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# IN RESPONSE...

29 January 2004

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## A RESPONSE TO DRs. HAZLETT, HAVENNER AND BAZELON

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### I. Introduction and Background

In July 2003, the Phoenix Center released POLICY BULLETIN NO. 5, *Competition and Bell Company Investment in Telecommunications Plant: The Effects of UNE-P* (July 2003)<sup>1</sup> that evaluated, using a simple economic and econometric model, the short-run relationship between UNE-P and Bell Company investment in telecommunications plant at the state level. For the analysis, data from the Automated Reporting and Management Information System ("ARMIS") and other publicly-available data were used. From the results of this econometric analysis, we concluded that UNE-P had a positive and sizeable effect on BOC investment in telecommunications plant, with each UNE-P line increasing on average BOC net investment by about \$759.<sup>00</sup>.

Subsequently, two formal responses to the analysis set forth in POLICY BULLETIN NO. 5 were released. On one hand, in a document filed in Verizon's Forbearance Petition (WC Docket No. 03-157), Verizon employed Drs. Thomas Hazlett (the Manhattan Institute), Arthur Havenner (Univ. California - Davis), and Coleman Bazelon (Analysis Group) to comment on the empirical analysis contained in the BULLETIN.<sup>2</sup> Verizon's advocates proposed several modifications to our model, and presented a few alternate specifications. On the other hand, Z-Tel Communications, Inc. asked Dr. Carter Hill (Louisiana State University) to review the declaration of Hazlett,

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<sup>1</sup> <http://www.phoenix-center.org/PolicyBulletin/PolicyBulletin5.pdf>.

<sup>2</sup> Declaration of Thomas W. Hazlett, Ph.D., Arthur M. Havenner, Ph.D., and Coleman Bazelon, Ph.D., on Behalf of Verizon Communications, Inc., Reply Comments of Verizon Telephone Companies in Support of Petition for Expedited Forbearance from the Current Pricing Rules for the Unbundled Network Element Platform, WC Docket No. 03-157 (filed Sept. 2, 2003) (available at: [http://gullfoss2.fcc.gov/prod/ecfs/retrieve.cgi?native\\_or\\_pdf=pdf&id\\_document=6514783810](http://gullfoss2.fcc.gov/prod/ecfs/retrieve.cgi?native_or_pdf=pdf&id_document=6514783810) and is also posted on the Phoenix Center's web page).

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Havenner, and Bazelon as well as provide comments on the econometric model in POLICY BULLETIN NO. 5.<sup>3</sup>

Although the Phoenix Center, as a non-profit research institution under Section 501(c)(3) of the U.S. Internal Revenue Code, does not seek to influence the political process by participating as a party in any regulatory proceeding or engaging in lobbying of legislatures, we nonetheless believed it appropriate to respond to the comments of Hazlett, Havenner and Bazelon (“HHB I”) and to Carter Hill (“Hill”) in the interest of academic debate. This response was issued as POLICY BULLETIN No. 6, *UNE-P Drives Bell Investment: A Synthesis Model* (September 2003), where we summarized an extensive effort to evaluate the robustness of the empirical results reported in POLICY BULLETIN NO. 5. While these new specifications represented a synthesis of the modeling preferences of the Phoenix Center and the aforementioned economists, they nonetheless remained true to the neoclassical model of investment and valid econometric practice, to the extent allowed by the data. Despite wide variations in model specification, all our new empirical models confirmed that UNE-P competition a positive relationship between Bell Company investment in local telecommunications plant. As such, our new models affirmed both the results and specification of the empirical models in POLICY BULLETIN NO. 5.

Notwithstanding, Verizon again hired Drs. Hazlett, Havenner and Bazelon to file a formal critique of POLICY BULLETIN No. 6 as part of Verizon’s comments to the FCC’s pending TELRIC proceeding.<sup>4</sup> Again, although the Phoenix Center as a non-profit research institution under Section 501(c)(3) of the U.S. Internal Revenue Code does not seek to influence the political process by participating as a party in any regulatory proceeding or engaging in lobbying of legislatures, we nonetheless believe it appropriate to consider formally the comments of Hazlett, Havenner and Bazelon (now “HHB II”)<sup>5</sup> as there is always the possibility that critical

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<sup>3</sup> Declaration of R. Carter Hill, Ph.D., on Behalf of Z-Tel Communications, Inc., *In the Matter of Petition for Forbearance From the Current Pricing Rules for the Unbundled Network Element Platform*, WC Docket No. 03-157 (September 18, 2003) (<http://www.phoenix-center.org/PolicyBulletin/HillComments.pdf>).

<sup>4</sup> *In re Review of the Commission’s Ruling Regarding the Prices of Unbundled Network Elements and the Resale of Service by Incumbent Local Exchange Providers*, WC Docket No. 03-173.

<sup>5</sup> It should be noted that some of the comments in HHB II are also contained in HHB I. For example, HHB II claim that their regression using BOC-wide data measured in levels illustrates that our results are not robust (HHB II at 8-9). We believe our thorough response to this assertion (*see* POLICY BULLETIN NO. 6) is sufficient to dismiss this criticism. We do add, however, that since ARMIS data is available on an annually basis only, it is unclear how the investment and revenue data used in HHB’s regressions are measured. Further, our ability to reproduce the positive effect of UNE-P on Bell investment across twenty different specifications of the investment question confirms that the finding is not spurious (*see* POLICY BULLETIN NO. 6 at Tables 1 and 2). Finally, we note that HHB claim that the model they attempt to use to show that our model is mis-specified is, by their admission, itself mis-specified (HHB I Appendix at p. 4). It is not legitimate to claim that our results are spurious by appealing to a model that is, by admission, mis-specified. *See, e.g.,* J. G. MacKinnon, *Model Specification Tests and Artificial Regressions*, 30 JOURNAL OF (Footnote Continued...)

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review, even when adversarial rather than academic in nature, may prove useful in improving our analyses or, in some cases, reveal trivial or substantial flaws that we may have inadvertently committed or overlooked. Moreover, such comments are also useful in providing direction for future research.

HHB II consists of five primary critiques of POLICY BULLETIN Nos. 5 and 6 (many of which are also contained in HHB I, though elaborated on in HHB II) in addition to a reply to Dr. Hill. Our response to each follows the general outline of the HHB II Declaration (in Appendix 2), though we do not address the rebuttal to Dr. Hill except to the extent that it directly refers to our analysis.

## **II. June versus December UNE-P Data**

HHB II argue that the positive effect of UNE-P on investment is eliminated when end-of-year (December) rather than mid-year (June) measures of UNE-P lines is used as the explanatory variable. We continue to believe that the June data is more appropriate, allowing at least a six-month lag between the growth of UNE-P and the investment decision.<sup>6</sup>

As recognized by HHB, it is often inappropriate to use current values in an investment equation (HHB I at 3-4). To the extent that UNE-P lines are a “leading indicator,” lagging the UNE-P variable (to the extent possible, which is admittedly limited) is more appropriate.

Second, even if the December data is used, the results reported by HHB show that UNE-P does have a positive effect on investment (about \$450), and this effect is statistically significant at about the 13% level. In other words, there is an 87% probability that this large positive effect (\$450) is not due to chance. So, the regression continues to show a strong positive influence of UNE-P competition on investment. The superior performance of the mid-year data suggests perhaps that the lag specification of UNE-P lines is a better choice than the end-of-year data (as HHB’s discussion of investment would suggest).<sup>7</sup> In the practice of econometrics, lag lengths are often selected by considering the relative statistical performance of different lag structures

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ECONOMIC LITERATURE 102-146 (1992) (“It is important to test a model against several alternatives, but it is also important that those alternatives be reasonable ones.”).

<sup>6</sup> The data from December 1999 is so poor and limited, it is not possible to contemplate longer lags with available data.

<sup>7</sup> We were limited to the six-month lag due to data limitations. The December 1999 data on UNE-P lines is notoriously inaccurate, and, consequently, we avoided its use.

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(*e.g.*, using the Akaike Information Criteria (“AIC”) or, in some cases, relative R<sup>2</sup>s).<sup>8</sup> We observe that the AIC is consistently lower for regressions using the June data. But whichever data is used, HHB show that the regression analysis tends to support a positive relationship between UNE-P and BOC investment. Certainly, there is no evidence of a negative relationship.

### III. Magnitude of the Estimated Effect

HHB claim that the magnitude of the investment effect indicates a problem with our estimation methods, nothing that the marginal effect of \$759 per line is roughly equal to the average net capital stock per line (HHB II at 2-3, 4). This focus on this estimated coefficient is a bit misleading. At an aggregate level, we estimated in POLICY BULLETIN NO. 5 that the effect of UNE-P competition was to increase Bell investment by about 6%, which is about \$50 per access line. Given that UNE-P represents the only successful form of local mass market competition the Bells have ever encountered and has reached a market share of nearly 20% in some markets, this figure does not seem unreasonable to us.

We do note, however, that we made no specific claims about the exact nature of this additional investment. Nor do we believe that a focus on investment is a valid policy goal. Capital is an input of production and, as such, we cannot conclude that an increase in capital expenditures improves industry performance.

### IV. Results for Resale, UNE-L, and UNE-P

HHB argue that our estimated effect for UNE-P cannot be correct (or robust) because they estimate the effect of UNE-L to be zero (not statistically significant) and the effect of Total Service Resale to be negative (and statistically significant) (HHB App. 2 at 4). (Our analysis suggested neither had a statistically significant effect.) We cannot imagine why either finding is a problem. Resale offers no possibility for innovation and provides no real competitive constraint on the Bells.<sup>9</sup> Thus, the competition/innovation explanation for investment is entirely absent. It is interesting to note, however, that the evidence presented by HHB indicate that a shift to Resale from UNE-P (a common proposal of the Bells) would lead to a decline in Bell investment.

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<sup>8</sup> See, *e.g.*, W. Enders, APPLIED ECONOMETRIC TIME SERIES (2004) at 357-364; D. Gujarati, BASIC ECONOMETRICS (1995) at 209. Using certain statistical tests to determine lag length may result in theoretical problems including pre-test bias. See, *e.g.*, Gujarati at 591.

<sup>9</sup> With Resale, service innovation is severely constrained. Resale merely allows carriers to offer the exact same services as the Bells. Further, the underlying cost of those using Resale is based on the Bell’s retail price (thereby eliminating any competitive control on prices).

As for UNE-L, most attempts at UNE-L entry have met with failure, so a lack of investment response to UNE-L is unsurprising.<sup>10</sup> Further, there is good reason to expect a negative relationship between UNE-L and Bell investment since UNE-L uses less of the Bell network to provide service (thereby reducing the amount of total plant needed by the Bells). Replacement investment should be less when Bell switching plant is left stranded by successful UNE-L entry.<sup>11</sup>

In sum, the three entry strategies are very different in nature. We have no reason to expect that they would all have identical or even similar effects on investment or competition. Thus, we do not find this criticism of our models compelling. We do note that the coefficient on UNE-P is consistently positive and statistically significant (at no less than about the 15% level in all models; HHB App. 2 at Table A3).

## V. The Neoclassical Model of Investment

HHB claim that the theoretical basis for our empirical analysis “has been abandoned by economists in recent decades (at 9).” The authors provide no specific claims as to what theory they believe has replaced the Neoclassical Model. Our review of the economics literature suggests to us that HHB’s claim that the Neoclassical Model of investment has been abandoned is inaccurate. For example, Galeotti, in a 1990 paper, observes “today’s most popular models of investment [are] the [Neoclassical Model] and Tobin’s  $q$  (at 471).”<sup>12</sup> Chirinko (1993) concurs, noting “[b]y far the most frequently used specification for the analysis of investment spending has been the Neoclassical Model pioneered by Jorgenson and his numerous collaborators (at 1878).”<sup>13</sup> Fazzari and Athey (1987) state “[m]uch of the current investment research is based on the neoclassical theory of optimal capital accumulation pioneered by Jorgenson (at 481).”<sup>14</sup> Recent contributions to the Neoclassical theory include Dorfman and Heien (1989) and Abel

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<sup>10</sup> See, e.g., R. Crandall and H. Singer, *An Accurate Scorecard of the Telecommunications Act of 1996: Rejoinder to the Phoenix Center Study No. 7* (January 2004); L. F. Darby, J. A. Eisenach and J. S. Kraemer, *The CLEC Experiment: Anatomy of a Meltdown*, PROGRESS ON POINT 9.23, September 2002.

<sup>11</sup> In fact, the zero coefficient on UNE-L lines may simply be the consequence of plausible negative and positive effects exactly off-setting each other.

<sup>12</sup> M. Galeotti, *Neoclassical Investment Theory: Testing the Adjustment Cost Approach*, 72 REVIEW OF ECONOMICS AND STATISTICS 471-480 (1990).

<sup>13</sup> R. S. Chirinko, *Business Fixed Investment Spending: Modeling Strategies, Empirical Results, and Policy Implications*, 31 JOURNAL OF ECONOMIC LITERATURE 1875-1911 (1993).

<sup>14</sup> S. M. Fazzari and M. J. Athey, *Asymmetric Information, Financing Constraints, and Investment*, 69 REVIEW OF ECONOMICS AND STATISTICS 481-487 (1987).

and Eberly (1996).<sup>15</sup> Recent support for first differencing in investment models includes Jorgenson (1991) and Tatom (1991).<sup>16</sup> Econometric support for the first-difference model (with panel data, as used in the BULLETINS) is provided by Wooldridge (2002).<sup>17</sup> What is ignored by HHB altogether is that our first difference specification can be supported by statistical concerns alone. Indeed, both Average Net Investment and Total Revenues are non-stationary time series in their levels, but stationary in first differences.<sup>18</sup>

Further, while HHB assert that the Neoclassical Model “produces unreliable empirical results,” Oliner *et al.*'s (1995) empirical “horse race” among competing models of investment shows that the Neoclassical Model performed much better than other theories, including the  $q$  theory and the more recent Euler Equation investment models.<sup>19</sup> Likewise, Abel and Blanchard (1986) conclude “the  $q$  model[’s] empirical performance has not been that impressive (at 249).”<sup>20</sup> Similarly, Schaller (1990) observes “the theoretical appeal of  $q$  investment has not been matched by its empirical success (at 309).”<sup>21</sup>

The fact is that none of the modern or historical theories or empirical models of investment are perfect. Those with good theoretical properties often perform poorly in empirical tests, whereas some theories with debatable theoretical features explain investment well. Chirinko (1993) calls for dose of “humility” when interpreting investment models, and we concur.<sup>22</sup> We

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<sup>15</sup> J. H. Dorfman and Dale Heien, *The Effects of Uncertainty and Adjustment Costs on Investment in the Almond Industry*, 71 REVIEW OF ECONOMICS AND STATISTICS 263-274 (1989); A. B. Abel and J. C. Eberly, *Optimal Investment with Costly Irreversibility*, 63 REVIEW OF ECONOMICS AND STATISTICS 581-593 (1996).

<sup>16</sup> D. W. Jorgenson, *Fragile Statistical Foundations: The Macroeconomics of Public Infrastructure Investment* presented at the American Enterprise Institute Conference on *Infrastructure Needs and Policy Options for the 1990s*, Washington, D.C. (Feb. 4, 1991); J. A. Tatom, *Public Capital and Private Sector Performance*, 73 REVIEW (Federal Reserve Bank of St. Louis) 3-15 (1991).

<sup>17</sup> J. M. Wooldridge, *ECONOMETRIC ANALYSIS OF CROSS SECTION AND PANEL DATA* (2002), at Ch. 10.

<sup>18</sup> Based on an analysis of BellSouth quarterly data (to increase sample size) from 1990 through 1995 (the period for which quarterly data is available in ARMIS). The Augmented Dickey-Fuller Test is used to test for a unit root. Gujarati at Ch. 21. Also see Hill at 2 (“because such slow turns are characteristic of variables that are non-stationary, it is probably the case that the HHB regressions are spurious”).

<sup>19</sup> S. Oliner, G. Rudebusch, and D. Sichel, *New and Old Models of Business Investment: A Comparison of Forecasting Performance*, 27 JOURNAL OF MONEY, CREDIT AND BANKING 806-826 (1995).

<sup>20</sup> A.B. Abel and O.J. Blanchard, *The Present Value of Profits and Cyclical Movements in Investment*, 2 ECONOMETRICA 249-274 (1986).

<sup>21</sup> H. Schaller, *A Re-Examination of the Q-Theory of Investment using U.S. Firm Data*, 5 JOURNAL OF APPLIED ECONOMETRICS 309-325 (1990).

<sup>22</sup> *Id.*

also recognize that with rich datasets the Neoclassical Model can be and has been expanded in many ways including the use of complex and lengthy lag structures. Our failure to incorporate such advancements is the consequence of limited data and not oversight.

That said, the most important contemporary questions about telecommunications competition are empirical in nature and, consequently, some effort must be made to estimate the direction and magnitude of important relationships. In POLICY BULLETIN NOS. 5 and 6, the Phoenix Center paired available data with a popular model of investment to address a policy question of immense importance. All the models were subjected to specification and estimated in a variety of forms to ensure robustness. To date, our analysis of this important policy issue stands alone.

## VI. Interpretation of Coefficients

HHB introduce a new criticism to the regression analyses in POLICY BULLETIN NOS. 5 and 6, namely that the estimated coefficients in POLICY BULLETIN NO. 6 were incorrectly interpreted (HHB II at 10 and App. 2 at 10). Rather than the \$750 or so dollars per UNE-P line implied by the estimated coefficients, HHB contend that the “effect of UNE-P on investment is zero (HHB II, App. 2 at 11).” HHB reach this conclusion by an *invalid* interpretation of the estimated regressions summarized in POLICY BULLETIN NO. 6. In essence, HHB argue that by including lagged capital stock as a regressor, the large positive and statistically significant coefficient on UNE-P lines is (by their mathematical magic) actually zero, whereas if the lagged variable is left out of the regression, the coefficient is as large, positive, and statistically significant as it is reported in the table. Clearly, this argument cannot be valid.

The investment models presented in POLICY BULLETIN NOS. 5 and 6 were intended to be an approximation to a dynamic process for which we have a finite amount of data to evaluate.<sup>23</sup> The hypothesis the regression was intended to test is whether, over the sample period (Years 2000 through 2002), Bell investment was related to UNE-P competition.<sup>24</sup> Our regressions did not allow us to reject this hypothesis, and the relationship was found to be large and positive.

What our regressions were not intended to do, despite HHB’s claims otherwise, is measure or provide the tools to measure the optimal long-run level of capital investment. Without a fully specified dynamic model, including regression equations measuring the dynamic

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<sup>23</sup> See, e.g., L. G. Godfrey, MISPECIFICATION TESTS IN ECONOMETRICS (1988) (“... econometric models are usually better regarded as more or less useful approximations to the actual process as opposed to complete and accurate representations of it (at 2)”).

<sup>24</sup> UNE-P data was available for December 1999, but this data is notoriously inaccurate. Further, the data is provided for very few states.

processes related to UNE-P lines, revenues, capital stock, and other factors, it is impossible to evaluate the influence of these factors in some long-run equilibrium (which is the basis for HHB's claims about mis-interpretation), however defined.<sup>25</sup> Even if one assumed that the estimated equations reported in the POLICY BULLETIN NO. 6 were perfectly true, the results we reported would not allow us (or HHB) to correctly determine the long-run equilibrium values of the variables of the system (e.g., the capital stock and UNE-P lines) and thus their relationship to each other.<sup>26</sup> Our regressions simply cannot, without strong and unreasonable assumptions, tell us something about the long-run equilibrium. Any effort to do is illegitimate.

An analogy to HHB's argument may be useful. Say we were interested in the relationship between the height (in inches) and age (in years) of Chinese boys (ages 6 to 18).<sup>27</sup> Using least squares regression, we determine that, on average, the relationship between the two variables is

$$\text{Height} = 31.2 + 2.04 \text{ Age} + \text{error}.$$

With an R<sup>2</sup> of 0.992, this model is an excellent predictor of the height of Chinese boys. An analogy to what HHB have done with our models is to claim that this regression implies that a 500 year old Chinese boy would be 88 feet tall (though mathematically this could be shown to be implied by the model). Obviously, the use of these regression results to make such a forecast is inappropriate, and no less so than attempting to use our short-run approximations of Bell investment to make inferences about some distant, unspecified long-run equilibrium.

Indeed, it is not clear to us what the long-run equilibrium relationship between UNE-P and Bell investment would be or how it should be modeled, and we make no specific claims about it. For example, it may be that UNE-P eventually leads to facilities-based entry, perhaps in a manner similar to that described in PHOENIX CENTER POLICY PAPER NO. 12.<sup>28</sup> If so, then we

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<sup>25</sup> HHB's assumption that nothing changes in long run equilibrium is dubious. This is but one (and a peculiar) definition of equilibrium. In the Solow-Neoclassical Model, for example, the steady state is characterized by all factors (e.g., population, capital, income, etc.) having identical long-run growth rates (not equal to zero).<sup>25</sup> There is absolutely no reason to assume, as a general matter, that UNE-P lines or any other variable does not change in equilibrium.

<sup>26</sup> Nor is it feasible (or perhaps plausible is the better word) to estimate the long-run relationship between these and other variables with the paucity of data available (three years of data).

<sup>27</sup> We use the (50<sup>th</sup> percentile) data from Families with Children from China: [www.fwcc.org](http://www.fwcc.org).

<sup>28</sup> T. Randolph Beard, George S. Ford and Lawrence J. Spiwak, *Why ADCo? Why Now? An Economic Exploration into the Future of Industry Structure for the "Last Mile" in Local Telecommunications Markets*, PHOENIX CENTER POLICY PAPER SERIES NO. 12 (2001) (with) (<http://www.phoenix-center.org/pcpp/PCPP12.pdf>); reprinted in 54 FED. COM. L. J. 421 (May 2002) (<http://www.law.indiana.edu/fclj/pubs/v54/no3/spiwak.pdf>).

would expect Bell capital stock to be lower (in per-capita terms, at least) in long-run equilibrium than it is now when its network is designed to serve 100% of the market. The fact of the matter is that we have no idea what the long-run equilibrium should look like, and make no pretense to estimate it. To contort our analysis into a long-run analysis is inappropriate.

Some may view the lack of a long-run focus to our regressions as problematic. Unfortunately, the data simply do not allow for a long-run relationship to be estimated with even a scrap of plausibility. That said, while our regressions are unable to tell us much about the relationship between investment and UNE-P a few hundred or a few thousand years from now (i.e., the long run), we believe the regressions are capable of telling us something about the short-term effects of competition on Bell investment and that seems to be the relevant policy issue (for reasons we do not fully understand). After all, in the long run, we are all dead. The empirical effect of competition policy on investment is of considerable interest to contemporary policy makers (the theoretical relationship is hopelessly ambiguous), and we will continue to contribute what we can to understanding the key relationships.

## VII. Econometric Issues

In the final section of the Appendix to their Declaration, HHB respond to the Declaration of R. Carter Hill, which was a review of HHB's comments on POLICY BULLETIN NO. 5. We do not intend to speak for Dr. Hill, and limit our discussion to direct references to the POLICY BULLETINS.

Specifically, HHB repeat their claim that the correct estimation methodology for the investment regressions is weighted least squares. At least now, after Dr. Hill's commentary on their first Declaration, HHB appear to recognize that weighted least squares is simply a correction for heteroscedasticity. Because the per-line form of the regressions in the BULLETINS (where the continuous variables were expressed on a per-line basis) did not suffer from heteroscedasticity (test results were reported), weighted least squares (a correction for heteroscedasticity) was not required. The only models that suffered from heteroscedasticity were Models 1 and Model 11 from POLICY BULLETIN NOS. 5 and 6, all three of which also failed the specification test RESET. As we stated clearly in the BULLETINS, none of our conclusions were based on those three models given their admittedly poor statistical properties. Relatedly, HHB assert that "the effect of UNE-P lines on capital did not hold up" when using weighted least squares (HHB App. 2 at 14), which is untrue. As shown in both POLICY BULLETIN NO. 5 and 6, the statistical significance of the UNE-P variable (and other variables) was consistently higher using weighted least squares than with ordinary least squares.

Additionally, HHB contend that "Section 271 certification process could alone produce an omitted variable problem resulting in substantial bias and inconsistency of the parameter estimates (HHB App. 2 at 14)." However, the RESET test statistics reported in both BULLETINS

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suggested that the estimation coefficients were not biased due to omitted variables bias. Recognizing that RESET is not a perfect test (no statistical test is), we evaluate the potential for bias caused by Section 271 by comparing the results of two models, one with a Section 271 variable and one without. The results are summarized in Table 1.

Variable	Model 1	Model 2
$\beta_{t-1}$	4.95E+07 (3.91)*	4.99E+07 (4.30)*
$\Delta R (\alpha_1)$	0.74 (4.05)*	0.66 (3.54)*
$\Delta U (\alpha_2)$	927.0 (4.46)*	917.4 (4.24)*
$\beta_t - \beta_{t-1}$	-8.1E+07 (-4.58)*	-8.3E+07 (-4.13)*
$K_{t-1}$	-0.035 (-2.71)*	-0.043 (-3.07)*
D271	...	2.9E+07 (1.20)
R <sup>2</sup>	...	...
White $\chi^2$	8.60	9.14
RESET	1.49	2.30**

\* Statistically significant at the 5% level or better.  
\*\* Statistically significant at the 10% level or better.

Both models are estimated by weighted least squares (as recommended by HHB). The 271 dummy variable (D271) equals 1 in the year 271 approval was granted for the state. As shown in Table 1, the addition of the 271 dummy variable has very little effect on the estimated coefficients. Our variable for UNE-P lines, for example, is \$927 in Model 1 and \$917 in Model 2. The 271 dummy variable is also statistically insignificant at standard levels. Thus, we find no evidence that the exclusion from the model of the Section 271 process produces any bias in our results.

Finally, HHB again assert that their aggregate BOC level regressions show that our state-level analysis is not robust. As mentioned in POLICY BULLETIN NO. 6, aggregating unnecessarily is unsound econometric practice. Additionally, it is not difficult to show that UNE-P could have a positive and identical relationship to investment at the state level even though a regression at the aggregate level would imply a negative relationship. If the aggregate investment model failed to account fully for differences in aggregate investment across BOCs, then a sign switch is quite possible.

Table 2. Aggregate versus State Relationships					
BOC	State	UNE-P	Inv.*	Total UNE-P	Total Inv.
BOC A	State 1	100	\$200	300	\$500
	State 2	200	\$300		
BOC B	State 1	110	\$160	330	\$430
	State 2	220	\$270		

\* For BOC A, Inv = 100 + UNE-P; For BOC B, Inv = 50 + UNE-P.

Table 2 provides a simple example. In each state, BOC A invests \$100 plus \$1 per UNE-P line. BOC B investment \$50 plus \$1 per UNE-P line. By definition, investment is positively related to UNE-P (\$1 per line). However, at the aggregate level, it appears that UNE-P is negatively related to investment (total UNE-P is higher for BOC B but investment is less), simply because BOC B invests less overall than BOC A. Econometrically, the sign switch would occur if the model failed to properly account for the determinants of other forms of investment (such as investment in wireless plant or overseas ventures) and overall investment tendencies. Furthermore, it is not possible to sensibly compare the results of the BOC level analysis and state analysis since the measures of capital expenditures and revenues are not the same (the sum of the state data in ARMIS will not equal the aggregate financial data reported to the Securities and Exchange Commission).

### VIII. Conclusions

We believe the additional efforts by Verizon advocates Drs. Thomas Hazlett, Art Havenner, and Coleman Bazelon to discredit the statistical analysis summarized in POLICY BULLETIN NOS. 5 and 6 to be unconvincing. The nature of HHB’s criticisms frequently relate to what cannot be estimated (given limited data), rather than what we have or can estimate. Much of their criticism is also directed at a statistical problem for which our models did not suffer (*i.e.*, heteroscedasticity). This line of attack is unpersuasive and, in many cases, irrelevant. As such, we find no reason to alter or change our analysis or interpretation of it as summarized in the POLICY BULLETINS. We leave it to the discerning public, armed with the POLICY BULLETINS and all relevant documents (all made freely available at the Phoenix Center website), to make an educated decision on the validity of the analysis and the criticisms of it. As always, we appreciate the attention (adversarial or not), which signifies the relevance of our work.