BEFORE THE
POSTAL RATE COMMISSION
WASHINGTON, D.C. 20268-0001

Postal Rate and Fee Changes, 1997   Docket No. R97-1

REBUTTAL TESTIMONY OF
JOHN S. YING
ON BEHALF OF THE
UNITED STATES POSTAL SERVICE

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1   AUTOBIOGRAPHICAL SKETCH

2   My name is John S. Ying. I am Associate Professor of Economics at the

3   University of Delaware in Newark, Delaware. I have taught economics there since 1987,

4   and I have also taught as a visiting assistant professor at the University of British

5   Columbia and the University of California, Irvine. I received a B.S. with high honors in

6   physics from the University of Michigan, and I hold M.A. and Ph.D. degrees in

7   economics from the University of California, Berkeley. My principal areas of teaching

8   and research are industrial organization, regulatory economics, and microeconomic

9   theory. I have published scholarly articles on these subjects in leading economics

10   journals, including the *RAND Journal of Economics*, the *Review of Economics and*

11   *Statistics*, and the *Journal of Business & Economic Statistics*.[1] My primary areas of

12   specialization are the telecommunications and motor carrier industries. In particular, my

13   research has focused on the econometric estimation of cost functions to analyze

14   regulatory issues. I have consulted for the Regional Bell Operating Companies on their

15   *motion to vacate the Decree which broke up the Bell System.*

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[1]A list of relevant publications is appended to this testimony.

1   PURPOSE AND SCOPE

2   My testimony is part of the new Postal Service study of mail processing labor

3   costs, conducted by Postal Service witness, Dr. Michael D. Bradley (USPS-T-14). In

4   response to that study, intervenor testimony was submitted on behalf of the Office of the

5   Consumer Advocate by Dr. J. Edward Smith, Jr. (OCA-T-600) and on behalf of United

6   Parcel Service by Dr. Kevin Neels (UPS-T-1). I have been asked to provide rebuttal

7   testimony to the direct testimonies of Dr. Smith and Dr. Neels. Dr. Michael Bradley is

8   also providing rebuttal testimony concerning some of the more detailed aspects of the

9   data, econometrics, and mail processing activities.

10   Following the filing of these testimonies, the Postal Rate Commission issued

11   Notice of Inquiry No. 4 (NOI No. 4) on the restriction in Dr. Bradley's study that slope

12   coefficients are identical across facilities. I have also been asked to comment on the NOI

13   and the response filed by Dr. Neels (UPS-ST-1).

14   Briefly, my conclusions are that most of Dr. Smith's testimony lacks credibility.

15   He makes numerous comments which indicate a less than clear understanding of basic

16   economic theory. It is also obvious from his exhibits that econometrics is not one of his

17   strengths. While he makes a few good comments about the data, the essence of his

18   testimony is that 100 percent variabilites should be maintained because many plots of the

19   data appear to him to have a slope of one.

20   I find Dr. Neels' testimony more credible, as he raises some issues which seem

21   plausible, at least on the surface. However, his concerns about the choice of variables are

22   misguided and those about the data scrub may not be valid. I cannot, moreover, agree

1   with his extreme position that there should be no data scrub at all. Besides maintaining

2   the existing 100 percent variabilities, the other main recommendation from Dr. Neels is

3   that the "between" cross-sectional model is better than Dr. Bradley's fixed effects model.

4   But his arguments are largely speculative, and there are many well known and valid

5   reasons for preferring panel to cross-sectional data.

6   Regarding NOI No. 4, I think there is consensus that the statistical tests show that

7   slope coefficients are not identical across sites, a somewhat obvious and expected result.

8   This rejection of Dr. Bradley's fixed effects model carries some tradeoffs however. Site-

9   specific variabilities may be not as reliable and necessarily require some aggregation

10   technique to determine system-wide variabilities. Because of these tradeoffs, I think the

11   results from the fixed effects model are still preferable. Another implication of the NOI

12   is that assuming 100 percent variabilities can clearly be rejected and should be

13   discontinued.

1   I.   APPRAISAL OF DR. SMITH'S TESTIMONY

2   A. Analytic Economic Framework

3   For better orientation, my discussion will follow the order of presentation in the

4   direct testimonies. Dr. Smith begins his testimony by claiming that Dr. Bradley's cost

5   equation fails to conform with economic theory because it is not derived from a

6   production function analysis. While he knows that a cost equation has been estimated,

7   Dr. Smith apparently does not know the difference between a cost equation and cost

8   function. As described in the testimony of Dr. John C. Panzar (USPS-T-11), estimation

9   of a Postal Service cost function (or Dr. Bradley's cost equation) only requires the

10   existence of "a reasonably well-defined set of operating procedures which determine the

11   steps taken and resources used to process a given volume of mail."[2] The operating plan

12   need not be optimal nor cost-minimizing, but must be reproducible and relatively stable.

13   A cost equation is not the same as a theoretically derived cost function.

14   Even if Dr. Bradley were estimating a cost function, the explicit specification of a

15   production function (or analysis) is not necessary. Economists use production functions

16   or input requirement sets to describe a firm's underlying technology or physically

17   possible production plans. The fundamental principle of duality in production states that

18   "the cost function of a firm summarizes all the economically relevant aspects of its

19   technology."[3] It provides the basis for all cost function estimation and the

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[2]"Direct Testimony of John C. Panzar on Behalf of United States Postal Service " (USPS-T-11), p. 14.

[3]Varian, Hal R., Microeconomic Analysis, Third Edition, W.W. Norton & Company, New York, 1992, p. 84.

1   correspondence between some underlying production function and a cost function. Dr.

2   Smith's testimony and his response in USPS/OCA-T600-6 (Tr. 28/ 15909-10) indicate

3   a basic lack of understanding of economic theory.

4   As for the issue of capital, Dr. Smith states his belief that capital should be

5   included in the specification. Here, he seems confused about what constitutes short run

6   and long run. Dr. Bradley's variable cost equation is, in an economic sense, a short-run

7   specification of costs in which some inputs such as capital may be fixed. The "actual

8   cost" concept applied by the Postal Service is intended to reflect changes possible over

9   the rate cycle, a period of only a few years, and is therefore closer to the short-run and not

10   the long-run all-inputs-variable definition used by economists.

11   In Section II C of his testimony, Dr. Smith claims that Dr. Bradley misuses time

12   trends as measures of technological change because they should only be used in

13   macroeconomic models. Time trends are obviously applicable to microeconomic

14   studies, as later acknowledged in Dr. Smith's response (USPS/OCA-T600-3, Tr. 28/

15   15904-06). His original comment is somewhat disturbing and indicates an unfamiliarity

16   with cost estimations, which commonly employ time trends. They are justified if there is

17   a lack of data on specific technological or other dynamic variables, as in this case.

18   It is true that time trends do lack precision, but the focus of Dr. Bradley's study is

19   on total piece handlings (TPH). The purpose of including the time trends is to control for

20   (not to explain) time-varying factors to eliminate bias in TPH. Given the possible time-

21   varying factors captured by a time trend, it would be difficult to describe any coefficients

22   as questionable. Any sign is possible. Also, why should time trends 1 and 2 agree?

1   There is reason to think they might differ, which is why Dr. Bradley uses two.

2   Dr. Smith's last comment in this section concerning the time period of analysis

3   again shows a basic lack of understanding of the theory. The fact that the data

4   observations consist of 13 four-week accounting periods over a year has nothing to do

5   with the "very short run." Whether a cost function is a long-run or short-run function

6   depends on its specification, not the frequency of observation. Does it treat all inputs as

7   variable or are some considered fixed? Clearly, in an economic sense, Dr. Bradley is

8   estimating a short-run cost equation, consistent with the Postal Service's desire to

9   measure "actual costs." Dr. Smith incorrectly associates the four-week data period with a

10   "four-week-run" cost equation. The unimportance of the frequency of data on the results

11   is confirmed in Dr. Bradley's re-estimation using annualized data.

12   Under oral cross examination, Tr. 28/15963, Dr. Smith cites a book by Dr.

13   Intriligator (although USPS/OCA-T600-6 refers to Greene, 1993)[4] to assert that only

14   outputs, input prices, and a time trend should be in a cost function. Again, he shows his

15   lack of familiarity with the cost function literature. The neoclassical cost function has

16   been extended to include a vector of "technological conditions," such as the route

17   structure of a railroad, and a vector of output qualities or attributes.[5] Such variables are

18   so commonplace these days that one would have to wonder when Dr. Smith has last read

19   a paper on the subject.

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[4]Greene, W.H., Econometric Analysis, Second Edition, Prentice Hall, 1993.

[5]See for example, Friedlaender, Ann F., and Spady, Richard H., Freight Transport
Regulation, The MIT Press, Cambridge, 1980, pp. 204-205.

1   B. Econometric Issues

2   In his introduction to Section III A, Dr. Smith correctly summarizes the

3   differences between the pooled, fixed effects, and random effects models, and notes that

4   the pooled model finds variabilities near 100 percent. However, his preference for the

5   pooled model seems based solely on those results. He provides us with his conclusion

6   that the different intercepts in Dr. Bradley's model reflect short-run, monthly facility-

7   specific differences, but fails to explain why. He seems to be arguing that the fixed

8   effects model is a short-run model because of the frequency of the data observations. As I

9   have already pointed out, Dr. Smith's inference about data frequency and the short or

10   long run is flawed. Specification tests clearly support the fixed effects model over the

11   pooled model. The different intercepts reflect site differences, which are not necessarily

12   short-run or monthly.

13   If Dr. Smith is basing his argument on a visual inspection of plots, I think he is on

14   very shaky ground. Such plots show little or nothing, and are subject to selective

15   interpretation. Under cross-examination and in USPS/OCA-T600-11 (Tr. 28/ 15916), he

16   seems to admit this problem. These data are best analyzed with the sound application of

17   econometric techniques. Failure to include site dummies could grossly bias the estimated

18   variability. The cited figure from Hsiao (1986)[6] warns against precisely the mistake Dr.

19   Smith is making: although the pooled model (incorrectly) suggests a slope of about one,

20   the true common slope is much less. His attempt to use that figure to support his

[6]Hsiao, Cheng, Analysis of Panel Data, Cambridge University Press, New York, 1986, p. 7.

1   contention is so ironic that one might question whether he understands the point being

2   made with the figure.

3   Dr. Smith's description of the pooled model line as "longer-run expansion path" is

4   nonsense. With respect to costs, the term, expansion path, refers to a curve displaying the

5   long-run cost-minimizing input combinations for various levels of output (at different

6   isoquants). Graphically, it is a curve graphed with inputs on the axes. More importantly,

7   *there is no basis for his implicit assumption that facilities would become homogenous*

8   (have the same intercept) in the long run. Even if we allow him his contention that the

9   different intercepts in Dr. Bradley's model reflect short-run differences, why is there any

10   reason to presume that the intercepts would be identical in the long run?

11

12   C. Data Issues

13   Given that Dr. Bradley is much more familiar with the postal data, I will limit my

14   comments to those of a more general nature. I agree with Dr. Smith that additional

15   variables would probably improve the specification, but they are apparently not available.

16   Note that many of the suggested additional variables are broadly captured by the facility

17   dummy variables, and some of the less quantifiable variables are probably best accounted

18   for with dummy variables.

19   Many of the data scrub questions raised by Dr. Smith about, for example, the

20   number of observations dropped and data reliability, are quite reasonable, and a good

21   econometrician should be concerned about the possibility of biasing the sample. In Dr.

22   Bradley's data scrub, however, there are no obvious selection rules which might skew the

1   results. I think a number of Dr. Smith's questions have already been addressed by Dr.

2   Bradley in his direct testimony. Regarding the application of MODS results to non-

3   MODS facilities, Dr. Smith is probably justified to be concerned, but without non-MODS

4   data, it seems that the MODS results are more likely to be representative than the

5   previously assumed 100 percent variability.

6

7   D. Regulatory Standards

8   In Section V of his testimony, Dr. Smith makes generally unconvincing arguments

9   about the inability of Dr. Bradley's cost study to meet regulatory standards. In particular,

10   he focuses on Bonbright's (1961) criteria. They are nevertheless subjective and I would

11   not presume to tell the Commission whether or not to follow them. Despite being able to

12   select this particular set of criteria, Dr. Smith still finds it difficult to make them fit his

13   criticisms of Dr. Bradley's model. For example, the first criterion concerning aspects of

14   practicality such as simplicity and understandability does not imply Dr. Smith's

15   completeness interpretation or the consideration of all modeling alternatives.

16   Dr. Smith claims that a second criterion is that a study be free of controversy, but

17   it is hard to avoid when relevant parties' interests diverge. He tries to generate some of

18   his own controversy by appealing to the "common sense" that elasticities are

19   approximately 1, based on the visual plots. But after Dr. Bradley's study, continuing to

20   assume 100 percent variability could be considered controversial as well. Following Dr.

21   Bradley's study need not necessarily affect rate stability. If current rates are grossly

22   misaligned because of untested, past assumptions, any changes could be implemented

1   gradually. Fairness and efficiency are probably better served by Dr. Bradley's study.

2   Finally, Dr. Smith refers to rate criteria set forth in the Postal Reorganization Act,

3   which states that rates should be fair and equitable and that each class or type of mail

4   should bear its direct and indirect costs. His argument hinges solely on his belief that Dr.

5   Bradley's study measures mail processing costs incorrectly, which I have already refuted

6   above. Issues of equity and cost-bearing are more relevant in later stages of the

7   regulatory rate process.

1 II.   APPRAISAL OF DR. NEELS' TESTIMONY

2 A. Data Issues

3   Dr. Neels begins his testimony by claiming that Dr. Bradley's approach is

4 defective because of inappropriate measures of costs and volume. Because Dr. Bradley

5 has worked closely with the postal data, he will be addressing these issues in his rebuttal

6 testimony. At first glance, some of Dr. Neels' comments seem plausible but as Dr.

7 Bradley clearly shows in his rebuttal testimony, they are misplaced because of Dr. Neels'

8 lack of familiarity with postal activities and costs.

9   Regarding the reliability of the data, Dr. Neels points out some possibly legitimate

10 concerns. For sites which report sporadically, it may be difficult to determine the cause

11 even after very careful review of the data. But then, data screening procedures such as

12 Dr. Bradley's data scrub are probably the best solution. Fortunately, Dr. Bradley's

13 original and rebuttal analyses of any measurement errors reveal that the problem is not

14 critical. As supposedly an example of attenuation from measurement error, Dr. Neels

15 reports the differences in automatic and manual variabilities, but they may simply be due

16 to the fact that they are just different and not attenuation. His interpretation is at best

17 selective.

18   The next main concern raised by Dr. Neels is the data scrub procedure. Careful

19 econometric work does require scrutiny of data, and I believe that Dr. Bradley has made a

20 good faith effort in his data scrub. Any scrub might seem subjective, but should remain

21 as objective and reasonable as possible. Because of its subjective nature, a data scrub is

22 an obvious area for possibly unfounded criticism. With thousands of data points, such

1   rules of thumb or guidelines are necessary to avoid adding nonsensical data to the

2   analysis. My understanding is that deleted "unusual" observations represent extreme,

3   physically impossible situations or obvious data entry errors. I agree that outliers which

4   are still feasible observations should be kept in the data set, but that does not seem to be

5   the case here. Dr. Bradley seems to have maintained objectivity by symmetrically

6   eliminating both high and low outliers.

7   Requiring a minimum of 39 continuous observations or three years is arbitrary,

8   but does not necessarily bias the sample either. Dr. Neels' re-estimated equations with

9   "complete" data show different results as expected, but they also indicate no systematic

10   bias. Some variabilities are higher and some are lower. In no way do they support the

11   use of 100 percent volume variability. In his rebuttal testimony, Dr. Bradley

12   demonstrates that lowering the continuity standard to 26 observations does not materially

13   affect the estimated variabilities. Despite being open to easy criticism, Dr. Bradley's data

14   scrub does not appear to have biased the results, either in principle or in practice. While

15   some of Dr. Neels' conjectures about the data scrub might seem plausible, they do not

16   appear to have any real impact on the results.

17   I would also disagree with Dr. Neels' contention that independent replication

18   means reaching precisely the same results and agreeing that each step is appropriate.

19   Perfectly reasonable econometricians may disagree on the exact steps in an analysis and

20   yet conclude that the basic results are correct.

21

22

1   B. Econometric Results

2   Dr. Neels' comments about the time trends are essentially the same as those of Dr.

3   Smith and my comments there apply. Time trends can capture any dynamic factors, not

4   just technological change or productivity.

5

6   C. Long-run Variabilities

7   Again, as in Dr. Smith's testimony, Dr. Neels seems to equate frequency of data

8   observations with length of the run. Estimating actual volume variabilities caused by a

9   sustained increase in volume (sustained, meaning a few years, as defined by the Postal

10   Service) does not require that data be over that time period. Instead, it depends on the

11   specification of variables in the cost equation. In an economic sense, the cost equation is

12   a short-run cost equation because some inputs are fixed, not because the data covers a

13   four-week accounting period.

14

15   D. Cross-sectional Model

16   As Dr. Bradley has already clearly described in his direct testimony, cross-

17   sectional analysis suffers from several limitations as compared to cross-section, time-

18   series analysis with a panel data set. A well-known reference on the subject is Hsiao

19   (1986), which has been cited frequently in these hearings. At the risk of being redundant,

20   advantages of panel data include a large number of data points (reducing collinearity and

21   producing more efficient estimates), being able to analyze important economic questions

22   which cannot be studied with solely cross-section or time-series data, and mitigating

1   omitted variable bias. Panel data allows us to make inferences about the dynamics of

2   change from cross-sectional evidence by following given facilities over time.

3   Dr. Neels' claim that a cross-sectional model provides better long-run results is

4   merely an assertion.   When data have been collected cross-sectionally for a given time

5   period, there is still no reason to presume that the facilities are in long-run equilibrium or

6   that relevant long-run variables have been collected.

7   Dr. Neels also claims that the cross-sectional results from Dr. Bradley's

8   "between" model provide superior results. (The "between" model is used in a Hausman

9   test for correlation and the rejection of the random effects model.) This "between" model

10   is cross-sectional in the sense that all time series observations for a facility are collapsed

11   into one. But this averaging over time periods throws away possibly valuable time series

12   information. He suggests that this averaging will tend to cancel out measurement errors.

13   If any measurement errors are systematic over time by facility, they would not "cancel

14   out" as Dr. Neels claims or hopes. Because his arguments are speculative at best, his

15   preference for the "between" model seems based only on the results of near 100 percent

16   variabilities.

17   In his recommendations to the Commission, Dr. Neels proposes the rejection of

18   all of Dr. Bradley's data scrubs. He feels all data should be used, even those observations

19   with likely errors. This approach of using error-ridden observations is likely to produce

20   biased estimates. Just because a data scrub procedure involves some judgement does not

21   mean an econometrician should throw up his hands, and ignore data problems altogether.

22   No scrub is probably worse than a less than perfect scrub. Dr. Neels considers possible

1   biases from Dr. Bradley's scrub but provides no evidence of such bias. Also, what is the

2   point of following his approach when ultimately, he recommends against his own

3   modifications?

4

5   E. Traditional Assumed Variability

6   In concluding his testimony, Dr. Neels' main thrust relies on what he calls,

7   "common sense." Common sense is hard to define in this case. I agree that volumes

8   should affect mail processing labor costs, but an exact relationship of 100 percent volume

9   variability for each activity strikes me as implausible a priori and far from common sense.

10   In his summary of conclusions, he supports his common sense argument with plots of the

11   raw data, which I think are quite questionable. The traditional 100 percent volume

12   variability is simple-minded and easier, but that does not necessarily qualify for common

13   sense. The old rule is arbitrary with little or no economic basis. My opinion is that Dr.

14   Bradley's study has clearly shown that the traditional assumption is not justified. No

15   econometric study is flawless, and clever econometricians can find seemingly reasonable

16   objections to virtually any study. The goal however is to convince most of the skeptics,

17   not all the skeptics.

18

19   F. Comments from Oral Cross Examination

20   Because no other sections are directly related, I would like to comment on some

21   of Dr. Neels' answers from his oral cross examination in this section. Concerning the

22   calculation of the elasticities at the means (Tr. 28/15794-97), he essentially states that

1   cross terms with TPH and lagged TPH are not used in the calculation. That statement is

2   simply wrong, and brings into question his understanding of the derivative used to

3   calculate the elasticity. For example, the coefficient from the interaction term between

4   TPH and the manual ratio (MANR) is certainly part of the derivative. It drops out in the

5   calculation only when the derivative is evaluated at the mean, in which case the ratio of

6   the inserted mean MANR divided by its mean equals 1 and of course, $\ln(1) = 0$. Away

7   from the mean, this term would not drop out. In either case, it is used in the calculation.

1   III.   APPRAISAL OF NOI NO. 4

2   A. Comments on NOI No. 4 Itself

3   NOI No. 4 considers a generalization of Dr. Bradley's model by allowing slope

4   coefficients to vary across facilities and requests a test of this hypothesis. It is certainly a

5   valid request, but both witnesses, Dr. Smith and Dr. Neels, do not even consider it, much

6   less provide any evidence concerning this hypothesis in their testimonies. Dr. Neels and

7   especially Dr. Smith's assertions about plots of the data argue for the pooled model with

8   restrictions on both slope as well as intercept coefficients, not a generalization of Dr.

9   Bradley's fixed effects model. Finding that slopes should be allowed to differ across

10   facilities is not likely to support the (pooled model) plots or 100 percent volume

11   variabilities.

12

13   B. Dr. Neels' Response

14   Notice that despite criticisms in his direct testimony, Dr. Neels uses Dr. Bradley's

15   data scrub in his NOI response. In estimating the site-by-site regressions, he seems to

16   incorrectly use the overall sample mean when he should use site means, if any mean at

17   all. Furthermore, he uses Dr. Bradley's original serial correlation coefficients when they

18   should be updated by site. I would consider Dr. Neels' study to be somewhat sloppy. In

19   any event, for purposes of this NOI, perhaps these deficiencies are not that important

20   since Dr. Bradley also finds that the null hypothesis is rejected.

21   The implausibility of Dr. Neels' facility-specific variabilities indicates some

22   miscalculation, or a lack of understanding of how to do the calculation, given his oral

1   cross examination. In his specification, he cannot simply use the first-order coefficients

2   on TPH and lagged TPH or evaluate at the overall sample mean.

3

4   C. Implications of the Results

5   Although also rejecting the null hypothesis that the slope coefficients are identical

6   across sites, Dr. Bradley's NOI response is more credible because it is based on more

7   careful econometric work. He compares the results of the various models over the same

8   set of sites, corrects for serial correlation by site, and accurately calculates the elasticities.

9   Although the statistical tests reject the fixed effects model, they more strongly reject the

10   pooled model. Moreover, the generalization of the fixed effects model with different

11   slopes does not support the use of 100 percent variabilities.

12   If site-by-site estimations are used, arriving at a system-wide variability would

13   require some aggregation of the site-specific variabilities. It is not obvious how they

14   should be aggregated, and interested parties would undoubtedly have their own ideas on

15   the matter. As Dr. Bradley points out in his NOI response, site-by-site equations involve

16   some other tradeoffs. They may produce less reliable estimates because of

17   multicollinearity, and be less representative of the system when equations for some sites

18   cannot be estimated.

19   As an example of aggregating the site-specific variabilities, Dr. Bradley simply

20   averages them to produce a single variability. These calculations find overall variabilities

21   which are lower than those from the fixed effects model and significantly lower than the

22   approximately 100 percent variabilities from the pooled model. The results generated

1   from NOI No. 4 suggest that the traditional 100 percent variabilities should not continue

2   to be used and I think Dr. Bradley makes a compelling case that his fixed effects model is

3   the best overall model.

John S. Ying
Relevant Publications

"Hospital Costs and Excess Bed Capacity: A Statistical Analysis" (with Theodore E. Keeler), Review of Economics and Statistics, Vol. 78, No. 3 (Aug. 1996), pp. 470-481.

"Imposing Linear Homogeneity on Box-Tidwell Flexible Functional Forms," (with Richard T. Shin), Journal of Business & Economic Statistics, Vol. 12, No. 2 (Apr. 1994), pp. 261-265.

"Costly Gains to Breaking Up: LECs and the Baby Bells," (with Richard T. Shin), Review of Economics and Statistics, Vol. 75, No. 2 (May 1993), pp. 357-361.

"On Calculating Cost Elasticities," Logistics and Transportation Review, Vol. 28, No. 3 (Sept. 1992), pp. 231-235.

"Unnatural Monopolies in Local Telephone," (with Richard T. Shin), RAND Journal of Economics, Vol. 23, No. 2 (Summer 1992), pp. 171-183.

"Pricing in a Deregulated Environment: The Motor Carrier Experience," (with Theodore E. Keeler), RAND Journal of Economics, Vol. 22, No. 2 (Summer 1991), pp. 264-273.

"The Inefficiency of Regulating a Competitive Industry: Productivity Gains in Trucking Following Reform," Review of Economics and Statistics, Vol. 72, No. 2 (May 1990), pp. 191-201.

"Regulatory Reform and Technical Change: New Evidence of Scale Economies in Trucking," Southern Economic Journal, Vol. 56, No. 4 (Apr. 1990), pp. 996-1009.

"Measuring the Benefits of a Large Public Investment: The Case of the U.S. Federal-Aid Highway System," (with Theodore E. Keeler), Journal of Public Economics, Vol. 36, No. 1 (June 1988), pp. 69-85.