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KEYNOTE ADDRESS: SEASONAL ADJUSTMENT OF SENSITIVE INDICATORS

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It seems appropriate on this occasion, since we have just spent the morning discussing highly technical subject matter, to try to keep the tone of my remarks somewhat light—and that is what I intend to do. It is also appropriate to begin by thanking the Census Bureau and the National Bureau of Economic Research for organizing this conference. As far as specific individuals are concerned, we, obviously, must thank Dr. Arnold Zellner, who is chairman of the Steering Committee, and Dr. Gary Fromm, who represents the National Bureau of Economic Research. I also have some words of high praise for Shirley Kallek, but I have decided to save them until the end, because she is keeping an eye on my time. If she cuts me off, those words of praise will be missing from my speech.

I would like to begin with a short, informal review of the computer methods used in making seasonal adjustments, at least so far as my participation was concerned, before I address some of the problems that were put before us this morning.

When the early work in the area of seasonal adjustments was going on, most of you were unaware of it; perhaps some of you were not even born. This is only the second major conference on seasonal adjustments since computer methods were developed. The first conference was held in Paris at the beginning of the 1960's and was sponsored by the Organization for European Cooperation and Development. Angus Maddison, a staff economist there, who is attending this conference, was a prime mover in organizing that conference.

When I tried to introduce computer–performed seasonal adjustments at the Census Bureau, I was a very lonely man. Few people paid any attention to me, other than my immediate supervisor who continually suggested that I do something else. But, one day, Angus visited me, because he had heard of my efforts at the Census Bureau and wished to apply my ideas to European data series. OECD had discovered that few of the European series were seasonally adjusted and that they needed a quick and accurate method of accomplishing this adjustment.

I remember sitting at a table with him going over the method we had developed and afterwards making arrangements for use to perform the OECD seasonal adjustment work at the Bureau. Years later, when OECD got a computer, they took over the task themselves. I should mention, in this context, another person who had a major role in this area and that is Donald J. Daly of the Department of Industry and Commerce in Canada. He was the person who, through us, brought about the wide-spread use of seasonal adjustments to Canadian data. I am very happy to see that the Canadian work has been expanded since those early days.

A comment about the agenda: It reveals a broad range of approaches, and I think that is very constructive. It is what we need at this time. We needed it earlier, but better late than never.

We have passed the 20th anniversary of the introduction of computerized methods in seasonal adjustments by the Bureau of the Census. Method I was introduced in 1954; this is 1976. We are now also past the 10th anniversary of the publication of the X–11 method, which was published in 1965. A lot of people currently are using the X–11 method. I want to explain, in this context, something that a few of you may not know—the meaning of X–11. Everyone uses the term "X–11," but I do not know how many of you understand what it means.

We had begun by introducing method I, a very quick job. Then we introduced method II, which we believed was a big improvement over method I. Naturally, however, we began to try to improve it. We decided to call our different variants of method II experimental methods and writing out the full words on the computer was difficult in those days, because the Univac memory was small by modern standards. So, we used X for experimental, and we had X–0,

X-1, X-2, and so on. I think the OECD presently is using X-9, but there are many variants. The idea implicit here was that we knew the method could be improved, and we explicitly recognized this by using the X identification of the variants.

I expected that after I left the Census Bureau in 1969, there would be an X-12, X-13, and X-14. I guess I should tell the Census Bureau that I am waiting for them to develop an X-12, because, if they do not, the BLS will.

I would like to make another point at the beginning of my speech, a point which some of the others have made, and that is that there are a great many options in X-11. Most people talk about the standard routine, but it is not necessary to use it. For example, comments were made this morning about the desirability of obtaining a more stable seasonal. It can be done with X-11; i.e., a completely stable seasonal can be computed with X-11 by selecting the proper options. Different weights also can be selected. You can select among many different variants of weighting and time periods. Thus, there is a great deal of flexibility in using this method.

I would like to tell you a little anecdote about how we got on to using computerized methods in the first place—or, about how I got on to it, at least. I used to be in a carpool. Before the beltway was built around Washington, we spent about 1 hour and 10 minutes, each way, going from where I lived to the Bureau of the Census. For awhile, the carpool consisted only of Eli Marks and me. Eli is still at the Census Bureau. I do not know how many of you know Eli, but those of you who do, know that he is a great talker. Believe it or not, I was the listener, and he was the talker.

Eli had been assigned to write the computer language for Univac I. When the Census Bureau bought Univac I, a language had not yet been written, and he, Joe Daly of the Census Bureau, and John Mauchly, who was one of the inventors of the Univac, were writing the language. So, I would ride 2 hours and 20 minutes, on the average, every day with Eli, and he would tell me about this machine that he was using all the time and all the troubles he was having with it.

One day, it occurred to me that this machine could be used for making seasonal adjustments. The Federal statistical agencies, at that time, had been under a lot of pressure from the Council of Economic Advisers to produce more seasonally adjusted series. I talked to some of the computer technicians to find out whether they could do the calculations required for seasonal adjustment, how long the job would take, and how much it would cost. It turned out that it would take 1 minute to do a 10-year series, and it would cost $2. That seemed like a good deal, so we decided to go ahead and do it.

The people who ran the computer, mostly Morris Hansen, and Ross Eckler, who was deputy director then, could not get the people working on other Census Bureau programs to use the Univac. No one wanted to tangle with this newfangled machine without a language. When I came forward with the proposition that someone develop a seasonal adjustment program, Hansen and Eckler went along with it. A programer was assigned to the job, and method I was developed. I do not have to cite many statistics for you; you know how long it takes to make seasonal adjustments today. But, my staff just gave me some figures on present computing time and cost. We now are getting this work done for us at the National Institute of Health. It takes, for all tables without the charts, a little more than 1 second. They charge us $1.84. With the charts, it takes 1.6 seconds; that is slightly more than 1½ seconds, and it costs $2.92. That is a pretty good bargain.

I want to tell you another anecdote about those early days. Arthur F. Burns was, at that time, chairman of the Council of Economic Advisers. I had worked for him at the National Bureau of Economic Research for a number of years, and one of my responsibilities had been to calculate seasonal adjustments. He was pressing—he and the whole Council—for more seasonally adjusted series. I met him once outside the old Executive Office Building and told him a little about the Univac method. He invited me over to his house one evening, where I spent several hours going over the computer run with him and Geoffrey H. Moore, then associate director of the National Bureau of Economic Research, who was visiting Washington for a few business conferences. I told them about the great wonders of this new machine and the high quality of the seasonals being produced.

That sounded great to them. One day, Arthur called me about 9 o'clock in the morning and said, "I have an appointment with President Eisenhower at 3 o'clock, and I would like to show him the seasonally adjusted series for female unemployment. You have that wonderful machine, and you can do it in a minute. How about delivering it in time for my appointment?"

I said "Okay, okay," but you now what that involved. First, I had to gather the data, have them punched, get time on the machine, and, finally, we all had to pray that the machine would work.

You know, believe it or not, we were able to go through all those steps that morning. It may be the only time in history that everything worked right. Murphy's law in reverse: Everything that could go wrong went right. We delivered the data about 2 o'clock that afternoon. Burns did show it to President Eisenhower and, henceforth, was completely sold on the method and strongly supported it.

One other little story. When I was trying to convince the rest of the Government to use method I (OECD and Canada were very easy to convince, because I was not in competition with them, but I was in competition with many of the agencies in Washington). They gave me a hard time, because they did not believe that the computer could do what their skilled technicians did.

Among these agencies, I had been talking to the people in charge of seasonal adjustments at the Federal Reserve, and I made the following challenge: I suggested that they take any series and spend as much professional time adjusting it as they wanted. We would run that same series through our computer program, and then both series would be plotted on a chart without saying which was which. We would get a small group of Federal Reserve people to judge the results.

They accepted the challenge, and among the group they selected were some of the most distinguished people in this field at the time. I do not know if many of you will remember their names, but the group included Woodlief Thomas, Frank Garfield, and Arthur Broida. When we delivered our chart, each one studied it separately, and the result was that they unanimously voted that the computer method was better. The whole Federal Reserve staff then took our computer program seriously. That was a big breakthrough.

Well, so much for early history. Now, let me tell you about our recent problems at the BLS. Unlike the technical problems that we are discussing today, these are mostly public relations problems. The role I usually have when representing the BLS is very different from the one I have today. Today, I am talking to a very sophisticated audience whose members have had considerable experience as statisticians. There is a lot of know-how in this group, and that makes attending this meeting a great pleasure for me. Most of the time, though, I talk to people who know almost nothing about the technical procedure of seasonal adjustment. These people include high officials of the Administration, many members of Congress (most often those on the Joint Economic Committee), the media, and the lay public. We receive more than 20,000 letters and inquiries each week at the BLS, asking us for one thing or another, and seasonal adjustment problems have not escaped notice. Because of our audience, we have a very different problem from the one you have when you are undertaking a research job.

When doing research, whether in a university or in the government, you are trying to obtain the best seasonal adjustment possible. By contrast, when you are dealing with the people I have just described—and, by the way, they are all very intelligent, very interested, and very concerned—the most important problem

is communication. You have to be able to communicate with them. And, they have to believe you, because there are bound to be many things concerning the statistical procedures that they are not going to understand. Thus, it is a more complex problem, requiring very different approaches, than simply concentrating on developing the best technical method of making adjustments.

Let me tell you about a few specific experiences, many of which you, no doubt, have read about or have heard about. Earlier this morning, the difficulties we had in seasonally adjusting the unemployment figures in June 1975 were mentioned. According to the official method of calculating unemployment, the economy registered a very sharp drop in employment that month. We knew in advance—well in advance, in fact—that this would be the case. We had estimated the drop to be between 0.5 and 0.7 percentage points.

I had told my staff that I did not think we should discuss our estimate of the drop but, instead, should keep hammering away at the fact that the June figure would be too low. But, someone slipped the estimate out, and soon everybody was waiting with bated breath for the June unemployment figure to appear.

The drop turned out to be 0.6 percentage point. I had told the JEC, 1 month in advance, before we even had taken the survey, that there would be a sharp drop in unemployment. that it would not be a valid figure, and that it would overstate the actual decline. Not only did I tell the JEC, but I also had occasion to meet with many other people during that period, and to give several speeches. Therefore, during May, I kept hammering away at the point, and it appeared in all the newspapers. I even had a short session with George Meany one day and explained it to him. He understood—perhaps not the specifics of the situation—but he understood the major point.

As I said, the decline turned out to be 0.6. Now, why were we so accurate in our estimate? Interestingly, our problem that year was not due only to a methodological problem with our seasonal adjustment. The error was largely due to the fact that no one I knew expected unemployment to double in that year. But, it did. You would expect, therefore, that the seasonally adjusted increase also would double if you use a multiplicative method.

Perhaps such a doubling was reasonable for male and female adult employment, but it clearly was not reasonable for employment among teenagers. The reason is simply that we knew roughly how many teenagers would come into the labor market in June, because we knew how many were in college, how many would finish school (within fairly narrow bounds), and that their employment level would be nothing like the one our method would indicate. After the actual figures for June came out, we received tremendous

acclaim for both the accuracy of our estimate of the June drop in unemployment and for our openness in saying that this official figure would overstate the decline. People could not believe that we were able to estimate anything that accurately, but doing so really was not very difficult.

A question I frequently am asked is, since we knew the June figure would be wrong, why did we not change the method we used to calculate the seasonal adjustment? The reason, and I cannot overestimate the importance of this point, is that we are locked in to using the adjustment factors that we publish at the beginning of each year. If we had changed the factors in the middle of 1975, the politicians and the media would have been after us. They would have thought that we were manipulating the figures. Although we became aware that the factors we were using were in error, we decided that the best thing we could do was to leave the factors alone, explain that there undoubtedly was a problem with certain seasonally adjusted unemployment rates, and take the flak, because we were unable to publish better figures. That is what we have done, not only in the unemployment series, but also for other series, such as wholesale and consumer price indexes, when we have had reason to believe that the current adjustments were deficient.

The comment was made this morning that the 0.6 drop in unemployment in June 1975 has been greatly revised. That is true, in a sense. We knew the mistake we had made in 1975; thus, at the beginning of this year, we changed the methodology in order to avoid making the same mistake again. We now use an additive adjustment for the teenage component, and that eliminates most of the problem. Thus, we did not have any trouble with the June 1976 figures. Had we been smart enough to forecast the doubling of unemployment and had changed our method of computing the seasonal adjustment 1 year before we did, we would have reported a May–June change in the unemployment rate of –0.3 (instead of –0.6) percentage point, which would have been revised to –0.2 (instead of –0.4) percentage point. The 0.1 revision compares with one of –0.4, which combines the change in methodology and the updating of the factors, and is shown by a direct comparison of the official figures for 1975 and 1976, published in those 2 years, and, subsequently, revised for our current tables.

The error we made, looking back, was to a great extent an error in forecasting unemployment. The error in making the seasonal adjustment was relatively small. (One thing you may want to think about is what the public reaction would have been had the BLS attempted to forecast unemployment and had produced an accurate forecast at the beginning of the year stating that unemployment would double in 1975.)

This year, we encountered trouble of a different nature. The economy is currently in a period of unprecedented high unemployment for our present stage of economic expansion. At the same time, the factors we are using to make seasonal adjustments are based on historical experience. Bear in mind that the Bureau is locked into these factors. That is, when you are in a political situation, such as this, with the Congress and the press peering over your shoulder at everything you do, it is very difficult to change anything. As I said, we did make one change—we introduced the additive adjustment for teenagers.

The major cause of the large drop in unemployment reported at the beginning of this year was the updating of the seasonal adjustment factors by including data for 1975. This updating is a routine practice carried out for almost every BLS series every year. I have said this many times, especially during the beginning of this year (I made five speeches during this period), and I went out of my way to say it: The revisions of the seasonal factors were probably too large. But, we did not have hard evidence to demonstrate that, and, therefore, I did not see what else we could do at that time.

If we had not updated the adjustment factors, we would have faced a tremendous amount of criticism. People would have said, "Why don't you update the factors this year, as you always do? What are you doing, manipulating the unemployment rate?" Thus, we had to do what we always do and update the factors.

The question really becomes this. Did we, in updating the factors, not only include the seasonal rise in unemployment but also some of the cyclical rise? We probably did. I think, when we revise our estimates later, we will see a smaller decline in unemployment at the beginning of the year and more improvement later in the year.

The point I am making is that, in many situations, we are locked into whatever methods and factors we are using, and, when your data are in the public eye, the way both our employment and price figures are, you just do not have much elbow room. You have to follow the established procedures; changes have to be made once a year when new seasonal factors for the year ahead are issued. As each year unfolds, we usually can see differences in the seasonal factors, and these become even more apparent when we revise the factors at the end of each year. Perhaps, in future years, when Watergate has been forgotten, Government statisticians will have more latitude in making changes in these sensitive indicators during the course of the year.

Let me describe some similar problems we faced with the public—not with technicians—in adjusting the wholesale and the consumer price indexes. My

predecessor, as commissioner of Labor Statistics, Geoffrey Moore, who is a great technician, altered the BLS adjustment method so that, instead of adding seasonally adjusted components of the indexes to obtain a total, we directly adjusted the total. I think that you can make a very good case for this method. I think Moore was right, from a technical point of view. If I had a free hand, I believe that I would use the same method. But, I, nonetheless, used to sit at the table with John Layng, who is in charge of our price work, and say, "John, someday we are going to get clobbered by this approach—let's change it as soon as possible." And, we did get clobbered before we could change it! John and I decided that we had to go back to adjusting the figures in such a way that they would add to the aggregate. But, we did not feel that we could change our method in the middle of the year. We, therefore, were waiting for the beginning of the following year in order to introduce this change at the same time as we would make the routine revisions.

Late that year, we found ourselves in a situation where the changes in the major components of the wholesale price index were smaller than the change in the aggregate. The Joint Economic Committee picked that up, and, believe me, I really had a rough time 2 months in a row. The first month, the weighted average of the changes in the individual components was smaller than the change in the aggregate itself, and, the following month, the weighted average was larger than the aggregate.

I pointed out that, if you just average the 2 months, you would come out okay. But, I could not sell that idea. In fact, some members of the press thought the situation was a huge joke and made fun of us. But, we survived. We changed the method for the two price series so that the aggregates now equal the weighted averages of the components. I think this is an example of a situation in which you change your method, but you really do not believe in that change. We do not get the best seasonal adjustment this way, because, I think, as Moore does, that better adjustments can be obtained by directly adjusting the aggregates. But, we were forced, because we needed a method that we could convincingly explain to the public, to shift to a method somewhat inferior, from a technical point of view. I think that was the sound thing to do, however, and I would do it again.

Now, let me give you an example of the difference between our situation when dealing with unemployment and prices compared to our situation when dealing with other series. Last month we published a figure that indicated a 0.3-percentage point drop in the average hours worked, per week, in manufacturing. It dropped from 40.2 to 39.9 hours, which looks like a big drop.

Moore was thorough—anyone who knows him knows

that—and he changed the methods for all the series. The BLS had been directly adjusting the aggregates for nearly every data series. If you look at the two major components of the average hours worked for August, you will see that durables did not change at all, and nondurables changed only by −0.1 percentage point. While it would appear that the 0.3 decline was too large, and, although it probably is a better estimate than that obtained by summing the components, it may be a little too high. But, no one has picked that up. I have not heard one word about it. We noticed it, and we worried about it; but, no one else so far has mentioned it. That is one indication of the big difference between the problems we encounter when explaining adjustments to series that interest the public, and the problems of explaining adjustments to groups with technical knowledge.

One other example of the type of problems we face with the public is the problem of equating sums. The X–11 method approximately equates sums of logarithms, but it does not equate sums of natural numbers. I say that it approximately equates sums of logarithms, because the X–11 method uses the 12-month arithmetic moving average and adjusts the seasonal factors so that their arithmetic average is 100. If it used a geometric moving average and adjusted the sums of the factors so that their geometric average is 100, the sums of the logs of the seasonally adjusted data for each year would be equal.

But, the sums of natural numbers are quite often far apart. Again, in a meeting such as this one, it is possible to speak of the advantages of allowing the differences in the sums of natural numbers to exist and the variety of reasons for doing so. But, if, in fact, the automobile industry produces 10 million cars in 1 year and someone adds up the seasonally adjusted figures, and they total only 9 million cars, the public will think you are nutty. You simply cannot do it this way. You have to equate the sums, because you are dealing with an informed and concerned public that demands information they can understand.

When we produced the seasonal adjustment program, method II, I was working at the National Bureau of Economic Research on a 1-year fellowship. We selected, from their huge files of time series, a stratified sample of 150 series. We tested every aspect of method II on each of these 150 series. It was the first experiment of this magnitude ever made in this field.

I think this type of testing is what you have to do. When I hear, and I do not fault anyone for this, because not everyone has the resources we had, people say that they have tested a method on two or even five series, it leaves me completely cold. When we publish the unemployment and employment figures each month, we turn out 250 seasonally adjusted series.

We probably have another 100 series included in the payroll employment survey and several hundred additional series included in the price surveys. We have still more series in the wages surveys. In effect, we probably are publishing roughly 1,000 seasonally adjusted series every month.

Unless we have a method that is generally applicable and one that can be trusted, because who can look at a thousand different series each month—we cannot use it. This requirement exists for anyone who has an alternative method. He or she has to convince the people who are accountable that a new method is better. Unless this can be done, there is little chance of getting a new method adopted.

I would like to say something about tests. There are a great number of tests built into the X–11 method; e.g., the F–test is part of the program. People have talked about testing our program with regard to minimizing revisions and have talked about sum preservation. When I first started using the X–11 method, we tested it by seasonally adjusting the seasonally adjusted series. I have discovered that this operation now is called idempotency. I did not realize what a fancy thing we were doing years ago. There is another test I do not even understand—orthogonality—but I believe that we did that too.

We applied our methods to a great variety of series and tested the results. I think that this type of testing has to be done. I also think that you need so many resources to carry out a program like this, that a university professor who wants to do similar tests on a method is at a great disadvantage. Someone may have the most brilliant and reliable method in the world, but, unless he or she can convince the people who are involved—the Administration officials, Congress, the media, the public—that a new method is substantially better and that it will work on a large variety of series, it is not going to be adopted even if it really is a better method.

Let me conclude by adding a few more observations. Another requirement is a method of making seasonal adjustments that is relatively simple to understand. The 12-month moving average method is easy to understand, because the logic of it is quite simple. I will not go into it, because most of you know it. Any college student with some background in mathematics and statistics can understand it. And that is a great advantage, because invariably you will be forced to explain your method to the public, and, if you have a complex method, it is not going to be saleable. A real problem exists concerning the need to explain these complex methods to lay audiences on the one hand, and the need for refining, elaborating, and improving these methods on the other.

There is the need for simplicity, no doubt about it, and, in fact, the X–11 method, as many of you know, is quite easy to understand despite the many steps in it. I avoid talking about seasonal adjustment methods publicly. But, I must say that I have been asked, during Joint Economic Committee hearings about the weights we use, the period we use, and many other technical aspects of our method. Right now, Senator Humphrey is almost an expert on seasonal adjustments, and so is Senator Proxmire. Every month, or nearly every month, they quiz me about our methods.

Let me conclude by saying that I think this conference is a wonderful idea. It brings together people who are in different fields and enables us to understand each other better. I would urge the management of this conference to plan another one in a few years at which we hopefully can review the results of some of the alternative methods that have been suggested today. While I personally do not know much about it, I already have asked Kathy Beall, who works for us at the BLS, to check out a suggestion that Dr. Dagum of Statistics Canada made—the X–11 ARIMA method. But, I am not going to try that on the Joint Economic Committee.

I would also urge that a future conference invite papers from users, people who are not in the business of developing seasonally adjusted data, in order to get their points of view. Perhaps one group that could be invited is the Joint Economic Committee staff. It really would be helpful to hear from them next time. Similarly, I would like to invite the opinions of some public affairs people. Some of them are very, very good. I have known several, in recent years, who really are very good at converting complex statements into fairly simple language. We need to hear from them. So, my concluding recommendation is that we plan another conference in a few years with the additional objectives I have just described. Finally, let me close, where I began, by thanking the National Bureau of Economic Research and the Census Bureau for making it possible for us at the BLS to be exposed to this group.

I have a lot more I could talk about, but I want to save a few minutes for Shirley Kallek. People here who are university professors may be pointing with great pride to their students—about 20 years from now—as if to say, that man or woman is great, because he or she took their course in elementary economics. I went to a conference some time ago where one of Paul Samuelson's professors spent the better part of twenty minutes explaining that he had been Samuelson's professor and implying that Samuelson owes all his great success to him.

Well, we cannot say that in the Government, because we do not have students. But people work for us, and Shirley worked for me. Do you remember that, Shirley? Did I give you a very hard time, Shir-

ley? "At times," you say. Well, anyway, Shirley worked very hard to make this conference a success, and she did a very good job, as she always does.

It was great being here. I think this is a wonderful experience, a very useful conference, and, with that, I close.

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