Volume Title: Seasonal Analysis of Economic Time Series

Volume Author/Editor: Arnold Zellner, ed.

Volume Publisher: NBER

Volume URL: http://www.nber.org/books/zell79-1

Publication Date: 1979

Chapter Title: Retrospect and Prospect

Chapter Author: Arnold Zellner

Chapter URL: http://www.nber.org/chapters/c3909

Chapter pages in book: (p. 449 - 479)

Section VIII.

General Discussion and Summary

RETROSPECT AND PROSPECT

Arnold Zellner
University of Chicago

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 $1^1/_2$ 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
Princeton University

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.

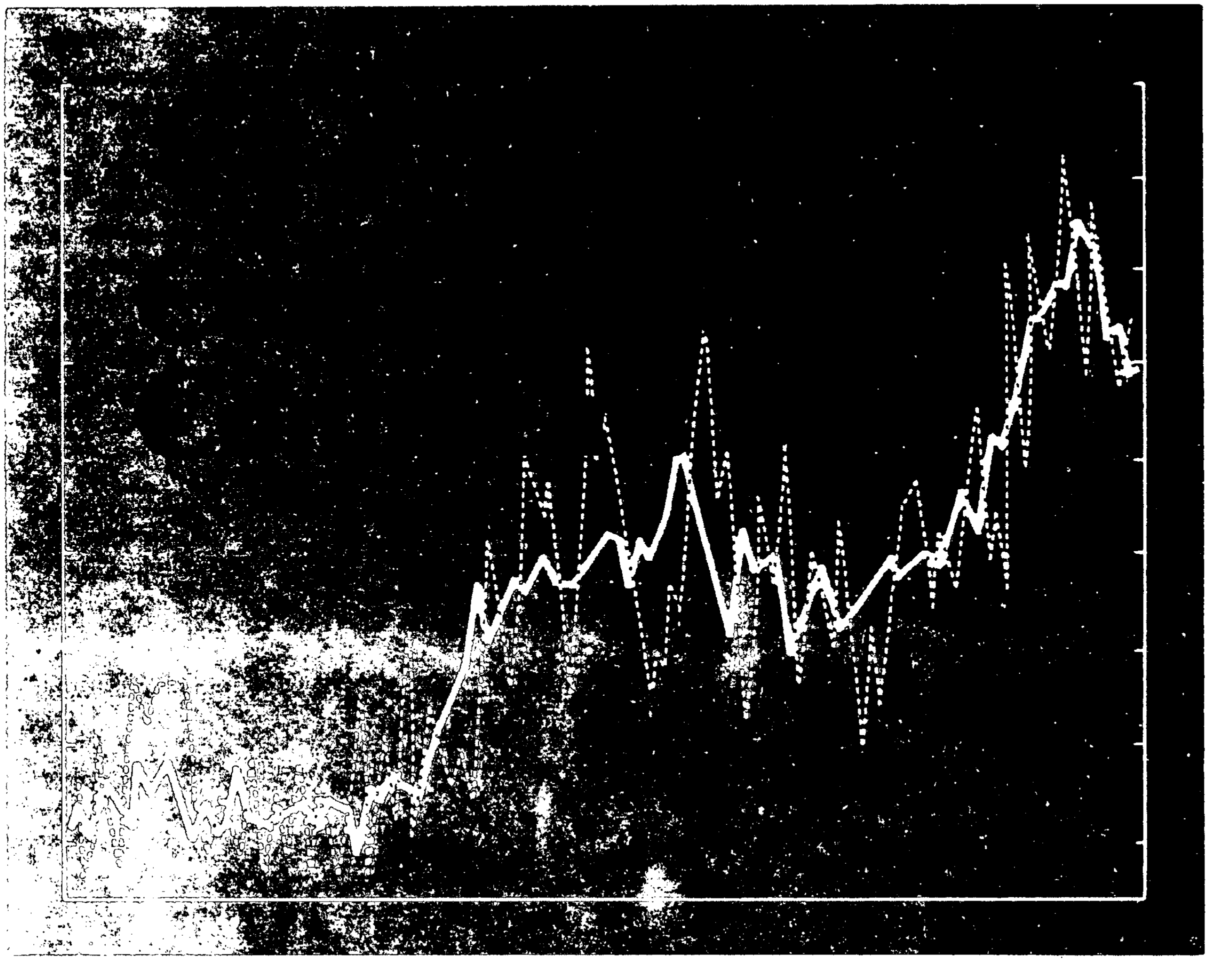
Raphael Raymond V. BarOn
Israel Ministry of Tourism

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.



COMMENTS ON "SEASONAL ADJUSTMENT WHEN BOTH DETERMINISTIC AND STOCHASTIC SEASONALITY ARE PRESENT" BY DAVID A. PIERCE

John P. Burman
Bank of England

This is a very illuminating paper, giving full practical details of seasonal signal extraction, using a particular ARIMA model.

The model $(1, 0, 1)_{12}$ used for the seasonal operator is different from the example in (1). But, both come within the framework of the partial fraction technique, described in the appendix of my discussion on Kuiper. An improvement, suggested by Pierce, is the extraction of any deterministic seasonal component (seasonal mean correction) from the differenced series.

He removes the trend by differencing and a nonseasonal autoregressive filter, instead of including this in a single seasonal ARIMA model; this makes for computational simplicity in finding the minimum of the spectrum of the seasonal component and also the weights of the signal extraction filter. But, if there is any interaction between the seasonal and nonseasonal parts of the model, this may not be the optimal procedure.

Another difference from (1) is that Pierce includes the positive real root of $(1-\Phi B^{12})$ in the seasonal model, although it is usually close to 1 and, thus, generates a spike in the spectrum at zero frequency. For example, for U.S. unemployment, Pierce finds $\Phi=0.547$, and its 12th root is 0.95.

COMMENTS ON "ANALYSIS AND MODELING OF SEASONAL TIME SERIES" BY GEORGE E. P. BOX, STEVEN HILLMER, AND GEORGE C. TIAO

John P. Burman
Bank of England

I found this a most stimulating and original approach to the problem of optimal seasonal adjustment. It combines ARIMA stochastic modeling with signal extraction and illustrates the method by an example. The method is presented in a more compact way, which shows how it can be generalized to include other Box-Jenkins models.

My discussion of Kuiper's paper sets out the partial fraction method of decomposing a seasonal model. Thus, Box, Hillmer, and Tiao's model can be written as follows:

$$\frac{\theta(B)}{\varphi(B)}=\frac{\eta(B)}{d(B)}+\frac{\Psi(B)}{U(B)}$$

where $\varphi(B)=(1-B)(1-B^{12})$

$$\eta(B)=\eta_o-\eta_1 B-\eta_2 B^2$$

$$d(B)=(1-B)^2$$

$$\Psi(B)=\Psi_o-\Psi_1 B---\ -\Psi_{10}B^{10}$$

$$U(B)=1+B+B^2---\ +B^{11}$$

The spectrum of the model$=\sigma_a^2\,\dfrac{\theta(B)\theta(F)}{\varphi(B)\varphi(F)}$

where

$$B=e^{i\omega},\ F=B^{-1} \tag{1}$$

The symmetry makes the operator in (1) a function of terms like $B^n+F^n=2\cos n\omega$, and the latter is expressible as a polynomial of degree n in $x=\cos\omega$. The denominator is a polynomial in x of the same degree as $\varphi(B)$ and can be decomposed into factors of the form $(1-\beta_i x)$, which is equivalent (apart from a numerical factor) to $(1-\alpha_i B)(1-\alpha_i F)$, where α_i is one of the roots of the equation

$$\beta_i=2\alpha_i/(1+\alpha_i^2)$$

Thus, the partial fraction of the function of x leads to a decomposition of the form

$$\frac{\theta(B)\theta(F)}{\varphi(B)\varphi(F)}=d_0+\sum\frac{d_i}{(1-\alpha_i B)(1-\alpha_i F)}$$

with higher powers appearing when there are multiple roots.

For the model used here,

$$\frac{\theta(B)\theta(F)}{\varphi(B)\varphi(F)}=d_0+\frac{d_1}{(1-B)(1-F)}+\frac{d_2}{(1-B)^2(1-F)^2}+\frac{\Psi(B,F)}{U(B)U(F)}$$

$$=\frac{\eta(B,F)}{(1-B)^2(1-F)^2}+\frac{\Psi(B,F)}{U(B)U(F)}$$

where $\Psi(B,F)$ is a symmetric function of degree 10 and $\eta(B,F)$ is a symmetric function of degree 2 in B and F. If d_t and b_t are independent white noises, with $\sigma_d^2=\sigma_b^2=\sigma_a^2$, an equation equivalent to Box et al.'s (47) is produced. The numerator $\Psi(B,F)$ can, in principle, be factorised into $\Psi(B)\Psi(F)$, via a polynomial in cos ω; but, some roots of this may be inside the unit circle, in which case, $\Psi(B,F)$ could be negative and the solution unacceptable. However, with the (0, 1, 1) $(0,1,1)_{12}$ model, this does not occur in practice.

The minimum of the seasonal spectrum is given by

$$\epsilon^*=\min_{|B|=1}\left[\frac{\Psi(B,F)}{U(B)U(F)}\right]$$

$$=\min_{|B|=1}\left[\frac{\theta(B)\theta(F)}{\varphi(B)\varphi(F)}-\frac{\eta(B,F)}{d(B)d(F)}\right] \tag{2}$$

With an obvious modification of the paper's notation,

$$\sigma_b^2\cdot\frac{\Psi_\cdot(B,F)}{U(B)U(F)}=\sigma_a^2\left\{\frac{\Psi(B,F)}{U(B)U(F)}-\epsilon^*\right\}$$

Hence

$$\sigma_d^2\cdot\frac{\eta_\cdot(B,F)}{d(B)d(F)}=\sigma_a^2\left\{\frac{\eta(B,F)}{d(B)d(F)}+\epsilon^*\right\}$$

is the spectrum of the optimal seasonally adjusted series. These equations together are equivalent to (49) and (50). The optimum solution has the troughs of the spectrum of the seasonal component as deep as possible—thus, minimising the loss of power of the spectrum of the adjusted series at interseasonal frequencies.

The extraction filter for seasonal adjustment is given by

$$h(B)=\frac{\sigma_{a}^{2}\cdot}{\sigma_a^2}\frac{\eta_{\cdot}(B,F)}{d(B)d(F)}\cdot\frac{\varphi(B)\varphi(F)}{\theta(B)\theta(F)}$$

$$=\frac{\sigma_{a}^{2}\cdot}{\sigma_a^2}\frac{\eta_{\cdot}(B,F)U(B)U(F)}{\theta(B)\theta(F)}$$

$$=\frac{\{\eta(B,F)+\epsilon^{*}d(B)d(F)\}U(B)U(F)}{\theta(B)\theta(F)}$$

Note that factorisation of $\eta_{\cdot}(B,F)$ into $\eta_{\cdot}(B)\eta_{\cdot}(F)$ is required for the elegant method of expansion of the filter, described in the appendix to Box, Hillmer, and Tiao's paper. However, it might be simpler to modify the expansion of $\frac{\varphi(B,F)}{\eta(B)\eta(F)}$ (in the Box et al. appendix notation). (A-1) becomes $\varphi(B,F)=\eta(B)C(B,F)$, and coefficients of $C(B,F)$ can be derived recursively, starting with the leading term F^r (r being the degree of the polynomials). (A-4) becomes

$$C(B,F)=\eta(F)X(B,F)$$

and the first $(r+1)$ coefficients of $X(B,F)$ can be obtained from a set of $(r+1)$ linear equations like (A-5).

Numerical estimation of (2) shows minima, as expected, at $\omega=0$ and close to $(j+1/2)\pi/6$ [$j=1, 2---5$].

The results are in tables 1 and 2. The lowest minimum is always small and positive; for low values of θ, it is at the right-hand end, but increasing with θ; and for high values of θ it is at zero, and decreasing with increasing θ. The results for the model ($\theta=0$, $\Theta=0.75$) agree closely with those given in the paper—though, of course, ϵ^* is different, because we have started with a different acceptable model.

Table 1. **VALUE OF LOWEST MINIMUM ϵ^***

θ =	Θ = 0.5	0.75	0.9
0	0.01	0.003	0.0004
0.3	.02	.006	.001
0.6	.036	.009	.002
0.9	.02	.005	.0008

Table 2. **POSITION OF LOWEST MINIMUM**

(ω as a multiple of $\pi/6$)

0	5.50	5.50	5.50
0.3	5.50	5.50	5.50
0.6	0	0	0
0.9	0	0	0

COMMENTS ON "A SURVEY AND COMPARATIVE ANALYSIS OF VARIOUS METHODS OF SEASONAL ADJUSTMENT" BY JOHN KUIPER

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INTRODUCTION

One method of seasonal adjustment, examined by Kuiper in his paper, is X–11 ARIMA, now being employed by Statistics Canada on labor force data.[1] This intuitively appealing method, described in the following, has not been examined at the Board of Governors until now. Our comment evaluates the performance of this method of seasonal adjustment for one time series of particular interest—monthly observations on the narrowly defined money stock, M1.

The difficulty with X–11, or with any other method that employs symmetric moving averages, is that symmetry cannot be preserved at the end points of the sample of data. For example, X–11 estimates trends with a 12-month moving average and also smooths the seasonal component across years with a 3×5-moving average. Thus, symmetry is lost for data within 3 years of the end of the sample. Instead, asymmetric filters are applied, resulting in phase shifts in the adjusted data. What the X–11 ARIMA approach does is to provide X–11 with an augmented sample of data so that all, or most, of the actual data are smoothed with symmetric averages. ARIMA models are, of course, employed in generating the extra observations. The choice of an ARIMA model to generate forecasts is merely one of convenience. Structural models that incorporate seasonality could also be used, although the distinction between these two approaches is somewhat artificial. Under most conditions, there exists a correspondence between the structural and time series representations of an endogenous variable.[2]

We have selected M1 as an example of a series having current seasonally adjusted levels that receive a great deal of attention from the public and press. In addition, these data are used as an input to policy decisions by the Federal Open Market Committee (FOMC). Furthermore, it is well known that these data, as first published by the Board of Governors, Federal Reserve System, often revise substantially as the seasonal factors are reevaluated in light of additional data.[3] Seasonal factors reestimated with several additional years of data lead to a much smoother series than that derived from the first published factors.[4] Accepting these later estimates as correct implies that the current seasonally adjusted data are not providing policymakers with good information about short-run movements in the money stock. In this comment, the major emphasis is on seasonal factors in the current year and those projected for the following year.

There are serious problems in the application of any seasonal adjustment method to the money stock. For example, since M1 is composed of currency and demand deposits, having structural equations that would be specified differently, we probably should use a multivariate approach.[5] Furthermore, for a series that is at least partially controllable, the policymaker's reaction function must be introduced before we can begin to make any meaningful statements about seasonality in these data. Investigation of these issues is clearly beyond the scope of this comment. Instead, we assume that seasonal factors obtained with X–11 from the interior of a data sample are correct seasonal factors.

In the next section monthly ARIMA models for the currency and demand deposit components of M1 are identified. These models are then used to generate forecasts with 1-, 2-, and 3-year horizons. In the third section, seasonal factor estimates based on samples, augmented with forecasts, are compared to those obtained without the forecasts.

THE MODELS

The current Board of Governors' staff procedure is to seasonally adjust the currency and demand deposit components of M1 individually and then to add them together

[1] See [3].

[2] See [8].

[3] The current practice is to reestimate seasonal factors for M1 once a year. Some judgmental review does take place before publication. For a description of this process, see [7].

[4] See [2].

[5] See [4].

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to get seasonally adjusted M1. For each component, we find that first and seasonal differencing are necessary for stationarity but that a logarithmic transformation is not required. Integrated moving average models of the following general form are then estimated:[6]

$$\nabla_{12}\nabla_1 x_t = \theta(B) a_t \tag{1}$$

where x_t is either the currency or demand deposit component of M1, ∇_1 is the first difference operator ($\nabla_1 x_t = x_t - x_{t-1}$), ∇_{12} is the seasonal difference operator ($\nabla_{12} x_t = x_t - x_{t-12}$), $\theta(B)$ is a polynomial in the backshift operator, $B(B^k a_t = a_{t-k})$, and a_t is white noise. The first estimation period for the currency component of M1 is from July 1953 through June 1965 and for the demand deposit component, from July 1950 through June 1965. These equations are then reestimated, changing the specification slightly, by "rolling up" the sample—adding a year at the end and dropping a year at the beginning. This process is continued nine times until June 1973, so that 3 years of actual data are left outside the last estimation sample.[7] Each equation is then used to generate a 36-month forecast. In general, this forecast reproduces the seasonal pattern quite well, although the level of the forecast after 36 months is often quite different from the actual level.

RESULTS

In assessing the results of the X−11 ARIMA method, we ask the following question: Do the conclusions reached by Statistics Canada with respect to their labor force data, namely increased stability of current and forecasted seasonal factors, also hold for the U.S. money stock? To answer it, we employ a measure of seasonal factor stability, used by Dagum and Kuiper, which will be described. Following Dagum [3] final seasonal factors are those from X−11 when there are available 3 additional years of data. For ordinary X−11, the current-year seasonal factors are just those for the last year in the sample, but we compute current seasonal factors in three additional ways—by augmenting the sample with 1, 2, and 3 years of forecasted data.[8] Recalling that the end point of the first sample of actual data is June 1965 and that this sample is "rolled up" nine times to reach June 1973, there are now 108 (9×12) observations on final and current seasonal factors. Note that there are four sets of current seasonal factors—one from ordinary X−11, one from X−11 ARIMA (1 year), one from X−11 ARIMA (2 years), and one from X−11 ARIMA (3 years)—all referring to the same months and years. The Dagum-Kuiper measure of stability is

$$\frac{1}{12}\sum_{m=1}^{12}\frac{1}{9}\sum_{k=1}^{9}\left|S_{m,k}^{final}-S_{m,k}\right| \tag{2}$$

where S denotes a seasonal factor—either current or forecasted, and the subscripts m and k denote month and year, respectively. The lower this statistic is, the less the current or forecasted seasonal factors are revising.[9]

In addition to current factors, policymakers are interested in forecasted seasonal factors, usually 1 year ahead. In fact, first-published data are seasonally adjusted with forecasted seasonal factors, because X−11 is not rerun until 12 new observations are obtained. These forecasted factors are generated by X−11 as

$$S_{m,k+1} = S_{m,k} + 1/2\,(S_{m,k} - S_{m,k-1}),\ m = 1, 2, \ldots, 12 \tag{3}$$

In practice, consecutive differences between seasonal factor estimates for a month are small so that these forecasts are essentially equal to the current factors. One-year-ahead forecasted seasonal factors for the X−11 ARIMA method are simply taken as end-year, next-to-end-year, or third-from-end-year factors in each of the augmented samples.

The results appear in the table. The first row presents the Dagum-Kuiper statistic computed on the X−11 method for the currency and demand deposit components of M1 for both the current and forecasted (1-year-ahead) seasonal factors. The next three rows present these same measures for the X−11 ARIMA method with 1-, 2-, and 3-year forecast horizons. Looking down the columns for current factors, we see that most of the improvement in stability comes from augmenting the sample with just 1 year of data. While the factors for currency are more stable than those for demand deposits, the absolute reduction in the measure of stability is roughly the same for each component.

The table also illustrates the difficulty of obtaining good forecasts of seasonal factors. For demand deposits, the stability measure jumps by one-third for all seasonal factor forecasts. The situation is slightly worse for currency, where seasonal factor forecasts are half again as unstable as current factors in all cases. As we read down the columns for forecasted factors, there are overall gains in stability, for both demand deposits and currency, of 20–25 percent, but they occur at different forecast horizons. These results suggest that significant improvements may be had for demand deposits by using X−11 ARIMA (3 years), but, for currency, a 1- or 2-year horizon is best.

[6]For a discussion of ARIMA model fitting, see [1, especially chs. 6–9].

[7]Coefficient estimates and summary statistics for these models are available on request.

[8]In all of these adjustments, the total sample size is restricted to 10 years. Options, in effect, are: Standard multiplicative run, with 1.5- to 2.5-sigma range for graduation of extremes, 9-term Henderson average for the trend cycle, 3×3- and 3×5-moving average smoothing of seasonal irregular ratios, and no preliminary trading-day adjustments.

[9]Compared to other criteria, such as root-mean square revision, this statistic does not give as much weight to large revisions. Since policymakers are probably more sensitive to large, rather than small, revisions in published data, we computed root-mean square revisions as well, with no qualitative change in the results of the table.

Table 1. **SEASONAL FACTOR STABILITY: RESULTS OF THE DAGUM-KUIPER STATISTICS FOR CURRENT AND FORCASTED FACTORS, BY PERCENT**

Model	Demand component		Currency component	
	Current	Forecasted	Current	Forecasted
X-11......	0.174	0.231	0.091	0.131
X-11 ARIMA (1 year)...	.155	.208	.068	.105
X-11 ARIMA (2 years) ..	.149	.202	.076	.104
X-11 ARIMA (3 years) ..	.142	.193	.072	.114

CONCLUSIONS

This exercise has shown that, on the average, increased stability of current and forecasted seasonal factors is to be derived from using X–11 ARIMA, rather than ordinary X–11, to seasonally adjust U.S. money stock data. However, there are a number of points to consider before adopting the X–11 ARIMA procedure. First, the method is not fully automatic—an important consideration for an agency that must seasonally adjust hundreds of series. An ARIMA model for the series must be obtained, usually with a substantial investment of time for specifying, fitting, and testing. Second, the model chosen must provide good forecasts of the series. Forecast accuracy is needed so that X–11 is operating on a series that is consistent in terms of its seasonal pattern. For an analysis ex post facto, there is no problem, since forecasting performance may be checked with actual data; but, for use ex ante, there are no actual data against which to test the forecasts. One must rely on goodness of fit within sample or on a judgmental assessment of the forecasts as reasonable. Third, the gain in seasonal factor stability (i.e., the amount of revision) should be balanced against the cost of achieving it. For instance, the greatest improvement in the table for current factors for demand deposits comes from using X–11 ARIMA with 3 years of forecasts. The difference versus ordinary X–11 is 0.032 percent. This means that, for a level of demand deposits of $230 billion, the numbers adjusted by X–11 ARIMA are, on the average, $74 million closer to the final numbers than are those adjusted by ordinary X–11. In terms of levels, this average improvement is not overwhelming. However, the average is somewhat misleading, since improvements up to 0.50 percent, or $1.2 billion, occur for particular months.

In conclusion, the X–11 ARIMA approach is to be recommended for those series for which reasonable ARIMA models can be built and where the gain in stability justifies the expenditure of resources. (Quite often, such models will already have been estimated for other purposes.) For series that are highly visible economic indicators and where small changes assume political significance, any gain in stability is probably worth the effort needed to achieve it. X–11 ARIMA is also to be recommended to individual researchers who want to seasonally adjust relatively few series, while avoiding some of the asymmetries implicit in X–11.

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COMMENTS ON "A SURVEY AND COMPARATIVE ANALYSIS OF VARIOUS METHODS OF SEASONAL ADJUSTMENT" BY JOHN KUIPER

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On the outset, I would like to emphasize that I found Kuiper's paper quite interesting and useful. This is particularly true, because practitioners need guidelines since so many methods are available. It was for this very reason that, 5 years ago, we, at the special studies section of De Nederlandsche Bank (i.e., the Dutch central bank), did a similar study, as Kuiper has presented now. I believe Kuiper's work is in the same spirit and follows the same methodology as we applied. We compared nine different methods, including the methods Kuiper compared.[1] However, keeping in mind our own results, I cannot believe Kuiper's main finding. This seems to be that it is not possible to discriminate between different methods of seasonal adjustment.

Our analysis, based on five representative Dutch series, employing the same criteria as Kuiper used, did suggest that the X–11 and the Burman methods perform well. This was particularly so, because these two methods produce stable seasonals. Stability, in this context, means that the seasonals did not change very drastically when new data became available. To study this property, it is useful to add, to a particular series, observations over 12 months successively over a reasonable number of years. (We took 5 years.) I think Kuiper did not follow this procedure quite well. Therefore, his remark that significant differences occurred for the recent period seems, to me, unjustified.

Finally, I would like to add that, for an analysis to employ the additive or multiplicative model, the search procedure, referred to by Durbin and Kenny [1], which, incidentally, is quite common in practice, is more appropriate and simpler than Kuiper's strategy on this point.

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COMMENTS ON "ANALYSIS AND MODELING OF SEASONAL TIME SERIES" BY GEORGE E. P. BOX, STEVEN HILLMER, AND GEORGE C. TIAO

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IDENTIFICATION

When decomposing an observed time series into unobserved components, it is well known that underidentification may be a problem. Let $[T_t]$, the observed time series, be the outcome of the process

$$\rho_p(B)T_t=\eta_q(B)d_t \qquad (1)$$

and let π_t and e_t denote the two unobserved components, so that

$$T_t=\pi_t+e_t \qquad (2)$$

where e_t is white noise. Thus, the ARMA process generating π_t is of the form

$$\rho_p(B)\pi_t=\alpha_{q^*}(B)c_t \qquad (3)$$

where $q^*\leq$ max (p, q). Model (3) is not uniquely determined from (1) and (2). In order to achieve identification of (3), Box, Hillmer, and Tiao (BHT) assume q^*=max (p, q), and select the model for which σ_e^2 is maximum [1]. In this note, we mention some situations where the appropriateness of these assumptions may be questioned and where the BHT procedure may lead to nonparsimonious specifications.

DECOMPOSITION

In an effort to estimate the permanent and transitory component of M1 [2], a similar decomposition was used. The AR polynomial in (1), $\rho_p(B)$, was estimated as $(1-\rho B)\Delta_{13}\Delta_{52}$.[1] In investigating the MA specification, it was found that the MA polynomial $\eta(B)$ in (1) could be explained simply by the noise-induced moving average $\rho(B)e_t$, resulting from substituting the model (3) for π_t into (2). Quite nicely, the sample ACF of $[T_t]$ could be explained by the specification

$$(1-\rho B)\,\xi_t=c_t$$

where $\xi_t=\Delta_{13}\Delta_{52}\pi_t$, together with (2). Thus, a decomposition, such as (2), allowed a fairly parsimonious model to explain a fairly complicated autocorrelation structure. Also, as in this example, p=66, q^*=1, identification of the model did not require any additional assumption concerning the variance of the noise, because the lemma 1 (from [2]) could be applied.

Lemma 1

Let the zeroes of $\alpha(B)$ lie on or outside the unit circle. Then, model (3) is identified iff $p>q^*$. When $p>q^*+1$ the model is overidentified.

Thus, an empirical type of consideration may lead to a different smoothing strategy in which a more parsimonious overidentified model is obtained. Of course, one cannot expect all empirical applications to explain the observed MA polynomial by the noise-induced one. Yet, even when this simplification is not possible, an exactly identified model with $p=q+1$ can always be found. Since ARMA $(p, p-1)$ models can be rationalized as the discrete time representation of continuous processes, lemma 2 can be easily proven.

Lemma 2

Let the zeroes of $\alpha(B)$ lie on or outside the unit circle. There is one, and only one, model (3) that satisfies (1) and (2), for which there is an underlying continuous stationary stochastic process.

Thus, a model-based consideration (continuity) leads to an alternative smoothing strategy, where the assumption σ_e^2=maximum is substituted by the assumption $q^*=p-1$ (i.e., α_p=0).[2]

[1] BHT apply their smoothing procedure to a trend-seasonal-irregular decomposition. But the permanent transitory decomposition does not require any seasonal extraction.

[2] The CPI example, considered by BHT can certainly be represented by the model $\Delta\pi_t=(1-\alpha B)c_t$, with $\alpha=-1$ and σ_e^2=0.00161. But, it is also perfectly consistent with $\Delta\pi_t=c_t$ and σ_e^2=0.00159.

The views expressed are not necessarily related to those of the Federal Reserve System. I would like to express my gratitude to David A. Pierce.

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COMMENTS ON "ANALYSIS AND MODELING OF SEASONAL TIME SERIES" BY GEORGE E. P. BOX, STEPHEN HILLMER, AND GEORGE C. TIAO

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As Box, Tiao, and Hillmer point out, models of the form

$$\Psi(B^s)\Theta(B)X=\delta(B^s)\gamma(B)\epsilon \qquad (1)$$

with ϵ white noise and Ψ, Θ, δ and γ low-order polynomials in their arguments, are successful in producing forecasting models of a wide range of time series, including seasonal components with period s.

This is all the more remarkable a finding since, when we examine these models in the frequency domain, this class of models implies some restrictions on the properties of X, which rule out a large part of all the stationary processes displaying Granger's "property S".[1]

In particular, the spectral density of X is given by

$$S_x(w)=\left|\frac{\delta(e^{-siw})\gamma(e^{-iw})}{\Psi(e^{-siw})\Theta(e^{-iw})}\right|^2\sigma_\epsilon^2 \qquad (2)$$

Setting

$$H(w)=\left|\frac{\delta(e^{-siw})}{\Psi(e^{-siw})}\right|^2$$

$$G(w)=\left|\frac{\gamma(e^{-iw})}{\Theta(e^{-iw})}\right|^2\sigma^2$$

we have

$$S_x(w)=H(w)G(w)$$

Now, H is periodic with period $2\pi/s$, while G, if γ and Θ are of as low an order as usual, is smooth. Thus, $\log S_x$ is the sum of an exactly periodic function and a smooth function. If $\log S_x$ has sharp narrow peaks at the seasonal frequencies and if S_x has the form (2), then the peaks are all of exactly the same height and width. That the peaks be all the same height rules out the possibility that the annual seasonal pattern of the series be consistently smooth or consistently rough. That the peaks be all the same width rules out the possibility that some frequencies in the seasonal pattern might be less stable than others, e.g., that the monthly first difference of the seasonal pattern might show more tendency to change from year to year than 3-month moving averages of the seasonal pattern.

This limitation on spectral densities of the form (2) does not mean that models of the form (1) will yield poor predictions. In fact, the property S can be interpreted in the time domain as asserting simply that there is a slowly changing annual pattern to a component of the series. Hence, any forecasting scheme that estimates the average annual pattern over the past several years and projects that average pattern into the future with little change will produce forecasts which are good in an absolute sense. Differences between forecasts based on the model (1) and those based on the correct model, when the series has the property S but is not well represented in the form (1), will show up mainly in differences in the accuracy with which changes in the seasonal pattern (by construction, small to start with) are forecast. There are some applications where this could be important, especially where we have a structural model of relations between seasonal patterns in different series and wish to extract seasonal components accurately to study their interrelations.

For example,

$$X_t=1.71473\,X_{t-1}+0.9801\,X_{t-2}+\epsilon_t$$

$$=2(0.99)\cos\frac{\pi}{6}X_{t-1}+(0.99)^2X_{t-2}+\epsilon_t$$

This process consists of a strong 12-period seasonal and very little else. However, it has a peak only at $\frac{\pi}{6}$, thus, the seasonal pattern is purely a sinusoid of period 12. Obviously this seasonal is better forecast from its own recent past than from its value 12 months ago.

An interesting open question is whether there is any convenient way to expand the class of models considered by Box, Tiao, and Hillmer to avoid these restrictions.

Obviously, the frequency-domain methods of seasonal decomposition and forecasting used by Geweke[2] are not subject to the limitations discussed here. They may, in turn, of course, be limited by difficulty in handling extremely sharp seasonal peaks properly.

[1] See [1].

[2] See [2].

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COMMENTS ON "ANALYSIS AND MODELING OF SEASONAL TIME SERIES" BY GEORGE E.P. BOX, STEVEN HILLMER, AND GEORGE C. TIAO

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Two of the conference papers contain an empirical example based on the same data set (one made available by the organizers), and it is of interest to compare the results of Box, Hillmer, and Tiao (BHT) with my own results for the manufacturers' shipments, inventories, and orders series, seasonally unadjusted. An initial handicap is that BHT work with the logarithms of the variables, whereas I do not, being influenced by the consideration that a full model describing the determination of these and other relevant variables would contain the identities given in the first paragraph of the section on manufacturers' shipments, inventories, and orders in my paper, "Seasonal Adjustment and Multiple Time Series Analysis," and, hence, would be easier to handle by linear methods if cast in terms of the untransformed variables. Despite this apparent lack of comparability, some similarities between the two sets of results do emerge.

First, in the univariate analyses, similarities in the models for the shipments and new orders series can be observed. Both BHT and I choose $d=D=1$, and once a strong seasonal autocorrelation has been accommodated, relatively little remains to be modeled: The comparison is closest if the MA representations of seasonality, given in footnote 1 of my paper, are considered. For the inventories series the choice of model seems less clearcut, and there is less agreement: BHT, again, choose $d=D=1$ and estimate a nonseasonal AR component of $(1-0.85L)$, whereas I am ambivalent between $d=1$ and $d=2$, with $D=0$ in each case, but, nevertheless, the results with $d=2$ yield a seasonal AR operator that contains the factor $(1-0.78L^{12})$.

In their multivariate analyses, BHT estimate what econometricians term "reduced forms," but the univariate models can still be regarded as (final equation) solutions of this system, and appropriate comparisons can be made. For this purpose, I consider the second, restricted set of estimates. (It should be noted that, in the first set of estimates, the matrix $\hat{\Phi}$ has a root outside the unit circle, suggesting that these estimates were not obtained by an exact maximum likelihood procedure.) The estimated autoregressive matrix is triangular, and the equation for I_t can be immediately separated and written as

$$(1-0.90L)\Delta\Delta_{12}\log I_t=(1-0.40L)(1-0.75L^{12})a_{3t}$$

which is close to BHT's univariate estimate. For the new orders series, the solution is

$$(1-0.97L)(1-0.90L)\Delta_{12}\log NO_t$$
$$=(1-0.36L)(1-0.90L)(1-0.74L^{12})a_{2t}$$
$$-0.345L^2(1-0.75L^{12})a_{3t}$$

and, if the contribtuion of a_{3t} to the right-hand side is ignored (its variance is but $1^1/_2$ percent of that of a_{2t}), then cancellation and slight simplification yields

$$\Delta\Delta_{12}\log NO_t=(1-0.36L)(1-0.74L^{12})a_{2t}$$

which, again, corresponds well with their univariate results. In the residual covariance matrix, the most striking feature is the strong correlation between the shipments and new orders errors—our two estimates virtually agree on a coefficient of 0.7, though it must be admitted that my estimate is based on a slightly different multiple time-series representation. The small correlation of the inventories residuals with the shipments and orders residuals is noted by BHT and is also present in my own results: This, together with the decomposability of their reduced form model leads BHT to suggest that the inventories series behaves independently of the other two series. However, an economist's structural model would no doubt include the direct relationship between shipments and inventories, referred to previously, and, while it is true that the failure of my final form representation to pass the test of a common autoregression could be due to decomposability, inspection of the coefficients does not support the view that the inventories series is causally prior.

COMMENTS ON THE NBER-CENSUS CONFERENCE ON SEASONAL ANALYSIS OF ECONOMIC TIME SERIES

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I would like to comment on several points concerning the X–11 method of seasonal adjustment.

The statement concerning trading-day variation in "An Overview of the Objectives and Framework of Seasonal Adjustment," by Shirley Kallek, views the subject too narrowly. The statement implies that reasonable empirical daily weights which express the percent of the week's activity that occurs on each day of the week are preferred to fitted weights, computed by the trading-day adjustment option in X–11. The research that provided the basis for the trading-day adjustment option, however, indicated that fitted weights perform better within and beyond the sample period than do reasonable empirical weights. It should be noted that, among other things, the research utilized daily retail sales in obtaining empirical weights—a better basis for such weights than would usually be available. The reason for the superior performance of fitted weights is that a substantial proportion of economic activity occurs on the basis of monthly plans and schedules that are drawn up with little or no attention to the number of trading days in calendar months and/or is recorded and reported on a basis that takes little account of the number of trading days in calendar months. Therefore, allowance must be made for the relation of each day of the week to the monthly volume of activity, rather than for the relation of the daily activity to the weekly volume of activity. The relation of each day of the week to the monthly volume can vary with the calendar composition of the month, while the actual daily rates that are proportions of the weekly volume remain fixed. Thus, Saturday and/or Sunday can be assigned substantial weight, even when simple observation indicates the activity is shut down. (See [3] for a further discussion of this point.)

In his discussion of the "Overview," Lawrence Klein was concerned about the introduction of the Slutsky-Yule effect in the seasonally adjusted series because of iteration in X–11. Iteration is used in X–11 to improve the identification of extreme values and the measurement of the trend cycle. There are three rounds of two iterations, each, as follows:

1. A, B
2. A, B
3. A, B

Iteration A is based on the 12-month moving average; iteration B is based on the Henderson curve. The purpose of rounds 1 and 2 is to find extreme values. The only effect carried over from round 2 to round 3 is the modification of extremes in the original series. Durbin [2] showed that iteration B has no effect on a stable seasonal factor, obtained as an average over all years, except for an end-point correction. Young [4] showed that iteration B has only a small effect on the moving seasonal factor in X–11. Thus, it does not seem that the Slutsky-Yule effect plays much role in the seasonal factors, although I am not aware that anyone has fully examined possible effects arising from the treatment of extreme values.

It is important that the record be straight concerning a point in the "Overview," which was commented on by Klein. The "Overview" suggests that statistical agencies, such as the Census Bureau, apply the seasonal adjustment procedure in such a way that the impact of strikes on the data is removed. That is not the case. The statement in the "Overview" pertains to the X–11 strike adjustment option that is available for improving the estimate of the trend cycle in strike periods. With either that option or the standard option, the impact of the strike remains in the seasonally adjusted data.

There are several statements by the authors and discussants that X–11 has been shown to be a fairly good approximation to an ARIMA model for some economic series, but not for others. These statements are based on work most recently reported in an article by Cleveland and Tiao [1]. In that article, X–11 is described as performing creditably on an airline passenger series and is compared with an ARIMA model that was fit to the series. However, X–11 performed poorly on a telephone disconnection series for which a different ARIMA model is appropriate. The poor performance is based on a misspecification of X–11 for the telephone data. The telephone disconnection series contains substantial trading-day variation. This is revealed by the autocorrelation structure of the irregulars, as shown in chart F of the article by Cleveland and Tiao. The autocorrelation structure corresponds closely with that of trading-day factors, based on equal activity for Monday–Friday, with near zero activity on Saturday and Sunday. If the trading-day option had been used, the autocorrelation structure of the irregulars would not have shown this pattern, and X–11 probably would have performed about as well on the telephone data as on the airline data.

Julius Shiskin indicated, in the keynote address, that several variants of method II have served as the official method of seasonal adjustment. It may be helpful to describe how these variants differed with respect to the calculation of the current and year-ahead seasonal factors. The seasonal factors in the X-3 variant reduced the size of revisions in many series by roughly one-third, compared to the original specification of method II. This reduction resulted from a change in the procedure for extending the SI ratios for a given month to the 3 years beyond the end of the series in order that the 3×5-moving average could be computed to the end of the series. The original procedure used an average of the SI ratios for the last 2 years as an estimate of the ratios for the next 3 years. This was replaced in X-3 with an average of the SI ratios for the last 4 years. The X-3 technique was retained in X-9 and is the standard option in X-11. The X-10 variant, which was developed in cooperation with the OECD, was never the official program in the United States. It fit a different curve to each month, ranging from a very short moving average to a stable seasonal, depending on a signal-to-noise ratio. This option is available in X-11 and, if used intelligently, can lead to reduced revisions for some series. To the best of my knowledge, the only major use of this option by the U.S. statistical agencies is for the import and export series adjusted by the Census Bureau. For these series, a 3×9-moving average is used for each month in place of the 3×5-moving average.

The X-11 ARIMA variant, developed by Statistics Canada, is another approach to tailoring the procedure for obtaining current seasonal adjustment factors to the series. The reduction in revisions, reported at the conference, of about one-fifth for X-11 ARIMA is important news. One hopes that the statistical agencies will follow up and test the method on U.S. series. As with X-10, the ARIMA variant requires considerable skill and judgment. One possibility, which should not be overlooked, is that tests with X-11 ARIMA may indicate that much of the possible improvement could be captured with a very limited number of suboptimal modifications of the X-11 weights. If so, this would facilitate the uniform application of the method on a large scale.

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