the desirability of refining TFP results "to reflect the intuited greater degree of interstate productivity."[8] As explained by NYNEX:

> However, the interstate market is based on output growth that reflects revenues primarily generated by MOU growth, as compared to intrastate which reflects a significant portion of output growth generated by slower line growth.[9]

Interestingly, NYNEX announces in its comments that it "looks forward to working with the Commission and other parties in this proceeding to develop appropriate, economically based refinements to the TFP results," but does not offer any specific methodological suggestions nor provide any indication of the process by which such a refinement would actually be developed in this proceeding.[10] Ameritech similarly indicates a willingness to consider an adjustment for an interstate output growth factor but fails to put forward any positive recommendation as to how such an adjustment could be made.[11]

Notwithstanding NYNEX and Ameritech's apparent enlightenment on the subject of interstate vs. total company productivity, both LECs tow the party line by endorsing the Christensen study, as NYNEX puts it, as "the best starting point in determining an appropriate Baseline productivity factor."[12] Given NYNEX's explicit recognition of the greater degree of *interstate* productivity, NYNEX's endorsement of the Christensen *total company* TFP result as the appropriate "baseline" productivity factor should be dismissed as illogical.[13] Actual interstate TFP measures reflecting interstate-only output growth have

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8. NYNEX Comments, p. 13.

9. *Id.*, p. 20.

10. *Id.*, p. 13. NYNEX's offer to work with the Commission and other parties in this proceeding to develop an "economically based refinement to the TFP result" is a curious one. While NYNEX is certainly able to communicate with the Commission on an *ex parte* basis, that process does not readily lend itself to working with other parties in the proceeding. NYNEX's failure to present a specific proposal for an interstate additive in its initial comments effectively deprives other parties a meaningful opportunity to respond to any subsequently-presented NYNEX proposal.

11. Ameritech Comments, p. 7.

12. NYNEX Comments at 13.

13. Inasmuch as the FFNPRM is directed at developing a *permanent* X-factor, the suggestion that a total company result (per Christensen) can serve as a "baseline" until the "correct" interstate-only TFP methodology can be adopted should be seen, at a minimum, as an admission by NYNEX that an X-factor that is based upon total company productivity growth understates the value that is appropriate for the interstate jurisdiction. It is also a

(continued...)

been calculated in both the ETI and Norsworthy studies and were introduced in the record in this proceeding by Ad Hoc and by AT&T, respectively. There is simply no logical basis for relying upon a total company result as the "baseline," when there are reasonable interstate TFP estimates available.

In the case of the ETI study, the data used to develop an interstate-only output measure is the very data set relied upon by Christensen himself. Specifically, the Christensen/USTA model identifies three distinct categories of interstate activity: interstate switched access, interstate special access, and interstate end user access. The total company output index used by Christensen in the calculation of TFP is comprised of deflated revenues for these three interstate-only service categories in combination with a number of other service categories. However, it is a straightforward process to develop an index of output growth which encompasses only the interstate categories.[14] Thus, the USTA assertion that interstate output cannot be separately measured is belied by its very own data.

In the case of the Norsworthy study, three types of physical units of outputs associated with interstate revenues, i.e., access lines (for end user common line activity), interstate access minutes, and special access lines, are used to measure interstate output directly.[15] As we noted in our earlier report, direct measurement of physical output, such as that utilized in the Norsworthy study, is preferable to Christensen's deflated revenue method.[16] For purposes of our report, however, Christensen's deflated revenue method was used, in part for reasons of data availability, and in part to demonstrate the ability to estimate separate interstate and state output growth rates using the Christensen deflated revenue method.

In any event, the record in this proceeding contains these two distinct methods of calculating an interstate-only output growth measure – the deflated revenue method used by ETI and the direct physical output method used by Norsworthy. Thus, it is abundantly clear that interstate-only output can be (and has been) readily measured. The calculations of interstate-only output presented by Ad Hoc and AT&T fully negate the "can't be done" rhetoric characterizing USTA's position on this issue.

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13. (...continued)
transparent dilatory tactic whose effect, if successful, would be to permit the LECs to capture undeserved revenues while NYNEX and its sister LECs "work[] with the Commission and other parties in this proceeding to develop appropriate, economically based refinements to the TFP results."

14. See ETI Report, p. 55.

15. Statement of Dr. John R. Norsworthy, *Analysis of TFP Methods for Measuring the X-factor of the Local Exchange Carriers' Interstate Access Services*, Appendix A to AT&T Comments, p. 23.

16. See ETI Report, pp. 17-18, 50.

With respect to the measurement of input growth, both the ETI and Norsworthy studies demonstrate that input growth in the interstate jurisdiction can be reasonably approximated by total company input growth.[17] By assuming inputs grow at the same rates for all classes of services, it is not necessary to make a specific allocation of costs to interstate services.[18] Thus, one of the principal arguments proffered by USTA and the LECs in opposition to the calculation of an interstate-only TFP measure, namely that there is no economically meaningful allocation of inputs between interstate and intrastate services,[19] is simply irrelevant.

Moreover, as explained by Dr. Norsworthy, "there should be greater economies of scale in the LECs' provision of interstate access than in their other telephone services."[20] Accordingly, the "resulting implied allocation of costs" associated with the assumption that inputs grow at the same rates for all classes of services results is *conservative*, i.e., biases the TFP in the downward direction.[21] The conservative nature of the uniform input assumption is also confirmed by ETI's analysis which applied separations factors to LEC inputs to separate interstate and intrastate quantities.[22] Thus, further refinements to directly allocate input costs to the interstate jurisdiction would only serve to increase the interstate-only TFP measures calculated in the ETI and Norsworthy studies.

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17. ETI Report, pp. 49-50; Norsworthy Statement, Appendix A, p. 27.

18. It would seem quite disingenuous for USTA to argue against the use of total company input growth as an approximation of input growth in the interstate jurisdiction, at the same time supporting the Christensen study which uses cost of capital and depreciation rates based on the US economy as a whole as proxies for critical LEC input parameters, particularly when there is a so much stronger rationale for the former.

19. See Christensen "simplified" study, pp. 26-27.

20. Norsworthy Statement, Appendix A, p. 27.

21. *Id.*

22. In our earlier report, ETI undertook to examine the sensitivity of our results (assuming uniform input growth) to the results that would be obtained were separations factors applied for each year included in the study. Over the period 1991-1994, the percentage of LEC costs assigned to the *interstate* jurisdiction, while relatively stable, actually showed a decline (from 24.35% in 1991 to 23.70% in 1994). See ETI Report, Table 2, p. 50. Consequently, use of a uniform input growth assumption, while a good approximation, actually *overstates* input growth in the interstate jurisdiction, and accordingly *understates* interstate TFP. Thus, the uniform input growth assumed by both ETI and Norsworthy were decidedly on the conservative side in terms of the impact of this assumption upon the resulting interstate TFP values.

Acceptance of USTA's argument that an economically meaningful measure of interstate TFP cannot be developed would give the Commission no choice but to reject the TFP approach altogether.

In arguing in support of the Commission's reliance on a total company TFP measure in calculating the X-Factor, USTA and the LECs would have the Commission ignore Part 36 and the legal standard[23] upon which it is based. As we noted in our earlier report, the identification and estimation of a separate interstate TFP for purposes of establishing a separate interstate X-factor and Price Cap Index *is no less "economically meaningful"* than the Commission's long-standing practice of identifying and allocating investment and operating cost as between the interstate and state jurisdictions. If one accepts USTA's argument that an interstate-only productivity measure is not economically meaningful because "jurisdiction is a political distinction only" and "[a]rbritary regulatory boundary lines have no economic meaning or basis with regard to the input or output components of the production function,"[24] *those very same arguments would also hold with respect to cost of service/rate of return type regulation as well.* Under RORR, state and federal regulators have been required to apply precisely the same "[a]rbritary regulatory boundary lines" as USTA insists would be needed to develop jurisdiction-specific TFP measures.

USTA's and its members' efforts to play off one jurisdiction against another and to argue for the use of a total company productivity measure on the basis of alleged "impossibility" of calculating jurisdiction-specific values have no economic or policy merit. The prevailing separations rules are an exogenous *fact* of the prevailing regulatory paradigm, and are not negated or even modified by the initiation of price cap regulation. USTA and its members may rightfully cite FCC adoption of an *interstate* TFP in arguing in state proceedings that *states* must adopt *intrastate* TFP results in setting their respective intrastate X-factor. However, LECs cannot and should not be permitted to claim that total company results must be used due to some sort of "impossibility" theory, a notion that is indisputably belied by the presentation of interstate TFP methodologies by both Ad Hoc and AT&T in this proceeding.

USTA's attempt to justify Commission reliance on a total company TFP on economic grounds should be seen for what it is – a thinly-veiled effort to game the system and to permit LECs to keep windfall profits in the interstate jurisdiction, while claiming poverty before state regulatory commissions. As explained in our earlier report, LECs will be all

23. *Smith v. Illinois Bell Telephone Co.*, 282 US 133 (1930).

24. USTA Comments, pp. 28-30.

but guaranteed an interstate windfall if the Commission adopts USTA's total company TFP method and ignores state regulation of rates as urged by USTA.[25]

If, on the other hand, the Commission accepts USTA's argument that it is not possible to develop an interstate-only LEC TFP measure (despite the fact that, as discussed above, USTA's argument has been shown to be false by the studies sponsored by Ad Hoc and AT&T in this proceeding), then as long as the jurisdictional cost separation requirement is imposed by law, the Commission will have no choice but to reject altogether the use of the TFP method in calculating the X-Factor. Instead, the Commission must adopt one of the simpler methods such as the realized earnings method, which calculates a jurisdictionally interstate measure of productivity without any of the controversy surrounding the interstate-only TFP measure.[26]

Input Price Differential

Like the previous studies submitted by USTA in this proceeding, the new Christensen "simplified" study erroneously assumes a zero input price differential.

The new Christensen "simplified" study, like the original Christensen studies submitted in this proceeding, uses a *short-term* post-divestiture input price data series for purposes of calculating LEC TFP, but a *long-term* pre- and post-divestiture input price series for purposes of calculating the input price differential. Indeed, the "simplified" study uses an even shorter (six year) data series (for the study periods 1988 to 1993 and 1989 to 1994) for purposes of calculating LEC TFP than the original studies, which utilized a study period

25. ETI Report, pp. 48-49. USTA's suggestion that state commission reliance upon intrastate-only results is, to say the least, disingenuous. First, having *asserted* the impossibility of developing an *interstate*-only TFP result, USTA can hardly argue that the Commission should ignore state commission use of intrastate data even if this Commission's reliance is merely for the purpose of impeaching USTA's claims. But members of USTA have argued before their respective state regulators that *only* intrastate – and not total company – results are relevant in a state proceeding. See, ETI Report, footnote 149, citing Rebuttal Testimony of Richard G. Petzold (Bell Atlantic-DC), p.18, District of Columbia Public Service Commission, Formal Case No. 814, Phase IV, September 15, 1995. While USTA and its members take fundamentally inconsistent positions depending on the jurisdiction, the *correct* answer is unambiguous: Intrastate costs (and productivity growth rates) are relevant at the state level, and interstate costs and TFP growth are relevant in setting interstate rates.

26. See ETI Report, p. 15.

encompassing the entire post-divestiture period (originally the nine years 1984 to 1992, later updated to include 1993).[27]

As detailed below, there is neither a theoretical nor empirical basis for Christensen's reliance on one (i.e., short-term) study period for purposes of calculating LEC TFP, and another (i.e., long-term) study period for purposes of calculating the LEC-US input price differential. There is, however, an obvious pecuniary motivation for USTA's persistent reliance upon long-term input price data series for purposes of calculating an input price differential. By doing so, as in his original study, Christensen is able to make the erroneous assumption of a zero input price differential, the consequence of which is an understated productivity offset and a correspondingly excessive annual price cap rate adjustment.

Based upon the very same data used by Christensen in the original study to calculate LEC TFP for the post-divestiture period, ETI calculates an input price differential for the post-divestiture period 1984 to 1993 of 2.1%.[28] After correcting Christensen's data for a number of errors, the resulting input price differential for the 1984 to 1993 post-divestiture period increases to 3.4%.[29] Similarly, Dr. Norsworthy calculates an input price differential of 2.54% for the post-divestiture period 1985 to 1994, based on his direct computation of labor, capital, and material input price indices for the LECs.[30] Thus, USTA's failure to include an input price differential in its calculation of the X-factor (based on the rationale that there is no long-term difference) is worth in the range of $5-billion to $7.5-billion in cumulative revenues to the LECs over the next four years.[31]

As discussed in ETI's earlier report, general principles of competitive markets support the concept of relying upon a short-term input price differential (i.e., such as the post-

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27. The inappropriateness of the "simplified" study's reliance on the truncated study period for purposes of calculating LEC TFP is discussed further below.

28. ETI Report, Table 4, p. 56. Christensen has not provided comparable input price data in the new "simplified" study that would readily permit the calculation of input price growth over the entire post-divestiture period. Ad Hoc submitted information requests to USTA seeking this data, but USTA has declined to provide it in a form that would permit the necessary replication. This matter is discussed further in Section 4 of this report concerning empirical requirements.

29. *Id.*

30. Norsworthy Statement, Appendix A, p.71.

31. As derived in the earlier ETI report (footnote 39, p. 10), each 0.1% change in the X-factor represents roughly $250-million in cumulative LEC interstate revenues over a four year period. Accordingly, a reduction in the X-factor in the range of 2% to 3% attributable to the input price differential translates roughly into between $5-billion and $7.5-billion in cumulative LEC revenues over a four year period.

divestiture time frame) as opposed to any long-term trend.[32] Moreover, important (and indisputable) structural changes occurred in the telecommunications industry at the time of divestiture that render pre-1984 data non-comparable to post-1984 data and provide a strong theoretical basis for recognition of a structural break in the data.[33] That USTA and the LECs would argue that the Bush and Uretsky analysis was somehow arbitrary in the selection of 1984, the year of the largest industrial restructuring in US history, as the place to test for a structural break in the data (by introducing a dummy variable in 1984),[34] is truly audacious, *particularly when USTA now supports the use of a truly arbitrary five-year period in the "simplified" version of its TFP work-up.* USTA and the LECs argue that Bush and Uretsky misuse the dummy variable technique but, as discussed in more detail below, it is USTA's experts that replace the theoretically sound hypothesis of there being a *permanent* structural change following divestiture with a variety of theoretically unsound hypotheses alleging *temporary* shifts that are necessarily of far less moment than the break-up of the Bell System.

USTA argues that movement of input prices represents "random noise: short-term changes ... equally likely to be followed by short-term differences in the opposite direction." However, in Christensen's new study, as mentioned above, USTA relies upon *even shorter-term* measures of input quantity growth for purposes of calculating TFP. As explained in our earlier report, TFP studies are based upon the fundamental accounting/economic identity: *expenditures (on inputs or outputs) equal prices times quantities.* For a study such as Christensen's to be valid, the three variables – expenditures, prices, and quantities – must be internally consistent. Data on any two of the three variables – expenditures, prices, or quantities – can (and is, in the Christensen analysis) used to derive the third unknown variable. It simply makes no economic sense to argue that the quantity data underlying an expenditure series is valid, but that the derived price data is not. This fundamental identity holds true regardless of whether input quantities are measured directly (as in the case of the Norsworthy Study) or computed indirectly by deflating input expenditures by input prices (the method used in the Christensen studies).

That Christensen is able to make this nonsensical argument – i.e., that the quantity data in his study can be relied upon because it is measured directly but that the derived price data cannot – is strictly an artifact of the index he has chosen to use. As pointed out by Dr. Norsworthy, the index used by Christensen, the Tornquist Index, "is a poor choice for exacting productivity measurement because its results vary according to whether the TFP is

32. ETI Report, p. 32. As we noted, individual firms react to the prices that they *currently* pay, and that their competitors also confront, for their inputs.

33. *Id.*, p. 32.

34. See, e.g., GTE Comments, p. 12.

computed from the price or quantity side."[35] As explained by Dr. Norsworthy, the Tornquist method produces the unstable result that the input price index obtained by dividing total expenditure on inputs by the input quantity index is different from that which would be obtained by aggregating the input prices of the LECs directly.[36] By contrast, the index used by Dr. Norsworthy, the "Fisher Ideal Index," produces an input price index that "is the same, whether it is inferred from the quantity index or computed directly."[37]

The statistical machinations presented by USTA and the LECs do not disprove the findings of FCC economists Bush and Uretsky that a post-divestiture input price differential exists and should be included in the calculation of the X-Factor.

USTA and a number of LECs respond to the questions raised in the FFNPRM concerning the input price differential with a number of computations submitted to rebut the empirical analysis performed by FCC Common Carrier Bureau economists C. Anthony Bush and Mark Uretsky.[38] As described in ETI's earlier report, *based on numerous statistical tests of the data, Bush and Uretsky confirmed that pre-divestiture input price conditions were not applicable to the post-divestiture period, and that an input price differential based upon post-divestiture data only should be included in the calculation of an X-Factor for the post-divestiture period.*[39]

The analyses submitted by USTA and the LECs rely upon a variety of different statistical techniques both simple and complex, but they are all very similar conceptually in that they purport to demonstrate the same result, i.e., that the long-term trend of the input price differential is zero and that any short-term input price differential measurable in the post-divestiture period is only a temporary shift not appropriately reflected in a permanent X-Factor. However, as discussed in more detail below, none of the statistical machinations

35. Norsworthy Statement, Appendix A, p. 21.

36. *Id.*, p. 72.

37. *Id.*

38. USTA (in the analysis of NERA) as well as Lincoln Telephone Company, present simple linear regression analyses in response to Bush and Uretsky. Bell Atlantic (in the analysis of Dr. Mel Fuss) presents minimization of the standard error of regression (SER) analyses, and non-nested hypothesis analyses including "J Tests" and "Cox Tests." GTE (in the analysis of Dr. Gregory Duncan) presents ARIMA time series analyses in response to Bush and Uretsky.

39. See ETI June 1994 Study, at 5-7; also see, C. Anthony Bush and Mark Uretsky, "Input Prices and Total Factor Productivity," ("Bush/Uretsky analysis"), *First Report and Order*, Appendix F, at 1.

presented by USTA and the LECs convincingly negate the Bush and Uretsky findings. In particular, none of the analyses submitted in rebuttal to Bush and Uretsky disprove the significance of divestiture in marking a structural change in the long-term input price trend for the LECs vis-a-vis the US economy generally, and therefore do not undermine the fundamental correctness of applying the post-divestiture input price differential in the calculation of a permanent X-factor. Indeed, if anything, the various analyses submitted by USTA and the LECs taken as a whole serve to strengthen the finding of a significant divestiture effect.

All of the analyses submitted by USTA and the LECs suffer from a number of common, and indeed fatal, flaws. First, like the previous analyses submitted by USTA in the earlier phase of this proceeding (in fact, the present USTA submissions are largely rehashes of the earlier analyses), the analyses presented in this phase of the proceeding are not designed to test the *correct* hypothesis as examined in the Bush and Uretsky analysis, i.e., whether there are in fact significant differences between movements of telephone input prices before and after divestiture. Rather, they create and test a variety of strawman hypotheses, which they claim demonstrate an equality between LEC and US economywide input prices. As discussed below, they do not.

Second, all of the various analyses rely on the same underlying LEC and US input price growth data. These data sets are neither objectively-determined nor verifiable, but rather were generated either by Dr. Christensen or internally within the Bell System.[40] In addition, these data sets do not incorporate quality-adjusted changes and other necessary corrections highlighted in this and our earlier ETI report – corrections which serve overall to *reduce* measured LEC input price growth vis-a-vis input price growth for the US economy as whole. These data sets also include, without justification or explanation, an outlier data point to which, as demonstrated below, can be attributed many of the purported findings of the statistical analyses presented by USTA and the LECs.

One set of statistical analyses performed for USTA by NERA and Christensen test whether one can *reject* the hypothesis that the LEC-US input price differential is zero.[41] These are the very same t-statistic tests submitted by USTA in the earlier phase of this proceeding and which Bush and Uretsky address and unequivocally dismiss in their analysis

40. See Christensen Appendix 3, pp. 41-42. USTA has had the opportunity, but has not provided information that would allow full replication and documentation of the results of these studies. See discussion in Section 4 of this report on empirical requirements.

41. NERA study, *Economic Evaluation of Selected Issues from the Fourth Further Notice of Proposed Rulemaking in the LEC Price Cap Performance Review*, Attachment C to USTA Comments, pp.4-5; Christensen Appendix 3, pp. 46-51.

of the input price differential issue.[42] In their current submissions, Christensen and NERA attempt to resurrect these previously-considered (and rejected) analyses by highlighting the distinction between the data set relied upon in NERA's earlier analysis and examined in more detail by Bush and Uretsky (so-called "Christensen 2"), and the data set relied upon in the February 1, 1995 Christensen Input Price Affidavit (so-called "Christensen 1").

Christensen and NERA are quite united in this new attempt to discredit the Christensen 2 data set, notwithstanding the fact that this data set was endorsed and used by NERA in the earlier phase of this proceeding, and based on earlier studies by Christensen himself previously submitted in a price cap proceeding before the North Dakota Public Service Commission.[43] Christensen and NERA both now argue that the Christensen 2 data set relied upon by NERA and examined in more detail by Bush and Uretsky is an inferior data set vis-a-vis Christensen 1. According to Christensen and NERA, "the latter data set provides the most theoretically consistent telephone input price time series available."[44] The stated rationale for this sudden renunciation of the Christensen 2 data set is its reliance on input data from a Christensen North Dakota study, and in particular, the manner in which that study measured the value of capital.[45]

What makes the NERA and Christensen renunciation of the Christensen 2 data set particularly bogus is that the Christensen 1 data set, now heartily endorsed by the USTA experts, *also* relies on the Christensen North Dakota study for selected years (albeit not as many). In addition, the apparently discredited method used in the North Dakota study to calculate the value of capital is essentially the same "internal rate of return" method reflected in the US cost of capital now being used by Christensen in the new "simplified" study as a proxy for LEC capital costs. Interestingly, when the LEC-US input price differential associated with the new "simplified" study is substituted for the original study results for the 1988 to 1992 time period, the NERA/Christensen finding that a zero price differential cannot be rejected, falls apart. In other words, when one substitutes the data on the input price differential from the new "simplified" study, one can squarely reject the hypothesis of a zero input price differential.[46] Whether this unfavorable statistical result

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42. Bush and Uretsky analysis, pp. 11-14.

43. See Christensen Appendix 3, pp. 40-42, NERA Report, pp. 3-4.

44. *Id.*, pp. 41-46; NERA Report, p.4.

45. As explained by Christensen, the North Dakota study measured the value of capital as revenue less labor and materials cost. See *Id.*, p. 43.

46. The t-statistic on the LEC-US price differential calculated using data from the "simplified" study is 2.45, as compared with the critical value of 2.36 (for a 95% confidence interval). By contrast, the t-statistic reported by NERA and Christensen, based on the original study data, is 1.3. See Table A1, Appendix A to this report.

will lead NERA and Christensen to abandon the new "simplified" data set, as they have done with the Christensen 2 data set, remains to be seen.

The fact is that neither data set, Christensen 1 nor Christensen 2, is adequately supported or verifiable, since they were both generated based on unsupported Christensen methodology and data development or internally-generated Bell System studies. As such, results of statistical analyses based upon this data cannot be relied upon.[47] In addition, both data sets (as well as the new "simplified" study data) would appear to suffer from the same critical data deficiencies, perhaps the most important of which is their failure to reflect hedonic or quality adjustments in the LEC input prices series. The significance of hedonic price changes in the telecommunications industry in both the pre- and post-divestiture periods was discussed at length in the earlier ETI Report as well as in the Norsworthy study.[48] As shown by ETI and Norsworthy, failing to reflect quality adjustments results in a gross overstatement of LEC input price growth, and accordingly an understatement of the LEC-US input price differential as measured over both the long-term and short-term.[49] Thus, the statistical tests offered by both NERA and Christensen in rebuttal to the Bush and Uretsky analysis using either Christensen 1 or the self-discredited Christensen 2 must be dismissed by the Commission because of their failure to accurately reflect quality-adjusted LEC input price growth on either a long-term or short-term basis.

In this context, it is clear that NERA's glib assertion that "[t]here is no genuine dispute in this proceeding that the long-term rate of growth of LEC input prices is the same as US input price growth" is simply not true. With quality-adjusted changes, there is little doubt that a more significant difference between LEC and US input price growth data would emerge, even as measured over the long-term. Because USTA did not provide the data required to replicate the long-term LEC and US input price growth series reflected in Christensen 1 and 2 either with its filing or on a timely (and orderly) basis in response to requests from Ad Hoc and other parties, it has not been possible to quantify the effect of incorporating quality-adjusted changes on Christensen 1 and 2. However, since hedonic adjustments would have the effect of lowering LEC input price growth vis-a-vis the US economy as a whole, it is reasonable to expect that the effect of incorporating quality-adjusted changes in Christensen 1 and 2 would be to widen the differential already evident

47. Notwithstanding clear directives by the Commission in Paragraph 15 of the FFNPRM, USTA did not submit, as part of its filing, the workpapers and other supporting data for the various studies relied upon by Christensen and NERA for the LEC and US input price data reflected in Christensen 1 and 2. While Ad Hoc and AT&T submitted data requests seeking the information necessary to permit replication of the input price results, the data provided in USTA's response does not readily permit replication. This matter is discussed further in Section 4 on empirical requirements.

48. See ETI Report, pp. 36-42; Norsworthy Statement, Appendix A, pp. 50-58.

49. *Id.*

in the unadjusted LEC and US input price data series, thereby providing additional grounds on which to reject the results of Christensen and NERA's statistical tests purportedly demonstrating no long-term input price differential.

Moreover, as pointed out by Dr. Norsworthy, by framing the question in the way that Christensen and NERA do, i.e., by *assuming LEC and US input prices are the same – despite measurable differences in the point estimates of the two series of data*[50] – and by testing whether one can *reject the assumption they are the same* (at some arbitrary level of statistical significance), USTA's experts create "a bias toward accepting the assumption that there is no difference between the movements of the two series of input prices."[51]

This same bias also appears in the time series analysis performed by Dr. Gregory Duncan for GTE. The Duncan/GTE analysis similarly frames the question in the negative, such that the only favorable conclusion that can be reached is that the data does not permit *rejection* of the hypothesis that the input price differential is zero.[52] Thus, like the NERA and Christensen analyses, the Duncan analysis does not demonstrate in the affirmative that LEC input price growth equals that of the US economy as a whole, or disprove the measurable difference between LEC and US input price growth that is readily observed;[53] at the very most, it suggests that (at some level of statistical significance), the posited equality between LEC and US input price growth rates is not an impossibility.

Another set of statistical analyses performed by NERA tests the hypothesis that the structural change in the data at divestiture found by Bush and Uretsky was a temporary change, reversing itself in the 1990-1992 time period.[54] NERA tests this "temporary shift" hypothesis by performing a regression analysis in which another dummy variable is introduced to the regression equation run by Bush and Uretsky. Using the Christensen 1 data set, this new dummy variable takes on the value of 1 for the years 1990, 1991, and 1992, and 0 for the years 1949 to 1989. Using the Christensen 2 data set, the dummy variable takes on the value of 1 for the years 1990, 1991, and 1992, and 0 for the years 1960 to 1989. Because this new dummy variable is statistically significant, and the

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50. For example, the mean input price change for the LEC as shown in the Christensen data for the period 1985 to 1992 is only 1.7%, as compared with a mean input price change for the US economy as a whole of 4.0% for that same period. See NERA Report, p. 5.

51. Norsworthy Statement, Appendix A, pp. 8-13.

52. See NERA Report, pp. 7-8, citing Direct and Reply Testimony of Dr. Gregory Duncan on behalf of GTE California. Dr. Duncan's testimony is attached in this proceeding to GTE's Comments.

53. See footnote 51, *infra*.

54. NERA Report, p.9, also Attachment A.

regression incorporating the new dummy exhibits stronger statistical properties (e.g., t-statistics, F-statistics, R-squared), NERA argues that this is evidence that "a one-time deviation from historical norms has reversed itself and that US and LEC input price changes should now again approximately equal one another."[55] NERA's analysis proves no such thing.

First, with NERA's addition of the new dummy variable purporting to represent the "temporary shift," the dummy variable for divestiture representing the Bush/Uretsky "permanent shift" hypothesis becomes even stronger.[56] This result provides important evidence of the robustness of the structural change at divestiture and further support for the calculation of the input price differential using post-divestiture data.

Second, closer examination of the data reveals that the dummy variable introduced by NERA purportedly to measure the so-called "temporary shift" hypothesis is really measuring, or in effect, compensating for, an outlying data point for the year 1990.[57] As Dr. Norsworthy points out, "the unusual difference in the year 1990 accounts for a large part of the standard deviation for the 1985-92 period."[58] Thus, correction for this outlier, more appropriately by its removal, or in the alternative, by the introduction of a dummy variable encompassing this data point as occurs in the NERA regression, understandably improves the statistical properties (e.g., t-statistics, F-statistics, R-squared) of the regression analysis. Any further meaning, such as the "temporary shift" hypothesis attributed to these regression results by NERA, is a fabrication.

As further evidence of this point, when the 1990 outlier is removed from the data series, the original Bush/Uretsky permanent shift hypothesis regression equation, i.e., the equation including only the one dummy variable for divestiture, performs as strongly as

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55. *Id.*

56. For example, in the regression using Christensen 1 Data and the dependent variable "LEC input price change," the coefficient of the Divestiture variable increases from -.0579 to -.0851, and the corresponding t-statistic increases from -3.8142 to -5.3981. These regression results are replicated in Tables A2, Appendix A to this report. Similar results are obtained in the other three regressions run by NERA using either different data sets or dependent variables, i.e., the regressions run with Christensen 1 Data and the dependent variable "LEC-US input price growth differential;" Christensen 2 Data and the dependent variable "LEC input price change;" and Christensen 2 Data and the dependent variable "LEC-US input price growth differential." See NERA Report, Attachment A.

57. As shown in NERA Attachment A, the LEC input price change jumps from a *minus* 3.7% in 1989 to a *positive* 11.9% in 1990 and then back down to 1.3% in 1991, by and far the largest one-year swing in the entire series of LEC input price change over the 1949 to 1992 period. When expressed in terms of the LEC-US input price growth, the 1990 value is a *positive* 7.7%, as compared with a *negative* 7.8% in 1989 and a *negative* 1.6% in 1991.

58. Norsworthy, p. 10.

NERA's "temporary shift" hypothesis regression equation, i.e., the equation including both the dummy variable for divestiture and the dummy variable for the years 1990 to 1992.[59] In addition, when the 1990 outlier is removed from the data series in NERA's "temporary shift" hypothesis, the dummy variable representing the "temporary shift" *is no longer significant* using the Christensen 1 data set preferred by NERA.[60]

The 1990 outlier similarly refutes the results of the regression analyses run by Lincoln Telephone Company. In the Lincoln analysis, a number of variations on the Bush and Uretsky regression equations are run, each incorporating dummy variables for a number of different years. In the first regression equation, the dummy variable for divestiture, which in the Bush and Uretsky analysis carried a value of 1 for each of the years 1984 to 1992, is altered to carry a value of 1 for the years 1984 to 1989, and a value of zero for the years 1990 to 1992. Because this particular regression produced stronger statistical results (i.e., higher t-statistics, F-statistic, and R Squared), Lincoln concludes that the observed divestiture effect is not permanent. In the second regression equation, Lincoln starts with the first variation and adds to it a second dummy variable taking on the value of 1 in the years 1990 to 1992. Because the resulting coefficient for the 1990 to 1992 dummy is not statistically significant, Lincoln concludes that LEC input price changes from 1990 to 1992 are not statistically different from the pre-divestiture trend.

Lincoln's conclusions are misguided. As pointed out in the case of the NERA regression analyses, stronger statistical results (vis-a-vis the Bush and Uretsky equation) equivalent to those produced by the Lincoln "temporary shift" variation can also be produced by merely removing the 1990 outlier data point.[61] Given the utter lack of a theoretical foundation for Lincoln's choice of the years 1984 to 1989 as the period to impose a structural break in the data, the strong theoretical foundation for assuming different structural conditions extant throughout the entire post-divestiture period, and the more viable attribution of the stronger statistical results to interaction with the 1990 outlier, Lincoln's conclusions based on its first regression variation should be dismissed as

59. In the regression using the Christensen 1 data set and the dependent variable "LEC Input Price Change," the coefficient on the divestiture dummy variable increases from -.0579 to -.07159, the corresponding t-statistic increases from -3.8141 to -5.2000, the F-statistic increases from 10.1512 to 15.9046, and the R squared increases from .43 to 55. These statistical properties are nearly identical to those from NERA's temporary shift hypothesis equation. These regression results are replicated in Table A6 and A7, Appendix A to this report.

60. The t-statistic for the 1990-1992 dummy variable falls from 3.3658 to 1.9065 in the regression equation with the dependent variable "LEC Input Price Change," and from 2.9429 to 1.6018 in the regression equation with dependent variable "LEC-US Input Price Growth." Both of these t-statistics fall below the t-critical value of 2.021 at the 95% confidence level relied on by USTA's experts in their own analyses. These regression results are replicated in Tables A7 and A8, Appendix A to this report.

61. See regression results presented in Table A8, Appendix A to this report.

demonstrating little other than how easily statistical analyses can be used to prove just about anything.

Moreover, it is important to note that Lincoln's alternate regression does not in any way diminish the sound statistical results of the permanent change hypothesis as modeled by Bush and Uretsky. In effect, Lincoln would appear to be recommending quite inappropriately that the Commission disregard one regression analysis that has a very strong theoretical foundation (i.e., the Bush/Uretsky analysis) in favor of one that is lacking in theoretical rationale (i.e., Lincoln's temporary change hypothesis) strictly on the basis of somewhat higher R-squared and t-statistics results. For obvious reasons, such an approach is generally regarded with disfavor. There is no particular reason why maximization of R-squared, for example, should be the basis of a policy decision.

With respect to Lincoln's second regression variation, there is no basis for the meaning attributed by Lincoln to the insignificant t-statistic on the coefficient for the dummy for the 1990 to 1992 period. Because the dummy variable is applied to only three years, and one of those years is the 1990 outlier, a relatively large standard error of the coefficient is to be expected. A relatively large standard error of coefficient, in turn, is responsible for the insignificant t-statistic. Thus, the insignificant t-statistic on the 1990-1992 dummy variable coefficient is an artifact of the 1990 outlier and the inherent variability in the data, rather than demonstrative proof, as alleged by Lincoln, that the years 1990 to 1992 represent a return to the pre-divestiture trend. The arbitrariness of Lincoln's interpretation of an insignificant t-statistic on the 1990-1992 dummy variable is highlighted by NERA's regression analysis, in which NERA would interpret an insignificant t-statistic on the same 1990-1992 dummy variable to mean something entirely different.[62] The differing interpretations and multiple combinations of dummy variables inherent in the NERA and Lincoln analyses further highlight the statistical games that are being played with the data. The NERA and Lincoln analyses offer a sharp contrast to the Bush and Uretsky analysis in which the dummy variable technique is appropriately utilized to account for the known structural event of divestiture.

The analysis of Dr. Mel Fuss submitted in conjunction with the comments of Bell Atlantic is yet one more variation along the same theme of statistical machinations to confound and detract from the solid findings of Bush and Uretsky. Like Lincoln and NERA, Dr. Fuss too postulates that there are "two competing hypotheses" for the change in LEC-US input price growth rate differential in the post-divestiture period: a "permanent

62. As discussed *infra.*, in NERA's analysis, the *significance* of the added dummy variable for 1990-1992 is interpreted to support the temporary shift hypothesis, i.e., signify a shift back to the long-term relationship. By contrast, Lincoln interprets the *insignificance* of the added dummy variable for 1990-1992 as signifying a shift back.

change hypothesis" and a "temporary change hypothesis."[63] Like Lincoln and NERA, Dr. Fuss conjectures that the LEC-US input price differential observed by Bush and Uretsky was a temporary phenomenon during the 1984 to 1989 period that ended in 1990, with a return to the long-term relationship of no differential.[64] Dr. Fuss's analysis is conceptually the same as that presented by Lincoln – he just carries it to a higher art form, by performing more complex statistical techniques and concocting more obscure rationale for the two competing hypotheses. As was the case with the Lincoln analysis, closer examination of Dr. Fuss's analysis reveals a number of problems that invalidate the purported results.

In Dr. Fuss's "informal method of choosing between competing hypotheses," he performs essentially the same analysis as Lincoln; he compares the results of a regression equation where the dummy variable for divestiture takes on the value of 1 throughout the entire post-divestiture period with a regression equation where the dummy variable takes on the value of 1 just for the years 1984 to 1989, and points to what he characterizes as superior statistical results.[65] However, Dr. Fuss adds an additional layer of statistical sophistication by using a technique referred to as minimization of the standard error of the regression (SER) to justify selection of the year 1990 as the year in which the long-run relationship resumed.[66] Notwithstanding his use of a somewhat more sophisticated technique than Lincoln, the reason why Dr. Fuss's application of the SER technique "selected" 1990 is the same reason demonstrated above that the regressions run by Lincoln (and NERA) produced their results, namely 1990 is an outlier data point. Thus, no particular importance should be ascribed to the use of the SER criterion.

Dr. Fuss's "formal procedure" involves the use of the Davidson and MacKinnon J Test and the Cox Test, two similar methods of testing "non-nested hypotheses."[67] These

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63. Fuss Declaration, pp. 6-7.

64. *Id.*, p.3.

65. Like the Lincoln analysis, the Fuss analysis does not imply (nor could it) that the permanent change hypothesis as modelled by Bush and Uretsky produces unsound statistical results. Dr. Fuss, like Lincoln, is recommending quite inappropriately that the Commission disregard one regression analysis that has a very strong theoretical foundation (i.e, the Bush/Uretsky analysis) in favor of one that is lacking in theoretical rationale (Dr. Fuss's temporary change hypothesis) strictly on the basis of somewhat higher R-Squared and t-statistics results. As noted above, such an approach is generally regarded with disfavor as there is no particular reason why maximization of R-squared, for example, should be the basis of a policy decision.

66. In Dr. Fuss's application of the SER technique, the standard error of the regression (SER) is shown to be minimized at X=1990, where X is allowed to vary between 1985 and 1992. See Fuss, Table A1 and A2.

67. As stated by Dr. Fuss, non-nested hypotheses simply are the case where one hypothesis is not a special case of the other. Fuss, p. 6.

procedures permit Dr. Fuss to consider the following four outcomes: (1) rejection of both competing hypotheses; (2) rejection of neither competing hypothesis; (3) rejection of the permanent change hypothesis, but not of the temporary change hypothesis; and (4) rejection of the temporary change hypothesis, but not of the permanent change hypothesis. According to Dr. Fuss, the results of both the J Test and Cox Test support the conclusion that the permanent change hypothesis is not correct and that the temporary change hypothesis is.[68] While Dr. Fuss once again adds an additional layer of statistical sophistication with his use of the two non-nested hypothesis testing techniques, doing so does not compensate for the obvious errors or biases in the data described above.

A fundamental principle of empirical analysis is that an analysis is only as good as the data underlying it. Thus, whatever added statistical functionality is provided by Dr. Fuss's use of non-nested hypothesis testing techniques such as the J-Test or the Cox Test does not offset obvious errors or biases in the data set relied upon by Dr. Fuss. Dr. Fuss, as did Lincoln (as well as Christensen, NERA, and Duncan), relies upon the very same data sets, so-called Christensen 1 and Christensen 2. As discussed above, these data sets are unverified long-run input price series that do not incorporate quality-adjusted changes and other necessary corrections highlighted in this and our earlier ETI report – corrections which serve overall to *reduce* measured LEC input price growth vis-a-vis input price growth for the US economy as whole.

These data sets also include the 1990 outlier data point, which as indicated above, invalidate the so-called temporary shift hypothesis conjectured by other experts for USTA and the LECs. This same result applies to the Fuss Analysis, i.e., when the 1990 outlier point is omitted from the data set, we can now reject Fuss's "temporary change hypothesis."[69] As a consequence, Dr. Fuss's conclusion that his alternative "temporary change hypothesis" is the preferred explanation of the data (as compared with the Bush and Uretsky permanent change hypothesis) according to the J Test and the Cox Test cannot be substantiated on the basis of the uncorrected data set.

Second, setting aside the problems with the underlying data set, Dr. Fuss provides no evidence to support his stated rationale for the two competing hypotheses. Upon closer

68. *Id.*, pp. 8-9.

69. In terms of Dr. Fuss's jargon: when the 1990 outlier is omitted from the regression equation run using the Christensen 1 data set ending in 1992 on the dependent variable "LEC Input Price Growth," the N statistic for α for the "H2 is correct" hypothesis (i.e., temporary shift hypothesis) turns from -.58 to -2.25, such that it *can be rejected* at the critical 5% value of N of -1.96. Similar results, i.e., rejection of the "H2 is correct" hypothesis, are obtained for other regression equations considered by Fuss, including the regression equations run using the Christensen 1 data set updated to include data for 1993 on the dependent variable "LEC Input Price Growth," and the Christensen 1 data set updated to include data for 1993 on the dependent variable "LEC-US Input Price Growth." The results of these analyses are presented in Table A9 - A10, Appendix A to this Report.

scrutiny, Dr. Fuss's explanations do not hold up from either an economic or common sense standpoint. As explained by Dr. Fuss, the "permanent change hypothesis" is "due to a permanent increase in the rate of technological progress in the equipment industry which would have resulted in a continuing decline in the relative rate of increase in capital prices."[70] The "temporary change hypothesis," according to Dr. Fuss, is "due to a temporary decline in the rate of increase in equipment prices resulting from the erosion of the profitability of the formerly dominant equipment suppliers." Thus, while Dr. Fuss recognizes that telecommunications equipment markets became increasingly competitive following divestiture when the BOC's were prohibited from purchasing inputs internally, his theory would imply, quite incorrectly, that equipment markets after 1990 have stabilized at a new competitive equilibrium such that further declines in equipment prices will not occur.

However, Dr. Fuss presents no evidence to support his theory that divestiture caused a one-time shock to the equipment market. The equipment market is regarded as a highly competitive market, and if anything, one that has become even more competitive in terms of price discounting in recent years. Furthermore, in a competitive market subject to a high rate of technological innovation, such as the telecommunications equipment market, profits are constantly being eroded over time as innovation continuously takes place and subsequently mimicked by competitors.[71] Dr. Fuss presents no evidence to support his theory that the rate of technological progress in the telecommunications equipment market has slowed down in recent years. Indeed, Dr. Fuss appears to ignore entirely the substantial quality changes in the performance of capital that have characterized the market for telecommunications equipment, second only perhaps to the computer market, and show no signs of subsiding.[72]

Finally, Dr. Fuss presents no evidence as how his theories actually would be reflected in the particular data sets he relies upon in his statistical analyses. Dr. Fuss's theories apply only to LEC capital input prices, yet he applies his statistical tests to the aggregate LEC input price series. Moreover, Dr. Fuss's theories are belied by USTA's experts Christensen and NERA who attribute the reduction in LEC capital input prices to reductions in interest rates:

> In particular, the short-term difference in measured capital input prices reflect the fact that measured LEC capital input prices put a much larger weight on interest

70. Fuss Affidavit, p. 12.

71. See, e.g., F.M. Scherer, "Innovation and Growth: Schumpeterian Perspectives," MIT Press, 1984, Chapter 6; also William L. Baldwin and John T. Scott, "Market Structure and Technological Change," Harwood Academic Publishers, 1987.

72. See ETI Report, pp. 36-42; Norsworthy Statement, Appendix A, pp. 50-58.

rates than measured US capital input prices, and the fact that up until 1993 the post-divestiture period has been a time of declining interest rates.[73]

While we strongly disagree with Christensen's attempt to justify ignoring the obvious LEC-US input price differential implied by his very own study, his explanation is instructive in that it would totally rule out Dr. Fuss's unlikely equipment market theories.

In estimating the X-Factor, it is appropriate to use data available for the entire post-divestiture study period as opposed to only five years' worth of data, as proposed by USTA.

The preceding discussion reaffirms a strong theoretical and empirical rationale for estimating the X-Factor – including *both* the TFP and input price differential components – on a post-divestiture basis. Given the strong rationale for estimating the X-Factor on a post-divestiture basis, it is appropriate to use data available for the *entire* post-divestiture study period. USTA provides no particular rationale for artificially truncating the study period and using only five years' worth of data as reflected in USTA's five-year moving average TFP estimate.

In general, the more years of data (i.e., the greater the number of observations) that can be used in an estimating procedure, the better. For one thing, the greater the number of observations, the greater the degrees of freedom available for estimating and testing hypotheses.[74] For another, the greater the number of observations, the influence of possible errors in any one particular data point will be lessened.

Furthermore, USTA's truncation of the study period is at odds with its own position that "short-term changes in input prices should not be utilized in a predictive fashion."[75] While we strongly disagree with USTA (1) that pre-divestiture data should be relied upon for purposes of calculating the input price differential; and (2) that pre-divestiture data indicates a long-term LEC-US input price differential of zero, for the reasons set forth above, the basic concept inherent in USTA's argument, i.e., that data gathered over a longer time period will be more robust for predictive purposes, is valid *except where it can be*

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73. Christensen Simplified Study, p. 49.

74. Degrees of freedom are the number of free or linearly independent sample observations used in the calculation of a statistic, which is just the number of observations reduced by the number of parameters being estimated. See Peter Kennedy, *A Guide to Econometrics*, MIT Press, 3rd edition, 1993, p.57.

75. USTA Comments, pp. 26-27. USTA argues, for example, that "short-term changes are equally likely to be followed by short-term differences in the opposite direction."

shown that some fundamental change has occurred – such as occurred at the time of divestiture – so as to invalidate the use of data from specific time periods.[76] In effect, where there is evidence of a change in structural conditions (i.e., a structural break in the data), a new "long-term" data series begins. Accordingly, it is appropriate to rely upon the greatest number of relevant observations possible, which in this case is the entire post-divestiture period, including the years 1984 to 1994.

The new "simplified" Christensen/USTA study suffers from a number of the same errors that Christensen made in the original study with respect to the calculation of TFP.

The preceding sections of this report discuss two overarching errors in the Christensen study: (1) Its failure to consider jurisdictionally *interstate* productivity; and (2) its failure to recognize and incorporate an input price differential based upon the post-divestiture experience of the LECs. In addition to these two fundamental errors, the new "simplified" Christensen/USTA study suffers from a number of other errors made in the original study with respect to the calculation of TFP.

Measures of LEC Output

Deflated Revenue Approach

With respect to LEC output growth, the new study, as did the old, derives output quantities using a deflated revenue approach that relies on seemingly flawed output price indices. As discussed in the earlier ETI Report,[77] and as implemented in the Norsworthy study for AT&T,[78] the preferred approach is to measure output directly based upon physical quantities such as minutes of use and lines.

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76. This was, of course, precisely what the Bush/Uretsky analysis demonstrated – i.e., that pre- and post-divestiture conditions were sufficiently different so as to invalidate the use of the pre-divestiture series as a predictor of post-divestiture conditions. USTA offers no corresponding analysis to justify exclusion of all but a "recent" five-year period.

77. ETI Report, pp. 17-18.

78. Norsworthy Statement, Appendix A, pp. 23-26

Measures of LEC Input

Hedonic Adjustments

With respect to LEC input growth, as mentioned above, Christensen fails to make *hedonic adjustments* in the measurement of the capital input to reflect changes in the capabilities and/or capacity of LEC capital inputs. This error has particular significance with respect to the Christensen/USTA claim of a zero input price differential, since the result is an overstatement of LEC input price growth for the capital input vis-a-vis the US economy as a whole. This error accordingly produces an understatement of the X-factor. The importance of incorporating hedonic adjustments in the calculation of LEC TFP was discussed at length in both the ETI and Norsworthy studies.[79]

Cost of Capital

There are other problems with the "simplified" study's measures of LEC input growth. Perhaps the most obvious error is in the *cost of capital* utilized by Christensen in the new study. In the original study, Christensen used the Moody's average yield on public utility bonds as the proxy for the LEC cost of capital. As discussed in the earlier ETI Report, there was no valid economic rationale for relying solely upon a cost of debt as the LEC cost of capital.[80] USTA now appears to readily concede this point.[81] In his new study, Christensen has chosen to rely instead upon the US economy cost of capital implicit in US National Income and Product Accounts (NIPA) as a proxy for the LEC cost of capital.[82] In doing so, Christensen has replaced one poor measure of cost of capital with another equally poor measure.

USTA claims that the "simplified" Christensen TFP calculation utilizes the cost of capital implicit in the US NIPA because "there is no publicly verifiable time series for the opportunity cost of equity," and the alternative of "developing a widely-accepted annual data series on the opportunity cost of equity would involve complex and lengthy debates that would not be worth the time and effort."[83] As in the case of the measurement of an

79. See ETI Report, pp. 36-42; Norsworthy Statement, Appendix A, pp. 50-58.

80. ETI Report, pp. 18-19.

81. USTA Comments, p. 17.

82. Christensen "simplified" study, p. iii., p. 29.

83. USTA Comments, p. 17.

interstate-only TFP measure, USTA's attempt to justify Christensen's use of the US cost of capital as a proxy for the LEC cost of capital on the grounds of expediency is belied by actual solutions presented to the Commission by other parties in this proceeding.

In particular, Dr. Norsworthy in his study for AT&T utilizes an approach for calculating the cost of capital that is based upon publicly available data, is theoretically sound, and moreover, is consistent with BLS total factor productivity methods. Specifically, Dr. Norsworthy's Performance Based Model (PBM) treats the difference between total revenues and expenses for the labor and material inputs as a gross return to capital.[84] This gross return to capital per unit of capital inputs (i.e., divided by the net capital stock) is taken to represent the cost of the capital input. Thus, Dr. Norsworthy's approach is based upon the *actual* internal rate of return earned by the LECs as a measure of the LEC cost of capital, in contrast to Christensen's approach, which is based upon an *assumed* rate of return.[85]

As explained by Dr. Norsworthy, the USTA assumed rate of return model is based upon assumptions of full competition and of cost-minimizing levels of the capital stock, conditions that simply do not exist at the present time in the markets for telephone services provided by the LECs.[86] Because these assumptions are not being met (and competition cannot be counted upon to eliminate excess returns), residual amounts – either positive (corresponding to an excess return on capital consistent with the *less-than-fully-competitive* characteristics of the LEC industry) or negative – will result.[87] Dr. Norsworthy goes on to consider both the case of positive and negative residuals under the USTA model, and concludes that "the LECs would be likely to advocate the USTA assumed rate of return model if they expect to earn a higher actual rate of return than that assumed in USTA's calculation of the X-Factor."[88]

In fact, there is indisputable evidence, based upon recent LEC actions, that the LECs do expect to earn returns that are well in excess of the competitive level. In the *First Report and Order* in this proceeding, the LECs were offered three alternative combinations of X-factor and sharing obligations. As noted earlier, the majority of the LECs have elected to be subject to the highest (5.3%) X-factor. The election of 5.3% implies an expectation of

84. Norsworthy Statement, Appendix A, p. 19.

85. *Id.*, p. 37.

86. *Id.*, p. 37.

87. *Id.*

88. *Id.*, pp. 40-45.

earnings (at the 4.0% X-factor baseline) of between 13.24% and 13.43%, as summarized in the Table 1 on the following page.[89] That is, at earnings levels below these "break-even" points, it would have been in the electing LECs' interests to have chosen the 4.0% X-factor; indeed, even the break-even amount is highly conservative, because by electing the higher 5.3% X-factor the LECs are giving up (collectively) some $325-million in "up front" revenues in exchange for the *opportunity* to retain earnings in excess of what would have otherwise been the sharing threshold.[90] Thus, in order to justify the election, the earnings expectation had to have been well in excess of merely breaking even.

Because Dr. Norsworthy's approach allocates all revenues to some cost category and thereby eliminates the possibility of excess returns under conditions of less than full competition, it is a preferable alternative to the proxy approach used by Christensen.

Christensen alleges that year-to-year changes in telephone industry cost of capital should follow year-to-year changes in the US economy cost of capital.[91] However, he provides no evidence to support the notion that the telephone industry cost of capital will necessarily follow year-to-year changes in the US economy cost of capital, and there is no reason to assume *a priori* that use of an economy-wide cost of capital will reflect the expected rate of return for the LECs. Indeed, Christensen's assumption is inimical to the concept of price cap regulation and is essentially a throw-back to rate of return regulation (RORR). Under RORR, the *authorized* rate of return could be periodically revised to reflect economywide changes in the cost of capital, and tariff rates could be adjusted, up or down, to reflect the revision. But under price caps the nominal rate of return is frozen, at least for a span of time until the next periodic review. In the instant case, the FCC has used 11.25% as the benchmark rate of return for purposes of defining the sharing range, earnings cap and low end adjustment for LECs electing other than the 5.3% X-factor. However, the FCC expressly declined to reinitialize LEC rate levels when it reduced the authorized (benchmark) rate of return from its previous 12.00% to 11.25% in the *First Report and Order* in March of 1995. Accordingly, the very price cap rules under review here effectively *institutionalize* the LECs' ability to consistently generate earnings well in excess of the "competitive" level.

Furthermore, the BLS does not itself utilize economywide cost of capital in its own detailed industry productivity studies. While the BLS employs a similar internal rate of

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89. Results shown in Table 1 for all RBOCs are based upon an analysis similar to that performed for Pacific Bell, shown in Table 6 (p. 65) of the earlier ETI Report.

90. This estimate is derived by multiplying aggregate LEC interstate revenues (roughly $25-billion) times 1.3% (the difference 4.% and 5.3%).

91. Christensen "simplified" study, p. 10.

return method in its TFP studies, the agency develops *industry-specific measures* for application in industry-specific analyses. Christensen's use of the US economy cost of capital as a proxy for the LEC cost of capital is therefore totally at odds with BLS procedures.[92] Christensen Appendix 1 presents a comparison of BLS and Christensen Total Factor Productivity Methods. Christensen's comparison is deceptive in that it only points out general similarities without presenting any discussion of specific differences between the two methods. For example, with respect to the calculation of capital costs, Christensen states that "BLS and Christensen compute the quantity of capital and capital cost in similar ways," citing the common use of the "perpetual inventory method" and "the rental price equation."[93] Christensen's discussion is totally silent, however, about the obvious difference between his method and that utilized by the BLS with respect to the calculation of such a key component of the rental price equation as the cost of capital.

Table 1

BREAK-EVEN INTERSTATE RATE OF RETURN FOR 4% AND 5.3% X-FACTOR OPTIONS

Company	Break-Even
Ameritech	13.36
Bell Atlantic	13.32
BellSouth	13.31
NYNEX	13.43
Pacific Telesis	13.29
Southwestern	13.25
US West	13.24

USTA attempts to minimize the importance of Christensen's choice of the US cost of capital implicit in the NIPA as a proxy for the LEC cost of capital by asserting that the cost of capital estimates as used in the TFP study have a "limited role and importance." This is simply untrue. As noted in our earlier report, even a small percentage change in the X-factor has a profound dollar impact upon rates for interstate services, and therefore the sensitivity of Christensen's results to specific corrections or improvements, including the substitution of the US cost of capital, will be highly significant and must be taken into

92. See Michael J. Harper, "The Measurement of Productive Capital Stock, Capital Wealth, and Capital Services," Washington, D.C.: U.S. Bureau of Labor Statistics, Working Paper No. 138 (1982).

93. Christensen Appendix 1, p. 34.

account.[94] The dollar impact of Christensen's choice for the cost of capital variable is significant, both with respect to the calculation of TFP itself and also with respect to the measurement of the input price differential.

Indeed, an obvious problem with Christensen's use of the economywide cost of capital as a proxy for the LEC cost of capital is that it assumes away LEC/US input price differences that may exist with respect to cost of capital. Christensen is already using economywide price indexes to represent the cost of materials; he now compounds that bias (away from measurement of an input price differential) by using an economywide cost of capital as well.

Rental Price Equation

In the "simplified study," Christensen bases the rental price equation on three year moving averages of the cost of capital and capital gains, instead of their current values.[95] According to Christensen, the use of the "three-year moving average approach yields considerably more stable implicit rental prices in our original study."[96] As shown in Table 6 of the Christensen study, however, the use of the three-year moving average approach does not appear to impact average measured TFP growth. It is unclear therefore, based on the empirical results provided by Christensen, why he has chosen to introduce a more complex rental price formula that differs from standard BLS procedure. In his discussion of the subject, Christensen cites to a study by Harper, Berndt, and Wood, in which the authors evaluate a variety of rental price equation formulations.[97] However, review of that research indicates that the authors do not explicitly endorse the moving average

94. See ETI Report, p. 10, footnote 39. Table 3 in Christensen's "simplified" study indicates a 0.2% reduction in TFP associated with Christensen's substitution of the US cost of capital. Using the method outlined in the earlier ETI Report, this 0.2% reduction in TFP translates into roughly $500-million in LEC interstate revenues over a four year period.

95. Christensen "simplified" study, p. 22.

96. *Id.*

97. *Id.*, citing Michael J. Harper, Ernst R. Berndt, and David O. Wood, "Rates of Return and Capital Aggregation Using Alternative Rental Prices," in D.W. Jorgenson and R. Landau, eds., *Technology and Capital Formation*, (Cambridge MA: The MIT Press, 1989), pp. 331-372.