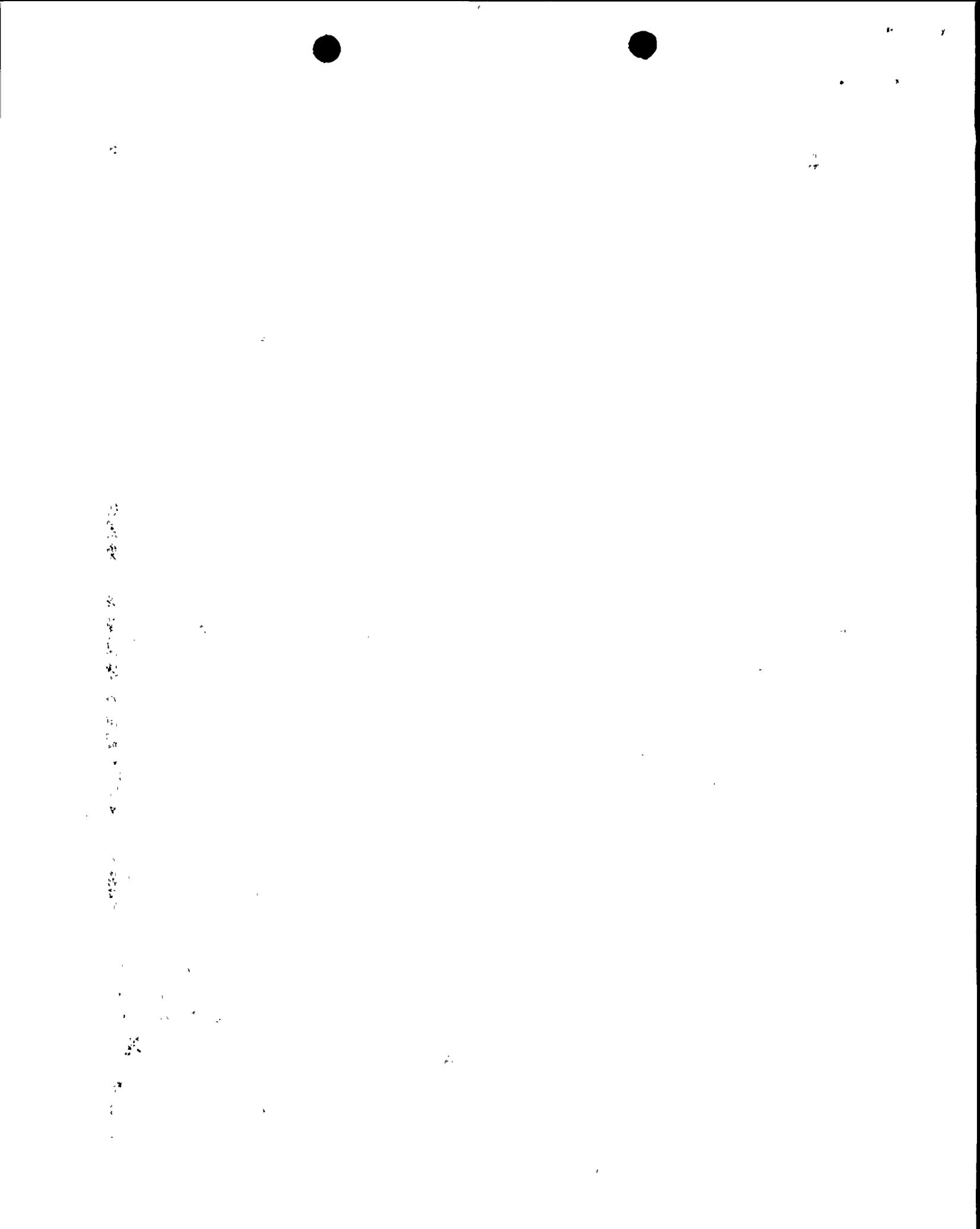
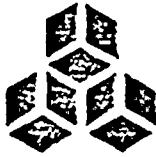


Figure 3.3c





S-CUBED

A Division of Maxwell Laboratories, Inc.

August 20, 1987

Dr. Jean Savy
Lawrence Livermore National Laboratory
P. O. Box 808
Mail Stop L-106
Livermore, California 94550

Dear Jean:

The following is my report on the San Francisco meeting of July 15 and 16, 1987, between the NRC staff and P. G. and E. I have confined my comments to the ground motion tasks of the Long Term Seismic Program.

The P.G. and E. work on ground motion is well focussed, the crucial problems seem to have been identified, and progress on these problems has been very impressive. Furthermore, virtually all comments previously made by the ground motion review panel have been, or are being, addressed by the project team.

Also impressive is the degree to which various parts of the study have now been integrated. For example, the site recording program has yielded important site-specific estimates of t (the scale factor in the attenuation exponent), and these estimates have been incorporated into the numerical modeling work performed by Woodward-Clyde. As a second example, a scheme has been devised and used to estimate site-specific spatial coherency; this was accomplished by combining results of P.G. and E.'s site recording program with Woodward-Clyde's numerical modeling program. The geology/geophysics team is presently developing estimates of the characteristic slip for a hypothesized Hosgri fault earthquake, and I hope that these results too will be incorporated into the numerical ground motion modeling.

There was little discussion at the workshop of the problem of integrating the ground motion results with the Soil Structure Interaction (SSI) analyses. The ground motion panel was provided with a written report on the December 1986 SSI workshop, which addressed this problem. That report suggests that an effort is being made to incorporate the ground motion spatial coherency models into SSI. However, the methodology should be discussed with the ground motion panel as well, I think.



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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

The numerical ground motion simulations shown at the workshop were systematically deficient in low-frequency energy. Unfortunately, the frequency band in which the simulations are deficient extends at least as high as 2 or 3 Hz in some cases, well within the frequency range of engineering interest. This is the inevitable result of using subevents of small source dimension. Because of the use of Joyner-Boore scaling of the subevent moment, the high-frequency asymptote of the simulated response (or Fourier) spectrum should be nearly invariant with subevent size, as confirmed by the numerical simulations. However, as the subevent size is reduced, the low-frequency problem gets worse, and extends to higher frequency. I think this argues against using the small subevent size in the simulations. I recognize that using a large subevent raises some concern about the theoretical validity of the Fraunhofer approximation (concern which I voiced myself in an earlier review letter). However, I think it is now clear that simulations using the larger subevents are more successful in matching the characteristics of recorded ground motion from large earthquakes.

The uncertainty in the numerical modeling results needs to be estimated. This might be accomplished by applying the modeling method to recorded earthquakes other than Coalinga and Imperial Valley (since these events were used to calibrate the model to begin with). It is obviously important that such modeling be carried out without incorporating source information (such as asperity locations) which would not be available *a priori* for a prospective Hosgri Fault earthquake.

Sincerely,



Steven M. Day





1

2

3

4

5

6

UNIVERSITY OF CALIFORNIA, SANTA BARBARA

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO



SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF GEOLOGICAL SCIENCES

SANTA BARBARA, CALIFORNIA 93106

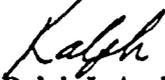
July 27, 1987

Dr. Jean Savy,
Lawrence Livermore Lab.
Mail L-196
P.O. 808
Livermore, CA 94550

Dear Jean,

Enclosed are my comments on the PG&E progress report presented July 15 and 16, 1987, in San Francisco. Please send to me the comments of Steve and Kei when you receive them.

Sincerely,


Ralph J. Archuleta

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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

Clearly PG&E has done a substantial amount of work in preparation for the final stages. The presentations by Drs. Coppersmith, Sadigh and Sommerville were real tours de force. I left this meeting with a much clearer idea of how PG&E was developing individual approaches and how it was integrating its various approaches. Loosely quoting George Bernard Shaw, 'Science never solves anything. For every question it answers it finds ten more.' This may be the reason behind Mr. Cluff's remark that in spite of the obvious progress being made there is always a "However."

I have some fundamental questions about the semi-empirical numerical modeling approach. The approach itself seems sound. My concern is the proposed application to DCPD given the geometry of the faults and velocity structure for DCPD.

(1) *The distribution of slip with depth*

Based on their analysis of the Imperial Valley earthquake PG&E has decided to use the distribution of slip with depth found by Hartzell and Heaton to model faulting pertinent to DCPD. Thus faulting near DCPD will have three to eight times as much slip at depth as near the surface. I regard this as a serious error which underdetermines the expected ground motion.

The velocity structure, which I could not find anywhere in the notes so I used one based on viewgraphs presented, between the Hosgri and DCPD is approximately:

Depth	Thickness	P-wave	S-wave	Density
0.0	0.2	2.0	0.7	2.4
0.2	0.3	3.5	2.0	2.5
0.5	0.25	5.0	2.9	2.6
0.75	11.25	5.5	3.2	2.7
12.0	=22.00	6.3	3.64	2.8

The significant aspect of this structure is that the elastic material in the seismogenic zone comes within one kilometer of the surface. This velocity structure is far different from that in the Imperial Valley which has been used for much of the calibration of the method. The Imperial Valley has large slip amplitudes in the region where the velocity (P-wave 5.65-5.85 km/s, S-wave 3.27-3.38 km/s) is high and uniform similar to that found between 0.75 km and 12 km depth at DCPD. The amplitudes of the ground motion scale directly with the shear modulus which is proportional to the S-wave velocity squared. Thus by restricting the large slip to occur only at depths around 10 km, the ground motion contribution from the upper parts of the fault that are closer to DCPD is significantly smaller. There is no justification for using the Hartzell and Heaton distribution of slip for DCPD. What should be used is the fact that wherever the elastic properties are similar to that in the known seismogenic zone one can expect similar values of stress drop and slip.

A major point of discussion was whether or not the upper two kilometers could store and

Vertical text or markings on the left side of the page, possibly bleed-through or a scanning artifact.

release elastic energy that would contribute to strong ground motion. In fact, PG&E has shown that that is true. In enclosure B, Section 4.5 PG&E discusses simulation of the San Fernando Pacoima dam record. First note that in Figure 4.26 that the maximum amount of slip occurs in the upper couple of kilometers. In Figure 4.28 the time history resulting from this distribution of slip is shown. In particular note the waveforms and amplitudes arriving around 10-11 seconds. In order to get a better match to the actual data (Figure 4.27) the slip distribution from Figure 4.26 is modified. The original and modified distributions are both shown in Figure 4.29. The only significant difference between the two is the amount of slip in the shallowest part of the fault 5.7 m compared to 3.25 m. Now compare Figures 4.30 and 4.28 paying particular attention to the accelerograms around 10-11 seconds. There is a substantial increase in the amplitude which must be due only to the change from 3.25 m to 5.7 m. This change was made to produce a better fit to the data (Section 4.5, Enclosure B) which shows the peak acceleration late in the record.

Although there are not a lot of well documented cases which distinguish between energy released in the upper kilometers, there are some. I have included a copy of the June 23, 1987 EOS. Please note the abstract under the Seismology heading. It has several relevant results including significant amount of energy release at shallow depths from a thrust fault. Interestingly the rupture apparently propagated downdip.

While the Imperial Valley may be among the best documented earthquakes, its slip distribution is strongly correlated with the velocity structure. I can see no reason to assume that such a slip distribution would be generic to other earthquakes that occur in different velocity structures.

(2) *The appropriate Green's functions for the semi-empirical source*

The empirical source is meant to account for variations in the seismogram that result from wavelengths shorter than the characteristic dimension of the source. There are some pitfalls in this approach as it is currently being applied by PG&E. First, the effects of propagation are being computed using generalized rays. Although I cannot be certain, I think that the only rays being considered are direct P wave, direct S wave and possibly the postcritical reflection from 12 km. These rays may be sufficient for a source which is deep (As shown below at least one more ray should be added). When the source is in the upper couple of kilometers, the surface wave contribution must be included. The calibration of the method thus far has not been adequate. The Imperial Valley source is taken from an aftershock at 9.5 km depth; the Coalinga aftershock is also at 10 km depth. The ground motion will not be representative of the response one would expect from a source at 1 or 2 km depth. The source must be convolved with the complete Green's function and not just one or two rays. I am including two figures which shows the complete response (0-16 Hz) at DCPD due to a double-couple, vertical, strike-slip, source at 2 km depth (DCSS2) and at 10 km (DCSS10). The source is 5.41 km from the observer at an azimuth of 67.5°, clockwise from North. The velocity structure is the same as that given above. The components of ground motion are vertical, radial and tangential from top to bottom, respectively. As one can see from these figures the radial and vertical components of ground motion from the source at 2 km depth is significantly different from that at 10 km. The ground motion on all three components for the 10 km deep source could be adequately represented by several generalized rays; however, that is not true for the shallow source.



20
21
22
23
24
25
26
27
28
29
30
31
32
33
34
35
36
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

RJA

It is worth noting that the prominent phase seen about 0.6 seconds after the direct S wave (and multiples thereafter) is not the postcritical reflection due to the boundary at 12 km. The observer is not at the appropriate range. This pulse is due to the reflection from the boundary at 0.2 km depth. (A much smaller pulse at 0.9 seconds after the direct S is due to the reflection from 0.5 km.) As one can see from the tangential component there is a site condition, i.e., the wave is bouncing between the free surface and the boundary at 0.2 km, that could lead to an enrichment of spectral content at 1.7 Hz. This particular ray is not presently included in the suite of generalized rays being used to compute the ground motion at DCP. When the source is at the right distance for the postcritical reflection from 12 km, the postcritical reflection and the near surface reflection are going to interfere.

My point is that the complete Green's functions are approximated by a couple of generalized rays representing body waves. These generalized rays may well represent the ground response for a deep source, but they are not appropriate for a shallow source that exists near DCP. Complete Green's functions are necessary when modeling the shallow part of the rupture. It would also appear that at least one additional ray is needed even for the deep sources.

The second issue is attenuation. The assumption being made in this approach is that the attenuation for the medium around DCP is equal to or greater than the attenuation of the medium from which the empirical source was recorded. This assumption has not been justified. In fact, PG&E had assumed less attenuation of high frequencies for a rock site compared to a soil site in the regression analysis approach. I think that attenuation is a solvable problem at DCP. It will require the semi-empirical approach to be modified if PG&E intends on using the Imperial Valley and Coalinga aftershocks as its empirical source functions. It was surprising to see that both the Coalinga and Imperial Valley aftershock had the same attenuation considering that Coalinga aftershock was mostly recorded on a rock sites.

(3) *Style of faulting*

There were several comments made about whether or not one could have strike-slip faulting on a fault with a dip significantly different from 90°. Part of the discussion centered on the North Palm Springs earthquake of July 8, 1986. There are other earthquakes that also show dominant strike-slip on shallow dipping faults. I have included references to both the M_L 6.4 Chalfant Valley earthquake of July 21, 1986, and the M_L 1952 Kern County earthquake. In both cases the strike-slip to reverse component is about 2 to 1. The Kern County earthquake needs a qualifier. On part of the fault near the hypocenter, dip of 73°, the reverse component is 1.2 times larger than the strike-slip. However, on the rest of the fault with dips of 35° and 20°, the ratio is 2 to 1 for strike-slip over reverse slip. Thus I think PG&E is not covering the full range of possibilities for faulting near DCP if they restrict strike-slip to the nearly vertical fault and reverse slip to the dipping faults. It seems that real earthquakes have both components on shallow dipping faults.

The next major issue is coherence. The coherence technique that follows from Smith et al. (BSSA, 1982, pp. 237-258) is dependent on the length of the time window being used. For example, in



Vertical text or markings along the left edge of the page, possibly bleed-through from the reverse side.

Small, faint markings or text at the bottom left corner of the page.

Comments on PG&E Meeting
July 15, 16, 1987

Ralph J. Archuleta
RJA

Smith et al. they find a spatial covariance of about 0.27 for the North-South component from stations 1 and 3 (Figure 15, distance of 55 m, no filter). However, Spudich and Cranswick (BSSA, 1984, pp. 2083-2114) find a peak correlation of almost 1.0 for the time during which the maximum amplitude S-waves are arriving for the same stations (Figure 7). The principal difference between the two methods is the length of the time window. Spudich and Cranswick use 1.0 second window; Smith et al. use a 6.0 second window. Since the coherence will be used for the input to the base, it may be that the appropriate time window will be considerably less than 1.0 second. Regardless, I think that the coherence analysis is going to require more investigation.

My final comments are on the regression analysis. I appreciate the efforts that are being made to distinguish between rock and soil sites, e.g., Appendix C, Enclosure E. I suppose from a statistical point of view there may be a 0.19 difference in the mean. But, my gosh, in looking at the scatter in the peak acceleration data for a given magnitude or a given distance of a rock site I wonder why bother. The factor of 0.19 is completely lost in the dispersion. I am going to bring up a point that I have previously objected to. The regression curves always have a term that represents geometrical attenuation like $1/(R + r)^\gamma$ where R is some measure of distance from the fault to the receiver, r is a constant and γ is a constant generally close to 1.0. In this work, r is disguised in the form C(M) where M is magnitude, e.g., $0.88\exp(0.47M)$, Enclosure E, p. IV-1. (I could not find the numerical values used for strike slip, this is the expression for reverse faults.) This term is always put in there to allow for saturation. If R is much greater than r, the peak acceleration falls like $R^{-\gamma}$; however, when R is much less than r, the peak acceleration is almost constant.

In the regression there are other terms for which a physical basis exists, but this one has none. In fact, contrary to all the arguments about goodness of fit, it is obvious that this term is an artifact. Consider Figure C-3, magnitude 6.3-6.7. The 1979 Imperial Valley earthquake is responsible for almost all of the strike-slip data points shown. A $1/R$ line fits the trend of the data from 5.0 km to 200 km. The saturation comes from points plotted at distances of 0.5 to 3.0 km. Yet, PG&E has gone through considerable effort to show that the asperity located at a true distance of about 12-13 km is the cause of the peak accelerations. The saturation is bogus. If the points plotted at 0.5-3.0 km were plotted at 12-13 km, there would be no saturation. In Figure C-1, M5.8-6.2 the two data plotted at 0.5 km which apparently show saturation are from the same station (Coyote Lake Dam)

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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

Comments on PG&E Meeting
July 15, 16, 1987

Ralph J. Archuleta
RJA

for the Morgan Hill earthquake. Yet, a recent paper by Hartzell and Heaton (BSSA, 1986, pp. 649-674) as well as abstracts by Beroza and Spudich show that the likely cause of the peak acceleration is a large asperity located about 12 km from this station. There are very few data which can honestly be plotted at 2-3 km distance, perhaps Pacoima Dam. In the very few cases where the data have been analyzed to get even a rough picture of the faulting, the analysis shows that the stations that are more like 10 km distant from the faulting responsible for the peak acceleration. If one puts numbers into this fudge factor $0.88\exp(0.47M)$, the distance r is quite large, e.g. $M=6.5$, $r=18.7$ km; $M=7.0$, $r=23.6$ km. These values are substantially greater than the depth of the seismogenic zone which might conceivably be a physical parameter that could limit peak acceleration. Considering the proximity of DCPD to the faults in the area, this factor $0.88\exp(0.47M)$ (or one like it) is the determining factor for peak acceleration, and yet it is the most questionable. It has no physical basis. It has almost zero data to support it.

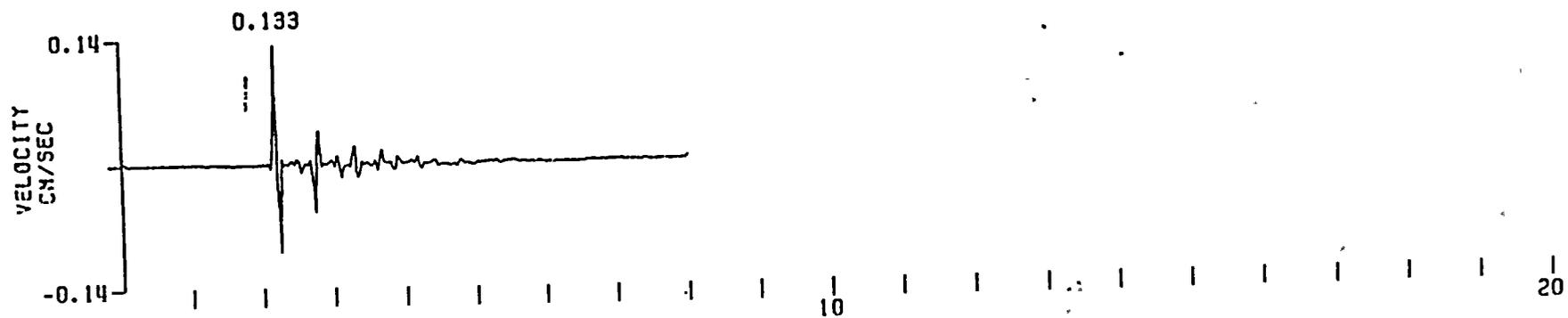
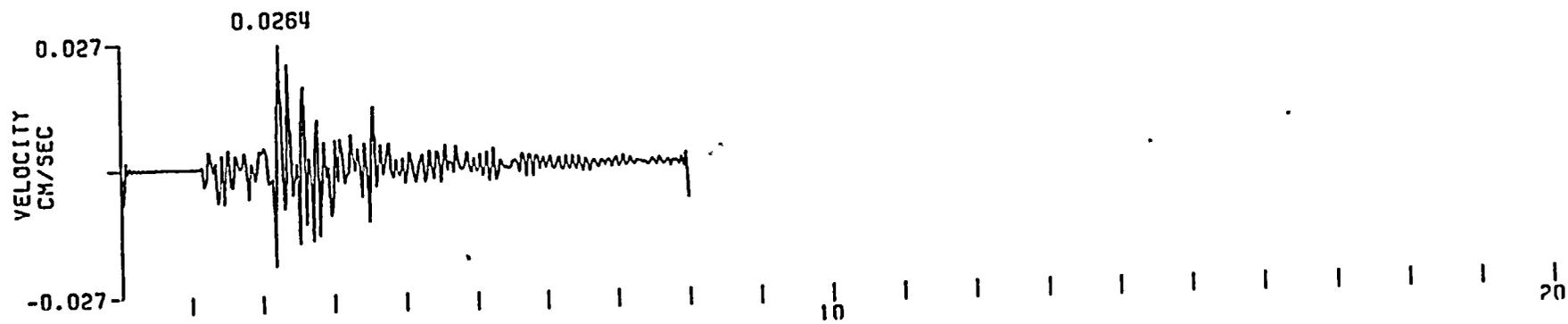
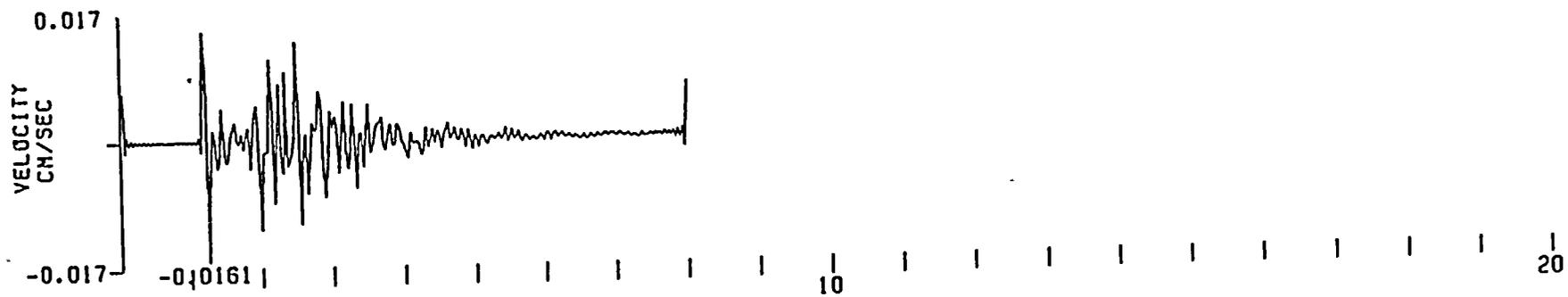
Overall I think that PG&E is converging on a final product. Despite my criticisms of particular aspects I was impressed with the quality and quantity of work. The instrumentation program is of the highest quality. The numerical modeling is bearing down on the important questions. The acceleration data base is probably the most complete anywhere. The PG&E effort is quite outstanding.



1
2
3
4
5

.....

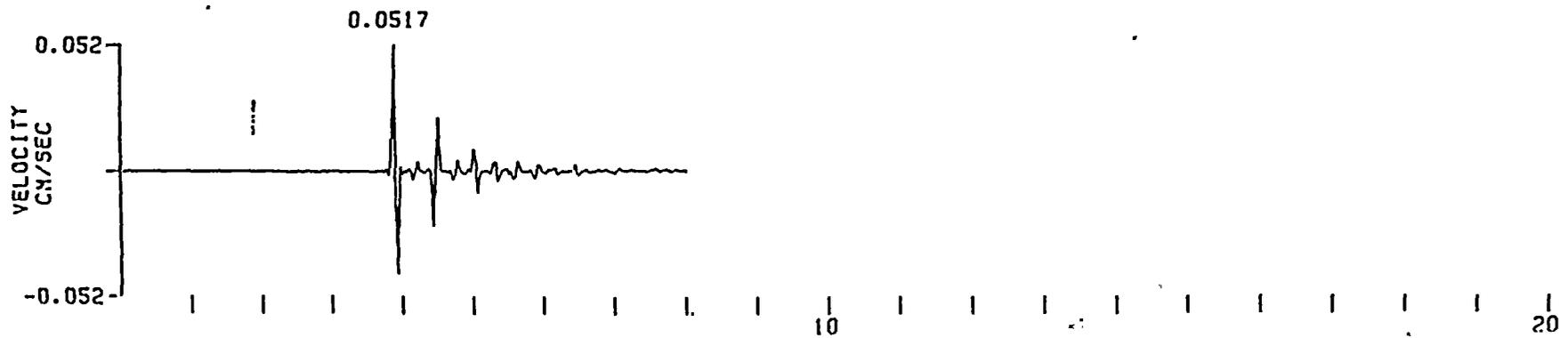
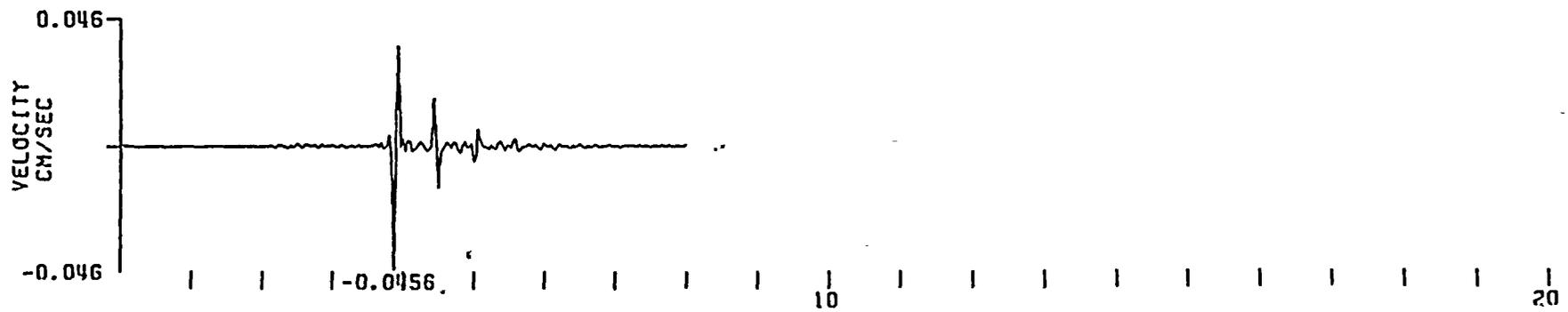
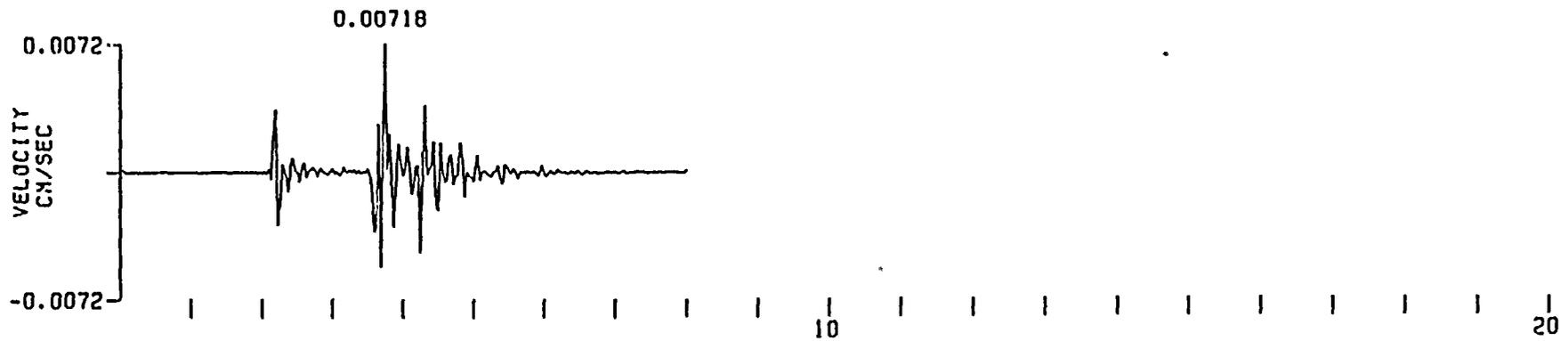
VDCSS2.16
RDCSS2.16
TOCSS2.16



SI UNITS



VDCSS10.16
RDCSS10.16
TOCSS10.16



SECONDS



• • •

[The main body of the page contains extremely faint and illegible text, likely bleed-through from the reverse side of the document. The text is scattered across the page and is not readable.]

Tectonophysics

2150 Plate boundary structures and processes EARLY CENOZOIC CRUST AT THE NORVEGIAN CONTINENTAL MARGIN AND THE CONJUGATE JAN MAYEN RIDGE
 J. Skogvold and O. Sidhe (Department of geology, University of Oslo, Norway)
 Seismic profiles at the Voring Plateau Margin off Norway and the northern Jan Mayen Ridge have provided a framework for the early Tertiary plate boundary and passive margin evolution. The Paleogene evolution of both areas is quite similar and the orogenic basement reflections were formed by extension of flow basaltic in the early Tertiary. During the first 2-3 million years of opening, islandic type spreading related to the North Atlantic Volcanic Province created the occurred dipping reflector sequences. At this time basalt flows also covered the adjacent thinned and included continental crust. Off Norway, the continent-ocean boundary is proposed to be located just landward of anomaly 248 a short distance west of the Voring Plateau Escarpment. Reflector K at the base of the dipping sequence is only re-recognized landward of the boundary defining a region of "transitional crust". It is suggested that these margins are of a "volcanic" type that are associated with initial uplift due to rifting in previously thinned crust. (Passive margin, marginal high, occurred dipping reflectors, Norwegian Sea.)
 J. Geophys. Res., B, Paper 686279

Volcanology

2174 Hydrothermal Systems HELIUM ISOTOPIES: LOWER GEYSER BASIN, YELLOWSTONE NATIONAL PARK
 M. H. Kennedy (Department of Physics, University of California, Berkeley, California 94720)
 J. M. Reynolds, S. P. Smith, S. M. Trumbull
 High ³He/⁴He ratios associated with the Yellowstone caldera reflect the presence of a magmatic helium component. This component is ultimately derived from a mantle plume capped by a cooling batholith underlying the caldera. In surface hot springs, fumaroles, etc., the ³He/⁴He ratio varies from 1 to 16 times the air ratio. The variation is produced by varying degrees of dilution of the magmatic component with radiogenic helium. The radiogenic helium is crustal-derived and is thought to be scavenged from aquifers in which the hydrothermal fluids circulate. We determined the helium isotopic composition in 17 different springs from the Lower Geyser Basin, a large hydrothermal basin within the caldera. The ³He/⁴He ratio was found to vary from 2.7 to 7.7 times the air ratio. The variations correlate with variations in water chemistry. Specifically, the ³He/⁴He ratio increased with total bicarbonate concentration. The dissolved bicarbonate is from gas-water-rock interactions involving CO₂ and Ca-silicates. The concentration of bicarbonate is a function of the availability of dissolved Ca, which, in turn, is a function of deep boiling with phase separation prior to Ca-bicarbonate conversion. The correlation of high ³He/⁴He ratios with high bicarbonate is interpreted as the result of deep dilution of a single thermal fluid with cooler water during ascent to the surface. The dilution and cooling waters deep boiling, and therefore both Ca₂ and He are retained in the rising fluid. Fluids that are not diluted will boil to a greater extent, leaving a large proportion of He, as well as Ca, leaving a helium-poor residual fluid in which the isotopic composition of helium will be strongly affected by the addition of radiogenic helium. The helium isotopic composition are also affected by the presence or absence of reactive volcanic glass in shallow reservoirs, the extent of these shallow reservoirs, and by the absorption of steam, derived from the early stage of deep boiling, into near-surface cold waters. (Helium isotopes, hydrothermal, Yellowstone caldera.)
 J. Geophys. Res., B, Paper 686145

Planetology: Comets and Small Bodies

6023 Interactions with solar wind plasma and fields MAGNETOTAILS AT UNMAGNETIZED BODIES: COMPARISON OF COMET GIACCHINI-ZINNIR AND VENUS
 D. J. McCombs (Los Alamos National Laboratory, MS D-438, Los Alamos NM 87545)
 J. T. Gosling, C. T. Russell, and J. A. Shin
 Both Comet Giacchini-Zinnir (G-Z) and Venus have magnetotails consisting of draped interplanetary magnetic field lines. This field line draping is caused by a velocity shear between regions of greater flow speeds away from the bodies and lower flow speeds near the bodies. This shearing within the Venus magnetotail by the Pioneer Venus Orbiter and within the G-Z tail by the International Cometary Explorer traversal of G-Z, have previously been combined with stress balance considerations to infer many of the physical characteristics of these two magnetotails. In the present paper we compare and contrast these physical characteristics and thereby examine these aspects of the interactions with the solar wind and draped magnetotail forming processes which are common at the two bodies, and those which are different. We find that the most analogous regions play a crucial role in the tail formation process at both Venus and G-Z, and that draping of the two very different sized bodies occurs on ionopause scale sizes. On the other hand, ion diameters, down tail mass fluxes, outward JxR forces, and like terms are factors of ~10⁴, 50, 100, and 20 times greater in the G-Z tail than in Venus', while both flow speeds and ion temperatures are factors of ~15 and 240 times lower. These large quantitative differences in the properties within the two magnetotails are attributable to the significantly greater upstream mass loading of the solar wind by the extended neutral atmosphere at G-Z (comets in general) composed of the gravitationally bound atmosphere of Venus. (Magnetic field draping, International Cometary Explorer, Pioneer Venus Orbiter)
 J. Geophys. Res., A, Paper 7A9190

Seismology

7215 Earthquake parameters DEPTH OF FAULTING DURING THE 1948 MEIKREIMI, AUSTRALIA, PARIBURAK SUPPLEMENT, DETERMINED FROM WAVE FORM ANALYSIS OF LOCAL SEISMOGRAMS
 Charles A. Langston (Department of Geosciences, The Pennsylvania State University, University Park, Pennsylvania 16802)
 Wave forms for eleven interlocks and 48 aftershocks of the M 6.9 Meikreimi earthquake recorded at the WASH station NW are analyzed to determine the depth distribution of faulting during this unusual interplate earthquake sequence. Clear depth phases including α_P and α_S are seen in the local seismograms at distances of 60 to 95 km and are studied using synthetic seismograms computed using generalized ray theory and waveform interpretation techniques. Later α_P/α_S ratios seen on the vertical component short-period data for many events imply source depths less than 2 km. The short-period P wave form contains the best depth estimator in the form of α_P so that depth can be estimated to within an uncertainty of about 1 km for most events. The interlocks cluster at less than 2 km depth and most aftershocks occur within 1 km of the surface. A few aftershocks occur as deep as 7 km. These results are consistent with a previous tectonically based study in which faulting was inferred to start near the surface at 1.5 km depth with rupture proceeding downward and not exceeding 6 km depth. These results coupled with previous stress studies in the Australian shield and models of crustal strength show that faulting in a cold shield area is a near-surface phenomenon and implies that most of the crust is too strong to be fractured. (Short-period phases, source depth, synthetic seismograms, Australian shieldivity.)
 J. Geophys. Res., B, Paper 7A5019

Space Plasma Physics

7811 Waves and Instabilities IONION AND ELECTRONION CROSS-FIELD INSTABILITIES NEAR THE LUMP WITHIN IRRADIATED
 S. Peter Gary (MS D438, Los Alamos National Laboratory, Los Alamos, NM 87545)
 Robert L. Tozer, Du Mingde

Instabilities near the lower hybrid frequency are investigated by numerically solving the linear Vlasov electrostatic dispersion equation. The configuration is that of two unmagnetized ion components, a less dense beam and a more dense core, streaming across a uniform magnetic field with magnetized electrons in the zero-current frame. All three components are considered to have similar temperatures. This paper studies the parametric dependences of maximum growth rate for three distinct modes: the electron/ion beam modified two stream instability, the electron/ion core modified two stream instability, and the ion/ion lower hybrid instability. In the electrostatic limit, the electron/ion instability has a lower threshold and larger growth rate than its electron/ion counterpart. For a most equal beam and core densities, our results also suggest that the electron/ion instability has a maximum growth rate that is equal to or greater than that of the ion/ion instability for all plasma beta values. Thus the electron/ion modified two stream instability is a likely candidate to account for the presence of lower hybrid fluctuations in the foot of the Earth's bow shock at low beta.

J. Geophys. Res., A, Paper 7A9111

7827 Numerical simulation studies MULTISCALE INTERCHANGE INSTABILITY I. NUMERICAL SIMULATIONS OF INTERMEDIATE-SCALE IRREGULARITIES
 R. Zargham and C. E. Saylor (School of Electrical Engineering and Laboratory of Plasma Studies, Cornell University, Ithaca, New York 14853)
 Numerical simulations of the generalized Rayleigh-Taylor instability (RTI) are presented. The model and simulations are applicable to both ion and tokamak spread F, unstable barium cloud dynamics, and collisional interchange instability in general. The principal result is that the evolution of the instability leads to an anisotropic state consisting of nearly decoupled (quasi-periodic) vortices along the effective electric field, and sheath-like structures propagating perpendicular to E_{eff} along the extremities of the quasi-periodic structures. The spectral properties of the nonlinear state are analyzed using non-dimensional power spectra calculated along spatial trajectories for selected angles to E_{eff} . In this way a direct comparison to *in situ* probe data can be made. The inherent anisotropy of the nonlinear state is reflected in major qualitative differences between the spectra taken parallel to and perpendicular to E_{eff} . The fundamental finding of the present work is that anisotropy in interchange dynamics is much greater than had been previously reported. This strong anisotropy can explain much of the spectral and spatial structural characteristics of both bottomside and topside spread F. In a companion paper, a comparison of the simulation results to various *in situ* data sets is given. (Numerical simulation studies, ionospheric irregularities, Turbulence, Plasma waves and instabilities)

J. Geophys. Res., A, Paper 7A9017

7851 Shuck waves ENERGETIC INTERPLANETARY SHOCKS, RADIO EMISSION AND CORONAL MASS EJECTIONS
 M. V. Cane (Laboratory for High Energy Astrophysics, NASA/GSFC, Greenbelt, MD 20771)
 M. R. Sheeley, Jr., R. A. Howard
 The interplanetary shocks which generate detectable low frequency (<1 MHz) radio emission, represent as a group, the most energetic shocks produced by the sun. For all interplanetary (IP) shocks which generated so-called IP type II events, we find, when observations were available, that the associated solar events involved fast (>500 km/sec) coronal mass ejections (CMEs). In comparison with the set of all CMEs detected by the Heliosid coronagraph, the CMEs associated with IP type II events are the most massive and energetic. The majority (>50%) belong to the structural classes described by the Heliosid researchers as "curved front" or "hale". Evidence presented suggests that these are the same class viewed from a different perspective. Our results are consistent with there being a close relationship between interplanetary shocks and fast CMEs. (Shock waves, radio emission, coronal mass ejections.)
 J. Geophys. Res., A, Paper 7A9014



Give AGU your new address!

Please print or type new address.

Please allow up to 6 weeks for change to be effected if mailed. Only one notice needed for AGU membership record and all AGU subscriptions, and Physics Today.

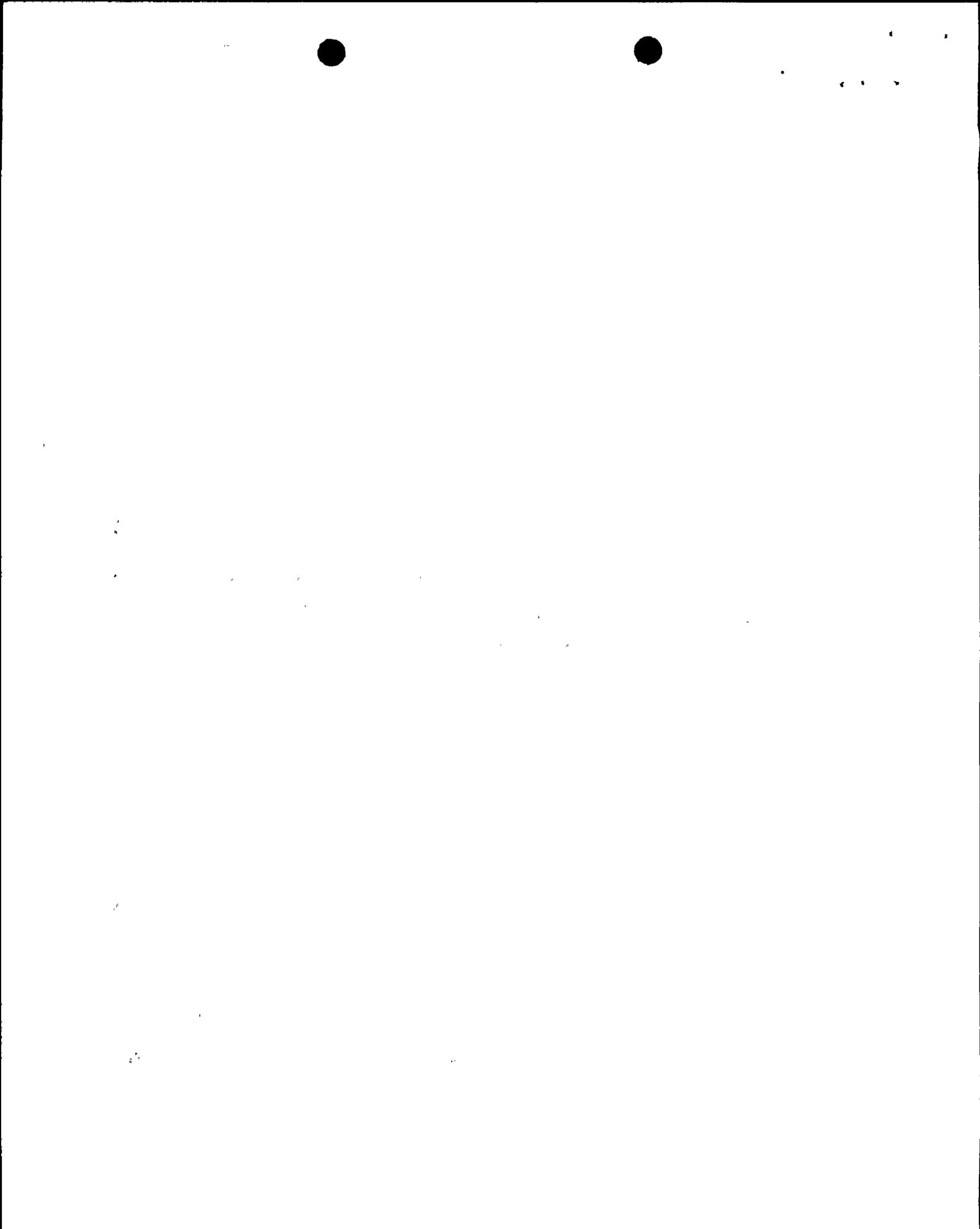
New phone numbers (will be published in Membership Directory)

Return this panel, with label to:

American Geophysical Union
 2000 Florida Avenue, N.W.,
 Washington, DC 20009

Office ()
 Home ()
Weekly Newspaper

1987



DEFORMATION ASSOCIATED WITH THE 1986 CHALFANT VALLEY EARTHQUAKE, EASTERN CALIFORNIA

BY W. K. GROSS AND J. C. SAVAGE

The observed deformation between January 1985 and July 1986 (postearthquake) of a precise trilateration network (Figure 1) in the epicentral area of the Chalfant Valley earthquake suggests that the earthquake involved 1.3 m right-slip and 0.7 m normal slip on a 15-km-long buried fault dipping $50^{\circ}\text{S}55^{\circ}\text{W}$. The inferred seismic moment is equivalent to a magnitude (M_s or M_c) 6.5 earthquake.

The 1986 Chalfant Valley earthquake sequence occurred within a trilateration network (Figure 1) that had been surveyed eight times between July 1972 and January 1985. The network was resurveyed in July 1986 following the main shock in the Chalfant Valley sequence. All measurements in the network were made with a Geodolite, and refractivity corrections were from end-point pressure measurements, and temperature and humidity profiles determined from an aircraft flying along the line at the time of ranging. The standard error in measuring the length of a 20-km-long line is about 5 mm. The details of the measurement procedures and the accuracy attained are given by Savage and Prescott (1973).

The measured line lengths for the 12 lines closest to the epicenter are shown as a function of time in Figure 2. Most of the lines show a significant change in length in the interval January 1985 to July 1986. We attribute those changes to the Chalfant Valley earthquake. Significant changes associated with the 1980 Mammoth Lakes earthquakes and the associated inflation of the Long Valley caldera (Savage and Lisowski, 1984) can also be identified in Figure 2. The 1982 to 1984 offset in the line Tungsten-Sherwin is presumably a coseismic effect of the 1984 Round Valley earthquake (Gross and Savage, 1985).

Lines west of the line Banner-Sherwin did not show appreciable changes in the 1985 to 1986 interval. This includes not only the five solid lines shown in Figure 1 but also an adjoining 35-line network to the west that includes the stations shown as unconnected triangles in Figure 1. In addition, the network shown by dotted lines in Figure 1 was also resurveyed in late July. It had last been surveyed in 1982. No appreciable changes in those line lengths were observed in the 1982 to 1986 interval.

The lines out of station Chalfant were measured on 22 July the day after the main shock in the Chalfant Valley sequence and again on 28 July. Both measurements are shown in Figure 2. As can be seen, no significant change in the lengths was detected in the one-week postearthquake interval.

The displacement of the trilateration stations in the interval January 1985 to July 1986 can be calculated from the observed changes in the line length in the same interval (Figure 3a). Because the trilateration measurements are internal to the network and not tied to an external frame of reference, the displacement field is arbitrary in the sense that any rigid body motion of the network as a whole may be added. We have resolved this ambiguity by selecting that displacement field which minimizes the displacements of the westernmost stations in the network (stations shown as solid triangles in Figure 3a) as well as some stations even farther west (stations shown as isolated open triangles in Figure 1). As long as the stations closest to the epicenter (Casa Diablo, Chalfant, and Falls) are excluded, the

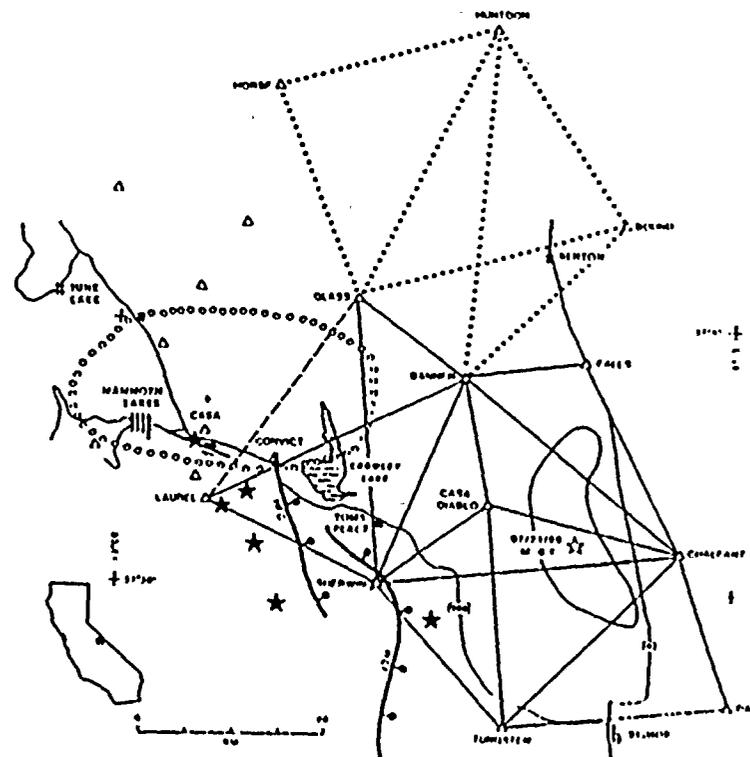


FIG. 1. Map of the trilateration network. The Chalfant Valley earthquake epicenter is shown by the open star north of Bishop. The approximate aftershock area is enclosed by the kidney-shaped contour. The triangles show trilateration stations included in the existing Geodolite networks. The solid stars show the epicenters of the previous principal shocks in the Mammoth Lakes sequence.

displacement solution is relatively insensitive to which stations are included in the set for which the rms displacement is minimized. The significant feature in Figure 3a is the magnitude of the displacements at Casa Diablo, Chalfant, and Falls.

We have constructed a dislocation model to explain the line-length changes observed in the interval 1985 to 1986. In this model, the earth is represented by an elastic half-space and the fault rupture by a rectangular dislocation loop (Mansinha and Smylie, 1971). The best-fit model was obtained by perturbing an initial trial solution that was suggested by the seismological data (aftershock distribution and preliminary focal-mechanism solution). The projection of the best-fit rupture upon the free surface is shown in Figure 3b. The fault dip is $50^{\circ}\text{S}55^{\circ}\text{W}$, with the rupture extending from a depth of 3 to 9 km. The along-strike dimension of the rupture is 15 km. The best-fit solution suggests 1.3 ± 0.1 right-slip and 0.7 ± 0.1 normal slip on this rupture surface. The residual displacements that remain after the coseismic



1 0 2

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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

101

102

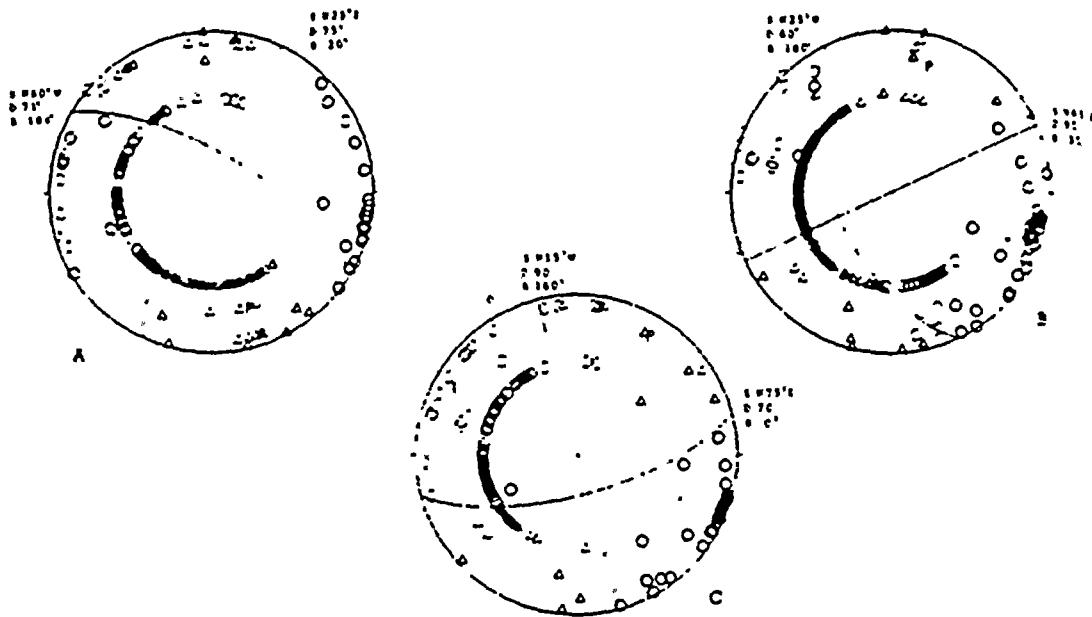


FIG. 6. *P*-wave first-motion plots (lower-hemisphere, equal-area projection) for the largest foreshock (A), the main shock (B), and the largest aftershock (C). Triangles and circles denote dilatation and compressional arrivals, respectively. *T* and *P* represent the calculated tension and pressure axes, respectively.

31 July at 0722 (UTC), an $M_L = 5.8$ aftershock occurred at the southeastern end of the seismic zone (Figure 4E) at $37^{\circ}35.5'N$, $118^{\circ}28.4'W$, and a depth of 9.2 km. This event appears to have had its own aftershock sequence that outlined a rupture zone to the north-northwest over a depth range of from 5 to 8 km along the main shock rupture zone. In the following 2 months, August and September, seismic activity continued along previously outlined trends throughout the seismic zone (Figure 4F), with two notable exceptions. First, an area 2.5 km north of the 31 July event became very active. Second, on 2 August, over a period of about 2 hr, a sequence of small earthquakes, the largest having a magnitude of 2.4, occurred at a location about 4 km north of Bishop (Figure 4F).

Focal mechanisms. *P*-wave first-motion fault plane solutions have been derived for three of the earthquakes larger than magnitude 5 and are shown in Figure 6. These fault plane solutions are especially well-constrained due to the dense station coverage near the epicenter and the fact that these earthquakes were sufficiently large to have had impulsive arrivals out to distances as great as 400 km. All three mechanisms show primarily strike-slip with a small component of normal slip. The preferred foreshock focal mechanism solution (Figure 6A) shows predominantly left-lateral strike-slip movement along a plane striking $N25^{\circ}E$ and dipping 75° to the northwest. In contrast, the preferred main shock focal mechanism solution (Figure 6B), which agrees with the distribution of the main shock aftershocks, strikes $N25^{\circ}W$ and dips southwest at 60° . The rake on this plane is -180° , indicating pure right-lateral strike-slip movement along the southwest dipping plane. The preferred plane for the 31 July aftershock strikes $N15^{\circ}W$, dips 90° , and has a rake of 160° (Figure 6C) that indicates almost pure right-lateral strike-slip movement.

Seismic and Aseismic Deformation Associated With the 1952 Kern County, California, Earthquake and Relationship to the Quaternary History of the White Wolf Fault

ROSS S. STEIN¹

*Department of Geology, Stanford University, Stanford, California 94305
U.S. Geological Survey, Menlo Park, California 94305*

WAYNE THATCHER

U.S. Geological Survey, Menlo Park, California 94305

Synthesis of geodetic, geologic, and seismic data from the White Wolf fault, California, indicates that the fault separates an area of late Quaternary and continuing rapid uplift in the Tehachapi Mountains and Transverse Ranges from even more rapid subsidence in the southern San Joaquin Valley. On July 21, 1952, rupture of the White Wolf fault produced the $M_L = 7.2$ Kern County earthquake. We used the aftershock zone to delimit the size of the faulted slip surface and applied constraints imposed by the known 1952-1953 horizontal shear strains to model the measured coseismic vertical displacements, with an elastic dislocation model. A curved fault trace with decreasing fault depth (27 to 10 km from the surface vertically to the base), slip (3 to 1 m), and dip (75° to 20°) from the 1952 epicenter at the southwest end of the fault toward the northeast provides the fit most consistent with the geodetic record, the measured seismic moment, the fault-plane solution, and the pattern of surface rupture. Two short relevelled lines near the 1952 epicenter tilted 4 and 17 μ rad down to the north from 5-10 years before the earthquake; the preseismic tilts differ significantly from ten other surveys of these lines. Left-lateral fault-crossing shear strain from 0.2-20 years before the quake was two times greater than both precismic off-fault strains and the post-seismic fault-crossing strains. During the first seven years after the earthquake, aseismic deformation was negligible. From 1959 to 1972 uplift reached 160 mm over an area larger than the aftershock zone, rising first in the epicentral region and then at the northeast end of the fault. This was unaccompanied by any surface fault slip. Reconstruction of the vertical separation on the White Wolf fault from late Quaternary and late Miocene stratigraphic marker beds shows that the rate of reverse fault slip increased forty-fold, from 0.1-0.2 mm/yr to 3-9 mm/yr, between the past 10-15 m.y. and the most recent 0.6-1.2 m.y. We estimate a 170- to 450-yr average recurrence interval for earthquakes on the White Wolf fault with slip equivalent to that in 1952. The 1952 earthquake appears to be characteristic of the Quaternary record of fault displacement in the increase in White Wolf slip toward the San Andreas fault, the ratio of reverse to lateral slip (1.3:1), and the ratio of vertical fault slip to emergence of the hanging wall block (3:1). The >8500-m-deep sedimentary basin on the down-thrown block cannot be explained by repeated slip of the White Wolf fault in an elastic medium.

TABLE 2. Model Fault Parameters

Fault Segment	1 (West)	2 (Central)	3 (East)	Net
Strike	N73°E	N58°E	N43°E	
Dip, deg	75	35	20	
Reverse slip, m	2.4 (2.9)	1.0	0.4	1.3 ----
Left-lat slip, m	2.0 (2.0)	2.0	1.0	1.7
Upper depth, km	5.0 (5.0)	3.5	2.0	
Low depth, km	27 (19)	15	10	
Moment, M_0 , dyne cm	5.7×10^{24}	3.3×10^{24}	1.6×10^{24}	1.1×10^{27}

The values shown in parentheses correspond to the 19 km deep fault model shown as a dashed curve in Figure 7. The dislocation program divides each segment into two planes with identical parameters except for strike, in order to smooth the strain field near segment junctions.

earthquake was calculated by Ben-Menahem [1977] to be 0.84×10^{27} dyne cm. Here $M_0 = \mu \sum uA$, where μ is the elastic modulus of rigidity, assumed to be 3×10^{11} dyne/cm², u is the slip magnitude, and A is the slip surface area. The geodetic model $M_0 = 1.0 \times 10^{27}$ conforming within observational error

depth and location of faulting and may not accurately record the buried slip and its variation along the fault strike.

Summary

Decreasing fault depth, slip, and dip away from the 1952



REPLY
REFER TO

United States Department of the Interior

GEOLOGICAL SURVEY
BOX 25046 M.S. 905
DENVER FEDERAL CENTER
DENVER, COLORADO 80225

August 5, 1987

Robert Rothman
NRC-Phillips Building Complex
7920 Norfolk Avenue
Bethesda, MD 20814

Dear Bob:

Enclosed is a review of the PG&E progress report of June 24, 1987 entitled *Empirical Ground Motions Investigations of PG&E Diablo Canyon Power Plant Long Term Seismic Program*. I have limited my comments to a review of this document since PG&Es empirical ground motion presentation at the July 15-16 workshop was confined to this report. All my comments expressed during the caucus after the workshop are included in the review. In addition, there are many more comments that relate specifically to the details of the analysis that have arisen as a direct result of my review of the document.

My comments are quite specific, many of them relating to a lack of documentation for many of the conclusions drawn in the report. I have also asked several clarification questions throughout the review. If these questions are answered and the report is modified to include the proper documentation, I will have an excellent basis from which to express my opinion regarding their empirical ground-motion studies.

I will be on annual leave from August 7-23. If you have any questions, I will be in the office on the 24th.

Sincerely yours,

Kenneth W. Campbell



1 1 2

1 1 2

REVIEW OF JUNE 24, 1987 PROGRESS REPORT
"EMPIRICAL GROUND MOTIONS INVESTIGATIONS FOR PG&E
DIABLO CANYON POWER PLANT LONG TERM SEISMIC PROGRAM"

Page I-1. There is no mention of vertical ground motions or peak velocities. Are these parameters going to be addressed as part of your study?

Page I-2. Site-specific response spectra are presented throughout the report as an 84th percentile estimate over a narrow period band anchored to a median PGA. Spectra should be analyzed and presented on a period by period basis using a consistent percentile throughout the entire period range of interest. The practice of anchoring a narrow-band spectral average to a median PGA results in a site-specific response spectra that has an inconsistent level of uncertainty throughout the period range of interest.

Page II-1. There is no mention of the shear-wave velocity criterion for defining a site as rock-like. At the workshop, you mentioned that a site was considered rock-like if the surface shear-wave velocity was 2000 fps or larger. Any criteria used to select sites for the PG&E rock site data base should be explicitly stated in the report.

Was the shear-wave velocity criterion consistently applied for all sites? It appears from Table II-1 that there are sites that do not meet this criterion. Especially suspect are those sites composed of Pliocene sediments. Also suspect are the Tabas and Gazli sites. The literature I have reviewed suggests that these are soil sites. What is your basis for classifying these sites as rock-like?

Also omitted from the report is your 3-meter limit of soil depth mentioned at the workshop. There are a few sites listed in Table II-1 that exceed this limit. Was this criterion consistently applied? I question the inclusion of sites having even a few meters of soil as rock sites, although, my results would suggest that their inclusion will have a tendency to conservatively increase estimates of ground motion at high frequencies.

Later in the report you state that your spectral analyses were done for frequencies in the range 0.5-25 Hz (0.04-2 sec). For site-specific spectra, you stated an interest in frequencies as high as 33 Hz. You should be aware that many of the response spectra used in your studies are not valid over this entire frequency range. Most spectra computed since 1971 have been lowpass filtered at 23 Hz, substantially reducing spectral ordinates at frequencies higher than this threshold. Even 1971 and older spectra have been filtered at 25 Hz. The spectra have been highpass filtered as well. Although the standard Caltech filter was



1 1 2

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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

15 sec, Trifunac has shown that many of these spectra are dominated by noise at periods substantially smaller than this. Most spectra computed since 1971 have a variable highpass filter that can be as high as 1 Hz. I would strongly recommend that you only use that portion of the spectra that is unaffected by the filtering process or not dominated by noise (as determined by Trifunac) for your analyses. The spectral values outside this band are totally unreliable.

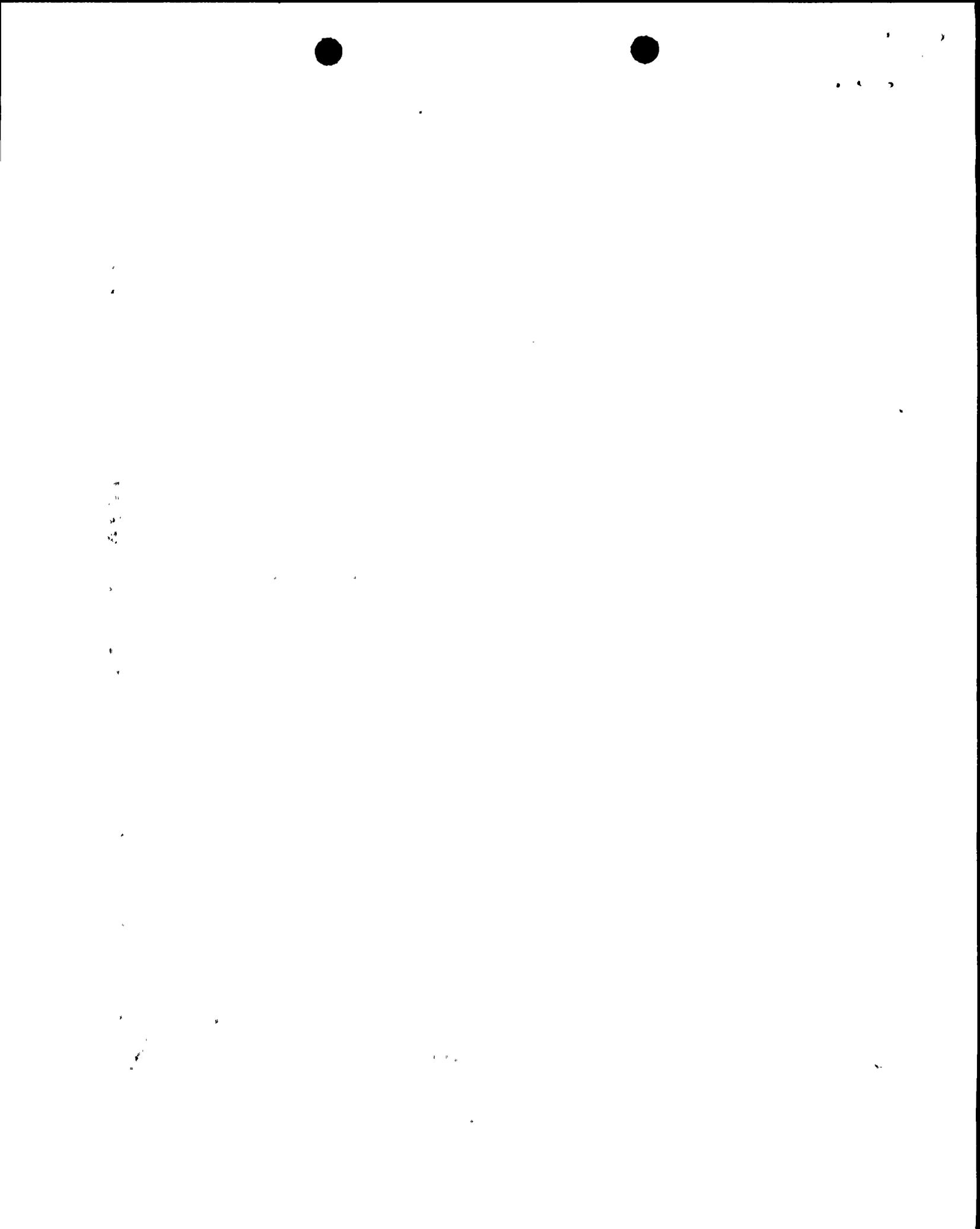
All parameters in the attenuation relationships should be explicitly defined. For example, did you use the mean of the two horizontal components to define PGA and response spectral values, or did you use both components? How was closest distance to the fault rupture defined and computed?

Page II-2. I object to the use of hypocentral distance for some of the "smaller" earthquakes when fault distance is unknown. Which of the earthquakes used in your analyses did not have fault distances available? Even earthquakes of $M=5.0-5.9$ have fault rupture zones that are a significant fraction or even larger than the distance of concern in this program ($R=4.5$). Furthermore, hypocentral distance is traditionally a very uncertain parameter because of the large uncertainty associated with the computation of focal depth.

Table II-2. Why is the 1978 Santa Barbara earthquake excluded? There were rock sites that recorded this earthquake. Its exclusion suggests that you have not explicitly stated all your criteria for selecting a recording for your data base. These criteria should be clearly and concisely stated.

Page III-1. I disagree with the use of "modified" soil recordings for the site-specific analyses. I acknowledge that there are very few recordings that meet your criteria for selecting such records, but one should not compromise the consistency of the data base because of this paucity. Site response is highly site specific, making generic corrections to specific recordings subject to great uncertainty. I would rather see you search for other rock recordings that meet your criteria, or modify your selection criteria to include more rock recordings. The lack of rock recordings at very close distances to $M=7.0-7.5$ earthquakes is a fact we have to live with. If there are insufficient rock data to do a site-specific analysis, then you will simply have to rely on the results of the regression analysis to estimate ground motion for the project.

Page III-2. Here and elsewhere I would recommend that you present all of your results separately for strike-slip and reverse/thrust fault mechanisms. If they are combined



with "equal probability" it is difficult to see the difference in the two results.

Page III-3. I feel that undue emphasis is being placed on the mean spectral value for the 3.0-8.5 frequency band. The entire frequency band of interest--stated later to be 0.5-25 Hz for the regression results and as high as 33 Hz for the site-specific results--should be treated with equal emphasis. Averages over specific frequency bands should not be used to characterize either site-specific spectra or spectra derived from regression analyses. I believe NRC would like to get away from the idea of a "spectral shape."

Page III-4. Since NRC may not accept your tentative selection of $M=7.0$ as the SSE for the site, I would suggest that you do an alternate site-specific analysis, if possible, for a $M=7.5$ event.

Page III-5. After reviewing the literature, the empirical results summarized in Appendix B, and my own empirical results, I feel that the factor of 1.2 used to convert between strike-slip and reverse/thrust fault mechanisms is too low.

Why are all of your spectral results presented in terms of pseudo-absolute acceleration, S_a ? It is more common to display response spectra in terms of pseudo-relative velocity (PSRV). Results presented in terms of PSRV would be more easily understood and compared with the results of others.

Table III-1. Why were the 1985 Mexican and Chilean earthquakes excluded from your site-specific study? There is only one earthquake that has a magnitude greater than your target magnitude. These events, if they were to fit your criteria, would add several more recordings at magnitudes exceeding your $M=7.0$ design event. In addition, if you were to do an analysis for a $M=7.5$ earthquake, these two events would be the only two that have magnitudes above this value.

Table III-4. Why not include the extensive work of Bernreuter and others at LLNL in addition to those listed in this table? They have developed frequency dependent site-correction factors for use in their seismic hazard characterization of the Eastern United States project.

I am unable to find adequate documentation of the Seed and Schnable (1980) reference. There is insufficient documentation in the stated reference. Could you provide me with a copy of their paper so that I may review the basis for their results?



v l d

Table III-6. As stated previously, I feel that the adopted factor of 1.2 is too low considering values available in the literature and the results of both your and my empirical analyses.

Figures III-6 and III-7. Were the various equations plotted in these figures used with the appropriate distance measure? If so, how were the various measures related to one another? Which measure was used to plot against? If not, these results can be quite misleading and your conclusions based on them are subject to considerable uncertainty.

Figure III-8. Trifunac and others and Bernreuter and others also have relationships that can be used to define spectral ratios between soil and rock. However, as stated earlier, I do not believe one should modify soil recordings to rock for the purposes of estimating site-specific spectra.

Page IV-1. You allude to supplemental analyses of soil site recordings from previous and current studies to help constrain some the parameters in the attenuation relationship. If so, these results should be presented or adequately referenced so one can independently review the basis for these constraints.

All parameters in Equations (1) and (2) should be explicitly and precisely defined.

You should present the results of all the statistical analyses performed in this section. A full documentation of these results is necessary if one is to have sufficient information with which to review the analyses. This includes the analyses used to establish the "constraints" on coefficients C_3 and $C(M)$.

Why was the analyses restricted to $M > 6.5$? This seems extremely restrictive for a regression analysis, especially when already constrained to a very limited rock and rock-like data base. How many earthquakes and recordings were used for this analysis? You should tabulate these data so one can judge their adequacy.

Was $C(M)$ determined from the multiple regression analysis based on Equation (2)? If not, what was the basis for determining this extremely important parameter?

Page IV-2. It appears from the discussion here that you further restricted your regression analyses to reverse/thrust events. If this is so, it appears that you have severely restricted your data to include only $M > 6.5$ reverse/thrust earthquakes with recordings on rock or rock-like sites. You should show the results of all analyses and



... >

...

...

explicitly tabulate each of the data bases used to perform these analyses so one can see the empirical basis of your attenuation relationships.

As stated previously, I do not believe that all of the spectra used in your analyses are valid over the entire frequency range of interest.

The analysis of S_a using the ratio S_a/a is again based on the concept of a "spectral shape." The estimation of S_a from S_a/a and a has much greater uncertainty associated with it than the estimation of S_a directly, if one properly computes the uncertainty as confidence limits. This is because you are combining two random variables to compute a third, which must necessarily result in larger uncertainty unless the two variables being combined are perfectly correlated.

The above discussion brings up a topic that is often neglected in the literature. That is, the uncertainty in a regression analysis is commonly reported in terms of the standard error of estimate. This, however, is not the only uncertainty that should be considered when making "predictions" from such a relationship. The uncertainty in a predicted value is also dependent on the uncertainty associated with the mean estimate. This latter uncertainty can be extremely large when the estimate is based on an extrapolation of the model. Therefore, the appropriate quantification of uncertainty in a predicted value is by means of confidence limits, which includes both types of uncertainty. Use of the standard error to quantify uncertainty in a predicted value can lead to a substantial underestimate of the true uncertainty.

You should tabulate the regression results for each period analyzed.

Was the $C_2'(8.5-M)^n$ and $C(M)$ terms empirically determined? If so, you should give the empirical results so one can judge the significance of these results. If not, what was the basis for determining these terms?

Page IV-3. I would expect there to be significant differences in attenuation with distance for S_a as a function of frequency, especially beyond 10-20 km. Your setting $C_3'=0$ violates this. Even though the "distance of engineering interest" maybe less than 30-40 km, your attenuation relationships are being determined from data recorded at distances as great as 300 km. Therefore, it is important that any difference in attenuation as a function of frequency be included if so required by these data.

Was the same functional form used to estimate S_a used to estimate the average S_a over the 3.0-8.5 Hz frequency band?

Was the spectral analysis restricted to $M > 6.5$ reverse/thrust events as was the PGA analysis? If so, this seems too restrictive for a multiple regression analysis. You need to specify the exact data base used for these analyses.

Page IV-4. You specify throughout the report that estimates are for equal weighting of reverse/thrust and strike-slip mechanisms. I would prefer to see the estimates and plots presented separately for each type of faulting. Were the strike-slip estimates estimated from the reverse/thrust estimates using the factor 1.2?

Figure IV-1. In this and subsequent figures, the captions refer to the results as those from the reverse/thrust attenuation relationships. Yet, data from strike-slip events are superimposed on the curves. This is extremely confusing. If the regression analyses were performed on the reverse/thrust data only, then only these data should be plotted. If you want to show how well the predictions agree with the strike-slip data, then you should present these as separate plots. The plotting of data not used in the analyses gives the impression that much more data was used to establish the relationships than was actually used.

Figure IV-17. The regression results plotted as a function of frequency on this and previous figures are extremely smooth. How was this smoothing obtained? By smoothing, you have modified the original regression results. I would prefer to see the unsmoothed spectra plotted in these figures. If you would like to show smoothed spectra, then they should be shown in addition to the unsmoothed values. If smoothed spectra are used to provide estimates of spectral ordinates, then your smoothing scheme needs to be fully documented.

References. Throughout the paper, two studies are used and referenced for which there is no adequate documentation. These are Sadigh and others (1986) and Seed and Schnable (1980). References to these studies are in the form of an abstract and a summary in a second publication, respectively. Unless sufficient documentation is available for these studies in the form of a report or journal article, I would strongly suggest that they not be used. If reports are available, I would like to have a copy to review. If reports are not available, how can you defend the use of results that are not documented?



v c 7

Appendix B. Although you present the algorithms for performing weighted nonlinear regression analyses, you never state whether you used a weighted analysis in the regression analyses discussed earlier in the report. Were weights used in your analyses? If so, what was the algorithm for computing them and what is the basis for the algorithm?

Approximate confidence intervals can be very poor estimates of the actual confidence intervals for coefficients having highly nonlinear distributions (such is the case for the c_1 and c_2 coefficients in the function $C(M)=c_1 \exp(c_2)$). In this case, Monte Carlo techniques must be used to establish the actual nonlinear distributions (see Gallant, 1975 in your reference list).

Appendix C. You should present the results of the regression analyses discussed in this appendix.

Your regression results using all the data, probably the most statistically robust analysis, results in a fault-type term of $C_6=0.32$, or a 38% higher PGA for reverse/thrust events as compared to strike-slip events. Yet, you have adopted a factor of 1.2 to relate these two fault types throughout the report. As stated previously, I feel that your factor of 1.2 is too low, considering the results in the literature, your empirical results, and my own empirical results.

The smaller difference in fault type seen for the larger earthquakes at larger distances may simply be a result of different attenuation rates for the two types of events. Since you are primarily concerned with near-source ground motion ($R=4.5$), it would seem more appropriate to emphasize the near-source results, and, hence, the larger factor for fault type.

If either the rock or soil site data were restricted to distances of 50 km or less, how would your results change. Would fault type become more significant? How would other coefficients in the relationship and predicted values change? Such an analysis should be done for all the studies presented in this report to insure that the use of far-source data has not unduly biased your near-source estimates.

DEPARTMENT OF CIVIL ENGINEERING
THE CITY COLLEGE OF THE
CITY UNIVERSITY OF NEW YORK
NEW YORK, NEW YORK 10031
212-690-4228

4 October, 1987

Dr. Morris Reich
Head, Structural Analysis Division
Department of Nuclear Energy
Brookhaven National Laboratory
Upton, Long Island, New York 11973

Re: Comments on Meeting of 15-16 July, 1987 with
Ground Motion Panel on Long Term Seismic Program
for the Diablo Canyon Nuclear Power Plant

Dear Dr. Reich:

This letter report presents a summary of my comments on the presentations made by the DCLTSP Project Team at the subject meeting. The meeting focused primarily on the current status of the numerical ground motion studies, with only a rather short summary presentation made of the state of the empirical ground motion developments. The meeting was extremely worthwhile for me to attend, however, to obtain information on the ground motion calculations which will eventually be used as input to both the fragility and the SSI tasks. Prior to the meeting, I received a copy of preliminary reports describing the detailed activities being conducted in the ground motion area. These reports were extremely helpful to me in preparing for the meeting and in reviewing after the meeting so as to be able to prepare this summary document. This procedure should be followed for all workshops.

Comments on Empirical Ground Motion Studies

Referring first to the empirical ground motion study, the program presented by the Project Team appears to be reasonable for such a complex problem, as I have stated previously. Currently, 157 sets of three component records obtained at distances within 300 km are available from 55 earthquakes. From this data base, a reduced set of 18 strong motion records



1 5 2

1
2
3
4

5
6
7

8

9

10

11

12

13

14

were selected to perform site-specific statistical analyses, these records being chosen based on magnitude, distance, site condition and fault type characteristics. Attempts were then made to suitably scale these recordings to account for differences between conditions at the recorded site and the DC site.

Due to the extensive scatter in the available data base, the recommendation was made at the meeting that the target PGA should be based on the 84th percentile of the data base, as is current practice, and not the 50th percentile as recommended by the Project Team. I have several additional questions on some details of this program. First, the reduced data base which is being recommended for input to both the structures and PRA programs is strongly influenced by the judgement that either the strike/slip or reverse/slip fault mechanism is predominant. No specific consideration, however, seems to be given to the impact of other assumed fault mechanisms on characteristics of the potential ground motions which may be sustained at the site (frequency content, as well as PGA). If it can be shown, for example, that fault mechanism does indeed have an impact on frequency content of the ground motion, this fact would play a role in the judgements to be made during the PRA study, no matter how remote the possibility of the particular fault occurring.

Secondly, I have some concern of the impact of the assumed spectral amplification in the 3 to 6.5 hz range on the ground motion program. This assumed amplification is not based on any DC site characteristics, but rather on some subjective judgements which were made early in the LTSP program. Statements have continually been made at the various ground motion discussions that this is the primary frequency range of interest to the SSI and fragility programs. This is in fact not the case, with the important frequency range probably more like 2 to 20 hz. A side concern associated with this artificial scaling in this frequency range is that actual accelerograms will not be able to be used directly in the structural response program. Rather, artificial motion histories will have to be generated to match these scaled spectra. No specific discussion of this problem has yet been presented.

Comments on Numerical Ground Motion Studies

Relatively detailed presentations were made on the various simulations being used in the numerical modeling area. The objective of this phase of the

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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

ground motion program is to provide additional information to the ground motion study that cannot be generated from the empirical program alone. Impact of site specific fault behavior and geologic configuration can potentially be studied to determine the impact of DC site parameters on seismic motions. As I have stated previously, this effort is impressive and, from an academic point of view, extremely interesting. The approaches being contemplated are extremely complex and at the forefront of the current state of the art.

However, it appears to me that a significant effort must still be expended to verify the adequacy of these computations, particularly in the frequency range of interest to the SSI and fragility studies. This verification becomes more important since the current plans outlined at the meeting indicate that of the total number of ground motion suites to be input to the fragility program, 12 will be taken from the empirical program and 14 from the numerical program. Thus the results from the numerical modeling study will now have a significant impact on all of the structural response calculations. Questions of the adequacy of the computations must therefore be addressed.

I will try to summarize some of my specific concerns with this program. I should mention that the following discussion is restricted to the numerical modeling study only. Thus the term "empirical" and "semiempirical" mentioned below refer not to the empirical ground motion study, but rather to the numerical methods being used in the calculations.

1. It is stated in Enclosure B of the handouts that the empirical Green's function approach, which was described at previous ground motion workshops, is limited by the necessity for using strong motion recordings from the Imperial Valley site to represent wave propagation characteristics at the DC site. In addition, the restricted number of recordings available from the Imperial Valley site further limits the applicability of the method. The implication of these statements, of course, is that the method should not be used to generate site-specific data for DC.
2. As a result of item 1, the Project Team has moved to an improved method, the semiempirical single source method. Again, this approach must be fit to the data at another site (Imperial Valley) before application can be made to the DC site. This method relies upon significant scaling assumptions to allow matching of the computed data with the measured data for the large

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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

101

event at Imperial Valley, with these scaling functions being to a significant extent intuitive in nature. In fact, although the method relies upon highly complex analytic solutions to the wave propagation problem, the scaling procedures used can to some extent be considered curve fitting. Thus, the applicability of the results to the DC site is again highly questionable.

3. If one compares Figures 4.16, 4.17 and 4.18 (horizontal motion) and Figures 4.20, 4.21 and 4.22 (vertical motion) of Enclosure B of the handouts, one can note significant differences between the results of the calculations with both the empirical and semiempirical methods as compared with the measured site data at Imperial Valley. This is especially true for the vertical recordings. Although the comparisons may be considered acceptable from a qualitative point-of-view, they certainly are not from an engineering perspective, where structural responses (loads and stresses) are directly proportional to acceleration levels and not the log of these levels. Differences of the order of two in acceleration are significant.

Since the methods are essentially being curve fit to the Imperial Valley data, discrepancies between measured and computed data are a direct indication of shortcomings of the methods of prediction. Differences at the DC site may in fact be even greater than those at the IV site. The Project Team apparently is also concerned since they have now introduced a third approach, the multiple source semiempirical method, to further improve the predictions. These again must be fit to the Imperial Valley data. Therefore, although the methods are based on complex wave propagation methods of analysis, significant efforts should be made to improve the confidence level in the computed responses.

4. The various computational methods presented are based on the generalized ray method of analysis. The Q structure model used to describe damping in the rock system is shown in Table 1 of Enclosure C to be variable with depth, with the P-wave damping taken as half the S-wave value throughout the system. A comment was made at the presentation that in fact the Q structure used for the computations was not variable with depth but rather was constant. No justification was presented anywhere in the presentation for these assumed damping values, nor were comparisons with laboratory damping data mentioned.

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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

Of more importance, no data was presented anywhere to indicate the sensitivity of the computed acceleration responses to the assumed Q-structure. Offhand, one would expect that the surface data would have significant impact on such calculations. In Enclosure C, it was further indicated that an average Futterman Q operator was assumed to simplify the time domain integrations in the generalized ray computations. The implication of this assumption, particularly in the low Q near-surface region, was not discussed. Thus the entire treatment of material damping, which would appear to have major impact in the frequency range of interest to the structures at DC, is neglected in the Project Team's presentations.

5. The generalized ray methods presented in Enclosure C consider only body waves (P and S waves) in the computations. These calculations indicate reasonable correspondence of primary arrival times with the more exact wave number solutions, as well as other general characteristics of the response. Rayleigh wave effects were shown to be small for the case of a fault asperity at a depth of 9.5 km. However, for the case of a fault structure which extends close to and even to the ground surface (apparently a reasonable possibility at the DC site), Rayleigh wave and other layer interface effects may be significant but cannot be included in the ray approaches.
6. The Project Team indicates that the frequency dependent scaling above 2 hz, used in the multiple source approach, is extremely difficult to quantify and in fact may lead to errors of the order of three or more. The multiple source functions are therefore corrected by essentially curve fitting the computed to measured responses at individual stations. The application of this procedure to any other location besides the site where the responses are measured is therefore highly questionable.
7. In Section 5 of Enclosure B, comparisons are made between computed and measured accelerograms (Figure 5.9). Again, although such comparisons may appear qualitatively appealing, they certainly have no engineering significance in themselves. Variations in peak g's (a more important engineering parameter) with range in Figure 5.10 indicate significant differences. The comment that the "simulations agree well with observations" is in my opinion not appropriate. Differences of the order of 2 or more in spectral acceleration in the 2-20 hz range are important (see, for example, Figures 5.11 to 5.34).

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
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100

Comments on Coherence Approaches

The approach proposed to treat the impact of high frequency incoherence on site response appears to be a reasonable one. Again, I have several specific questions about the proposed program.

1. The incoherence models are to a great extent being calibrated from the small event that occurred offshore, directly opposite the site. No discussion has yet been presented to indicate that incoherence effects for such small events will be the same as those associated with the larger events of interest to this program.
2. Judgements made about source contributions to the incoherence model which are based on the Imperial Valley simulations must be considered highly suspect, since these calculations may have significant numerical errors at the higher frequency range, where incoherence effects are most important.
3. The data from the offshore event mentioned above, as well as the site data developed from the air gun experiments being conducted, are influenced by the fact that the power plant structures are now in place. The assumption is made that these structures do not have an effect on the measured site responses. It is not clear to me that SSI effects are not impacting the coherence calculations, with the effective result of double counting these effects in the structures/fragility studies.

Comments on Proposed Utilization of Ground Motions

In Enclosure D, the Project Team presented results for fourteen different simulations, and recommended model 9 as a typical ground motion which could be used in the deterministic SSI study. Since any of the computed ground motion records could be expected to occur with equal probability at the DC site, the Team should justify this selection. It appears that model 6 of Table 3.2, with a peak ZPA of 1.05 g's, would lead to a more critical structural response than model 9, which has a peak ZPA of 0.75 g's.



11 11
11 11

Summary

In summary, therefore, it seems to me that more work is required by the Project Team to verify that the numerical results being generated by the various generalized ray methods are reasonably accurate, particularly at the frequency ranges of interest for the DC structures. This is particularly important since the current plan calls for a major dependence on these numerically generated accelerograms as input to the fragility studies. Sensitivity of these results to reasonable variations in the modeling parameters must therefore be determined.

In addition, the frequency range of 3 to 8.5 hz may be an important factor in arriving at site specific ground motions. This range appears to be relatively arbitrary, and not associated specifically with the DC site. As mentioned previously, a direct result of the scaling associated with this frequency range is a loss of the specific time histories associated with the spectra being developed in the empirical program.

In closing, I would like to reiterate that attendance at the Ground Motion Panel meeting was extremely valuable to me in being able to judge the adequacy of the information being supplied as input to the SSI program. Such information will obviously play an important role in evaluating the completeness of the SSI calculations currently being performed. I would also like to take this opportunity to thank the members of the Project Team for the effort they made in their presentations.

Respectfully submitted,



Carl J. Constantino

Professor of

Civil Engineering

101 2 2
101 2