

THE ROUND TABLE DISCUSSION: INTERACTIONS AND FEEDBACK BETWEEN THEORY AND EXPERIMENT

EDITED BY S. R. HANNA

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S. HANNA: "Interactions and feedback between theory and experiment" is a very general topic that covers everything that we do. It is interesting to begin with the basic scientific method which we were taught when we were about 10 years old in our first science class. The scientific method consists of several steps, beginning with the statement of an objective or a hypothesis. I feel that in some research projects, this step is not taken, and people are going about their business without any clear objective in mind. In the case of the research discussed this week, there are two types of modeling objectives. One is the development of a comprehensive model, such as a model for the ozone distribution over Israel. Other examples of comprehensive models are meteorological models which handle the sea breeze and the depth of the marine boundary layer, models which simulate the mesoscale flows in the Jordan Valley, and models explaining ozone and SO₂ distributions. These research projects have a very comprehensive objective. The second type of objective is more specific and is generally oriented towards one particular aspect of the problem. An example of this type of project is the Thorney Island field study, which took place in England. The scientific objective was to determine what happens to a column of dense gas when it is instantaneously released into the atmosphere at ground level. Although there are not any real-world industrial sources that are exactly like this, the field data allow us to investigate a very narrow aspect of the problem.

Another issue is whether wind tunnel data are just as good as field data. I have often looked at the Fackrell and Robins wind tunnel data, which have been used by several people on this panel and in the audience to try and validate models which predict the concentration fluctuations in a plume. However, I see many things happening in the Fackrell and Robins wind tunnel study that I have not seen happening in the field. There seems to be a double hump in the cross-wind distribution of concentration fluctuation intensities observed in the wind tunnel. Many of the models attempt to simulate this double hump, which has not been observed in the field. Also, mesoscale eddies are important in the real atmosphere but cannot be simulated in a wind tunnel. These mesoscale eddies determine whether the concentration intensity σ_c/\bar{C} approaches 0 or 1 or some other constant. In the real atmosphere, for as long a sampling time as you have, there are always

mesoscale eddies. These eddies will cause fluctuations and σ_c/\bar{C} could approach any non-zero value, depending on the particular characteristics of the mesoscale eddy field.

The final point that I wanted to make is that one main use of experimental data is to determine which parameters are really important for the scientific phenomenon that you are studying. These parameters can be identified by applying factor analysis and multivariate analysis to field data in order to determine if there are fundamental functional relations that should be simulated by the theory. Or, before deriving any theory, you can simply plot your data in a similarity form and calculate the correlations. This will tell you which of the parameters are strongly related to the other ones, what types of similarity analysis should be done, and which factors are unimportant. If there is a parameter that is not correlated with anything else, then perhaps it is not important enough to be included in the theory.

R. STULL: Steve suggested that I give a specific example, namely the relationship between experiments, theory, and applications for transilient matrix theory.

We finally figured out how to measure the transilient mixing coefficient in the atmosphere. Picture the following scenario: Assume you have a tall tower and at some height on this tower you release a series of small smoke puffs, and you track the movement of those puffs – say by lidar – over a finite time interval. Each puff might end up in a different location. After a large number of puffs have been tracked you have a distribution of puff destinations. This is in an absolute dispersion sense; you are not looking at the spread of individual puffs, but the movement of puff centers. Thus you have, more or less, a distribution of puff destinations for one source height. That distribution, if you describe it as a probability distribution that has an integral of unity, gives you one column of the transilient matrix, by definition. Assume that you continue to track those smoke puffs over longer times and of course they move to different places. You will have a different distribution for longer times and it will give you a different transilient matrix. Remember that the matrices depend on the time interval as well as on the turbulent mixing.

In this way you can measure one column of the transilient matrix for different times. Of course if different puffs are released at different source heights, then you can get the full transilient matrix. You know what all the air is doing, where it is coming from, and where it is ending up. Now step back from this field experiment and consider what you have – smoke puffs released from particular heights are like smoke emissions from a smoke stack. That means that if you know the transilient matrix which describes the characteristics of the flow and the turbulent mixing, you can immediately pick out any column of your transilient matrix to determine how this smoke disperses from a smoke stack at any height – say, tall smoke stacks in one column of the matrix, shorter stacks in a different column.

For example, for any one particular type of smoke stack you could look at the transilient matrices for different time intervals and see how the distribution will change effectively with distance downwind of this stack. These transilient matrices

can be measured in the real atmosphere or simulated with large eddy simulation models. What we have seen so far, is that these transilient matrices obey similarity theory; namely, for the convective mixed layer the matrix has a certain pattern. With a measured matrix for some particular state, like free convection or a neutral boundary layer or a combination of the two, one can scale the matrix with respect to the depth of the mixed layer and with respect to appropriate scaling velocities like u_* or w_* . These empirically measured matrices can then be applied in Israel, United States, Germany or wherever, once you “unscale” the matrix using the similarity variable. That is a way to go from field experiments using the theory of transilient turbulence to an application for different smoke stacks.

The theory has potential for site studies and planning for future power plants or industries. You can ask yourself what happens if you increase the height of a smoke stack; you can look right into this transilient matrix and pick out different columns and see what the smoke will do. If you can measure the mixing from all possible heights and destinations, then you have all the information for any height stack that you want. It is already there in the matrix. This potential application has not been used much yet. Hopefully in the next year or two my colleague, Eloranta (who has a lidar at Wisconsin) and I will be measuring mixing with smoke puffs in the real atmosphere. This will be our first attempt to measure the thing in real life, and it is exciting for us. We think that we can capture with transilient matrices not only random-like situations, such as dispersion, but also the deterministic behavior, such as that speculated by Deardorff, simulated by Lamb, and measured by Briggs.

M. POREH: Alternatively, all you have to do is use the prediction schemes to give you exactly the value of the transilient matrixes, the probabilities, the similarities, and so on. You do not even have to go to real experiments.

R. STULL: You are right, if our parameterizations were perfect, then we would not have to use experiments. However, our parameterizations are not perfect. So I think that at least for various special cases it is really important to measure transilient matrices for the real atmosphere. Once we confirm that the measured matrices match the simulated or predicted mixing from theory, then perhaps – if the scientific community is willing, we can continue to use theories to calculate the matrices without having to do expensive field experiments.

B. SAWFORD: I think that discussing the interaction between theory and experiment is a difficult thing to do, as Steve pointed out in the beginning. Most of us are very much used to working with the whole range of scientific tools, from theory through experiment, field work and so on, so here we are tending to state the obvious.

I would like to try and put a philosophical point of view on some of these ideas. I think the first point to make is obvious, which is that the whole is greater than the sum of the parts. In fluid mechanics in general and atmospheric science in particular,

we need to have at our finger tips, all three tools of theory, laboratory experiments, and field experiments. They are very much complementary; no one of them will give the whole story to most problems. It is really quite dangerous to be just a theoretician or an experimentalist and not give due regard to the other activities. Perhaps the most dangerous thing to be is just a modeler and not be properly aware of the experimental aspects of the work. Steve also mentioned the need to have a clear objective in whatever you are doing. This is a very important point and I suspect that sometimes experimentalists tend to charge in and make a range of measurements without perhaps thinking how those measurements might be used, and this is sometimes a failing in the data. We come across that problem as theoreticians when we try to test our theories using data. We find that the data are inadequate because something that we need for our theory has not been measured. That is a very good example of an instance where there needs to be interaction between the theoreticians and the experimentalists. Perhaps on a slightly controversial note, I would like to suggest that fluid mechanics is basically an empirical and experimental science. I know that there are lots of exceptions and I have managed to think of some. I think that for instance the Kolmogorov similarity theory is one example of something that came out theoretically and then was confirmed experimentally. At about the time that it was confirmed Kolmogorov stood up and said that the theory was not quite right, that we have some adjustments to the theory, and away they went again with intermittency corrections. That is one example where theory came first. By and large, fluid mechanics is an experimental science. A lot of the theories are descriptive and they involve unknown constants, "fudge factors", whatever you like, which at the end of the day require the experimental data to determine. One other point that I might raise here is a question really; where do the various sorts of large numerical calculations fit into the scheme of things, are they experiments or are they theories? I tend to think of complete turbulence calculations, direct numerical simulations of turbulence, for instance, as experiments. Basically they are solving Navier-Stokes equations exactly for particular resolution. It is rather like doing a laboratory experiment for that resolution. But you obviously have a whole range of numerical calculations, going through large eddy simulations, where you are doing a little bit of modeling and still doing a lot of almost exact calculations. Those sort of things sit on the borderline between experiment and theory. Perhaps carrying on this theme of the importance of experimental work, I can close by saying that today as more and more computing power becomes available, there seem to be more and more people taking up computers and becoming "computer jockies".

Therefore, I would like to make an appeal for people to bear in mind that the ultimate confirmation or ultimate truth comes back to making experiments. I think that the computer people are very much in need of good experimental data so that when they are doing large eddy simulation, for instance, where there is still some modeling involved, they can keep their work on track.

D. RIDE: I think as scientists we have yet to make a proper distinction between descriptive and predictive models. As scientists we tend to include as much science as we can, although we have had two papers at this conference which clearly demonstrate that there is a limit to the usefulness of putting more physics into our models. But many predictive models require a far greater simplicity. For instance, if you have a descriptive model which needs the first, second, and third moments of your concentrations, you may very well get a good descriptive fit. When you come to prediction, most of us would agree that you can predict a mean concentration, say, with a fair degree of confidence. You can also perhaps predict the variance, but few of us would predict the third moment, apart from saying that in many situations, probably the pdf is strongly positively skewed. As a user of models, I come constantly up against the problem that I am dealing with elegant models developed by scientists who, in a way, want to demonstrate that they have done good science. However, the elegant models are not always good predictive models.

B. SAWFORD: To some extent, when I talked about predictive and descriptive models, I was perhaps thinking a little more generally than you obviously are, in the sense of predictive theories being those which actually come up with a new result which is then demonstrated experimentally. Most of the models we deal with I would regard as descriptive in the sense that we have some sort of range of phenomena that we observe, whether it be dispersion in smoke plumes or whatever it is, and our models are really just empirically describing that phenomenon. They make prediction of detail, of course, of concentrations and things like that, but I was thinking perhaps more generally and philosophically about the terms “predictive” and “descriptive”.

H. KAPLAN: So far, most of the models are designed to calculate average concentrations, and many experiments are designed to fit parameters to those models. We know from theoretical works and from experimental measurements, that fluctuations around the average are of the same order of magnitude, or even an order of magnitude greater. Do you think that the next generation of models and experimental fitting to these models – and I am speaking about applied models, not theoretical ones – will be to design models to predict probabilities and not average concentration?

B. SAWFORD: I think that it is difficult to know where we are going in terms of being able to properly build models of higher order properties of plumes and dispersion, especially in a practical sense. My feeling is that we will eventually fall back on the sorts of things that Steve showed the other day, which are fairly simple models which have lots of problems with them. But because the problems we are dealing with are so noisy, so hard to measure properly, in a practical sense those simple models are the best models. Making a model more complicated doesn't necessarily improve things, especially from a regulatory point of view. When you

necessarily improve things, especially from a regulatory point of view. When you are asking fairly simple questions, even when you ask about variation of concentrations and so on, you are still going to be talking in fairly broad terms. Whenever you try and make specific comparisons in the atmosphere, you run into the enormous variability, aside from variability of the turbulent part itself, of all the other factors that Steve discussed in his paper.

R. STULL: I think that part of the work that researchers do is based on the regulatory environment of what is required. Is a 1/2 hour or a 1 hour average concentration required? Do we not care about fluctuations in the regulatory sense? If that's the case, then a lot of our effort, spent in finding mean concentration values, has been driven by certain needs – by regulatory needs, among other things. If these regulatory needs change, then our research goals might change.

P. CHATWIN: It seems to me that we know something that most regulatory authorities don't know – quite often what regulatory authorities want is nonsense, scientifically. I actually feel that given the need to chase dollars, there is a conflict between the need to get the dollars for research and what we think is proper research. I am constantly faced with this conflict. I feel that we as a community, and I'm talking as scientists and engineers, not as regulators, ought to oppose this and say that what is really wanted is models of good scientific sense, based on sound physics. Perhaps I will say more about that later.

S. HANNA: I think that, unfortunately, we are dealing with a complex question, involving scientific, social, political, economic, and legal issues. In the United States, lawyers usually make the regulations. We have suggested to the EPA that concentration fluctuations are important and that they ought to account for PDF's and so on. However, whenever EPA scientists bring this up with the lawyers who make the rules on air quality, they just don't want to deal with the uncertainties that are implied. Lawyers like absolute truths, and don't want to hear that the mean concentration is $100 \mu\text{g}/\text{m}^3$ with a 95% confidence range from 50 to $200 \mu\text{g}/\text{m}^3$.

P. CHATWIN: That's exactly the point.

M. GRABER: I am speaking as a regulator. Laws are being written by lawyers but they know nothing about lognormal distributions, etc. They put into the law whatever the scientists tell them, and if it is a 1/2 hour average or a 1 hour average, or this type of distribution, or another, it should come from the scientific community, who have to convince the lawyers to write those laws according to what science dictates. I think when it comes to regulations it is always a compromise based on the law, and input from lawyers, and what the scientific discipline has to offer. That is my experience.

P. CHATWIN: I think this is exactly right. There is obviously a keen scientific interest in fluctuations and in the statistical nature of gas dispersion which is not reflected in the laws and regulations. Governments use science which is usually at least 10 or 15 years old. I think that it is difficult to get the lawyers to accept proper advice, given the present scientific controversy.

M. GRABER: They have such difficulty in understanding us. You need lots of patience to tell a lawyer, even if he is intelligent (and most of them are intelligent), to understand what you mean by those numbers and formulas. It can be done, but it takes a lot of effort and patience.

S. HANNA: This discussion points out that there is a great need for communicators who can translate the findings of the scientists into something that can be understood by lawyers and law-makers.

M. POREH: I want to make a confession. I don't study theoretical diffusion problems because I want to provide prediction tools for regulators. Sometimes we don't tell them all we know, so they will continue to support our study. For example, we don't know whether concentration fluctuations are really the decisive factor, in fact, on human beings concerning, for example SO_2 .

I think we can agree on one thing – that turbulent diffusion in general, and in the atmosphere in particular, is a difficult problem. We must be very careful about using methods that don't have a theoretical basis. On the other hand we realize that such methods limit perhaps our ability to provide theoretical methods to analyze such non-linear problems. The overall capability of numerical work is increasing, when for \$10000, you can put a powerful computer or two on your desk. Most of the students feel, and I think they are right, that they want to work on theoretical modeling. They would rather not worry about experimental work, since it is much more difficult, and you need more funding. Universities provide computers, they don't provide equipment for large experiments. We should realize that most of the numerical schemes are based on semi-empirical closures, and therefore we do need data. Field studies are a must; after all, you want to study the real atmosphere. However we must realize the inherent limitations of these studies. For example, because of the diurnal cycle, we don't get to steady state. Sometimes we don't get good averages, if averaging time is not sufficient, particularly for events that do not occur very often, such as relatively high concentrations. When I looked at some laboratory data, I found that once in 60000 points I got a value of instantaneous fluctuations that was 15 times the average. However, when I calculate the probability that this will happen in the atmosphere, it is clear that this fluctuation is very difficult to find at a particular location.

With respect to small-scale physical modeling, as an experimentalist I celebrate the success of the most simple physical model – the water tank, which is not correct from a Reynolds number point of view, but which does provide a basic

understanding of the complicated convective boundary layer. I was happy to hear the results of the CONDORS experiment; actually the main conclusion was that the Deardorff water tank experiments were correct. I know they are not perfect, and we must realize that physical models are models. They do not and will never provide a full, accurate description of the atmosphere. The Reynolds number is not correct. However water tanks provide good data, controlled data, with which we can test our models, by changing the Reynolds number and seeing how the model can predict something that we know very well. This is not the case in the atmosphere. I think we have a lot of work to do together.

When I look back, a lot has been accomplished in the last twenty years, and I hope that we can say so 10 years from now.

G. KALLOS: This discussion seems like the discussions we have in our department once or twice a year about experimental, modeling, or theoretical work. I don't think it is true that there is a gap between theory, modeling, and observations. This is something we have to accept. If something is not in our field, we have to look carefully and maybe think about it – someone may come and present a new idea. We cannot reject it immediately. Both methods, experiment and theory, are tools. First of all we have to define some basic concepts of the problem we are studying and then we look at the theories. Then we look at the tools available, and then we choose the right tools. For example, if we have a series of screwdrivers and we want to repair a watch, we need a specific screwdriver. If we want to repair a car we need another screwdriver. It is the same for the experimental work as it is for the modeling work. For an experimental work I would ask some basic questions: what is the time scale or the space scale of the phenomenon we are studying? How representative are the selected monitoring locations? The same applies for the models. Is the right physics included in the equations? What are the scales and how are they described by a model? What are the assumptions we make? If we cannot control these, the results (and consequently our conclusions) have nothing to do with reality. What is the behavior of the natural phenomena? We are trying to describe nature on a piece of paper. What is needed is a careful design first, according to the key parameters, the development of a conceptual model, and finally the selection of the most appropriate tools. This selection must be done according to the status of knowledge. It does not matter if it is experimental or modeling. I think that the combination of both approaches always gives better results. Therefore, there is need for cooperation between experimental and modeling groups.

M. POREH: I fully agree except that we have to realize that we are not free in reality to choose always the right technology, because we would have to be experts in the technology. The example of the large eddy simulation is very striking. It appears that most scientists cannot afford to take this approach. When you look, you find that this technology has been used particularly in large research centers.

G. KALLOS: I think this is the art of science, to choose the most appropriate tools for the specific problem. We cannot rely only on experimental measurements or only on modeling results. In experimental work, we can make the same mistakes. Let's say that we choose the wrong way of sampling, or the wrong sensor or whatever, and the whole thing collapses. It is not only the modeling work which suffers from such mistakes. I strongly disagree with people who say that they evaluate model results with some experimental results. I will never accept this argument, or this methodology. For me, it is something in between. I can say that this is a comparison between experimental work and modeling work, and not evaluation. Experimental results suffer a lot from time and space representativeness. I agree with Dr. Poreh on the cost of the technology we want to use. This problem is more severe in experimental studies.

G. BRIGGS: If I can add to that: social and regulatory needs are always far beyond the solidly established science, so we are always forced to extrapolate models further than data would permit. An awful example is the extrapolation of Pasquill's original curves 100 times beyond the distance of most of the data – all stable and unstable curves were based on the Prairie Grass experiment, which only extended to 800 m from the source. But when the regulatory need was for distances of hundreds of km, someone was not afraid on logarithmic coordinates to extend Pasquill's lines rather drastically. I agree that experiments can never give us all the information that we need. My own experiment, CONDORS, as an example, tests only the passive case, while most sources in the real world are buoyant. But the intent of this game is to build confidence in both theoretical and laboratory models and in numerical simulations. Laboratory tanks can be used for far more varieties of experiments that I can undertake in the field, for 1/4 million dollars or so. This is true likewise for large eddy simulations. But only through field experiments can we find the weaknesses and limitations in these tools.

H. VAN DOP: I would like to look at some current problems. We all know that K-theory fails in many cases. We are looking for alternatives to that theory in order still to be able to obtain a reasonable description of the transport processes in the atmospheric boundary layer. An example is for instance the transilient turbulent theory which is a way to get a better description of transport. Some say that Lagrangian models are an alternative to describe transport and that they have features which are not included in the K-theory. I think that modern theories have certain advantages and whether you want to apply specific theories or not depends on the practical situation where you want to apply them. To come to my second point: In my view we really need many more observations and much more interaction with experimentalists, involved in cloud physics and transport processes. Here we have a case that is a combination of processes. It is turbulence, buoyant transport and condensation where various phases are involved. The work is all based on well established theory: the turbulent dynamic and thermodynamic equations. So basically

the theory is there, but it is the synthesis that is lacking, which is the main point that I want to make. Atmospheric science, contrary to other sciences, is a science where we have to live from synthesis, from putting things together and it is unlike many other fields where a problem is approached by breaking it into tiny pieces. We look at these tiny pieces in detail keeping everything else constant and well defined. That is analytical science. In the atmosphere we are doing “synthetical” science. We have to deal with many processes at the same time: turbulent transport, thermodynamics, chemistry, and radiation. These processes influence each other and that is where the problem arises. What is going on in the atmosphere is a combination of thermodynamic, chemical and turbulent processes in which each, on its own, is quite well understood, but put together, the interacting system is largely not understood. For instance, take the ocean and atmospheric circulation in climate science; we are not able to predict the future state of the atmosphere, not only with regard to physical parameters like temperature or cloud cover, but also with regard to chemical composition. We know roughly what the chemical composition of the atmosphere is now, but nobody dares to say what it will be in 50 years, not because we do not understand the science, but because we have a problem of putting it together. That is where we need better links with experimentalists. We use insufficient observations and that is why we, the modelers, have so much freedom; there are no self-correcting observations. That is my main remark but I have an additional point. Because of the complexity of models, verification is often very difficult. In a simple analytic experiment you can say: “this is my observation, this is my theory,” compare them and you can say whether the theory is wrong or right. In complex atmospheric experiments there are often so many parameters involved that it is hard to tell whether you have made a satisfactory model. So, we are often accepting models by consensus. That means that a group of peers agrees on this or that model, or on specific techniques and parameterizations, which are considered to be up to the common standard of knowledge. Therefore, we accept a model which contains pieces of these “agreed” elements as an acceptable model. That is a way of deciding upon model performance which you will not encounter in other sciences.

W. SADEH: I would like to make a comment on an aspect of turbulent diffusion that bothers me. If I go back to conferences of 20 years ago and, unfortunately, I do not have here a tape recording of these conferences, I feel that we had exactly the same discussions. I remember the first time I met Steve Hanna more than 15 or 20 years ago and then we had the same type of discussions: we need one more experiment (!), we need one more model (!), what is our objective? I think we are faced here with a problem which is much more basic. We have been measuring the same things for many years. The differences between the results of measurements of the plumes that I have seen at this conference is that we have better instrumentation and better computers to analyze the data. The question is: **Do we have better understanding and or knowledge?** We have models, we try to develop

one model after another, and we make measurements to death! It is much easier to develop a model if we do an experiment and collect some data and to publish a paper instead of sitting down and asking a couple of basic and simple questions: **What do we know and what we do not know?** and, **what we are missing in our understanding of the problem?** And, I would like to give an example. To some extent, all of us are in the business of a concept called “boundary layer” introduced at the beginning of the century by Prandtl. We have a concept, a good concept. And, we try to see how this concept can be applied to turbulent diffusion in the atmosphere, and we are trying hard to apply this concept to solve a host of specific problems. The next question is: Do we have an objective to try to gain a basic understanding of turbulent diffusion? When my students ask me this question I have a simple answer: **We are fortunate that we are in this field because we shall be in business for life!** The chances of a general understanding or a general solution are so minimal that you have to look at each individual problem and solve it. If we were to present the turbulent diffusion problem in those terms to the regulators, the lawyers, and the politicians, we would have a much better chance of success. I do not see in the foreseeable future any way to achieve a full understanding of turbulence or of turbulent diffusion that will enable us to come up with an equation, like some basic fluid mechanics equations such as the Bernoulli equation, that will give us a reasonable operational answer. Let’s face the facts. The problem of turbulent diffusion is so complex that we have to do more basic thinking and less measurements and modeling. The issue is that the problem is so complex and of such a nature that in all likelihood a general approach is unfeasible, and not realistic. If we look at each individual problem and if we try to solve each individual problem through a synergism of thinking, analysis, computer simulation, well designed and planned measurements and modeling we will be much better off. But, the thinking about the problem prior to computer simulation, modeling and measurements is what is first needed.

H. VAN DOP: Of course I agree with this statement. Apart from a few details you are right. All the laws and all our knowledge will not improve the situation very much because these laws are already known. It is the synthesis which is the big problem but I do not agree with you that there will not be any progress, because 20 years ago the weather prediction was for one day ahead but now it is for six days ahead with equivalent skill. So there is progress. And you can say that physical insight has not changed since that time, but progress has come as an improved understanding of a complex system.

W. SADEH: For years I was a weather forecaster, and I remember using synoptic maps, drawing isobars and making forecast for pilots. When I look at the forecasting system today, it is very sophisticated. To what extent is it better than the simple forecasts that I gave to pilots 25 years ago?

P. ALPERT: I want to comment on that. I think that we, like all people, like to talk about forecasts. And I think what Han van Dop just said is true. We do have 5 to 6 day forecasts for the general flow of the atmosphere that have the same accuracy we had 20 years ago for one day forecasts.

P. CHATWIN: I think that you will not be surprised that many of the points I wanted to make have already been made. Like Michael, my comments are basically driven by my prime interest, to understand better the behavior of the concentration field in turbulent flow, and particularly properties other than the ensemble mean. I believe that from the scientific point of view, a statistical description is essential. As we have already discussed, I do believe that, eventually, it will be recognized, not only by scientists, engineers, meteorologists and oceanographers, but also by lawyers and politicians, that scientific descriptions, even of weather forecasts, ought to be statistical. The probability that it will rain tomorrow in Eilat is very low, although the probability that it will rain in London is 50 percent (or 75 percent). The recognition of that as a correct sort of description seems to me to be progress. Like everyone else on the panel, I stress that experiments are essential. I feel like Professor Kallos, nevertheless, that there is a need to recognize that experiments are not gospel. We must ask certain questions about them, as well as models. I just want to mention three points that were in my paper and I do think they are important. Steven Hanna, Brian Sawford, and one or two others already referred to the needs for clear objectives in investigations, and I think it is important in doing experiments involving turbulence to make sure we define the underlying ensemble. I also think that it is important to investigate, ask questions about, and check that we know the relationship between the measured concentrations and the real concentrations. I also believe that we have not perhaps been as critical or as careful in investigating phenomena like instrument smoothing and the treatment of noise. I want to emphasize again that molecular diffusion is a key process. If I could just single out one sort of experiment that would interest me (and, I think, many others), it would be that we pay more attention to the effect of the source, and particularly source size, on the structure of the concentration fluctuations.

M. GRABER: Could you explain your last comment on the size of the source?

P. CHATWIN: The concentration distribution you observe when you release contaminants in turbulent flow comes from a source; it may be an explosion, or may be a continuous release like from a smoke stack. I think that it is well understood theoretically, that the bigger the source, roughly speaking, the smaller the concentration fluctuations. I can give you references to that. As far as I know, what is not known is how long these effects persist, and what parameters are most important. I think that this is a general point that has not yet been investigated to an adequate extent. There have been experiments, but not very many.

P. ALPERT: I have a question regarding the transilient matrix method. In a sense it is not different than trying to solve the turbulence problem in second, third, and higher order closure. I see the mechanistic way of thinking in the transilient method, but I don't see how the principles of physics. In contrast, I do see how the principles of physics are used in the second, third, or higher order closure.

R. STULL: The transilient matrix is just a statistic. It allows you to retain some measure of non-local mixing, which you don't have in most local methods. When you are looking for a turbulent closure method in the transilient matrix, you can try to close this non-local mixing. In the transilient method, you can describe this effect, whereas in the second-order closure method, everything is local. Of course, with second-order closure, people hope that some of the non-local mixing is brought in through the higher order terms, but in reality, the closure is entirely local. You're taking only the local gradients or the mean values, or it could be the second or third moments. As we have seen by the CONDORS experiment, there are very many non-local processes, and a lot of people are trying to find different ways to capture the large eddy, or the coherent structure of the non-local process. Whether it is a CONDORS experiment, where we see the deterministic results, or a stochastic simulation with the Langevin formula, or the transilient approach, there are many different ways to capture the coherent structure and its effects, which are not captured in most of the K- theory models.

M. POREH: I'd like to make a comment regarding the pessimism that has been voiced. The type of questions we are asking today are much more advanced and sophisticated than the ones we asked 20 years ago. Then, we did not understand the convective boundary layer, and used σ_z and σ_y curves. We are embarrassed today that we used them. We didn't know how to predict well the stability of the atmosphere because we used discrete stability classes. The contributions of Jeff Weil showed clearly that we were mistaken. We interpreted many stable cases as neutral. Our basic understanding is on a much higher level than 20 years ago.

S. HANNA: I would like to get back to what Dr. Kallos said. Some model developers have pointed out that the mesoscale numerical model predicts an average over a grid, whereas the observation is only at a point. Therefore, you really shouldn't be attempting to evaluate the predictions of the mesoscale model with a point measurement. It is sometimes claimed that the model produces better results than the point measurements. However, I have always felt that the point data must be worth something in the evaluations. How should observations from a mesoscale wind network be used to evaluate the predictions of a mesoscale model?

G. KALLOS: If I want to check the performance of my model (if it is good or bad, close to reality or not) I need a set of data which must have a certain scale. When we put in a monitoring station, we have two options: to put it on the

roof of the building or next to it; of course the results are completely different. What I would like to see sometimes is a better way, a method of filtering the data, for example, to obtain a data set which will be representative in a certain space and time scale comparable to that of the model used. Mesoscale and several other scales of models provide in general a better picture of a phenomenon because of the time and space resolution which cannot be obtained even with a very dense monitoring network. It is difficult to tell whether a model always gives better results. I never said that, and I don't believe in that. My disagreement is in assuming that experimental results are always true because they are measured. Evaluation of experimental measurements is equally important as model evaluation.

P. CHATWIN: Such a model is usually deterministic, but what you are looking at is one realization of a statistical process. Experimenters (and regulators) should ask modelers to provide estimates of the statistical uncertainties associated with their model predictions.

S. HANNA: I think we have reached the end of our time and I would like to thank you for your participation in this session.