tive ways of achieving these objectives more carefully. Most important, perhaps, we should look at the criteria by which we select these alternatives. This is not only a problem which applies to fishery organizations, but to individuals as well. Each individual has a finite amount of time. Given a climate of increased social responsibility, we need to judge how best to allocate our time and financial resources to making better resource decisions.

3. We need to recognize that it is no longer sufficient for a responsible scientist to provide simply a shopping list of problems. The responsible fishery scientist, in addition to providing a list of problems, must also indicate which of these problems is most important. We must ask, "Why are we doing what we do?"

4. We need to allocate much greater effort toward investigating cause and effect mechanisms and much less effort toward developing correlations. The limited success in fishery oceanography and stock and recruitment studies results from a heavy reliance on a correlational approach.

5. We need to challenge the assumption that universities are providing people with the skills and background to cope with contemporaneous fishing resource problems. We have a new set of questions today and I think we have to ask our universities whether they are providing graduates with the skills needed to handle these problems.

6. This will involve the direct development of new techniques as well as the application of techniques already existent in other fields but not used in fisheries. For example, most of the mathematics used in fisheries rely almost totally on applied statistics and calculus. Indeed we have had substantive contributions to our knowledge from the application of statistics and calculus to fisheries problems. Unfortunately, statistics and calculus are not (with a few minor exceptions) helpful in answering the most critical fisheries question: "Who gets what?" How do we allocate resources in space and time to harvesters and processors? The general class of problem is also important from a biological point of view. To take one example, "How are prey allocated among the predators? What objective function do predators have?" There is a body of mathematical techniques that is applicable to these problems. It is called mathematical programming. It is curious that such powerful mathematical techniques for the study of allocation have not really been applied in the area of fisheries or aquatic sciences, and yet the application of these techniques may give us new and broad insight into these critical questions. This is, of course, but one example of available methods which could be applied to help us make our progress in fisheries more dynamic.

In conclusion, I have given my view of the fact, fiction, and dogma of fishery science. I do not think that our progress is totally fictional; on the other hand there are significant and material planning actions that have to be undertaken if we are to adequately face our responsibilities as we accelerate our progress toward making fishery decisions in an environment of increasing complexity.

FISHERY SCIENCE: FACT, FICTION, AND DOGMA PANEL DISCUSSION

Frey: Gentlemen, are there any additional thoughts concerning fishery science: Fact, Fiction, and Dogma?

Isaacs: Brian, in your particular shopping list problem, I didn't hear you express your priorities, or did you do what I did, assume your whole list a set of number one priorities and sent it by freight?

Rothschild: You are right, I did not indicate any priority mainly because I thought all these were number one. The second reason is that it is a very bad thing nowadays to think in priorities. The reason is illustrated by the story I frequently tell about the shopping list that has a cadillac, a beef roast, and shoe laces, and you ask anybody what their priority is and to rank them in that order. When I say, "Oops! you only have a budget of $27," the beef roast suddenly becomes number one, shoe laces number 2, and the cadillac number three. The answer is, if you tell me how much money you have I can give you my priorities.

Frey: Question regarding fishery allocations. Do you think this is the fishery scientist's job or do you feel this is a decision that should be left to politicians?

Rothschild: That is a real good question. It is a decision that should be left to politicians. However, the problem is that one reason we may not have been making as good decisions as we might is because politicians frequently make their decisions on more or less ad hoc kinds of information. It is the job of the scientist to present to the politician a menu of alternatives, and to provide for the people who make the decisions the consequences of the kinds of decisions they make. I think we can see this in nuclear power plant siting. I think it is the job of the scientist to present the new alternatives and the consequences.

Isaacs: Take one case, one I already referred to, excluding the porpoise from the catch. The scientists were not presented any freedom of alternatives. The politicians presented the scientists with a task and as far as I can find out, the scientists didn't even start to consider the significance or the alternatives, only how do you do what the politician asked them to do. True?

Rothschild: No I don't think it is.

Isaacs: Oh! The politician was presented with a list of possibilities.

Rothschild: No, I don't think the politician was presented with a list of possibilities at the outset, but I think this is certainly happening now.

Isaacs: I have always maintained that a rational view of the universe must include irrationality, as far as this universe is concerned, anyway.

Rothschild: I think it was Seneca who said, "In every genius there is a touch of madness," which is my way of saying the same thing.

Radovich: I would like to comment on one thing; John Isaacs' point in discussing the relationship of recruitment to spawning stock. John said a good relationship existed between sardine stock and recruitment, with a downward trend, but the points on the graph were widely scattered. If you look at the data more closely, rather than scattered, it appears as two other regimes. This can be interpreted in different ways. John interpreted the scatter as the effect of an anchovy population on the sardine population. By adding in the anchovy, he considers the total biomass as one population. Another interpretation of the three regimes is that more than one stock of sardines existed on the coast. If you had a far northern stock, off British Columbia, Washington, and Oregon, the curve would represent the total population being fished. Sardines were fished in the summer off British Columbia. Later in the fall, the same fish were caught in California. The northern populations were more vulnerable and disappeared first. As the northern populations disappeared, the curve dropped to lower regimes.

Isaacs: John, I think you are right, and it is the mechanism of my main point, which was that it's explicable on merely a shrinking lebensraum for the creature, not from changing conditions or entirely from the fishery but from the encroachment of the competitors. From the actual data it appears that there were three episodes of shrinking space for the sardines.

Radovich: Yes, so it appears as if you have three regimes. If the northern population declines, and if the middle one also disappears, you are left with the lowest curve. It seems to me you can explain the graph on that basis.

Another point concerning adding anchovy data is that earlier anchovy data, which is extremely scarce, gives two different interpretations on the size of the anchovy population. One interpretation, as John Isaacs indicated, is that if you added the anchovy population to that of the sardine, the total biomass would be higher and respond as a single population. Another interpretation is that there weren't that many anchovies around at a time when sardine population was low, and adding anchovies didn't give that picture at all. This was Paul Smith's interpretation, if I am not mistaken. The data are sufficiently scanty that you can do with them what you wish, and that is unfortunate.

Isaacs: John, I did spend some time correcting these data and they were restricted to very short-length intervals that I considered were properly sampled, for reasons that I could go into *ad nauseum.* These data are the estimates of sardine populations multiplied by the ratio of sardine/anchovy larvae plus unity (I'm using the larvae as a proportional correction) as the best estimates of adult stock. You are quite correct, it looks like two other regimes in respect to the sardine, a total of three—but for the total, only one! One might still consider at any point in one regime that the population is in a good position, and yet they actually are disappearing, not that they don't have the proper statistics within the regime, but because there is another species that is crowding their living ground.

Rothschild: I jotted down some notes as you were talking.

John Isaacs, I think you have raised some good points on the catch-per-unit-of-effort problem. I wanted to emphasize a few of them. My first point is that the problem of deviation from models is an indication that models perhaps are not quite what they might be.

I think this is probably not the most important function of models, but the most important function of models is to generate questions. If you have a model, it means you are modeling through a problem rather than muddling through a problem and you can therefore ask questions, "Why have I gone wrong?"

The second point is that I think to some extent you deprecated the use of the computer, and I share your deprecation in a lot of instances. I think one of the reasons they have gone bad is because in many instances we have used computers for what people do well, and then turning around we have used people for what computers do well. This is a very important phenomenon that has caused considerable problems with computers.

Isaacs: I am really concerned, not just from a standpoint of fisheries, but this kind of Word-of-God effect that computers seem to pour out. Talking about meteorologists, if you will notice since computers came in meteorological forecasts seem to be rather poor. The reason for that is the well experienced seat of the pants type meteorologist has no input. He says this forecast is idiotic but he has no way to argue with this Word-of-God the computer is putting out. So I think it is a neo-theological effect that you are dealing with here and arguing about, rather than the inherent capacity of the device.

Rothschild: Precisely.

Radovich: Let me comment too, while we are on this. I did not mean to imply in any way that models are not useful. I thought I made that clear. What I said was, if we forget what models really are and use them as what John Isaccs has called a "new developing theology", then we are in trouble, but as long as we use them as a tool and a guide and remember that we are solving something on paper, then it's OK. I think there has been a tendency to go the other way.

Rothschild: That's a good point and I agree with it.

My third point, you finished up on what population dynamicists really say about catch-per-effort and I agree with it. My only thought is that I wonder if we are thinking about the concept in terms of catch-per-unit-of-fishing-mortality rather than catch-per-unit-of-nominal-fishing-effort; whether or not the same criticisms that you generated would be applied. The second thing is the whole question of catchability, and there are at least two studies that I know of offhand in which catchability does vary as a function of population size. One is the study on king crabs that we did in Alaska that had very clear indications of catchability varying as a function of population size, and the other is a paper that just came out. It was an ICNAP presentation by David Garad that showed, I think in the cod fisheries in the Bering Sea, a very clear relation between catchability and population size. At high population size, the catchability was lower than at low population size. The next is age structure—models independent of catch-per-unit-of-effort. There is a series of models that work independent of effort, one is back calculating, we call it the Murphy method, but those who know John Gulland know it is the John Gulland method because he put it out at the same time.

Those that really know the literature know it is the Ricker method because he put it out 10 years before either of them, and I call it the iterative solution of the catch equation, and incidentally this solution does put out catchability as a function of age, size, and many other things, so it needn't be constant.

Other methodology is in tagging models which are probabilistic models for estimating a whole variety of rates and using this methodology you can estimate effort independently of catch-per-unit-of-effort.

I hate to mention something as old as the virtual population estimators which do have problems with bias, but I suppose you could measure real effort from these independent of nominal effort.

My fourth point is with respect to the fisheries independent methodology ranging anywhere from space craft to someone standing in a salmon stream counting the fish as they go between their legs. I think we have to look at the question of cost.

Finally, with respect to the menhaden example, the fish became concentrated and as a result of this they became much more vulnerable. What happened to the anchovetas off Peru? The water got warm and the concentration of the anchovetas increased. Catch-per-effort went up without abundance going up.

Isaacs: May I ask a question there? I've puzzled about these concentrations. One possible explanation is their preferred feeding mode is feeding on some sort of zooplankton schools of one sort or another—herring do.

Rothschild: I'm not sure anchoveta feed on zooplankton.

Isaacs: As the population goes down there are more available to them, these accumulations of food, in which they then go into schooling mode of particulate feeding more than a more dispersed mode for filtering. Whether it is true or not, an interesting question about the food of these fishes is: "Do the stomach contents of specimens that are caught in usual fishing represent the usual food materials?" It is quite possible to be misled as to normal feeding habits because one looks at only specimens taken only in a special feeding mode when they school or otherwise behave differently.

Rothschild: That is quite right, I keep thinking anchovetas feed on phytoplankton more than zooplankton.

Isaacs: They do feed mainly on phytoplankton, but these also may be spotty. When the fish are numerous perhaps they don't have these accumulations that they school around as commonly.

Rothschild: I wanted to mention on the troll fisheries in England, this question of density versus catch-per-unit-of-effort was pretty well demonstrated in parts of the 1963 symposium on the measurement of abundance of fish stocks (Cons. Explor. de la Mer., 155:1–223).

Now may I comment on some remarks by John Isaacs. I thought that these questions that John raised were quite valid—Questionable Inherent Assumptions in Fisheries Science. I have to express some disappointment that John is in an antichoic chamber and somehow we aren't attacking these questions better. I would say, a lot of people are thinking of these questions—people I know are thinking of some of them, and I would comment on the first one with respect to the normal distribution of perturbations. This gets into what you mean by probability. On the one hand I think that one could argue that just about everything is normally distributed if it's an average, by the central limit theorem. On the other hand, there are distributions that have no finite mean and variance, such as cauchy distribution. Maybe that is what our plankton distributions are drawn from, they very well could be and in that case it makes little value to work on them.

The second thing that has concerned me about primary productivity and its relation to fisheries, since this colloquim is oriented toward fisheries, is the measurement of the noise between plankton and fish. In other words, how variable do the plankton have to be to make them observable in what the fish do?

I was particularly interested, John, in your comment about exactly competing organisms with slightly different life histories because this is what happens within a species. We have almost exactly competing organisms with slightly different life histories and I think this is what evolution is all about, is how certain of the genetic potential is carried forth.

Isaacs: Fisheries develop subraces of salmon—the clearest case.

Rothschild: Right—and my ultimate point is your comment about regulation and management strategy, all of which I pretty much agree with, but when you say that the only fisheries that have problems are the ones that are regulated, it reminds me (Isaacs actually said "Only those fisheries that have escaped regulations have been successful.").

Isaacs: Is that what I said?

Rothschild: I hope it's not a spurious correlation—my last remark was that I think a lot of the things we have been talking about illustrate what is wrong with our present models, and I think this focuses in some priority sense that we have to come up with better models which aren't necessarily mathematical but simply an arrangement of concepts that leads us to asking new questions.

Isaacs: Of course, the most valuable model is pragmatical—the world is an imperical object, it has multiple interactions of all kinds of entities all interacting precisely the way that they do in this world without any college education whatsoever or any computer or anything that a computer can now foresee or that it can handle. A study of the past, as I pointed out, the sediments, makes the best model you can erect, a truly pragmatic model.

Frey: Thank you gentlemen, we are out of time.

B. OCEANOGRAPHY AND FISHERIES OF BAJA CALIFORNIA WATERS.