in this docket; and (10) a LEC price cap performance review should be scheduled annually, with a more extensive and intensive review scheduled to take place every three years.

Respectfully submitted,

AT&T CORP.

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Appendix A

List of Other Parties Submitting Comments In Response to Fourth Further Notice of Proposed Rulemaking in CC Docket No. 94-1

Regional Bell Operating Companies ("RBOCs")

Ameritech

Bell Atlantic Telephone Companies ("Bell Atlantic")

BellSouth Telecommunications, Inc. ("BellSouth")

NYNEX Telephone Companies ("NYNEX")

Pacific Bell and Nevada Bell (collectively "Pacific")

Southwestern Bell Telephone Company ("SWB")

U S West Communications, Inc. ("U S West")

Other LECs

Cincinnati Bell Telephone Company ("CBT")

GTE Service Corporation and its affiliated domestic telephone operating companies ("GTE")

The Lincoln Telephone and Telegraph Company ("Lincoln")

The Southern New England Telephone Company ("SNET")

United States Telephone Association ("USTA")

Other Commenters

Ad Hoc Telecommunications Users Committee ("Ad Hoc")

American Petroleum Institute ("API")

Frontier Corporation ("Frontier")

General Services Administration ("GSA"), on behalf of Federal Executive Agencies

International Communications Association ("ICA")

MCI Telecommunications Corporation ("MCI")

Sprint Corporation ("Sprint")

Telecommunications Resellers Association ("TRA")

APPENDIX B

Reply Statement of Dr. John R. Norsworthy[1] and Dr. Ernst R. Berndt[2]

RESPONSE TO COMMENTS OF LOCAL EXCHANGE CARRIERS ON METHODS FOR MEASURING THE X-FACTOR FOR THEIR INTERSTATE ACCESS SERVICES

In this Statement we respond to matters raised in the comments filed in the Commission's rulemaking proceeding (CC Docket No. 94-1) by the United States Telephone Association (USTA) and Regional Bell Operating Companies (RBOCs) concerning the measurement of the X-Factor to be included in the price cap formula for the interstate access services furnished by Local Exchange Carriers (LECs). We particularly address certain points discussed in attachments to the Comments of USTA and various RBOCs, prepared by Christensen Associates (Christensen), National Economic Research Associates (NERA), and Dr. Melvyn A. Fuss. We further comment on the revised USTA model of total factor productivity (TFP) put forward by Christensen, known as the "Simplified Christensen Model." Finally, this Statement provides updated results for the Performance-Based Model, developed by Dr. Norsworthy in conjunction with AT&T Corp., to measure the appropriate X-Factor for the LECs' interstate access services.

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2 Professor of Applied Economics, Alfred P. Sloan School of Management, Massachusetts Institute of Technology.

A. Introductory Observations and Overview of this Statement

What are the principal measurement issues that we must address concerning the productivity offset, or X-Factor, for price cap regulation of the LECs? There are two -- the quantification of the input price differential and the computation of TFP for the LECs' interstate access services.

Both measurement issues derive from the fundamental question: How shall the productivity target for the LECs be established? The Commission has concluded that a price cap should be used that benchmarks price increases of the LECs to that of the national economy, as measured by the GDP-PI for the nonfarm private business sector of the U.S. economy. The principle is to limit the price increase for the LECs' interstate access services to the rate of increase of the GDP-PI. Toward this objective, the productivity offset or X-Factor that establishes this benchmark is determined mathematically:

(1)
$$\Delta P_{US} = \Delta P_{LEC} + X$$

where

(2)
$$X = (\Delta TFP_{US} - \Delta TFP_{LEC}) + (\Delta P_{LEC} - \Delta P_{US})$$

The important point to note, then, is that *there can be no argument* about whether there *is* appropriately an X-Factor in the price cap equation, nor about whether it *should* include an input price differential. The debate must focus, therefore, on how the components of the X-Factor should be measured. The dispute about how to measure TFP primarily concerns whether the X-Factor applied in capping LEC interstate prices should be based on an estimate of interstate

productivity of the LECs. On a total company basis (combining interstate with intrastate), the revised USTA model (the Simplified Christensen Model) gives a TFP average growth rate closer to that computed in the Performance-Based Model than the original USTA model did. The controversy, therefore, involves the measurement of TFP separately on an interstate basis. The use of the total company data, as was done in the Christensen models, results in a substantial understatement of the productivity growth for the LECs' interstate services.

The dispute about the input price differential (IPD) should be similarly focused on how to measure that differential. We will show that the IPD-related issues addressed by USTA and the RBOCs are largely off the mark, and that they are addressed by Christensen, NERA and Fuss using data that are not appropriate to the issue. There can scarcely be a convincing argument made to support the proposition that the pre-divestiture behavior of the LECs with respect to input prices is relevant to their expected near term behavior now. Further, we shall show that the arguments that the input price differential is zero (Christensen and NERA) and that the rise of the differential in the 1980's was a "temporary" aberration (Fuss) are spurious, and represent a distraction from the main task, which is to determine the size of the input price difference between the LECs and the national economy.

Further, as we shall show, relevant input price data for the pre-1984 period, used by Christensen, are not official governmental statistics for the private nonfarm sector of the U.S. economy -- even though such statistics are and have been available. [Rather, the Christensen data on U.S. input prices are those it] developed on its own, applicable to the private sector of the economy and based on very different methods for measuring output than those applied in the national income

accounts. Moreover, U.S. input price data for the 1985-1993 period, as used by Christensen and NERA, are not for the private nonfarm sector, but are for the total private sector of the U.S. economy.

Both issues -- the separate measurement of TFP for interstate access, and the measurement of the input price differential -- are quantitatively quite important. They each affect the size of the productivity offset by two to three percentage points. USTA and the RBOCs have attempted to blunt the dispute about TFP measurement by adopting a revised "simplified" TFP measurement procedure. The quantitative importance of the debate over the revised Christensen procedures is of relatively less significance in measuring TFP and the X-Factor.

One of the principal issues in TFP measurement concerns the adjustment of capital for quality change. In the Performance-Based Model the change in TFP that results from adjustment of capital input is exactly offset by a compensating change in the input price differential, so that the quantitative importance of the capital input adjustment *as a modifier of the X-Factor* is nil. Rather, the importance of this adjustment lies in understanding a very significant source of LEC productivity growth in the post-divestiture period. The input prices for the LECs in the TFP analysis, constructed by Christensen, use the GDP-PI itself as the price of materials and service inputs, which is about 25 percent of the total value of inputs in the post-divestiture period. This practice tends to bias the difference in input price movements toward zero. The materials component also grows faster than the other inputs because there is a trend, at least among the RBOCs, toward out-sourcing services. The nature of the LEC input price series as used in the

Christensen Model raises the question of its appropriateness to address the issue of the size of the input price differential.

In the first section of this Reply Statement, we focus on the input price differential. We show first that the arguments of Christensen and NERA are directed away from the main line of **economic** inquiry -- *how large is the input price differential called for in the X-Factor* -- and toward a purely **statistical** issue -- *if it is **assumed** that the input price differential **is zero**, is there a five percent chance that the assumption is correct?* The latter question, addressed by Christensen, NERA and Fuss, has an exact counterpart -- *if it is **assumed** that the input price differential **is not zero**, is there a five percent chance that the assumption is wrong?* The answer to the first statistical question is yes, if the question is addressed in a longer term context; i.e., a time frame that extends back in time well before the Bell System divestiture. In the period subsequent to divestiture the LECs have made decisions concerning inputs, outputs and technological changes in a market context of largely unchallenged dominance of local telephone services. Interestingly, the answer to the second statistical question, addressed in the same context, is no. The result is particularly strong, because the input price series for the LECs is biased toward the GDP-PI, as noted above. Both questions, however, are separate from the main issue to be considered in this proceeding -- how large is the input price differential appropriate to price cap regulation of the LECs?

After examining the relevance and reliability of the data used by Christensen, NERA and Fuss to measure the input price differential, we address the statistical argument made by Christensen and NERA. We then consider the statistical argument advanced by Fuss, that the movements in the

IPD in the post-divestiture period are "aberrational," and we show that his statistical techniques are flawed because they ignore the time series properties of the price and bond yield data used in his analysis. Two serious issues concerning the relevance of the data used in the Christensen, NERA and Fuss analyses are also raised. Finally, we show how the input price index for the LECs can be improved by a separately constructed price index for materials inputs, and by a measure of the price of capital inputs constructed for the LECs derived from data routinely reported to the FCC. Based on the resulting index, we estimate the IPD for the period 1985-1994. The best estimate of the input price differential for this relevant post-divestiture period is 2.8 percent, based on proper measures of LEC input prices, and the input price series for the U.S. private nonfarm sector, as recently released by the Bureau of Labor Statistics. Our conclusion thus confirms the findings of Commission economists, Bush and Uretsky, although they based their analysis on LEC and U.S. input prices taken from the Christensen data set.

B. Measurement of the Input Price Differential For LEC Price Cap Regulation

1. Perspectives on the measurement of IPD

Although there was consensus among parties to the LEC performance review that economic theory shows that an input price differential (IPD) is a proper component of the computed X-Factor, there has been considerable controversy concerning the size of the differential. Christensen has stated that his calculations indicate that the IPD is between 2.2 and 2.9 percent, while it is not "significantly" different from zero; Bush and Uretsky have taken Christensen's (and NERA's) data and argued that the IPD since 1984 has been substantial: about 2.2 percent. Fuss uses Christensen's and NERA's data and presents evidence that purports to show that the IPD was transitory -- a "temporary" phenomenon between 1984 and 1989. We have used the same data and shown that stationarity assumptions made by Fuss are

inappropriate; we also argue that the wrong maintained and null hypotheses are employed by Christensen and NERA.

There are two very important issues that should be addressed to help clarify and resolve this controversy. First, does economic theory have anything to suggest? If so, a foundation of economic theory should be used to specify the maintained hypothesis in investigating the IPD. Second, are the data underlying this empirical debate reliable and consistent, both with each other and with the framework specified by the FCC?

Concerning economic theory, it is clear that if one makes assumptions about movements in factor prices in an economy, along with using stylized facts, one can obtain a prediction for the sign of the IPD. Specifically, suppose one assumes that, between the LECs and the national economy, all capital inputs are homogeneous, all labor inputs are homogeneous, and all intermediate material inputs are homogeneous, but that there is heterogeneity among the *proportions* of capital, labor and intermediate materials used in the two sectors. This is a very strong assumption, but it provides a useful perspective. If markets are competitive and factor inputs are mobile among sectors of the economy, one would expect that the prices of capital inputs would grow at the same rate for the telecommunications sector as for the economy as a whole, that the prices of labor inputs would grow at the same rate for that sector as for the economy as a whole, and that the prices of intermediate material inputs would grow at the same rate for the telecommunications sector as for the economy as a whole.

This equal growth in input-specific input prices does not imply, however, that the IPD is zero. There are two reasons for this. First, an uncontested fact is that over the last forty years, prices of capital have increased less rapidly than the prices of labor and intermediate materials. Second, another uncontested fact is that when compared to the economy as a whole, the telecommunications sector is considerably more capital-intensive (see, for example, the BLS and Christensen data). Together these two uncontested facts imply that, given competitive and mobile factor markets, economic theory would *predict* that the IPD is negative, i.e., aggregate input prices are growing less rapidly in telecommunications than in the economy as a whole. This prediction is borne out by the results reported by all investigators.

Now while we do not put forward the position that the competition and mobility assumption for the national factor market is absolutely true, it does at least help us as to what we might expect, and what the appropriate maintained hypothesis ought to be. It is certainly less extreme than the assumption made by Duncan[3] and others that market forces will ensure that the IPD will always tend to be zero. This position asserts that all sectors will tend toward the same production technologies, which, for purposes of telecommunications policy in the near term, is nonsense.

The second issue identified above -- namely, are the data underlying this empirical debate on the size and timing of the IPD reliable and consistent? -- has been largely ignored to this point. This is a critical oversight.

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[3] See GTE Comments, Appendix F, Statement of Gregory M. Duncan.

For the purposes of argument, let us assume, as the logic of the Commission's price cap plan asserts, that one wants to compare aggregate input price growth of the telecommunications sector to that of the private economy. What one means by *the private economy* must be specified more carefully. It is our understanding that the definition of private economy that has been specified by the FCC is the nonfarm private business sector -- a sector whose definition is well-defined, widely accepted and studied by Federal statistical agencies. Specifically, TFP growth and corresponding prices of inputs and outputs are published annually by the Productivity Research Division of the Bureau of Labor Statistics (BLS).

If one wants to compare aggregate input price growth, using publicly available data and accepted procedures, then

(i) it is desirable that the measures of aggregate input price growth for the nonfarm private business sector are those either published by the BLS, or are known to replicate BLS procedures and are verifiable;

(ii) it is desirable that measures of aggregate input price growth for the telecommunications sector are those either published by the BLS (not feasible in the near future), or are known to mimic BLS procedures for sector input price measurement and are verifiable;

(iii) these two consistent series can then constitute the basis for a reasonable comparison.

However, if one looks at the IPD used to date in this empirical controversy, it is seen that certain investigators fail miserably in meeting these desirable minimum standards.

(i) Concerning estimates of aggregate input price growth for the nonfarm private business sector, Christensen's (and NERA's) data employed to date

(a) are based on the entire domestic private sector, not the nonfarm private business sector; and

(b) are not published BLS data, but instead are Christensen-generated data, in which calculation procedures and assumptions have apparently varied greatly over time, with the nature and significance of such varying assumptions being neither clear nor verifiable.

(1) Although Christensen references his 1970 paper with Jorgenson, it apparently was not only updated in 1986, but some of the procedures were quite likely radically altered (e.g., use of educational attainment data to alter real labor input and its price; use of relative capital utilization to alter real capital input and its price). (Although the papers in which these altered procedures are discussed were specifically requested, they have not been provided us by USTA.)

(2) In some of his work, Christensen employs BLS estimates of multifactor productivity growth, but does not utilize the underlying

input price data, and instead utilizes inconsistent data from other sources, such as the Bureau of Economic Analysis.

(ii) This information inevitably leads to the conclusion that Christensen's estimates of the economy-wide input price series are unreliable and unverifiable, and that to the extent they have been employed as one component of estimating an input price differential, that use is totally inappropriate.

2. Data issues concerning the Christensen, NERA and Fuss historical analyses

There are two points concerning the Christensen data that speak to the reliability of the Christensen, NERA and Fuss analyses regarding the input price differential. First, Christensen's data on the input price series for the national economy in the 1949-1984 period do not represent the official governmental productivity statistics for the private nonfarm sector of the U.S. national economy, even though those data are available from the U.S. Bureau of Labor Statistics (BLS). Instead, Christensen, NERA and Fuss rely on U.S. input price data for 1949-1984 that were developed by Christensen alone, and do not agree with the BLS data. Second, although the U.S. input price statistics for the 1985-1993 period are derived from the official governmental source (BLS), they do not represent the private nonfarm sector of the U.S. economy, as called for by the Commission. Rather, they represent the total private sector of the economy which includes the volatile agricultural sector.

In his testimony submitted before the California Public Utilities Commission, Dr. Christensen cited a September 1986 update of work carried out earlier with Dale W. Jorgenson and published in the 1969 and 1970 volumes of the *Review of Income and Wealth*. It is clear from examining his recent work for USTA that the methodology reported there has been changed from the earlier methodology in his 1969-1970 work carried out jointly with Jorgenson, so that the details of the sector represented by the statistics are unknown. For example, Christensen does not now include an adjustment to capital input based on capacity utilization in measurement of total factor productivity, while his earlier collaborative work did do so. We have repeatedly requested from USTA the documents cited by Christensen in his California testimony, as well as other information that would clarify this issue. However, as of the time this Statement was completed (February 29, 1996), we have not received from USTA the information responsive to our request, and thus we are unable to elaborate further on this aspect of Christensen's questionable use of data.

The input price series for the national economy from the "Christensen data," versions 1 and 2, appearing in the Christensen, NERA and Fuss analyses are neither official U.S. statistics nor do they represent the U.S. nonfarm private business sector, the target sector specified for benchmarking the price caps for the LECs. We have compared the input price series for the U.S. private nonfarm sector from the Bureau of Labor Statistics with the series shown in the Christensen data used by Fuss. The data series from BLS that we used is that which was official during the comment period for this proceeding (September 1995 through December 1995). This is the series that Christensen, NERA, and Fuss should have used in their analyses. For the 1949-

1984 period, there is hardly a year in which the annual data for the two different series (Christensen v. BLS) actually match. The same is true for the period 1985-1993; indeed, the correlation between the two different series is only 0.38. From the perspective of the Commission's objective to benchmark interstate access prices of the LECs to the private nonfarm sector of the U.S. economy, the Christensen input price series is thus irrelevant. Similarly, the analyses of the input price differential carried out by Christensen, NERA and Fuss are irrelevant and unreliable. For this reason alone, these analyses are invalid for purposes of this proceeding.

A further question emerges in this connection. The Christensen and NERA analyses have been considered and offered as evidence earlier in this docket to show that the input price differential between the LECs and the appropriate sector of the U.S. economy is zero. These analyses have been cited and defended in the same manner during the comment period, and the Fuss analysis has been introduced in the same vein. The Christensen and NERA studies have also been introduced as evidence in various state proceedings (Louisiana, California, North Carolina). The sources of the data have been obscured by citations to works which contain even further citations. It has been nowhere clearly stated that these data are simply off the mark, while the official relevant data have been readily accessible. This situation has undoubtedly misled investigators in other proceedings, and perhaps analysts on the Commission Staff, such as Bush and Uretsky. The purpose of presenting irrelevant evidence, masquerading as representative of official statistics, is open to question. While it is not possible to determine the purpose directly, sound scientific practice would insist upon calling such an "approximation" to the attention of the readers of the

research results. Whatever the purpose, we urge that the evidence in the above studies be disregarded.

3. Implications of the criticisms of the Bush-Uretsky study

The Christensen, NERA, and Fuss comments are all directed at the findings of the Bush-Uretsky analysis, concluding that the input price differential should be measured on the basis of the post-divestiture period and that the IPD is about 2.2 percent per year. As noted above, the Bush-Uretsky study had to rely on data supplied by Christensen. But even if the Christensen and NERA responses were based on data appropriate to the Commission's inquiries, their criticisms, as shown below, do nothing to change or effectively challenge the basic conclusions in the Bush-Uretsky analysis. Christensen and NERA address issues at best tangential to the key question: how should the input price differential between the LECs and the U.S. nonfarm private business sector be measured? Indeed, one of the criticisms directed to the Bush-Uretsky study seems to derive from confusion on the part of *NERA* itself concerning the data used in the NERA study. Christensen asserts that the NERA data are not consistent with the more recent – and now superseded -- data in the early 1995 Christensen Affidavit on input prices for the LECs and the national economy in the pre-1984 period. Even if this interpretation of Christensen's critique is correct, it is not clear what its import would be, because the bulk of the Bush-Uretsky analysis involves calculating the IPD during the post-1984 period. Moreover, Bush and Uretsky applied their methods to both the Christensen and NERA data sets (including the pre- and post-1984 periods), and they reached the same conclusion: that there is a substantial input price differential that is "essential" to include in the LECs' X-Factor. Of course, Christensen *could* criticize Bush and Uretsky for using totally wrong data for representing the private

nonfarm business sector, but to do so in a clear and direct way would reveal the irrelevance of the data used in the submissions by Christensen, NERA and Fuss.

4. Is the Input Price Differential zero?

This question has been addressed by Bush and Uretsky for the FCC, considering both the Christensen (1995 Affidavit) and NERA data sets. From their analysis of these data, Bush and Uretsky conclude that the average annual input price differential for the 1985-92 period is 2.2 percent.[4] They apply econometric methods to compare longer term movements in telephone and economy-wide input prices. They find that there are significant differences between movements of telephone input prices before and after divestiture, and that post-divestiture prices should be the basis of price cap regulation of the LECs. Their results using the Christensen data (asserted by Christensen to be superior for the purpose) are shown below in Table 1 as annual averages for each time period 1949-92, 1949-84, and 1985-92.

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[4] Bush and Uretsky, at p. 10.

Table 1

Comparison of Price Movements in LEC and US Input Prices as Computed by Christensen			
	LECs	**US**	**Difference**
Average Annual Change: 1949-92	**4.70**	**4.75**	**0.05**
Standard Deviation: 1949-92	4.44	2.64	4.02
Average Annual Change: 1949-84	**5.36**	**4.93**	**-0.43**
Standard Deviation: 1949-84	4.12	1.49	2.88
Average Annual Change: 1985-92	**1.73**	**3.95**	**2.23**
Standard Deviation: 1985-92	4.89	0.70	4.85
95% Confidence Interval for differential			-7.28 to 11.78

Source: Computed from data in Table 2, App. A, AT&T Comments.

The argument put forth by Christensen, as consultant to USTA, seeks to divert attention from this straightforward determination shown above. NERA, also on behalf of USTA, poses a somewhat different question: "Have LEC input prices changed differently from the national economy input prices *as they have been constructed by Christensen*?" The answer to this question is not a number; it is a yes-or-no reply in the manner of academic discourse. When actually addressing the question, NERA and Christensen alter it further, asking a *statistical* question: "Can we reject the hypothesis that *input price movements are the same* for LECs and for the (Christensen version of the) national

economy?" That is, *if we assume* that LEC input prices and U.S. economy-wide input prices move in the same way, *and if we assume* that the annual price movements in each series are random samples taken independently of each other and independently in time, is there statistical evidence that forces us to conclude, with only a five percent chance of error, that the two sets of price changes move differently? By asking and answering the question in this way, NERA and Christensen create a bias in a direction that most favors the LECs -- a bias toward *accepting the assumption* that there is no difference between the movements of the two series of input prices. In the following discussion, we point out the flaws in the statistical arguments by USTA's consultants, having already established the dubious quality of the national economy price data derived by Christensen.

The correct way to assess statistically whether the two series are the same is to compare them directly. When Christensen and NERA propose using the movements in the U.S. input price index to represent movements in the LEC input price index, *they are proposing that the series are the same*. Christensen's and NERA's proposition is *not* the same as saying that there is no significant difference between the movements of the series. A chi-squared test can be applied to compare the two series (of price changes in this case) and give the probability that the series are the same, by testing the statistical hypothesis that they are the same. Only if this hypothesis is not rejected, could one be on safe grounds statistically using the series interchangeably, as proposed by Christensen and NERA. (It could still be argued persuasively, on economic grounds, that the price movements in the pre-divestiture period are irrelevant because the decisionmakers, their objectives, and their markets for inputs and outputs were radically different after 1984.) The results of the chi-squared test are shown in Table 3 for the three time periods examined by Bush and Uretsky. For each time period,

the probability that the two series are the same is extremely small. *We cannot reject the hypothesis that the GDP-PI and the LEC input price series move differently for any of the three time periods.* We therefore conclude, in agreement with Bush and Uretsky, and based on the same Christensen data that they examined, that it is inappropriate to use the U.S. input price index to represent movements in the LEC input price index, i.e., to assume that the movements are the same. It is worth noting that the Christensen- NERA method of analysis can be applied to "prove" a number of propositions that are downright silly.

In order to clarify further the flaws in the Christensen-NERA reasoning and their conclusion, we conducted a similar hypothesis test based on the 1985-1992 period, the latest that Bush and Uretsky examined. We based our test on the same set of Christensen data used by Bush and Uretsky. Table 1 above shows the standard deviations for the differences between the series for each time period. The standard deviation is a measure of the sampling error for a set of observations that are assumed to come from the same distribution. For 1985-1992, the average annual change in LEC input prices was 1.725 percent, and for U.S. input prices, 3.95 percent, giving a difference of 2.23 percent per year. The standard deviation of this difference is computed as 4.85 percent. (As shown in Table 2 of Appendix A to AT&T's initial Comments, there is an unusual difference in 1990, which accounts for a large part of the standard deviation for the 1985-92 period. Moreover, 1990 is the only year in the post-divestiture period for which the Christensen data shows LEC input prices growing faster than those in the U.S. economy.)

Table 2

χ^2 Test for Equality of Price Movements in LEC and US Input Prices

Hypothesis that Price Movements are Identical for Stated Time Period	**Probability that Hypothesis Is True**	**Hypothesis Rejected or Not Rejected**
CHO-SQ:Pr(LEC=US) 1949-92	0.0000000001	**Rejected**
CHI-SQ:Pr(LEC=US) 1949-84	0.0000119396	**Rejected**
CHI-SQ:Pr(LEC=US) 1985-92	0.0000000091	**Rejected**
Source: Computed from data in Table 2, App. A, AT&T Comments, using Excel 5.0		

5. Are input price movements in the post-divestiture period aberrations from a long-term trend?

Dr. Fuss provided a Declaration, appended to the Bell Atlantic Comments, in which he considers the Bush-Uretsky analysis and discusses whether the post-divestiture input price differential they measured was a "temporary" phenomenon and whether the long-term IPD should be zero. In addressing these questions, Fuss examines the Christensen data, which cover 1949-1992, and the NERA data, which cover 1960-1992. He then considers each data set extended to 1993. In assessing the econometric properties of the Fuss data, we consider his extended data sets.

Fuss asserts that he uses the Moody's Public Utility Bond rate data. However, the series used in his analysis do not match any of the following published Moody's Public Utility Bond series: Aaa, Aa, A, Baa or the composite of all these series. It is therefore unclear what the value of the Fuss analysis is to the determination of the input price differential between the LECs and the U.S. private nonfarm economy. The U.S. national input price data he uses do not match that specification in any year; and the bond rate data that he purports to use -- because it was used by Christensen in his assessment of the service price of the capital input -- is not what he states it to be; and therefore he does not conform to the Christensen calculations. Finally, as shown below, his statistical work is plagued by various time-series problems in equations 4.1 to 4.4.

Fuss estimates the models which test his hypothesis that the IPD movements from 1984 are departures from a long-term trend, through using ordinary least squares (OLS) estimation. His two model specifications are shown below.

We first examined the variables used in the Fuss models in terms of conventional distribution statistics, as reported in Tables A-1 and A-2 in Attachment 1 to this Reply Statement. In view of the relatively small number of observations in each series, we then tested the shapes of the distributions of the price change and Moody's bond rate series by examining their skewness and kurtosis using the tests outlined by Davidson and MacKinnon (1995), and found that each series is reasonably well described by the normal distribution. The results are shown in Tables A-3 and A-4 in Attachment 1.

There are seven economic variables in the several Fuss models, in addition to the dummy variables he used. These are:

CPT	Change in telephone input price index, 1949-1993, Christensen data
CPE	Change in national economy input price index, 1949-1993, Christensen data
CPDIFF	Difference between LEC input price changes and economy input price changes, 1949-1993, Christensen data
NPT	Change in telephone input price index, 1960-1993, NERA data
NPE	Change in national economy input price index, 1960-1993, NERA data
NPDIFF	Difference between LEC input price changes and economy input price changes, 1960-1993, NERA data
MOODY	Moody's 30 year utility bond rate, 1949-1993

The models from which Fuss derives his conclusions are specified as follows (the error terms are omitted for simplicity):

(4.1)
$$CPT=c_0+a_{US}CPE+a_DDUMMY+a_MMOODY$$

(4.2)
$$CPDIFF=d_0+d_DDUMMY+d_MMOODY$$

(4.3)
$$NPT=c_0+a_{US}NPE+a_DDUMMY+a_MMOODY$$

(4.4)
$$NPDIFF=d_0+d_DDUMMY+d_MMOODY$$

Each of these equations is estimated by ordinary least squares. Concerns with the time series properties of such single equation models have come to the forefront of econometric analysis in recent years. It is now conventional best practice to test the time series properties of regression models for the assumptions that must hold for valid inferences to be made about the estimated coefficients. Otherwise, the model may be affected by spurious regression, and lead to false inferences. *The inferences from these equations concerning the relationships among variables are valid only if the residuals from the regression are stationary* (Hamilton, 1994, pp. 557-62; Enders, 1995, pp. 216-22). The demonstration of the spurious regression problem and its description are generally credited to Engle and Newbold (1974) and its theoretical elaboration to Phillips (1986). There is a variety of ways to test whether the residuals from the regression are stationary. We have performed the classic Engle-Granger cointegration tests, and the results show that the variables in the

Fuss regressions are not cointegrated. The inferences from those regressions are therefore likely to be invalid.

a. Results of time series analysis of variables in the Fuss equations

A reason for the failure of the cointegration tests of the Fuss equations may be that the time series structure of the equation is misspecified. Box-Jenkins analysis of CPT and NPT suggests that both variables are generated by ARMA (1,1) processes, whereas the corresponding equations estimated by Fuss based on CPT and NPT have no time series structure, i.e., no autoregressive (AR) and no moving average (MA) component. Unit root tests of the variables, shown in Tables A-5 and A-6 (Att. 1), reveal relatively high probabilities of unit roots for all variables except for CPDIFF and NPDIFF, the differences between the telephone and national price index changes in the Christensen and NERA data, respectively. This result suggests that the variables may be integrated of the same order, a prerequisite for cointegration.

When these cointegration tests are performed, the variables for each equation are shown to be not cointegrated. We performed Engle-Granger cointegration tests for each equation specification in the sequence of dummy variable specifications that Fuss estimated for equations 4.1 through 4.4. As the results in the tables show, most of the specifications are not cointegrated. The results are interpreted as follows: For each equation specification, the results of the cointegration are reported. The first entry is the test statistic from the test. The second entry is the probability that the residuals of the specification have a unit root, i.e., that the variables in the model are not cointegrated. The third entry shows the number of lags in the optimal cointegration equation. The optimal structure is that