RETROSPECT AND PROSPECT

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At this point, many of us feel overwhelmed by the wealth of material that has been hurled at us during this conference. It calls to mind the story that the economic historian M. M. Knight used to tell. Some of us looking for answers regarding seasonal analysis may feel as if we're in a dark room looking for a black cat. That's bad, but it's not as bad as it could be. Pity the philosophers who are in a dark room looking for a black cat that isn't there.

On the serious side, let me begin by thanking everyone for their very generous contributions to the conference. In particular, Shirley Kallek and Elmer Biles and their staff at the Census Bureau have been very helpful in organizing local arrangements, reproducing and distributing the papers, and, in so many different ways, it is hard to know how to thank them enough. At the National Bureau of Economic Research, Gary Fromm and his staff handled more details than I'd like to count. Also, many major elements in the arrangements for the conference were handled by them. The members of the Steering Committee worked long and hard selecting papers for the conference and contributing to the formulation of policies for the conference. The chairmen of the conference sessions managed the sessions extremely well. And, above all, I thank the authors and discussants for the tremendous contributions that they have made to the conference. Their research input, I think, is outstanding.

Regarding the conference, it comes at a most propitious time for several reasons. First, the Census Bureau is eager to improve its methodology in the seasonal area that includes seasonal adjustment, seasonal analysis, and other topics. Second, there have been major developments in theoretical and applied time series analysis in the last few years, and I believe that the seasonal area is a very good testing ground for these new theoretical and applied time series analysis techniques. Third, there have been tremendous advances in econometric modeling techniques and applications over the years. In fact, I have often suggested that we recognize the econometric modeling industry, provide it with an SIC number, and, above all, institute an annual model show. Fourth, you are well aware of the many advances in computer technology that made possible the processing of large numbers of series, produced multipurpose computer programs, such as the X–11 program, and facilitated applications of advanced statistical techniques. We can expect to see more advances in computer techniques that will be extremely helpful in the seasonal analysis area. Fifth, we are getting more and better data, a development that is of the first order of importance. Of course, in science, generally, measurement plays an extremely important role. In economics, my view is that we are getting more hard boiled about the quality of data, and this changed attitude is having an impact on the quality of all sorts of analyses including seasonal analyses. The last point that makes the timing of this conference very satisfactory is that we are getting many, many more sophisticated users of Government data in industry, Government, academia, and elsewhere. From personal observations, I believe that the level of sophistication in the business community has risen tremendously over the last decade. I believe the same can be said about users of data in Government and in academia. The congruence of the major developments alluded to earlier sets the stage for the conference that has been characterized by a most constructive attitude among the participants and sponsors. This constructive attitude will help to insure that the steady progress in seasonal analysis during the last few decades, outlined in Shirley Kallek's paper and described in the keynote address by Julius Shiskin, will continue on in the future at even a more rapid rate than in the past.

Now, let me turn to consider the conference program, since I am to be retrospective in the first part of this talk. The program for the conference was formulated, as follows, by the Steering Committee: The program started with discussion and analysis of the objectives, philosophy, and overview of the problems of seasonal analysis and adjustment. Next, attention was brought to bear on a review and analysis of procedures currently in use and then to improvements in procedures currently being used. From this topic, we went on to new methods of seasonal analysis and to econometric modeling and seasonality. Last, a session was devoted to the analysis of special problems, in particular, aggregation problems and seasonal analysis.

From this review of the program, it is clear that we planned to move from the general framework to a consideration of the older, better known procedures, possibilities for improving these procedures, newer statistical and econometric methods, and then on to some very troublesome aggravation problems, I mean aggregation problems. Thus, the older approaches were considered in the context of newer approaches; this is very fine and in line with John Tukey's remarks about the desirability of viewing particular procedures in broader contexts. Also, concerning newer approaches, we all realize that some of them are still experimental, particularly from the point of view of those who are on line, have to produce the numbers, and must face the public. Some of this newer experimental work can be regarded as similar to what engineers do with their experimental models in wind tunnel experiments. That is, some of the experimental seasonal models have been put into a wind tunnel and tested a bit with one, two, or a few economic time series. Wind tunnel experiments are very helpful and can lead to interesting insights concerning how procedures will probably work in practice. The next stage is to get a real model built, put a test pilot up in the model and have him take the model plane through strenuous test flights. The problem of some of the developers of new seasonal models and methods will be to find test pilots who are willing to fly these experimental models. On the other hand, I do believe that some of the new modeling techniques described at the conference will be found extremely valuable in practice.

Retrospectively, again, it is almost impossible to summarize all the contributions that were made in our conference program. There are so many "goodies" that it is hard to mention them all. However, what I'll try to do is to describe, using my personal filter, what I consider to be some of the main themes developed for us at the conference. Further, I shall try to put them together in such a way that we can obtain some guidelines about what might happen in the future.

We started with the very important problems of the definition of seasonality and the objectives of seasonal analysis. The thoughtful papers by Kallek and Granger, comments by Fromm, Klein, Sims, and Tukey, and remarks of many others covered aspects of the definition of seasonality and the many-sided question of the objectives of seasonal analysis. Shirley Kallek's paper has a very fine list of objectives, and others have contributed thoughts on this subject. One of the major points that emerged is that the objectives are multidimensional and interrelated. We have to keep this point in mind in discussing particular contributions and techniques. The objectives of seasonal analyses have to be considered very seriously in evaluating techniques. Some objectives I would group under the general heading of scientific understanding of seasonal phenomena. Scientific understanding is often desired for the purpose of getting better predictions, which is very important, since there are many people in the world who have to make forecasts and predictions. Improved understanding of seasonal phenomena can undoubtedly help in producing better predictions. Secondly, in many areas, e.g., labor markets, the housing industry, money markets, agricultural markets, etc., seasonality poses serious policy problems. Thus, there is a link between the formulation of good seasonal policies and scientific understanding of seasonal phenomena.

There is also a strong interest, on the part of many, in isolating and measuring different components of economic time series. The nonseasonal changes in economic series are of great interest to many, as Shirley Kallek pointed out in her paper, and measuring them may be a very basic goal in the efforts of some in analyzing economic time series data. A second point, here, is that the seasonal patterns in economic time series, in and of themselves, are of great interest from the point of view of scientific understanding and seasonal policymaking on the part of firms, households, and governmental units. On this point, I like to think back to my friends in the fishing industry in Seattle, where I spent 2 years studying their seasonal pricing patterns and how conservation measures possibly affected them. Industry personnel and conservation authorities were certainly concerned about whether one season differed from another, how prices varied from day to day during the season, and related seasonal matters.

Scientific understanding involves approaches at different levels, as many have emphasized. At one level, there is the process of description that involves a descriptive or empirical approach. In this approach, the objectives are to describe, measure, and categorize seasonal effects and fluctuations and to classify series according to the processes underlying them, much in the spirit of the Burns' and Mitchell's NBER research on measuring the properties of business cycles and of Kuznets' early work on seasonal movements in industry.

A second, more ambitious approach, that emerged at the conference, is a statistical modeling approach. Here we have a model for the observations, and, with a formal statistical model for the observations, as many commented, one can do much more in terms of drawing inferences from the data, of deriving optimum predictions, and in estimating seasonal and other components. Of course, it is assumed that the model is an appropriate model that is not too badly misspecified. As long as one has a reasonably satisfactory model, you can do all these things that are harder, or impossible, to do in an empirical or descriptive approach.

At a still more ambitious level, we encountered the econometric-statistical or causal modeling approach in several of the conference papers. Here I mention, in passing, the well known uses of econometric models, namely, prediction, control, and structural analysis that become possible with a structural, causal, econometric model. Of course, this is a more ambitious level of modeling than is involved in a purely statistical modeling approach.

Many, including Box, Hillmer, and Tiao, have emphasized the interaction of work, at all levels, as being very fruitful, a point that will serve as a focal point for some of my retrospective review of other major themes at the conference. Concerning the interaction between descriptive empirical results and theory, one important point that was stressed during the conference is that theory can often rationalize procedures that work in practice. I've emphasized this point for years in teaching econometrics.

The work of Cleveland and Tiao, giving conditions for rationalyzing in a mean-square-error-sense use of certain well-known moving average filters for seasonal adjustment, is a beautiful example of current work that has made a great contribution to research and understanding of what is being done in applied seasonal work. The earlier rationalization of moving average filters in terms of deterministic seasonal components, I think, was extremely important, too. Further, Kuiper's work showing that different methods in use produced approximately similar results for the few series that he analyzed for historical periods but different results for recent and current values, perhaps due to the asymmetric filters used for adjusting the current values, is a very intriguing finding reported in his paper. I've been meaning to ask Estella Dagum whether empirical findings of this kind encouraged her to think of the combined X–11 ARIMA procedure described in her comment. It may be that the combined X–11 ARIMA procedure is a very promising approach to deal with the instability of current values. Shiskin mentioned to me that he's ready to experiment with the X–11 ARIMA technique. Furthermore, it was pointed out that the method utilized in the X–11 ARIMA procedure was put forward earlier in theoretical work of Cleveland, Parzen, and others as an optimal procedure. Thus, interaction between theory and application has been very strong in at least these two or three areas that have been featured at the conference.

Another consideration in terms of this interaction between theory and practice is BarOn's and Tukey's emphasis on having an expert on the phenomena being modeled and analyzed present on the scene to help the methodologist do his work. There is no question but that a person who knows the data, the way the data have been generated, the historical setting, the local nonstationarities that may be very important, major events impinging on a series, etc., can have a tremendous impact on the quality of an analysis and also may prevent the analyst from making errors of the first order of magnitude. Furthermore, I am very sympathetic to the point that BarOn made about local nonstationary events that may have a temporary impact on a series. In fact, in stock market work some years ago, one of our bright doctoral students at our school of business analyzed stock price data, taking account of the impact of news events, including such items as wars, presidential illnesses, strikes, etc. He got these items from going through pages of past newspapers, employing content analysis, and then analyzed stock market data following these major news events. Lo and behold, he picked up departures from the random walk model following these major events. Unfortunately, not enough of a departure was found so that one could make money from exploiting it. On the issue of local nonstationarities, I would say that Box responded to this point very knowingly by saying that intervention analysis could be used to handle effects of this kind. This means that use of generalized seasonal ARIMA models in the analysis of particular series would be of great interest to see if there is, indeed, a remarkable improvement in beating down, e.g., the mean-squared error of prediction, by taking account of local nonstationarities or interventions, or whatever you want to call them.

Another type of nonstationarity is provided by Bloomfield's example showing seasonality in the variances of monthlies. This example raises points that are extremely relevant for analyses of seasonal processes, some of which Tiao mentioned that he has encountered in analyzing air-pollution problems where variances were found to be nonstationary. These examples suggest that, instead of just being concerned about the measure of location month by month, one should also be interested in other aspects of the distribution that may vary seasonally, a point also emphasized in the Cleveland-Dunn-Terpenning paper.

Series for which current procedures do not work too well provide, I think, extremely good opportunities for theory to broaden existing models. The work of Durbin, Murphy, and Kenny on mixed models falls in this category. The work on robust, resistant techniques of Cleveland, Dunn, and Terpenning that may take account of outliers in more satisfactory ways reflects concentration of methodological work on areas of difficulty that can be helped by more structured theoretical approaches. As I stated earlier, I think that it is a good strategy to concentrate research effort on areas where difficulties are being experienced and thinking about theoretical approaches that can possibly provide improved procedures.

The next area that we covered was the development of the statistical modeling approach. Here, I think that Granger's emphasis on the need for models not purely deterministic and not purely stochastic was well reflected in the mixed deterministic stochastic models of Pierce, Wallis, and others who indicated that such models have much to recommend them. Pierce's approach is an operational approach that appears very flexible, promising, and generalizable in various dimensions. A second point on the modeling approach, which is very important, is that the decomposition into components can be done utilizing a minimum extraction principle, employed by Box, Hillmer, Tiao, earlier by Parzen, and in the work of Pierce.

Concerning statistical models for seasonal analysis, the Box-Jenkins multiplicative seasonal model certainly facilitated many analyses and is viewed by Barnard and many others as an outstanding contribution. Recently, Julius Shiskin asked me, "What does ARIMA stand for?" I told him autoregressive integrated moving average process, but I really should have told him something that I jotted down here—"all arise in monumental acclamation"—the "word" has arrived. These processes are extremely useful, and you can see some evidence bearing on their predictive performance very clearly in Plosser's plots in which he compared 12-month ahead predictions for each of 10 years with actual outcomes. As he points out, his 12-month ahead predictions had an error that is rarely more than 1–2 percent. However, Lombra pointed out that $1\frac{1}{2}$ percent may not be good enough. The question that then comes up is whether taking account of the innovations that

BarOn mentioned or, perhaps, expanding the models in some way to become mixed models would effect any considerable improvement in predictive ability. This is a whole area of work that was suggested, I think, by the discussions and contributions at the conference. Furthermore, as you may recall, Plosser's analysis showed that, in certain cases, the restrictions required to produce a multiplicative seasonal model may not be implied, in general, by economic models. This raises issues regarding the value of broadening multiplicative seasonal ARIMA models. Will broadened models produce much better results, or will results with multiplicative seasonal ARIMA be satisfactory? Also, Sims' discussion of spectral analysis and its implications for the choice of models is very important. The restrictions we are putting on processes when we opt for multiplicative seasonal ARIMA models should be studied very carefully. It could turn out that they are good enough approximations for many, many purposes and that would be just fine. The simpler the model, in my opinion, the better.

However, some questions arise in connection with this principle. Is the minimal extraction principle, minimizing the variance of the seasonal component, general enough to be applicable to all problems? Does it put something into a series that should not be there? Should the principle be rationalized in terms of subject matter considerations? For example, from a business point of view, is it reasonable to minimize the seasonal variance? Usually minimizing something costs money. You may not get to the minimum value, because it is too costly, i.e., you may stop before you get to the minimum value. Taking account of such considerations would imply a different solution. In a very clear example, given by Pierce, application of the minimum variance principle involved setting a parameter's value equal to minus 1 in order to achieve identification of the components. Whether this a priori restriction on the stochastic process for an economic variable makes economic sense or sense in terms of decisionmaking really should be examined very, very closely. I think Hillmer's remark about the somewhat arbitrary nature of the minimal extraction principle is well worth heeding. Furthermore, some have expressed great interest in having the trend-cycle component be smooth. Is this objective consistent with providing minimal variance for the seasonal component? This issue deserves further study. Closely related is the problem of determining the power of diagnostic checks using residuals to pick up departures from assumed models. This problem and other problems associated with using large sample inference techniques in samples of the sizes with which we usually work are topics that also deserve much further work.

One theoretical development that came out of the modeling, or analytical approach, was the emphasis that Sims, Tukey, Wecker, and others placed on what I will term the "dimple problem." Dimples or dips appear in the spectral density functions at the seasonal frequencies for seasonally adjusted series. Granger, Pierce, and others pointed to this problem as being one that requires further thought. Are the dimples there because a minimum mean-square-error point estimate of the seasonal component was employed? If a broader loss function taking account of smoothness were used, will the dimples still be as prominent? That is, would another criterion that links the seasons and incorporates smoothness considerations reduce or eliminate the dimple effect, or is it something with which we have to live? This is a problem that deserves more theoretical analysis. It also figured importantly in discussions of criteria for good seasonal adjustment.

The areas of multivariate seasonal analysis, considered and presented in the Granger, Box, Hillmer, Tiao, Engle, Plosser, Geweke, and Wallis papers, and the relation of univariate and multivariate seasonal adjustment procedures in connection with aggregation and other problems are only recently opening up and seem, to me, to be of tremendous importance. Results obtained by Geweke indicate, e.g., that multivariate adjustment offers great room for improvement, but, in this connection, however, I think back on multivariate regression and how the number of parameters pile up when you get into a multivariate situation. We really have to keep down the number of parameters, particularly in the covariance structures of error processes and elsewhere. I believe that we have to find good reasons for putting patterns on covariance matrices of error processes. This and other devices can help to keep down the number of parameters in multivariate analyses and lead to better results in multivariate problems.

Regarding the econometric-statistical-causal modeling approach, Engle, Plosser, and Wallis have, in their papers, illustrated the use of causal, structural econometric models in approaching seasonal problems. This approach is still in an experimental stage, in part, because of the tentative nature of econometric models. Engle employed an unobserved component, ARIMA approach. Engineers and others are aware of the fact that one can take the engineers' state variable representation model and convert it to a restricted ARIMA representation. The question is whether the state variable representation model plus the assumptions made about the seasonal components will be flexible enough to be useful, a topic that deserves further research. Concerning Plosser and Wallis, they exhibit the relationship between traditional causal econometric modeling techniques and statistical time series techniques that Palm and I emphasized in our earlier work. Fortunately, we in econometrics have discovered an intimate link between these two areas. Earlier, most econometricians believed that time series analysts were off by themselves, doing something completely different from what econometric modelers have been doing. In the last few years, there has been considerable recognition of the fact that these two areas are very closely related and that interaction between workers in these two areas can be most fruitful.

Regarding the use of seasonally adjusted data in constructing and analyzing econometric models, Plosser, Wallis, and others have exhibited some of the dangers of this procedure. The emphasis in Plosser's and Wallis' papers has been put on the use of seasonally unadjusted data in econometric modeling. Lombra, in his comments, remarked that this amounted to considering seasonal adjustment or seasonal analysis as part of the problem of econometric modeling. I believe that the techniques put forward by Plosser and Wallis will be studied intensively in the years ahead and will prove to be very valuable.

One point about the approach used by Plosser and Wallis, which is very basic and is embodied in earlier work by Palm and myself, is that we try to take a step-by-step approach in the econometric modeling area. First, we attempt to determine the forms of processes for individual variables. They may be found to be in the multiplicative ARIMA seasonal form. These processes are like building blocks. They can be used for certain purposes, namely for prediction and for diagnostic checks of the assumptions built into the structural equations. Then, we have another set of equations to discuss, the transfer functions. They can be used for prediction, control, and diagnostic checking. Thus, when we determine the forms and estimate the transfer functions in this approach to econometric modeling, we have a useful output in that these relationships can be used for prediction and control. Then, it may be that a structural model is obtained that is consistent with the transfer functions and processes for individual variables. If so, you may have some confidence that the structural equations of the model that you estimate are reasonably in agreement with the information in the data.

Another point that has emerged in the discussions at the conference and elsewhere is the following important methodological issue: Many time series workers have identified reasonably simple ARIMA models from the data. Now, some econometric modelbuilders point out that, when you take a large scale econometric model and algebraically derive the processes for individual variables, they should come out to be much more complicated than have been found by the data analysts. My feeling on this issue is that probably the data analysts are right and the econometric models are wrong. In my opinion, the models should have a simple structure that predicts what the data analysts are finding from the data. It was remarked, I think, by Plosser, that the St. Louis model has a very nice recursive structure that will help simplify the ARIMA processes on individual variables. The early pioneering work on monthly models by T. C. Liu had the structural model completely in reduced form, i.e., a complete model in the form of a set of autoregressions with input variables. He had everything in a very simple form from the point of view of structural econometric models. Discussions at the conference and the results in the papers by Plosser and Wallis serve to emphasize further the point that the simple models discovered from the data have to be rationalized in some way, and I think it will come from rethinking the specification of the structural equations of econometric models.

I shall now turn to some prospects for the future. I will mention a few briefly and then you can help me to finish this part of my remarks in the discussion. The list of projects that Shirley Kallek has in her paper are specific projects that need doing. I think it constitutes a fine set of research projects that are worthy of being on the agenda, and I am happy that some of the items were covered at the sessions of the conference, e.g., the problem of aggregation, and some others. Second, on the question of X–11 ARIMA, this procedure will probably come into more widespread use after considerable testing. Naturally, one doesn't want to put anything on line that hasn't been thoroughly tested. Once a procedure is on line, it is necessary to take responsibility for its output. I hope that the X–11 ARIMA procedure will be thoroughly tested, and my prediction is that this modification to X–11 will probably be found very useful. Also, it has an interesting offshoot. To use it, you have to identify a number of ARIMA processes, and that is very, very valuable, because this work will help us understand and appraise the ARIMA processes much more fully. In fact, I propose that we have a handbook of ARIMA processes, similar to handbooks of physical constants. Such a handbook would be very useful from the point of view of those wanting to make predictions, predictions that can be checked against actual outcomes. It will also be useful for those econometricians who want to take a time series approach in building their models. Thus, work with the X–11 ARIMA procedure could have a substantial impact on other parts of the seasonal analysis area, promote more beneficial interaction between theory and practice, and produce more stable current seasonal factors.

The work on evaluation of alternative models for seasonal adjustment and seasonal analysis will continue. Whether we need a broader range of models than the class of ARIMA models is certainly worthy of research. Do we have to go to mixed models rather than use purely stochastic multiplicative models? All of this work, I feel sure, will proceed rather rapidly. The strenuous testing that has to be done before the procedures are adopted for use will constitute a tremendous amount of work, and, in the process, we shall accumulate a lot of valuable research experience.

Finally, I believe that additional work and thought will be directed not only to the theory of the statistical models underlying the observations in terms of their lags and error serial correlation properties but also to the nature of error distributions. As you know, not everything is normal in the world these days. There are many cases in which student-t and other nonnormal distributions of the errors are encountered in practice. Work will probably proceed by deriving traditional likelihood estimates for nonnormal models and comparing these results with robust, resistant estimates. This is a very interesting road that will be traveled.

Furthermore, one has to consider the formalization of how seasonal analyses are going to be used in practice. If you think businessmen are not concerned about seasonals, I refer you to William Wecker, who analyzed prediction problems of department store sales. Department stores have to order huge batches of items, such as girls' dresses, boys' shoes, etc., and they have to set the prices for these items at the beginning of the year. They have to forecast sales, which are highly seasonal, for the rest of the year. If they err seriously in their forecasts, they can lose a lot of money. Their criterion is not MSE; it is something much more practical. I would urge some decision theorists to get into this area and use more practical criteria, such as minimizing expected costs or maximizing expected profits, and combine this decision-theoretic-oriented approach with some of the elements Sargent brought forward in his comments on the contribution of economic theory in enhancing understanding of seasonal problems. There is a lot of work that can be done on the economic theory of seasonal problems. Combined with the data analysis emphasis that Harry Roberts and many others stressed, it appears, to me, that a decision-theoretic modeling approach can lead to fruitful results which will enhance our understanding of seasonal phenomena.

In summary, as is evident from the papers, prepared comments and discussions presented at the conference, rapid progress is being made in the disciplines that impinge on the seasonal analysis area. Coupled with substantial progress within the field of seasonal analysis, represented by these papers, I believe that prospects for the seasonal analysis and adjustment areas in the next few years are very bright.

GENERAL DISCUSSION

John W. Tukey
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As an ex-chemist, I want to point out that when I was learning that trade, the most standard book of physical constants was unofficially known as the *Intentionally Cryptical Tables*. On the cover it said "International Critical Tables," so I'm not sure that you necessarily have a good paradigm to follow.

Raphael Raymond V. BarOn
Israel Ministry of Tourism

First, I'd like to thank both Arnold Zellner and the others for organizing this conference and enabling us to get together. I found this conference, the first since 1960 on a large scale, to be of great importance. There have been sessions of the International Statistical Institute (ISI) dealing with specific aspects of time series analysis and attempting to get experts together at two previous meetings of the ISI in Washington and in Vienna, but this is the first time that so many people and so many papers have been gathered. Now, if I can comment to the Steering Committee meeting following, could it perhaps consider what is the next phase of this time series? With all the information that we have now and all the information that will be obtained in the next year or two, how do we proceed in order to avoid, as far as possible, unnecessary duplication of effort, which is one of the major problems of scientific work today, and maximize the communication and interchange of knowledge and information?

In connection with the present conference, I think some of the papers in which the same series have been analyzed by a number of different techniques have been very revealing, and this kind of workshop/laboratory, where the same specimens involved were analyzed by a number of different techniques, provided valuable insights and information to everybody concerned. I was interested to see that there were a number of people from different countries at the conference, but I am sure that it is only a small subsample of researchers in the seasonal analysis area at this time. I wonder whether it would be possible to organize, perhaps with the International Statistical Institute, another major session, and if we could possibly have the papers in advance of the session.

Herbert M. Kaufman
Arizona State University

I think that before we adjourn, we ought to acknowledge formally the work of Arnold Zellner and the Steering Committee and show our appreciation for this excellent conference.