Study Reviews:

Regulatory Behavior and Competitive Entry *by* James Eisner and Dale Lehman

Key Points: The authors conclude that the demand curve for unbundled loops slopes upward. Obviously, that's a problem.

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The frequently cited piece of empirical analysis cited by the Bell Companies is a draft paper by Drs. James Eisner and Dale Lehman entitled "Regulatory Behavior and Competitive Entry," dated June 28, 2001. Using "confidential" data collected by the Commission from CLECs (which was available to Dr. Eisner in his capacity as an FCC employee), the study estimates numerous regression models with three dependent variables: a) the number of CLEC reported facilities-based lines served; b) the number of CLEC lines served using unbundled loops; and c) the number of CLEC lines served using total service resale. Factors that are used to explain variations in these dependent variables are chosen in a somewhat a*d h*oc fashion, with neither an appeal to any specified economic model or econometric considerations. The arbitrary selection of model specification makes the results of their analysis difficult to interpret, both for theoretical and econometric reasons. Evaluating the results in a rigorous manner is also made more difficult because the Commission has not made the entire data set available for review (by the CLECs, at least) under a protective order. I will focus on two of the paper's self-proclaimed "relevant" findings of the paper, and in this review a number of the more important flaws of the study are revealed.

The Eisner-Lehman study finds a positive relationship between the price for an unbundled, analog, two-wire loop and the number of lines provided on a facilities basis by CLECs. This result is interpreted by Bell Company advocates as evidence that the promotion of facilities-based competition requires high unbundled loop rates. A second noteworthy conclusion of the Eisner-Lehman study is that the quantity demanded of unbundled loops is positively related to the price of loops. This result and conclusion is most peculiar; after all, a most fundamental tenet of economic science is that demand curves slope downward.

Drs. Eisner's and Lehman's ad hoc model specifications lead to concerns over the interpretation of the results due to omitted variables bias and other econometric problems. In fact, Eisner-Lehman's econometric analysis is a case study in omitted variables bias, with the authors apparently attempting to convince the reader that each of their models is incorrectly specified. For example, according to their econometric models, the HCPM estimate of loop cost is a statistically-significant determinant of lines served by CLEC facilities (see Models 7-11). Yet, this variable is absent from Models 1-6. In other words, Models 7-11 show that Models 1-6 are tainted by omitted variables bias. Thus, the estimated coefficients of Models 1-6 are biased and inconsistent (the estimated coefficients do not measure the true relationship (bias), even at large samples (inconsistency))– both characteristics being a consequence of omitted variables bias. By the same analysis, Model 24 invalidates models 21, 23, 25, and 26 (with respect to the variable "average UNE"). Models 21, 22, 23, 26, and 27 invalidate Models 20, 24, and 25 (with respect to the "271" variable). Model 27, given the results on "employment change," suggests that Models 20-26 are mis-specified. Such comparisons can show nearly every model of the study is mis-specified. The peculiar, ad hoc method of model specification basically forces the reader to conclude that all estimated models (Models 1-27) suffer from omitted variables bias; indeed, it appears as if the authors are trying to convince the reader of that fact by providing a list of omitted variables for each individual equation.[1] Additionally, the authors provide a list of potential reasons why their models may suffer from omitted variable bias (pp. B21-B22).

The most important omitted variable in the Eisner-Lehman models is not even considered by the authors. The authors measure market size by the number of employees in the state.[2] Market size is an important determinant in the model; the number of lines served by CLEC facilities is linearly related to market size and that variable alone explains about 90% of the variability in CLEC lines (see Regression 1 in Table 2), leaving very little variation for additional factors to explain. The problem with the market size variable selected by Drs. Eisner and Lehman is that it makes New York, perhaps the most competitive local telecommunications market in the world (which does not say much), look a lot

[1] Bias is of degree, and in some cases more efficient estimates are traded off for small amounts of bias. The instability of many of the Eisner-Lehman estimated coefficients suggests that the bias introduced by their ad hoc, hokey-pokey specifications is non-trivial.

[2] The number of access lines would serve equally well as a measure of market size, given that lines and employment are highly correlated ($\rho = 0.986$). This highest value of the correlation coefficient (ρ) is 1.00, implying that the two variables are perfectly correlated. For econometric analysis, perfect correlation implies the variables are identical (except with respect to the constant term which will measure any scale differences between the two series).

like Florida, Illinois, and much smaller than Texas and California.[3] Yet, New York has far more competitive activity than any other state, even after accounting for its relative size (i.e., employment); total CLEC provided lines in New York exceeds the next largest state (California) by 64%. Combine this fact with New York's above average loop rate (for the sample period), and the potential for New York to exert a powerful influence on the regression estimates is substantial.

To test for the influence of New York on the results of the Eisner-Lehman study, assume New York is different from the other states, and that a dummy variable indicating that state (1 for New York, 0 otherwise) can capture these differences. This dummy variable allows for a statistical test of the uniqueness of New York. Consider Model 2 from the Eisner-Lehman study. Using the version of the dataset available to CLECs (a more limited dataset than that available to Bell advocates), Eisner and Lehman's Model 2 is replicated and reported as Regression (2) in Table 2. As in the Eisner-Lehman study, a positive and statistically-significant relationship between loop rate and CLEC provided lines is found with their model specification, although the results are somewhat different given the limited data set made available to CLECs. Results from an alternative specification of Model 2, including a dummy variable for New York (DNY), are summarized as Regression (3). As revealed in the table, once the peculiarities of New York are accounted for, there is no statistically-significant relationship (meaning we cannot reject the hypothesis of no relationship) between the loop rate and lines provided over CLEC facilities.[4] Examination of the data also revealed that the state of Ohio was an important outlier, and the results from Regression (4) reveal this fact.[5]

Regression (5) in Table 1 estimates the Eisner-Lehman Model 2 with the December-2000 edition of the dependent variable data. Eisner-Lehman used the June-2000 version of the data in their study. Importantly, using the same specification as Eisner-Lehman, no variable other than market size is statistically

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[3] While New York accounts for only 6.7% of total employment, 36% of UNE loops are in that state (according to the data used by Drs. Eisner and Lehman).

[4] To some extent, the importance of New York as an outlier was observed by Eisner and Lehman. The authors observe, "[i]t appears that the statistical significance of the average UNE rate for total CLEC entry disappears in the presence of the 271 variable (p. B19)." Since New York is one of four states included in the 271 dummy variable, the possibility that New York (or some other state) was driving the results should have been apparent to the authors – but, apparently not.

[5] Ohio has a very low loop rate, but virtually no facilities-based entry by CLECs.

significant with the updated data – data only six-months different than that used by Drs. Eisner and Lehman.[6] (The same is true using the June-2001 data for the dependent variable). The t-statistic on the loop rate (“average UNE”) is 0.59, which is well below standard levels of statistical significance.[7] In this case, even if we ignore the influence of New York on the results, there is no relationship between the loop rate and the amount of distribution plant provided by CLECs. This finding suggests that the Eisner-Lehman results on loop price may be the result of spurious correlation, even ignoring the effect of outliers.

A fundamental tenet of economic science is that demand curves slope downward. Despite this fact, Drs. Eisner and Lehman reach the peculiar conclusion that the demand curve for unbundled loops slopes upward (“higher UNE rates tend to be positively associated with greater use of UNEs (p. B17)”), a conclusion that in itself requires all the results to be viewed with a dollop of skepticism.[8] Fortunately, as with the CLEC facilities regressions, this particularly absurd result can be shown to be a statistical illusion. For this analysis, Model 20 from the Eisner-Lehman study is reproduced, and my replication of the results (due to data limitations) is presented as Regression (6). The results are similar, but not exact, and it appears that the authors must have scaled the “employment change” variable (which is not material to the analysis).

Adding the New York dummy variable to Model 20, the results summarized as Regression (7) are produced. The coefficient on the New York dummy variable indicates UNE demand is much higher in New York, and note the size of the t-statistic (13.86). When the New York dummy is included as a regressor, the sign on the loop rate flips, becoming negative (demand now slopes downward), though the coefficient is not statistically different from zero. The signs on the “HCPM Loop” and “employment change” variable are reversed, with the latter variable now being statistically significant at the 10% level. The R-Square of the regression, a measure of how well the regression explains the dependent variable, has increased from 0.47 to 0.95 (i.e., the model’s explanatory power increased from explaining about half of the variation of the dependent variable, 47%, to almost all of the variation in the dependent variable, 95%). Obviously, the New York dummy variable is an important omitted variable in the

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[6] Observe that the more recent data has five more observations, so the degrees of freedom of the estimates have increased.

[7] Significance levels of the t-statistics are about 1.7 at the 10% level and 2.00 at the 5% level.

[8] The authors do recognize this result as perverse, and expend some effort to describe to the reader the many potential mis-specifications that may contribute to it (pp. B21-B22).

Eisner-Lehman analysis, and ignoring this fact seriously biases the estimated coefficients of that study.

The problem of omitted variables bias (even with respect to New York) is apparently detected by Eisner and Lehman, but not fully appreciated. The authors state," the statistical significance of the UNE rates appears to depend critically on whether or not 271 entry is included as an independent variable (p. B16)." Their regressions show that when 271 is included as an explanatory variable, the UNE loop rate is not statistically significant (and vice versa). The 271 variable, however, *is* statistically significant. Because the exclusion of relevant factors leads to biased estimates, it is no surprise that the exclusion of the 271 variable (which is mis-specified itself, but still statistically significant) affects the other estimates of the regression (which is typical of omitted variable bias).[9]

In addition to omitted variables bias, the Eisner-Lehman study, by the authors own admission, suffers from simultaneity bias – a serious econometric problem. In footnote 13 of the study, the authors assert, "… 271 approval and entry are simultaneously determined (p. B16)." Yet, the authors make no attempt to correct for simultaneity, thereby risking biased estimates.[10] As with omitted variables bias, simultaneity bias leads to biased and inconsistent coefficient estimates. In defense of ignoring this severe econometric error, the authors contend, "there are too many unobservable variables … to estimate such a model satisfactorily (p. B16)." Unfortunately, the problems with simultaneity cannot be ignored just because the authors' dataset is somehow incomplete. Nor is it the case that more variables are needed to account for simultaneity; there are a number of techniques that could potentially address that issue and require no more data than that possessed by the authors. But, if these "unobservable variables" are required to determine the true relationships, and the authors apparently believe they are, then they have concluded that all their models are mis-specified.

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[9] Eisner and Lehman include in their 271 dummy variable states that had not received 271 authority as of the date of their dependent variable data. As a consequence, the estimated coefficient has no meaningful interpretation (the variable is "mis-measured," which is another severe econometric problem).

[10] Oddly, while only one state had received 271 authority by June 2000, the authors give four states a value of 1.00 for the 271 dummy variable. This invalid specification of the 271 variable was necessary, according to the authors, to "protect the confidentiality of the UNE line counts (ft. 9, p. B8)." Unfortunately, the rules of econometric analysis do not bend to confidentiality issues. Perhaps a better way to protect the confidentiality of the data is not to share it with BOC economic advocates in the first place (only to then deny such access to the CLECs).

With econometric analysis, it is possible for a few observations to exert a powerful influence on the estimated coefficients, and care must be taken by the researcher to evaluate the presence and influence of outliers. An outlier (New York, and to some extent Ohio), and not genuine economic phenomena, drives the results of the Eisner-Lehman study. This observation, however, does not suppose that the Eisner-Lehman study is otherwise valid. Indeed, there are many other problems with the Eisner-Lehman study, and I will summarize a few of these problems below.

With respect to lines served by CLEC facilities, it is difficult to imagine conceptually why the price for an analog, two-wire loop would influence the decision of CLECs deploying, for the most part, high capacity loops, unless that price is a proxy for some truly relevant factor (perhaps the reciprocal compensation). In addition, it is unclear why interim rates would influence decisions regarding long-term, sunk investments (in unlike facilities) by CLECs. Indeed, the authors state, "facilities based entry takes time (p. B16)," and short-term, interim rates may not be a reliable indicator of long-term prices for UNEs. Nor is it clear how to interpret the coefficient on the loop rate, which is one measure of loop cost, when another measure of loop cost (the HCPM estimate of loop cost) is included in the regression. The authors provide no assistance in answering these important questions.

Additionally, there appear to be some data problems with the study. For example, the authors include as regressors the average UNE price ("average UNE") and the "lowest UNE price available" (low UNE). However, for 23% of the states, the "low UNE" variable exceeds the "average UNE." Clearly, there is something wrong with the data on rates.

One interesting finding of the study (upon which I give no credence given the problems discussed above) is the negative relationship found between the ILEC's embedded cost and CLEC entry. Why would the ILEC's embedded cost affect CLEC entry? The authors provide no explanation, but I can think of at least one. While the entrant may not be interested in the incumbent's embedded costs, the incumbent most likely is. As Bell advocate Dr. Shelanski observes, "[n]o firm wants to strand costs (p. 12)." Entry, and the competition that inevitably follows, puts the recovery of embedded costs at risk, thereby reducing the profits of the incumbent. In markets where embedded costs are high (the incumbent is inefficient), therefore, the incumbent may have an increased incentive to deter competitive entry. The negative relationship between embedded cost and CLEC entry suggests that such entry deterrence is effective.

Finally, consider, just for the sake of argument, the Bell advocacy position supported by the Eisner-Lehman study. This ILEC position, based on the alleged positive relationship between CLEC facilities and the loop price, is that in order to promote facilities-based competition, the unbundled loop rate should be increased. However, this advocacy position does not makes sense. From the estimated relationships of the Eisner-Lehman study, a $1.00 increase in the loop rate will increase CLEC facility lines by about 4,000 units. That same increase, however, raises the number of unbundled loops by about 10,000 lines.[11] Thus, increases in the loop rate will actually promote more element entry than facilities entry, shifting the competitive mix toward unbundled elements. This result is both counter-intuitive and contrary to the other empirical evidence in the record, and again indicates that this study provides little if any useful empirical evidence on competition in local exchange markets.[12]

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[11] These marginal effects are based on the estimated coefficients found in the Eisner-Lehman study.

[12] For more sensible estimates of demand curves for unbundled elements, *see* Beard & Ford (2002); Robert B. Ekelund Jr. & George S. Ford, *Some Preliminary Evidence on the Demand for Unbundled Elements*, available at www.telepolicy.com.

An Analysis of the Eisner-Lehman Study

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		E-L Model 2	E-L Model 2 (Adj)	E-L Model 2 (Adj)	E-L Model 2 (Adj)	E-L Model 20	E-L Model 20 (Adj)
Constant	-35951.2	-97984.7	-69154.1	-58397.3	-60197.3	6288.91	-137589.0
	(-2.66)a	(-2.55)a	(-1.80)b	(-1.71)b	(-1.29)	(0.02)	(-1.36)
employment	0.042	0.043	0.040	0.041	0.052	0.042	0.028
	(14.68)a	(15.15)a	(14.39)a	(16.41)a	(14.10)a	(3.10)a	(6.37)a
Pricecap	…	6027.34	-1773.82	411.97	2454.29	…	…
		(0.30)	(-0.09)	(0.02)	(0.10)		
average UNE	…	3614.39	2371.35	1650.41	1254.01	15858.2	-2130.49
		(1.95)b	(1.30)	(1.00)	(0.59)	(1.97)b	(-0.74)
DNY	…	…	97586.8	94259.5	…	…	981812.4
			(2.15)a	(2.34)a			(13.86)[a]
DOHIO	…	…	…	-104525.4	…	…	…
				(2.81)a			
HCPM Loop	…	…	…	…	…	-9021.39	3901.35
						(-0.76)	(0.99)
employment change	…	…	…	…	…	-508985.1	213905.8
						(-1.57)	(1.83)[b]
R2	0.89	0.90	0.92	0.94	0.87	0.47	0.95
Obs.	30	30	30	30	35	27	0.27

[a] Statistically Significant at the 5% level.
[b] Statistically Significant at the 10% level.

Alternative studies on the issue of unbundling and facilities based entry include:

Mandated Access and the Make-or-Buy Decision: The Case of Local Telecommunications Competition, T. R. Beard, G. S. Ford, and T.M. Koutsky, December 2002.

Pursuing Competition in Local Telephony: The Law and Economics of Unbundling and Impairment, T. R. Beard, R. B. Ekelund Jr., and G.S. Ford, November 2002 (forthcoming in the Journal of Law, Technology, and Policy, Spring 2004).

Unbundling and Facilities-Based Entry by CLECs: Two Empirical Tests, G. S. Ford and M. D. Pelcovits, July 2002.

Also see

Competition and Bell Company Investment in Telecommunications Plant: The Effects of UNE-P, Phoenix Center Policy Bulletin No. 5 (July 8, 2003).

The Truth About Telecommunications Investment After the Telecom Act, Phoenix Center Policy Bulletin No. 4 (June 24, 2003).

The Financial Implications of the UNE-Platform: A Review of the Evidence, T. R. Beard, G. S. Ford, and C. Klein (Forthcoming in CommLaw Conspectus, Summer 2003).

On the Relationship between Telecommunications Investment and Economic Growth in the United States, R. Beil, G. Ford, and J. Jackson (June 2003).

The Myth of Below-Cost UNE-P Prices, G.S. Ford (February 2003).

Bell Companies as Profitable Wholesale Firms: The Economic Implications of UNE-P, T. R. Beard and C. Klein, Phoenix Center Policy Paper No. 17, November 2002.

What Determines Wholesale Prices for Network Elements in Telephony? An Econometric Evaluation, T. R. Beard and G.S. Ford, Phoenix Center Policy Paper No. 16, September 2002.

Make-or-Buy? Unbundled Elements as Substitutes for Competitive Facilities in the Local Exchange Network, T. R. Beard and G. S. Ford, Phoenix Center Policy Paper No. 14, September 2002.

An Empirical Examination of the Unbundled Local Switching Restriction, Z-Tel Policy Paper No. 3, March 2002.

Facilities-Based Entry in Local Telecommunications: An Empirical Investigation, T. R. Beard, G. S. Ford, and T. M. Koutsky, June 2002.

Does Unbundling Really Discourage Facilities-Based Entry? An Econometric Examination of the Unbundled Local Switching Restriction, Z-Tel Policy Paper No. 4, February 2002.

The Demand for Unbundled Elements in Telephony Revisited, Audrey Kline, November 2000.

Preliminary Evidence on the Demand for Unbundled Elements, R. B. Ekelund, Jr. and G. S. Ford, June 2002.

The UNE-Platform, Impairment and Natural Monpoly: Bell Company Estimates of Cost Disparities and Their Consequences, T. Koutsky and G. S. Ford (January 2003).

Innovation, Investment, and Unbundling: An Empirical Update, R. B. Ekelund, Jr. and G. S. Ford, June 2002. See update by Ekelund and Ford, Mandatory Unbundling, UNE-P, and the Cost of Equity: A Rejoinder (July 2003).

Why ADCO? Why Now? An Economic Exploration of Industry Structure for the "Last Mile" in Local Telecommunications Markets, T. R. Beard, G.S. Ford, and L. J. Spiwak, May 2002.

A Fox in the Hen House: An Evaluation of Bell Company Proposals to Eliminate Their Monopoly Positions in Local Telecommunications, G. S. Ford, PHOENIX CENTER POLICY PAPER NO. 15 (September 2002).