UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

In the matter of:

PACIFIC GAS & ELECTRIC COMPANY

(Diablo Canyon Nuclear Power Plant)

Docket No. 50-275 OL
50-323 OL

Location: Avila Beach, Calif. Pages: D-1154 - D-1441

Date: Monday, November 7, 1983

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING APPEAL BOARD

In the Matter of:		
PACIFIC GAS & ELECTRIC COMPANY	:	Docket Nos. 50-275 OL 50-323 OL
(Diablo Canyon Nuclear Power Plant)	:	

Bay View Room
San Luis Bay Inn
Avila Road at Avila Beach
Avila Beach, California

Monday, November 7, 1983

The hearing in the above-entitled matter was convened, pursuant to notice, at 9:00 a.m.

BEFORE:

THOMAS S. MOORE,
Chairman, Atomic Safety & Licensing Appeal Board

JOHN H. BUCK,
Member, Atomic Safety & Licensing Appeal Board

W. REED JOHNSON,
Member, Atomic Safety & Licensing Appeal Board

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APPEARANCES:

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I N D E X

WITNESSES:	DIRECT	CROSS	REDIRECT	BOARD	RECROSS
DR. STANLEY KAPLAN) RICHARD C. ANDERSON)	D-1160	D-1162	D-1415	D-1420	D-1425

EXHIBITS:	IDENTIFICATION	EVIDENCE
Governor's Exhibit:		
No. 45 (Article on Quantitative Definition of Risk by Kaplan and Garrick)	D-1335	--
No. 46 (Safety Goals & Related Questions by Kaplan)	D-1337	--

INSERTS:	Page
Testimony of PG&E Panel 6, and Professional Qualifications of Dr. Stanley Kaplan	D-1161

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P R O C E E D I N G S

JUDGE MOORE: Come to order, please.

Mr. Norton, are you ready to proceed?

MR. NORTON: Yes. We would like to call the sixth panel at this time, Dr. Kaplan and Mr. Anderson.

Your Honor, while they are being seated, there are some typographical type corrections to the testimony that I would like to make, and perhaps we should do that first.

If everyone will get their testimony out, I will give those changes, and then I will ask the witnesses if they are correct as changed.

May we proceed with that, Your Honor?

JUDGE MOORE: Proceed, Mr. Norton.

MR. NORTON: Page 12, Line 5, the fifth word, which is "for" should be "to," T O .

Page 22, Line 4, the fourth word, the letter "a" should be the word "the," T H E.

Page 25, Line -- actually it's 9½, the word "system" should be plural, "systems."

JUDGE MOORE: Mr. Norton, for my benefit, would you repeat the one on Page 22, please?

MR. NORTON: Certainly.

Line 4, the fourth word in, the letter "a" should be the word "the."

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JUDGE MOORE: Thank you.

MR: NORTON: Page 25, it's actually Line 9½, the word "system" should be plural, "systems."

Page 30, the Figure 3-4, the last two words should be deleted, "and disciplines." That should just be deleted.

Page 36, Line 9, fourth word, it should be "checked" with an E D, instead of "check."

Page 37, which is the next page -- this is a liitle bit awkward to describe, but if you look across the top, you have got nine columns, take Column No. 9, and then vertically you've got three columns, take Column 3, and move over four columns to Column 9, the two lines on the outside should be deleted. There should not be a box there at 9-3. Those lines should just be deleted.

It would, therefore, look like, on Page 30, if you go back to Page 30 and look at the diagram there, you will see that that box is deleted. That 9-3 box or the 3-9 box, whichever way you want to refer to it.

JUDGE JOHNSON; You are, in essence, saying there is no instrumentation and control component of desigh for System 9; is that correct?

MR. NORTON: Yes.

All right. Now moving to Page 39, Line 24, there was a word dropped out at the very end of the

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sentence, the word "for," F O R.

Line 24, Page 39, the last word of the line was dropped out. It should be the word "for," F O R.

MR. STRUMWASSER: "Checked for horizontally"?

MR NORTON: Right.

JUDGE JOHNSON: I don't think that makes sense.

MR. NORTON: Excuse me?

MR. STRUMWASSER: It makes sense. It just doesn't make elegance.

MR. NORTON: Dr. Kapalan, that is the change you have given me?

MR. KAPLAN: That's right. It makes sense to me.

MR. NORTON: The next page is Page 46, Line 4, "lamda electric power," the word "electric" should be deleted, and in its place should be "4KV," the number four, K V.

Page 57, Line 19, first word bis "that." It should be "their," T H E I R, and -- I'm sorry -- I skipped one page -- 54, Line 6. The Figure is 4-1; it should be 3-2. Page 54, Line 6, it says "Figure 4-1," and it should read "3-2."

And then finally Page 66, the same change on the lower right-hand box. In other words, 3-9 or 9-3, however you read it, those two lines should be deleted, the samae as back on Page 37.

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Those are all the corrections I have.

At this time, I would like to have Dr. Kaplan introduce himself.

Dr. Kaplan, your name and occupation, please?

MR. KAPALAN: My name is Stanley Kaplan, and I am a consultant.

Whereupon,

STANLEY KAPLAN

was called as a witness and, having been first duly sworn, was examined and testified as follows:

Whereupon,

RICHARD C. ANDERSON

resumed the stand and, having been previously duly sworn, was examined and testified further as follows:

DIRECT EXAMINATION

BY MR. NORTON:

Q Gentlemen, do you swear or affirm that the testimony I just named as submitted by you gentlemen is true and correct, to the best of your information and belief?

A (Witness Kapalan) Yes.

A (Witness Anderson) Yes.

MR. NORTON: At this time, your Honor, I move that the testimony be inserted in the record as though read, along with the professional qualifications of Dr. Kaplan,

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which have previously been submitted.

JUDGE MOORE: So ordered.

(The written testimony of Pacific Gas & Electric Company Panel No. 6 and the professional qualifications of Dr. Stanley Kaplan follow.)

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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

Before The Atomic Safety And Licensing Appeal Board

In the Matter of Pacific Gas and Electric Company (Diablo Canyon Nuclear Power Plant, Units No. 1 and 2)	Docket Nos. 50-275 O.L. 50-323 O.L. Reopened Hearing - Design Quality Assurance

TESTIMONY OF PACIFIC GAS AND ELECTRIC COMPANY
PANEL NO. 6
ADDRESSING CONTENTIONS 1(c) AND 2(c)

INTRODUCTION

Q 1 State your name and affiliation please.

A 1 My name is Stanley Kaplan. I am an associate consultant to the firm of Pickard, Lowe and Garrick, Inc. (PLG).

Q 2 What is the purpose of your testimony?

A 2 I have been asked if I could shed some light on the the question of the appropriateness of the use of sampling statistics in Phase II of the Independent Design Verification Program (IDVP). My purpose is to do that.

Q 3 What credentials do you bring to this task?

A 3 I am an engineer and applied mathematician with a number of years experience in nuclear power. For about the past 10 years, I have specialized in what I

call the "science of uncertainty"; i.e., in questions of risk, probability, decision, etc. I have been particularly interested in questions of "inference"; that is, in questions of what conclusions can we infer, and with what degree of confidence can we infer them, from a given defined body of information and data, which might include, for example, measurements on a sample taken from a population. I have been a major contributor to the methodology for probabilistic risk assessment (PRA) developed by PLG and used in the Zion, Indian Point, Midland, Seabrook, and other PRA studies. These studies are generally considered to be major advances in the state of the art. In particular, I was primarily responsible for the matrix theory of event trees, the discrete probability distribution (DPD) method for probabilistic calculations, the seismic risk assembly methodology, the "Level 2" risk diagram concept, the cause table idea, and the "Two-Stage Bayesian" approach to data interpretation.

Q 4 Could you summarize your understanding of the debate about the appropriateness of the use of statistics in Phase II?

A 4 Yes. My understanding is that in setting up guidelines for the verification program the Commission requested that there be "consideration of statistics

in the sampling plan." In the IDVP Management Plan, a commitment was included that TES would consider "any appropriate use of statistical methods which may augment the program." Subsequently, the NRC Staff presented the finding that "rigorous statistical techniques are largely inappropriate for a design verification program." The IDVP engineers agreed with this position. Mr. Hubbard and the joint intervenors disagreed and have called for "statistically valid sampling techniques" including "confidence level," "statistical basis for the sample size," etc.

Thus, basically, the IDVP and Staff have taken the position that: "It is not appropriate." Hubbard and intervenors have said, in effect: "Yes it is," and there the discussion seems to stand, as I see it. Upon reading these statements, I had the thought that I might be able to produce at least some clarity with respect to these disparate positions, and possibly even some resolution and reconciliation.

Q 5 Can you give us a summary of your conclusions with respect to these disparate positions?

A 5 Yes. I think the issue in this discussion, as so often happens, is at least partly one of semantics. It is a case of the same words having different meanings to different people at different times.

Thus, if we interpret "statistics" in the narrow (and conventional) sense to mean the classical body of ideas and techniques developed for sampling problems involving balls and urns, or widgets on a production line, etc., then I think that the IDVP and the Staff are right. It is largely inappropriate for design verification. On the other hand, if we interpret it more broadly, to mean "probabilistics," i.e. the science of states of confidence, then I think it is appropriate. It is possible that this interpretation is ultimately what Mr. Hubbard has in mind. If so he is asking a question that is of interest to all of us, one that is basic to our being here; namely, "On the basis of the IDVP and the ITP, of the Staff reviews and of all the work that has been done, what degree of confidence can we have that there are no design deficiencies in the Phase II portion of the plant which would endanger the public health and safety?"

This question is the key underlying question and I believe it is capable of being answered numerically. But it cannot be answered within the domain of classical sampling statistics. It is answerable within a broader domain of ideas and techniques for which I like to use the word

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"probabilistics," to distinguish it from the narrower domain of statistics.

Within this broader domain, this question can be well posed and well answered. The answer necessarily includes expert judgment and opinion as well as all the "statistical" evidence available.

Q 6 Can you summarize why you say statistics in the conventional sense is inappropiate to design verification?

A 6 Yes. Basically, I think the statistical framework, or model, is too limited to include the questions of main interest to us. It asks other questions which are less useful for decision-making. Also, it has difficulty including the kinds of information that are developed in a design verification program. Conventional statistics grew out of consideration of a category of problems that I call Ball and Urn problems. In such problems, there is a set of well defined elements, or balls, all equal and well mixed and all readily discernible as either black or white. Statistics then attempts to say something about the frequency of black balls from the results of a sample drawn from the urn. Obviously, this is a very simple model and it is difficult to impose it on, or fit it to, the reality of a design verification program for a nuclear power plant.

When I say difficult, I do not mean mechanically. The process can be mechanically carried out. What I mean is that in squeezing the design verification program into this simpler framework, one loses essential parts of the problem. The balls in this problem are not equal nor are they separate. They are interconnected in various ways and this interrelationship, this "How the parts work together," is a key aspect of design verification. Moreover, designs are not black or white, right or wrong. Errors come in different types and degrees and significances. The errors of most importance, of course, are those which result in significant safety hazards. To find these in a design verification program, one does not want to sample randomly, but purposefully and intelligently, looking where such errors are most likely to be.

All this, of course, is not meant to say that the statistical approach, that is the Ball and Urn model, is completely without value. Some insights can perhaps be gained this way, but these are far from the whole story.

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1. THE BALL AND URN MODEL - CONVENTIONAL STATISTICS AND THE BAYESIAN APPROACH.

Q 7 Can you give us a concise statement of what you mean by Ball and Urn problems?

A 7 In a typical such problem, we have an urn containing black and white balls, thoroughly mixed, and the problem is to estimate the fraction of black balls from the result of a sampling experiment. Thus, let the Greek symbol lambda (λ) denote the fraction of balls that are black, and suppose we have drawn a random sample of size n out of which k are black. The question we now pose is: What can we infer about λ from this sample and with what degree of confidence can we infer it?

The most succinct way of answering this question is to present a probability curve which sets forth our state of knowledge about the value of λ based on the results of the sample.

Such a curve is represented in Figure 1-1. The area under this curve between any two points on the horizontal axis represents our degree of confidence that the true value of λ lies between those points.

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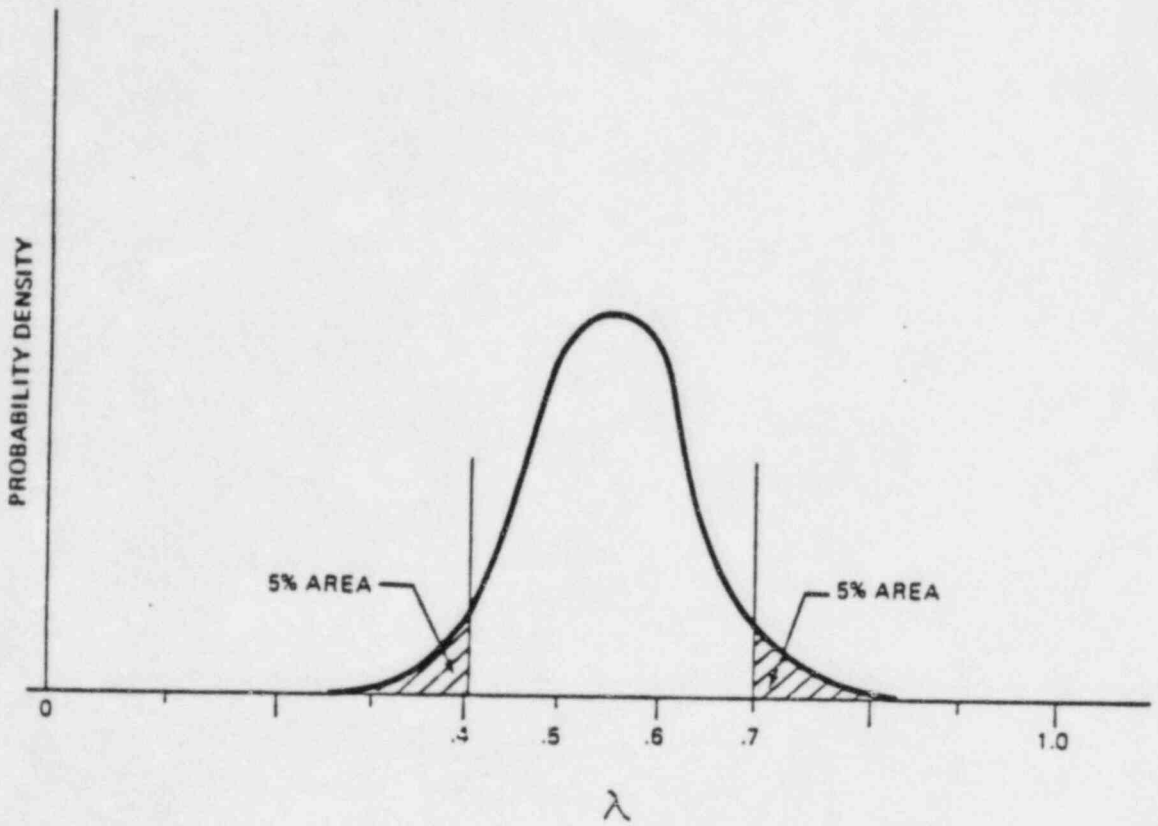


FIGURE 1-1 STATE OF KNOWLEDGE PROBABILITY CURVE FOR THE PARAMETER λ

Thus, for example, suppose the shaded area to the left of 0.4 is 5% of the total area under the curve. Then we would say that we are 95% confident that λ is greater than 0.4. Equivalently, we would say that the probability is 0.95 that λ is greater than 0.4. Similarly, suppose the area to the right of 0.7 is also 5%. Then we are 95% confident that λ is below 0.7. We are thus also 90% confident that λ lies between 0.4 and 0.7. Alternatively, we would say that the interval 0.4 to 0.7 is a "90% confidence interval" for λ.

Thus, if we were able to produce such a probability curve against λ, it would express our level of confidence that λ lies in any given interval. In fact we are able to produce this curve. In this sense then, the problem is completely solved. We have inferred all that can be logically inferred about λ from the sample results. We may say, therefore, that the mathematical theory of Ball and Urn problems is complete, finished, and totally satisfactory.

"Well," we may imagine the IDVP engineer saying, "this is all very fine for balls and urns, but how does it apply to verifying the design of a nuclear power plant?" This is a key question in the debate about the appropriateness of statistics. Let us try then to cast the design verification problem into the form of a Ball and Urn problem. The degrees to which

we succeed and fail in this effort will cast light on the discussion and will show what we can and cannot expect to achieve with conventional statistical methods.

Before doing this, however, I think it is necessary to pause here and take note of the fact that even in this simple Ball and Urn problem a sharp divergence occurs between the viewpoint of conventional statistics and what I call the Bayesian approach.

Q 8 What is this divergence between the statistical and the Bayesian viewpoint?

A 8 The way I have used the words "confidence" and "probability" in the previous paragraphs is in the everyday sense of "degree of certainty." I call this also the "Bayesian" sense. In this way of using them, the words "confidence" or "probability" thus refer to an internal state of mind. Phrases like "95% confident" or "95% probable" amount then to assigning numerical values to these internal states. These numerical values would relate to the odds that a decision maker might take in choosing among his options.

The classical statistician rejects this whole idea of quantifying states of mind. He considers it "unscientific," "subjective," nonrigorous," etc. When he says "confidence interval" he means something quite

different. He means, as best I can transliterate it, something like the following:

> "When I as a statistician give a 90% confidence interval for a parameter, what I mean is that, if we consider the thousands of times in my life that I will make such statements, then in 90% of those times the true value of the parameter will actually lie in the interval stated."

I personally find this concept very difficult to think with. It is not what I am looking for when I am contemplating a decision under uncertainty. I much prefer the notion of confidence or probability as a direct numerical scale measuring my degree of conviction or certainty. Thus, "100% confidence," or "probability = 1.0" connotes to me "total certainty," "50% confidence" or "probability = 0.5" means I have no preference one way or the other. Similarly I can calibrate the entire probability scale. To me this definition is much more useful for making decisions and for communicating about uncertainty.

A second aspect of this divergence is that the Bayesian says: In addition to what I learned from my sample I may have some other knowledge about the value of λ. If I do have some such "prior" knowledge I certainly should use it in coming up with my probability curve. Not using it would amount to withholding from and thus misinforming my decision maker.

The classicist again rejects this inclusion of prior knowledge as being "nonobjective," "nonscientific," etc.

So this is the divergence that occurs at this point in the approach for the Ball and Urn problem. However, I want to emphasize that this divergence is not my main point nor is it really a big issue in this hearing. I mention it only incidentally, as part of the cultural background to the debates going on here, and in other hearings, about the use of statistics.

Obviously, I think the Bayesian approach to the Ball and Urn problem is vastly superior, but I must emphasize again that this is not my main point in this testimony. My main point is not that one or another approach is superior in the Ball and Urn problem. My main point is that the Ball and Urn problem itself is a poor model, a poor vantage point from which to view a design verification program.

Q 9 In light of this divergence, how would the two schools of thought develop the curve of Figure 1-1?

A 9 Actually I think a conventional statistician would ordinarily not produce this curve since he does not use probability in this sense. However, in principle a similar appearing curve could be produced, from which one could read his confidence intervals. A Bayesian, of course, would develop this curve using Bayes'

theorem which he regards as the fundamental law of logical reasoning.

A 10 Can you give us a brief description of Bayes' theorem?

A 10 Yes. It takes only two lines to derive Bayes' theorem. We begin with the basic relationship:

$$p(A \wedge B) = p(B)p(A|B) \qquad (1.1)$$

where

$p(A \wedge B)$ is our probability, i.e., our level of confidence, that propositions A and B are both true

$p(B)$ is our probability that proposition B is true

$p(A|B)$ is our probability that A is true given that B is true.

Some people consider the relationship (1.1) as an axiom, i.e., something that we postulate. My view is that we need not postulate it; it itself follows from our fundamental definition of probability as a numerical scale for quantifying degrees of confidence.

Either way, if (1) is true then so also is

$$p(A \wedge B) = p(A)p(B|A) \qquad (1.2)$$

Therefore, equating the right sides of (1.1) and (1.2) and dividing by p(B) we obtain:

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$$p(A|B) = p(A)\left[\frac{p(B|A)}{p(B)}\right] \qquad (1.3)$$

This is Bayes' theorem. What it says in words is that our level of confidence in proposition A, after we learn that B is true, is equal to our confidence in A before learning about B, times a correction factor, the term in brackets.

Observe the important role played in the bracketed term by p(B|A). Bayes' theorem thus tells us that p(A|B) is directly related to p(B|A). This agrees with our common sense. For when we contemplate the liklihood of A being true, given evidence B, we naturally ask ourselves, "If A were true, how likely is it that B would have been observed?" For example, suppose B were impossible given A. Then, if B is known to be true, we can conclude that A is not true (because if it were, B would be false).

Q 11 How does Bayes' theorem apply to the Ball and Urn problem?

A 11 In the Ball and Urn problem, the role of A is played by the proposition:

The true fraction of black balls in the urn is λ.

The role of B is played by the results of the sampling experiment, e.g.:

k black balls found in a sample of size n.

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Let us denote this evidence by E, and the preceding proposition simply by λ. Then we would write Bayes' theorem for the Ball and Urn problem in the form:

$$p(\lambda|E) = p(\lambda) \frac{p(E|\lambda)}{\Sigma p(\lambda)p(E|\lambda)} \qquad (1.4)$$

where the denominator is the sum over all possible values of λ. If the number of balls, N, in the urn is finite, then the possible values of λ are discrete. For each of these possible values $p(E|\lambda)$ is given by a simple well accepted formula and Eq(1.4) than yields a discrete version of Figure 1-1. As N gets larger and larger, this discrete probability distribution approaches a continuous curve like Figure 1-1.

The term $p(E|\lambda)$ is the place in the formula where the specific evidence of the sample result is entered. If one has any other knowledge "prior" to the sample result, this knowledge is encoded into the formula through the term $p(\lambda)$. The formula then blends these two forms of knowledge into the posterior probability function $p(\lambda|E)$.

Non-Baysians, which includes most conventional statisticians, tend to be ill at ease with the term $p(\lambda)$ because it is an encodation of judgment. Since two different people might have different $p(\lambda)$ functions, these people would then get different posteriors, $p(\lambda|E)$. The posteriors then are

"subjective" and, to them, "unscientific and "nonrigorous."

The Bayesian responds that judgment is inescapable when making decisions, so why not recognize it explicitly. Why not make it visible so that all concerned with the decision can comment on it?

Moreover, the Bayesian points out, if one really wanted to make the posterior independent of the individual person, hence "objective," one could simply dictate a fixed function $p(\lambda)$. For example, one could choose $p(\lambda)$ to be a constant, which would say that prior to the sample, as far as we know, all values of λ are equally likely. But this choice, a Bayesian would note, is also an exercise of judgment.

Q 12 Is there anything else you want to say about Ball and Urn problems?

A 12 I just want to note that such problems can be elaborated and complicated in various ways. For example, the Urn could contain different "strata" in each of which λ is different. Indeed, λ could be a function of the height, h, of the strata. We might then try to find out something about this function, $\lambda(h)$, by sampling each strata. All such problems can be handled very cleanly with Bayes' theorem.

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2. APPLICATION OF THE BALL AND URN MODEL TO DESIGN VERIFICATION

Q 13 How can one apply the Ball and Urn Model to the design verification problem?

A 13 To cast the design verification problem into a Ball and Urn framework, the first step must be to identify the population from which we are sampling. For this purpose, one must conceive of the plant design, embodied in a mountain of blueprints and documents, as composed of a large set of discrete and identifiable design "items" or "elements."

Let us now represent these design elements as the points inside the rectangle of Figure 2-1. The rectangle itself then represents the set or "space" of design elements. Inside the space, a certain number of the elements may be incorrect; that is, they may involve design "errors." Let us represent these in the figure by crosses, and let λ stand for the fraction of all points which are crosses; i.e., the fraction of design elements which are errors. We may now, if we desire, attempt to find out something about how big λ is by undertaking a program of sampling points from the space.

Thus, we have cast the design verification program into the form of a Ball and Urn problem. There are several soft spots in our formulation, however.

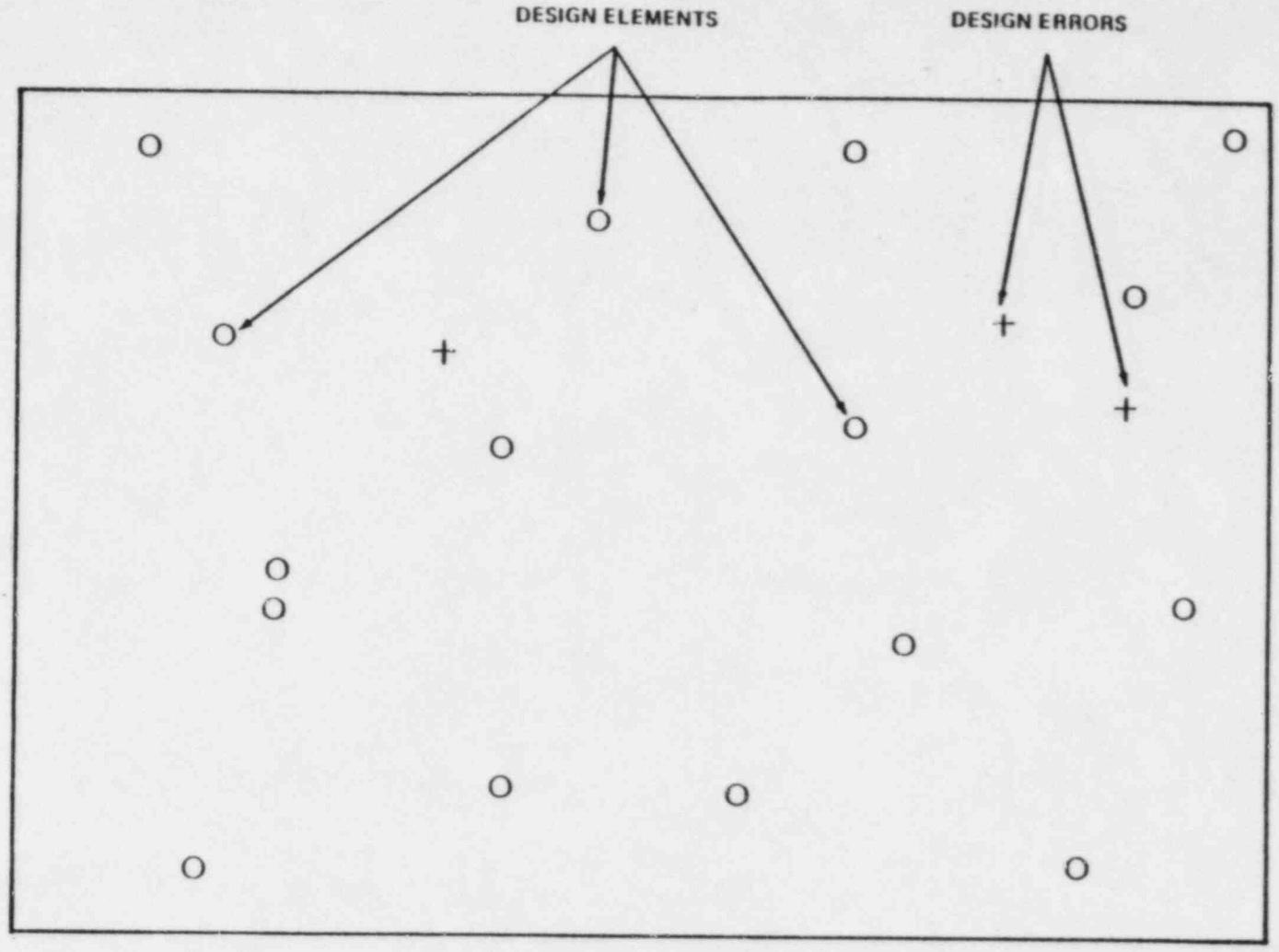


FIGURE 2-1 REPRESENTATION OF DESIGN ELEMENTS – THE SAMPLING SPACE

Q 14 What are these soft spots?

A 14 The first soft spot is that the notion of "design element" is very imprecise. Obviously, if one wishes to think in terms of a sampling model, there must be a well-defined population of well-delineated elements from which to sample. But how shall we define this population and these elements in the case of a design verification program? Should elements be pieces of hardware? Should they be aspects of hardware such as the size of a pump or the spacing of electrical cables? Or should we think of design elements as elements of the design "process," i.e., as calculations, actions, and decisions made in the course of design? How "big" should elements be? Should they be at the level of "components," "parts," "systems," "subsystems," etc.? Obviously, our numerical value of λ will depend drastically on what size elements we define.

Needless to say, the concept of design element leaves something to be desired. It is arbitrary and fuzzy. This arbitrariness to my mind reflects the mismatch of the Ball and Urn model to the design verification situation. Nevertheless, for our present purposes, let us ignore it. Let us assume that we have somehow decided on a reasonable, even if arbitrary, discretization and forge onward.

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A second soft spot is that the definition of "error" is imprecise. How far off from "exact" does a calculation or a design dimension have to be before it is called an error? Obviously also, not all errors are of equal significance and not all design elements are of equal importance. So we have some fuzziness here too. However, again for present purposes, let us simply assume that in some way we have established a workable meaning for "error" and move on.

Q 15 Assuming arguendo that these soft spots could be handled, are there any other problems in fitting the design verification to the Ball and Urn framework?

A 15 Yes indeed. The major problem is: Suppose we knew exactly what λ is; what would it mean? What would we do with it or conclude from it? It is a well known fact that engineers and designers are human beings and as such make errors. This is true in bridges, automobiles, courtroom buildings, foods, drugs, and everything else. In recognition of this fact, the process of design engineering has evolved so that major errors escape detection only rarely; minor errors occur all the time. The process and practice of design, the traditions of "safety factors," "redundancy," "fail safe," "conservatism," "defense in-depth," etc., have been developed, particularly in nuclear plants, to give

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confidence that those errors which do slip through will not endanger the plant or the public.

So, from our experience as human beings, we know at the outset that λ is not zero. Of what value is it to us to know exactly how much λ is? Not only is λ a poorly defined quantity, but in addition, there are no codified or published standards of reference for it.

Q 16 Does this mean that attempting to find λ by a sampling process is entirely useless?

A 16 No, not entirely. There is some usefulness to it. For example, if λ were to fall near the extreme end of its possible range, we could, based on our experience, make certain rough conclusions and judgments. Thus, a high value of λ would suggest "shoddy workmanship." A low value would suggest good quality work. λ for major errors should be far less frequent than for minor errors. So, although there is much fuzziness in the precise numerical value of λ and in the conceptual definition of it, there is nevertheless some rough information content in it and perhaps some insights to be gained from the process of calculating it.

These difficulties in the definition and use of λ, to my mind, reflect the "largely inappropriate" conclusion of the staff with respect to the sampling statistics approach to the design verification problem. The design verification problem does not fit the Ball

and Urn framework without a great deal of squeezing and stretching. The design verification problem, to my way of thinking, is more akin to the problem of: "Is there a needle in a haystack?" or "Is there a submarine in the fjord?" than it is to "What is the fraction of black balls in the urn?" Nevertheless, it is a fairly common experience in analytical work that in the course of attempting to do something vague or ill defined, or even foolish, we often learn something or see something of value. In this spirit I could attempt to calculate the value of λ.

Actually, I could do more than that -- I could attempt to gain still further information by imposing a structure or "geometry" on the basic sampling space and examining the value of λ in various subregions of the space. This structure would also help illuminate the sampling scheme used by the IDVP in comparison to that advocated in the statistical approach.

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3. STRUCTURING THE SAMPLE SPACE

Q 17 Would you compare the statistical sampling approach to what the IDVP did?

A 17 In the Ball and Urn example, we selected our sample "randomly;" that is, we blindfolded ourselves, shook the urn vigorously, reached in and grabbed a ball at an arbitrary depth. One might think to apply the same random approach to design verification, checking a calculation here, a blueprint there, etc., rolling dice or something to guide the selection. In principle, this process could be carried out, but it was not adopted by the IDVP. This process is really not very workable or meaningful because of the way design calculations and assumptions are interlocked. It is much more efficient and satisfying to do what the IDVP engineers did; namely, select specific systems and check essentially the entire design of those systems from beginning to end.

3.1 MACROSTRUCTURE OF THE DESIGN SPACE

Q 18 Can structuring the design space help us understand the relation of the IDVP sample to the whole design process?

A 18 Yes. To visualize the IDVP sampling process, let us impose a structure on the space of Figure 2-1. We divide the rectangle as shown in Figure 3-1 vertically

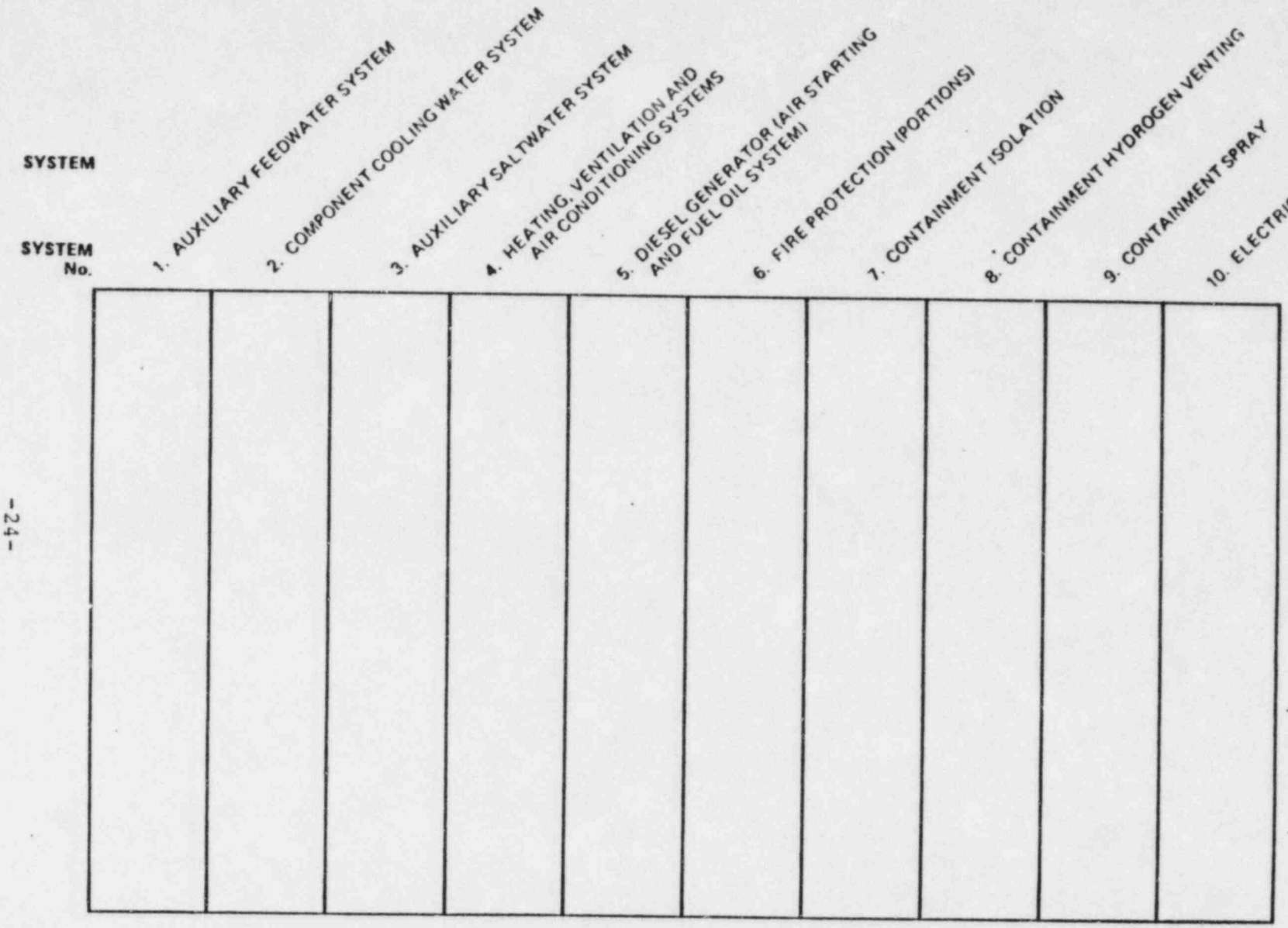


FIGURE 3-1 COLUMNAR OR VERTICAL STRUCTURE OF THE DESIGN SPACE BY SYSTEMS

into columns representing the various plant "systems" considered under Phase II of the IDVP. The IDVP defined 10 such systems as listed in Table 3-1.

TABLE 3-1. SYSTEMS INVOLVED IN PHASE II IDVP

The PG&E-designed safety-related systems (Reference IDVP Final Report, Section 4):

1. Auxiliary Feedwater System (AFW)
2. Component Cooling Water System (CCW)
3. Auxiliary Saltwater System
4. Heating, Ventilation, and Air Conditioning (HVAC) System
5. Diesel Generator (air starting and fuel oil system)
6. Fire Protection (portions)
7. Containment Isolation
8. Containment Hydrogen Venting
9. Containment Spray
10. Electrical Systems

In a similar way, we can imagine the space to be structured horizontally, as shown in Figure 3-2, into rows representing different design "disciplines"; that is, different kinds of design work done by different teams of specialists. The disciplines are identified in Table 3-2.

TABLE 3-2. DISCIPLINES INVOLVED IN PHASE II IDVP

1. Mechanical (Mech)
2. Electrical (Elec)
3. Instrumentation and Control (I&C)
4. Heating, Ventilation, and Air Conditioning (HVAC)

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Structuring the space by disciplines is useful because this is the way the design work is organized. The same team does a specific discipline's work in all the systems, using by and large the same individuals, the same design methods, checking procedures, etc. Thus, reviewing the work of a discipline under one system gives a sample of the work of that team over all systems.

Q 19 Having structured the space into subregions this way, can we now talk about values of λ for each subregion?

A 19 Yes. In Figure 3-2, let us use the indices i and j to denote a typical row and a typical column, respectively. Thus, the box which is the intersection of the ith row and the jth column in the figure would represent the design work done by the ith discipline team in connection with the design of system j. We can now denote by $\lambda_{i,j}$ the error rate corresponding to this box. Similarly, let us denote by λ_i the error rate of the ith row (i.e., the ith discipline) and by $\lambda_{,j}$ the error rate of the jth column; i.e., of system j.

3.2 REFINEMENT OF THE MACROSTRUCTURE DIAGRAM

Q 20 Does Figure 3-2 display the structure of the space adequately?

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A 20 The diagram of Figure 3-2 begins to communicate the structure of the design process; however, it needs refinement in several respects. First, not all the design disciplines are involved in all the systems. To indicate this fact we present Figure 3-3, which is like Figure 3-2 except that those discipline/system combinations which contain no design activity have been removed from the diagram. Thus, the rectangular design space of Figure 3-2 is replaced by the irregular outline of Figure 3-3.

A second useful refinement is to recognize that the 10 systems of Table 3-1 are vastly different in terms of size, complexity and amount of design activity involved. For example, system 9, the containment hydrogen venting system, is a very simple system by comparison with, for example, the AFW or the CCW. In Figure 3-4, we have attempted to communicate at least a coarse sense of the relative amount of design work in the various systems by assigning different widths to the various columns. Figure 3-4 then gives a little more accurate visual impression of the overall structure of the design effort.

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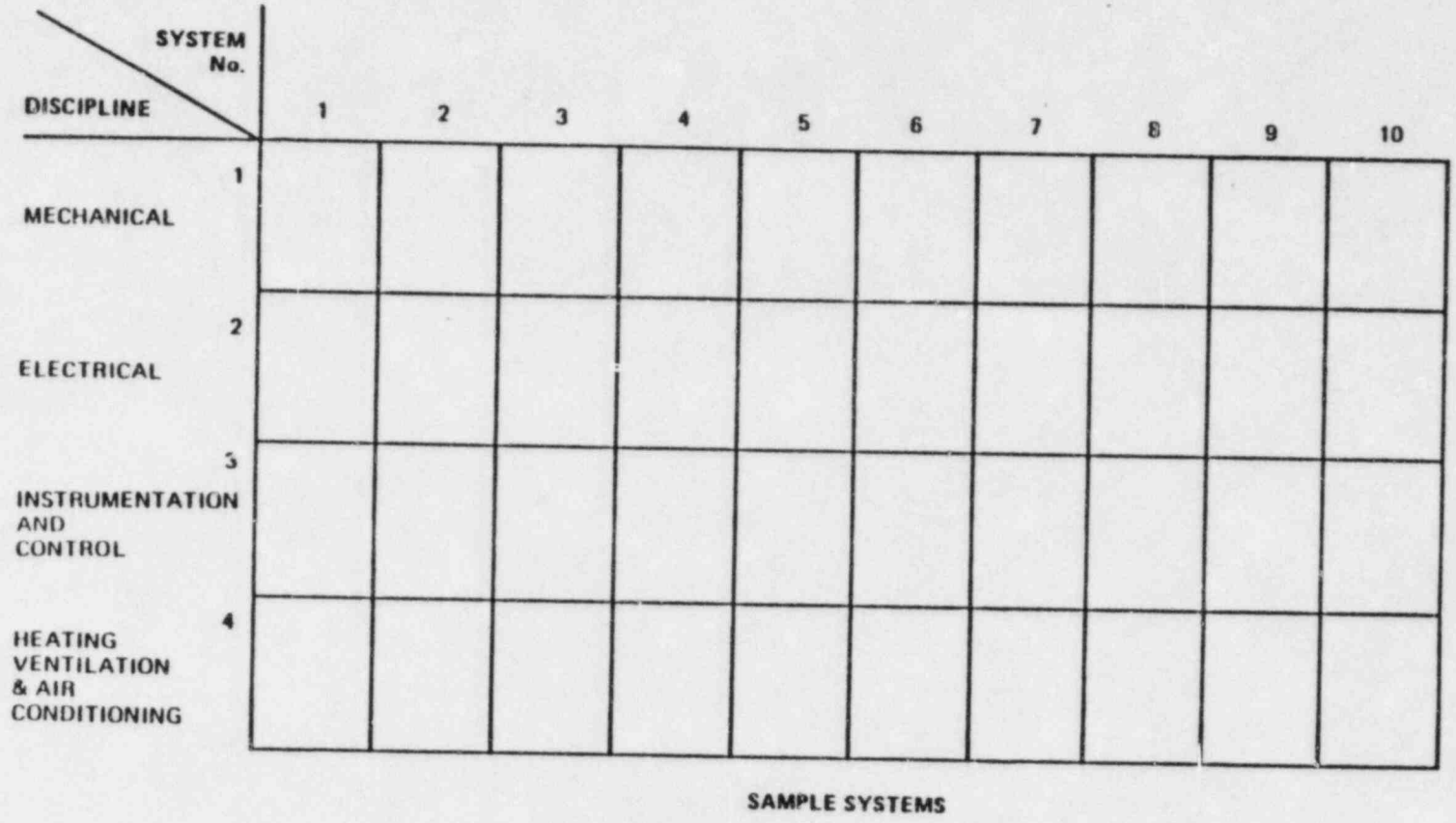


FIGURE 3-2 VERTICAL & HORIZONTAL STRUCTURE OF THE DESIGN SPACE

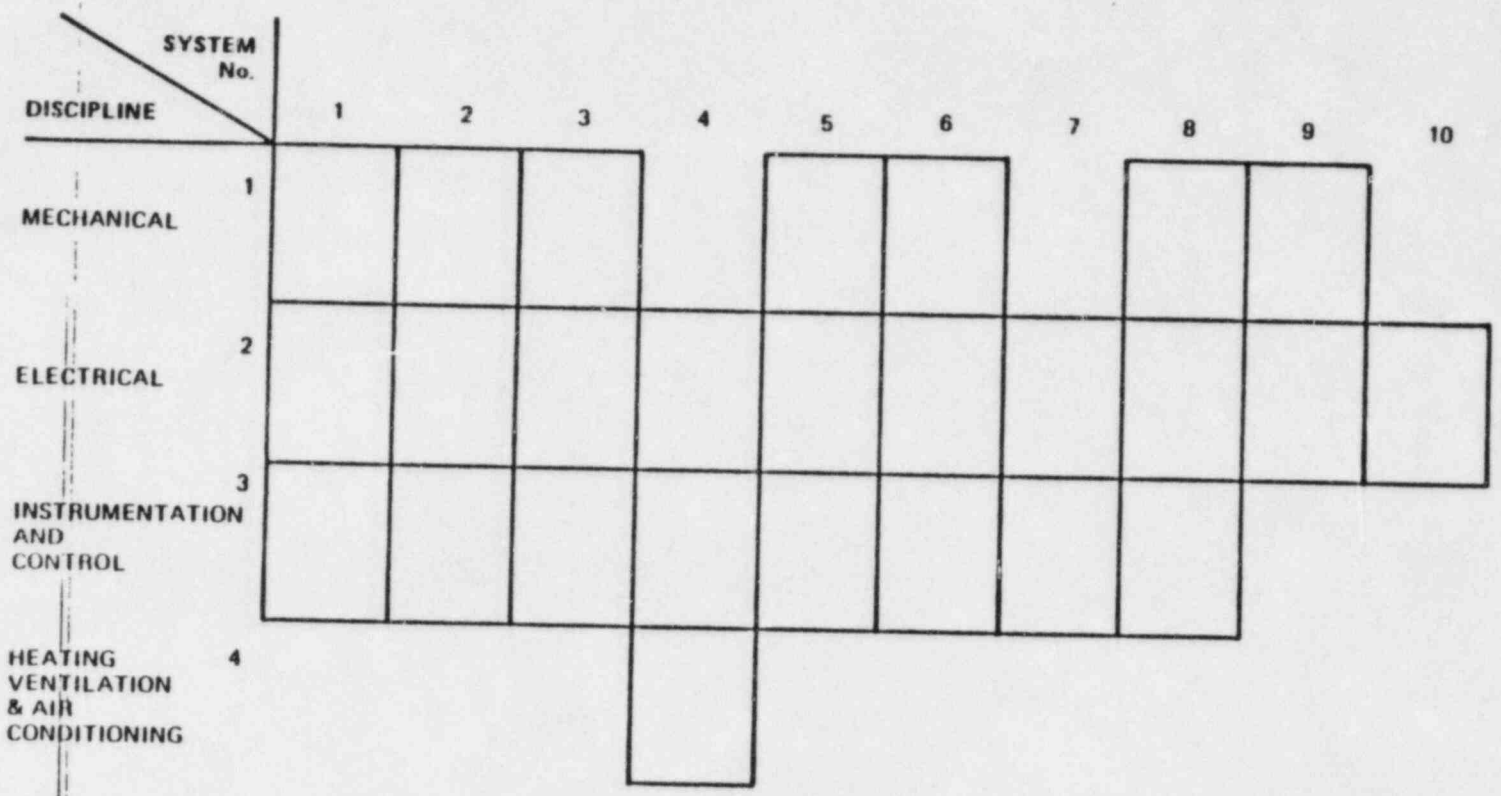


FIGURE 3-3 DESIGN SPACE WITH NONAPPLICABLE BOXES REMOVED

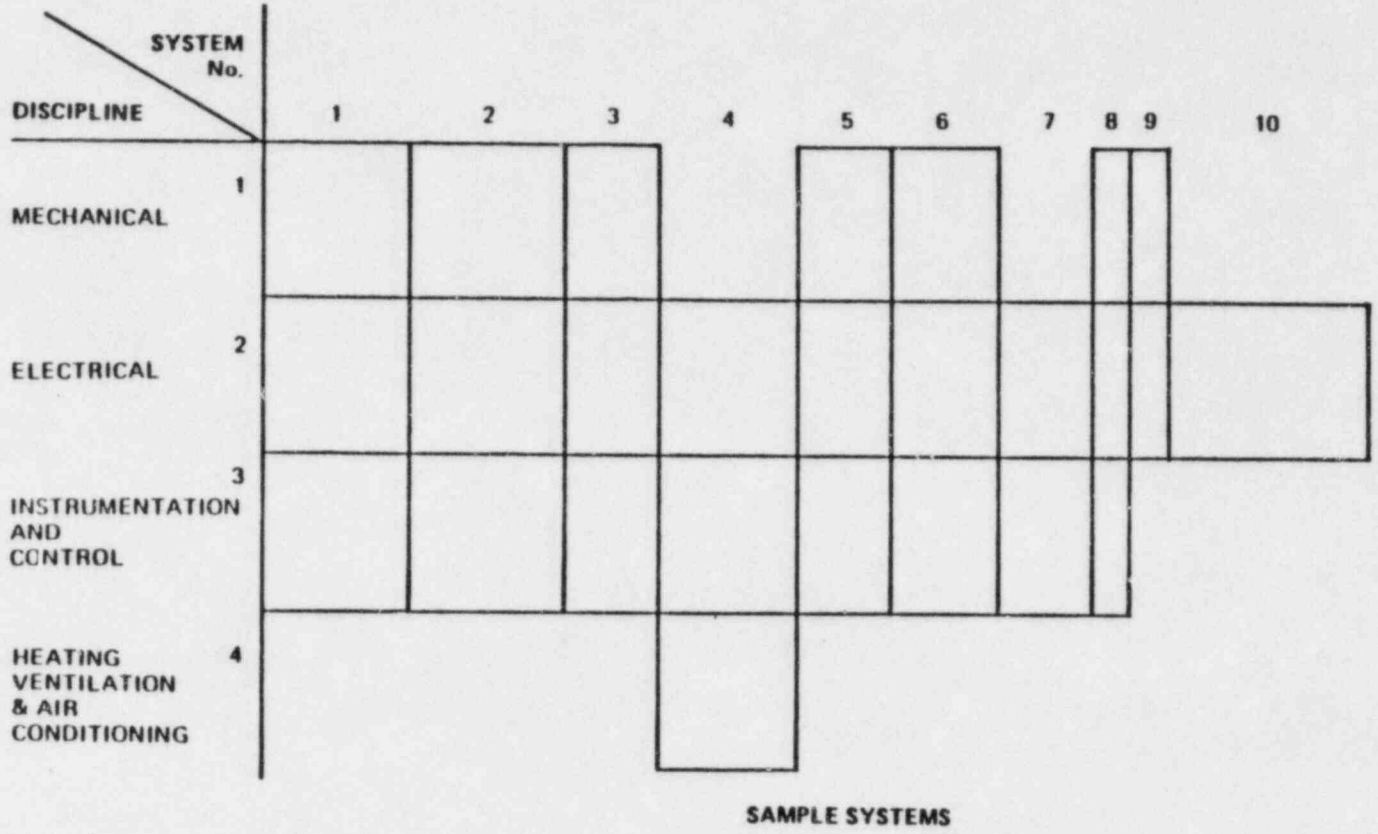


FIGURE 3-4 DESIGN SPACE WITH UNEQUAL WIDTHS OF COLUMNS TO REPRESENT DIFFERENT SIZES OF DESIGN ACTIVITY IN DIFFERENT SYSTEMS AND DISCIPLINES

4. SUBSTRUCTURE

Q 21 Are there further structural features of the space which it is useful to recognize?

A 21 It is useful to distinguish a further level of structure of the sampling space by breaking down the rows and columns in Figure 3-4 into still smaller units.

Thus, for example, the 4,160V AC system, which is a main part of the electrical system, can be broken down into subsystems, or subcolumns. These subsystems can be thought of as "components" or "circuits." Similarly, the row representing the electrical discipline can be broken into subrows as shown in Table 4-1, representing different aspects of a circuit or component design that the designer must consider and specify.

With this row and column breakdown, the box "electrical design/4,160V AC system" would have the substructure diagrammed in Figure 4-1. The X marks in this figure indicate where actual design work was done, i.e., show where the discipline aspect applies to the subsystem.

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We could, of course, continue breaking this structure down into smaller and smaller elements. However, for the sake of manageability let us stop here and agree to define the boxes with check marks in Figure 4-1 as "design elements."

In a similar way, we may define a substructure and design elements for all the system/discipline combinations.

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TABLE 4-1

ASPECTS OF ELECTRICAL DESIGN

CRITERIA

- System & Functional Requirements
- NRC Requirements
- Codes & Standards

ENVIRONMENTAL

- Temperature & Pressure
- Chemical Exposure
- Humidity
- Radiation Dose
- Seismic Requirements
- Aging Requirements

ELECTRICAL

- Power
- Redundancy
- Impedance
- Starting Current & Voltage
- Overcurrent Protection
- Insulation

LOCATION

- Fire Separation
- HELB (Jet Impingement) Separation
- Electrical Separation

PHYSICAL

- Physical Requirements
- Raceway Requirements
- Color Code

OTHER

- Test Data
- QA Requirements

	4 KV						480 VOLTS						LOW VOLTAGE					
	LOADS	SOURCES	CONVERTERS	INTERRUPTERS	SENSORS	CIRCUITS	LOADS	SOURCES	CONVERTERS	INTERRUPTERS	SENSORS	CIRCUITS	LOADS	SOURCES	CONVERTERS	INTERRUPTERS	SENSORS	CIRCUITS
SYS. RQMTS		X	X	X	X	X										X	X	X
NRC RQMTS		X	X	X	X	X										X	X	X
CODES, STDS		X	X	X	X	X										X	X	X
TEMP & PRESS																		
CHEM																		
HUMIDITY																		
RADIATION																		
SEISMIC		X	X	X	X	X										X	X	X
AGING																		
POWER		X	X	X	X	X										X	X	X
REDUNDY		X				X												X
IMPED.		X	X	X	X	X										X	X	X
STARTING		X																
OVER CURR.		X	X	X	X	X										X	X	X
INSUL.		X	X			X												X
FIRE SEP.		X	X	X	X	X										X	X	X
HELB SEP.																		
ELEC SEP.						X												X
PHYSICAL RQMTS.		X	X	X	X	X										X	X	X
RCWY						X												X
COLOR						X												X
TEST DATA		X	X	X	X	X										X	X	X
QA RQMTS		X	X	X	X	X										X	X	X

FIGURE 4-1

DESIGN ELEMENTS OF THE 4KV SYSTEM (ELECTRICAL DISCIPLINE)
COMPONENTS AND CIRCUITS

5. THE SAMPLING USED IN THE IDVP

Q 22 How does the structure help us understand the sampling plan of the IDVP?

A 22 In light of the structure of the space just established, we can now give a fuller description of the sampling process used by the IDVP engineers. As mentioned before, rather than selecting elements at random throughout the design space, because of the interwoven nature of the design process, and because of the importance of checking the "integrative" aspects of the design (i.e., the relationship of structures, systems and components to each other), the IDVP engineers found it more satisfactory to choose three systems and check the complete design of those systems. These systems are the auxiliary feedwater (AFW) system, the control room ventilation and pressurization (CRVP) system, which is one of the more complex portions of the heating, ventilation, and air conditioning system (HVAC), and the 4,160V AC system, which is a main part of the electrical systems. If, in the course of verifying these systems, an error was found which had "generic potential," that error was checked for horizontally through the whole chart. This process was called "additional verification." For example, in the course of checking the CRVP system, the spacing of low voltage electrical cables was found not to be according

to design criteria. The spacing of these cables was then checked in all systems throughout the plant.

In terms of our diagram of the sampling space, this means that within the row "electrical" the subrow for "spacing" was checked horizontally across the entire rectangle. Thus, in our diagram, if we wished to represent by shading the areas which were verified by the IDVP, we would shade in the entire three columns representing the three check systems and, in addition, the subrow for spacing of cables and the subrows representing the other additional verification items. (The additional verification items identified by the IDVP are listed in Table 5-1.) The resulting shaded area would appear as in Figure 5-1. This figure gives a visual impression of the relation of the sample chosen by the IDVP to the total space of design elements.

Q 23 Why were these three particular systems chosen by the IDVP?

A 23 The three checked systems were selected by the IDVP so that together they would constitute an adequate sample of the work of the several design discipline teams. They were selected so as to include in the sample an air system, a liquid system, and an electrical system. They were chosen with an eye to the size and complexity of the systems and to their significance to safety.

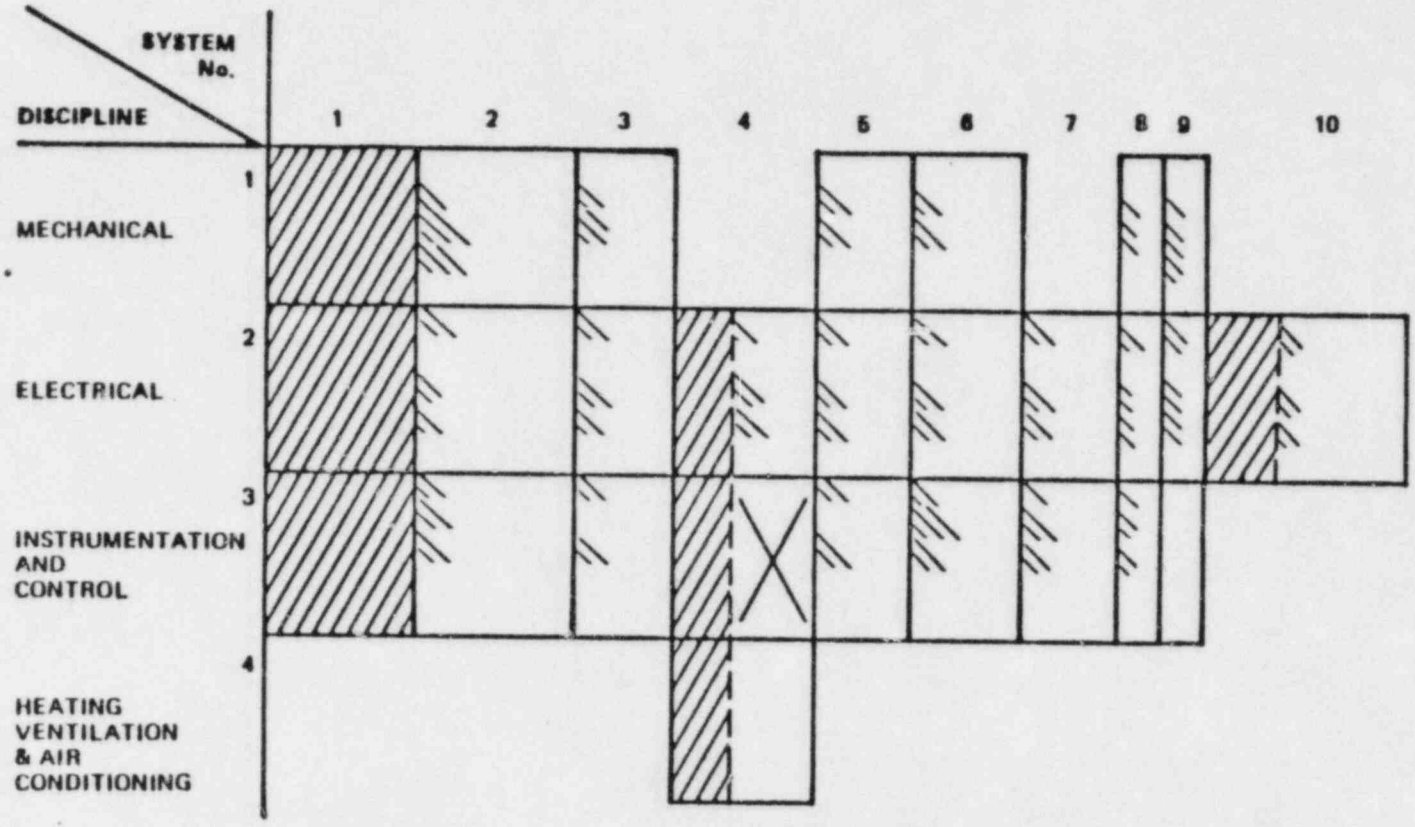


FIGURE 5-1 GEOMETRY OF THE SAMPLE SPACE

SHOWING:

IDVP SAMPLED AREAS

ADDITIONAL VERIFICATION SAMPLED AREAS

X NO DESIGN ELEMENTS EXIST IN THIS SPACE

TABLE 5-1. ADDITIONAL VERIFICATION ITEMS

1.	Redundancy of equipment and power supplies in shared (Units 1 and 2) safety-related systems.
2.	Selection of system design pressure and temperature, and differential pressure across power-operated valves.
3.	Environmental consequences of postulated pipe rupture outside of containment.
4.	Circuit separation and single failure review of safety related electrical components.
5.	Jet impringement effects of postulated pipe ruptures inside containment.

Also, recognizing that errors are more likely to occur at the interfaces between disciplines and in the communications among design organizations, the review team selected the three systems to include samples of the most important interfaces and those most vulnerable to communication errors.

The IDVP sample is thus an excellent example of what is known as "stratified sampling." It was chosen to include the various kinds of strata, i.e. air, liquid and electrical, the various design disciplines and the interfaces between diciplines. Also in choosing complete systems, it was able to include the very important integrative aspects, i.e. how the components work together in the system, how the system as a whole performs its various functions under various conditions. The sample was stratified also in that it emphasized areas of complexity which were more likely to contain errors.

Q 24 In the course of its review, how did the IDVP treat discoveries that could have implications in other systems?

A 24 In the course of reviewing the sample, when something was found raising the suspicion of a generic error, this error, as we mentioned, was checked horizontally, i.e., in all systems. If the suspicion or concern were of a broader nature, the Program Plan

allowed for additional vertical sampling, i.e. of systems outside the chosen three. No such concern of a broader nature arose during the IDVP. However, the individual engineers looked into other areas of the design, outside both the chosen vertical systems and the additional verification horizontal items if, in the course of their work, any question came up which made them want to do that. They were to "follow their noses" in this respect. Indeed, they were free to check in this outside area solely for reasons of curiosity.

In light of these features of the IDVP sampling program, i.e. additional verification, additional sampling and following of noses, we may regard that program as an example of what is known as "sequential sampling." That is, selection of further elements of the sample is guided by what has been learned from earlier elements.

Thus, the IDVP sampling plan encompassed both the ideas of "stratified sampling" and "sequential sampling." These ideas are recognized by both engineers and statisticians as contributing greatly to the effectiveness and efficiency of the sampling plans. They are also recognized by both professions as ideas requiring in their application the informed judgment of experts in the subject matter at hand.

The total IDVP sample thus consisted of the three selected systems, the five additional verification items, and the "nosing around" described above, of which, I am told, there was a substantial amount. All together the sample reviewed can be characterized as emphasizing those areas which were most likely to contain errors. This type of sampling was termed "judgment" sampling by the IDVP. I would call it "intelligent" sampling, as opposed to "random" sampling.

Q 25 Do the representatives of the Governor and Joint Intervenors accept this method of sampling?

A 25 Mr. Hubbard (supplemental affidavit, March 26, 1983) and the Joint Intervenors strongly disapprove of judgment sampling because it is "subjective" and "nonrandom." They prefer the random approach, which to them is "objective" and "statistically valid."

Q 26 Why is it the IDVP chose judgment sampling?

A 26 The IDVP with the concurrence of the NRC staff chose to use judgment sampling with the following explanation, given in Appendix C of the Phase II Program Management Plan:

5.2 STAFF AND COMMISSION POSITIONS

The key issue is, of course, what is the appropriate role of statistical methods? There was considerable discussion of this

point during the February 3, 1982 meeting. The opinions of PG&E and the independent design verification program participants at that meeting are excellently summarized by the NRC "Staff Findings and Resolutions of Comments" transmitted to the Commission by SECY-82-89, as follows:

> "The staff believes that statistical techniques are largely inappropriate for a design verification program. In our opinion, the use of expert engineering experience in choosing the design samples to be verified, in understanding the significance of differences in the results of the sample and original analyses, and finally in determining whether additional samples are required provides significantly greater assurance that all critical aspects of design have been considered."

Q 27 Do you agree with the IDVP on this?

A 27 My personal opinion is strongly in agreement with the IDVP engineers. I think the random approach is throughly inappropriate to design verification type problems. When one is searching for submarines one does not randomly sample the ocean. One spends his search energies looking where he thinks they are likely to be. The notion that one can predetermine his level of confidence in advance by specifying the sample size is dubious even in conventional sampling problems, much less in design verification problems. In design verification, random sampling, if done at the level of choosing systems, might well miss the important feature of sampling each of the various strata. If done at

below the system level, i.e. random selection of components to be verified, the process would miss the crucial integrative or "how it fits together" aspect of the verification process. It would also be an extremely wasteful, non-cost-effective way to sample, as compared with reviewing whole systems as the IDVP did; for, to review an individual component, it is necessary for the engineer to also study the design of all the "surrounding" components, those that interact with the given component, so that he can know the boundary conditions and requirements that the given component must meet. As a result, random sampling of components would yield far less information per unit of verification effort than was actually obtained through the IDVP method.

Nevertheless, the underlying desire to reach a specified level of confidence at the end of the sampling process is understandable if not fully logical. A level of confidence cannot be specified in isolation. It must be chosen along with the cost of attaining it. Clearly the Law of Diminishing Returns is operative in a verification program as it is everywhere.

In any case, the assessment of the level of confidence can only be done within a Bayesian framework, not a conventional statistical one, and

target levels can only be attained within a "sequential sampling" approach. That is, the level of confidence is only known after we see the results of the sample -- it cannot be determined beforehand simply by specifying a given sample size. After seeing the results of the sample, we can decide whether further sampling is desirable and whether it is worth the application of resources that could be used productively elsewhere.

The IDVP engineers have, in fact, used a sequential sampling approach and a Bayesian framework, albeit informally. They have said, in effect, that their confidence level is now high enough that further sampling is not worth the effort.

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6. NUMERICAL RESULTS BASED ON THE IDVP SAMPLE AND THE BALL AND URN MODEL

Q 28 From the result of the IDVP sample can you make inferences about the various λ values?

A 28 Yes. I can do this using Bayes' theorem in the manner suggested previously. For all of the aforementioned reasons, however, the numerical results are to be interpreted with a large grain of salt and with awareness of the definitions of "error" and "elements" used.

6.1 ERROR RATES FOR SYSTEMS

First, let us consider the error rates λ_j for the systems which have been verified completely. In the auxiliary feedwater system, the IDVP group reviewed the entire system design as constituted in at least 276 drawings and 1,725 pages. The number of design elements in this system is 417. The number of "Category A or B errors," as defined by the IDVP, is six. Thus, for the vertical column representing this sytem, the failure rate is

$$\lambda_{\text{auxiliary feedwater}} = .014 \text{ errors per design element.}$$

We may consider this number to be "exact" from a sampling point of view since 100% of the elements in this system were sampled.

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In the 4 KV electric power system, 0 errors were found out of a total of 102 design elements, embodied in 35 drawings and 630 pages. Thus we have

$\lambda_{\text{electric power}} = 0$ errors per design element.

In the control room ventilation system, embodied in 845 pages and 150 drawings, three Category A or B errors were found out of 229 design elements for a failure rate of

$\lambda_{CRV} = .013$ errors per design element.

6.2 ERROR RATES FOR DISCIPLINES

As mentioned, these error rates are "exact" since they come from 100% samples. Next, turning to the rows or disciplines, we shall be dealing only with partial samples. We will be able then only to give probability curves for the λ_i. These curves will reflect the uncertainty resulting from the fact that the samples are less than 100%.

In Table 6-1, we list the total number of design elements, the number sampled, and the number of errors found for each discipline. From these, using Bayes' theorem, we are able to develop probability curves for the λ_i for each discipline. These curves are given in

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Figure 6-1. 1/ From the curves, the median estimates are abstracted along with the 10% and 90% confidence limits, and are presented in Table 6-2.

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1/ In developing these curves, we have used in Bayes' theorem in effect a "flat prior"; i.e., we have said in effect that prior to the sample results, we have essentially no knowledge of where on the scale these λ_i are. We have also assumed that the portion of the discipline's elements contained within the three verified systems was "representative" of the entire population of that discipline's elements. Saying this another way, we have assumed that what we have learned about a disciplines' performance under one system is "transferable" to other systems. Let us examine this point a bit more closely.

We may discern at least three levels or senses in which what we learn about a discipline's performance on one system tells us something about the performance of that discipline on other systems. First, the same people for the most part are involved so that learning the quality of their work in one place is indicative of that quality elsewhere. Second, the same models, methods, and computer programs would generally be used on all systems, though possibly with different input. Thus, if these models, etc., are verified correct under one system, that correctness is transferable to all systems. Third, the very same numerical results calculated for one system (e.g., temperature and pressure conditions) are often used in the design of other systems. Thus, if those numerical values are verified under one system they are thereby also verified for the other systems.

The last two points just mentioned show another way in which the Ball and Urn point of view does not apply to design verification. For when we check the work of a discipline team under one system, the sample, in effect, is not limited to that system. Part of the sample reviewed constitutes also an actual check of part of the work done by that team for the other systems. In light of this the IDVP sample is actually considerably larger than the three systems cited.

TABLE 6-1. SAMPLE AND POPULATION SIZE FOR DESIGN DISCIPLINES

Discipline	Total Design Elements	Design Element In Sample	Category A or B Errors
Mechanical	743	196	5
Electrical	2,207	469	4
Instrumentation and Control	719	168	0
HVAC	299	78	0

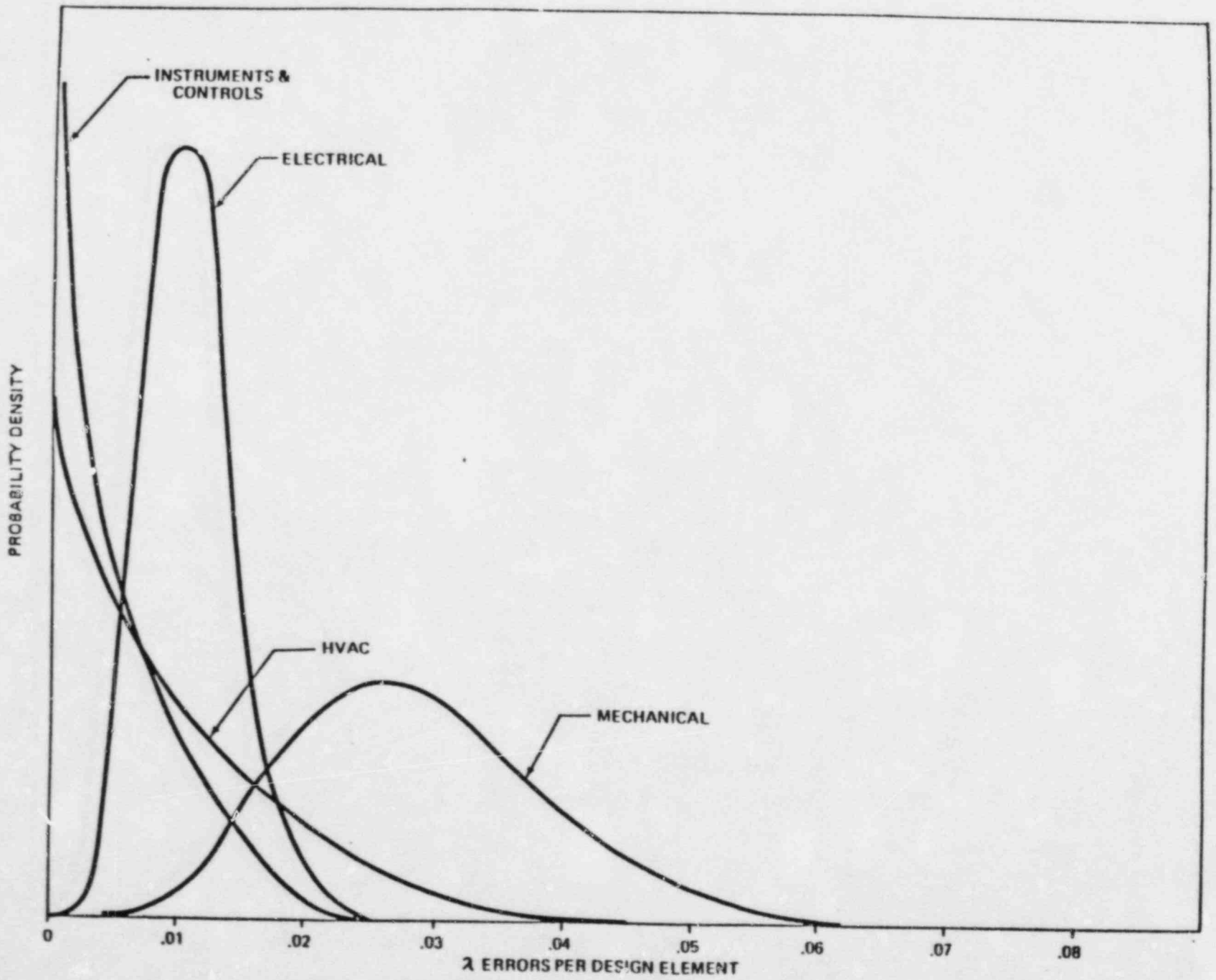


FIGURE 6-1 PROBABILITY CURVES FOR ERROR RATES OF EACH DISCIPLINE

TABLE 6-2. MEDIANS AND CONFIDENCE LIMITS FOR DISCIPLINES

Discipline	10th Percentile	Median	90th Percentile
Mechanical	.016	.028	.0445
Electrical	.005	.010	.016
I&C	0	.005	.012
HVAC	0	.006	.023

6.3 ERROR RATE FOR THE TOTAL POPULATION

To estimate the error rate, for the total population, we write

$$\lambda = \sum_i w_i \lambda_i \qquad (6.1)$$

where w_i is the fraction of all design elements which are contained within discipline i. Carrying out this computation, using the λ_i from Figure 6-1 and w_i obtained from Table 6-1, we obtain the final probability curve for λ shown in Figure 6-2.

Figure 6-2 is the final result of the Hall and Urn analysis of the Phase II IDVP sample. It says that the error rate is of the order of 1.3 errors per one hundred design elements; possibly as high as two per hundred or as low as six per one thousand. This result must be interpreted in light of the definition of design elements used in Section 4.0, the definition of "error" as IDVP category A or B, and the transferability comments of the footnote in Section 6-2. Thus interpreted, these values give us an overall view of the quality of the Phase II systems design work in the Diablo Canyon Plant and of the quality of the work of the various design disciplines. I think it is fair to say that these numbers support the IDVP view that the PG&E Phase II design effort is generally high quality engineering work.

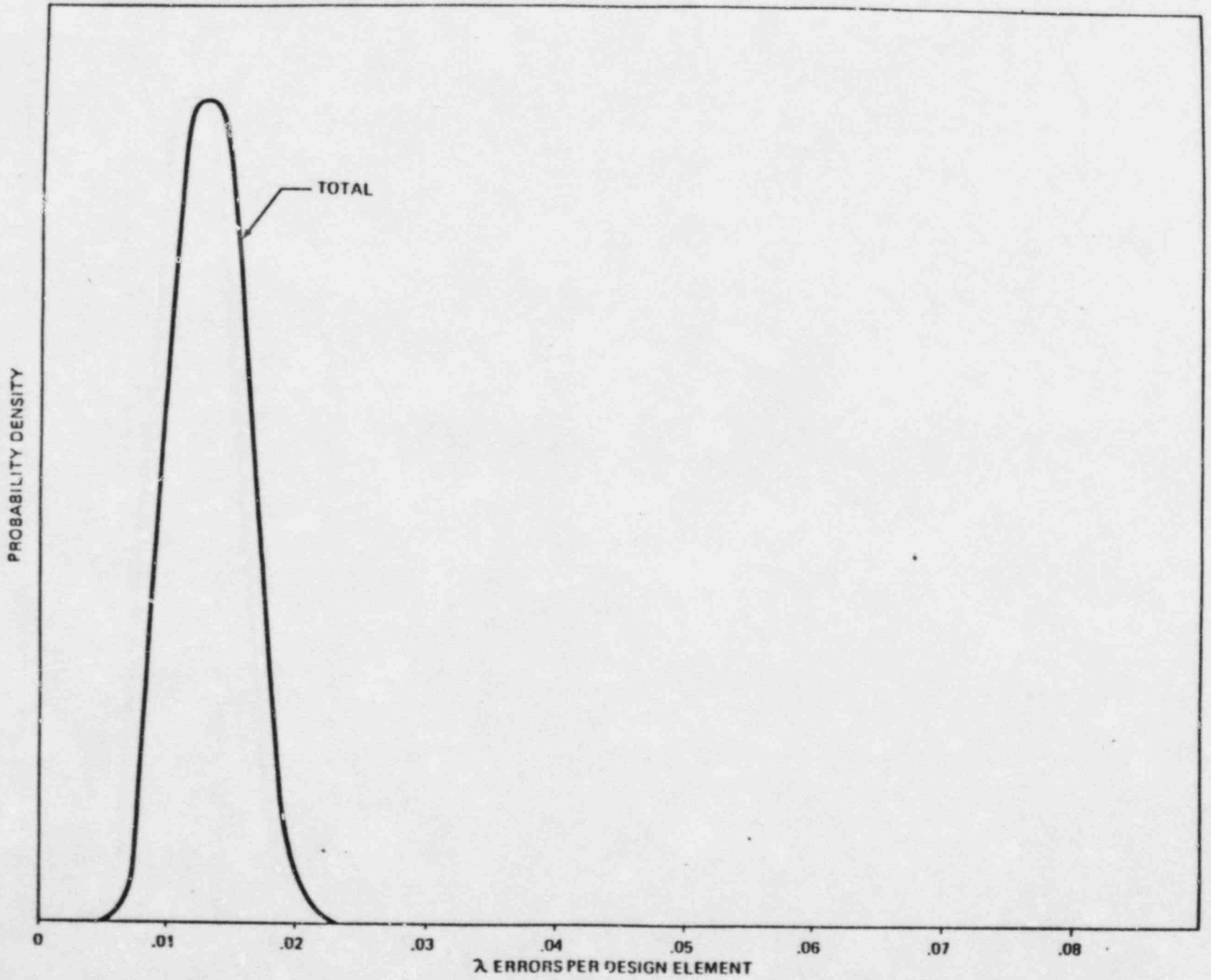


FIGURE 8-2 PROBABILITY CURVE FOR TOTAL POPULATION ERROR RATE

7. SIGNIFICANCE OF ERRORS - THE BASIC PROBABILISTIC QUESTION

7.1 SIGNIFICANCE SCALE

Q 29 The previous section calculated the frequency of category A or B errors. What evidence do we have from the IDVP about the frequency of errors having significance to safety?

A 29 Let us now look more closely at the safety significance of the errors that have been found. For this purpose, let us establish a quasi-quantitative scale for measuring the significance of errors. On this scale, we shall distinguish three regions as in Figure 7-1.

| Region 1 | Region 2 | Region 3 |

Greater Significance →

FIGURE 7-1. SCALE FOR SIGNIFICANCE OF ERRORS

These regions correspond to the key questions we would ask in evaluating any given error. First, we want to know: Does this error constitute or result in a threat to the public health and safety? If so, the error falls in region 3 of the scale. If not, we ask: Does this error constitute a threat to the plant? That is, does it or could it result in significant damage to or outage of the plant? If yes, the error falls in region 2. If the answer to both these questions is no,

we place the error in region 1 of the scale. Locating any specific error on this scale obviously requires judgment. Some errors may easily be categorized while others may be borderline cases.

We can envision this scale as forming a third dimension on the rectangle of Figure 4-1, to form a block as shown in Figure 7-2. Now we can envision the errors, or crosses from Figure 2-1, as distributed through the depth of the block. Thus, we have another dimension of stratification on the space and we wish to know how many errors lie in blocks 1, 2, and 3.

For this purpose, I have returned to our sample and asked the DCP to render its judgment and categorize the errors found there according to our significance scale. The net result of this is that of the errors identified by IDVP, none are considered in the judgment of the DCP to constitute a threat to the plant or to the public.

So with respect to the question of whether there exists any errors of types 2 or 3 in the design, the evidence from the IDVP program is: no such errors were found in the sample of 911 elements from the 3 systems and the 5 additional verification items.

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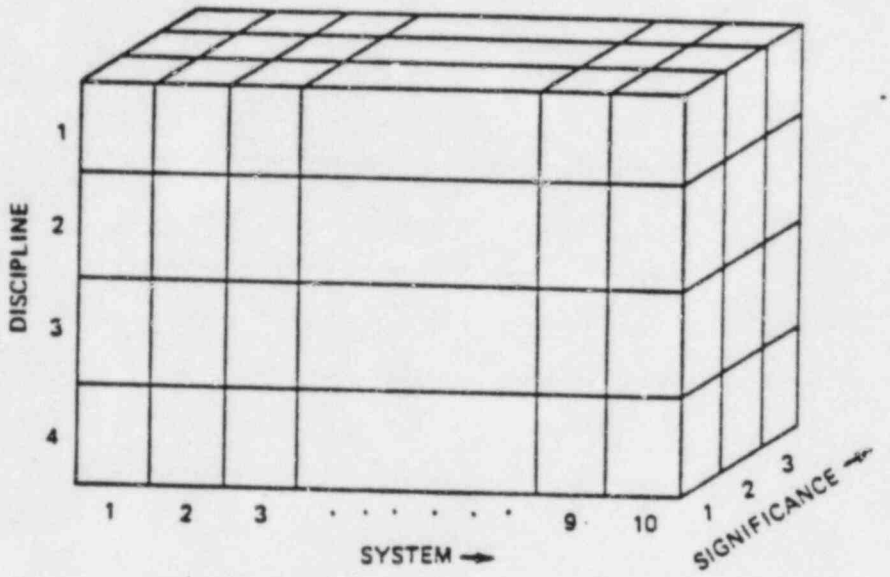


FIGURE 7-2 DESIGN ELEMENT SPACE WITH SIGNIFICANCE DIMENSION

7.2 THE BASIC PROBABILISTIC QUESTION

Q 30 How confident can we now be, on the basis of the IDVP sample, that there are no errors of type 2 or 3 in the Phase II part of the design?

A 30 To address this question, we first rephrase it slightly so that it can be dealt with quantitatively. For this purpose let us introduce an index, n, which can take on the value 0,1,2, ... etc. We then ask: What is the likelihood, in the light of IDVP results, that there are n errors of type 2 or 3 in the Phase II Diablo Canyon system design?

In this form, the question is perfectly posed for an application of Bayes' theorem. We therefore let E stand for the evidence from the IDVP program and write the theorem in the form:

$$p(n|E) = \frac{p(n)p(E|n)}{\sum_n p(n)p(E|n)} \qquad (7.1)$$

where

$p(n|E)$ = our probability in light of evidence E that there are n errors of type 2 or 3.

$p(n)$ = our probability, prior to having evidence E, that there are n errors of type 2 or 3.

$p(E|n)$ = the probability, if there were precisely n errors of type 2 or 3, of having the result E from the IDVP program.

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The set of numbers, p(n), for each n is the "prior distribution." It expresses our state of knowledge before having the results of the IDVP program, and as such must reflect our knowledge of the plant and the design process. Different people may have different prior distributions here. Ultimately, the decision makers, the panel of judges in this case, will have to use their own prior. As a matter of interest, however, I have held discussions with a number of members of the DCP and elicited what I consider to be their collective prior as follows:

$p(n=0) = .999$

$p(n=1) = .001$

$p(n=2) = \text{negligible}$

Thus, the DCP is saying that prior to the verification program, and, therefore, based only on that knowledge of the plant, of the design processes of the NRC reviews, and of the large amount of acceptance testing that has taken place, their level of confidence that there are no type 2 or 3 errors is .999. Now the question is, how does that level of confidence change based on the IDVP results?

For the answer to this question, we turn to Bayes' theorem and look at the terms $p(E|n)$. Beginning with

$p(E|n=1)$ we ask: If there were one error of types 2 or 3, what is the probability that the IDVP result would have been what it was: i.e., evidence E? This brings us to what is always the most interesting part of using Bayes' theorem; namely, interpreting or making clear exactly what is the evidence E. In the present case, there are several aspects to E, the results of the IDVP. We shall attempt to unravel these one at a time, pulling apart the statistical aspects and the judgmental aspects as follows:

ASPECT 1

The simplest interpretation of the IDVP output would be to say that we sampled 3, or almost 3, out of 10 systems and did not find a type 2 or type 3 error. We shall call this interpretation E_1.

E_1 = No errors of type 2 or 3 found in 3 out of 10 systems .

Given the evidence E_1 then, on a purely statistical basis we have

$$p(E_1|n = 0) = 1.0 \qquad (7.2)$$

$$p(E_1|n = 1) = \frac{7}{10} \qquad (7.3)$$

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Using these numbers in Bayes' theorem, we then have

$$p(n = 1|E_1) = \frac{(.001) \times \frac{7}{10}}{(.001) \times \frac{7}{10} + .999} = .0007 \qquad (7.4)$$

Observe that in writing Equation (7.3) we have assumed that all 10 systems were equally liable to contain the error. That being so, the probability of finding the error in a sample of 3 is 3/10 and thus the probability of not finding it is 7/10. In this purely statistical way of interpreting the evidence, we find that our prior probability of .001 is reduced by the evidence E_1 to .0007.

ASPECT 2

The true evidence E, however, contains much more information than the limited interpretation E_1. As an example of a somewhat fuller interpretation, let us observe from Table 6-1 that the total number of elements in the space is 3968 and in the sample is 911. Thus, a second interpretation, which we shall denote E_2, is: out of a total of 3968 elements we did not find an error in a sample of size 911. Thus

E_2 = No error of type 2 or 3 found in 911 out of 3968 design elements

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Assuming, again, that all elements were equally likely to contain the error, we then have

$$p(E_2|n = 0) = 1.0 \qquad (7.5)$$

$$p(E_2|n = 1) = \frac{3057}{3968} = .77 \qquad (7.6)$$

Using these values in Bayes' theorem, we have

$$p(n = 1|E_2) = \frac{.001 \times .77}{.001 \times .77 + .999} = .00077 \qquad (7.7)$$

Thus, again, on a purely statistical basis, but considering the number of elements sampled, we have a reduction of the probabillty of a type 2 or type 3 error from .001 to .00077.

ASPECT 3

But this assumes that all elements were equally likely to have a type 2 or type 3 error. This is clearly not the case. Some elements are much more significant to safety than others. In fact, the more important design elements tend to be included in the IDVP sample. Accounting for this fact, we are led to a third interpretation, E3, of the evidence:

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E_3 = No error of type 2 or 3 found in the set of design elements constituting the IDVP sample

With this interpretation, $p(E_3|n = 0)$ is still = 1.0, but $p(E_3|n = 1)$ must now be evaluated judgmentally. For this purpose, we again ask the DCP for its judgment. Their answer is:

$$p(E_3|n = 1) = 0.15 \qquad (7.8)$$

In other words, the DCP feels that the sample contains most of the elements important to safety and vulnerable to error, so that if there were a type 2 or 3 error somewhere in Phase II of the design, they consider it 85% likely that this error would lie within the elements sampled by the IDVP. Thus, the probability that such an error would not be found in the sample, given that the error existed, is 1.0 - 0.85 = 0.15.

In this case, our posterior probability of type 2 or 3 error is

$$p(n = 1|E_3) = \frac{.001 \times .15}{.001 \times .15 + .999} = .00015 \qquad (7.9)$$

or 15 in 100,000. $p(n=0|E_3)$ is then. .99985.

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ASPECT 4

Interpretation E_3, however, is still not the totality of the evidence E. In addition to the fact that no types 2 or 3 were found, E contains the frequency of type 1 errors, the details of what specific errors were found in which elements, and the general impression of the quality of the Phase II design effort.

Let us denote these additional "quality" factors by Q and let us then say, for short, that

$$E = E_3 \wedge Q \tag{7.10}$$

Now, by Bayes' theorem

$$p(n=1|E) = p(n=1|E_3 \wedge Q) \tag{7.11}$$

$$= p(n=1|E_3)\left[\frac{p(Q|n=1,E_3)}{p(Q|E_3)}\right] \tag{7.12}$$

The bracketed term here again needs to be set by judgment. The number entered here would reflect the inconsistency in the proposition that the quality of the work is as good as it is in the region sampled and at the same time there is an error of type 2 or 3 in the region not sampled.

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For the value of this term I have again asked the DCP for its judgment. They have given a value of 0.2. Using this we obtain finally the values

$$p(n=0|E) = .99997$$
$$p(n=1|E) = .00003 \qquad (7.13)$$

and conclude therefore that on the basis of the IDVP sample and the judgment of the DCP, we are highly confident that there are no type 2 or 3 errors in the Phase II Diablo Canyon System design.

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8. ADDITIONAL SAMPLING, THE DCP REVIEW

Q 31 Do we have any further evidence besides the IDVP results?

A 31 Yes. The results of the IDVP are not the only evidence we have with respect to our basic probabilistic question. In addition to the systems reviewed by the IDVP, the DCP and the NRC staff have reviewed extensive areas of the design space, though perhaps not always as deeply as the IDVP. Figure 8-1 is a portrayal of the additional areas reviewed. In this review, seven additional "open item" errors were found. None were considered a significant error with respect to public safety or plant damage.

The total sample is thus considerably bigger than the IDVP alone. Although I will not attempt to quantify it, the DCP and staff reviews thus provide considerable additional evidence that there are no type 2 or 3 design errors in Phase II. They therefore reduce our probability that there is such an error to below that of Equation (7.13).

Thus we have arrived at a numerical expression of our level of confidence that there are no type 2 or 3 errors in the Phase II design, based upon the verification sample that has been studied. This I think is basically what the Intervenors have asked for. Although this number cannot be arrived at within the

narrow limits of the conventional statistics approach, it can be reached within the broader framework of probabilistic and Bayesian reasoning and it must by its nature incorporate the expert judgments of the knowledgeable people involved. Those judgments have been made and incorporated. The final number shows sufficient confidence to support, in my judgment, the conclusion that the sequential sampling process need not continue beyond this point.

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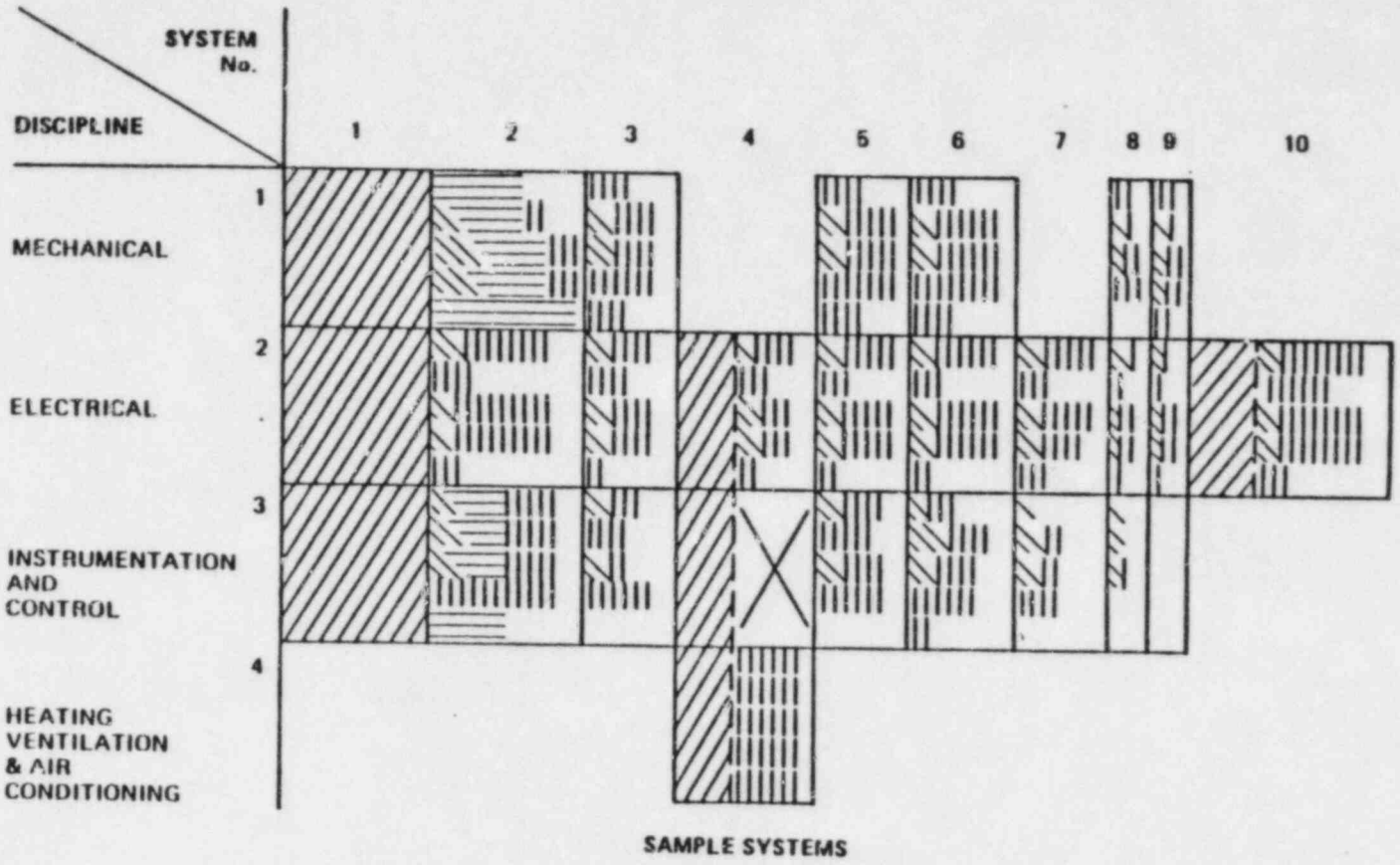


FIGURE 8-1 GEOMETRY OF THE SAMPLE SPACE

SHOWING:

IDVP SAMPLED AREAS

ADDITIONAL VERIFICATION SAMPLED AREAS

NRC REVIEWED AREAS

ITP REVIEWED AREAS

X NO DESIGN ELEMENTS EXIST IN THIS SPACE

9. SUMMARY AND CONCLUSION

Q 32 Would you now summarize your major points for the Board?

A 32 In this hearing, a decision must be made whether sufficient design verification has now been done to provide reasonable assurance that no design errors exist in the Phase II portion of the Diablo Canyon plant which would endanger the public health and safety. This decision must be made by judgment. The three judges must make this judgment. As an aid to this judgment, I have tried in this testimony to do two things:

First, I have tried to clarify the controversy over the appropriateness of statistics. In this controversy I have tried to sort out the smoke from the substance. I believe I have shown, by actually doing it, that a statistical-like program can be carried out, that error rates can be obtained, along with probability curves giving confidence intervals about these error rates, for the design as a whole as well as for various subareas or strata of the design. I believe I have shown that because of the arbitrariness in their definition, and because of the absence of standards of comparison, these error rates are of only minor interest to the decision makers.

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It will be objected that these error rates and these confidence intervals are "nonrigorous" and "statistically invalid" because the samples were chosen by engineering judgment and are hence nonrandom. I answer that, with respect to the vertical strata, for the systems sampled the error rates are exact, since the sample was 100%. With respect to the disciplines, or horizontal strata, it may be that my probability curves are not rigorous in a statistician's sense. However, considering that the sample was chosen to emphasize the likelihood of finding errors, I would say that, if anything, my probability curves are biased towards the high side of the λ values.

In any case, when one considers the totality of the IDVP, DCP and NRC sample as shown in figure 8-1, the whole question of random sampling becomes moot because the sample covers such a large portion of the design space.

The second thing I have tried to do in this testimony is to move the focus off of the question of What is the error rate? and on to the question I think the Board really needs to render its judgment on, namely: What is our probabilty, i.e., our degree of certainty, in light of the IDVP results and all the other evidence, that there is no design error in the

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Phase II portion of the plant which would endanger the public health and safety?

To assist the Board in dealing with this question, I have presented a language and a conceptual framework for quantifying levels of certainty and for making explicit judgments about the effect on those levels of various items of evidence. This language and framework, of course, is that of Bayes' theorem and the theory of probability in the Bayesian sense.

Within this framework, I have attempted to dissect the total body of evidence, separating out the various nuances, aspects or interpretations of the IDVP results, putting into another category the DCP and staff results, and putting all the rest of our information into yet another category which reflects our state of knowledge "prior" to the results of the design verification program.

To assess the prior knowledge and the value of the aspects of the IDVP results, I have asked the most knowledgeable people I know on these subjects, namely the DCP (Bechtel) personnel, to render their judgments in quantitative, numerical form. These numerical values are presented as information to the Board. They are to be regarded as expression of those experts' opinion. After considering these expert judgments and

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the judgments of others, the Board, as is the case with all decision makers, must render its own judgment.

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

Before The Atomic Safety And Licensing Appeal Board

In the Matter of Pacific Gas and Electric Company (Diablo Canyon Nuclear Power Plant, Units No. 1 and 2)	Docket Nos. 50-275 O.L. 50-323 O.L. (Design Quality Assurance)

AFFIDAVIT OF
PROFESSIONAL QUALIFICATIONS

I, Stanley Kaplan, being duly sworn, depose and state that the attached statement of my qualifications is true and correct.

Executed this 13th day of October, 1983.

Stanley Kaplan

Subscribed and sworn to
before me this 13th day
of October, 1983.

C. T. NEAL MADISON
NOTARY PUBLIC – CALIFORNIA
CITY AND COUNTY OF
SAN FRANCISCO
My Commission Expires Dec. 27, 1985

C. T. Neal-Madison

C. T. Neal Madison, Notary Public
in and for the City and County
of San Francisco, State of
California

My commission expires December 27, 1985.

PROFESSIONAL QUALIFICATIONS OF

DR. STANLEY KAPLAN

EDUCATION:

Senior Post-Doctoral Fellowship, University of Southern California, 1967-1969.
Ph.D., Mechanical Engineering and Applied Mathematics, University of Pittsburgh, 1960. Post-doctoral courses in mathematics at the University of Pittsburgh and Carnegie Institute of Technology, 1960-1965.
M.S., Mechanical Engineering, University of Pittsburgh, 1958.
Graduate of the Oak Ridge School of Reactor Technology, 1955.
B.S., Civil Engineering, City College of New York, 1954.

PROFESSIONAL EXPERIENCE:

General Summary

Mathematician and engineer well known for contributions to risk analysis and reliability theory, reactor physics, kinetics, and computational technique. Specializes in probabilistic methodology; decision theory; risk analysis; and, particularly, applications of Bayes' theorem. In this connection, has worked specifically and recently on developing probabilistic and decision theoretic treatments of various phases of the energy business. Included here are PRA analyses of several existing nuclear plants, hazardous material transportation and storage, spent fuel pools, aircraft impact, offshore oil drill (environmental risk), underground oil storage, pipelines, and tarsands projects (business and construction risk). Developer of the DPD method for probabilistic calculations, the two-stage Bayesian technique for data analysis, the "set of triplets," "probability of frequency," "cause table," and "environmental table" concepts in risk analysis. Originator of the Matrix Theory of Event Trees and DPD approach to seismic risk analysis.

Chronological Summary

1977 - Present President, Kaplan & Associates, Inc., a consulting firm specializing in risk analysis and applied decision theory.

Concurrently Adjunct Professor, Department of Chemical, Nuclear and Thermal Engineering, University of California, Los Angeles, and Associate Consultant, Pickard, Lowe and Garrick, Inc.

1975-1977 Private consultant specializing in risk analysis and decision theory.

1972-1975 Holmes & Narver, Inc., Anaheim, California
Director, Advanced Technology Division;
Director, Systems Sciences Division
Technical Director, Nuclear & Systems Science Group

1971-1972 Director of Software Development, COMARC Design Systems, Inc., San Francisco, California.

1969-1971 Product Manager and Senior Staff Member, Computer Sciences Corporation, Los Angeles, California.

1967-1969 Special Research Fellow, U.S. Public Health Service at University of Southern California, Los Angeles.

1955-1967 Westinghouse Bettis Atomic Power Laboratory, West Mifflin, Pennsylvania.
Experimentalist, Experimentalist in Charge, Scientist, Senior Scientist, Fellow Scientist, Advisory Scientist.

1954 Lecturer, Department of Civil Engineering, City College of New York.

1962-1967 Concurrently Adjunct Professor of Mechanical Engineering, University of Pittsburgh; Lecturer, Department of Mathematics, Carnegie Institute of Technology.

MEMBERSHIPS, PAST OR PRESENT:

American Society of Civil Engineers
American Nuclear Society
Society of Industrial and Applied Mathematics
New York Academy of Sciences

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING APPEAL BOARD

In the Matter of	)	
	)	Docket Nos. 50-275
PACIFIC GAS AND ELECTRIC COMPANY	)	50-323
	)	
(Diablo Canyon Nuclear Power Plant, Units No. 1 and 2)	)	Reopened Hearing - Design Quality Assurance
	)	

CERTIFICATION

I, Dan G. Lubbock, hereby certify:

1. I am one of the attorneys for Licensee Pacific Gas and Electric Company in the above-entitled matter and, as such, am authorized to execute this certification.

2. The following witnesses for Licensee Pacific Gas and Electric Company were physically absent from the state or the country and were not able to submit before October 17, 1983, affidavits or declarations of their professional qualifications:

Charles W. Dick

John B. Hoch

Edward Kreh

Robert Wiesemann

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3. As soon as possible and prior to testifying, each such person will submit an affidavit or declaration attesting to his professional qualifications which have been included herein.

I certify under penalty of perjury that the foregoing is true and correct.

Executed at San Francisco, California, on October 15, 1983.

Dan G. Lubbock

DAN G. LUBBOCK

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mgc 1-6

MR. NORTON: We have no further questions.

JUDGE MOORE: Mr. Strumwasser, proceed with your cross-examination.

CROSS-EXAMINATION

BY MR. STRUMWASSER:

Q Let me first ask a question about one of the questions.

Dr. Kaplan, would you turn to Page 66 of your testimony.

System 9 is a containment spray system, is it not?

A . guess. I will look it up.

Q So the change that Mr. Norton just read into the record, he indicates that it is your understanding that the containment spray system has no instrumentation and control design work; is that correct?

A That's correct.

A (Witness Anderson) It has no instrumentation and control work in the PG&E portion of that system.

Q It has it elsewhere?

A Yes, the Westinghouse portion of that system has controls instrumentation.

Q Thank you, Mr. Anderson.

Dr. Kaplan, it is your opinion, is it not, that we can answer numerically the question, What is the probability

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mgc 1-7

that there are no design deficiencies in the Phase II design at Diablo Canyon?

A Yes.

Q And indeed, you have shown, have you not, that one can carry out a statistics-like program to obtain the probability of errors, haven't you?

A To obtain an error rate.

Q And that error rate would be, then, used as a part of an assessment of the probabilities of errors?

A You mean of errors of the same type?

Q Okay.

A Yes.

Q Part VI of your testimony, you estimate an error rate for the IDVP work, right?

A Yes.

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ki 2:1

Q Now, this estimate of the error rates that you develop in Part 6 of your testimony is based upon the definition of an error using the IDVP type A or B errors?

A Yes.

Q And in that analysis, you get a point estimate of 1.3 percent errors, is that correct?

A Yes.

Q And you also provided us with a curve at page 52 of the range of that estimate?

A Yes.

Q Why don't you give us your definition, if you would, please, for the term "point estimate."

A You introduced that term. I don't think I used the term in my testimony.

Q That's right. I just didn't see a word that worked the same. Are you familiar with the term?

A Yes.

Q What do you understand it to mean?

A Well, it's a term that people who do probabilistic work use to describe the results of people who don't.

(Laughter.)

It's a single number calculation, as opposed to a probability curve which reflects your state of knowledge about the quantity in question.

Q That was really more what I was looking for.

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ki 2:2

(Laughter.)

So let me make sure that we understand one another on this term. The term "point estimate" means that you are describing some attributes of a probability curve by describing those attributes in terms of a single number. Is that not correct?

A No. I think when you say "point estimate," you usually refer to the result of somebody who has not done probabilistic calculation. He just does a single number calculation. And so when you want to distinguish that kind of calculation from probabilistic calculation, you call that a point estimate

Q Okay. The 1.3 percent, is that a median figure?

A Yes.

Q One could, instead, use the mean figure, could one not?

A Sure. Use for what?

Q To describe the error rate. That would be another valid way to describe the rate of errors.

A As far as I'm concerned, the curve describes the error rate.

Q The mean of this curve would be higher than the median, would it not?

A I don't know.

Q Are you postulating anything about the symmetry

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of the curve?

A Just by eye, it looks fairly symmetrical.

Q Is that a characteristic of error rate curves?

A It could be. Sometimes it is; sometimes it isn't.

Q Why don't you tell us where that shape comes from, the curve on page 52?

A Well, it comes out of Bayes' theorem in which, as I have described, I have used a flat plotter and particular evidence that was found in the IDVP, or the particular evidence as we have characterized it here -- so many errors in so many design elements.

Q You used some kind of computer program to generate the curve?

A To do the arithmetic, yes.

Q And that program gave you a set of figures which described the density of the curve?

A It describes the curve.

Q Describes the curve.

And does the figure 6-2 on page 52 accurately portray the shape as it came out of the computer for you?

A Yes.

Q Now, this sample was supposed to be representative of the total design work, except maybe a little worse in the sense of error rates; right?

A I'm not sure how to take your phrase "supposed to

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be representative." What I did is said, if you want to carry through a statistical program, looking at each horizontal item, each horizontal band, the discipline bands regarding the sample we have in that band as a random sample, we then calculated the probability curves for each band shown in your earlier figure, 6-1; then combined those to get the one in 6-2 for the total space.

Q Is it necessary to assume random sampling in order to develop these curves?

A It isn't strictly necessary, not the way I did it. I took the sample as if it were a random sample.

Q Now, is it not your opinion that the sample is representative of the total nonseismic design work, except perhaps a little worse?

A Yes, that's my opinion.

Q Now, if we were to ignore for a moment "the little worse" part, let's just set that aside -- are you with me? We could then take your error rate from section 6.3 of your testimony and use that as an error rate for the unsampled nonseismic design work, couldn't we?

A Excuse me. Would you say that again?

Q Sure.

Assuming -- taking the proposition that your sample is representative of the total design -- Phase II design work --

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ki 2:5

A Yes.

Q Setting aside for the moment the "a little worse" part --

A Yes.

Q We could take the results of your calculations and develop an error rate for the unsampled portion, could we not?

A You mean for the total?

Q Yes, for the total.

A That's what we did.

Q So the 1.3 percent is intended to describe the error rate for the whole nonseismic design work?

A I'm hesitating a little at your word "intended," but I'll say it does describe that.

Q Okay. Now, from that 1.3 percent, we could compute a median value for the number of Class A or B errors in unsampled design, couldn't we?

A Sure.

Q And to do that, we would just multiply the median error rate per design element by the number of design elements still unsampled?

A Yes.

Q Now, in your review, you calculated 3,968 design elements in the Phase II work?

A Yes.

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ki 2:6

Q By the way, for present purposes, we will assume Phase II is nonseismic. Okay?

A Yes.

Q Now, of that 3,968 design elements, 911 have been sampled; right?

A Yes.

Q At least 3,057 elements unsampled?

A Yes.

Q So we could multiply the 3,057 elements by 1.3 percent error and obtain a median error rate of 39 -- median number of errors of 39.64; is that correct?

A Okay.

Q So that your analysis in Section 6 indicates that there are about 40 Class A or B errors in the unsampled design; is that right?

A All right.

Q Now, I had set aside the matter of "little worse."

A Yes.

Q If we wanted to, we could adjust that 1.3 percent figure to take into account the "a little worse" part?

A That's right.

Q So if we were to, for example, to drop the error rate by 10 percent, take it from 1.3 percent to 1.7 percent --

A If you took it to be 10 percent.

Q Right. Multiply that by 3,057 and we would get

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ki 2:7

35.8 errors in the unsampled work; right?

A Yes.

Q We could drop it by 25 percent, take it down from 1.3 percent to 0.98 percent, we would still get 29.8 errors; does that sound right to you?

A I'm taking your word for that. I'm not following your arithmetic.

Q But the multiplication is --

A Sure.

Q Even if we were to cut the error in half, we could take it from 1.3 percent to .65 percent; correct?

A Yes.

Q Multiply that by 3,057 -- does it sound right if I told you that would give us 19.87 remaining errors?

A Yes.

Q Now, of course, we don't have any basis, other than someone's judgment, to assume that the error rate in the unsampled portion would actually be different than the sampled work, do we?

A We have that judgment.

JUDGE JOHNSON: Wait a minute. I thought I understood you to say it would be different in the unsampled than in the sampled portion of the work. Did you say that?

MR. STRUMWASSER: I was following Dr. Kaplan's opinion that the sampled work is a little worse. That's his

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ki 2:8

phrase.

JUDGE JOHNSON: Okay.

MR. STRUMWASSER: Now I'm probing that aspect.

JUDGE JOHNSON: Fine. I understand.

WITNESS KAPLAN: Mr. Strumwasser, did I use the phrase "a little worse"?

BY MR. STRUMWASSER:

Q You accepted the phrase here. You used that in your deposition.

A Well, let me say I'm not sure it's a little worse. It may be substantially worse. That's more in Mr. Anderson's domain than mine.

End 2

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3:1

In any event, whoever's domain it is, it's based on a judgment rather than on any quantitative analysis of that question.

As far as I'm concerned, it's based on Mr. Anderson's judgment. Mr. Anderson might have some quantitative evidence behind it.

Q Let's ask him that in a second,but first let me ask you this. If two well-informed, reasonable experts were to disagree on whether the error rates in the sampled or unsampled portions were the same or different, that disagreement itself could be resolved using statistical methods, couldn't it?

A I don't know what you have in mind.

Q Well, one could sample -- one could take a sample from the as yet unsampled design work, and compare error rates in that with the error rates in the early sample; right?

A Sure. If you are to determine the error rate in the unsampled portion, that would answer. Is that what you mean by "result"?

Q Right.

A Correct.

Q Mr. Anderson, has anybody tried to determine the error rate, the unsampled portion in this fashion?

A (Witness Anderson) Not in that fashion. We have used judgment, knowing the systems and the kind of work that

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ki 3:2

was done and the review of the engineering process in the three systems that were selected. We did not make a rigorous attempt to define error rate in the unsampled systems.

Q Will you turn, Dr. Kaplan, to page 41 of your testimony? There, in the middle of the page, appears the 1.3 percent rate; right?

A (Witness Kaplan) Yes.

Q At the bottom of page 51, you say, "I think it is fair to say that these numbers support the IDVP view that the PG&E Phase II design effort is generally high-quality engineering work." Right?

A Yes.

Q Now, these numbers basically refer to the 1.3 percent, and the other information you have about that curve; right?

A Yes.

Q Isn't it true that a 1.3 percent error rate for Type A or B errors is well beyond the expected error rate for good nuclear power plant design?

A Considering the type errors and considering the errors per design element, as we have defined design element, I would not consider it beyond what is expected.

Q So you would consider a 1.3 percent per design element error rate for Class A or B errors to still reflect, generally, high-quality engineering work?

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ki 3:3

A I would consider it yes.

Q Do you recall your deposition that you gave?

A Yes.

Q Do you have a copy of it there?

A No.

Q Do the parties have copies?

(Document distributed to Board and witnesses.)

Q Isn't it true, Dr. Kaplan, that in your deposition you said you would expect errror rates in the range of 1 percent or 10 percent for Class A or B errors?

MR. NORTON: Excuse me, Your Honor. I believe NRC procedure for asking this type of question is to first refer the witness to the deposition, and then ask the question.

JUDGE MOORE: Mr. Strumwasser, it would be helpful sir, so -- I see what Mr. Norton is getting at. Mr. Norton -- other than the fact of fairness to the witness, if you're going to point out a statement in a lengthy deposition, it seems to indicate you might tell him what you were referring to.

I have no objection to him asking the witness that question without referring, Mr. Norton, and the witness can respond he doesn't remember precisely, whatever, as the situation -- and then he can refer him if he wants to pin him down. I think we can proceed that way.

Go ahead, Mr. Strumwasser.

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ki 3:4

BY MR. STRUMWASSER:

Q Dr. Kaplan, in the interest of time, do you recall giving me that answer?

A Vaguely. I don't recall the context in which we were speaking. I'll tell you want I do recall.

I think, as I recall, I was thinking at that time we were talking about an error rate measured in terms of errors per design decision. That's the way I was thinking about it at that time. Out of how many design actions or decisions does a person make , what fraction of those are in error -- here we're talking about a little different notion. We're now talking about elements per design element, which is a very different thing than a design decision.

A design element, as we defined it, may have many decisions.

Q Just so we're all together on this, if you turn to page 54, and you will see at line 8, I asked you about Class A or B errors, what are an appropriate background rate --

A Yes.

Q Then we are favored with a colloquy between Mr. Norton and I. Can you pick up the question at line 24, just A or B, would I have a feeling for how frequently errors of that type ought to be expected in the design process; right?

A Yes.

Q And you say it would be extreme, on page 55, to be

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ki 3:5

1 in 10, and on page 7 you say that typical engineering work is like 1 in 100, 1 in 1,000, that sort of thing.

A Yes.

Q So that is 1 percent to 1/10 of a percent?

A Yes.

Q You say that was per design decision?

A Yes.

Q How many design decisions are you assuming there is in their design element?

A I'm not assuming -- making any assumption at all on that score.

A (Witness Anderson) Some elements, there may be one design decision, and some elements may have many. For example, one element in electrical has 152 to 200 circuits in it. There are many decisions associated with those 200 circuits.

Q So you would not say, then, that an error rate of 1.3 percent per design element was above the range one would expect from nuclear power plant design?

A (Witness Kaplan) That's correct. I did not say that.

Q Now, after the verification program, Dr. Kaplan, isn't it true that errors may still remain undetected in the Phase II design work for Unit 1?

A Yes.

Q Including errors of Type A or B?

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ki 3:6

A Yes.

Q In fact, isn't it your opinion that it's virtually certain they remain undetected A or B errors in the Unit 1 non-seismic design?

A Yes.

Q And the same is true for Unit 2, isn't it?

A Yes.

Q Virtually certain that there remain undetected A or B errors?

A Yes. Beyond the IDVP sample.

Q Right.

Turn to page 20 of your testimony, if you would, please; at lines 21 and 22, you say that minor errors occur all the time.

Is it your testimony, Dr. Kaplan, that in nuclear power plants Class A or B errors occur all the time?

A My testimony is, I'm not surprised to find errors of that type in any engineering work. That's what I'm saying here.

Q Do you expect there to be Class A or B errors in nuclear power plant nonseismic design work?

A I would expect so, sure.

Q So for the design work of nuclear power plants, you would expect all the time that there are errors, design errors, causing licensing criteria to be exceeded?

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ki 3:7

A You are putting a little different coloration on it with the way you phrase it. I am speaking of errors of the type found here, called Type A and B, and I'm saying that I'm not surprised that errors of that type occur in any engineering work fairly frequently.

Q Occur and remain undetected.

A Occur and remain undetected?

JUDGE JOHNSON: Excuse me, Dr. Kaplan. Were you repeating the question or verifying that you agreed --

WITNESS KAPLAN: No, I'm repeating the question. I'm trying to make sense of the question in my mind exactly, because if an error is detected during the design process and it's effectively not made -

BY MR. STRUMWASSER:

Q Is that right?

A (Witness Kaplan) I mean, if I'm designing something and I make a mistake in arithmetic or something, and before it leaves my desk I find it and correct it, would you say that that error occurs or doesn't occur? That notion is a little fuzzy to me.

But I would think that errors of this type remain undetected in almost all engineering work.

Q Well, would you say that the error was made if it left your desk with a Class A or B error, but was detected by a first level reviewer?

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ki 3:8

A I would say I made the error, but the design team, if you will, did not.

Q With respect to A or B errors, is it your testimony that large numbers of Class A or B errors would survive the entire design process?

A Large numbers?

Q Well, any. How about any?

A Any of a typical design process?

Q Of a nuclear power plant.

A Yes, yes. I would think errors of that type would survive the design process. Yes.

Q So it's your testimony that you would expect in a typical nuclear power plant that Class A or B errors woul be made, and escape the quality assurance process?

A Yes.

Q Several dozen?

A Possibly.

A (Witness Anderson) I think we want to go back and explain that further, go back to your definition of "error" as not being design criteria.

We have been assuming that an A or B error is something that does not meet design criteria or commitments in the FSAR. Commitments are often descriptions. So it's very possible that some of these errors, likely that many of these errors would be errors that would not meet some kind of

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ki 3:9

commitment, that was really originally intended to be a description.

Q Well, it fails to meet the specifications of the FSAR, right?

A The criteria and commitments, which include description, yes.

Q Do you share the view, Mr. Anderson, that you would expect several dozen Class A or B errors in the typical nuclear power plant?

A I think with the low threshold of definition of error involved with Class and B in this particular program, I would expect that kind of number. I would expect those kinds of errors to still be found in the plant.

For example, a heating and ventiliating system could be found to have more heat load in a room as a plant starts up and warms up than the calculations indicated. That's very typical in nuclear power plant design, and adjustments are made at the time that that occurs. An adjustment here, if sand pulleys and belts were changed, that would be a Class A error, indicating a modification in the field. Or if calculations were done that showed that the additional heat load would not be detrimental, that would be a Class B error.

I think with that low threshold of errors, I would expect to find a number of Class A and B errors in a typical

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nuclear power plant design.

Q Several dozen such errors in the nonseismic portion of the design?

A It would be possible, yes.

Q Possible or typical?

A Several dozen in nonseismic. I think that would be typical, yes.

Q Can you name a median plant that you think today has several dozen such errors?

MR. NORTON: Objection, Your Honor. What is a mediar plant?

MR. STRUMWASSER: Typical plant.

JUDGE MOORE: Overruled. I think the witness might be able to answer.

WITNESS ANDERSON: I don't think I can.

BY MR. STRUMWASSER:

Q So if I asked you to give me the name of a nuclear power plant in the United States, where you would expect to find, let's say, 30 or 40 Class A or B errors in the nonseismic design work, you couldn't give me such a name?

A (Witness Anderson) I don't think so, relative to a specific number. I'd be very surprised if there were any nuclear plants that have no Class or B errors, but I would not be able to say what plants would have what kind of error rate. I'm just not able to say that.

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ki 3:11

JUDGE JOHNSON: Excuse me. Mr. Strumwasser. Can I follow that a minute?

MR. STRUMWASSER: Sure.

JUDGE JOHNSON: Mr. Anderson, did you ever look at licensing event reports filed by the operators of nuclear power plants?

WITNESS ANDERSON: Yes.

JUDGE JOHNSON: From your review of these, do you ever find LERs which represent what were previously undetected design errors, the detection of which represent the LER?

WITNESS ANDERSON: Yes. I think a number of the LERs deal with design errors of one type or another. Most of them deal with design errors in equipment manufacture, which we are not dealing with here. We're dealing with systems design here, and these errors -- LERs do -- well, it's fairly common to find situations where a plant does not meet the letter of some criteria or design commitment in the FSAR.

JUDGE JOHNSON: But you have not done any sort of systematic review of LERs to determine what any sort of rate of design error might be found or might be forthcoming subsequent to operation?

WITNESS ANDERSON: No, we haven't.

JUDGE BUCK: May I ask one question, Mr. Anderson? In your figure of 30 or 40 errors, are you talking about the ones that survive the preoperational testing, or the ones that

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ki 3:12

are there, and you start to operate and run your preoperational testing, and you find some of these or a lot of them --

WITNESS ANDERSON: I think most of these would be caught in the preoperational testing phase, and that goes into the actual startup of the plant. So preoperational testing would have a very high probability of catching most of those kinds of errors, yes, systems design.

End 3

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mgc 4-1

BY MR. STRUMWASSER:

Q Dr. Kaplan, at Page 20 of your testimony, you refer to safety factors, defense in depth and so on, right?

A (Witness Kaplan) Yes.

Q Is it your testimony that these characteristics of nuclear power plant design are there exclusively to allow for design errors that cause the design to exceed the criteria?

A Not exclusively, but inclusively.

Q Primarily?

A I wouldn't say primarily.

Q Actually, isn't it true that those are there to allow for a whole host of contingencies of which design errors are but one?

A Correct.

Q And you would want that margin of error to be there, even if you were absolutely sure that the design met all its criteria, wouldn't you?

A Well, if the safety margin is there to cover a host of factors, and if you were sure that one factor was not present, you would need less safety margin.

Q Do you have any way of estimating how much less?

A Not off the cuff.

Q Not without a very extensive study, would you?

A Probably so, yes.

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mgc 4-2

Q And in general, when design errors cause criteria to be violated, that takes away the safety margin for other contingencies, doesn't it?

A Yes.

Q Now the 1.3 percent error rate, Dr. Kaplan, that comes from the IDVP results only, right?

A Right.

Q And you point out in your testimony that DCP and the NRC Staff have done additional reviews, right?

A Right.

Q You acknowledge that those reviews are less complete than the IDVP's, do you not?

A I acknowledge that possibility. This is just based on what I have been told.

Q You don't provide any quantitative results of that review, do you?

A That's right.

Q You don't even indicate how many design elements were reviewed.

A Well, there is an indication in Figure 8-1.

Q You don't give us any numbers, do you?

A That's correct.

Q You don't say how many aspects vof the design each element was examined for.

A I don't say that.

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mgc 4-3

Q Okay. You understand that the IDVP sought to test each element it looked at against a wide range of virtually a complete set of criteria in their estimation?

A Yes.

Q And the review that you are describing for the DCP may have been for specific purposes having to do with only some of the design criteria, right?

A It may have. Mr. Anderson would know that better than I.

Q Mr. Anderson, is that true?

A (Witness Anderson) Yes. I think in the internal technical program, we were reviewing these systems looking specifically for errors. We were trying to get documentation, paperwork, up to date. We were reviewing the systems to find errors. If there were going to be errors, we wanted to find them and fix them in the most expeditious way, so we definitely focused our attention on the functional areas, the areas that would be most important and the areas where we would most likely find errors.

Q That's a narrower kind of review than the IDVP did?

A Somewhat narrower than the IDVP, yes.

Q Dr. Kaplan, you don't report how many A and B errors were found in the DCP review, do you?

A (Witness Kaplan) I think I do mention the number

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mgc 4-4

of open items that they found.

Q Right, but are those A and B errors?

A I don't believe they were categorized as an A or a B by the DCP.

Q You don't even try to adjust the probabilities from your Section VI study to take into account the DCP and the NRC reviews, do you?

A That's true.

Q May we safely assume that if you could have quantified these results, and if they had proven favorable, it would have appeared in your testimony?

A No, you may not assume that. I could have quantified it, if I wanted to. If it had come out unfavorably, I would certainly have shown it.

Q Why did you not quantify it?

A Well, the framework under which I did this was that the IDVP was the sample. It was the so-called independent sample. And insofar as there was a statistical-like treatment done in that sample, that is the way I wanted to do that.

After that is all done, we need to recognize that there is additional information around, and so I mention that in Chapter VIII. I didn't attempt to quantify that. I felt the story I wanted to tell was already told, and there was no need to rub it in further.

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A (Witness Anderson) From our perspective, there has been great emphasis placed on independence in this program. The NRC Order and the letter demanded that independence, and basically it has been an independent design verification program.

We did not expect to get much credit for what we have been doing in addition to that independent program. The independent program was to stand on its own, so we felt that there was no need to further describe what we did in the independent program.

Q Dr. Kaplan, have you not --

A (Witness Anderson) In our internal program; I'm sorry.

Q Thank you.

Dr. Kaplan, have you not, in your publications, urged that safety issues be resolved quantitatively?

A (Witness Kaplan) Yes.

Q At Page 4 of your testimony, Dr. Kaplan, you characterize the issue for this Board as being the assessment of the degree of confidence that there are no design deficiencies in the Phase II portion of the plant, which would endanger the public health and safety, right?

A Right.

Q And at Page 67 of your testimony, you say that the Board must decide whether the verification program has

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provided reasonable assurance that no design errors exist in the Phase II portion of the Diablo Canyon plant, which would endanger the public health and safety.

A Yes.

Q Who told you that was the issue?

A No one told me that. That's the issue in my mind.

Q Is that the product of your expertise as a statistician?

A The statement that that is the issue?

Q Right.

A No.

Q As an engineer?

A Yes.

Q What in your experience and training as an engineer enables you to tell this Board what the issue is before it?

A Well, I put myself in the role of the Board here and in a sense I am in that role, because in a sense this is a common decision that we are all making, whether to operate this plant of whether to order further study on it. So I asked myself, "What is the issue governing that?" I asked myself, "Should we order further study of the plant beyond what has been done or not?"

And the real issue relating to that is, Is there

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any error still lurking there that it is important to find? Will I spend more time and resources studying this, if I were the Board? So that is the question. I would only do that if I felt there was something -- a sufficient likelihood that there be something there worth finding.

Q You understand that there are legal standards for what this Board has to address, don't you?

A Yes.

Q So you are basically relying on your engineering expertise to identify for the Board what the legal issue is?

A No. I am saying what I think the underlying question is.

Q You said the choices are between operating the power plant or having further study?

A Well, again, you are making legal distinctions between the specific questions in front of the Board, and I am not really knowledgeable on the fine legalities and procedures and so on. I am thinking as an engineer, as a citizen.

I think the question we are all facing, the community, is, do we operate these plants or do we study them further?

Q You understand that --

A So the truth is, I don't know specifically the

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questions in front of the Board or the specific history, and my interpretation of the underlying question is at least in this part of the hearing that I am concerned with -- I am sure there are many things there, but the part about which I am testifying here, I think the underlying question there is, Should we study more? Is the sample sufficient, complete?

That seems to be the question raised by Mr. Hubbard and the Intervenors here, and I am saying with respect to that, the underlying question is, Should we sample more? Should we study more? Or should we go ahead.

And underlying that is, yes, you should always study more if you think there is something worth studying. If you think you have studied enough, that there is very little likelihood of something serious, of a serious error remaining, then it's not worth further study.

Q Dr. Kaplan, I don't mean to pick on you on legal issues, but one of the clear assumptions of your testimony is that this Board ought not to focus on violations of licensing criteria, but on safety-significant violations of licensing criteria.

Who told you that that was appropriate?

A Nobody told me. And as far as the implication, again I am speaking as an engineer, and I am asking, What are the real issues here? The real issue is safety and

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significant design errors, and that is why I am concerned with it.

The technicality of what is a criterion or what is a description, as Mr. Anderson was saying earlier, I'm not really familiar with that whole area.

So I asked myself the fundamental questions. In fact, those are the questions I asked the people around me when I first got involved in this. I asked Mr. Anderson, What has he found? What is the real safety significance of what has been found? That kind of question.

Q Well, on that kind of question, there has been no systematic study of the safety significance of the errors that have been uncovered, has there?

A I'm not sure I would agree with that. I have read a description of the errors in the Phase II Final Report. I would consider that a reasonable amount of formal study has been given to the significance of these errors.

Q So it is your belief that a competent engineer can determine whether a design error has safety significance by inspection?

A Your word "inspection" minimizes it a little, but I would say by thoughtful consideration, yes.

Q Is that your opinion also for errors leading to common mode failures?

A Thoughtful consideration can determine whether a given error has common mode potential or not.

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Q You do probablistic risk assessments for a living, do you not?

A That's not quite true. I have contributed to the theory and conceptual structure of such studies.

Q Thank you for that correction.

Would not a PRA provide a sounder basis for analyzing significant -- safety-signicance of designer errors?

A Well, a full-blown PRA is the soundest basis we know for assessing risk, but it doesn't follow from that that you need a full-blown PRA to make a judgment on every little thing that comes up. I think a competent engineer can make an assessment on the kinds of errors that were found in the IDVP program.

Q You are familiar with the design of the power plant?

A Familiar with the power plant somewhat.

Q A PRA was done there to assess risks, right?

A Yes.

Q Limerick?

A Yes.

Q A PRA was done there to assess risks?

A Yes.

Q Shoreham, another PRA?

A I understand that such was done, yes.

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Q Indian Point?

A Yes.

Q Do you know why it would be worth the effort to assess the risks formally in those power plants and not at Diablo Canyon?

MR. NORTON: Object. Insufficient foundation that a PRA was never done for Diablo Canyon.

JUDGE MOORE: Lay the foundation for the question.

BY MR. STRUMWASSER:

Q Do either of you know whether a PRA has been done for Diablo?

A (Witness Anderson) Some work was done on the probablistic risk assessment relative to the auxiliary feedwater system. I don't know of any other formal PRAs that were done for this plant.

Q Now I return to the original question. Why was a full-blown PRA required for four plants that I identified and not for Diablo Canyon?

MR. NORTON: Same objection, Your Honor.

JUDGE MOORE: Sustained.

BY MR. STRUMWASSER:

Q Let me make sure I've got this straight.

Mr. Anderson, you testified that as far as you know, no PRA has been done, is that right, other than the limited one you just described?

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A (Witness Anderson) That's correct.

Q If a PRA had been done for Diablo Canyon, would you know about it?

A I think so.

Q Dr. Kaplan, you testified for the other four power plants that PRAs were done to assess the risks at those power plants?

A (Witness Kaplan) Yes.

Q Do you know any reason why -- do you know anything about those four power plants that would suggest that PRAs were necessary there to assess the risks, but not at Diablo Canyon?

A You seem to be asking the question as if I have said that a PRA is not a good thing at Diablo Canyon and should not be done, and asking me to justify why?

Q Should one be done at Diablo Canyon?

Q Well, I think that a PRA is a good tool on all nuclear plants and on many other plants and engineering systems, so I encourage systematic quantitative assessment of risk wherever it can be gained from that kind of assessment.

Q Now a PRA could take into account the possibility of design errors, could it not?

A Yes.

Q And the risks that they present?

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A Yes.

Q So that a PRA for Diablo Canyon would provide us with a sounder basis to assess the safety significance of the errors that have been detected so far, right?

A No, I don't think that follows exactly. I think the kinds of errors that have been found can be assessed without a full-blown PRA.

JUDGE BUCK: Before you go on, could I have somebody define what is meant by "a full-blown PRA"?

MR. STRUMWASSER: I think Dr. Kaplan is eminently qualified.

JUDGE BUCK: You used the term first. I want to know how you used it.

MR. STRUMWASSER: Actually I have been relying on things that I think may have come out of his definition.

BY MR. STRUMWASSER:

Q Why don't you define "a full-blown PRA" for us, if you can?

A (Witness Kaplan) The best definition is by example, and I would offer the Indian Point and Zion probablistic risk assessment studies as an examples.

When I say "full-blown," they are big studies (gesturing).

(Laughter.)

JUDGE BUCK: That's my problem. "A big study"?

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Are you saying that they did this for every system, for every unit, for every piece of equipment?

WITNESS KAPLAN: Just about. You know the question that one poses to oneself when you undertake such a study is, Is it possible that radioactivity could get out into the atmosphere, and how is it possible? What are any conceivable paths or scenarios by which that could happen?

That is within the province of PRA. That is what I mean by "full-blown."

JUDGE BUCK: Only in the respect of radioactivity, certain amounts of radioactivity getting out, in other words?

WITNESS KAPLAN: Yes.

JUDGE BUCK: Thank you.

JUDGE JOHNSON: Can I follow up a little bit more?

Are you intimately familiar with either of those studies?

WITNESS KAPLAN: I am quite familiar.

JUDGE JOHNSON: All right. In those studies, your earlier testimony has indicated that you would not find it unlikely that there be several dozen Class A or B errors in any nuclear power plant; is that correct?

WITNESS KAPLAN: Yes.

JUDGE JOHNSON: Presumably, then, you would expect, for instance, typically to find a similar number of design

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Class A or B errors in Indian Point and in Zion; is that a fair characterization of the things you have been talking about this morning?

WITNESS KAPLAN: Yes. I would say I would have expected to find a similar number of that kind of error in design.

Now when you consider all the operation that has taken place in the years of testing and so on, it may be that most of those that have surfaced have been discovered one way or another and corrected. So I'm not -- I don't know that I am willing to say that there is a similar number --

JUDGE JOHNSON: I understand the distinction you are making, and it seems reasonable.

However, let us assume that there is some number of design errors lurking yet in a plant that has operated for some period of time, that has undergone testing.

You would agree that there is a possibility of that, would you not?

WITNESS KAPLAN: Yes.

JUDGE JOHNSON: In the process of carrying out a probablistic risk assessment study, in what way, if any, are those lurking design errors, Type A or B or any type of design errors, accomodated in the analysis which is performed?

Do you understand my question?

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WITNESS KAPLAN: Yes, I think so.

And the answer is not easily -- it is not at all clear to me how the usual PRA procedures would spot this kind of error.

JUDGE JOHNSON: Is this kind of error built into the failure rate which one uses to carry out a PRA? In other words, do those failure rates incorporate or include as one element --

WITNESS KAPLAN: Yes, I hear you.

The impression I have from scanning Mr. Anderson's description of the Phase II Final Report, his description of the errors that were found, the impression I have is that they would not manifest themselves as changes in the failure rates of equipment that would go into the PRA.

JUDGE JOHNSON: I sort of left -- perhaps improperly, but I sort of left Diablo Canyon behind and was talking about lurking design errors in any nuclear power plant which are not discovered, which were made and not discovered by the QA program or the operational or pre-operational testing.

Now do those errors, lurking errors, show up in a PRA in terms of hypothesized or measured failure rates?

WITNESS KAPLAN: I think the kinds of design errors that result in additional failures of equipment

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are reflected in the historical data we have on failure rates of equipment.

WITNESS ANDERSON: I think from my experience in PRA, and I worked with the Rasmussen team as it put WASH-1400 together, errors this minor would not have shown up in that study directly. But as the team went through and assigned probabilities of equipment failure or system failure, they inherently took conservative positions, much to our dismay at times, but they took conservative positions to account for those kinds of things.

So I think there was some accounting taken for the possibility of design errors in a formal PRA; however, in a full-blown PRA, a large study that we are talking about, it would be very difficult to explicitly cover such minor errors in the design.

JUDGE JOHNSON: Fine.

MR. NORTON: Excuse me, Your Honor. The objections that I made regarding foundation for the risk analysis of the PRA, there is one in evidence in this case on Diablo Canyon. That is Amendment 50 to the FSAR, and it was filed with the NRC, reviewed by the NRC, and was indeed a part of the efforts regarding the low-power license back in the late '70s, far before Mr. Anderson had anything to do with this project, but there is indeed a seismic probablistic risk assessment done for Diablo Canyon, and it

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is in evidence in this case.

JUDGE MOORE: Thank you, Mr. Norton.

Mr. Strumwasser?

BY MR. STRUMWASSER:

Q Dr. Kaplan, the point I was trying to elaborate, perhaps we could tie it up with the questions that the Board asked.

The PRAs with which you are familiar generally assume that a system or component will function as it is designed to function, do they not?

A (Witness Kaplan) Yes.

Q So to the best of your knowledge, to do a PRA that took into the account of design error requires an elaboration of the PRA methodology, does it not?

A I think current PRA methodology doesn't do as good and as explicit a job on the whole issue of design errors as we would like to achieve.

JUDGE JOHNSON: I've got to interrupt again.

I thought in a probablistic risk assessment, you said that systems would function as they were designed to function.

Is there not the probability included in a PRA that the system will not function when called upon?

WITNESS KAPLAN: True.

JUDGE JOHNSON: So every system has such a

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probability associated with it in such an analysis, does it not?

WITNESS KAPLAN: That's correct.

JUDGE JOHNSON: So in every case, then, a system does not work as it is supposed to work when you do a probablistic risk assessment?

WITNESS KAPLAN: Well, that's true. The PRA has in it a frequency, yes, for failure of the system.

MR. HAVIAN: Mr. Chairman, I had an objection to what Mr. Norton said, and Mr. Strumwasser started the question, and I didn't want to interrupt him in the middle of it.

But Mr. Norton's remarks about the existence of a PRA in the FSAR, I believe, are incompetent as evidence, and I believe they are improper. We can't obviously cross-examine Mr. Norton about the nature of the PRA or the scope of that PRA, and I think it's improper for him to state that he knows there is something that he characterizes as a probablistic risk assessment in the FSAR.

The document speaks for itself. If he would care to put on a witness to testify as to what this document is, then I have no problem, but I do have a problem of him offering that kind of testimony and that characterization.

JUDGE MOORE: Your objection is overruled. What is in the record is a matter of record.

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Continue, Mr. Strumwasser.

BY MR. STRUMWASSER:

Q Dr. Kaplan, I think we are back again to this question that you and Dr. Johnson and I have been addressing.

The PRAs that you are familiar with do explicitly address the probability that a system or a component will fail, correct?

A (Witness Kaplan) Correct.

Q Fail to function?

A Yes.

Q But those probabilities are the probabilities that are associated with contingencies other than design errors, are they not?

A Not entirely, as Dr. Johnson has pointed out. This historical record relating to equipment of that sort, failures of equipment of that sort, would include failures that come from design errors.

Q Design error -- the contribution of design errors is not explicitly broken out as a contributing factor to the failure rate?

A Not explicitly broken out.

Q Now would you agree, Dr. Kaplan, that an error's safety significance could elude recognition by a competent engineer?

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A Well, it could. Anything could anything. I think it's unlike.

Q In particular, would you agree that synergistic effects of design errors would be very difficult for an engineer to identify without a formal study?

MR. NORTON: Excuse me, Your Honor. Does that question presume that the design errors are known, that the engineer is looking at, or is he supposed to be addressing synergistic effects of unknown errors?

MR. STRUMWASSER: Of a known error.

JUDGE JOHNSON: I'm sorry, Mr. Strumwasser. I'm having a little trouble understanding. I know what synergism is, but I don't know what the synergistic effect of a design error is. You might want to lay some foundation for that.

MR. STRUMWASSER: Thank you.

BY MR. STRUMWASSER:

Q Dr. Kaplan, in risk analyses, are you familiar with the term "synergistic effects of contingencies" generally?

A (Witness Kaplan) I haven't heard that term used in the context of risk analysis, but I know what you mean.

Q Have you heard the term "synergism" or "synergy"?

A Sure.

Q How was the notion of synergy addressed, or to

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what does it refer in risk analyses?

A Well, as I said, I haven't heard that term used in the risk analysis context, but I think I know what you mean.

Q I'll bet you can do a better job -- I'll bet you're right, and you can define it better than I can.

(Laughter.)

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Why don't you take a shot?

A I think you mean what I call an oddball confluence of effects coming together to cause an undesired result.

JUDGE JOHNSON: I'm going to push that a little bit, because in my understanding, synergism is the concept of nonlinearity and the way you have just described a confluence connotes superposition or activity as far as I'm concerned, and nonlinearity is what we are talking about when we're talking about synergism.

Is that a proper characterization of the word?

WITNESS KAPLAN: No. By "confluence," I think I mean the same thought that you have; nonlinearity, this oddball combination of things just happens to work together to produce an undesirable result. There's a synergistic effect.

JUDGE JOHNSON: An undesirable result which is worse than the additive result of the individual components taken separately?

WITNESS KAPLAN: Yes.

BY MR. STRUMWASSER:

Q Thank you.

With that high-quality definition of synergism now, would you agree synergistic effects of errors, one or more errors, are difficult for a competent engineer to identify without a formal analysis?

A (Witness Kaplan) I would say they are difficult to

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evaluate with formal analysis. The kinds of things you are talking about are very subtle and obscure, so in principle, I would say under all the possible ramifications of some combinations of errors and oddball circumstances, are difficult to foresee.

So I would agree with you as an in principle thing. But when you refer to the kinds of errors found in the IDVP, I think one can say with high confidence that these kinds of errors are unlikely to cause serious results with or without synergy. This is very far-fetched, to think that they would case -- result in any serious safety degredation.

Q Dr. Kaplan, are you familiar with any incidents in the nuclear power industry in the United States where synergistic effects led to reduced safety for operation of power plants?

MR. NORTON: Excuse me, Your Honor. Is that question limited to design errors?

MR. STRUMWASSER: No.

MR. NORTON: May I ask the relevancy?

MR. STRUMWASSER: The question has to do with whether a person can identify synergistic effects' contingencies.

JUDGE MOORE: The question is all right. Overruled.

WITNESS KAPLAN: Yes, I am familiar with circumstances like that.

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BY MR. STRUMWASSER:

Q Why don't you name a couple of them for us?

A (Witness Kaplan) Well, I think Three Mile Island is an example. Browns Ferry is an example.

Q Okay. Now, in those cases, the problem that led to the synergy, if you will, appeared innocuous, did it not?

A I'm not sure how to answer that. Are you saying that people were aware of the individual parts of the problem, consciously aware, and say that these are innocuous; but when they occurred in combination, there was an unforseen synergy, something like that are you saying?

Q Fine.

A I don't know that that's true. I don't know that it's true that people were aware of and thought they were innocuous.

Q Fine. In any event, the synergistic effects of design errors could be more adequately assessed by a PRA than without one; wouldn't you agree?

A Synergistic effects of design errors could be more adequately assessed by a PRA than what --

Q Without one.

A Than without one. Well, again, I will say yes, in principle.

Q Okay. Would you agree also that the PRA --

JUDGE MOORE: Excuse me. How about in practice?

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WITNESS KAPLAN: In practice, as I've said several times this morning, I think the specific category A and B errors we're talking about do not require an extensive PRA to assess their significance.

JUDGE MOORE: Go ahead, Mr. Strumwasser.

BY MR. STRUMWASSER:

Q Would you agree, Dr. Kaplan, that producing a formal PRA helps make the decision process more visible?

A (Witness Kaplan) Which decision process are you speaking of?

Q The decision process that you described before, in which the community assesses the risks of a power plant.

A Sure.

MR. STRUMWASSER: Mr. Chairman, I'm happy to proceed, but if you're looking for a convenient place to stop, this is one.

JUDGE MOORE: Thank you, Mr. Strumwasser. It is about the time we take a break. We will recess for 15 minutes and reconvene at 20 minutes to 11.

(Recess.)

End 6

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JUDGE MOORE: Come to order, please.

Mr. Strumwasser, continue, please.

MR. STRUMWASSER: Thank you.

JUDGE BUCK: Are you now off of synergism, or are we going on?

MR. STRUMWASSER: I'm off of synergism. I have a couple of follow-up questions, but it should not take very long.

BY MR. STRUMWASSER:

Q I believe one or both of you testified that you expected the errors, design errors, Class A or B, might or would be detected in pre-operational or in operation; is that right?

A (Witness Anderson) Yes.

Q Now particularly with respect to pre-operational testing, isn't it true that the systems and components are subjected only to normal operating conditions?

A They are subjected to normal operating conditions primarily, but there are some conditions in the tests that try to simulate operation under accident conditions.

Q But as a general proposition, an error that had to do with, for example, fire protection design would be unlikely to be detected pre-operationally, wouldn't it?

A That's correct.

Q Also in operation itself, right?

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A Yes.

Q The same, for example, for a flood or torando?

A Yes.

Q Earthquake?

A Yes.

Q Now if a design error that we would call a Class A or B error were detected in pre-operation or in operation, would an LER issue?

A In pre-operation, we would not ordinarily issue an LER, but if it was during operation, after fuel had been loaded or a license had been received and an error was detected, normally that would be shown in an LER.

JUDGE MOORE: Excuse me, Mr. Strumwasser. We are having difficulty this morning with our sound system, and I see the gentlemen -- if we could all just stay in place for the moment, so I can get it straightened out.

(Discussion off the record.)

JUDGE MOORE: Let's come to order, please. We apologize for the delay.

Mr. Strumwasser, are you ready to proceed?

MR. STRUMWASSER: Ready.

JUDGE MOORE: Continue, please.

BY MR. STRUMWASSER:

Q Mr. Anderson, just picking up where we were a moment ago, if a design error is detected in pre-operational

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testing that has caused the power plant's design to exceed design criteria, would that fact be reported in any way to the NRC?

A (Witness Anderson) It depends really on when the pre-operational testing was done and what the circumstances would be. If the test was part of the design -- in other words, the designer did the design and clearly specified the test -- it's all part of the design process. I don't think it would be needed to be reported to the NRC.

However, if this occurred after the license had been granted and the plant was in an entirely different status, usually such an error would be reported under an LER.

Q Well, there are sort of canned pre-operational tests that are required in all cases, right?

A Yes.

Q Now if one of those standard tests detects a Class A or B Error as we have used that term here, is it your testimony that that would or would not be required to be reported to the NRC?

A And you are talking about now a Class A or B design error, and not some equipment malfunction kind of thing?

Q Right.

A But a Class A or B design error. I don't think I could really answer that. I think it would depend

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so much on the circumstances. If it was clearly a design error but represented some kind of a breakdown in the process, I think it could very easily be reported to the NRC.

If it was a very minor kind of error that would be expected to be detected by the testing program, then I think there would be no reason to report that to the NRC.

You know, during the testing program, the NRC is intimately involved with the testing process, and the local NRC inspectors would be very much aware of whatever malfunction was occurring during the test and would be aware of the reason for that. So reporting is somewhat difficult to define.

I think the NRC would know about it, just by their normal involvement with the pre-operational testing program.

Q Well, for example, we have a cable separation problem detected in Phase II, right?

A Yes.

Q Is it your opinion that would have been detected in pre-operational testing?

A No, I doubt if that would have shown up in any pre-operational testing. That was a local problem inside of panels where wires did not meet the specific words in the criteria for physical separation. That would not be detected in pre-operational testing.

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Q You had some problems with pressure and pressure cross valves?

A I think that could have been or could be detected in pre-operational testing.

Q And if it had been detected, would it have been reported to the NRC?

A I think the NRC would have known about that, yes.

Q On the basis of your experience, Mr. Anderson, do you know of any nuclear power plant in which several dozen incidents or findings were reported as a result of errors that we would have called A/B in the non-seismic design?

A Again, you are talking about strictly design errors?

Q Right.

A No, I do not know of any plant that has had that number of design errors reported. I think earlier we were talking about the possibility of those kinds of errors existing.

Q Dr. Kaplan, do you know of any plant that has had several dozen A/B design errors reported as a result of pre-operational and operational?

A (Witness Kaplan) No.

MR. STRUMWASSER: Dr. Buck, I am now done with synergy, if you would like to pick that up.

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JUDGE BUCK: I just wanted to ask you a question before we got off the subject.

I would like to know if you have an example or two of what you think of as synergistic occurrences that might occur from the failure, shall we say, of two design errors. Do you have some examples of that in your mind?

MR. STRUMWASSER: I?

JUDGE BUCK: Yes. Does the State have some examples that they can think of of design errors that would cause a synergistic effect?

MR. STRUMWASSER: I only know of their existence. I can't identify them. But now that the question has come up, I will see to it that before we are done here, we will give you some.

JUDGE BUCK: Okay. Thank you.

BY MR. STRUMWASSER:

Q Gentlemen, would you turn to Section VII of your testimony, starting on Page 57. The testimony refers to prior distributions derived from the DCP.

Mr. Anderson, for purposes of this testimony, are you the DCP man?

A (Witness Anderson) Yes, I am representing the DCP.

Q So you speak for these priors?

A Yes.

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Q Mr. Anderson, when did you begin work on Diablo Canyon?

A In April, I believe, of 1982.

Q When did the verification program begin?

A Well, I think it started really in September. It was formalized more in November of '81, and it was getting rolling in March and April of '82.

Q Does that include the non-seismic work?

A Yes, that includes the non-seismic work.

Q When were you asked about these prior distributions?

A Well, when Dr. Kaplan was asked to participate, this was in the spring of this year, we started thinking of what that number might have been, tried to estimate what that prior was.

Q He asked you for prior distributions this past spring?

A We were talking about this, these kinds of notions, what kind of error rate we might expect in design errors in these particular systems in that timeframe, yes.

Q So, Mr. Anderson, these were not prior at all, were they? They were done after you were familiar with the results of the verification program, right?

A That's correct.

Q And to do what Dr. Kaplan asked, you had to

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pretend that you didn't know about the last two years' results of the verification program, right?

A Yes. I tried to think back when we first came on the project in March or April of '82, what we found, what the condition -- what our first assessment really was of the design work in the non-seismic area, and I think that that was generally the basis -- that thought process was generally the basis for our number here.

Q Dr. Kaplan, prior distributions are supposed to come before the evidence is seen, aren't they?

A (Witness Kaplan) Well, I think you are laying a little too much stress on the notion of time involved in prior. It doesn't quite necessarily mean earlier in time. It means without this information.

Q You can understand the source of my emphasis, right?

A Yes, I can.

Q It does appear in the word "prior"?

A Yes, I can, but I am explaining that that isn't the real essence of the meaning. Perhaps the choice of word isn't optimal in that respect. It doesn't necessarily mean earlier in time.

What it really means is absent the specific information in question.

Q The term "prior" is a widely accepted term for

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Bayesian analysis?

A That is correct.

Q To the extent that the DCP and Mr. Anderson and his colleagues were unable to exclude evidence of the verification program's results from their concoction of their prior distributions, then to that extent they would be double-counting the results of the verification program, wouldn't they?

A Well, your premis is that they were able to exclude.

Q Right.

A So if they were unable, then I would have to agree that they were unable.

Q The result would be to double-count the evidence, right?

A If they counted the evidence both in their prior and in the likelihood function, then they have counted it twice.

Q That would render the posterior distribution invalid, wouldn't it?

A If that were done.

Q Now, Mr. Anderson, are you confident that you could exclude from your mind the influence of the last two years' results when you gave Dr. Kaplan your priors?

A (Witness Anderson) We certainly tried. I'm not

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entirely sure we were able to completely exclude all we have learned in the last two years.

We tried to go back and remember our initial impressions of what we found when we looked at the systems and when we talked to the engineers that were involved in the design of those systems.

Q That was as of April '82?

A That was as of April '82 when Bechtel was first brought on to the project.

Q As of April '82, there were already some results from the verification program, weren't there?

A Relatively few. The independent program at that point did not have any results. There was certainly some information that had been pulled together by the PG&E people in reviewing the systems, but I wouldn't say there were excessive amounts of results, a large amount of results available at that time.

Q What is excessive?

A Well, maybe that was a poor choice of words. I really don't think that there were any conclusions that were given to us as to the quality or the completeness of the engineering work. We simply looked at the plant as we saw it, kind of almost like a fresh look, and we looked at the systems, we talked to the people, and we arrived at some conclusions initially that the system work looked

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pretty good.

Q Well, the preliminary looking that you are talking about, is that included within the evidence that is reported by Dr. Kaplan as the evidence added to the prior, do either of you know?

A I think that's included. I think that's the

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condition that we found the plant in when we first looked at it, before this extensive verification program was done.

Q So that was a part of the information that you used to come up with your prior?

A Yes.

Q And that is part of the evidence, Dr. Kaplan, that are using to adjust the prior?

A (Witness Kaplan) You'd better say this again. I've lost the thread here.

Which is the part of the evidence that you are speaking of?

Q Those portions of the verification program that had been done prior to April '82.

A I asked Mr. Anderson and the DCP to encode in the prior number their state of confidence, based on everything they know, other than the results of the IDVP.

Now have you gotten Mr. Anderson to say that his number is not that?

(Laughter.)

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Q What Mr. Anderson has told me is that he had the results of some verification work done by PG&E prior to April '82.

± A (Witness Anderson) I don't think I said that. I don't think I said we had results. I said we had some information available. I don't think we had results. They had been pulling together information, so it was a little easier to look at the systems, the files, the information that one would want to look at in reviewing the design process, but I don't think at that point there were any results of the verification program that were available.

Q How did that information, those files and so forth, contribute to your formulation of a prior?

A Well, it made it very easy for us to look at the systems, see what they had, what they had done, and identify the people that had done it, talk to the people, get an assessment, a ,quick assessment, of where we stood on the systems design for the plant.

Q I understand how that would contribute to an auditing kind of effort, but what does that tell you about the probability of there being a safety-significant design error?

A I think if we looked at the engineering work and it looked generally good, that would generally give us a feel that there was a minimal probability of a major

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design error.

I think, in addition to what we looked at at that time, we also knew of PG&E's involvement in systems design on fossil plants. They had been doing their own fossil plant design for many years. We knew they had good systems designers and were a very competent company in the systems design area.

So that influenced our understanding. And then as we came on to the project and looked at the work, looked at the systems, we just gained further confidence that the systems work was done quite well.

Q So your evaluation of the engineering documentation that you looked at, that you looked at in April of '82, contributed to your prior?

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A Yes, I believe it did.

Q Now, Dr. Kaplan -- strike that.

You have identified the engineering work you looked at and the PG&E history as factors you took into account when you formulated your prior; correct?

A (Witness Anderson) Are you speaking to Dr. Kaplan?

Q Mr. Anderson, I'm sorry.

A Would you repeat that question?

Q You said you identified the engineering work you examined in April of '82, plus your knowledge of PG&E's history in engineering work, in engineering design, as factors you took into account in formulating your prior.

A Yes.

Q Anything else?

A I think that we looked at our experience in the industry as to the probability of a large error occurring in these kinds of systems, basically supporting kinds of systems, and that contributed to our prior, too, our judgment from other jobs.

We talked to a number of people in our own organization, people that have been involved in the safety area of nuclear power plants for many years, people that have been involved in probabilistic risk assessment, and we got some information from them that contributed to that number.

Q When did that talking take place?

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A When we were putting this together, June, July, August of this year.

Q Anything else that you took into account?

A I can't think of anything else.

Q Okay. So you got the engineering documentation you looked at in April of '82, PG&E's history in designing, other kinds of plans and industry experiences, other factors; right?

A Yes.

Q And solely on that basis, you came up with a 99.9 percent certainty that there did not exist any Region 2 or 3 errors?

A Yes. One chance in a thousand that there would be a significant design error in the system.

Q Did you consider, when you formulated your prior, the seismic design errors known as of late 1981, early 1982?

A I don't think we considered those directly. Certainly that relates to, as we talked about before, the overall quality assurance program. But we did not consider that directly, because the seismic technology is so unique and so different from the systems design, that it was not -- the seismic errors would not be a major factor in out asssessment of the systems design errors.

Q Did you take into account the cause of the seismic errors?

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A I think we did.

Q Is there anything esoteric or technologically advanced about the cause of the reverse blueprint error?

A No, I don't think in those terms, that there is anything particularly unique. However, the seismic design, with all the changes and all of the difficulties involved in seismic design, of judging where you are -- it's, you know, a very complex analytical effort, a numbers game if you will, and it has some unique features having taken place over a long period of time. But we certainly considered it in that regard.

Q How did the reverse blueprint error happen, Mr. Anderson?

A I don't really know. I was not on the project at that time. I'm told that it was a communications problem within the design groups between PG&E and their service-related contracted.

Q Are you aware that a diagram was sent to a service-related contractor for Unit 2 without a marking of which unit it was?

A I believe that's the general essence of the problem, yes.

Q Are you aware that there were found to be general -- generally inadequate document control procedures?

A I believe some inadequacies were found in those

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areas, yes.

Q Do you know how the reverse blueprint error was found?

A Not specifically, no.

Q Do you know whether it was found as part of the QA program?

A In the broader sense, it may have been found as part of the QA program; someone reviewing, looking, checking.

Q Okay.

Mr. Anderson, the reverse blueprint error was found by accident, wasn't it?

A I'm not sure of that.

Q If I told you that, would that influence the prior that you chose of 99.9 percent?

A No, I don't think it would, because again I think the PG&E engineers were very familiar with systems design. That's a different aspect of nuclear power plant design than the seismic design.

Q What assumptions were you making about the QA program when you came up with your priors?

A Well, we were making the assumption that some breakdown had occurred in the QA program, the formalized QA program, and I think we knew that. Yes, we did consider that.

Q Now, standing in late November 1981, knowing

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about the reverse blueprint problem and the way it occurred, knowing that there had been deficiencies in the PG&E QA program, knowing about the incorrect calculation of masses in the annulus -- are you familiar with that?

A Yes.

Q You were still prepared to say that there was only a 1 in 1,000 chance that there was a safety-significant error in the nonseismic design of Unit 1?

A Yes, I believe that's true.

Q Dr. Kaplan, would you believe that 1 in 1,000 is a very strong prior?

A (Witness Kaplan) It is what it is. I don't know how to put those words on it. It's 1 in 1,000.

Q Is the term "strong prior," is that a term used in the community in which you work?

A Well, strong compared to what? In some circumstances it would be a weak prior; in others it would be a strong prior.

Q But the term "strong prior" is a term that's widely used in Baysian analysis, isn't it?

A I don't know. I haven't seen it very often, but I will concede it. So what?

JUDGE JOHNSON: Can I interject here? Your equation, for instance, on page 59, 7-4, the results -- since the P-0, you choose P-1 to be .001, P-0 is a number very close

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to 1; is that not correct? It's .999?

WITNESS KAPLAN: Yes.

JUDGE JOHNSON: So that, given the formulation you have there, is not your result almost directly proportional to your prior? Is it not influenced only slightly by the subsequent information?

WITNESS KAPLAN: In this case, the number is almost exactly as you say. It is almost directly proportional to the prior.

JUDGE JOHNSON: In other words, the effect of the prior in the numerator --

WITNESS KAPLAN: Is the dominant effect.

JUDGE JOHNSON: So if you chose .002, the answer would simply be .0014.

WITNESS KAPLAN: That's right.

JUDGE JOHNSON: Would this be a characteristic of a strong prior? And I am not at all familiar with the term, but I am probing to see if I understand what Mr. Strumwasser is getting at.

WITNESS KAPLAN: I think he is saying that we have very high confidence that N equals 0. That's what I mean by the word "strong," and very high is 999.

JUDGE JOHNSON: You say you have a high probability that P-0 is equal to a number close to 1? P-0, the probability of not finding any?

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WITNESS KAPLAN: Yes. The probability that there are none. The probability N equals 0 is .999. And Mr. Strumwasser wants to call that strong, I'm willing to accept that term.

JUDGE JOHNSON: But even if it increased by a factor of 10, it would still be -- it would still dominate, or the result, the dominant effect of the difference between one large number and one small number would still be felt in this equation or in this formulation, would it not?

WITNESS KAPLAN: Yes.

JUDGE JOHNSON: Why don't you carry on, Mr. Strumwasser?

MR. STRUMWASSER: Thank you.

BY MR. STRUMWASSER:

Q Dr. Kaplan, is it fair to say that the prior that Mr. Anderson has given you indicates that he is virtually certain there are no safety significant errors?

A (Witness Kaplan) He is .999 certain.

Q Would you call that figure a point estimate?

A No.

Q Does it not stand in analytical terms for a curve?

A No.

Q It reflects absolute certainty there's no lack of confidence in that number, right?

A That's correct. There is no uncertainty in

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probability. Probability is the language we use to express our degree of certainty. There is no uncertainty in uncertainty, in our language, at least in the language the way I use it.

Q Right.

(Laughter.)

A You may have a meaning to it. If you'll explain that, I'll be happy to talk that way.

Q I certainly want to use yours. Are you familiar, Dr. Kaplan, with applications in which one uses a curve for a prior distribution?

A Of course.

Q Now --

A We have a curve here, in effect.

Q That's right, but it's a discrete curve, right?

A Correct.

Q That is to say, if you are plotting certainty -- well, what would the ordinates of the curve be?

A N. The abscissa of the curve would be N.

Q N, the number of errors?

A Yes.

Q And the vertical axis would be probability?

A Yes.

Q You would have a column of zero width, 1.0 tall, that N was zero -- right? Excuse me. You'd have a column .99 -- .9 tall; right?

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A Yes.

Q Now, Mr. Anderson and his colleagues could have told you I am just plain certain that the odds were no more than 1 in 1,000; right? They could have expressed certainty in their voice, in the way they described their prior; right?

I'm just asking you to acknowledge that that is a way in which they could have expressed their prior.

A No. I would not have permitted them to speak that way, because that kind of language doesn't fit within the conceptual framework that I am using here.

Q What do you mean by that?

A I mean by that what I said a minute ago; that probability is the language we use to communicate our degree of confidence. And within that understanding, to speak about uncertainty in probability doesn't mean anything.

Q Okay. But one could have had a discrete curve in which -- strike that.

How would you as a Baysian doing this kind of analysis, capture the relative certainty -- strike that also.

The teleology is giving me difficulty. You do not, then, acknowledge the possibility that Mr. Anderson could have said 1 in 1,000 for the role of probability with varying degrees of certainty?

A See, that's not only not a possibility, Mr. Strumwasser, it doesn't make sense. It's nonsense, talk,

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if you know what I mean.

Given the meaning of the words we are using, there is no meaning to saying that I am uncertain about the probability. So it's just a nonsense utterance; there's no sense to it.

Q Did Mr. Anderson know that? Mr. Anderson, did you know that?

(Laughter.)

A (Witness Anderson) We just gave our best judgment as a number.

Q Can you, Mr. Anderson, conceive of the possibility that you would have said yes, 1 in 1,000, but I'm not very sure about it?

A I think we were fairly sure that having the number that high, it would certainly be what we believe what our confidence level is. We thought perhaps about numbers lower, but considering all the factors, we thought that that number 1 in 1,000 was a reasonable number.

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Q And in fact, you thought that it was virtually certain that there was not more than one safety-significant error, right?

A Yes, I think we believed that.

Q So you thought it was basically impossible for there to be more than one safety-significant error?

A It was very unlikely that there would be more than one. I am not willing to say that it's impossible.

Q Well, the probability was negligible?

A I think we can say that, yes.

Q If someone were to say to you that his prior would have been only .99, would you have felt strongly that that person was wrong?

A (Witness Kaplan) Who are you asking?

Q I am asking Mr. Anderson.

A (Witness Anderson) Yes, I think that that's too high, one in a hundred. I would feel fairly strongly that that was unreasonable.

Q And if someone were to say that his prior was only .9, one in ten, that there was a safety-significant error, given what we knew in late November 1981, you would have felt sure that that figure was too low?

A I would have felt very sure that that figure was wrong, knowing the systems and how they operate, and the testing and all that that has gone one. I think that

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would be very unlikely?

Q What would your prior have been for the likelihood of a reverse blueprint error before you discovered it?

A Of that specific error, without regard to its safety significance of an error like that, is that what you mean?

Q Yes, an error like that, an error of that magnitude of safety significance, however you assess that.

A Well, it would be -higher than one in a thousand, maybe in the range of one in a hundred or one in ten, something like that.

Q Before it was discovered, why would you have had any different number?

A Well, I think again the nature of seismic design and the complexity of transfer of information of the numbers all the way through the seismic analysis would make that type of engineering work much more prone to that kind of error.

Q What about the seismic design of Westinghouse equipment?

A Equipment seismic design is quite different. That is very specific to a piece of equipment or a local system. It doesn't involve a whole large structure that is quite different from other structures. It's just not the

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same kind of engineering work, as I understand Westinghouse's detailed seismic design.

Q Even something as complex and large as the NSSS?

A Well, the NSSS is complex and large, but from a seismic analysis point of view, it's rather focused on the major equipment and some rather well-defined piping systems.

Q In November of 1981, November 1, 1981 -- let's do it earlier than that -- mid-1981, what would your prior have been that there was an interface error in the seismic design of the NSSS, leading to erroneous calculations?

A It would be very low.

Q Now you have assigned this probability of one in a thousand for Reg 2 or 3 error, right?

A Yes.

Q And you have used Dr. Kaplan's definition of Region 2 as jeopardizing the plant and Region 3 as public health and safety, right?

A Yes.

Q You would say the prior likelihood of a Region 1 error would be substantially higher, wouldn't you?

A Yes.

Q Isn't it true that in your opinion none of the Diablo Canyon design errors revealed in the last two years

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would be a Region 2 or 3 error?

A I think that's correct.

Q So to qualify for Region 2 or 3, an error would have to be more safety-significant than a reverse blueprint error?

A Yes.

Q And it would have to be more safety-significant than the incorrect calculation of masses in the annulus?

A Yes.

Q And if we asked you for a prior distribution for errors of safety significance comparable to those errors, you would give us a substnatially higher value than one in a thousand, wouldn't you?

A Well, I think that it would depend on what kind of engineering activity you are looking for errors in. I have said that in seismic design, where there is a great deal of emphasis on numbers and transfer of information and complex calculations, I think the number would be higher; however, in systems design, I don't think that's the case. I think the number is indeed lower in the systems design area.

Q Mr. Anderson, last Monday you testified that Phase II produced fewer errors than you would have expected, right?

A Yes.

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Q Now according to Dr. Kaplan's synthesis of the IDVP results, there was a 1.3 percent error rate, right?

A The way he has defined that, yes.

Q So your prior distribution, if you asked for one for the rate of A or B errors before seeing the IDVP results would have been higher than 1.3 percent per design element, right?

A I don't know that. I think that may be a little higher, yes.

Q Can you say how much higher?

A No. Offhand, I don't think I can say how much higher.

Q Just for purposes of terminology here, let's talk about the likelihood of detection, and I will define that as the probability that if there exists an error, it will be in the sample, okay? Is that understood by you gentlemen?

A Yes.

Q Dr. Kaplan, you got such a likelihood of detection estimate from the DCP, right?

A (Witness Kaplan) Yes.

Q That was .85, right?

A Right.

Q That means that if there is a safety-significant error, it is nearly six times as likely to be in the IDVP

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sample as not to be in the sample?

A In the judgment of the DCP people.

Q Mr. Anderson, what is the basis of that assessment?

A (Witness Anderson) Well, the basis primarily is that the systems that were sampled represent a very comprehensive look at the engineering process. As we mentioned before, the systems have complex interfaces. They have a wide range of engineering activities. I think the review of those systems gives a very good picture or a very good understanding of the engineering work that was being done, and that's why it would be very unlikely that some systematic error or some error of significance would show up in other systems that were not reviewed and would not show up in the systems that were reviewed.

Q A safety-significant error does not have to be systematic, does it?

A Not necessarily, no.

Q But you are confident that even if it was not systematic, that a safety-significant error would be six times as likely to appear in the IDVP estimates as not?

A I think that is our judgment, based on the thoroughness of the IDVP review of the engineering process that was actually used at Diablo Canyon.

Q Now if someone else said that he thought it was, say, only 70 percent instead of 85 percent likely that you

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would detect it, would you feel strongly that that person was wrong?

A Seventy percent rather than 85 percent?

Q Right.

A No, I don't think I would feel strongly about 70 percent.

Q Dr. Kaplan, as a Bayesian, when you receive a prior distribution, shouldn't you adjust the prior before using it to incorporate any information that you have about the prior?

A (Witness Kaplan) You mean, should I adjust it to reflect my own --

Q Not your own.

A -- prior information?

Q Your own information about the prior.

A My own information about the prior? Your language confuses me. The prior is the language in which I express my information.

Q It's the language in which Mr. Anderson reflects --

A That's his prior. His prior would reflect his information; my prior would reflect mine.

Q That is the point I am trying to get at here.

If you were going to use Bayes' Theorem and you wanted to collect information from Mr. Anderson to build

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a prior and if you had information about Mr. Anderson and his prior, you would want to adjust the raw number he gives you before using it, wouldn't you?

A If I were using Bayes' Theorem to reflect my state of knowledge, then I would put in my prior.

Q I take it your testimony is intended to help the Board to assess these probablistic issues, right?

A That's correct.

Q The Board ought to be at liberty, shouldn't it, to adjust the prior?

A They are not only at liberty, I am sure they will.

Q Well, but in the framework which you have laid out here, given that you have not identified for the Board anyplace where it may adjust Mr. Anderson's prior, have you?

A I beg to differ. I have identified that.

Q Can you tell me where in your testimony you do so?

A Sure. I say in here the Board will have to use its own prior. As a matter of information, we give the DCP's prior.

Q Where is that?

A Page 57, beginning on Line 5, or at the top of the page, "The set of numbers, p(n), for each n is the prior distribution. It expresses our state of knowledge before having the results of the IDVP program, and as such, must reflect our knowledge of the plant and the design

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process. Different people may have different prior distributions here. Ultimately, the decision-makers, the panel of judges in this case, will have to use their own prior. As a matter of interest, however, I have held discussions with a number of members of the DCP."

Q If you were going to undertake this, if you were the judge, so to say, would you adjust Mr. Anderson's prior before using it in Bayes' Theorem?

A That's a very good question. It's a general situation of the person in the position of making a decision, and he has, as part of his information input, the opinions of experts, I think it's almost always true that the decision-maker tends to discount somewhat the opinions of his experts. How much depends on the degree of confidence he has in his experts, but ultimately he has to use his own judgment, and part of what he knows is that sometimes experts are wrong. More often they are right or "righter" than he is, so all that has to be put together in his mind.

The example I like to use here is, if you and I were going to drill for oil, all right, and we had bought a piece of ground with the rights to drill on it, and we engaged a geologist, and we asked him how much oil is there under this location, if we drill here, how much oil are we going to find, and if he is a geologist schooled in probablistic thinking, he won't answer with a number --

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that is, he won't give a point estimate; he will give you a probability curve with reflects his state of knowledge about how much oil is down there, and that state of knowledge would reflect all the information he has, whatever testing he has done, his knowledge of the geology and all his experience.

So he would give a probability curve against the abscissa in this case, which would be the number of barrels.

All right, now you and I are the decision-makers because it is our money that is going to pay for the drilling, and we have his probability curve. I think it's inevitable that in our minds the probability curve would be somewhat broader than the experts, because we don't have his knowledge. We don't have his depth of experience. We recognize that he knows a lot more than we do, but we are not as confident as he is. We can't be; we don't have his information base.

So the probability curve in our minds is somewhat broader than the expert's, and I think that is a general phenomenon. So the answer to your question is, the decision-maker almost always has a somewhat broader probability curve than his experts, and that's the way it is, and that's sensible. That's the way it should be.

Q Perhaps we can help the Board make use of the

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methodology you have here. You received -- that is to say, we all, I guess, have received the DCP's one in a thousand prior, right?

A Yes.

Q Can you identify any bases by which it should be adjusted, if you were the decision-maker, if you were the Board?

A If I were the decision-maker, I would go through the same kind of thinking that the DCP went through. I would try to learn as much about the design, about the likelihood of serious errors occurring in this portion of the design, this Phase II portion of the design, the likelihood that they could escape detection through the extensive testing process. I would review the process.

Q Excuse me. We are talking about November of '81, right; that's when the prior was to be assessed, right?

A No. You stress too much the notion of time in the word "prior." That isn't really what it means.

Q The information available in November of '81.

A That doesn't mean that exactly either. It's a little bit of a misinterpretation, and I will concede that the word "prior" facilitates that misinterpretation, but that's not really what it means in Bayes' Theorem. It doesn't necessarily mean earlier in time. It really means that you encode all of your information. You take

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everything you know, and you put it in two pots. One pot is what we call the current information, and the other pot is everything else, everything that we haven't singled out for specific treatment, okay? So we single out some of this information, and we call that current information. That is going to get specific treatment in Bayes' Theorem. Everything else is encoded in the prior.

So the real question that I put to the DCP and Mr. Anderson is, setting aside the information from the design verification program, the Phase II, encode everything else you know as a state of confidence in this number that we are calling a prior, so that everything else he knows includes his knowledge of the testing program and the testing results and his knowledge of the design of the plant, the likelihood of errors occurring in this portion of the design, his knowledge of the design process and the people who carried it out, and his knowledge of the way the plant works and whatever we take for errors occurring in this portion of the plant to be of safety significance.

Q Let's return to the .001 number that you got from Mr. Anderson.

A Yes.

Q You knew it came from DCP personnel, right?

A Yes.

Q You knew as a general proposition that those

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personnel want to see the Diablo Canyon power plant licensed, right?

MR. NORTON: May we have some foundation for that, Your Honor?

JUDGE MOORE: I think the witness can understand it. We don't need any foundation on it.

WITNESS KAPLAN: Well, you know, I don't have any first-hand evidence on that score. I haven't interviewed people or anything, but I would guess that most of them are in favor of licensing the plant.

BY MR. STRUMWASSER:

Q They had some knowledge that their information was going to be used in your testimony here, didn't they?

A (Witness Kaplan) Yes.

Q As a general proposition, they could be expected to understand that if their priors had expressed suspicions of many design errors, that that would affect the licensing?

A Yes.

Q Would that not tend to bias their priors downward?

A Mr. Strumwasser, I don't think so. You are suggesting that the people in question here were basically inaccurate or not quite honest. Aren't you suggesting that in giving me their priors?

Q Thank you for raising that point. When I say

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they were biased, I don't mean to have any of the perjorative conotations that one might find in a casual use of that term. I am talking about bias in a statistical sense; that is, it tends to move -- it tends to have an influence on the figure you are going to be given in one direction or the other, rather than just randomly in either direction.

A Yes, I understand that. You are not talking about a random bias. You are suggesting that the people who gave me their opinions here did not give me their accurate opinion because of the effect the accurate opinion might have on the board. Isn't that what you are suggesting?

Q No. I am suggesting that it might very well be unintentionally biased.

A They may be optimistic in their assessment; is that what you are saying?

Q They might be reluctant to give you a frank assessment of the probabilities, and they might be unaware of that reluctance on their part.

A Yes. Well, there are two things. "Reluctance" to me means that I am aware, and I am not going to do it. So that suggestion to me means that they were coloring their statements because of the effect it would have on the Board.

Q I don't mean that. I don't necessarily mean that.

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A But it may be a possibility?

Q I want to allow for the possibility that they are coloring it without intending to do so -- without knowing it.

A All right. So now we have come down to, they are optimistic. Would you agree with that?

JUDGE JOHNSON: May I interrupt a minute, because it seems to me that you are trying to walk both sides of the street, Mr. Strumwasser, with your question.

To me, there are two types of bias, and maybe if we discuss them in that way, we can make the question and the answer more clear.

There is the unconscious or unintentional bias that a person who is involved in the design of power plants might have that would cause him to be perhaps optimistic in the way he would assess this probability.

And then there is the type of bias which one who might like to influence decision-makers would use, and it seems to me that you are sort of using those interchangeably.

So I think if you make that clear distinction for the witness between which type you are discussing at each moment, it might help us eventually figure out what the answers are.

MR. STRUMWASSER: Thank you. I appreciate that.

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BY MR. STRUMWASSER:

Q Let's talk, then, Dr. Kaplan, about the unintentional optimism, unintentional over-optimism.

A (Witness Kaplan) Okay.

Q Are there any grounds for suspecting DCP personnel might be unintentionally optimistic?

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A Well, there are grounds -- if you're asking me whether the net -- you know, there are grounds on both sides of the scale. If you're asking me whether the net result of all of that is that I think the DCP people were optimistic, I don't know. I haven't really reflected on that deeply.

Q So you don't know whether you would adjust the prior given to you by the DCP if you were the decision-maker, to compensate for possible over-optimism?

A I think I have said, as a decision-maker, I almost always broaden the distribution given to me by experts.

S2 BU

Q Broaden the distribution is different. "Broaden" means you would move the distribution so that it would be both more and less optimistic. I'm discussing here a bias that is sytematic only; only one side of the curve is affected.

A In this case, there only is one side of this curve; N equals 0, N equals 1, N equals 2. There's no minus side of the curve, so "broaden" means that you raise the probability that N equals 1.

Q You don't know whether you would do that if you were the decision-maker?

A I probably would. I want to say on that that, in a sense, I already have. In the sense that during our conversations, during my conversations with the DCP people, their numbers, the numbers of the various individuals, were all on the low side of .001, and the number I put in the report

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was sort of the high side of the range of numbers of the individuals in the group, so in that sense, I have already reflected the kind of adjustment you are talking about.

Q So the DCP people give you ranges?

A No. There were various individuals in the discussion, and the different individuals had different numbers in mind.

Q Now, we have talked about the adjustment to the prior to compensate for an intentional over-optimism. Would you make any adjustment for the purpose of what Dr. Johnson refers to as attempting to influence the decision process?

A No. My own feeling is of that, that the people in question gave me their accurate reflection of what was in their minds. Their accurate reflection, with no attempt to influence the Board --

Q How would you know if they were off?

MR. NORTON: Excuse me, Your Honor. Could the witness finish his answers before Mr. Strumwasser asks the next question? He was in the middle of a sentence.

MR. STRUMWASSER: I'm sorry. I couldn't tell that.

BY MR. STRUMWASSER:

Q Go ahead.

A (Witness Kaplan) Getting down to blunter language, I don't think that the people in question were dishonest. You're asking me how I would know whether they are or not?

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Short of a lie detector test or a voice analyzer, or something like that, you know, one uses the kinds of cues he always uses in dealing with people. You get a sense of who you are dealing with and what kind of integrity they have.

Q Dr. Kaplan, were any of the people whom you asked involved in the original PG&E design?

A I don't know that.

Q Were any of them PG&E employees?

A Some of them were.

Q Do you know of any --

JUDGE BUCK: May I ask a question here just a moment?

Were the PG&E employees the ones that would be working in the plant; assuming that it was to operate, would they be working in the plant after operation?

WITNESS ANDERSON: No, I don't think we talked to anyone in that category. There were some PG&E employees that were part of the discussion. I don't know of any PG&E employee that was part of the original design.

JUDGE BUCK: I'm not talking about the original design. I'm talking about the PG&E employees. They were the ones that would expect to be concerned about the plant afterwards, or working in the plant afterwards?

WITNESS ANDERSON: Working with the plant in engineering, yes.

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JUDGE BUCK: What proportion of those people were PG&E? Do you know, Dr. Kaplan?

WITNESS KAPLAN: No, I don't.

JUDGE BUCK: Do you know, Mr. Anderson?

WITNESS ANDERSON: No. Just a very few.

JUDGE BUCK: Very few of the group that Dr. Kaplan talked to?

WITNESS ANDERSON: Yes. Very few of that group.

JUDGE BUCK: What were the rest?

WITNESS ANDERSON: The rest were people on the Diablo Canyon Project now.

JUDGE BUCK: They would be what -- Bechtel employees and --

WITNESS ANDERSON: And current -- yes, mainly Bechtel employees. Current PG&E employees.

JUDGE BUCK: And the current PG&E employees I assume you would expect to be there after the plant operated, is that correct?

MR. STRUMWASSER: Excuse me. Where is "there"?

WITNESS ANDERSON: I think we would expect them to be involved with engineering on this plant after it operates, assuming there's engineering work to do.

JUDGE BUCK: Not necessarily at the plant?

WITNESS ANDERSON: Not necessarily down at the plant, no.

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JUDGE BUCK: Thank you.

BY MR. STRUMWASSER:

Q Mr. Anderson, the people who were involved in the original design are still around PG&E; right?

A (Witness Anderson) Some of them are, yes.

Q And the people who were asked about these priors, would you expect them to know that a higher prior would tend to be a reflection on the design work of their colleagues?

A I would expect them to have some understanding of that, yes.

Q Dr. Kaplan, would that tend to bias the prior downward?

A (Witness Kaplan) Well, I don't know. I can't get inside people's heads. I guess all I want to say is, in the discussions that took place here, I saw no evidence of that. I saw nobody say anything that suggested that.

Q Okay. Do you know anything, Dr. Kaplan, about the PG&E/Bechtel financial arrangements for Diablo Canyon?

A No.

Q Do you, Mr. Anderson?

A (Witness Anderson) I know somethirg about that, yes.

Q Do you know whether Bechtel has a financial incentive in getting the plant operating quicker?

A Well, there was originally a financial incentive, but I think those dates have long passed. I really don't

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think there is much financial incentive to Bechtel at this point, at least for Unit 1; there may be some for Unit 2, following Unit 1, but not for Unit 1 that I know of.

Q Is there any financial disincentive if it takes longer?

A No, not to my knowledge.

Q As far as you know, all the penalty -- penalties in the contract that are associated with delays in licensing have already been incurred?

MR. NORTON: Object. There's no foundation that there are any penalties in the contract.

JUDGE MOORE: You better lay the foundation.

BY MR. STRUMWASSER:

Q Do you know, Mr. Anderson, whether the contract provides for penalties if the licensing takes longer than the specified duration?

A (Witness Anderson) I do not believe it provides for any penalties of any kind.

Q Just lost incentives?

A Yes.

Q Dr. Kaplan, you know, do you not, from your research you yourself have participated in, that experts tend to underestimate failure rates in nuclear power plants?

A (Witness Kaplan) That's a little bit of a strong statement. There is some suggestion to that effect.

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Q Well, there is some --

A I wouldn't make a flat conclusion about that.

Q There is some literature to that effect, is there not?

A Yes.

Q Some of which you have contributed to?

A Yes.

Q Now, in terms of what we are doing here, that literature would suggest again that the DCP estimates of likelihood of errors, safety-significant errors, would be biased downwards, wouldn't they?

A I think out of that literature, one could draw the suggestion that the DCP prior is optimstic.

Q If you were the decision-maker, would you adjust the DCP figures to take that into account?

A I have already said two things on that point. I said I would, and that I have.

Q Now, in each of these cases, the adjustments tend to raise the priors; right?

A It raises the prior N equals 1.

Q And that would turn to raise the posterior probability that N equals 1?

A Yes.

Q Now, this likelihood of detection factor of .85; you have reasonable -- all the factors that we have

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discussed thus far could influence that factor as well, could it not?

A Yes.

Q And this time it would tend to make the probability detection higher than it otherwise would?

A Yes.

Q Have you sought to make any adjustment for that purpose?

A In that number, no. In that number, I took sort of a median number of the opinions of the various people involved.

Q Was there a large range?

A No.

Q The effect, then, of your not making any adjustment could be to overstate the confidence that this would catch safety-significant errors; right?

A No, that is the judgment of the DCP.

Q But to the extent that the DCP's judgment is subject to bias, again statistical bias, the failure to adjust for that bias would tend to overstate the confidence in detection, wouldn't it?

A Mr. Strumwasser, this is presented as the judgment of the DCP. As such, it's totally accurate. If someone else wants to say here's the judgment of the DCP, what is my judgment, then in my judgment, I may change the numbers.

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But this is still the DCP judgment.

Q All right, the DCP judgment -- well, if you were the decision-maker, would you adjust that figure?

A I might, a little.

Q And that would tend to reduce the probability of detection?

A Yes.

Q And increase the likelihood of a Region 2 or 3 error?

A Yes.

Q Dr. Kaplan, in your testimony, you refer to the DCP/Bechtel personnel as the persons most knowledgeable on prior distribution.

A Yes.

Q On page 69 of your testimony, if you would like to find it, in fact aren't they about the least useful sources of priors that you could use?

A No.

Q In November of '81, none of them was involved with Diablo Canyon, right?

MR. NORTON: Object, Your Honor. The witness has said, time and time again, that time is not the key to the term "prior," that it's a state of knowledge; that you put certain knowledge in one basket, and certain knowledge in another. And we keep going back to November of '81.

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JUDGE MOORE: Isn't it a bit repetitive, Mr. Strumwasser?

MR. STRUMWASSER: I don't think so. It's not going to take me long to develop this point. I think it's not repetitive, and I think the Board will see it in a moment.

JUDGE MOORE: What is your response to Mr. Norton's -- his objection is repetitiveness in what he just stated.

MR. STRUMWASSER: I'll withdraw that part because that, in fact, is something he has already testified to, so I will withdraw that question.

JUDGE MOORE: Proceed.

BY MR. STRUMWASSER:

Q Isn't it true that any nuclear engineer,Dr. Kaplan, would have as much information on which to base a prior as the DCP people would?

A (Witness Kaplan) Any nuclear engineer?

Q Any knowledgeable nuclear engineer --

A It seems to me the question has got its own answer. People as knowledge as the DCP people in plant design would know as much as they do.

Q Well, the DCP people, when you were asking them in 1983 to construct priors having knowledge about the verification program have to factor the verification program results out of their calculation of priors.

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A That's correct.

Q That's hard to do, isn't it?

A I don't know. I don't think that's so hard. I don't find it very hard myself.

Q To the extent they fail, we've already established that they would try to count the evidence twice; right?

A Yes, if they failed in the direction that you say.

Q One could --

A They might overcompensate. You have to acknowledge that possibility also.

Q You could have gone to a competent inthe generic sense, knowledgeable nuclear engineer, someone knowledgeable about the industry generally, who did not have detailed knowledge about the verification program, given them a well-defined body of information, and on that basis asked them to develop their own priors, couldn't you?

A I could have.

Q And those people would not have the same difficulty, whatever difficulty it is, in excluding the evidence. Right?

A Okay.

End 10

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Q Dr. Kaplan, obviously when you take different priors and different likelihood of detection figures, one could get a different posterior probability, right?

A Yes.

Q Could you give us, sitting here, give us the different probabilities if I gave you a different prior and a different likelihood?

A Of course.

Q Tell you what, I would like to give you some figures and ask you to calculate for us the value in Equation 7.9.

MR. NORTON: Excuse me, Your Honor. That's a mathematical calculation. Mr. Strumwasser has his own witnesses, and we would ask that they do their own calculations. It serves no purpose for him to go through mathematical formulas as a witness.

MR. STRUMWASSER: Mr. Chairman, it demonstrates a point that Dr. Johnson was getting to; namely, the sensitivity of his results to his prior distribution in his answers.

JUDGE MOORE: Mr. Norton, I think we will permit a bit of this.

Go ahead, Mr. Strumwasser.

BY MR. STRUMWASSER:

Q Dr. Kaplan, I would like you to assume a prior

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that p for n = 0 is .4; p for n = 1 is .3; and p for n is greater than 1 is .3

Do you have that?

A (Witness Kaplan) Uh-huh, yes.

Q That is instead of the figures on Page 57 of your testimony.

JUDGE JOHNSON: I'm not sure I'm hearing all of it, but don't they have to add up to 1?

MR. STRUMWASSER: Yes.

JUDGE JOHNSON: I thought you said p at 0 is .4 and p at 1 or greater was .3?

MR. STRUMWASSER: p of 0 is .4; p of 1 exactly is .3, and p of greater than 1 is .3.

MR. NORTON: Excuse me, your Honor. The problem I have with this -- I will restate my objection -- there is no foundation for these numbers. To sit and ask the witness to substitute numbers in a formula and do the mathematics has no relevancy.

JUDGE MOORE: I think in context, there is plenty of foundation.

Go ahead, Mr. Strumwasser.

BY MR. STRUMWASSER:

Q Then, Dr. Kaplan, for the likelihood of detection function that you have at Line 8 of Page 61, I would like you to assume the probability of E3, given n = 0, is 1,

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of course; the probability of E3, given that n = 1, is .3; the probability that E3, given n = 2 or greater is .1.

Okay, do you have those figures?

A (Witness Kaplan) Yes.

Q Can you now give us the probability that n = 0, as you have calculated it at Line 22 of Page 61? I don't know whether -- if you would like to give us an estimate of how long this would take you, this may be a lunchtime break, but this is the only such problem I have.

A We can do it right here. I can't do anything that you can't.

In Equation 7.9, you have nowgot .4 times 1 in the numerator, and the denominator, .4 plus .6, so that is .4

Q All right. Let's look at Equation 7.9. You are calculating p(n = 1) --

A No. You asked me for n = 0.

Q Okay. p(n = 0), given E3, and you are coming out with a probability of .4; is that right?

A Yes.

Q So given the priors and the likelihood function I have given you --

A Let's go ahead. I'm not able to concentrate right here too much, but let's go ahead.

Q We are.

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A You are? Okay. I'm a little rattled for this kind of work right here.

MR. STRUMWASSER: May I suggest we take our lunch right now?

JUDGE MOORE: There is a problem with this, Mr. Strumwasser. If you are going to ask -- is this the only one, as you stated, that you want?

MR. STRUMWASSER: Yes, sir: .4, .3, .3 for the prior, and 1.3 of .1 for the likelihood of detection.

JUDGE MOORE: I think that we will break, but we will take an added fifteen minutes for lunch, so that the witness not only has time for his lunch, but also has time to do your calculations, and we will reconvene at 1:45. It is now 12:05.

MR. NORTON: Excuse me, Your Honor. We can be back by 1:30, in terms of the witness.

JUDGE MOORE: That won't present a difficulty?

MR. NORTON: That gives us an hour and a half. That's fine.

JUDGE MOORE: Fine.

MR. NORTON: I also would, however, like to ask how much longer we can expect the cross-examination of this panel to go on, because the IDVP is here, and they would like to know whether or not they are going to go on today or tomorrow.

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JUDGE MOORE: Mr. Strumwasser, do you have an estimate of the length of your remaining cross-examination of this panel?

MR. STRUMWASSER: I would guess about two hours. I would suggest that the IDVP be excused for the day.

JUDGE MOORE: Mr. Havian?

MR. HAVIAN: Yes. I think we're going to go two hours, Mr. Chairman.

JUDGE MOORE: Now I take it that each of your two hours is not the same kind of estimate we had on Friday?

MR. HAVIAN: No, Mr. Chairman. I can't speak for the Governor, but no, I think it's safe to say we will have easily two hours' worth of questions for this panel.

MR. STRUMWASSER: Two hours is my best-point estimate for a curve, sir.

(Laughter.)

JUDGE MOORE: We will be back, then, at -- let's make it an hour and a half -- 1:35.

(Whereupon, at 12:05 p.m. the hearing was recessed, to resume at 1:35 p.m. this same day.)

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AFTERNOON SESSION

1:35 P.M.

JUDGE MOORE: Come to order, please.

Whereupon,

DR. STANLEY KAPLAN

and

RICHARD C. ANDERSON

resumed the stand as witnesses on behalf of Applicant and, having been previously duly sworn, were examined and testified further as follows:

MR. NORTON: Excuse me, Your Honor, we have a preliminary matter before the cross examination starts. We have just been informed that the Joint Intervenors made a filing with the Commission on Friday, and have apparently mailed copies of their Friday's mailing to the parties.

It has not been received, at least in San Francisco. I haven't been able to contact my office to see if we received it in Phoenix yet, but we would ask that the Board enter an order now that any filings made by the Joint Intervenors or any of the parties be hand-served on us where we obviously are, as opposed to mailed to the offices where we obviously are not while these proceedings are in progress.

JUDGE MOORE: Mr. Havian.

MR. HAVIAN: If Mr. Norton will tell me what the nature of this filing is, perhaps I can respond to it, but

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unless he does, then I have no idea what he's talking about.

JUDGE MOORE: It apparently came from the Los Angeles office.

MR. NORTON: Well, it's his client's filing, because they haven't bothered to give us a copy. It's very difficult for me to explain the nature of it. It's apparently a very dark secret. And the point is that we would like to have them hand-delivered to us while we're here.

MR. HAVIAN: I have no problem with hand-delivering things, and we have been doing that up until now, every time we've had the chance. Obviously, it was not possible -- assuming something was filed on Friday, for Mr. Reynolds to drive up here so that Mr. Norton would have the courtesy of having one in his little hands, I'm sure he has a couple of hundred lawyers waiting back in San Francisco and Phoenix, who are capable of sending him things just as well as Mr. Reynolds is.

I would be happy in the future, to the extent that we have something up here or can get it here, to hand-deliver it to him.

JUDGE MOORE: Thank you, Mr. Havian.

The Board would appreciate that any party making a filing would first serve us while we are here by hand; secondly, the same -- extend the same courtesy to all the other parties, and to make an effort to get it here, even if

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it comes from an office in another city.

MR. HAVIAN: By the way, Mr. Chairman, I was not aware of any filing that was made on Friday.

JUDGE MOORE: Fine, Mr. Havian. But in the eventuality that future filings are made, we would appreciate it if they can be handled as I've just discussed.

Are there any other prelimarinary matters from the Staff or the Governor or the Joint Intervenors?

(No response.)

JUDGE MOORE: Mr. Norton, do you have anything else?

MR. NORTON: No, I don't. Thank you.

JUDGE MOORE: Mr. Strumwasser, continue.

MR. STRUMWASSER: Thank you, Mr. Chairman.

XXXX

CROSS EXAMINATION (CONTINUED)

BY MR. STRUMWASSER:

Q Dr. Kaplan, when we last tuned in, you were going to do some calculations for us. Have you now --

JUDGE MOORE: Excuse me, Mr. Strumwasser. Dr. Kaplan and Mr. Anderson, is the glare off these windows troublesome to you, or off the water?

WITNESS KAPLAN: No.

WITNESS ANDERSON: No.

JUDGE MOORE: Continue.

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BY MR. STRUMWASSER:

Q Have you now, Dr. Kaplan, completed those calculations?

A (Witness Kaplan) Yes.

Q Do you have a value for the probability N equals zero?

A Yes.

Q And that is?

A .77.

Q So just to recapitulate, if one were to take the prior that I gave you, and the probability, the likelihood of detecting an error that I gave you, the application of Bayes' theorem would lead to the conclusion that there is a 23 percent chance that there is at least one safety significant error remaining undetected?

A Yes.

Q Thank you.

Now, there is a bracketed term in your equation 7.12 which appears on page 62 --

JUDGE JOHNSON: I have a question of you, Mr. Strumwasser. The numbers that you gave Dr. Kaplan before the break, that he was to use to make his calculations, are you going to offer any indication of their significance, or were they simply numbers that you pulled out of your pocket somewhere?

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MR. STRUMWASSER: For present purposes, Dr. Johnson, I would just like it to be understood those were intended to be illustrative of the effects on the results of different -- of a different kind of assumption of a prior distribution.

JUDGE JOHNSON: They were so very, very different -- okay, fine. I understand that.

Go ahead.

MR. STRUMWASSER: And I might add, Dr. Johnson, I don't know whether we will be presenting evidence on a prior at this time as well.

BY MR. STRUMWASSER:

Q This bracketed figure and equation 7.12, Dr. Kaplan.

A (Witness Kaplan) Yes?

Q That is the inconsistency of the proposition that the quality of the work is as good as it is in the sample work, and that there would be a Type 2 or 3 error in the not sampled work; right?

A Yes.

Q And without going through the details, many of the same points we made this morning or we discussed this morning about the potential for biases in that figure would also be applicable here; would it not?

A You're saying DVP could have underestimated that number?

Q Right.

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A Because of their deliberate or unknowing bias?

Q For all the reasons we discussed.

A Yes. Well, I have to acknowledge there's a possibility, but I would also like to say that there's a possibility in the other direction; that they have stated these numbers conservatively.

Q But the factors that we have discussed all tend to suggest a bias in one direction, do they not?

A That's the factors that you have emphasized. I would just like to acknowledge that there are other factors in the picture also.

Q Perhaps your counsel on redirect would like to bring those out.

Let's take, though, this .2 figure that you have for the bracketed value.

A Yes.

Q And just crank through the value that you came up with, with my assumptions for equation 7.9. That would still give you roughly 5 percent probability of at least one safety significant error; right?

A Right. Using your assumption. Remember, your assumption was, going in, you had a 60 percent confidence that there was an error, a major error in the Phase II.

A I understand. That's right. I wouldn't want that to be lost.

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Mr. Anderson, we spoke this morning a bit about the question of preoperational testing, and the likelihood of an error being detected during the preop testing.

A (Witness Anderson) Yes.

Q Isn't it true that Diablo Canyon had already been through one complete set of preoperational tests in November of 1981?

A Yes, that's true. I don't know if it was exactly entirely complete, but it had gone through rather extensive testing.

Q So all of the design errors that we have seen as a result of the IDVP's work, excluding those derived from the corrective action program, all of those design errors had already survived the preoperational testing and not been detected; right?

A To the extent that the testing had been done. You understand, during normal startup of the plant, there are additional tests that are done.

Q Right.

Dr. Kaplan, would you turn to page 5 of your testimony? You discuss in this general area of your testimony some attributes of the verification program that in your view render what you call classical statistics inapplicable.

I would like to go over those factors with you.

First of all, at lines 18 and 19, you point out

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one needs a set of well-defined elements; right?

A Yes.

Q Isn't it true that the IDVP, to do its non-statistical verification, had to have a set of definitions of the design that it was verifying?

A A set of definitions of the design?

A Did it not need a well-defined set of design work that it was going to verify?

A It had a well-defined set of design work. It didn't have to design -- it didn't have to define the design elements in the sense of a ball and urn problem that we're referring to here.

Q Well, you have shown, have you not, that one could develop such a set of elements?

A One can go through the motions.

Q You indicated that the design elements need to be equal and well-mixed.

A That's the understanding of what I call a ball and urn problem.

Q Is that really a requirement of classical statistics?

A It is within the framework, in the framework of what I am calling an elementary ball and urn problem. That's part of the definition of that problem.

Q When George Gallup polls American voters, are they

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all equal and well-mixed?

A All American voters are not equal, nor are they well-mixed.

Q They come --

A If you're asking me whether George Gallup polls randomly or selects his sample randomly out of that population, I don't know first-hand, but I have heard that he doesn't.

Q You understand that he does stratified sampling, doesn't he?

A Yes.

Q Within a stratum, does he poll at random?

A I don't know that either, but I would say if you know that you have a homogeneous population within a stratum, then one would normally sample at random within that stratum.

Q And homogeneous need only be homogeneous with respect to whatever attributes you're studying; right?

A Of course.

Q But to the extent that you have a heterogeneous sample -- excuse me, population -- aren't the inequalities solved by the very process of random sampling, as long as you ensure that each element is equally likely to be selected?

A Would you say it again?

Q Sure. To the extent that you have a heterogeneous population --

A Yes.

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Q -- as, for example, Americans are --

A Yes.

Q -- as long as you ensure that each element in the population has an equal chance of being selected for the sample and you randomly select on that basis, don't you avoid all the problems of inequalities or heterogeneity of the sample?

A No, not at all.

Q Well, let us take the Gallup problem. Do you have any doubt that one could simply randomly sample from the American population without stratifying?

A When you say "one could," you mean you could physically carry it out?

Q You could do it without violating any of the principles of classical statistics.

A That is a broad question. I'm not sure what it means. You'll have to tell me what you are looking for, what answer you are trying to get from the stratum process, and we'll talk about the process by which you try to get it.

Q Okay. Let's say we're on the threshold of an election. My population is registered voters.

A Yes.

Q It's a heterogeneous population; right?

A Yes.

Q And I want to conduct a sample, perform a sample

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so as to estimate the proportion of that population that intends to vote for Candidate A. Okay, are you with me?

A Yes.

Q Could I not randomly sample from American registered voters, without regard to strata?

A You could. You could carry it out. You could physically do it.

Now, whether that would be a wise thing to do is another question.

Q Well, elements of wisdom are involved in that decision?

A The elements of wisdom or knowledge. The knowledge is if you know something about the strata; the wisdom is the wisdom to take advantage of that knowledge.

Q And by taking advantage of that, you do two things. I think you can reduce your costs; right?

A Correct.

Q And you can gain some information about the strata; right?

A Correct.

Q But if all you're trying to do is to sample -- is to study the overall rate for the whole population, and you are indifferent to the cost of the sampling, you could just as easily do it by sampling without regard to strata; right?

A If you're indifferent to the cost, you want to

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just check everyone.

Q Let's say you're indifferent to the cost within a range that limits you to, say, 10,000 people.

MR. NORTON: Excuse me. Are we talking about a national election, 10,000 people now? Are we talking about the president or the sheriff of San Luis Obispo County? What are we talking about?

BY MR. STRUMWASSER:

Q National.

A Counselor, if I had a budget to sample 10,000 people for a national election, I would think very hard about how to make that sample most effective.

Q I understand that. But if you chose not to think that hard, you could still get valid results, couldn't you?

A If I chose not to think, I could sample randomly, and the results would be valid in a certain sense.

Q What sense?

A In the sense that I sampled randomly, and here's the result that I got.

Q And the rate, the proportion that you got, would be usable as an estimate of the rate of the frequency of people who prefer Candidate A, right?

A Depending on how you use it, sir, as an estimate. You took a sample, and you get an answer from the sample.

Q Right.

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A Now, what you infer from that about the population as a whole is another question.

Q You would get a proportion, right?

A You would get a proportion?

Q Right.

A You mean the sample as a proportion of the whole?

Q No, no. You'd get number of people for Candidate A, divided by the --

You'd get a number of people for Candidate A which could be divided by the size of your sample, right?

A Yes.

Q And that would give you a proportion of the population -- of the sample for Candidate A; right?

A Yes.

Q And you could, by standard statistical techniques, construct a confidence interval around that proportion, right?

A Yes.

Q And you could say things about the proportion of the population that was for Candidate A, right?

A You are entering onto a subtle point and a point that is at issue between Bayesians and classicists. So rather than stir all of that up, let me say one can always make statements about his degree of confidence on the population. Whether you could take the standard statistical confidence intervals that you referred to and interpret them as meaning

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your degree of confidence in my sense, in a Baysesian sense, at that point we would have to have some talk.

Q Probably the Board doesn't want to watch the talk.

A I don't blame them.

Q The point I think we need to get closure on here is that one can randomly sample from a heterogeneous population, without violating the rules of classical statistics. Would you agree with that?

End 12

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A I'm not sure what it means. I think you can carry it out. It is a question of what you infer from that. That would be the interesting question.

Q And a Bayesian could sample randomly from a heterogeneous population and perform all the standard Bayesian manipulations of the data, right?

A Yes.

Q And that would be valid, right?

A I'm not sure what you mean by "manipulations." If one makes a valid inference, then it's valid.

Q On Page 5 you also say that the elements must be readily discernible, as black or white.

A Yes.

Q I take it that that is a reference to the problem of identifying whether something is an error or not?

A Yes.

Q Isn't that the same problem that confronted the IDVP?

A Determining whether something is in error or not?

Q Right.

A Well, I think the IDVP simply, if anything was at all in question, they held it up and put a big question mark on it.

Q But the point is, once they declared something finally as a Class A or B error, that was a categorization

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that is analogous to the determination that something was black or white, right?

A Sure.

Q So that is not a problem of statistics; that is a problem that is inherent in the process of evaluating engineering?

A It's not a problem in statistics. It is a problem of fitting the real-world problem into this particular simplified model. I'm saying here that one of the things you have to do if you do that is, you have to establish a black/white definition.

Q But that is also a problem in doing what the IDVP did. They had to establish a black/white or A-B-C-D definition, right?

A They had to decide whether to call something a Category A or B, yes.

Q And in fact, for your analysis, you had to establish black/white and green definitions for regions 1, 2 and 3, didn't you?

A Yes.

Q Those are at least as fuzzy as A, B, C, D categorizations, aren't they?

A Sure.

Q At Page 6, you object to classical statistics used in the verification program because of the need for a

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distinction of safety-significant errors.

A Excuse me. Where are you reading?

Q Lines 11 through 14. I take it that that point you are making there is that classical statistics is inapplicable, because of the need to take into consideration safety-significance of errors; is that the point?

A No, that's not really the point. I am saying, the errors of most importance are those that result in significant safety hazards.

Q So basically, then, Dr. Kaplan, if one simply adopts a definition of black-and-white that incorporates the criteria for safety significance, you can accomodate the need to take the safety significance into account, if there is such a need, in a classical statistics framework, couldn't you?

A I don't think I'm going to agree, because I think you have a little different meaning to your question.

Let me say it this way. It is certainly valid to ask if something is a safety-significant error or not, and it is certainly valid to ask the question, How many such errors are present or are there any present?

Then if you want to know if any such are present, one designs a sampling program to try to find those. Now if that is what you mean by "incorporating" in a statistical framework, .then I will agree. But I don't know, you may

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mean something else by that.

Q That's all I mean.

A Terrific! That's what I think the IDVP did.

Q Now you point out that the classical statistics statistician will focus on frequencies and would presumably use frequencies in the sample to derive the probability for the unsampled population, right?

A I'm pointing out that the classical statistician, in his ball-and-urn framework, is asking the question about what is the frequency of the black balls, and that his program consists of estimating that frequency from the results of a sample. So translating that to a design verification problem, you have to define some kind of frequency that you are interested in, and then attempt to estimate that frequency from a sample. I'm saying that that doesn't fit.

Q Well, a Bayesian would use frequency data, would he not?

A If he had it, sure.

Q But a Bayesian would also try to use expert opinion and other kinds of information, right?

A He would use everything he has.

Q Would a Bayesian who felt that -- who had frequency data and who felt that expert opinion was not useful, would he instead use a flat prior? Right?

A If he felt that expert opinion was not useful?

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You mean he didn't have any --

Q That he thought was any good.

A Sure.

JUDGE JOHNSON: May I interrupt?

MR. STRUMWASSER: Surely.

JUDGE JOHNSON: I'm always on the edge of this argument or controversy between classical statisticians and Bayesians, and I am not sure how important it is, but let me ask with respect to the urn-and-ball problem, in your opinion, Dr. Kaplan, if in the process of taking a sample from the urn, --

WITNESS KAPLAN: Yes?

JUDGE JOHNSON: -- the sampler, who in this instance will be a classical statistician, happened to notice that all the white balls were round and the black balls were ellipsoidal in shape, and this was consistent within his sampling, --

WITNESS KAPLAN: Yes?

JUDGE JOHNSON: -- would a classical statistician have any way to incorporate this information into his ultimate estimate of what was going to happen?

In other words, presumably a Bayesian might -- I mean, in your view, the Bayesian takes every bit of information he has and adjusts things accordingly, and he might be able to note that there is a distinction other than

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just color between these two elements.

WITNESS KAPLAN: Right.

JUDGE JOHNSON: Does not a classical statistician have some way of doing the same thing?

WITNESS KAPLAN: I'm afraid I'm a little bit vague on what we are talking about. If what we are interested in is the fraction of black balls in the urn, I don't see that the shape is relevant. You simply -- right. Maybe I'll leave that at that.

I would like to --

JUDGE JOHNSON: I think you're right. I think perhaps I am inferring something else. Perhaps the sampler need not withdraw the ball to look at it, if he has made the secondary determination that the one is ellipsoidal and one is round.

I think I would like to stop this line right now, and let Mr. Strumwasser go back. He obviously understands more about it than I do.

WITNESS KAPLAN: You may be thinking he has some other information about the fraction of ellipsoids or some other way of finding that?

JUDGE JOHNSON: Yes.

WITNESS KAPLAN: Then definitely he would use that. But the issue that I am raising here is not Bayesian versus classicist. I am really saying, the design

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verification problem situation does not fit in the ball-and-urn framework.

JUDGE JOHNSON: What I was really driving at is whether or not the classical statistician has ways in which to diverge from the -- the Bayesian can take into account lots of things about his -- what he's finding out, and you seem to be saying that the classical statistician is somehow bound to reduce everything to a ball-and-urn type problem.

And I am saying, are there not ways in which the classical statistician can also accomodate the things -- a more complex problem?

WITNESS KAPLAN: All I can tell you is what I think, all right? What I know about this.

I have the impression that the classical statistical apparatus pretty much rests on that ball-and-urn problem, and there are elaborations of it and multiplications of it, but that is the root of it.

What the Bayesian framework adds beyond that is that it is willing to talk about degrees of confidence. That is the essential notion. We say we have to make decisions; we make them under uncertainty, and that is a way of making better decisions and communicating about uncertainty. We want to establish a scale to quantify the degree of uncertainty. That is the first notion that

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the classical statistician does not accept. He thinks of that as subjective and unscientific. He doesn't accept the notion of quantifying the degree of confidence.

The second point is in line with that. He doesn't like the idea of incorporating judgment. The kinds of information he likes to deal with are the outcomes of sampling experiments. To take a sample of ten and five of them are black, you know, that's nice and clean. So to him, that is scientific, objective, et cetera, and it is. It's nice and clean.

But the other kinds of information that we have out of our experience which are not in that simple form, those other kinds of information which we've roughly covered by the word "judgment," that kind of information doesn't enter into -- or it's difficult to enter that into the classical framework.

JUDGE JOHNSON: All right. Thank you.

Carry on, Mr. Strumwasser.

BY MR. STRUMWASSER:

Q Basically, Dr. Kaplan, aren't you saying that one should use statistical decision theory to make these decisions?

A I would call it probablistic decision theory.

Q Okay. But basically you are talking about the process of making explicit assumptions about probability,

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probabilities, costs, benefits and so forth, right?

A Right, but not quite. We don't make assumptions about probability. We use probability to express our knowledge.

Q Okay. And a Bayesian who felt that the only evidence he had that was worthy of use in that problem was a frequency count would use a flat prior in his decision, right?

A If he knew nothing else, then the results of the sample -- the way you say, "I know nothing else," in Bayesian language is to use a flat prior.

Q In effect, you do that in Part VI of your testimony, right?

A That's correct.

Q And if he did that, he would get numerical results which were identical to the classical statistician, right?

A I don't know that that's exactly the case. I have thought that that was the case and convinced myself at one time that it was in some simple examples, but I have been told lately that that is not the case. It is not always true; that the statistician's confidence interval amounts to a Bayesian confidence interval with a flat prior, so I don't know that that's -- to what extent that that's true.

Q As a general proposition, would you agree that

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as one's doubts about non-frequency information increases, the classical and Bayesian methods approach the same result?

A As one's doubts?

Q As the Bayesian's doubts about --

A You are saying -- I think you are talking to the same point I just answered. You are saying if the prior gets flat, would a Bayesian's result be the same as a classicists? That's what you are asking?

Q Right. As a general proposition, because I gather you have grouble about some specialized --

A Yes. I just answered, I don't know whether that's true. I used to think that, but I don't know if that's true anymore as an alternate proposition.

Q You do know that it is often true?

A I don't even know that.

Q Also on Page 6, you talk about the interconnected nature of design as a limiting factor in classical statistics' use in verification, right?

A Excuse me. Which page?

Q Page 6, Lines 6 through 9, they are not separate; they are interconnected in various ways.

A Yes.

Q Now first of all, even sampling whole systems like the IDVP did misses some of the interconnected aspects of the design, does it not?

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A In the sense that systems work together.

Q Right.

A Yes.

Q Now if one were interested in doing random sampling and one wanted to capture the integrated aspects of the design, one could enumerate all of the inter-relationships among systems and components, couldn't one?

A I'll pass to Mr. Anderson the question of whether that's a feasible way to go.

A (Witness Anderson) It would be very difficult to identify all the integrative aspects without doing a detailed review of all the systems.

Q Take your time.

A It could be done, yes.

Q Okay. And then you could sample, Dr. Kaplan, could you not, from among the population of inter-dependencies, right?

A (Witness Kaplan) You are envisioning a huge list of all the relationships of every piece to every other piece?

Q Right.

A And what you are supposed to do under each circumstance, and you are saying you could, in principle, sample out of this?

Q Right.

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A And ask whether that relationship is --

Q Is as it is supposed to be.

A As it is supposed to be. In principle, you can do that.

Q Are you familiar, Dr. Kaplan, .with the way in which the IDVP constructed design chains?

A Not in great detail.

Q Well, you are aware that, I believe, Mr. Reedy produced a listing of what purports to be all of the interfaces between all of the design groups.

A Yes, I've heard that. Yes.

Q And in much the same way that he examined information flow across some of those design groups, did you see any analogy between that process and the process of enumerating interdependencies among components and systems?

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A The analogy you're suggesting, I guess, is that you have a relationship between two parts?

Q Right.

A Or between two people or two groups, so there's an analogy in the sense that there's relationships. Is that what you're saying?

Q You can enumerate relationships in sample from among them.

A Fine.

Q There's not a whole lot of point there, but there's a little point there.

Turn, if you would, Dr. Kaplan, to page 42 of your testimony.

JUDGE BUCK: Before you go ahead, Mr. McGurren, I have a second message for Mr. Knight. I see he isn't here, but it's getting late in Washington. If he's supposed to call back there, you might want to get ahold of him.

MR. MC GURREN: Thank you.

JUDGE BUCK: Excuse me. I just wanted to get the message, because it's after five back in Washington.

BY MR. STRUMWASSER:

Q Dr. Kaplan, starting at page 42, you criticize classical statistics for requiring one to predetermine one's level of confidence. Right?

A (Witness Kaplan) I didn't exactly criticize

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classical statistics. I'm saying the notion that one can predetermine his level of confidence is dubious. To the extent that the language of the classical statistician suggests that, I would criticize that.

And I think that suggestion is one that non-statisticians receive in reading that language. I think the statistician knows what he means and doesn't really mean that.

Q On page 44, you said the level of confidence is known only after we see the results of the sample. It cannot be predetermined beforehand by specifying a given sample size.

Do you see that, starting at line 2?

A Yes.

Q Isn't it true that one can and should set the desired target confidence level ahead of time?

A In planning a sampling program?

Q Exactly.

A For a design verification program?

Q Sure.

A Well, you could go about it that way. You could ask yourself this question. You could say suppose I sample this system or this much of the design, and don't find any major errors there, what degree of confidence would I have at the end of that experiment, that there are no such major errors remaining? And if you felt uncomfortable with that

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degree of confidence, you might design a different experiment, a bigger sample, and then you would have to ask yourself well, how much confidence would I get in that sample, and how much do I have to expend in resources and time to do that bigger sample, and is the extra confidence worth the extra expense?

Q More particularly, Dr. Kaplan, assuming whatever you assume about our prior ahead of time, we know that we sample one element and find no error: we make certain contribution to our confidence.

A Yes.

Q And we can, ahead of time, say how much additional contribution there will be for each new element that is error-free. Right?

A All right.

Q And we can say, in advance, what our confidence will be that there are no errors in the population after sampling any given number of design elements and finding them all error-free.

A Yes, if you presuppose the results of the experiment, then you can say what effect that would have on your degree of confidence.

Q Right. And it is merely the reverse side of the same coin to say, if I want a certain level of confidence, a given level of confidence to come out of this experiment, how many of these design elements need I sample and find error-free

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ki 14:4

before I have achieved that. Right?

A Yes. How many or which ones? How many and which ones?

Q Well, which ones goes to your stratification question, right?

A Correct.

Q Now there is no reason we couldn't set our desired level of confidence in advance, could we?

A In a way, there is a reason, because the desired level of confidence is one desires always total confidence.

Q If one takes into account the cost of getting that --

A Oh, sure, taking account of cost, then you can and should ask what is the optimum strategy in terms of what to sample to get the most information in the most efficient way.

Q And one of the things we should do is to identify how confident we want to be about the safety and determine what would be required of the sample ahead of time, to get that level of assurance, isn't it?

A I think we're going over the same ground. When you ask how confident you would like to be, you'd like to be totally confident.

Q No. Don't read that into my statement. How confident you want to be, given whatever you want to take into account for that, costs, risks, and so on.

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ki 14:5

A I just don't ask the question that way and say how confident you would like to be, given the costs. The way I would phrase the question is, what's the optimal strategy in light of confidence and costs?

Q Setting aside the question of stratification for a moment, if I know in advance the costs per design element sampled, we may assume that?

A Fine.

Q And I have done a careful analysis to determine how confident -- strike that -- a careful analysis to determine what the costs of being wrong about my confidence is. Okay?

In other words, the costs of erroneously assuming a safe design.

S2 BU A All right.

Q Then I could identify an optimal sample size that would achieve the desired level of confidence. Right?

A Well, you would choose your confidence level in conjunction with the cost of attaining that confidence.

Q Right.

A And, having done that, you would know what sample size.

Q Right. What sample size.

A Setting aside the stratification issue.

Q And if I do that ahead of time and I explicitly state those things, I have given my verification program a

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ki 14:6

quality of objectivity that is desirable, wouldn't I?

A No. That wouldn't sway me very much one way or another about your doing this in advance. I don't consider that all that desirable a characteristic.

Q Is it more objective? Does it appear more objective?

MR. NORTON: Excuse me. Which of those two questions is he supposed to answer?

JUDGE MOORE: Specify.

BY MR. STRUMWASSER:

Q Does it appear more objective?

A Does it appear more objective?

Q Uh-huh.

A And we're in a simple context now, not a design verification context stratification?

Q We're design verification, but we're ignoring stratification.

A That's an essential part. You can't ignore that. If you ignore that, then as far as I'm concerned, we're out of a design verification context into something like a ball and urn context.

Q So you cannot remove the stratification section and answer my questions?

A Oh, I could, but I wouldn't be in the design verification context anymore.

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ki 14:7

Q What is that?

A Well, because the stratification is essential on the design verification context.

Q All right. I'll play your game. Let's say I'm going to stratify.

A Good.

Q Okay.

A All right.

Q I'm going to stratify however way you think it's important to stratify, and within each stratum I'm going to now compute an optimal sample size. Do you buy that?

A All right.

Q Now, if I say in advance how many error-free elements I have to pick before I'm going to be satisfied, does that not give my verification program the appearance of objectivity?

A Objectivity in what sense?

Q How about the insulation from the suspicion that my exercise is result-oriented?

A I guess I just don't understand that -- suspicion that my result -- my exercise is result-oriented? I'm not sure what you're saying in that.

Q Let's say you don't say ahead of time what your criteria are for satisfaction.

A Yes.

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ki 14:8

Q And you sample a certain number of elements and get certain results. And at that point, you then announce that you are satisfied that the design has been verified. Okay? Are you with me?

A At that point, you would have a certain state of confidence.

Q Right.

A About there being a bad error remaining.

Q Right.

A At that point, you go into a decision theory phase and ask should I sample more or shouldn't I?

Q I understand that.

A I do not understand what you're getting at.

Q If I am operating in a realm in which there are people whose feelings we care about, that suspect that that decision is clouded by a desire to reach a specific result irrespective of the data, do I not insulate my program from that criticism if I have stated my objectives in advance?

A I don't know. You're asking me to speculate on the psychology of people like that. And my first response would be that those people are not going to have their position swayed, whatever you do, whatever the evidence.

Q Do you think there's any value in general and public policy-making to having explicit criteria for making decisions?

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A Well, my approach to decision-making is always to look at what is my alternatives, what is my information, what is my probability on each outcome, and what value do I place on each outcome, and to make a decision right there, if some preassigned mechanical recipe or, quotes, "criterion" or the rule and regulation leads to a decision other than that, I would say it's leading to the wrong decision. And I would not give much value to that way of decision-making.

Q Did you hear anything in my question to suggest that the result was wrong?

A Well, if your criterion in all cases gives the same results as my way of making decisions, I would have no objection to it.

Q But given that fact --

JUDGE MOORE: Mr. Strumwasser, are you about through with this line of questioning? I suggest you move on. I'm not sure you're really going anywhere.

BY MR. STRUMWASSER:

Q I'm through with this line of questions, Dr. Kaplan.

I would like to turn to your fjord example. Do you recall the submarines on the fjord? Right?

A (Witness Kaplan) Yes, I do.

Q I've got a fjord, and I suspect there are some enemy submarines in it; right?

A Yes.

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Q If my objective is to locate and remove the maximum number of submarines as quickly and cheaply as possible, then I take it you would advise me to hire myself a competent U-boat commander to give me expert advice on where to find them. Right?

A Yes.

End 14

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Q And if, let us say, he tells me to look in 911 places, and I do, and I find and remove nine submarines, that's fine, I may very well have gotten those submarines as cheaply and quickly as possible, right?

A Yes.

Q Of course, even if I look everywhere he tells me, one more submarine might still be out there, unless he tells me to look -- exhaust the fjord, right?

A Right.

Q So I have to acknowledge the fact that my expert's knowledge is imperfect, right?

A Always.

Q And in fact, that the use of an expert could well lead me away from some of the submarines, couldn't it?

A It's possible,

Q Because the guy under the water thinks what my guy is thinking and --

A Yes, but then my guy is knowing that the guy under the water is taking him back --

(Laughter.)

So what it comes down to is, whether you take your expert's advice or your own or whether you trust to the dice.

MR. NORTON: Excuse me, Your Honor. May I ask for the relevancy? I suspect design errors are in thinking

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what the engineers that are looking for them are thinking.

(Laughter.)

BY MR. STRUMWASSER:

Q There are mechanisms, are there not, --

JUDGE MOORE: Mr. Strumwasser, I fail to see the relevancy as well. Would you like to illuminate it for me?

MR. STRUMWASSER: Surely. Dr. Kaplan has offered up the needline in the haystake or submarine in a fjord problem as an example of why you would not randomly sample, and I am probing the differences between that and the design verification program to illuminate his reasoning.

JUDGE MOORE: Until your last question, I would agree that that is what you are doing, but once you got into your last question, I think you reached the end of the line on that.

MR. STRUMWASSER: I perhaps went up a creek, backing up --

JUDGE MOORE: And I just took your paddle.

(Laughter.)

BY MR. STRUMWASSER:

Q Backing up, there are things about using an expert in a design verification program that could lead me also to miss design errors, right?

A (Witness Kaplan) It could. It's possible.

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Q Well, for example, if the person who is managing the design originally has a stable of designers available to him or her and takes the design problems as they come up for engineering work and allocates the most difficult problems to the best engineers and designers, --

A Yes?

Q -- I could very well have a disproportionatley high error rate in the easiest work, couldn't I?

A That's possible in principle.

Q And if I then sent my verification team in to look where they thought the errors were the greatest risk, they could very well miss this easy design work that has lots of errors, right?

A Not if your verification team knows that the best designers were put on the difficult parts.

Q Do you know whether the IDVP pursued that possibility?

A I don't really know that.

Q Going back to the submarine just for a moment here, even after I've done my 911 most-likely places, let's say I now want to assign a probability that there remains at least one undetected submarine in the fjord, okay?

A Yes.

Q It's very difficult to go from my non-random sample to a generalization about the whole fjord, isn't it?

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A Difficult?

Q Yes.

A It requires a judgment, if that's what you mean.

Q Okay. And would I, in the course of that judgment, be stratifying the fjord into two strata, the places where the U-boat commander says they are likely and the other places?

A Yes.

Q But to draw conclusions about the other places, the places he didn't think they were likely, I would have to make assumptions about the likelihood of a sub being there, when my consultant thought they wouldn't, right?

A You, in assigning a probability to there still being a submarine there, have to exercise your judgment on that point. You call that an assumption. I call it stating your judgment.

Now if you decide to sample randomly, you have, in effect, stated your judgment. In doing that, you have stated that it is equally likely that the submarine is everywhere. If that's what you think, then you should sample randomly.

Q Well, with respect to the residual part of the fjord, --

A Yes?

Q -- within that, I gather that I could go in there

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and sample randomly because my U-boat commander hasn't told me any special things about any part of it, right?

A If you don't know, if you have no reason to think any one place in there is any more likely than any other place, then you might as well sample randomly.

Q And if it really mattered to me whether there remained any undetected submarines in the fjord, would I want to sample from both strata, the high probability and low probability areas?

A If it really mattered, you would want to come very close to one hundred percent sampling, and for each little increment of sampling effort, you would now have to ask yourself, Where is the best place to look now, given all the places I have looked up until now and given everything I have found up until now? So each little incremental piece of sampling effort is another decision process.

Q Would you agree, then, that if you are trying to assess the probability of an undetected submarine, you would use a different sampling strategy than if you are trying to locate as many submarines as cheaply as possible?

A If you were trying to assess the probability of an undetected submarine?

Q Remaining after my anti-submarine program.

A That you would use a different strategy?

Q Than just trying to locate --

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A You have to excuse me. I go tilt at this language again, you see, because your language presupposes that there is such a thing as a probability out there that you are undertaking to assess. That's not the way I use that word, so I am out of sync with you.

Q Do you understand the concept I'm trying to get at?

A Give it to me again.

Q I want to evaluate the likelihood that there is an undetected submarine in the fjord, okay?

A I have the same problem right there. Say it this way: You want to know whether or not there is a submarine there.

Q Okay.

A And on that proposition, you will express your state of confidence.

Q I also want to know the rate of submarines in the fjord.

A The rate? Now you've got a different example. You are saying there are lots of submarines there. When you say "rate," you mean per square mile or something?

Q Cubic feet.

(Laughter.)

A Okay.

MR. NORTON: I hope there's not more than one

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per cubic foot!

(Laughter.)

A very crowded fjord.

WITNESS KAPALAN: See, now, this is exactly the distinction between the ball-and-urn problem and the submarine-in-the-fjord problem. In the ball-and-urn problem, you are saying I have a certain fraction. There's lots of black balls in there, and the question is what's the fraction, so that is one kind of problem.

The other kind of problem is, there may or may not be one submarine there. The question is, What is our degree of confidence that there is or isn't?

BY MR. STRUMWASSER:

Q What if a --

A (Witness Kaplan) I'm saying, that framework is more suitable to the design verification problem than the other framework.

Q What if our question is, What is the probability that there are a given number of submarines left?

A All right.

Q Would you sample randomly or deterministically?

A Oh, I would consult with my submarine commander --

JUDGE BUCK: I'd suggest he go out for a squeeze pretty soon.

(Laughter.)

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WITNESS KAPALAN: I might find someone else to ask right now, but I would certainly use all of the information and intelligence available to me, rather than trusting in the rolling of the dice or some table of random numbers, yes.

BY STRUMWASSER:

Q I would like to turn to your analysis value. At Page 58, you say the IDVP sampled three out of ten systems, right?

A (Witness Kaplan) Almost three.

Q Almost three? Is that what you are saying?

A Yes.

Q Where do I see the "almost" on Page 58?

A On Line 14.

Q But in the Proposition E1 at Line 18, it is just plain three out of ten, right?

A That's true.

Q Now, in fact, they did not sample three out of ten systems, did they?

A That's true.

Q For one of the systems, electrical, they only sampled one of the four subsystems within it, right?

A Correct.

Q And for HVAC, they only sampled one subsystem there also, right?

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A Correct.

Q Isn't it true, Dr. Kaplan, that the same engineer may work a given problem correctly one day and get the same problem wrong the next?

A Yes, I think so, a similar problem.

Q The same would be true for a given engineering team?

A Yes.

Q And isn't it true that a given designer or design team could correctly use a computer code one day and incorrectly apply it the next day?

A That's true.

Q And within a given discipline, isn't it true that a given designer is better at certain kinds of work than at other kinds of work?

A Yes.

Q Isn't it true that a desing team engaged in a power plant design over, let's say, fifteen years is likely to experience personnel changes?

A Yes.

Q Would you expect those personnel changes to alter the likelihood and distribution of errors they made?

A Somewhat.

Q Isn't it true that different contractors providing design services would vary in their error rates?

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A Yes, I think so.

Q At Page 26 of your testimony, you appear to equate a discipline with a team; is that right -- Lines 1 through 8?

A In the sense that there is usually a design team doing the work of a discipline.

Q Do you know that to be the case in Diablo Canyon?

A I have been told that that was the case.

Q Were there not different contractors that also engaged in the same disciplines?

A I believe there were.

Q And over time, there were also changes within the team, weren't there?

A I don't know that, but I suppose so.

Q That's true, isn't it, Mr. Anderson?

A (Witness Anderson) Yes, there were some changes. You have to understand there are relatively few contractors involved in the Phase II work. The Phase II work, systems work, was designed primarily by the PG&E groups.

Q But there were changes within the organization of the Engineering Department at "G&E?

A There were some changes in personnel, yes.

Q You don't really know, Dr. Kaplan, whether a single team did all the work at a given discipline in Diablo Canyon Units 1 and 2?

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A (Witness Kaplan) You mean whether that team was unchanged over the whole time it took to do that work?

Q Let's just say a single team which may or may not have changes in its personnel. You don't even know if there's a single team that did all the work for a given discipline.

A I have been told that that was by and large the case.

Q But you don't know the extent of it, right? I mean there was some contracted work, right?

A I think so.

Q And there are certain engineering disciplines that do not have a specific team for them; isn't that true?

A One of these four disciplines?

Q No, not necessarily. A non-seismic design discipline.

A (Witness Anderson) I think that would be all covered under one of these disciplines, which would have the primary responsibility or the interface responsibility for it.

Q So, for example, the discipline of radiation or health physics is covered by one of these four?

A Yes.

Q Which one?

A It would be generally covered by Mechanical.

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Q And Chemical Engineering?

A Would be generally covered by Mechanical.

Q Is there some chemical engineering done outside of Mechanical?

A I can't think of any offhand. There is relatively little chemical engineering per se done in this kind of work.

Q How about radiation work?

A Radiation work would be covered by the Mechanical discipline.

Q Would there be some done outside of it?

A Oh, I think some in Instrumentation. There would be some -- the actual equipment would be looked at by the Instrumentation Group.

Q Dr. Kaplan, have you sought to assess the frequency of personnel changes within discipline teams?

A (Witness Kaplan) No.

Q Now in some design work, errors are relatively frequent but unlikely to be safety-significant; would you agree?

A In some design work, errors are frequent but unlikely to be safety-significant? Well, I guess I would concede that in certain portions of the design, errors are unlikely to be safety-significant. As to their frequency, I don't know. I don't know that I am ready to concede that certain areas of the design, the errors are frequent.

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Q Well, are you prepared to concede that error rates vary according to the kinds of design work that is being done?

A In principle.

Q What about in practice?

A I don't really know in practice.

Q Do you know, Mr. Anderson?

A (Witness Anderson) If error rates vary depending on the kind of design work being done? Yes, I think there would be some variation in error rates, depending on the design work being done.

Q In some of the relatively higher frequency -- higher error rate design work would be in the kind of work in which there is relatively little safety significance, right?

MR. MC GURREN: Your Honor, I object. There is no foundation for that question.

JUDGE MOORE: It might be helpful. Go back and lay a little.

BY MR. STRUMWASSER:

Q All right. Let's talk about the design of safety-related structure systems and components, Mr. Anderson. There is a variety of disciplines involved, right?

A (Witness Anderson) Okay.

Q I think you have now testified that the rates of

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errors may well vary from one discipline to another.

A Yes.

Q And maybe even you could identify other kinds of divisions of error rates variation besides just interdisciplinary, right?

A Possibly.

Q The safety significance of an error would also vary, wouldn't it? Some kinds of work is more forgiving than others, right?

A Yes.

Q Now can you conceive of a kind of design work, either a discipline or some other division of the design work, where errors are relatively frequent, but unlikely to be safety significant?

A I can't think of any offhand. I imagine such a category could exist, maybe in the area of seismic design, which we're not talking about here. You might have a large number of small errors that would be not safety significant.

Q What about the converse? Can you conceive of work where errors are rare, but where they exist, they are very important?

A Yes. I think there would be such kinds of work.

Q Now, Dr. Kaplan, you have testified that the IDVP sought to get a Phase II sample that included work where errors are most likely to occur?

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A (Witness Kaplan) That's correct.

Q And also --

A That's what I've been told

Q And also included work where errors are most likely to present a risk to safety?

A Yes.

Q Those are, in principle, two different objectives, right?

A Well, then, let me clarify that. What I have been told and what I asked was that the region looked at was the region where, if there were safety-significant errors, this was the region where they would most likely be. So that criterion, if you like, encompasses both frequency of errors and significance from their location.

Q So that sampling strategy, then, required the designers of the program to assess the safety significance of unseen errors; is that right?

MR. NORTON: Excuse me. I'm going to object foundationally. The witness just testified that he has been told that this was a result. He didn't testify that he has been told that the IDVP did that going in. There is no foundation as to who told him or what he was told.

JUDGE MOORE: Response?

MR. STRUMWASSER: Well, this is the kind of information which, while it is hearsay and all that, is the

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kind of information on which experts routinely testify, offering their opinion. And to the extent that Dr. Kaplan is relying on it, I am entitled to probe it.

MR. NORTON: That wasn't my objection at all. His probing I don't mind. I would like some foundation, because Mr. Strumwasser is implying now that he was told that by the IDVP, that the IDVP did that at the beginning of the program, and there is no foundation for that.

JUDGE MOORE: Restate your question and specify, and we will avoid the problem.

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BY MR. STRUMWASSER:

Q From whom have you obtained your information about the nature of the IDVP sample?

A (Witness Kaplan) I talked with John Krechting, K-r-e-c-h-t-i-n-g; Bill Cooper.

Q When?

A When --

Q On this topic.

A Over the last month, month and a half.

Q And they told you what you just described about the IDVP sampling strategy?

A Yes.

Q Now, is it your understanding of what they said that that sampling strategy was something they had embarked upon ahead of time? That is to say, that is what they were looking for when they designed the sample, or when they designated the sample?

A It was my understanding, yes, that that was one of the components in their thinking, to look for the areas where, if there were any severe errors, they would be most likely be in those areas.

A (Witness Anderson) But the overriding -- at least it's our understanding, we don't know exactly how the IDVP picked those systems, but it's our understanding that they were looking for systems that gave them the most information

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about the engineering process that had been used on Diablo Canyon. And to the extent that that allowed them to probe into areas that were most prone to errors, that's a byproduct; but they were really looking for systems that gave them the best opportunity for gaining information on how the design process was carried out at Diablo Canyon.

Q Is it your understanding, Mr. Anderson, that the sampling strategy of the IDVP required them to assess where errors, if they occured, would be most likely? Would most likely be safety-significant -- I'm sorry.

A That may have been a consideration. Certainly the systems they chose were good systems from that point of view.

Q But you don't know?

A I don't know if that was in their thinking.

Q Do you, Dr. Kaplan?

A I don't know. As I just said, I have the impression that that was in their thinking.

Q Now, Dr. Kaplan, isn't it true also that they sought to achieve a diversity of disciplines?

A Yes.

Q That is in itself a separate objective, is it not?

A Yes.

Q It's not identical to the objective you just described, right?

A That's right.

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ki 16:3

Q So if one were to incorporate that, one has to somehow weight those separate objectives, right?

A Yes.

Q You don't really know, Dr. Kaplan, do you, how the IDVP weighted those objectives?

A No. I can answer what I think on that.

Q I don't want speculation. If you have information, that would be useful.

A I have an absence of information, so I don't think there was a formal weighing process gone through. There were those two considerations and some others, and they were all informally talked about, and a judgment was made. That's the impression I have of the way they did it.

Q Do you agree, Dr. Kaplan, that extrapolation about design quality for Diablo Canyon, Unit 2 from the verification of Unit 1, is not as direct evidence as would be available from actual sampling of Unit 2 design work?

A Of course.

Q Mr. Anderson, isn't it true that PG&E will have as many as 4 to 500 engineers and designers working on Unit 2?

A (Witness Anderson) Yes. At the peak, I think that's about the number.

Q And that work is exclusive of Unit 1 work, isn't it?

A That work, in many cases, is very similar to the

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Unit 1 work where piping is being redone, but it is separate from the Unit 1 work.

Q But it is new work in the sense that there are new opportunities for error, right?

A Yes. There are new opportunities for errors, but it is all done under the present QA program that was used for Unit 1.

Q What about the work that was done before November of '81? Was that done under the QA program that was in effect before November '81?

MR. NORTON: Excuse me, Your Honor. We had a Unit 2 panel, and I don't see how this relates to statistics as to pre-81 work on Unit 1. We had a Unit 2 panel that Mr. Strumwasser was free to cross examine. We had Mr. Cranston here to testify about the program for Unit 2.

JUDGE MOORE: Where are you going with this line?

MR. STRUMWASSER: Applicability of sample findings from Unit 1 to Unit 2. Dr. Kaplan is the person who is going to tell us what the --

JUDGE MOORE: Go ahead.

BY MR. STRUMWASSER:

Q The Unit 2 design work was done under the QA program in effect for Units 1 and 2 before November of '81, is that right?

A (Witness Anderson) To my knowledge, that's true.

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ki 16:5

There may have been some minor differences.

Q Now, much of the Unit 2 design work that is taking place is taking place substantially different time than the corresponding work for Unit 1, is that right?

A There were some differences in time, but what we're dealing with here, at least in the Phase II effort, we are dealing with systems that are basically the same for the two units, and the design work was done for Unit 1 and it was copied for Unit 2.

Q Nevertheless, Mr. Anderson, you will have 4 or 500 engineers during design work explicitly on Unit 2, right?

A Very few of them are working in the systems area. They are mostly working in the area of piping design, which is separately done, and is being completely redone. That's involved in the seismic design area.

Q Can you tell me how many engineering years are going into exclusively Unit 2 design work? That's for the Unit 2 systems design work?

A Unit 2 systems design work? I can't give you a number. Very little. The systems are essentially designed on Unit 1. Very little system design work is going into Unit 2.

Q Nevertheless, there are opportunities for systems design work in Unit 2 -- for errors in that work that would not be reflected in Unit 1, right?

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ki 16:6

A There may be some minimal opportunity, but I can't really think of any offhand.

Q You can't assess the fraction of the work for which that is true, can you?

A I can't give a precise number on that, no.

Q Dr. Kaplan, is it your testimony that this Unit 2 design work is adequately assessed by sampling exclusively from Unit 1?

A (Witness Kaplan) I made no such testimony.

Q So you don't have an opinion?

A Sure. My opinion is that an inference about Unit 2 from the study that has been done on Unit 1 is clearly a matter of judgment. And I would turn for that judgment to the people who are most knowledgeable.

Q Dr. Kaplan, I have a question for you about your graphic. I don't know quite how to cite it. It's the same graph that keeps appearing with different shadings and elaboration.

MR. NORTON: How about a page number?

BY MR. STRUMWASSER:

Q Let's start with page 30 of this graphic. Are you with me?

A (Witness Kaplan) Yes.

Q Now, on page 30, you have adjusted the column widths to reflect the different sizes of design activity, right?

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ki 16:7

A Yes.

Q You have not adjusted the row widths to reflect the differences in design activity by discipline, have you?

A That's correct.

Q So to some extent, this graph can give the viewer an erroneous impression, right?

A It will also give an erroneous impression if we adjusted the widths of the rows.

Q Because?

A Because the real -- if you wanted to really give a right impression, you would make each box here have the right proportion of area to the other boxes, but if you do that, you then have to abandon the rectilinear aspect of the figures.

So I played with these various possibilities a little bit, and decided that this was probably as good as anything, less confusing, and told a little more of the story than the previous figure which seems to say that all systems are equal.

Q But we then cannot rely on the area of any of these boxes to actually represent the amount of design work that exists or has been sampled?

A Yes, I would agree with that. I think what you can see from the figure is the width of the column communicates the amount of design work in that system.

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ki 16:8

Q Now, turn to page 66, would you? One cannot tell from the density of shading here differentials in the depth of review or analysis by the various reviewing entities; is that correct?

A I think Mr. Anderson would answer that better than I.

A (Witness Anderson) I don't think you could tell anything about the depth. What we were trying to illustrate here is something that deals with the 3 out of 10 issue; that although the systems may not be defined exactly as 3 out of 10, roughly 30 percent of the engineering work was indeed included in the sample. So you can count the systems any way you like.

We're trying to illustrate that there were different amounts of engineering that went into the different systems, not into the different disciplines.

Q I'm talking about the question of shading now. And the question is whether the shading convention used on page 66 fails to distinguish between the depth of analysis by the various entities that did some of this review.

A I think it does fail to distinguish between the depth in a rigorous way.

Q Dr. Kaplan, you say that the IDVP did sequential sampling.

A Yes.

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ki 16:9

Q Isn't it true the IDVP never did any initial sampling in Phase II?

A Well, I think of the three systems as the initial sample and the so-called additional verification items as being the sequential sample, if you like, together with other things they looked at when questions came up that caused them to want to pursue something.

Also, in the design program, within the program plan was the option to take a whole new additional systems, if they felt that was desirable, so I would call that an element of sequential sampling in the plant.

Q In a traditional sequential sampling program, you would take a sample, examine the results with the lamda in terms of your convention, and fix the errors you found; then, if it was too high, take additional samples. Right?

New BU

A Not exactly, Mr. Strumwasser. The issue here is not the lamda. The lamda isn't even a well-defined animal. The real issue is to determine the quality of the design, and are there any safety-significant errors in there? That's the real issue, not lamda.

Q Well, if you took an initial sample and you were not satisfied that the design was adequate, adequately proven by that sample, you would then take additional samples. Right?

A Sure.

Q And each time when you found errors in these samples,

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you would be correcting those errors. Right?

A Sure.

Q So in a question of sampling, you would expect the quality of design work to be improving from sample to sample, right?

A You mean because corrections are being made?

Q Right.

A All right.

Q Now, you don't have any reports of sequential quality of design work from sample to sample from the IDVP, do you?

A Reports of sequential quality of design work? There were reports on what corrections were made.

Q There is no indication that there was any additional sample that was better than a previous sample in quality of time?

A It seems to me you have confused the point here. You said a minute ago on this notion of quality of design, improving with sequential sampling, that it was improving because you fixed the errors that you have already found. So how does that relate to your question about a new increment of sampling having different quality?

Q It's a little hard to relate because, in fact, there is none of this reporting of a sequence of samples and the quality of the design in each sample of the sequence, right?

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MR. NORTON: I'm going to object. Insufficient foundation.

MR. STRUMWASSER: In the interests of time, I will withdraw the question.

JUDGE MOORE: Continue.

BY MR. STRUMWASSER:

Q You also indicated the IDVP has done stratified sampling.

A (Witness Kaplan) Yes.

Q In fact, they divided the sample into two strata, one which they sampled completely, and one from which they didn't take any samples, right?

A You can view it as such, yes.

Q As a statistician, do you understand that to be valid stratified sampling?

A I consider this a valid sampling procedure for the purpose of the sampling process, yes.

Q Do you understand this to be what statisticians mean when they talk about stratified sampling?

A When they talk about stratified sampling, they're not talking about design verification problems. So the thoughts and language appropriate to the kinds of things they are talking about don't really fit here. For this problem, I think this sampling process was appropriate.

Q Then your use of the term "stratified sampling," a

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term that comes out of statistics, is misleading, isn't it?

A No. My use of the term relates to the fundamental idea of stratification; namely, that different parts of the space are different. You take advantage of that knowledge. That's the fundamental idea. The specifics of the size of the sample within each strata is not the essential part of it.

Q How about the existence of any sample from each strata?

A What about that, Mr. Strumwasser?

Q Would you agree that in stratified sampling, you sample from each strata?

A Generally, yes.

Q And, in fact --

A I think that was done here.

Q And that, in fact, you randomly sample within strata, don't you?

A Not necessarily.

Q Can you name any published research that claimed to be using stratified sampling in which they sampled within strata non-randomly?

A You mean sampled 100 percent within strata?

Q No, non-randomly.

A Well, why would you sample non-randomly within a strata?

Q That's my question.

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A Within the framework you are putting forth, where you have a strata and within that strata you essentially have a ball and urn situation, lots of little elements all equal, et cetera, et cetera, and you're looking for a lamda within that strata you want to define, you want to sample randomly.

Now, here, if by strata you mean a system, you don't want to sample randomly within that system for all the reasons I discussed here, because one wants to look at the integrative aspect of how the pieces work together. You don't want to sample little pieces of the design and say is that piece right or not.

First of all, it misses an essential ingredient of what you're looking for; and secondly, it's enormously wasteful of your sampling resource.

Q I'm sorry. I need an answer to my question. Can you name any published research that claimed to be using stratified sampling that sampled within the strata non-randomly?

A I don't know of any such.

Q Can you name any published research that claimed to be using strata by sampling that did not sample something from each stratum?

A Mr. Strumwasser, I am not familiar with the sampling literature, period.

Q Thank you.

JUDGE JOHNSON: May I ask a question? How are

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you defining strata here? I went along, very happily, thinking that the different strata might be mechanical, electrical, instrumentation, and control in HVAC.

WITNESS KAPLAN: That's one kind of strata. There are multiple kinds of strata in this design verification situation.

JUDGE JOHNSON: What would be another stratification?

WITNESS KAPLAN: Air systems, water systems, electrical systems would be another kind.

End 16

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JUDGE JOHNSON: So you've got your space stratified horizontally and vertically, according to your figures, such as 8-1, is that it?

WITNESS KAPLAN: Yes. The different systems are themselves a strata. That's a kind of strata. The question of an air, water, or liquid forms another strata. Excuse me. Air, liquid, or electrical is another kind of strata.

JUDGE JOHNSON: Back earlier, you gave an answer to a question that seemed to say there were two kinds of strata, one in which you took samples, and one in which you didn't take any samples at all. Did you get an answer to a question like that, Mr. Strumwasser?

MR. STRUMWASSER: Yes.

JUDGE JOHNSON: I mean, it seems to me that there was stratified sampling of the veritical systems, sampling from each of the veritical strata, if not from each of the horizontal strata.

WITNESS KAPLAN: There certainly was sampling from each of the horizontal strata.

MR. STRUMWASSER: I think it was just the opposite.

JUDGE JOHNSON: Okay.

WITNESS KAPLAN: Three total vertical columns were chosen, and they were so chosen as to cover all the horizontal strata.

JUDGE JOHNSON: All the strata in a vertical

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direction is what I was talking about,which would be the horizontal strata, I suppose.

WITNESS KAPLAN: Okay. At any rate, all the horizontal strata had been touched in the sample. In addition, when you consider the additional verification items, the sample touched all the vertical columns.

JUDGE JOHNSON: Okay. By your answer, what you were implying is that there were vertical strata which were not -- some vertical strata, which without the sequential sampling were not explored?

WITNESS KAPLAN: Or without the sequential sampling, without the additional verification items, all those vertical strata, other than the three sampled systems, were not included.

JUDGE JOHNSON: Okay. I think I understand it.

BY MR. STRUMWASSER:

Q Dr. Kaplan, you played no part inthe design of the IDVP program, did you?

A (Witness Kaplan) That's correct.

Q Nor in the design of the ITP?

A That's correct.

Q You spoke to Dr. Cooper for the first time, just a few weeks ago?

A Yes.

Q And at that time, he told you he had only begun

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to do some reading on the statistics, the previous few weeks?

A He told me he had begun to do some reading on the Bayesian point of view.

Q And he was only beginning to understand how it could be used?

A Something like that.

Q That was summer or fall of 1983?

A Yes.

Q Are you aware, Dr. Kaplan, that you are the statistician for the DCP?

A Am I aware of that? Nobody has labeled me that, as far as I know. I don't think of myself as a statistician.

Q Are you aware that you are the expert in statistics for the DCP?

A I would rather call myself the expert in probabilistics.

Q Are you aware, Dr. Kaplan, that you are also the expert in statistics for the IDVP?

A I will say the same thing.

Q And when did you first begin to work on Diablo Canyon?

A Last spring, late last spring.

Q Dr. Kaplan -- wait a minute. Late last spring -- you mean 1983?

A Yes.

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Q Around June or so?

A Yes. May perhaps.

Q Dr. Kaplan, if the IDVP had sought out your services in 1981, could you have helped them design their program?

A I certainly could have participated in the thinking and planning. I'm not sure the result would have come out any different than it did.

Q Would the program have looked any different?

A That's what I meant by result. I'm not sure that had I been there, I would have argued for a different set of systems to sampling than they took.

Q Would you have urged them to make their assumptions visible and their objectives visible?

A Perhaps.

Q Would you have recommended to them in 1981 that they do a PRA?

A As part of the design verification program?

Q Yes.

A Not as part of the design verification program. I might recommend PRA for other times and purposes.

Q Dr. Kaplan, I am having a document distributed to the Board and the panel that has been marked Governor's Exhibit 45.

(Document distributed to Board, parties, and witnesses.)

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This is an article written by you and John Garrick on the quantitative definition of risk.

A Yes.

JUDGE MOORE: Governor's Exhibit No. 45 is marked for identification.

(The document referred to was marked Governor's Exhibit No. 45 for identification.)

XXXX

BY MR. STRUMWASSER:

Q Dr. Kaplan, would you read aloud the first paragraph on page 11, which is the first page?

A (Witness Kaplan) "As readers of this journal are well aware, we are not able in life to avoid risk, but only to choose between risks. Rational decision-making requires, therefore, a clear and quantitative way of expressing risk so that it can be properly weighed along with all other costs and benefits in the decision process."

Q Does that correctly reflect your views on the subject?

A Yes.

Q Do you think it's applicable to Diablo Canyon?

A Yes.

Q The verification of design.

A Well, I would say yes in principle, and in general, depending on what you want to conclude from that.

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Q Turn, please, to page 24 of that article, and if you would, please, read aloud the third full paragraph, the one that begins with the word "Thus."

A "Thus, one cannot talk about risk in isolation. One needs to adopt a decision theory point of view and ask what are my options, what are the costs, benefits, and risks of each? That option, with the optimum mix of cost, benefit and risk is selected. The risk associated with that option is acceptable. All others are unacceptable."

Q That also expresses your views?

A Yes.

Q And it's also applicable to Diablo Canyon?

A In principle, yes. And in general, yes.

MR. STRUMWASSER: Mr. Chairman, may Governor's Exhibit 45 be received in evidence?

JUDGE MOORE: Any objection?

MR. HAVIAN: No objection.

MR. NORTON: Yes, Your Honor. I'm not sure what the rest of the article contains and what the intended use of it by Mr. Strumwasser is.

JUDGE MOORE: Mr. Strumwasser.

MR. STRUMWASSER: Why don't we defer the question -- or the admission request?

JUDGE MOORE: Excellent idea. Continue.

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BY MR. STRUMWASSER:

Q I have another article for you and the Board, Dr. Kaplan, which has been marked Governor's Exhibit 46.

(Document distributed to Board, parties, and witnesses.)

JUDGE MOORE: Mr. Strumwasser, I think this would be an excellent time to take a break.

MR. STRUMWASSER: I'm only a couple of minutes from conclusion.

JUDGE MOORE: All right.

BY MR. STRUMWASSER:

Q This is an article by you, entitled "Safety Goals and Related Question," right, Dr. Kaplan?

A (Witness Kaplan) Yes.

JUDGE MOORE: Governor's Exhibit No. 46 is marked for identification.

(The document referred to was marked Governor's Exhibit No. 46 for identification.)

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BY MR. STRUMWASSER:

Q In this article, you are evaluating the question, what is accpetable risk, right?

A (Witness Kaplan) Evaluating the question itself, rather than giving an answer to it?

Q Correct.

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A Yes. I'm discussing that, the sense of asking such a question.

Q Turn to page 268, would you, and read aloud the paragraph, the second full paragraph?

A "As a contribution --

Q I'm sorry, the second full paragraph.

A "We shall argue, in fact, that so phrased, and taken at face value, the questions are logically absurd, and in this sense bear a resemblance to other well known questions of the same ilk, such as how many angels can stand on a pin; have you stopped beating your wife; what is the sound of one hand clapping; does the barber who shaves those men who do not shave themselves shave himself; and is the statement, 'This statement is false' true or false?"

Q And you are lumping in the same category with those questions, the question what is an acceptable risk? Right?

A Right.

Q Now, if you would turn to page 274 and if you would read the first full sentence, down to the end of that paragraph.

A "The problem is in the idea suggested by the acceptable risk type of language, that decisions can be based upon risk alone, considered in isolation from the cost and benefit elements of the triplet; clearly this idea is absurd, for none of us would accept any risk at all, except when there

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are concomitant benefits that make it worth while. We thus now see the linguistic problem, for considered in isolation no risk is acceptable. The only risk that is acceptable is zero risk. The only amount of safety that is safe enough is infinite safety. Total safety is the only valid safety goal."

Q Now, if you would, Dr. Kaplan, would you turn to page 65 of your testimony and read the last sentence, please, of that page?

A "The final number shows sufficient confidence to support, in my judgment, the conclusion that the sequential sampling process need not continue beyond this point."

Q Dr. Kaplan, would you agree that the phrase "sufficient confidence" is as meaningless as the phrase "meaningless assurance"?

MR. NORTON: Object. Foundation. Reasonable assurance is meaningless.

MR. STRUMWASSER: It's in the exhibit you refused to have admitted.

MR. NORTON: Excuse me, Your Honor. Insufficient foundation.

MR. STRUMWASSER: He just testified that was --

JUDGE MOORE: He has not testified at all about the reasonable assurance, has he?

MR. STRUMWASSER: Yes, sir --

JUDGE MOORE: In his testimony?

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MR. STRUMWASSER: In the first part of the article about safety goals, reasonable assurance -- I'm sorry, acceptable risk. Thank you, Mr. Norton, I misread that.

MR. NORTON: Gee, I'm sorry.

(Laughter.)

JUDGE MOORE: Start over for my benefit and state it all.

BY MR. STRUMWASSER:

Q Dr. Kaplan, would you agree that the phrase "sufficient confidence" is as meaningless as the phrase "acceptable risk"?

A (Witness Kaplan) No, I would not agree.

Q Would you agree that the sentence on page 65 that you have just read does not address the question of the costs of additional verification?

A No, I wouldn't agree with that at all. I think the sentence is made in the decision theory context. Sufficient confidence to support, in my judgment, the conclusion that further sampling is not needed.

Part of my judgment considers the costs of further sampling.

Q Are you telling us that sentence has implicit within it the costs?

A Obviously, because if there are zero costs, one would keep going forever.

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Q Turn back, if you would, to Governor's 46, page 274, the letters ORC there stand for what?

A I don't remember.

Q Old Regulatory --

A Criteria. Thank you.

Q Would you read the first two full paragraphs of that page, starting with "Now"?

A "Now, the proponents of the Old Regulatory Criteria are, of course, aware of this point and, when pressed on it, say something like, "Yes, of course, but the cost and benefit elements are well known, and therefore need not be brought explicitly into the discussion. They may regard it as understood in the background."

Q Go on.

A "This would not be too bad a position if it were true. Unfortunately, it is not clear that it is understood by certain vocal elements of the public and the media. It may not even be understood by certain segments of the regulatory establishment, for one sometimes hears, coming from these segments, such statements as" -- there's an extra "as" in there -- "such statements as safety goals must be met, regardless of cost, and 'it is not our job to consider costs; our job is just to protect the public safety.'"

Well, if you don't consider costs, the only acceptable risk is zero risk.

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Q Dr. Kaplan, you're saying it's okay for you to treat the costs of further verification implicitly, is that right?

A You mean as opposed to quoting how much it's going to cost you, how much more, et cetera, et cetera?

Q As opposed to explicitly.

A Yes.

Q And that's because you're not a member of any of the vocal elements you referred to?

A I've lost you, sir.

Q Why is it okay for you to treat them implicitly, but the vocal elements you referred to in the article cannot?

A My point is, in this paragraph,that the vocal elements are not treating costs implicitly or otherwise. That's how they come to a logically absurd conclusion.

Q But you are?

A Treating costs?

Q Yes.

A Yes, I am.

Q What is it, then, that is well understood about the costs of further verification?

A What is it -- what is understood is that further verification is expensive.

Q Do you have any numbers associated with that?

A I don't have any specific numbers, but I do know

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what it costs to keep a multibillion dollar facility idle.

Q Now, did anybody tell you that it was necessary to keep it idle while the verification was going on?

A No.

Q In theory at least, it's possible to do additional verification while the facility operates, isn't it?

A Yes.

Q That would dramatically reduce the costs, wouldn't it?

A Yes.

Q Would it still be your opinion that there was -- the cost was too high to justify additional sampling?

A Not necessarily. I would have to sit down with the knowledgeable people on the subject and make a judgment as to whether additional sampling ought to be done, and what ought to be done, and whether it is worth the investment of resources that could be used elsewhere.

Q And you would, in the course of that, have to come to some kind of an assessment of the costs of a Region 2 or 3 error?

A Of course.

Q And the reliability of one's assessment of safety significance of an error?

A Yes.

Q You haven't done that yet, have you?

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A Not, not explicitly.

Q Would you turn back to Governor's 46 one more time, Page 276, and read the last paragraph that starts with "We can."

A "We can then begin to identify optimal options in a rational way. We cannot, of course, examine all options and thus theoretically speaking, will never do a complete analysis of a decision tree. In practical terms, however, a reasonably moderate set of options will be found to be a sufficiently good approximation, together with an honest attempt to quantify the impact vector of each option, including the uncertainties in that quantification. When this set of options has been calculated, our understanding will have been improved to the point where we can confidently give definite guidance as to when, how, and where to build plants, make changes, et cetera.

"The reasoning behind this guidance will also then be clear, and as a result of this clarity, we may hope to replace current adversarial and antagonistic attitudes by understanding, by recognition of joint interests, and by cooperative actions for the common good."

Q That is also applicable to Diablo Canyon?

A Yes.

Q And the verification program?

A Yes.

Q Now, would you agree that you have thus far at least

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not provided the costs, benefits, and risks necessary to assess that question?

A Not in an explicit form.

Q Isn't it true that you have failed to provide the clear reasoning about costs, benefits, risks that you say should be provided for a regulatory decision like whether additional verification is appropriate?

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A Well, I think we certainly provided clarity on the question of whether there might be a serious error in the remaining portion of the design. We certanly put a lot of clarity on that. And so that relates to the question of the value of further sampling. The question of the cost of further sampling, I have not attempted to do explicitly.

MR. STRUMWASSER: Mr. Chairman, that is the extent of my use of 45 and 46. I ask that they be received in evidence at this time.

MR. NORTON: Your Honor, without knowing what is in the rest of the document, I may not have any objeciton, but not knowing what Mr. Strumwasser might intend to do with the rest of this very lengthy document, I have no objections to the read portions that he asked the witness to read being in evidence, but I don't know what blindside awaits me for those portions that were not read.

MR. STRUMWASSER: I'll tell you what, Mr. Chairman. I have learned not to bring here documents that have less than all the pages attached. If Mr. Norton would like to take the recess to decide if there is anything objectionable, --

MR. NORTON: I'm not going to spend my ten-minute recess reading a thirty-page document.

JUDGE MOORE: Mr. Norton, you will, if we would like you to.

MR. NORTON: You'll have to give more than ten

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minutes is what I'm telling you.

JUDGE MOORE: But I think, why don't we just hold admitting those until Mr. Norton gets a reasonable opportunity to read them, so I can hear any further objections he might have, and then deciding. Tomorrow morning will be soon enough for admitting them.

MR. STRUMWASSER: I have no further questions for this panel.

JUDGE MOORE: We will take a fifteen-minute recess and then reconvene.

(Recess.)

JUDGE MOORE: Come to order, please.

Are the Joint Intervenors prepared for cross-examination?

MR. HAVIAN: Yes, Mr. Chairman.

JUDGE MOORE: Prepared to proceed?

MR. HAVIAN: I like to think so.

CROSS-EXAMINATION (CONTINUED)

BY MR. HAVIAN:

Q Dr. Kaplan, will you please turn to your professional qualifications, the back of your testimony?

A (Witness Kaplan) Yes.

Q You have a Ph.D. in mechanical engineering and applied mathematics, correct?

A Yes.

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Q Is that a single Ph.D. or two separate Ph.Ds.

A That's one.

Q Do you have a Ph.D. in statistics?

A No.

Q Have you ever taken any graduate level courses offered by a Department of Statistics at any university?

A No.

Q Have you ever taken any undergraduate level courses in a Department of Statistics at any university?

A No.

Q Have you ever heard of the American Statistical Association?

A Yes.

Q The Institute of Mathematical Statistics?

A Yes.

Q The Biometric Society?

A Yes.

Q Aren't those three of the main American statistical societies?

A I believe so.

Q Are you a member of any of the three?

A No.

Q Do you know what a referee journal is?

A Yes.

Q Can you, in your resume, point to which articles

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of yours have been published in a referee journal in statistics?

A None in a statistical journal.

Q What is a referee journal?

A It's a journal which, when you send your articles, they sent it out to referees before accepting it for publication.

Q Isn't the purpose of sending it out to those referees to ascertain whether the methodologies used in the article are valid?

A That's one of the purposes.

Q And I believe you said earlier you do not consider yourself to be a statistician?

A That's correct.

Q Dr. Kaplan, suppose a meteorologist looks at the sky, and using his experience and judgment, he says that there is an 80 percent chance of rain.

In your view, would this be in the realm of probablistics?

A Yes, I would take that as a probablistic statement.

Q And what is another meteorologist, using his judgment, looks at the sky and says there is a ten percent chance of rain? I take it that would also be in the area of probablistics.

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A Yes.

Q How do you reconcile these two statements?

Let me state that differently. How is one to ascertain which of those two statements is the more accurate?

A I would say they are both entirely accurate as expressions of the individual's state of knowledge.

Now if you want to ask me what my probability is that it is going to rain, that's another question.

Q But if one only has the opinions of the two meteorologists and no further information, is there any rational basis upon which to make a decision about what the chances are that it is going to rain?

A One would make a decision about whether or not to take an umbrella or not, so you would want to base that decision on your own probability, your own assessment about whether it's going to rain or not.

Is there a rational basis? I think one can be rational in that circumstance.

Q No. I'm saying, is there a rational basis to determine which of these two meteorologists is more likely to be correct?

A I don't remember; did you specify that both were equally knowledgeable and reliable meteorologists?

Q Yes. If I didn't, I meant to.

A Given that you have specified that, there is no

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basis to prefer one or the other.

Q Can you define "scientific method"?

A I suppose I could try.

I would say a scientific approach is characterized by logic. It is often characterized by numerical expression, systematic, et cetera.

Q Isn't the ability to replicate the results a cornerstone of the scientific method?

A When you are talking about the measuring of physical phenomenon, before you assert that a phenomenon exists, you like to see it repeated, that it's there all the time. If it's not there all the time but it is there some of the time, then what you can say is, "It's there some of the time."

So some phenomena are consistently repeatable, and others are not.

Q But what about the scientific analysis? Isn't it a cornerstone of the scientific method that one must be able to reach consistent results using the same methodology, even if it's employed by different persons?

A Could you give me an example of what you are talking about? It's sort of an abstract definition of scientific method and methodology and so on.

Q In other words, you have an experiment, and as part of that experiement, you observ e some phenomenon, and

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you subject it to certain testing, and you get results.

And what I'm saying is, isn't it important that if I do the testing, as opposed to you do the testing, that we both end up with the same results? Isn't that a cornerstone of scientific method, that it be replicable?

A I don't understand. If you do the experiment and you get a different result from the result I get when I do the experiment, then the evidence that we have is that there are two different results.

Q I don't think I will argue with that.

A You are seeming to say that you would like to infer from one of the experiments that something is true, and if someone else does the experiment and gets a different result, which is contrary to that inference, then you can't make the inference. I have to agree with that.

JUDGE JOHNSON: I think, Dr. Kaplan, Mr. Havian is talking about perhaps when you use the word "experiment," I think you are looking at perhaps a statistical experiment.

Let's say we are dropping a weight and measuring the acceleration of gravity as a result of this experiement. If we describe that experiment in a journal, presumably if anyone else reads our description of the experiment and reproduces our experiment, he should get approximately the same value of the acceleration of gravity, if he carries out the same experiment. And I think this is the fundamental

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cornerstone of the scientific method that Mr. Havian is discussing.

WITNESS KAPLAN: I hear you. Actually the way I look at it, it goes the other way around. If you measure the acceleration of a body and Mr. Havian measures it and Mr. Galileo measures it and everybody gets the same acceleration, then we enunciate a principle, and the principle says, "Every time we measured, it followed, and regardless of who measures it, they get essentially the same number; therefore, it appears that this number is, quote, universal or constant. And that statement is now really just a shorthand way of describing the results of all these experiments.

BY MR. HAVIAN:

Q Let me ask it a slightly different way. If three different individuals all perform an experiment and all reach different results, --

A (Witness Kaplan) Yes?

Q -- doesn't that imply that the value of the methodology used in the experiment may be suspect?

A I find those words a little vague, "value of methodology." What I hear is, whatever procedure these three people are using are leading to three different results, then there is something about the procedure that is not the same in the three cases. If you want to speak

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about in terms of value of methodology, okay, I hear it in terms of there is something different between the three applications.

Q Isn't it possible that the nature of the procedure lends itself to idiosyncracies of the person who is applying the vprocedure?

A Of course. The vprocedure may not have all of the key variables under control.

Q You said you are an expert in probablistics.

A I like to think that.

Q Can you cite a reference to a single textbook or monograph not authored by you that uses the term "orbablistics"?

A Inthe way I do?

Q Yes.

A Well, yes. I think DeFinetti tends to use the way I do.

Q Can you cite me to the article or text?

A There are a couple of textbooks. I don't remember the exact titles. There's a couple of volumes of a textbook by DeFinetti.

Q And when was that published?

A I don't know. Somewhere in the last decade or two.

Q Can you tell me who Mr. DeFinetti is?

A Other than that he is the author of these textbooks and an Italian mathematician of some sort, I really don't

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know much about him.

Q Are there any other authors who have used this term in the way you have?

A Sure. There is an Englishman called Lindley, who has a sequence of books that looks at it much the way I do.

Q Does he use the term "probablistics"?

A You mean as opposed to "probability" in the way that I use "probability"?

Q Just does he use the term "probablistics"?

A I don't know whether he uses "probablistics." I can't remember specifically seeing that word. But he uses probability, I believe, much the way I use it, so "probablistics," I'm using it to mean the subject of probability.

Q Anyone else?

A Sure. Myron Treibus.

Q Again, can you tell me the text thatyou are thinking of?

A The specific title, I don't know, but it has something to do -- it's a thermodynamics text.

Q Do any of these authors refer to you or anything you have written as the source of any of their ideas?

A Not to my knowledge.

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Q Has any other author ever referred to anything you have written, picked up on this concept of probablistics

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and expanded upon it?

A There are lots of people who refer to my papers, and there have been lots of people who have picked up on my point of view on probability.

Q Are these people who have also written articles?

A Oh, yes.

Q And they have picked up. And have they expanded upon your notion of probablistics?

A Expanded upon it?

Q Let me ask it this way. Have they utilized it in other contexts?

A Yes, I think they have certainly utilized it.

Q Can you describe for me what those other contexts have been?

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A Well, I saw one recently that had to do with the question of making inferences about the uses of drugs in people from the experiments on those drugs in animals. There is an example of application that I didn't do, that someone else has done. That is one that leaps to mind immediately.

Q Any others?

A Well, I am sure there are such. Nothing is coming to mind immediately.

Q Yesterday, or maybe it was the day before -- I'm sorry, not yesterday, last week, Dr. Seed offered testimony in these hearings. Were you here during Dr. Seed's testimony?

A No.

Q During his testimony he stated essentially that if an idea is published in the context of soils and seismic areas, that if it is a new idea and the scientific community thinks the idea is valid, other people tend to pick up on it and it tends to be expanded upon and people explore it.

Do you agree with that as it applies to the area of probability?

A I would agree with that.

Let me correct something here. There seems to be a premise in your question that I originated this way of looking at probabilities. Is that so?

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Q No, I just got the impression from your testimony that you were a pioneer. I didn't get the impression that you necessarily originated the idea.

MR. NORTON: Excuse me, could we get to a question and answer that we could all join in on?

MR. HAVIAN: No, Mr. Norton --

JUDGE MOORE: Is that a veiled objection, Mr. Norton?

MR. NORTON: We just seem to have statements with no questions. I am objecting to the form of the question.

MR. HAVIAN: The witness asked me for a clarification and I provided it.

JUDGE MOORE: I think it is clear, but it does help if we put questionmarks at the end of our statements.

MR. HAVIAN: Fine.

WITNESS KAPLAN: If I understand the question, do I think when somebody has a good idea in the scientific area that other people pick up on it and use it?

The answer to that is yes.

BY MR. HAVIAN:

Q Will you please turn to page 6 of your testimony? The sentence that states -- are you there?

A (Witness Kaplan) Yes.

Q The sentence begins on line 5 that says, "Balls in this problem are not equal nor are they separate."

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By "balls" I assume you mean systems at Diablo Canyon, is that correct as an example of what you mean by "balls"?

A It is really however you choose to carve the design up into whatever pieces, you will find that those pieces are not equal, nor are they separate.

Q And so how much can you say about the balls that are not identical based on your look at the -- let us call it the unique ball?

A You are referring to the sampled portion of the design versus the unsampled portion, is that what you are asking?

Q That's correct.

A How much can you say about the unsampled portion from the sampled portion?

Q Yes.

A Yes, that is exactly the question we are looking at in Section 7, and that is exactly the quetion I put to the Diablo Canyon people when I asked them "What can you say now about the likelihood of a serious error existing in the unsampled portion from the evidence of the sampled portion?" That was basically what I asked them. I pulled that question apart into a couple of pieces, but that is the question.

Q Isn't it difficult to know the limitations of how much you can extrapolate from a sample, unless that sample is

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random?

A I'm sorry, the question just doesn't register.

Given acertain piece of information with respect to another proposition you can ask what is the probability of that proposition in light of that information. You know that is where it stops, the way I look at it.

Q Maybe I can get at the question a different way.

You said that judgement is difficult to enter into the classical framework earlier in your testimony.

A Yes. The classicists tend to set up their points of view and formulations to keep judgment out of it.

Q Isn't it possible to combine statistical -- classical statistical techniques and judgment?

A It is possible.

Q And in fact, even in classical statistical techniques, isn't the use of judgment essential in order to stratify the sample properly?

A Certainly.

Q For instance, let me give you an example, let's take pipe supports, are you familiar with what pipe supports are?

A Yes.

Q Couldn't you use engineering judgment to ascertain that pipe supports aredifferent from valves, for example?

A I would think so.

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Q And then wouldn't you, couldn't you randomly sample from the population of all pipe supports?

A You could.

Q And so random sampling in that case would not preclude the use of engineering judgment, would it?

A No, nor does the use of judgment preclude random sampling.

Q Now isn't the purpose of doing a randome sample to screen out biases?

A The purpose of a random sample is to guard against unknown stratifications. That is the way I would say it.

Q Isn't that another way of saying to screen out potential biases?

A All right.

Q Isn't that true of unforesee as well as foreseeable biases?

A If you have a foreseeable stratification, you would make use of it, and so you do use random sampling to guard against unforeseenstratification.

Q In fact, isn't the whole reason that random sampling is useful, is that it will screen out biases that are not apparent to the decisionmaker?

A I think you are saying the same thing. One would use random sampling to give you some protection against unforeseen stratification.

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Q Now isn't it also true that those who are proponents of Bayes' theorem, namely Bayesians, still believe in the use of random sampling?

A Of course, even I do where appropriate.

Q Now, in the case of your testimony here, don't you operate under the assumption that the sampled systems are indeed a random sample?

A You mean in my calculations of those curves?

Q Yes.

A Oh, yes. I treated those as if they were random samples from the horizontal strata in those calculations.

Q What basis do you have -- strike that.

Isn't it also true that you would not have been able to apply those formuli without the assumption that this sample was random?

A That's correct. Those particular formuli that I used assumed that it was a random sample.

Q And aren't the conclusions in your testimony based upon the results of those equations?

A You mean those curves?

Q Yes.

A Those curves are based on those equations.

Q And in turn, aren't your conclusions about the adequacy of the IDVP based upon the results of those curves?

A Based on those curves, no.

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Q Where is your independent basis then, apart from those curves and the equations for your conclusions?

A Chapters 7 and 8 is my basis for conclusions about the adequacy of the sampling program, not those curves which I take pains to say are not very informative for decision purposes.

Q Aren't the equations that you utilize in Chapter 7 and 8 dependent upon the assumption that the sample is random?

A No.

Q Isn't the tranditional use of Bayes' theorem as you use it here dependent upon the assumption that the sample in question is random?

A Here being chapter 7?

Q yes.

A No.

Q Doesn't chapter 7 utilize Bayes' theorem?

A Yes.

Q And isn't one of the fundamental underlying assumptions of Bayes' theorem that there is a random sample?

A No.

Q You said that you were not an expert on sampling literature, is that correct?

A That's right.

Q Have you read any sampling literature?

A Yes.

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mm8± Q Can you tell me what you have read? Give me a few examples of the major texts on sampling that you have read.

A Well, there is a book by Lass and somebody called "Probability and Statistics."

Q When was that published?

A I don't know.

Q When did you read it?

A Last night.

Q Was that the first time you had read it?

A That book?

Q Yes.

A Well, I have looked at it before.

Q Looked at it. Please tell me what you mean?

A Well I also looked at it last night, I didn't read it cover to cover.

Q How long did you look at it the first time?

A I have referred to it from time to time through the years.

Q Have you ever actually read it?

A Cover to cover, no.

Q What portion of it would you say you have read?

MR. NORTON: Your Honor, I am going to object. I don't know whether we are getting to qualifications of the witness, but if we are those challenges were supposed

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to have been submitted to the Board about a week or two before the hearings. We seem to be spending an inordinate amount of time challenging, or attempting to challenge qualifications.

JUDGE MOORE: We will listen to your response, Mr. Havian.

MR. HAVIAN: We will concede that this individual is qualified to offer expert testimony on statistics to the extent that he meets the standard, which is simply that he is better qualified than someone that you would pull in off the street. That does not imply that the weight of his conclusions are entitled to the same kind of weight that somebody with a great deal more training in statistics --

JUDGE MOORE: We are well aware of your point. I think you have already made it.

MR. HAVIAN: This particular point, Mr. Chairman, refers to sampling techniques which I think is the fundamental point that we disagree with Dr. Kaplan's theories. And I think it is very important to probe his understanding of sampling specifically as opposed to his understanding of statistics generally.

JUDGE MOORE: Make it brief, Mr. Havian. I stand on what I have said, I think you have made your point, but go ahead for your record.

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BY MR. HAVIAN:

Q Are there any other texts that you have read on sampling techniques?

A (Witness Kaplan) There are numerous texts and papers that I have looked at from time to time in my life. And I don't have them on the tip of my memory.

Q You can't name -- can you name another one besides the one you have already named?

A Actually I can't. I don't remember names or authors.

Q Have you ever read William Cochran's text called "Sampling Techniques"?

A No.

Q Were you aware of the fact that that was one of the most fundamental texts in the field?

A No.

Q Do you know who Roger Reedy is?

A I have heard his name.

Q Were you aware that Mr. Reedy during his deposition stated that if one were to look at a quality assurance program, one would be more likely to find deficiencies in that program when it looked at areas of lesser safety significance than if one looked at the program in areas of greater safety significance?

Were you aware of that conclusion?

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A No.

Q Do you agree with it?

A I don't know, I haven't thought about it. I would like to know the context of the statement before offering an opinion about that.

Q Does it make sense to you that perhaps people are more cautious when they are dealing with systems that are particularly important to safety than they would be with systems that are not considered as important to safety?

A Yes, indeed it does.

Q Would you turn to page 36 of your testimony?

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A (Witness Kaplan) All right.

Q Lines 25 and 26.

A Uh-huh.

Q Will you read those lines, please?

A "They were chosen with an eye to the size and complexity of the systems and to their significance to safety.

Q I believe earlier when Mr.Strumwasser asked you, you could not recall whether significance to safety was one of the criteria which the IDVP used to choose the sample.

Does this refresh your recollection?

A No, I did not say that.

Q I'm sorry.

A It doesn't stimulate my recollection, no.

What I said was, my understanding was that complexity and significance to safety were parts of the thinking that went into selecting the systems.

Q Isn't it possible that because the IDVP considered systems that were particularly important to safety, these would be the systems least likely to contain errors?

A It's possible, but they would be the systems most likely to contain safety-significant errors.

Q In your view, would that bias the sample?

A I would think it would bias the sample in the direc-tion of greater likelihood of coming upon safety-

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significant erros, if such existed.

Q Are you familiar with the components that failed during the accident at Three Mile Island?

A Not in great detail.

Q If I told you that one of the components that failed would not normally have been considered an item that was particularly important to safety, would that surprise you?

A No.

MR. NORTON: Excuse me. I am going to object as to foundation and also clarification of the question. It obviously wouldn't surprise the witness, because he just said it, and I'm not sure what the thrust of the question was. There is no foundation for that evidence.

MR. MOORE: Mr. Havian, your response?

MR. HAVIAN: Mr. Chairman, I asked the witness, I simply gave him a hypothetical as to whether or not that would surprise him if that were true, and he answered no.

MR. NORTON: Excuse me, Your Honor. He didn't put in the "if it were true" part. That's what puzzled me.

BY MR. HAVIAN:

Q Would your answer change if I said "if it were true," would that surprise you?

A (Witness Kaplan) I thought you did say "if I told your" or "if I were to tell you this." That's what I thought

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you said.

Q I want to make Mr. Norton happy.

A "If I tell you that a non-significant piece of equipment failed, would that surprise you?" No, it wouldn't surprise me.

Q Would it surprise you to know that it was responsible for the most major accident in the history of the nuclear industry?

A That is your interpretation of responsibility. I would have to hear your arguments for it.

Q Am I to take it from your response --

A That would surprise me, yes. Your interpretation would surprise me.

Q Are you familiar with the accident at Three Mile Island?

A Somewhat.

Q Will you describe what your understanding is of the events there?

A My understanding is that the major determinants of that incident were mistakes made on the part of the operators.

Q Isn't it also true that had some of the equipment at the plant not failed, those operator errors would not have resulted in the accident that, in fact, occurred?

A Well, that may be true, but that's a different

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statement from what you said. You said this equipment is the major cause, or something to that effect.

Q Isn't it, in fact, more likely that the cause of a major nuclear accident would be found, at least in part, in components whose safety significance was not considered major prior to the accident? Isn't that possible?

MR. NORTON: Excuse me, Your Honor. The first question, "Isn't it likely," and then at the end of the question, he said, "Isn't that possible," again, which of the two questions is the witness to answer?

BY MR. HAVIAN:

Q Isn't it possible?

A (Witness Kaplan) Isn't what possible?

Q That a major nuclear accident could be the result of a safety component which, prior to the accident, had not been considered of great safety significance?

A I suppose that's possible.

Q Are you familiar with the nature of components and systems that are considered safety-related?

A Not in great detail.

Q Generally?

A Somewhat.

Q Isn't the reason that those components and systems are designated as safety-related, that they are required in order to safely shut down the plant in the event of an

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accident?

A I don't think I would say it that strongly. My understanding of it is that they are systems which contribute to shutdown, but not necessarily required, not in the sense that I wouldn't use the word "required" in the sense that if that doesn't work, that it can't shut down.

There are other systems and methods which can shut the plant down. And the fact that there are other such does not mean that a given system can't be called a safety system.

Q I would like to go to your earlier testimony about the probability, the priors you got, the prior distribution that you got from the DCP.

A Yes?

Q When considering the validity to attach to that probability, would it influence you to know that, prior to 1981, PG&E maintained that it had a fully adequate QA program, and that the plant contained no major design or construction errors?

A That would interest me.

Q As the decision-maker, if you were given a probability of .999, as you were, by the DCP, how much would you discount that prior, based on the information that prior to 1981, PG&E was confident that its QA program was adequate, and that there were no major design or construction errors

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at Diablo Canyon?

A Given solely that statement?

Q And the knowledge that that statement, in fact, proved to be incorrect.

MR. NORTON: Object, Your Honor. There is absolutely no foundation for that. As a matter of fact, construction was ruled upon by this Board very recently.

MR. HAVIAN: The statement as a whole was not correct. There were obviously design errors and obviously qauality assurance deficiencies, so strike the construction aspect of it.

JUDGE MOORE: And now restate it.

BY MR. HAVIAN:

Q Dr. Kaplan, if you knew that prior to 1981, PG&E had erroneously maintained that it had a fully adequate QA program and that there were no major design errors at Diablo Canyone, how much would that lead you to discount the prior figure given to you by the DCP?

MR. NORTON: Your Honor, may we ask for a clarification of "major design errors"? We have, in this case, so far talked about A and B errors, and the witness is talking about safety-significant errors. So now we have a new term by Mr. Havian.

BY MR. HAVIAN:

Q Let's talk about the errors that have been found

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to date that you are aware of.

JUDGE MOORE: There is a whole range of errors. Why don't you classify them into the terms the witness is familiar with.

MR. HAVIAN: Okay.

BY MR. HAVIAN:

Q Dr. Kaplan, are you familiar with the design errors that have been discovered at Diablo Canyon over the past two years?

A (Witness Kaplan) Not in detail.

Q In a general way, are you familiar?

A Yes.

Q And were you also aware that PG&E's quality assurance program, prior to 1981, was found to have been deficient?

A Yes, I have heard that.

Q Were you also aware that prior to 1981, PG&E had consistently maintained that its quality assurance program was adequate, and that there were no significant design errors at Diablo Canyon?

A Was I aware of that? No, I was not aware of that.

Q If it were true that PG&E had maintained that, how much would that cause you to discount the figure of .999 that wou were given by the DCP?

MR. NORTON: Objection, Your Honor. Now that we

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have called it significant design errors, that must fall aside, as did construction, because there have been no significant design errors from a safety-significant standpoint.

That is the only evidence before this Board.

MR. HAVIAN: Mr. Chairman, I can pull out deposition transcripts, if we want to go that route.

JUDGE MOORE: The objection is overruled. I think the witness can answer that question, Mr. Norton.

BY MR. HAVIAN:

Q Dr. Kaplan, do you understand my question?

MR. NORTON: Excuse me, Your Honor. May I ask for a clarification?

Is the witness to assume that that was a false statement, that there are indeed significant design errors, because that is what the question is asking? He wants him to assume that there were.

JUDGE MOORE: Go ahead and put it in your hypothetical or your question, and state it as a hypothetical, and there will be no difficulty with it.

BY MR. HAVIAN:

Q Dr. Kaplan, if it were the case that PG&E had maintained, prior to 1981, that it had an adequate quality assurance program and that there were no significant design errors at Diablo Canyon, how much would that lead you to discount the prior distribution figure given by the DCP?

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MR. NORTON: Same objection, Your Honor.

JUDGE MOORE: I am going to sustain the objection.

What you are failing to do is to get in your hypothetical for the witness to assume that significant errors have been found, significant safety errors.

MR. HAVIAN: Okay. I can define the term "significant" as it was defined in depositions.

JUDGE MOORE: That will make the problem go away.

BY MR. HAVIAN:

Q Please assume for the purposes of this question that "significant errors" means Errors A or Errors B.

With that clarification, if it were the case that prior to 1981, PG&E maintained that it had a fully adequate QA program and that there were no significant design deficiencies at Diablo Canyon, how much would that lead you to discount the prior given to you by the DCP?

A (Witness Kaplan) With the definition of significance you have just given, if anybody maintained that about any engineering project, that there were no errors of that type in the project, you know, I would thereafter not have a lot of confidence in the sayings of that person.

If we are talking about that kind of error category, A and B kins of errors, of the type of the ones that I have scanned here, so that is my response on that part of it.

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The other part of it is on asking the DCP people, as far as I know, which are not the same as PG&E people, who may have said that, so there's one difference, and we are talking not about Category A and B errors, but about what I call Type 1 or 2 errors.

Q Have you been operating under the assumption that the DCP is different from PG&E?

A Yes.

Q Were you aware that the DCP was made up of a substantial number of PG&E employees?

A I know that, yes.

Q Were you also aware of the fact that PG&E has a major voice in the decision-making process of the DCP?

A I don't know about that, what kind of voice they have in what kinds of decisions, but if you are suggesting that the people who gave me information relating to these probabilities, that their numbers were colored by some sort of association with PG&E, I don't give that prospect a lot of credibility.

Q So you don't believe that people's opinions are likely to be biased by their personal views -- strike that.

You don't believe that when people offer expert conclusions or judgments, that their opinions are likely to be biased by their personal views?

A When you say "personal views," you mean whether

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mgc 20-1b they stand to gain or lose from the use that is made of their information, rather than their views --

Q Take that as an example.

A Their views? I am asking them for their views, in effect, so to ask whether their views are biased by their views, that doesn't ring. So what I think you are saying is, would they give me erroneous numbers, numbers that they really don't believe, because of some connection they have with PG&E or some loyalty to PG&E or something of that nature.

That possibility is there, and it's in my mind. I do not give it a great credibility, out of my experience with the people concerned and out of the process we went through to get these numbers.

JUDGE JOHNSON: May I ask you a question, Dr. Kaplan?

WITNESS KAPLAN: Yes, please.

JUDGE JOHNSON: Is it not true that the people whom you asked within the DCP were of two major groups, one primarily PG&E employees, the other primarily Bechtel employees?

WITNESS KAPLAN: Yes.

JUDGE JOHNSON: Was it possible or is it possible for you to categorize the answers you got with respect to this prior estimate, the PG&E people being substantially different than the estimates you got from those from Bechtel?

MR. KAPLAN: No, I don't think that's possible.

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JUDGE JOHNSON: They were not substantially different, or you never looked at them from the standpoint of trying to distinguish one category from the other?

WITNESS KAPLAN: I never looked at it that way.

WITNESS ANDERSON: I think from my understanding, they were pretty much the same. In other words, the Bechtel people and the PG&E people were in fairly close agreement on that prior number.

JUDGE JOHNSON: Okay. Fine, thank you. Back to you, Mr. Havian.

BY MR. HAVIAN: Will you please turn to page 37 of your testimony, under column 4, and if you look across Heating, Ventilation and Air-Conditioning -- strike that. Just column 4 in its totality.

Is one to infer from that column that nearly 50 percent of HVAC was looked at by the IDVP, was sampled in their initial sample?

A (Witness Kaplan) Yes.

Q Are you aware of the fact that there are nine systems in the HVAC area?

A (Witness Anderson) I think, Mr. Havian, it shows somewhat less than 50 percent, if you look at the scale.

Q Mr. Anderson, excuse me, but he's already answered the question, and I didn't ask for your opinion.

Dr. Kaplan, were you aware that heating, ventilating,

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and air-conditioning contain nine systems?

A (Witness Kaplan) Nine subsystems. I'm aware that there are more subsystems.

Q In fact, isn't it true that HVAC contains, in addition to the control room ventilation and pressurization system, the technical support center pressurization system, the auxiliary building HVAC system, the fuel handling building HVAC system, the diesel generating compartment HVAC, the 4160 volt switchgear room ventilation, the force draft shutter in the containment area, the auxiliary saltwater pump containment HVAC system, and the 480 volt switchgear ventilation system?

Does that sound reasonable to you?

A Yes.

Q How many of those systems did the IDVP verify?

A As far as I know, they did the control room ventilation system.

Q So they did one out of nine systems.

A They did one.

Q Do you believe that this chart is misleading in demonstrating that almost 50 percent of HVAC was sampled by the IDVP?

A I will have to refer that question to Mr. Anderson.

Q Mr. Anderson, do you feel that the chart is misleading in its representation of the amount of HVAC design

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that was looked at?

A (Witness Anderson) No, I don't. I think, first of all, there's quite a bit less than 50 percent that is shown here. We tried to estimate the amount of engineering that goes into these -- into that system from the total that went into all of HVAC, recognizing that some of the systems that you read off are very simple ventilation systems, and do not take a great deal of engineering.

Q What percentage of HVAC systems design do you believe was sampled by the IDVP in their initial sample?

A I would say somewhere in the range of 35 percent.

Q And what do you base that on?

A Just our understanding of the total HVAC systems and the work that goes into those systems.

Q If one were to use the measure used by Dr. Kaplan when he arrived at the number 3900 total, would one still come up with that percentage?

A You mean for the number of elements?

Q Yes.

A 3900 elements?

Q Yes. If one used that same measure in the HVAC area.

A Well, the elements are about equal size. You might come up with something a little bit less than 35, maybe something in the range of 25, something like that.

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ki 21:4

Q If it were 25 percent, do you think that the chart on page 37 would then be misleading?

A No. We're not trying to show the number of elements in this chart. We're trying to illustrate the amount of engineering work.

Q So in other words, the calculations or, rather, the representations on this chart are not really based on Dr. Kaplan's use of this notion of elements? Is that correct?

A No, not entirely. In fact, it's not. It's based on the amount of total engineering effort or engineering work which may be different than the number that is strictly a number count of the elements.

Q Dr. Kaplan, did you prepare this chart?

A (Witness Kaplan) No.

Q Mr. Anderson, did you prepare this chart?

A (Witness Anderson) I was involved in the preparation of the chart. It was prepared by some people under the Diablo Canyon Project's direction.

Q Dr. Kaplan, did you have any input into the design -- into the preparation of this chart, to the extent that you're aware?

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A (Witness Kaplan) Well, I had input into the conception, the idea of the chart, and into certain aspects of it. I did not have input into the detailed areas of the thickness of the shaded areas, or anything like that.

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Q I would like to direct your attention to the series of questions and answers that Mr. Strumwasser asked you with respect to the articles you had published.

A Yes.

Q Do you believe that compliance with the licensing criteria should be dependent on how much it costs to achieve that compliance?

A Let me answer this way. In designing a plant or anything else, an airplane or anything else, the major criterion is safety and performance. One strives to achieve those things, and is willing to incur considerable cost to achieve those, commensurate with the cost of not achieving it.

So if one is designing an airplane, and recognizes the cost of having an airplane accident, one is willing to spend a lot of effort to ensure that it doesn't happen.

On the other hand, you also realize the airplane has to fly. You can't make it big and heavy, with lots of steel. So there, as everywhere, one has to do an intelligent and judgmental tradeoff between matters of cost, benefit, and risk.

Now, the notion of criteria, the way you are using it, --

JUDGE JOHNSON: May I interrupt here? I think there may be a mismatch in that I think Mr. Havian was referring to NRC criteria or Licensee commitments, as opposed to general --

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the sorts of design criteria that the Applicant might set up for himself.

So are we using the term "criteria" in the sense of licensing criteria commitments?

MR. HAVIAN: Yes, Dr. Johnson.

WITNESS KAPLAN: Well, my understanding of licensing criteria is, as Mr. Anderson said earlier today, often they are more descriptive than the satisfying -- the satisfaction of some official rigid condition. So, to the extent that a criterion is substantive or meaningful, it has to be satisfied. To the extent that it is formal or it just happens to be the way it is, but could just as easily be other ways, without a reduction of safety or performance, I would not be willing to allocate a great deal of resources to match that kind of criteria.

BY MR. HAVIAN:

Q Is it your understanding of Commission regulations that they give the Utility the option of not complying with licensing criteria?

Is that your understanding?

A (Witness Kaplan) I have no understanding of regulatory dimensions of this much at all. I am speaking as an engineer who set out to design a thing.

Q Mr. Anderson, isn't it true that the licensing criteria reflect the Commission's determination of what degree

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of risk is acceptable?

A (Witness Anderson) If we're talking about basic criteria, I think that is true. The basic criteria determines the level of risk acceptable. But if we're talking about criteria and commitments in the broader sense,the letter of the commitments in the FSAR, I have never seen anything that relates the letter of perfection, you might say, in meeting the commitments to safety.

Q Isn't the NRC's approval of license commitments another way of saying that the level of risk embodied in those commitments is the level which is acceptable?

A I don't know. I can't speak for the NRC and what they think is acceptable. They review the plant, they understand the plant as it is presented, and they agree that the plant is acceptable.

To that extent, I guess you could say that the commitments have some relationship to their acceptance. But I don't know how -- you know, what their scale would be or what their thought process would be in some kind of an acceptable error rate on those commitments if they were very small or very minor errors.

Q Is it your position that the licensing commitments need not be satisfied if the cost is too great?

A I don't think I would relate that to cost. Certainly we make every effort to meet the licensing

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commitments and I think we have certainly tried to do that here. It's difficult to relate that to cost.

Q So is your answer to my question no?

A I think maybe a qualified no.

Q In what way would you qualify it?

A Well, I think cost -- you know, obviously, cost has some -- is a factor. The cost of achieving perfection in a design would be great. If we wanted to have perfection, that all criteria and commitments were met, and we wanted to have some guarantee that all criteria and commitments would be met, I think the cost could be very substantial.

I think that we go as far as is reasonable to be sure that we meet those commitments, but we're not 100 percent sure.

Q If there were 40 instances where those commitments had not yet been met, in your view, would you have gone far enough?

A Well, I don't think we have 40 instances where the commitments are not met. I know that number has been used.

Q Accept that as a hypothetical.

A Forty instances in the systems design area where the commitments have not been met?

Q That's right. Would you feel that you had gone far enough?

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MR. NORTON: Excuse me, Your Honor. I think it's an improper hypothetical. There's no information as to in what way the criteria hasn't been met. If we're of by 1/1000 of a percent in something;that can be one answer; if we're off by 200 percent, there could be another answer.

JUDGE MOORE: The hypothetical is ambiguous. Specify.

BY MR. HAVIAN:

Q If all you knew was that the license criteria were not met in 40 instances, and you didn't have any other information, would you feel that you had gone far enough? Would that be enough, in and of itself, to make you believe that you had to do more?

A (Witness Anderson) I really don't think I could answer or make that decision or answer that question without knowing more.

I would have to know how far the licensing criteria were not met, or what the significance was. I just couldn't make that kind of a judgment or give that kind of an answer without having more information.

Q I think that answers my question.

End 21

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Dr. Kaplan, will you turn to page 57 of your testimony. For the purposes of brevity, I don't want you to actually go through -- I'm sorry, not 57, 59 -- I don't want to ask you to go through the entire calculation, but just glancing at this, if the prior probability instead of being .001, were .999, wouldn't you come out with a posterior pretty close to one.

A (Witness Kaplan) .999 would say you were essentially absolutely certain beforehand that there was such an error. And if you are absolutely certain, the evidence of three systems out of ten would not shake that certainty very much and that would be reflected in a number still close to mine.

Q So, isn't it safe to say that not only is your prior assumption significant, but it is the factor which drives this analysis?

A No, I wouldn't say that. It depends on what you mean by "this analysis."

Q The analysis that you have set forth on page 59.

A Yes, yes. It depends on the reading interpretation you are giving to it.

Certainly, and obviously, the number used here for the prior affects the number for the posterior. Now what the Board -- what a decisionmaker has to reflect about is not just the prior as a thing on its own, but the prior for this plant relative to the prior for other plants

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and as for the impact of what is special to this plant, namely the verification program and to how much additional evidence that gives on this plant which you don't have on other plants, so you know if the decisionmakers are thinking here that Diablo Canyon is under a cloud of some sort different than other plants because of its history, because of the , you know, the mirror image error, then that cloud or that suspicion would be reflected in a larger prior here for the Phase II work than one would assign to some other plant.

So, it is not just the absolute value of that prior that enters into the decisionmaking, but the relative value for this plant as opposed to other plants presuming that other plants are okay, are licensable, but that this one has got a cloud on it because of that symmetry error.

So we have here a prior that is somewhat bigger than for, let's say an average plant.

Now, to address the cloud or that suspicion, one has instituted a design verification program. So what really is getting weighed in the balance here is the difference in the prior initially which reflects the additional suspicion hanging on this plant, versus the additional information found out about this plant in the design reficiation program.

So it is not just the prior that drives it, and it is not the absolute value of the prior. It is, say a

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relative value of the prior driving it along with the value of the evidence found from the verification program. That is what is driving a decision.

Q Assuming that the evidence found by the verification program is a given,in that case, wouldn't the results be wholly dependent upon your prior?

A You are saying --

Q In other words, another way of saying it would be, isn't it true that the results are highly sensitiv to your prior given the results of the IDVP?

A If you are saying, for example in equation 7.9 that this final numerical value depends strongly on the value inserted for the prior, given thatthe other number is fixed, certainly that is true.

Q In fact, doesn't this equation as you have structured it, add relatively little to our knowledge about the likelihood of discovery of design errors prior to the institution of the verification program?

A Not to me. I think it adds a good bit. What it adds is, it tells me what the DCP, who I consider knowledgeable people on this matter, what their appraisal is of the effectiveness of the IDVP program in finding a bad error if one is postulated to be present. That is worth something to me in my decision process.

Q But if you start out with the notion that there

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probably wouldn't be any significant errors, you end up with the conclusion that there probably are no significant errors. And if you start with the converse,that there probably will be a lot of significant errors, you end up with the conclusion there probably are significant errors.

Isn't that true?

A Well not quite. If you start with the notion that a significant error is very unlikely and you have the evidence that in a significant sample you didn't find such, that strengthens your confidence.

If you were to find such, of course, that would change the result dramatically.

Now on the other side of the picture, if you start out highly certain that there is such an error there, then given the DCP's value to the E3 evidence, your confidence after the evidence E3 would come down substantially.

Q Dr. Kaplan you said earlier --

A I mean your confidence that there is an error would be reduced by almost a factor of 10. And when you include the other aspect of the information, aspect 4, it would come down further.

Q Dr. Kaplan, you said earlier that a probabilistic risk assessment is the soundest basis for assessing risk.

Would a PRA also be the soundest means of assessing risk of a significant safety hazard at Diablo

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MR. NORTON: Excuse me, your Honor. What is a significant safety hazard? Are we talking about design or are we talking about something totally irrelevant?

JUDGE MOORE: I hope we are talking about the same class we were previously talking about.

MR. HAVIAN: I don't see any reason to assume we weren't.

MR. NORTON: Well, the word "design" is not in the question, your Honor.

JUDGE MOORE: Let's specify.

BY MR. HAVIAN:

Q Would a PRA be the soundest means to assess the risk of a significant safety hazard in the design of Diablo Canyon, resulting from an error in design at Diablo Canyon?

A (Witness Kaplan) I think that PRA as currently practiced is not especially useful in finding design errors. I think if you are focusing on design errors, the kind of verification program that was used here is more pertinent.

Q But the question I asked was for assessing the risk that there is a significant safety hazard as a result of the design errors which have been uncovered at Diablo Canyon.

MR. NORTON: Excuse me, your Honor, he didn't ask

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that at all, that is a different question.

MR. HAVIAN: I won't quibble, that is the question I would like to ask now.

JUDGE MOORE: Ask it once again so the witness has it clear.

BY MR. HAVIAN:

Q Dr. Kaplan, isn't it true that a PRA would be the soundest means of assessing the risk that a significant safety hazard has been caused as a result of the design errors uncovered at Diablo Canyon?

A (Witness Kaplan) Now let me see if I got the question.

You are asking me is a PRA the soundest method of assessing the health and safety, public health and safety risk coming from whatever design errors have been found?

Q Yes.

A Is that a fair translation --

JUDGE JOHNSON: Are you saying the errors that have been found, or the errors that have not been found?

MR. HAVIAN: The errors that have been found.

MR. JOHNSON: Okay.

WITNESS KAPLAN: All right. So, given a specific error you are asking, is PRA the way to assess the safety significance of that error, the soundest way?

Given that my understanding of PRA as the logical

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approach to assessing safety of a plant,and there is only one such, then it certainly is the soundest. I have to agree with that. My understanding of PRA is that it is the logical process of assessing the risk of a plant. And as part of that process you could assess the risk coming from the specific error. So, I agree with that.

But the converse of that, the statement that in order to make an assessment of a given error, that you need to do a full-blown PRA as currently understood and exemplified by the design in Indian Point, I don't agree with that at all.

BY MR. HAVIAN:

Q As a result of the IDVP, do you know anything about the design errors that may have gone undetected at Diablo Canyon to date?

Do you have any information about what those would be likely to be like?

A (Witness Kaplan) Well, the information I have is the kinds of errors that have been found in the portion that was sampled, the so-called category A and B errors, and that is information which goes to say it is quite likely that similar kinds of things would exist in the areas unlooked at.

Q Wouldnl't a probabilistic risk assessment also be the soundest means of assessing the risk that a significant safety hazard might result from the undisclosed design errors

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at Diablo Canyon, assuming there may be undisclosed design errors?

A Well, to make sense of that question, I have to ask myself what you mean by a PRA as the soundest method. Do you mean--

Q I was taking that language from your earlier statement. I believe that was the language you used.

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A Well, my understanding of PRA is the logical process of quantifying risk, all right? So yes, that is the soundest process.

Now as currently practiced, the examples of PRAs that I think we all have in mind are huge voluminous studies. The statement that you need to do all of that in order to get a sufficient assessment of risk of certain specific errors, I don't agree with.

Q So in other words, your answer is yes?

MR. NORTON: Excuse me, Your Honor. I'm going to object to this line of questioning. I don't understand how you can do a PRA of errors that have been found and fixed. I mean, the assumption underlying all of Mr. Havian's questions is that the errors are found and ignored, when the facts in this case and the evidence in this case are to the opposite.

JUDGE BUCK: He's talking about unfound errors.

MR. NORTON: Some of the time he is.

MR. HAVIAN: That was my last question.

JUDGE MOORE: Mr. Norton, your objection is a little late, since that was three questions ago.

MR. NORTON: He just repeated it again. He just started the next question.

MR. HAVIAN: No, I didn't.

MR. NORTON: Fine. No question.

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JUDGE MOORE: Gentlemen, would you please cease the exchanges between yourselves and go on to your next question, Mr. Havian?

BY MR. HAVIAN:

Q Mr. Anderson, did you say earlier that there was a -- that you would approximate, if you had to do a prior on the chances of a seismic error at Diablo Canyon, before the blueprint switch was discovered, that you would estimate that prior to be approximately one out of ten?

A (Witness Anderson) I don't know if I gave that exact number, in what context it was in. I think the prior for a seismic error would be higher, much higher, than the prior for an error in the non-seismic area. I don't remember quantifying it quite in that way.

Q Could you give me an estimate as to what your prior would be?

A Well, what kind of an error are we talking about? I'm not sure I understand what kind of an error.

Q An error of the nature of the switched blueprint error.

MR. NORTON: Excuse me, Your Honor. That was asked and answered by Mr. Strumwasser this morning.

MR. HAVIAN: Well, if Mr. Strumwasser asked and answered it, I still need the witness' answer.

JUDGE MOORE: Mr. Norton, I'm going to permit it,

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because that line of questioning -- partly because of the lateness of the hour -- is a bit fuzzy in my mind. It may well have been asked and answered, but I will permit it.

MR. HAVIAN: Thank you, Mr. Chairman.

BY MR. HAVIAN:

Q Mr. Anderson, do you recall --

A (Witness Anderson) What was the question again?

Q Can you put a number on your prior for the switched blueprint error, assuming you would consider the time before that error occurred?

A I think that number --

MR. NORTON: Excuse me, Your Honor. He switched the question. The first time, it was that type of an error. This last question, if you listen carefully, it's that specific error.

JUDGE MOORE: Which do you mean, Mr. Havian?

MR. HAVIAN: Let's go with that specific error.

JUDGE MOORE: Now ask the question again.

BY MR. HAVIAN:

Q What would your prior be for the switched blueprint error that was discovered at Diablo Canyon?

A (Witness Anderson) In the range of one chance in ten, something like that.

Q Have you discovered errors in the seismic design of piping at Diablo Canyon?

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A Yes, I believe there have been some.

Q What would have been your prior with respect to the errors in seismic design of piping that have been discovered at Diablo Canyon, assuming that you believed there to be an adequate QA program in place?

A With a very low threshhold of error that we looked at in this program and the tremendous number of piping problems and calculations, it is vertually 1 that some kind of an error would exist of that minimal significance.

Q But what would be your prior for the piping design errors that have actually occurred? In other words, for all of those?

A I don't quite understand the question. For all of the errors? We would be giving a prior for that number of errors?

Q That's correct, assuming that you did not know that the QA program was inadequate.

A I would have no idea what that number would be.

Q Would it be a greater probability or a lesser probability than your prior for the switched blueprint error?

A With the low significance of errors and the number of possibilities for error in the piping design program, I think it would be higher.

Q But I think you switched back a little bit. I was saying for -- your prior is now your prediction about finding

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the same type and the same number, essentially the same errors that have actually been found.

Does that change your answer?

MR. NORTON: Excuse me. May we have the question again?

JUDGE MOORE: Certainly.

Would you mind repeating the question?

BY MR. HAVIAN:

Q The question is, what would be your prior with respect to the seismic design errors in piping discovered at Diablo Canyon, assuming -- if you were going to assume that there was an adequate QA program?

Then I think the next question was, you said you didn't know --

JUDGE MOORE: Let's ask them one at a time.

MR. HAVIAN: He already said he didn't know when I asked him that one. So I am asking him, would it be greater or lesser than the one --

JUDGE BUCK: May I ask a question? You are assuming that the spectra that was given was correct and no error was made in the spectra for the plant? Is that what you are asking now?

MR. HAVIAN: I am assuming that this assessment is being made prior to the switched blueprint problem, yes, prior to any knowledge that there is an inadequate QA program.

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If you were going to make a prior probability estimate about there being the piping design errors that were found at Diablo Canyon, what would that estimate be?

WITNESS ANDERSON: I just don't think I can answer that. There are just too many factors that I would have to know something about before I could give any kind of reasonable answer there.

BY MR. HAVIAN:

Q Would it be a higher probability than one in ten?

A (Witness Anderson) I don't know.

JUDGE MOORE: Mr. Havian, how much additional cross-examination do you have of the panel?

MR. HAVIAN: Probably about half an hour at the most.

JUDGE MOORE: All right. We will take a very brief recess.

(The Board confers.)

MR. HAVIAN: Mr. Chairman, I think I can finish up fairly quickly.

MR NORTON: Your Honor, if he's going to be --

JUDGE MOORE: One moment, Mr. Norton.

Mr. Chandler or Mr. McGurren, rather, how much cross do you have for the panel?

MR. MC GURREN: I think we have one question at this point, Your Honor.

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JUDGE MOORE: How much redirect will you have, Mr. Norton?

MR. NORTONL A couple of questions at this point.

JUDGE MOORE: I think we will try to finish with this panel this evening, so why don't we take literally five minutes, and we will reconvene at eight minuts after five to finishe up.

(Recess.)

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JUDGE MOORE: Come to order, please.

MR. NORTON: Your Honor, I hate to say this, but we had a discussion during the break, and there may be a great deal of confusion about the last 15 minutes of questions by Mr. Havian. He kept using the "risk" in his questions. When I talked to the witnesses, he interpreted that to mean risk to the public health and safety, and someone else thought Mr. Havaian meant risk in terms of probabilistics or probability in the questions. And, if so, we have a horrible mismatch between questions and answers for about the last 20 minutes.

I think it ought to be explored, perhaps between the Board and counsel, between the Board and the witness on the record.

MR. HAVIAN: I think I can solve it. Why don't we just assume that Dr. Kaplan's interpretation is the one I intended, and then we will solve all the problems here without the need to go over all this again. Frankly, that is not something --

MR. NORTON: Wait a minute. We can't assume that Dr. Kaplan interpreted it the way he meant it, if he didn't.

JUDGE MOORE: No, no. Mr. Havian is adopting, I believe, Dr. Kaplan's interpretation of risk, which I believe makes the problem go away, because it merely incorporates Dr. Kaplan's meaning into Mr. Havian's questions.

MR. NORTON: Perhaps Dr. Kaplan could state for the

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record how he interpreted the word "risk" in those questions.

JUDGE MOORE: I don't think it's necessary at this point, Mr. Norton, with the understanding that Mr. Havian has just given us. I think the record will be clear, then, since -- didn't the witness -- I might stand corrected. Dr. Kaplan, it will save time, instead of me trying to figure out what you previously said, to state your understanding of "risk" as used in the prior questions.

WITNESS KAPLAN: My understanding of risk is as it is set forth in this paper; that is, essentially a listing of a set of scenarios, possible occurrences, each with a probability, and each with an assessment of consequences, so I call it a set of triplets. With that understanding, I have to admit I couldn't make a great deal of sense out of Mr. Havian's questions.

JUDGE BUCK: Your "consequences" meaning to the public?

WITNESS KAPLAN: Yes.

JUDGE MOORE: As stated in your testimony, health and safety was the only consequence that you were concerned with.

WITNESS KAPLAN: That's the main consequence. There is also some concern with damage to the plant.

JUDGE MOORE: Mr. Havian, do you wish to back up and ask any clarifying questions before you proceed with your

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line of questioning in light of all that?

MR. HAVIAN: Mr. Chairman, frankly, I'm not sure now which questions the witness is referring to.

BY MR. HAVIAN:

Q Which ones are we talking about now --

JUDGE MOORE: Mr. Norton, is Panel 6 going to be available tomorrow?

MR. NORTON: They certainly are, but I would hope they didn't have to be.

JUDGE MOORE: Well, the thought I had in mind was that the transcript will be here first thing in the morning. The quick and easy way to resolve it is to, at that point, take a look and see if these questions still remain. Otherwise, we can delve back into it now, and it will take, probably, some considerable time.

Mr. Havian, do you have any thought on the matter?

MR. HAVIAN: If I knew which questions, which series of questions the witness was referring to, then we could probably clear this up.

BY MR. HAVIAN:

Q Dr. Kaplan, do you recall just generally the subject matter of the area that you are confused --

A (Witness Kaplan) It's coming back to me now. I remember a very long question about the risk of there being a hazard, something like that, which I couldn't make a lot of

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sense of, but as I recall, we worked that question around to asking what risk comes from a specified error, whether a PRA would be a way to go about doing that. That question I understood and answered that.

MR. HAVIAN: I think I know the question he is referring to.

JUDGE MOORE: Go back and ask him.

BY MR. HAVIAN:

Q Actually, if it's the question I'm thinking of, my question was, the risk of there being a substantial safety hazard isn't a PRA a most -- the soundest basis for -- okay, here's the question. Isn't a PRA the soundest means of assessing the risk of a significant safety hazard as a result of the undiscovered design errors at Diablo Canyon?

That was my question. Is that the one that you understood, Dr. Kaplan?

A (Witness Kaplan) I think that's the one we're talking about.

Q And since I incorporated in the word "risk" of a significant safety hazard, I assume there should be no problem with the transcript.

JUDGE MOORE: Okay, fine. Why don't you then continue, Mr. Havian?

BY MR. HAVIAN:

Q Mr. Anderson, when you were discussing how you

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came up with your .999 prior, you said that you had considered as part of that that there was an inadequate quality assurance program. Is that correct?

A (Witness Anderson) We had considered that errors had been found in the seismic design area. I don't think that we had specific knowledge at that time that there was an inadequate QA program. We knew that some errors had been found, and to the extent that that related to the quality assurance program, we had knowledge in that regard.

Q If the only information that was changed was that you knew that the quality assurance program -- you knew of the deficiencies which have now been uncovered in the quality assurance program -- if that was the only thing that were changed, would your prior change?

A I don't think so, because in the systems area, we have a great deal of testing and a great deal of scrutiny that is really beyond the normal design quality assurance program.

I don't think that that would change the number significantly.

Q Do you believe the QA deficiencies that were discovered were significant?

MR. NORTON: Excuse me. Which area, Your Honor? Are we talking about seismic or nonseismic systems area?

JUDGE MOORE: The question is ambiguous. Specify.

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BY MR. HAVIAN:

Q Were the QA deficiencies discovered different in the seismic and nonseismic areas? And I'm talking just about the QA deficiencies.

A (Witness Anderson) Speaking of QA in the broad sense of quality assurance and quality control, I would say yes, they were different.

Q Was quality assurance deficient in the area of nonseismic design, in your opinion?

A Maybe a formalized program was somewhat deficient, but certainly the work that was done and the way it was done was, in my opinion, not deficient in the nonseismic area.

Q But you do believe that the quality assurance program in that area was deficient. Correct?

A No. There was some lack of formalization in the program, but apparently, from everything we've seen, they did have good design control. And to that extent, I don't think we could say the quality assurance program, again in the broadest sense of the word quality assurance program, was deficient in the nonseismic area.

Q Weren't there programmatic deficiencies discovered in the quality assurance program?

S2 BU A I really am not prepared to talk about quality assurance per se. That's not my area of expertise.

Q Dr. Kaplan, in your deposition, didn't you state

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that Mr. Hubbard, the thought went through your mind that Mr. Hubbard might be, quote, "primarily throwing smoke and trying to create confusion," closed quote, with respect to his conclusions about statistics?

A (Witness Kaplan) Yes, I said something like that.

Q And didn't you also state that one reason for this thought going through your mind was that you were aware that Mr. Hubbard was, quote, "generally an antinuclear activist," closed quote?

A Yes.

Q Where did you get that information?

A I answered that also in the deposition. The answer was gossip.

(Laughter.)

Q Do you have a copy of Mr. Hubbard's affidavit in front of you?

A No.

MR. NORTON: Which one?

MR. HAVIAN: His supplemental affidavit.

MR. NORTON: Which one?

JUDGE MOORE: Why don't you give it to the witness, Mr. Havian, and then, for the record, tell us the dates and what it accompanied.

(Document handed to Mr. Norton.)

MR. HAVIAN: It's dated March 29, 1983. And as to

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what it accompanied, I don't recall.

MR. NORTON: I think the affidavit is dated March 26th, if it's the same one you just handed me. Attached to a pleading that was mailed on the 29th.

MR. HAVIAN: Okay. Yes.

MR. NORTON: It accompanies the -- it doesn't say what it accompanies.

MR. STRUMWASSER: I believe it accompanied nothing. It was a self-contained document.

JUDGE MOORE: May I see it, please, before you proceed, Mr. Havian?

(Document handed to the Board.)

MR. STRUMWASSER: Mr. Chairman, this was sent to the Board in anticipation of the oral argument in April on the motions to open.

JUDGE MOORE: Unaccompanied by leave for filing, if my memory serves me correctly.

Continue, Mr. Havian.

BY MR. HAVIAN:

Q Dr. Kaplan, I direct your attention to paragraph 44 of that affidavit.

A (Witness Kaplan) Yes.

Q Isn't that the paragraph that deals with statistics?

A Yes.

Q Can you point to any statements in that paragraph

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which would tend to indicate that Mr. Hubbard was primarily throwing smoke and trying to create confusion?

MR. NORTON: Excuse me, Your Honor. What's the relevancy to this line of questioning?

MR. HAVIAN: I think it will become clear in a moment.

JUDGE MOORE: I'll give you just a bit. Tie it up quickly.

WITNESS KAPLAN: I would point more to the aggregate of what is said than to any one specific statement. And the aggregate -- really, you want to know what is my thinking behind that -- behind that possibility that crossed my mind?

BY MR. HAVIAN:

Q Yes, aside from the fact that he was generally an antinuclear activist.

A (Witness Kaplan) Yes. That's one of the elements contributing to that thought. The other element would be that the whole proposal here, all of his items, A through F, are coming out of the framework, the ball and urn framework, and it's just so inappropriate to a design verification program that I have to think internally that either the man doesn't see the inappropriateness of this, or he sees it, but is using it nevertheless to create confusion and delay.

So that is what crossed my mind.

Q By whom were you employed when you did the PRA

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for Zion?

A You mean for whom was I consulting at the time?

Q Yes.

A I was consulting for another consulting firm called Pickard, Lowe & Garrick, who were in turn working under contract to Commonwealth Edison to the Power Authority, State of New York, and to Consolidated Edison of New York.

Q For whom did you work when you did the PRA for Indian Point?

A Well, it's the same three. Same arrangement.

Q And Midland?

A I didn't work on the Midland very much myself.

Q By whom was your firm employed?

MR. NORTON: Excuse me. Whose firm, Your Honor?

JUDGE MOORE: I presume you mean Dr. Kaplan's firm, but you better specify in the time frame, because as you note from his resume, those things changed.

Q I direct your attention to page 2 of your testimony, lines 11 and 12.

A (Witness Kaplan) Yes.

Q The firm I'm referring to is PLG.

A Yes.

Q By whom are they employed at Midland?

A By the owners of that plant, which I believe is Consumers Power.

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Q How about Seabrook?

A Seabrook? What is their precise name? It's the Utility of New Hampshire. I don't know their precise name.

Q Have you personally ever offered expert testimony for any party adverse to an Applicant for a nuclear license?

A No.

Q Has PLG ever offered expert testimony for any party adverse to an Applicant for a nuclear license?

A Not to my knowledge.

Q Have you or PLG ever been retained by a party adverse to an Applicant for a nuclear license for the purposes of expert advice?

A Not to my knowledge.

Q So is it fair to say, to paraphrase your own deposition, that you are, quote, "generally a pro-nuclear activist"?

A I don't know that I'm an activist as such, but I'm definitely pro-nuclear.

Q And wasn't the first document that you were shown in this case Mr. Hubbard's supplemental affidavit?

A I don't know whether it was the first. It was among the early ones.

Q Weren't you brought into this case for the express purpose of offering testimony disputing what Mr. Hubbard said in that affidavit?

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A I would say that that's fair. No one put that to me as such. It was more a matter of people handed me pieces of paper, that one among them, and in reading them, I got the sense Of what the situation was, and then I came forward with what I might be able to contribute. So I don't remember anybody saying, "Can you dispute this testimony."

No. It was more a matter of my seeing it there and saying, "Oh, this is inappropriate, and I think I can shed some light on it."

Q But it was your impression that that was what you were expected to do, correct?

A Yes.

MR. HAVIAN: Thank you. I have no further questions.

JUDGE MOORE: Mr. McGurren?

CROSS-EXAMINATION (CONTINUED)

BY MR. MC GURREN:

Q My name is Henry J. McGurren. I am with the NRC Staff.

Would you please turn to Page 65 of your testimony?

I believe you were asked this morning by Mr. Strumwasser about your statement on Line 7, about sufficient confidence.

A (Witness Kaplan) Yes.

Q You go on to say "to support, in my judgment, the conclusion that the sequential sampling process need not

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continue beyond this point."

Do you recall those questions by Mr. Strumwasser?

A Very well,

Q And I believe you testified that thre was a cost aspect involved here; is that correct?

A Correct.

Q Could you indicate what that cost factor is?

A Well, in my mind, the costs I had in mind were the costs of additional engineering work and the costs of additional delay.

MR. MC GURREN: That's all we have, Your Honor.

JUDGE MOORE: Mr. Norton, redirect?

MR. NORTON: I have a couple of questions.

REDIRECT EXAMINATION

BY MR. NORTON:

Q Dr. Kaplan, the same subject matter of costs.

You were asked, I believe, by Mr. Havian regarding meeting criteria or commitments and costs.

What are the factors that you would consider in arriving at a decision as to whether meeting a specific criteria should be recommended or recommended against in terms of the cost of meeting that criteria?

A (Witness Kaplan) Okay. Part of the answer to that, let me just explain the notion that I have of criteria.

Everybody talks about criteria, and it's a little

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fuzzy in my mind what exactly this means, and I have a feeling it means different things at different times.

So let me just tell you the picture I have in mind of the process. According to that picture, the designers set forth a description of the plant. There are certain fundamental criteria. Then the designers set forth this description of the plant, and the Staff reviews the details of that, and anything that is in there, then, in effect become a criteria, once the Staff says yes. If you design the plant that you have described here, I think it will be safe.

So then there are those description which then become criteria and commitments, because if we meet those, we now have the Staff which has said, "If you meet these, the plant will be safe."

Now when one gets into the details of it, one may find that the specific technicalities of some descriptions that were made earlier, one doesn't want to meet in terms of meeting the letter of it, but in terms of the spirit of it, one can achieve equal safety or more in a little different way.

So does that not meet the criteria? Is that a violation of the criteria? And if you hang those kinds of words on it, you have created a misimpression, I would say. So what ought to be done in that kind of case, and what I assume is done, is that the designers and the Staff discuss

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the point in question, and in effect, come forth with new criteria that the Staff accepts the change, and in the sense that we are using the term "criteria," that becomes new criteria, I would think.

Q Let me try and give you a specific example.

If, for example, you had a criteria regarding frequencies of supports, and the criteria were, to pick an arbitrary number, "The piping supports shall all meet a frequency of a certain given value," and let's say that number is 25, just to pick a number, and suddenly one is discovered -- one of thousands is discovered that is not 25, but is 24.9, and therefore technically does not meet its criteria, but an analysis can be done that shows it makes no difference whatsoever that that one-tenth of one percent doesn't affect the peformance or safety of anything, and yet it would cost a million dollars to get it up to 25, .because you would have to take out the old one and put in a new one, how would you evaluate that kind of, quote, meetijng the criterion? What would you recommend be done?

MR. HAVIAN: Objection. There is no foundation that any such hypothetical exists. Total lack of foundation for the question.

MR. STRUMWASSER: My concern is different, Mr. Chairman. I don't know who he is that he is now evaluating. Is he the NRC, or is he advising someone?

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MR. NORTON: He's Dr. Kaplan.

MR. STRUMWASSER: As a philosopher prince or what?

JUDGE MOORE: Mr. Strumwasser, just the skeleton, not the meat at this hour. The foundation objection is overruled. I don't have any trouble -- I think the foundation is clear. The "he," I thought the question was posed to Dr. Kaplan, .and "he" is the designer, I believe, in the question; is that correct?

MR. NORTON: "He" is a person who goes through a decision-making question as to what you do, what factors you weigh.

JUDGE MOORE: Fine.

MR. NORTON: I wanted him to go through that very quick process to get an idea --

JUDGE MOORE: I'll permit the question.

Dr. Kaplan, would you be kind enough to answer it?

WITNESS KAPLAN: Yes. Given the situation as Mr. Norton has described it, 24.9 versus an official criterion of 25, and that there is no safety significance implication to that defense but a million dollars in making that correction, I would think that I had better ways to spend that million dollars which would produce more safety than bringing that 24.9 up to 25.0.

BY MR. NORTON:

Q And, in fact, under the definition of a Class A

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error, as I described it -- or excuse me -- the hypothetical that I gave you, that indeed would be a Class A error, would it not?

A (Witness Kaplan) I think so.

Q Mr. Anderson, I would like to refer you to figure 8-1. I am not sure of either Mr. Strumwasser's precise question or your precise answer from this morning, but the naswer dealt with how much of the non-seismic systems were reviewed, and I don't believe I heard Mr. Strumwasser say by the IDVP or the verification program with small v or small p, and I'm not clear in my mind what he asked you in that regard, but you answered 30 percent.

So I'm unclear from that exchange as to what percentage was reviewed by whom. So let me ask you first, what percentage is your understanding of the non-seismic systems that were reviewed by the IDVP?

A (Witness Anderscn) The percentage of engineering work, I would say about 30 percent.

Q All right. And what percentage of the engineering work, in addition to that 30 percent, over and above that 30 percent, was reviewed by the ITP, Diablo Canyon Project ITP?

A Somewhere between 45 and 50 percent.

Q All right. So if my mathematics is correct, are you testifying that the verification program, small v, small

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p, being the IDVP and the ITP, between them reviewed someplace between 75 and 80 percent of the non-seismic systems engineering woork?

A Yes, I believe in that range.

MR. NORTON: No further questions.

Wait a mintue. I do have one more.

BY MR. NORTON:

Q Wee there any significant errors, significant to safety as we have talked about here all day, discovered by the ITP in its review of the additional 45, 50 percent?

A (Witness Anderson) No, there were not.

MR. NORTON: Nothing further.

EXAMINATION BY THE BOARD

BY JUDGE JOHNSON:

Q More or less on the same line, Mr. Kaplan, earlier the fact that such additional sampling was performed by the DCP and some sampling by the NRC Staff was mentioned, and you were asked why this information was not included, and your answer was more or less related to the independence of the IDVP, I'm a little surprised that from the standpoint of a Bayesian, though, that you would not have taken this additional information and in some way used it.

In other words, according to Mr Anderson's most recent statement, an additional 45 percent of all the work was sampled, as an experimentalist myself -- and that looks

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like the results of two experiments -- one, the IDVP verification program and, two, the DCP program -- and if I was doing two experiments or had the results of two experiments, I would certainly want to know how the results of those two experiments compared with one another, and I guess my question -- one more part of the question -- I don't really see how you are utilizing this additional information in any way jeopardizing the independence of the IDVP activity, so I would like a little further discussion as to why this additional information was not included.

JUDGE MOORE: No, Mr. Norton, you may not object on the grounds that it is compound.

MR. NORTON: I just want to know if I can ask a question like that sometime.

(Laughter.)

JUDGE JOHNSON: When you get a little older.

(Laughter.)

WITNESS KAPLAN: Well, certainly with respect to calculations of Chapter VII, --

JUDGE JOHNSON: Yes?

WITNESS KAPLAN: -- certainly, the DVP work does not compromise the independence of the IDVP or negate that. It would add to it.

So your question is, then, why didn't I take that other step, having assessed the evidence from the IDVP,

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the effect of that evidence, why not go further now and assess the evidence of the DCP, or the way you said it was, why wouldn't I as a Bayesian use it?

The answer is, I, as a Bayesian, do use it. I didn't explicitly quantify it in here. My thought was to set out the framework of the argument and allow the judges to weigh this additional evidence within that framework.

BY JUDGE JOHNSON:

Q That is precisely my point, because you say something about this evidence existing, but you don't say it in any way that a judge can use it in the sense that -- you say, "In this review, seven additional open item were found." That doesn't even -- an open item is not even something we've talked about before this statement in this particular testimony, and seven doesn't mean anything until I know how many items were sampled.

So it's in essence a throwaway statement here.

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A (Witness Kaplan) I see. Then --

Q Having preached a sermon --

A Let me try to salvage what I can to rectify that. The open items, I felt this was a well-understood term here. My understanding of these so-called open items is that they were equivalent to Category A and B type errors.

Q Is that what you meant? We can read those as being equivalent?

A That's my understanding of it. DCP officially labeled them open items. They didn't officiall label them Category A and B, but I have been told they essentially have the same meaning.

With respect to the number of elements, that can be provided in numerical form, I attempted to show it in visual form in Figure 8-1. But more to the point, here I no longer am pushing it, this lamda, and you know, the posture throughout this whole thing is that lamda is not the issue. The issue is what is the likelihood that a bad error remains.

Q Well, that likelihood, is it not, depends on the -- through your various cases, and particularly, I think, E-3 is a case in which the subsequent -- or the information that you gather relates to the number of items which were sampled. And no class or Class 2 or 3 errors were found, and here specifically there have been additional items sampled, presumably again with no Class 2 or 3 items being found. So that the

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situation is somewhat different than you have pictured in your aspect of three, I suppose.

A That's true. So if you wanted, now, to include the evidence of the DCP, you would define an E-3', which would have the meaning that no error of that type was found in the total sample IDVP plus DCP, and you would have to put forth a number to that.

And likewise, you would put forth a number for this other ratio in equation 7-12, where we replace E-3 by E-3', now referring to the bigger sample.

My thought was twofold on that. I felt the point was already abundantly made, just considering the IDVP results, and that the DCP was just so much more icing on the cake, and to go through it again would be just to rub it in deeper. That was one thought.

The other thougt is that, having set forth the form of the thinking which is, you know, what I think is my contribution to this whole issue, that the Board would have that framework in which to insert its own judgments, and that would be more meaningful than my putting my judgment certainly. And if the Board wanted to ask for the DCP's judgment on that, they could.

Q It is true that the prior .001 is merely the considered judgment of a number of educated people and experienced people, is it not?

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A (Witness Anderson) Yes. It's the considered judgment of people that were skilled in the systems design, specific disciplines, the best kind of judgment we could come up with.

JUDGE JOHNSON: I have no further questions.

JUDGE MOORE: Any recross, Mr. Strumwasser?

MR. STRUMWASSER: Yes, Mr. Chairman.

JUDGE MOORE: Keep it limited to the redirect.

RECROSS EXAMINATION

XXXX

BY MR. STRUMWASSER:

Q Mr. Anderson, in your answer to Mr. Norton, he added 50 percent to 30 percent, and got 80 percent; is that right? I'm trying to recall the exact numbers of the verified work. 40 and 30, is that right?

A (Witness Anderson) Yes, it was 30 percent, and then 45 to 50 percent would give us 75 to 80 percent, in that range.

Q Is it your testimony that the verification work the DCP did that comprises that 35 to 40 percent is disjoint from the IDP's --

MR. NORTON: Excuse me, Your Honor. We got a new number, 35 to 40 percent.

BY MR. STRUMWASSER:

Q Excuse me. 45 to 50 percent is disjoint from what you call 35 percent that the IDVP did?

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A (Witness Anderson) Yes. It's independent of the IDVP work. We were also reviewing the systems and putting the files in order and looking specifically for errors.

Q Now, isn't it true that that is not a systematic review?

A Well, it's not nearly as systematic as the IDVP's review, programmatic, well-defined. However, it is systematic in that the review was done to the procedures of the -- the engineering procedures that require certain documentation and certain steps to be taken.

Q It is, in fact, not documented as a review process. is it?

A It's documented in the files as the design process.

Q In your deposition on September 26th, did you not say it was not documented as a review process?

A Well, it's not documented as a review process. That's correct.

Q And you have acknowledged that it was not of the same depth as the IDVP review, right?

A It is not of the same depth, that is corrrect.

Q Now, really, did you testify in your deposition -- I believe you called the DCP review there 50 percent, right?

A Well, at that time, you were asking me what an estimate of the percentage review was, and I said it was less than 100 and more than 50. I believe that's what I said.

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ki 26:5

Q Did you not understand yourself to be saying that that was an estimate of both the DCP and the IDVP work together?

A I understood that was an estimate of both, somewhere between 50 and 100 percent, yes. At the time of the deposition, I think I just hadn't thought about the percentage of engineering work that the Diablo Canyon project had reviewed enough to give you a precise answer.

Q I'm going to ask you to take a look at your deposition on this point.

MR. STRUMWASSER: I'm sorry to do this, Mr. Chairman --

JUDGE BUCK: While you're getting that, may I ask a question?

MR. STRUMWASSER: Surely.

JUDGE BUCK: Mr. Anderson, can you give me a clarification of what you mean by "not inthe same depth." What was the difference in the procedure?

WITNESS ANDERSON: Well, in our process of review, we were looking at the systems for functionality; we were looking at the systems for, as I said, for errors. We were putting the files in order, and the documentation, the paperwork in order. So we were not going into the same depth that the IDVP was going into, where they were looking at a system in its totality, all of the requirements, all of the commitments,

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all of the engineering that went into that system.

JUDGE BUCK: In other words, you were doing sort of an item review, rather than a system review. Is that correct? Is that what you're saying?

WITNESS ANDERSON: I think we would still call it a system review, but it probably was not to the same depth that the IDVP looked at it. They looked at these systems in great depth. The actual engineering work that they did took well over six months, and Stone & Webster that did the review was a very competent company to spend that much time on three systems. They must have done a very in-depth review.

We did not spend that much time on three systems.

JUDGE BUCK: I'm trying to find out where the difference is. Did you do as much calculating, or was this primarily a records review, or what?

WITNESS ANDERSON: No, it was a review of the system. We did calculations, we did reviews of the calculations. We were looking for errors. The reason, obviously, was that we wanted to find any errors that were there, and get them fixed before they would contribute to a delay in the schedule.

JUDGE BUCK: I'm still disturbed, what you mean by the difference in depth as to what you missed that an IDVP review would have gotten.

WITNESS ANDERSON: It's just my opinion that we

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didn't go as deep as they did. We didn't look at it,at the systems as thoroughly as they did. I think we did a quite thorough review of the systems, but we didn't look at it in the depth that they did.

I guess there is some distinction there.

JUDGE BUCK: Maybe I'm not thinking right tonight, but I don't quite understand the word "depth." What are you missing that the IDVP did?

WITNESS ANDERSON: Well, I don't think we looked at all of the details to the extent that they did. We think we looked at the important details for system functionality.

JUDGE BUCK: Okay. Now I begin to understand what you're talking about. Okay.

(Document handed to witness and Bench.)

JUDGE MOORE: Go ahead, Mr. Strumwasser. Can you give me a page number?

MR. STRUMWASSER: Page 51, Mr. Chairman.

BY MR. STRUMWASSER:

Q Mr. Anderson, turn to page 51. Do you see a question there on line 5, "Would you consider that this review would constitute 100 percent review of the nonseismic design of Unit 1," and your answer was no. Is that correct?

A (Witness Anderson) That's correct.

Q Fifty percent review and your answer is,"I think it could be in the range of 50 percent."

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A I think that's true at that time, and I think I remember saying later that I thought it was above 50 percent. You were asking me for an estimate that earlier I had said I was really not prepared to give. I think in further review of what the Diablo Canyon project did, I am prepared to raise that number from 50 percent to something higher, in the range of 75 to 80,

Q On the same page, line 26, the question is, does your 50 percent figure include both the DCP work and the IDVP work? Your answer, "The 50 percent figure was a very rough estimate, and I guess it could."

A It could include both. That was myunderstanding at the time.

Q So it's your testimony, at the time that the two sets, the set of design work that was verified by the IDVP and the set of design work that was verified by the DCP is not necessarily -- are not necessarily too disjoint sets?

A Well, there is some overlap. I think we did review of the systems work for the three systems that were selected before the IDVP reviewed them.

Q So if there is some overlap, you can't just add the two percentages together, can you?

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A I guess in a strict sense, we couldn't add them directly, but I think the total still would be in the range of 75 to 80 percent.

Q Now is it your testimony, Mr. Anderson, that between September 26th of 1983 and November 7, 1983, that the DCP reviewed 25 percent of the non-seismic design work?

MR. NORTON: Object, Your Honor. That is both argumentative and misleading and misstates the prior testimony of the witness. He stated that at the time of his deposition when he was asked the question, that he had never thought of it in terms of percentage, and if you read the transcript that Mr. Strumwasser doesn't want to read, the following sentences, that becomes very obvious.

MR. STRUMWASSER: I will rephrase the question.

BY STRUMWASSER:

Q Is 50 percent a good number for September 26th, Mr. Anderson?

A (Witness Anderson) No, it is not.

Q What is a good number, Mr. Anderson?

A Well, I was unsure of the number. I said it could be in that range, and after going back and looking at the amount of work that was done and developing a better understanding of that work relative to the total engineering work, I am prepared to raise that estimate.

Q To what, as of September 26th?

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A As of September 26th, the work had essentially been done by that time. It was simply my understanding of what that number was, my guess, and you are asking for that.

Q So it is your testimony that as of September 26th, the real number was 75 percent or so?

A The real number was something higher than that, yes.

JUDGE JOHNSON: Mr. Strumwasser --

JUDGE MOORE: Excuse me, Dr. Johnson.

Your last answer, Mr. Anderson, was "higher than that"?

WITNESS ANDERSON: Higher than 50 percent, lower than 100 percent.

JUDGE MOORE: Okay. Thank you.

JUDGE JOHNSON: Is it entirely clear -- I'm going back to the previous page on this -- and it seems like on Page 50, the discussion is relative to DCP work, so that the statement that you read earlier in which he gave the number between 50 and 100 percent, if I were reading this, I would certainly read it and interpret it as being in reference to DCP work, and the only time the total of IDVP plus DCP comes with Mr. Kaufman's question following a request for clarification by Mr. Norton halfway down the page.

And so it is only then at the bottom of Page 51, is it precisely made clear that it is the sum total of DCP

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plus IDVP work that is being considered.

And then Mr. Anderson more or less changes his answer in relation to that. I am having difficulty with what parts of this are being used and what parts of it are not being used in terms of what the context of his answers are.

WITNESS ANDERSON: Maybe I can clarify. We were having some difficulty in the deposition with the definition of seismic and non-seismic work, and it wasn't clear what was being -- really being talked about there.

Obviously in the seismic work, we did a great deal of review of the non-seismic work at the same time. We talked about that in terms of piping systems and stress analysis and all of that. We do the piping -- the seismic loads and the non-seismic loads together, and so I think there is some confusion in developing what these percentages were.

I think if we look at the percentage of non-seismic work and include the non-seismic loading aspects of piping design, and we add that to the systems design, I think the 50 percent figure that I gave would certainly stand on its own as just DCP work.

Now if they are talking about just the systems design aspect of it, then at that time I was meaning something less than 50 percent, so the total would be 50 percent. But at that time, I was asked the question without much time to think about it. I made a very rough guess, a rough estimate,

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in response to their question, and I think that after further looking at it, I would like to raise that estimate.

JUDGE MOORE: Go ahead, Mr. Strumwasser.

BY MR. STRUMWASSER:

Q So then your current estimate is about 75 percent for the two combined for systems work?

A (Witness Anderson) For systems work, 75 percent for the two combined, yes.

Q Now, Mr. Anderson, if you had a list of calculations, a set of calculations of a common type, for example, calculations of electrical loads for components in a system, let's say you had 20 such calculations, and you selected two of them and looked at them to verify in your own mind that the numbers were calculated properly, would you have reviewed two or 20?

A Well, I think we would have reviewed two. In that case, we would actually nave looked at all of that particular type of calculation. That's an important type of calculation. I would not consider that looking at 20; that's looking at two.

Q So when you say that you reviewed 40 to 45 -- I'm sorry -- rcent?

A Forty-five percent.

Q -- 45 percent yourselves, that is not 45 percent that was subjected to sampling, but that is actually 45

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percent that you verified item by item?

A That's actually 45 percent that we verified item by item, yes.

Q And with respect to that verification, you did not seek to confirm that each applicable design criterion and licensing commitment were met with respect to the piece of design work that was being done, did you?

A Well, that's all part of the design review. We were attempting to review it to the criteria, yes.

Q Well, not explicitly, right?

A Well, not explicitly from the point of view of a program, but the engineering procedures require that review and verification be done, and we were doing it to those procedures.

Q Mr. Anderson, when you testified that DCP review was of less depth, isn't one aspect of that lesser depth that the DCP review was less likely to capture a Class A or B error than the IDVP's review?

A Oh, it may have been slightly less likely, but I think the DCP review would have captured in essence the Class A or B errors.

Q So it is your opinion that the careful procedure by which the IDVP identified all the applicable criteria, established checklists and verified the three systems designs, that was excessively cuatious for purposes of identifying design errors?

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A No, I don't think it was excessively cautious. I think that for an independent company on the outside to do a proper review, one would have to do that. We are reviewing the systems having direct knowledge, first-hand knowledge from the original designers, from the files that we have. I would say that obviously the IDVP had to have specific, detailed procedures to come in from the outside and do such a review.

MR. STRUMWASSER: Okay. I have no further questions.

JUDGE MOORE: Mr. Havian?

RECROSS-EXAMINATION (CONTINUED)

BY MR. HAVIAN:

Q Mr. Anderson, is that 45 to 50 percent, 45 percent whatever number, based on design elements? Is that a percentage of design elements you looked at?

A (Witness Anderson) No, not specifically on design elements; on total engineering work, I think, is what that estimate would be based on.

Q What is your understanding of the percentage of work that the IDVP looked at on that same basis?

A I said about 30 percent.

Q If you look at Dr. Kaplan's testimony and you calculate it based on design elements, do you know what you come up with for the IDVP's review, what percentage?

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A Somehwere in the range of 23, 24 percent, I believe.

Q Would you take my word, for the purposes of these questions, that it is 23 percent?

A Okay.

Q So don't you come out, if you use Dr. Kaplan's method, you come out substantially lower in terms of percentages than you do if you use your method of calculating percentages, at least in the case of the IDVP?

MR. NORTON: Excuse me, Your Honor. Percentages of what?

MR. HAVIAN: I think it's absolutely clear what percentages we are talking about here, Mr. Chairman.

JUDGE MOORE: If it's so clear, why don't you make it clearer.

BY MR. HAVIAN:

Q Percentage of the design elements that have been looked at, percentage of the design review is what you have been discussing, and that is what my questions are directed towards.

MR. NORTON: Excuse me, Your Honor. I am more confused now than I was on the previous question as to what we're talking -- what percentages we're talking about.

MR. HAVIAN: Again, the percentage he offered, which was 30 percent based on his method of calculating the

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amount of the design which was reviewd.

And then I asked him if he was familiar with Dr. Kaplan's numbers. If you calculate the percentage of design review under Dr. Kaplan's method you come out with 23 percent, so you are calculating the same thing in both instances. In one case, you come up with 30 percent; in the other case, you come up with 23 percent.

JUDGE MOORE: Did the witness understand the question?

WITNESS ANDERSON: In one case, it is a percentage of design elements. In the other case, it's the percentage of engineering that is reviewed, which may be different -- well, in fact, are different.

BY MR. HAVIAN:

Q So the percentage you come up with depends on how you define what you're looking at, right?

A To some extent, yes.

Q And that can be a difference of as much as in excess of a 25 percent difference, can't it?

MR. NORTON: Excuse me, Your Honor. A 25 percent difference between what, again?

MR. HAVINA: Obviously there is a range of at least 25 percent variation, because the difference between 30 percent and 23 percent is 25 percent or nearly 25 percent.

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JUDGE MOORE: Can the witness respond to that?

WITNESS ANDERSON: Well, it is a difference, and certainly that difference could be from 30 percent down to 23 percent. But we are talking about different things. We are talking about three out of ten systems or 30 percent of the engineering work in Dr. Kaplan's statistics, and in here you are talking about 23 percent of the total design elements, the design elements that were reviewed in the sample of 911, as opposed to the total of 3968. That's 23 percent.

BY MR. HAVIAN:

Q So if you use Dr. Kaplan's method of calculating this percentage, isn't it possible that the ITP's percentage of design reviewed would be substantially lower than the 45 percent that you gave?

A You mean if you talked about design elements? I don't know.

JUDGE JOHNSON: Mr. Havian, excuse me.

Dr. Kaplan uses both in his Chapter VII.

MR. HAVIAN: Right.

JUDGE JOHNSON: He calculates the ratio of design of entineering work, and he calculates the ratio from his aspect, too. There is the relative number of design elements. So when you categorize one as being Dr. Kaplan's, both methods are used by Dr. Kaplan, and therefore you really can't determine -- you can't label them that way. They have

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got to be design elements or engineering work.

MR. HAVIAN: My understanding of Dr. Kaplan's first method, Dr. Johnson, was that he had just taken the three out of ten systems and gotten his three-tenths figure that way. And I think he also stated that it was not his testimony that, in fact, three out of ten systems had been looked at.

But when I refer to Dr. Kaplan's method, Mr. Anderson, I mean with respect to the derivation under the design elements method of calculation.

WITNESS ANDERSON: I see.

BY MR. HAVIAN:

Q I'm sorry. I lost your answer. If you use that method, that design elements method, isn't it possible that you would end up with a number considerably less than 45 percent for the amount of work reviewed by the DCP?

A (Witness Anderson) It's possible that it could be less, although it's also possible that it could be more. I just don't know how the design element count would turn out. I didn't do that.

Q What is the basis for your 45 percent figure?

A It's a judgment that I have arrived at after talking to a number of the design groups, people that have been doing the work, looking at some of the work that they did, familiarizing myself with the kind of level of detail

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that they went into. It is my judgment based on that kind of information.

Q Given the fact that your judgment has changed since your deposition, would you agree that this number is rather soft?

A I think it's rather soft, yes.

q Is there any way for the Board to verify your number?

A Not that I know of.

MR. HAVIAN: I have no further questions.

MR. MC GURREN: The Staff has no questions, Your Honor.

JUDGE MOORE: We wish to thank the panel, and they are dismissed.

(Witnesses excused.)

JUDGE MOORE: We will recess until 9:00 a.m. tomorrow morning.

(Whereupon, at 6:05 p.m., the hearing was recessed to resume at 9:00 a.m., Tuesday, November 8, 1983.)

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CERTIFICATE OF PROCEEDINGS

This is to certify that the attached proceedings before the

NRC COMMISSION

In the matter of: PACIFIC GAS & ELECTRIC CO. (Diablo Canyon Nuclear Plant)

Date of Proceeding: Monday, 7 November 1983

Place of Proceeding: Avila Beach, California

were held as herein appears, and that this is the original transcript for the file of the Commission.

Mimie Meltzer
Official Reporter - Typed

Mimie Meltzer
Official Reporter - Signature