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In the Matter of: POSTAL RATE AND FEE CHANGE

Docket No. R2000-1

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ANN RILEY & ASSOCIATES, LTD.
1025 Connecticut Avenue, N.W., Suite 1014
Washington, D.C. 20036
(202) 842-0034

CHAIRMAN GLEIMAN: You are already under oath in the proceeding so your counsel can proceed with your testimony.

MR. McKEEVER: Thank you, Mr. Chairman.

Whereupon,

KEVIN NEELS,

a witness on behalf of United Parcel Service previously duly sworn, was further examined and testified as follows:

REBUTTAL DIRECT TESTIMONY

BY MR. McKEEVER:

Q Thank you, Mr. Chairman.

Dr. Neels, I have just handed you a copy of a document entitled, "Rebuttal Testimony of Kevin Neels on behalf of the United Parcel Service" and marked UPS-RT-1, which was previously served in this proceeding.

Do you have any changes to make to the version that was previously served?

A I don't believe so, although I did notice in preparing for today that there is some inconsistency in mathematical notation and I thought it might be useful just to clarify for the record what my intention was.

I can do that now if you like.

Q Could you do that, please?

A The inconsistency shows up in the first equation, which begins on page 10 and runs on to page 11 and if you

look at the bottom of page 10 in the equation there is a term following the beta 2, which is a parentheses containing within it the log of cubic foot miles for Contract J divided by the average of cubic foot miles calculated across all the contract segments in the sample and then that whole expression is squared.

Now if you look in the following page, there is a comparable term for route length. It follows the beta 4 coefficient but there is a slight difference in notation in that the parentheses are -- they follow the designation for the logarithm rather than preceding it.

The way that the equation is shown on page 11 is actually the notation used by Dr. Bradley in his purchase transportation testimony.

My intention was to switch to the notation shown on the bottom of page 10 because that makes it clear that the order in which the operations are being made is first the term -- first you take the logarithm and then you square the result rather than the reverse of squaring and then taking the logarithm.

Now I think that the notation shown on the top of page 11 is ambiguous from a mathematical standpoint. However, my understanding is that everyone who has used the trans-log in any of the proceedings in this, in any pieces of testimony in this proceeding understands what the

intention is and what happens is I noticed as I go down I actually mix the two forms in the equation, so I don't know if it is necessary to modify the testimony or if this explanation is sufficient.

I think everyone understands it. I just wanted to be precise for purposes of the record.

MR. McKEEVER: Mr. Chairman, I think the question really is as an example on the bottom of page 10 that one parentheses after the beta 2 whether you would remove that and move it to after the LN or not, and it could be interpreted to be either way and I think Dr. Neels, as I understand him, is just saying since it is unclear as to which operation is performed when he just clarified which operation is to be performed when so the notation is okay as it is but he is just clarifying is my understanding.

BY MR. McKEEVER:

Q Is that an accurate statement, Dr. Neels?

A I think so. I show it both ways in the equation but I think everybody understands what we are doing here, at least all of the people who are involved in the technical aspects of the testimony.

MR. McKEEVER: With that, Mr. Chairman, I would suggest that there be no need to make any revision in the testimony since the explanation is now provided on the record, and I would move that the rebuttal testimony of

Kevin Neels on behalf of United Parcel Service and marked UPS-RT-1 be admitted into evidence and transcribed into the record.

CHAIRMAN GLEIMAN: Is there an objection?

[No response.]

CHAIRMAN GLEIMAN: Hearing none, if counsel will provide the court reporter with two copies of the rebuttal testimony of Witness Neels I will direct that that testimony be transcribed into the record and received into evidence and I am convinced that all of the people who understand it understand it and those who don't, don't.

[Rebuttal Testimony of Kevin Neels, UPS-RT-1, was received into evidence and transcribed into the record.]

UPS-RT-1

BEFORE THE
POSTAL RATE COMMISSION

POSTAL RATE AND FEE CHANGES, 2000	:	DOCKET NO. R2000-1

REBUTTAL TESTIMONY
OF
KEVIN NEELS
ON BEHALF OF
UNITED PARCEL SERVICE

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BIOGRAPHICAL STATEMENT

My name is Kevin Neels. I have previously submitted testimony in this proceeding on the volume variability of mail processing labor costs (UPS-T-1) and on purchased transportation costing (UPS-T-3). My biography is set forth in that testimony. See Tr. 27/12773-74.

SCOPE AND PURPOSE OF TESTIMONY

I have been asked to review and comment on the testimony of Michael A. Nelson regarding the revenue requirement associated with certain categories of transportation costs, and on the variability of purchased highway transportation costs (MPA-T-3).

Mr. Nelson's revenue requirement testimony is flawed. He argues for reductions in the revenue requirement to reflect certain potential cost savings. I will show that the cost savings he asserts the Postal Service can achieve are speculative at best, that he has failed to analyze the feasibility of the changes in operations which he suggests, and that he offers no credible evidence that these savings can be realized in the test year.

There are also serious flaws in Mr. Nelson's alternative econometric model of purchased highway transportation cost variability. For example, he has used inappropriate estimation techniques, and he has failed to carry out elementary statistical tests of his model. I will show that these tests demonstrate decisively that his model should be rejected.

DISCUSSION

The Assertions Regarding the Revenue Requirement Are Speculative and Unsupported.

1. The Assertions

Mr. Nelson offers a number of comments on the revenue requirement associated with certain categories of purchased transportation costs. His opinions are contained in discussions of four "Roll-Forward Issues":[1]

- **Highway Contract Renewal Process.** Mr. Nelson asserts that the rates paid by the Postal Service when highway contracts are renewed are materially higher than those paid when a new contract is issued through a competitive bidding process.[2] He then argues that the Postal Service could realize substantial savings by tightening administrative requirements for these contracts, and he recommends that Test Year costs be reduced accordingly.[3]

- **Highway Contract Obsolescence.** He also asserts that because of changes in market conditions and service requirements, a substantial number of highway contracts provide service that, by the end of the contract period, is no longer needed.[4] He argues that the Postal Service could realize

1. Tr. 28/13416-21. In addition, he discusses a number of other issues affecting the revenue requirement for which, he says, data are not yet available.
2. Tr. 28/13416.
3. Tr. 28/13417.
4. Tr. 28/13418.

substantial savings by renegotiating these contracts in a more timely manner, and he recommends that Test Years costs be reduced accordingly.[5]

- **Amtrak Premium and Terms.** Mr. Nelson asserts that a substantial portion of the mail carried on Amtrak could be moved on the highway network at a lower cost.[6] He then argues that the Postal Service could realize substantial savings either by negotiating more aggressively with Amtrak, or by diverting mail to the highway system. He recommends that Test Year costs be reduced to reflect these potential savings.[7]

- **Freight Rail Rates.** Mr. Nelson notes an interrogatory response by the Postal Service indicating that there are no volume incentive or discount rates in any of its contracts for rail transportation services.[8] He attributes the absence of such rates in part to the fact that Conrail, the primary provider of freight rail service, enjoyed a near absence of intramodal competition in much of its service territory.[9] He predicts that the breakup of Conrail will enable the Postal Service to achieve reductions in the rates it pays, and he urges that Test Year costs be reduced to reflect his estimated savings.[10]

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5. Tr. 28/13418.
6. Tr. 28/13419.
7. Tr. 28/13420.
8. Tr. 28/13420.
9. Tr. 28/13421.
10. Tr. 28/13421.

2. Analysis

A number of common themes appear in Mr. Nelson's revenue requirement testimony and in his testimony regarding the volume variability of purchased highway transportation costs. For this reason, it is useful to consider them together.

For example, Mr. Nelson generally begins with an assertion that the Postal Service is operating inefficiently. In some cases -- specifically, in his highway contract obsolescence, contract renewal, and Amtrak arguments -- these assertions are clear and direct.[11] In the case of freight rail rates, he lays a portion of the blame on the doorstep of Conrail, although he also asserts that "the Postal Service should be able to obtain volume discounts from at least some of the other railroads from which it purchases transportation services."[12] Purchased highway transportation is the only area in which he does not explicitly accuse the Postal Service of inefficiency, apparently because the econometric models of Dr. Bradley draw most of his fire.[13]

In most cases, Mr. Nelson presents little or no evidence documenting that the efficiency gains he discusses are in fact achievable. For example, he asserts that increasing vehicle size would allow the Postal Service to expand highway capacity at a relatively low incremental cost.[14] While this is not an implausible assertion, he presents no data to back it up. In other instances, his assertions are less plausible but just as undocumented. The only basis we have for believing that the Postal Service could

11. Tr. 28/13416-21.
12. Tr. 28/13421.
13. Tr. 28/13411.
14. Tr. 28/13411-12.

renegotiate "obsolete" contracts, for example, or negotiate lower Amtrak or freight rail rates, are his assertions that this is so. Even there he hedges his statements, using phrases like "should be" rather than "will be."[15]

In all cases, Mr. Nelson disregards the effects his recommendations might have on other categories of Postal Service costs. This is apparent in his treatment of purchased highway transportation.

He notes that in many instances the vehicles used by highway contractors are smaller than the largest vehicles generally in use: "[T]he Postal Service procures transportation using vehicles with a wide range of capacities. These vehicles are typically not the largest capacity vehicles (vans or trailers) that are available."[16] He then asserts that the Postal Service has "considerable latitude to alter the sizes of vehicles used" throughout its network.[17] Citing the testimony of Postal Service witness Young in Docket No. R97-1, he argues that it is much less costly to increase the size of the vehicles used on a route than to increase the number of trips made.[18] From these two assertions, he reasons that it should be possible to expand capacity at a low incremental cost.[19]

However, he provides no evidence that this is how the Postal Service actually expands capacity. Increases in capacity can be achieved by using larger vehicles,

15. Tr. 28/13418.
16. Tr. 28/13409.
17. Tr. 28/13409.
18. Tr. 28/13409.
19. Tr. 28/13411.

lengthening routes, increasing the number of runs, adding new contracts, or taking any of an infinite number of combinations of these actions. In such a complex environment, it is unlikely that a single mode of capacity expansion will or should universally dominate. Mr. Nelson's only basis for arguing that changes in vehicle size will or should be the primary mode of capacity expansion is his assertion that this is the cost-minimizing thing to do.

Mr. Nelson concedes that increasing vehicle size is not always an option.[20] Even in such instances, however, he asserts that it will often be possible for the Postal Service to reorganize its network to provide less circuitous, and hence less costly, service. He concludes that "there is an elasticity of gross CFM [cubic foot miles] with respect to net CFM that is less than 1.0, and that causes the Postal Service highway transportation models to overstate the true variability of cost with respect to the volume of mail being moved . . . ."[21]

Concentrating mail in a small number of very large vehicles may economize on transportation costs, but it could also exacerbate the peaking of activity and staffing requirements for platform activities at mail processing facilities. Mr. Nelson concedes that minimizing overall costs could lead to transportation costs higher than if transportation costs were minimized without regard to the impact of doing so on other operations.[22] Of course, costs should be minimized overall. Moreover, Mr. Nelson ignores the possible impact of his recommendation on the need to meet service

20. Tr. 28/13409.
21. Tr. 28/13410.
22. Tr. 28/13436.

standards. Nonetheless, he makes his recommendations without attempting to analyze their broader effects.

Mr. Nelson also fails to account for the direct costs associated with his recommendations. In the case of his highway contract renewal proposal, the clear implication of his testimony is that at the end of its term, every contract should be put out for competitive bid.[23] But this would create a substantial administrative burden, and undoubtedly would require the hiring of additional contracting personnel. He ignores the possibility that the Postal Service might have to compensate contractors to induce them to terminate contracts for services that are no longer needed. None of these costs, or similar costs associated with his other proposals, is factored into his assessment of the changes he suggests.

The most serious flaw in Mr. Nelson's approach is the extremely speculative nature of the cost savings he argues are possible. Mr. Nelson does not assert unequivocally that these savings will be realized. He merely states that "It is my expert opinion that the savings of this type that I estimate can be achieved in the test year. It is up to the Postal Service to determine whether it will take the actions needed for these savings to be realized in the test year."[24] He adds, however: "It is my understanding, from MPA witness Cohen, that the Postal Service is assessing the merits of taking steps

23. See Tr. 28/13417-18. He is somewhat equivocal on this point, talking only about "Tightening administrative requirements to ensure competitive terms." Tr. 28/13417. However, he provides no indication as to what action might achieve this result other than putting each contract out for competitive bid.

24. Tr. 28/13442.

to achieve these savings in the test year."[25] Thus, all we know is that the Postal Service is considering his suggestions. We do not know that they will be adopted, nor when the promised savings can be expected to materialize.

Mr. Nelson's testimony provides a shaky and unreliable basis for making substantial changes to Test Year costs. His estimates of savings reflect simple calculations from scanty data. In addition, he has failed to consider at all the costs of implementing his recommendations, or the impact that they would have on other aspects of postal operations (including the need to meet service standards). Thus, we cannot tell whether they would survive serious consideration and evaluation. Even if the measures he advocates were to proceed, unanswered questions would still remain about when the implementation process would start, how long it would take, and whether they would effectively realize all of the alleged savings.

Mr. Nelson's Analysis of the Volume Variability of Purchased Highway Transportation Costs Is Unreliable and Should Be Ignored.

Mr. Nelson has argued that the study of purchased highway transportation cost variability first introduced by Postal Service witness Bradley in Docket No. R87-1 as refined in subsequent cases (including this proceeding) presents biased results. In particular, he asserts that Dr. Bradley's econometric models overstate the volume variability of costs because they ignore the efficiencies associated with the use of larger vehicles[26] -- a contention discussed above. Using a combination of a priori argument

25. Tr. 28/13442.

26. Tr. 28/13410.

and results derived from an alternative econometric analysis, Mr. Nelson concludes that the volume variability of purchased highway transportation costs is much lower than the Postal Service has estimated.[27] He presents a set of alternative volume variabilities that have the effect of transferring a large body of costs from the attributable category to the institutional category.[28]

One of the most prominent features of this new approach, according to Mr. Nelson, is a stratification of the estimation sample between contracts that rely upon the largest available vehicles and those in which smaller vehicles are used.[29] In the case of the former contracts, he argues, the only way to increase the amount of capacity provided is to increase the number of trips made.[30] Accordingly, he treats the costs associated with these contracts as 100 percent volume variable.[31] These contracts do not enter into his econometric analysis. Rather, data for his regression analysis come solely from the contracts in which smaller vehicles are used.[32]

1. Differences between Mr. Nelson's Approach and Dr. Bradley's Approach

Although his testimony emphasizes his stratification of the contract sample, Mr. Nelson's analytical approach differs in a number of other important respects from that of Dr. Bradley. To clarify the record, I summarize those differences below:

27. Tr. 28/13411.
28. Tr. 28/13424 (Table 1).
29. See Tr. 28/13411-12.
30. Tr. 28/13412.
31. Tr. 28/13412.
32. Tr. 28/13412.

(a) Differences in Sample Selection

Mr. Nelson's sample selection procedures differ from those of Dr. Bradley. Mr. Nelson excludes observations corresponding to "power only" contracts.[33] In contrast, Dr. Bradley associates a standard trailer size with each of these observations.[34] Mr. Nelson also asserts (without providing evidence to support this assertion) that Dr. Bradley's sample selection criteria "appear in some instances to exclude good data."[35] He thus applies different selection criteria that yield a somewhat different sample.[36] Finally, as I stated earlier, he limits his sample to contracts with less than full size trucks.

(b) Differences in Model Specification

While Mr. Nelson bases his econometric analysis on Dr. Bradley's data, the mathematical form of his model differs substantially. Dr. Bradley's model uses a generalized mathematical form; Mr. Nelson's is more restricted, and is in a sense a special case of Dr. Bradley's model. The difference between the two approaches is most clear when they are expressed in mathematical form.

Dr. Bradley's model, shown at page 21 of USPS-T-18, has the following form:

$$\ln Cost_j = \alpha + \sum_i \delta_i D_i + \beta_1 \ln\left(\frac{CFM_j}{\overline{CFM}}\right) + \beta_2 \left(\ln\frac{CFM_j}{\overline{CFM}}\right)^2 + \beta_3 \ln\left(\frac{RL_j}{\overline{RL}}\right) \quad (1)$$

33. Nelson Workpaper WP-4, page 1.
34. USPS-T-18, page 24.
35. Tr. 28/13411.
36. Nelson Workpaper WP-4, page 1.

$$+\beta_4 \ln\left(\frac{RL_j}{\overline{RL}}\right)^2 + \beta_5\left(\ln\frac{CFM_j}{\overline{CFM}}\right)\left(\ln\frac{RL_j}{\overline{RL}}\right)$$

where $Cost_j$ is the cost associated with contract j. CFM_j and RL_j represent total cubic foot miles and run length, respectively. The subscripted terms represent the values for a specific contract. The barred terms represent averages computed across all contracts. The summation term contains a set of region-specific dummy variables. α, β and δ represent estimated parameters. In similar notation, Nelson's model can be written as:

$$\ln\left(\frac{Cost_j}{Runs_j}\right) = \alpha + \sum_i \delta_i D_i + \beta_1 \ln\left(\frac{CFM_j}{Runs_j}\right) + \beta_2 \ln RL_j \qquad (2)$$

where $Runs_j$ is the number of trips taken under contract j.[37]

With a little effort, the relationship between these models can be made clear. First, to simplify notation, I will ignore the summation term that is common to both models. One can view this as "folding" the summation term into the constant α. Second, I remove the mean-centering from Dr. Bradley's equation to arrive at the following somewhat simpler form:

$$\ln Cost_j = \alpha + \beta_1 \ln CFM_j + \beta_2 \ln(CFM_j)^2 + \beta_3 \ln RL_j + \beta_4 \ln(RL_j)^2 \qquad (3)$$

$$+\beta_5 \ln CFM_j \ln RL_j$$

37. Nelson Workpaper WP-4, page 3.

In Dr. Bradley's analysis, mean-centering is a computational convenience that "allows the relevant elasticity to be derived easily from the estimated equation."[38] It has no substantive effect on his results.[39]

I note that by definition:

$$CFM_j = RL_j \cdot Runs_j \cdot VC_j \quad (4)$$

where $Runs_j$ is the number of trips, or runs, provided for under contract j, and VC_j is the average capacity in cubic feet of the vehicles used in contract j. Insertion of equation (4) into equation (3) suggests the following fully generalized translog model that includes both Dr. Bradley's model and Mr. Nelson's model as special cases:

$$\ln Cost_j = \alpha + \eta_1 \ln RL_j + \eta_2 \ln Runs_j + \eta_3 \ln VC_j + \eta_4 (\ln RL_j)^2 \quad (5)$$

$$+ \eta_5 (\ln Runs_j)^2 + \eta_6 (\ln VC_j)^2 + \eta_7 \ln RL_j \ln Runs_j$$

$$+ \eta_8 \ln RL_j \ln VC_j + \eta_9 \ln Runs_j \ln VC_j$$

The coefficients in equations (5) and (3) are related as follows:

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38. USPS-T-18, page 21.

39. The coefficients derived from Dr. Bradley's mean-centered data will be slightly different from the coefficients that would be produced by equation (3). This slight difference results from the way in which Dr. Bradley carries out his mean-centering. He first mean-centers the underlying *CFM* and *RL* variables and then forms the squared and cross-product term. This introduces some slight nonlinearity into the model and causes the mean-centered results to differ slightly from those produced when natural units are used. If Dr. Bradley had instead first formed the square and cross-product terms and then mean-centered the data, the two approaches would yield strictly identical results.

$$\eta_1 = \beta_1 + \beta_3 \qquad (6)$$

$$\eta_2 = \beta_1$$

$$\eta_3 = \beta_1$$

$$\eta_4 = \beta_2 + \beta_4 + \beta_5$$

$$\eta_5 = \beta_2$$

$$\eta_6 = \beta_2$$

$$\eta_7 = 2\beta_2 + \beta_5$$

$$\eta_8 = 2\beta_2 + \beta_5$$

$$\eta_9 = 2\beta_2$$

Thus, equation (5) can be transformed into Dr. Bradley's model by imposing the appropriate set of linear restrictions on the coefficient values.

In a similar way, one can demonstrate the relationship between equations (2) and (5):

$$\eta_1 = \beta_1 + \beta_2 \qquad (7)$$

$$\eta_2 = 1$$

$$\eta_3 = \beta_1$$

$\eta_4 = 0$

$\eta_5 = 0$

$\eta_6 = 0$

$\eta_7 = 0$

$\eta_8 = 0$

$\eta_9 = 0$

In short, Mr. Nelson's cost equation falls within the same general class of models as Dr. Bradley's. Mr. Nelson's, however, is far less general.

(c) Differences in Estimation Technique

Mr. Nelson and Dr. Bradley rely on different econometric techniques to estimate the coefficients of their models. Mr. Nelson weights each observation according to the number of trips, or "runs," that it represents.[40] He presents no econometric or statistical justification for this weighting scheme, justifying it instead by a desire to guarantee that "observations no longer differ with respect to the number of runs they represent . . . ."[41]

40. Tr. 28/13412.
41. Tr. 28/13412.

2. Flaws in Mr. Nelson's Econometric Work

Mr. Nelson has failed to follow appropriate and generally accepted procedures for carrying out econometric studies of the type he has introduced. After making what is essentially an operational argument about how the Postal Service does (or rather should) increase the amount of highway capacity it purchases, Mr. Nelson makes a number of modifications to Dr. Bradley's econometric analysis that are of dubious relevance to his operational arguments. These modifications yield substantially lower volume variabilities. He proffers these results as superior to those of Dr. Bradley. However, he has failed to carry out even the most elementary statistical tests to determine whether the data support his approach or his claims regarding its superiority.

(a) Failure to Test Model Restrictions

Mr. Nelson's model (and, for that matter, Dr. Bradley's model as well) can be regarded as a member of the generalized class of models depicted in equation (5). To arrive at Mr. Nelson's model, one must impose a priori restrictions on a large number of model parameters. Since Dr. Bradley has already introduced and defended a more general translog cost function, good practice demands that Mr. Nelson test whether or not the restrictions he imposes are consistent with his data. It is a simple matter to use a standard F statistic to test the null hypothesis that his coefficient restrictions hold against the alternative hypothesis that the true values of the coefficients are inconsistent with his assumptions. However, he has not conducted any such tests.[42]

42. Tr. 28/13438.

One can readily postulate a number of more general versions of Mr. Nelson's model that would appear to represent reasonable alternatives. For example, Mr. Nelson normalizes cost and *CFM* by the number of runs specified in the contract. An obvious generalization of his model would be:

$$\ln Cost_j = \alpha + \beta_1 \ln CFM_j + \beta_2 \ln RL_j + \beta_3 \ln Runs_j \qquad (8)$$

Mr. Nelson's specification corresponds to a version of equation (8) in which $\beta_3 = 1 - \beta_1$.

One could also test Mr. Nelson's specification against the fully generalized translog shown in equation (5).

In the context of a debate about how to measure the volume variability of purchased highway transportation costs, these alternatives are (or should be) obvious to anyone wishing to move the debate constructively forward. The econometric analysis sponsored by Dr. Bradley has been the accepted standard for a number of years. If Mr. Nelson wants to urge rejection of Dr. Bradley's model in favor of an alternative approach, the burden of making this case falls on him. The Commission, the Postal Service, and other intervenors deserve evidence that the model Mr. Nelson is putting forward is superior to the accepted alternative, or at least that it is equally consistent with the data. He has failed to provide this.

(b) Inappropriate Use of Weighted Regression Analysis

Mr. Nelson's rationale for weighting by the number of runs in each contract is inconsistent with generally accepted criteria for the use of this technique. The estimator he uses -- weighted least squares -- is an appropriate response to the problem of

heteroscedasticity, a condition in which the requirement that the error term for a regression have an equal variance for all observations is violated. In the presence of heteroscedasticity, efficient estimation involves giving relatively more weight to observations with a low error variance, and relatively less weight to observations with a high error variance. Nowhere in Mr. Nelson's testimony, however, is this issue of error variance mentioned. No relevant calculations are contained in his testimony or workpapers. He provides no quantitative support for the use of this estimator, or for the specific weighting scheme he employs.

Mr. Nelson does not set forth explicitly or clearly his reasons for using weighted regression analysis. The fact that he has used this procedure is contained in a footnote to a statement about how his normalization of cost and CFM by numbers of runs guarantees that observations "no longer differ with respect to the number of runs they represent."[43] I infer from the context that he apparently believes that by normalizing his data and weighting by number of runs, he is somehow able to treat each run as a separate observation. This, of course, is nonsense. A contract is still only one contract, regardless of how many runs it covers. A single contract for 500 runs is likely to look dramatically different from 50 contracts for 10 runs each, or 500 contracts for one run each. A large contract does not provide any more information about how cost varies with output than does a smaller contract. Each still represents only one observation.

43. Tr. 28/13412.

(c) Statistical Tests of the Nelson Specification

I have used Mr. Nelson's less-than-full-size intra-P&DC van and trailer samples to estimate coefficients for the model specifications shown in equations (2), (8), and (5). All of these models (including Mr. Nelson's own model) suffer from an extremely high degree of multicollinearity among their right-hand side variables. To improve the precision and reliability of the regression estimates, I employed a multicollinearity correction procedure that uses an auxiliary regression to break the collinearity. For equations (2) and (8), this regression takes the following form:

$$\ln RL_j = \gamma_0 + \gamma_1 \ln(VC_j \cdot RL_j) + \varepsilon_j \quad (9)$$

In place of the log of run length variable, I use the residual term ε_j from equation (9). Since this substitution represents a linear transformation of the X matrix for the regression, mathematically it has no effect on any of the overall regression statistics of interest. However, it produces a cross-products matrix that is less nearly singular and that, as a result, can be more accurately inverted.

For equation (5), my auxiliary regression has the following form:

$$\ln Runs_j = \gamma_0 + \gamma_1 \ln(VC_j \cdot RL_j) + \gamma_2 \ln RL_j + \varepsilon_j^* \quad (10)$$

I then substituted the residual term ε_j^* from equation (10) for the variable representing log of number of runs.

Finally, because I see no substantive justification for Mr. Nelson's use of a weighted regression, I have estimated these regressions using ordinary least squares.

From these results, one can perform a number of specification tests. One can test whether the data support the decision to normalize by number of runs; one can also test both the Nelson specification and the unnormalized version of his model against the generalized translog that includes Dr. Bradley's model as a special case. The F statistics corresponding to the null hypotheses that the simpler models are correct are shown below in Table 1.

TABLE 1

Alternatives to Nelson Model for Estimating Volume Variabilities: Intra-P&DC Vans and Trailers – Less than Full Sized Trucks; Variables Adjusted for Multicollinearity

Vans

Model Comparison	F Statistic	F Statistic Critical Value (99% Level)
Nelson vs. Unnormalized Nelson	256.37	6.64
Nelson vs. General Translog	93.95	2.64
Unnormalized Nelson vs. General Translog	63.73	2.80

Trailers

Model Comparison	F Statistic	F Statistic Critical Value (99% Level)
Nelson vs. Unnormalized Nelson	70.30	6.64
Nelson vs. General Translog	48.11	2.64
Unnormalized Nelson vs. General Translog	40.41	2.80

For every comparison presented in Table 1, the simpler and more restricted model is rejected by a decisive margin. Both for vans and for trailers, the data are strongly inconsistent with Mr. Nelson's decision to normalize by number of runs. The unnormalized regressions are clearly preferred. In addition, the data strongly support selection of the generalized translog over either of the other two forms. In short, within

this contract segment the data provide no empirical support for Mr. Nelson's model form.

Although strictly speaking one cannot extrapolate these findings for Intra-P&DC vans and trailers to other contract segments, I know of no reason to expect analyses of other contract types to yield different results. These results do, however, clearly underscore the importance of conducting such statistical testing and demonstrate that Mr. Nelson has failed to provide elementary and important information critical for the proper evaluation of his testimony.

* * *

In the end, Mr. Nelson's econometric analyses are largely irrelevant to the principal thrust of his argurnents regarding purchased highway transportation cost variability. He asserts that it is less costly to expand output by increasing vehicle capacity than by expanding the number of runs. If that is in fact the case, there is no reason why an appropriately specified general cost model like the translog model cannot document that fact. Mr. Nelson's highly restricted models are far more likely to present a biased picture of the cost structure of purchased highway transportation than those that have been offered by Dr. Bradley and previously accepted by the Commission.

RECOMMENDATIONS AND CONCLUSION

In his testimony, Mr. Nelson identifies a number of ways in which he believes it is possible for the Postal Service to improve the efficiency of its transportation activities. For the most part, these ideas are plausible on their face. However, the evidence

presented is far too limited to permit their thorough evaluation. We cannot conclude that they would prove to be feasible, or that, if implemented, they would not compromise service standards or yield savings of the magnitude claimed. Even if they were to prove to be every bit as promising as Mr. Nelson suggests, it would still be necessary to implement them fully before those efficiency gains could be realized. That process has not even begun. Hence, it would be foolish and unwarranted to adjust Test Year cost estimates as Mr. Nelson recommends.

The econometric studies of purchased highway transportation cost variability sponsored by Mr. Nelson should also be disregarded. The conceptual model he presents has little or no connection to his empirical work. His econometric analysis is methodologically flawed and is not supported by the data in the record. He has failed to conduct the most elementary statistical tests of the validity of his approach. The analysis that I have been able to conduct indicates strongly that if he had conducted such tests, he would have been compelled to reject his own models.

MR. McKEEVER: Thank you, Mr. Chairman. Dr. Neels is available for cross examination.

CHAIRMAN GLEIMAN: The Postal Service is the only party that I am aware of that has asked for oral cross examination of this witness.

Is there anyone else who wishes to cross examine the witness?

MR. McBRIDE: Yes, Mr. Chairman.

CHAIRMAN GLEIMAN: Wearing your multiparty or --

MR. McBRIDE: My multiparty hat, yes.

CHAIRMAN GLEIMAN: Okay, that being the case, why don't you proceed, Mr. McBride.

MR. McBRIDE: Thank you.

CROSS EXAMINATION

BY MR. McBRIDE:

Q Good afternoon again, Dr. Neels. We have met before.

I think I understand what you just said to your counsel and was agreed upon on the record, but I gather what you're saying is the logarithmic work should be done first before the squaring of the term within the parentheses, if I understand.

A That's exactly right.

Q In any event, I want to ask you about a few other things with respect to Mr. Nelson's testimony on behalf of

the MPA et al, community, on whose behalf I appear today.

You've made a number of comments about Mr. Nelson's transportation testimony.

A I have.

Q And it's that to which I want to direct your attention.

First of all, you say -- and I'm going to apologize to you in advance here if the miracles of modern technology have given me a different pagination for my testimony than what you had, because it looked like when your counsel was running through it.

But I was able to follow the equations. But I'm looking at page 17, and a paragraph that beings, "In the context of a debate..." but I'm going to guess here that you may be a page off.

A Yes. I show that as being on page 16, starting on line 9.

Q Fine. I'm a page ahead of you, but in any event, there's a sentence that reads: If Mr. Nelson wants to urge rejection of Dr. Bradley's model in favor of an alternative approach, the burden of making this case falls on him.

Do you see that?

A I see that, yes.

Q All right, I'd like to ask you then a little bit about Mr. Nelson's model versus Dr. Bradley's model.

On page -- well, it's my 13, so it's probably your 12. It's the page that has Footnote 39 on it.

A Okay, I have Footnote 39.

Q Okay, the sentence from which Footnote 39 hangs, describes Dr. Bradley's analysis, and refers to the phrase, mean centering; do you see that?

And then the next sentence says it has no substantive effect on his results.

A I see that.

Q And then you have a Footnote 39 that explains that it could have, actually, the way he did it, make a bit of a difference, but if he had done it a different way, it would have made no difference; is that a fair summary of that footnote?

A Yes.

Q Okay

Now, would you, if you happen to have it there, turn to Professor Bradley's testimony, USPS-RT-8, which is already in evidence in this case. Do you happen to have that there?

A RT-8?

Q Yes.

A I don't have that before me.

MR. McBRIDE: May I approach the witness?

COMMISSIONER LeBLANC: Please. To make sure the

record is clear, is that Dr. Bradley's rebuttal?

MR. McBRIDE: Yes, it is. It's the rebuttal and is the testimony that was put on the record on this last Monday as RT-8.

And I have directed the witness's attention to pages 18 and 19 of that testimony.

COMMISSIONER LeBLANC: Thank you. Mr. McKeever, have you seen a copy of it? Are you familiar with it.

MR. McKEEVER: Yes, Mr. McBride showed me a copy on his way to the witness.

COMMISSIONER LeBLANC: Fine, thank you very much. Go ahead, Mr. McBride.

MR. McBRIDE: Thank you.

BY MR. McBRIDE:

Q Dr. Neels, is what appears on page 18, starting at about line 14, through page 19, line 9, Dr. Bradley's analysis which you referred to as -- it's his testimony in part from R97-1 which he's readopting here, and then some further testimony.

And does that include the concept that you referred to as mean centering?

A Give me a chance to read it.

[Pause.]

I don't believe that it does.

Q The words don't appear there, as I read it, but I

want to direct you back to your own testimony for a minute, and let's go side-by-side here for a moment, if we can.

It's the paragraph and the work that leads up to that Footnote 39 and the text accompanying it, to which I just directed your attention.

A Yes?

Q And you refer to mean centering in Dr. Bradley's work. And I'm told that that is a shorthand to refer to what he is describing there on page 18 and page 19, to which I just directed your attention.

A In mean centering, you would be dividing each of the -- as I understand it, you would be dividing each of the variables that appear in his cost equation by the average for on a variable-by-variable basis.

The average is calculated across the contracts in the sample. And that would re-scale them, but it wouldn't affect their variance, and it would be the variance that would influence -- that would -- it would be differences in variance that would cause heteroskedasticity, and I don't believe that mean centering would eliminate that.

Q Okay.

A I believe --

Q Is another term for this normalization?

A Normalization in the sense that I think what Dr. Bradley was trying to do was take the constant term out of

his model and arrive at a model where he could pull the variability off a single coefficient without having to calculate variabilities, including multiple coefficients.

So it is a normalization.

Q Very good. And in your Footnote 39, I think you draw the conclusion that if Dr. Bradley had done his work in a certain way as we discussed earlier, it would have had no effect on his results?

A That's correct.

Q Right. Now, is it a fair statement that, nevertheless, at pages 18 and 19 in the part of Dr. Bradley's testimony to which I have drawn your attention, he concluded that it would change the results?

A I --

MR. McKEEVER: Mr. Commissioner, a point of clarification: Unless I'm mistaken, at one point when we were talking about Dr. Bradley's rebuttal testimony, I think there was an indication that Dr. Bradley, in that testimony, was referring to his testimony in a prior proceeding.

MR. McBRIDE: Yes, he is.

MR. McKEEVER: And I jus want to make sure that we're clear whether we're talking about Dr. Bradley's work in this case or in the prior case, or are they the same?

MR. McBRIDE: It's both, because if you -- and I will be happy to give you a moment to look at this -- you

will see that Dr. Bradley quoted his testimony from R97-1, then went on to give some additional testimony beyond that, and so it's both.

MR. McKEEVER: Thank you, Mr. Commissioner.

COMMISSIONER LeBLANC: Do you need to take a minute? We can go off the record, if you'd like, Mr. McKeever, and meet with your witness to make sure?

MR. McKEEVER: No, I'm comfortable.

COMMISSIONER LeBLANC: Mr. Neels, are you comfortable with that situation?

THE WITNESS: Yes, I am.

COMMISSIONER LeBLANC: Please go ahead, Mr. McBride.

BY MR. McBRIDE:

Q What I'm asking there is if Professor Bradley has drawn a conclusion, made a statement in this proceeding in which he claims that mean centering was appropriate to calculate the correct results.

A I'm a little confused because the portions of his rebuttal testimony that you've directed my attention to, discuss heteroskedasticity.

Q Right.

A Which I understand to be a different issue than mean centering. And I don't believe that mean centering would be either a cause or a cure for heteroskedasticity.

I believe that mean centering is just, as I said in my rebuttal testimony, a computational convenience.

Q Which should not change the results, isn't that correct?

A It shouldn't change the result.

Q And are you familiar with the work of Professor Greene whose testimony is going to follow you, hopefully later this evening?

A I own his book.

Q Great.

A I have read some of his stuff.

Q You don't happen to have the book there, do you?

A Not on me.

Q Would you be delighted to know that Professor Greene, it seems to me, agrees with you, because at page 229 of his textbook, he says that, referring to normalization, he says, in practice analysts sometimes normalize, quote/unquote, the measured variables by dividing by their respective sample means.

It turns out that the interesting elasticities in this model are unaffected by the normalization.

Does that sound correct to you?

A That's what I would expect, yes.

Q All right. So if Professor Bradley normalized and claimed that it changed his results, his work would not be

consistent with your view of the world and Professor Green's; isn't that correct?

A That is correct.

Q Okay, now, I also would like to know if you would agree me as to the following:

Near the end of your testimony, you talk about -- and I'm now on what I think is my numbered page 21, and it may be your 20. It's right above your Recommendations and Conclusions.

A Yes.

Q You concluded that Mr. Nelson's highly restricted models are far more likely to present a biased picture of the cost structure of the purchased highway transportation than those that have been offered by Dr. Bradley and previously accepted by the Commission.

And then in the recommendations, you talk about Mr. Nelson's ideas which you say are plausible on their face, but would have to be determined, whether they're feasible. Did I accurately characterize that testimony?

A I think, generally, yes.

Q You use the phrase, for the most part, these ideas are plausible on their face, and then a few lines later you say, we cannot conclude that they would prove to be feasible, et cetera, right?

A Yes.

Q Now, you're presenting econometric and statistical testimony. So was Mr. Nelson, so is Dr. Bradley, so is Dr. Bozzo, so is Professor Greene.

But would you agree with me that the testimony of Postal Service witnesses who actually contract for transportation and do it every day would probably be the most reliable testimony about what is feasible in the way of reducing transportation costs, rather than people who are doing this work indirectly by doing statistical and econometric work?

A Well, I think certainly the people who are involved in purchasing transportation and managing transportation for the Postal Service would have a real concrete sense of how things work, and they should have a sense of what the constraints are and what kind of limitations that might put on themselves.

Having said that, I know many organizations will hire consultants from outside to try and get a fresh perspective about what's possible.

And I think that, you know, sometimes an organization can -- may not be aware of different ideas, different approaches, may have sort of -- just be, because of -- unaccustomed to thinking of certain ways -- and I would certainly think that it's possible that someone from outside the Postal Service might have some fresh ideas,

because that is certainly something that happens broadly.

But I would certainly agree that the people in the Postal Service have a very good sense of what would work, given their operations.

Q Were you here on Monday?

A I wasn't.

Q Are you aware that Mr. Young, who is in charge of transportation purchasing for the Postal Service, as I understand it, testified that there were savings that could be achieved in the highway transportation and some other things, and he explained that that would be based, for example, on filling up -- using the backhaul rather than having trailers come back empty, that sort of thing; were you aware of that?

A I wasn't here on Monday, as I said.

Q Okay, so you don't have any basis to disagree with Mr. Young then about what he said he could save; would you?

A No.

MR. McBRIDE: That's all I have for now, Mr. Chairman. Thank you very much.

CHAIRMAN GLEIMAN: The Postal Service has no cross-examination for this witness?

MS. DUCHEK: That is correct.

CHAIRMAN GLEIMAN: This time around?

MS. DUCHEK: That is also correct.

CHAIRMAN GLEIMAN: I don't believe there are any questions from the bench.

Are there follow-up questions to Mr. McBride's cross-examination?

I am getting a little giddy at this point, I think.

MR. McKEEVER: We have no redirect, Mr. Chairman.

CHAIRMAN GLEIMAN: Is there is no redirect, then, Dr. Neels, that completes your appearance at this point in time. We will see you again a bit later, and you are excused for right now.

THE WITNESS: Until next time.

[Witness excused.]

CHAIRMAN GLEIMAN: Hopefully, it won't be too long before we see you again in the stand.

Mr. Alverno, I believe you have got the next witness.

MR. ALVERNO: Thank you, Mr. Chairman. The Postal Service calls Donald O'Hara.

CHAIRMAN GLEIMAN: And here I go again, you know, mixing up my cases. I can't remember whether I have seen Mr. O'Hara this time or whether it was last time. We haven't seen him in this proceeding.

MR. O'HARA: That's correct.

CHAIRMAN GLEIMAN: That is what I thought.

Whereupon,

DONALD J. O'HARA,

a witness, having been called for examination and, having been first duly sworn, was examined and testified as follows:

CHAIRMAN GLEIMAN: I just want you to know that the people who keep track of this stuff can't seem to keep it straight either.

MR. ALVERNO: Thank you, Mr. Chairman.

DIRECT EXAMINATION

BY MR. ALVERNO:

Q Dr. O'Hara, earlier I handed you two copies of a document entitled "Rebuttal Testimony of Donald O'Hara on Behalf of United States Postal Service," and this is marked as USPS-RT-19. Have you had a chance to examine those documents?

A I have.

Q And was this testimony prepared by you or under your direction?

A Yes, it was.

Q And do you have any changes or corrections to make?

A None.

Q And if you were to testify orally today, would your testimony be the same?

A It would.

MR. ALVERNO: Mr. Presiding Officer, I ask that the rebuttal testimony of Donald O'Hara on behalf of United States Postal Service, which is marked as USPS-RT-19, be received as evidence and transcribed at this time.

CHAIRMAN GLEIMAN: Is there objection?

[No response.]

CHAIRMAN GLEIMAN: Hearing none, if you would please provide two copies of the testimony to the court reporter, I will direct that the material be received into evidence and transcribed into the record.

[Rebuttal Testimony of Donald J. O'Hara, USPS-RT-19, was received into evidence and transcribed into the record.]

USPS-RT-19

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

POSTAL RATE AND FEE CHANGES, 2000	Docket No. R2000-1

REBUTTAL TESTIMONY
OF
DONALD J. O'HARA
ON BEHALF OF
UNITED STATES POSTAL SERVICE

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REBUTTAL TESTIMONY OF DONALD J. O'HARA
AUTOBIOGRAPHICAL SKETCH

My name is Donald J. O'Hara. Since 1997, I have served as the Manager, Classification and Product Design in the Marketing organization. Earlier this year, I began serving as the Acting Manager of Pricing. In this capacity, I have direct responsibility for supervising the work of the economists and pricing specialists in Pricing, as well as the work of consultants retained by the Postal Service on pricing, classification, and costing matters.

I have been employed by the Postal Service since 1981. For most of this period, I was a Principal Economist in the Planning Department, where I produced information and analyses used in the strategic planning process. During this time, I also played a major role in the development and implementation of the Postal Service's Total Factor Productivity (TFP) measurement system. In the 1992 reorganization, I moved to the reclassification project. I have made three previous appearances in proceedings before the Postal Rate Commission. In Classification Reform I (Docket No. MC95-1), I provided testimony on rates and classifications for First-Class Mail. In Classification Reform II (Docket No. MC96-2), I provided testimony on rates and classifications for Nonprofit Periodicals. In Docket No. R97-1, I provided testimony on the rate levels proposed by the Postal Service.

I received a Ph.D. in Economics from the University of California at Los Angeles in 1971, and from 1970 until 1980 I taught at the University of Rochester, first as an Assistant Professor of Economics (through 1976), and then as an Associate Professor. In 1980-81, I served on the staff of the President's Commission for a National Agenda for the Eighties.

I. PURPOSE AND SCOPE OF TESTIMONY

My testimony addresses two subject matter areas. The first part addresses the concerns raised by Newspaper Association of America (NAA) witness Tye and Association of Alternate Postal Systems (AAPS) witness White concerning the competitive effect of the Postal Service's proposals for the Enhanced Carrier Route (ECR) subclass. I begin with a brief summary of the concerns raised by witnesses Tye and White. I then explain how the Postal Service's proposed rates for ECR facilitate competition in advertising markets. I also address concerns that the proposal unfairly targets heavy weight ECR matter, and I refute the intervenors' claims of economic harm.

The second part of my testimony discusses how single-piece First-Class mailers benefit from an averaged first-ounce rate and why the Office of Consumer Advocate's (OCA's) courtesy envelope mail (CEM) proposal should be rejected.

II. THE ECR SUBCLASS PROPOSAL IS MANIFESTLY REASONABLE.

A. Summary Of NAA and AAPS Positions

Both NAA and AAPS contend that the Postal Service's proposal for the ECR subclass cost coverage and the proposal to reduce the pound rate for the ECR subclass are motivated in large part by a desire to divert business from newspapers and alternative delivery carriers. They have represented that if the pound rate is reduced as proposed by the Postal Service, their organizations' members will suffer economic harm due to diversion of advertising from alternative media, such as newspapers, to Standard Mail ECR. For instance, NAA witness Tye claims that the proposed ECR cost coverage and the proposal to reduce the ECR subclass pound rate element "would have the

effect of diverting volume from private enterprise competitors of ECR mail." Tr. 30/14742. Witness Tye confesses, however, that he did not review the rates charged by newspapers for inserts, and he offers no other quantitative data to support his conclusion that volumes will shift as his testimony portends. Tr. 30/14781, 14831, 14895, 14906. Witness Tye nonetheless suggests that the Postal Service's ECR rate proposal is motivated by a "stealth objective of diverting mail from private enterprise competitors." Tr. 30/14740. He submits that the proposal is "part of an ongoing effort to divert ECR mail from private enterprise competitors" and should therefore be rejected by the Commission. Tr. 30/14693. In support of this claim, witness Tye cites to Table 12 in witness Tolley's testimony, which contains figures relating changes in ECR volume for the period 1994-99 to a number of variables, including increases in prices for newspaper advertising. Tr. 30/14821. Witness Tye proposes that the Commission recommend a cost coverage for ECR that is no lower than the cost coverage recommended in Docket No. R97-1 in relative or absolute terms, and that the Commission propose common rate increases for piece-rated and pound-rated nondiscounted ECR, thereby resulting in an increase in the pound rate element for pound-rated ECR pieces. Tr. 30/14743-44.

AAPS witness White similarly suggests that the ECR proposal is motivated by an effort "to create diversion from alternate media." Tr. 22/9948. In support of this proposition, witness White cites the Postal Service's ongoing commissioning of SAI research, as well as its stated intent with regard to the proposed ECR pound rate reductions in Docket Nos. MC95-1 and R97-1, as proof of the Postal Service's intent in

this docket. Tr. 22/9954-56. Witness White claims that the Postal Service's request for a lower pound rate will result in a market that is "less competitive." Tr. 22/10006. He foresees a "severe impact" on the private delivery of heavier pieces, because "[l]owering the pound rate would further damage alternate delivery's ability to compete . . ." Tr. 22/9961, 22/9940. Witness White proposes that the Commission not recommend any reduction in the ECR pound rate element. Tr. 22/9962.

B. The Postal Service's Proposal Promotes Competition in Advertising Markets.

NAA and AAPS witnesses criticize the Postal Service's direct case, claiming that the Postal Service did not consider the effect on competitors under section 3622(b)(4) of the ratemaking criteria. Tr. 22/9941, 30/14695. Yet the allegations that NAA and AAPS raise fail to address the *effect on competition*, which I understand to be the judicially interpreted concern embraced by the section 3622(b)(4) reference to competitors. Indeed, the NAA and AAPS testimony is diametrically opposed to the proliferation of choice--and ultimately of competition--in advertising markets. In effect, NAA and AAPS would have the Commission maintain ECR rates for heavier weight pieces at levels far in excess of the relationship suggested by their costs. This necessarily implies that a more affordable alternative, in the form of a more attractive rate for heavier weight ECR mail, would be denied to mailers for the sake of the protectionist self-interest of NAA's and AAPS's members, thereby restricting choice and *reducing* competition. Simply put, the 3622(b)(4) requirement that the Commission consider the effect on competition weighs in favor of the Postal Service's proposal, for it will enable competition to flourish in the market for high circulation advertising, to the benefit of advertisers.

It should be noted, moreover, that the ECR proposal's positive effects on competition extend beyond the market for distribution of advertising. As a subclass composed primarily of advertising messages, ECR provides advertisers, particularly those serving consumer markets, with an affordable option for the geographically targeted or widespread distribution of high circulation advertising for products and services. This information, in turn, increases recipients' awareness of advertisers' products and services, and enables consumers to make better and more informed choices about consumption. As consumers become more sophisticated in their knowledge of product markets, competition is enhanced, and consumers benefit, such as, for example, through introduction to innovations; improvements in availability, access and quality; and lower prices. Thus, to artificially inflate the proposed ECR rates, as NAA and AAPS urge, would effectively reduce the level of consumer information for products and services.

C. The ECR Proposal Is Motivated By Costs, Not Diversion.

NAA and AAPS allude to "stealth objectives" of diverting volumes from their members (Tr. 30/14740) and to an "anticompetitive bent" on the part of postal management (Tr. 22/9936). In support of these claims, both NAA and AAPS suggest that Postal Service's ECR proposal evinces postal management's intent to deliberately divert advertising pieces from their members to ECR mail. We understand the concern regarding the impact that a large institution such as the Postal Service can have in the marketplace. In particular, we are mindful of the effect of our pricing proposals on the level of competition in the marketplace. In recognition of section 3622(b)(4), we do not

price with the specific intent to drive competitors from the field. At the same time, however, we must be mindful of the needs of the marketplace and be careful that the understandable desire to protect alternatives, both large and small, is not pursued at the expense of consumers and customers in the marketplace.

I submit that claims of anticompetitive intent and conduct in this context are unfounded and distort the real motive and effects of the Postal Service's proposals. The ECR proposal sets prices well above costs. While it is true that the proposal would reduce the pound rate element for heavier weight ECR pieces, and thereby result in a rate reduction for ECR pound-rated pieces in excess of from anywhere between 4 and 6 ounces (depending upon presort tier and dropship profile) (Tr. 10/3911-12), there is absolutely no evidence that the prices of these pieces would not more than adequately cover their costs. Indeed, figures from witness Daniel's testimony show that the additional cost due to additional weight in ECR does not increase as rapidly as the pound rate (Tr. 10/3986-87), and that the pound rate exceeds the costs of heavier weight pieces by a comfortable margin. The implicit cost coverages for pound-rated ECR mail that witness Moeller supplies in his direct testimony (USPS-T-35 at 21) are essentially equal to the corresponding implicit cost coverages for piece-rated ECR mail, and thereby dispel allegations that the proposed pound rate element would be unfair. Thus, there is nothing to suggest that the proposal would result in anticompetitive predatory pricing.

Claims of "stealth objectives" are also dispelled by an overall examination of postal rates used by the newspaper industry for their advertising products. It is

remarkable that the ECR proposal, particularly the cost coverage, has received such heavy scrutiny from NAA. NAA's members not only offer alternatives for preprint advertising through private distribution, but they also are substantial users of the ECR High-Density category for their "total market coverage" (TMC) advertising products. Under the Postal Service's proposal, mail in this category will, on average, receive *a rate decrease*. In fact, of the seven categories for which volumes are forecasted, the High-Density nonletter category is the *only* one for which this is the case.[1] Surely, this does not evince evidence of intent to unfairly target competitors for heavy saturation mail; to the contrary, it shows that the Postal Service's proposal is not an attempt to favor any particular industry over another.

In addition, the recent trend of significant growth in the High-Density category provides further evidence of an absence of unfair competition on the part of the Postal Service. From 1998 to 1999, High-Density nonletters grew 6.6 percent, even while Saturation nonletter volume declined 2.4 percent. The figures for pound-rated pieces are even more revealing: High-Density grew 17.9 percent, while Saturation declined 11.1 percent.[2] These data suggest that saturation mailers are finding less costly alternatives for their heavier pieces. This is troubling—not because it shows a decline in Postal Service volume—but because it shows a decline in volume in a category that is unquestionably over-priced relative to costs.

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[1] See response to NAA/USPS-T35-43 at Tr. 10/3904-05. The only rate category to see a volume increase in the after-rates scenario is High-Density nonletters, which implies a rate reduction, on average, for that category.

Proof that the Postal Service's ECR proposal is driven by costs and not by "stealth objectives" can be found in the Postal Service's proposals in this docket regarding matter that may be carried by alternative delivery carriers. AAPS witness White testifies that his organization's members are engaged in delivery of both TMC and saturation shopping guides, community and telephone directories, and merchandise samples. Tr. 22/9942. Although witness White devotes much attention to the USPS proposed ECR pound rate that would apply to mail competing with TMC and saturation shopping guides, he is strangely silent about the Postal Service's proposals for rates for mail matter whose contents include directories and product samples. In fact, the Postal Service is proposing substantial rate increases for such mail. Telephone and community directories weighing more than one pound travel at Bound Printed Matter rates. For a 1.5 pound carrier route presorted BPM piece the current "Local" rate is 50.5 cents. The lowest proposed rate for such a piece is the DDU rate of 58.1 cents, an increase of 15 percent. Similarly, the typical merchandise sample weighing less than 3.3 ounces and subject to the residual shape surcharge currently pays 21.4 cents at the ECR Saturation DDU rate. The corresponding proposed rate is 27 cents, an increase of 26 percent. Both of these increases are driven by cost considerations, just as the proposed decrease in the ECR pound rate is motivated by cost considerations. Taken together, these rate proposals clearly demonstrate that there has been no effort to target the alternative delivery industry in the development of the Postal Service's

[2] USPS Billing Determinant data. USPS-LR-I-125 and USPS-LR-I-259 at Schedule G-3, page 2.

proposals in this case. Rather, as explained by witness Moeller, the cost data serve as the underlying motive behind the ECR pound rate change. USPS-T-35 at 19-23; Tr. 10/3879-80.

D. There Is No Evidence Of Economic Harm.

Although NAA and AAPS witnesses allege that the proposal will divert their members' volumes to ECR mail and hurt their businesses, these claims are wholly unsupported. It is quite telling that neither witness Tye nor witness White provided industry-wide surveys of the prices of alternative media. Indeed, witness Tye did not even bother to ask for price information of newspaper advertising (Tr. 30/14781) or compare absolute levels of prices between ECR and newspapers (Tr. 30/14895). Nevertheless, he conceded that such information "would certainly [have been] an additional piece of data" that, if available, he would have "certainly" looked at. Tr. 30/14905-06. This unexplained and glaring omission seriously undermines the credibility of their conclusions. By contrast, the price data for alternative media in this docket, including the *Miami Herald* 2000 rate card supplied by Alliance of Independent Store Owners and Operators witness Baro (Tr. 30/14412-14; AISOP LR-1), as well as the price schedule provided by AAPS witness White for his company's alternative delivery products (Tr. 22/9981-82), indicate that the published prices of alternative media are generally below the Postal Service's proposed prices, and this does not even consider the negotiated discounts that they may offer to their customers. Thus, if anything, NAA's and AAPS's failure to back their claims undercuts their allegations of diversion, for there is absolutely no showing that the industry's prices are anywhere

near or above those of the Postal Service's proposed rates. Indeed, the recent information identified by NAA in a supplemental interrogatory response demonstrates that newspaper insert volumes have experienced healthy growth patterns,[3] notwithstanding witness Tye's finding (Tr. 30/14740) that the inflation-adjusted ECR pound rate has declined over time. Furthermore, the fact that the NAA is touting recent gains in newspaper advertising expenditures in the first quarter of 2000, on the order of 5.7 percent over the same period last year,[4] suggests that the newspaper industry is hardly suffering negative consequences from what witness Tye characterizes as a "pronounced" inflation-adjusted decline in the ECR pound rate. Tr. 30/14737-40. Since the newspaper industry has so well weathered the decline in the real pound rate (Tr. 30/14737), fears that the reduction in the pound rate will result in large-scale diversion are grossly exaggerated.

AAPS and NAA also fail to consider that the advertising market need not be perceived as a zero-sum game, where every gain in ECR volume comes at the expense of another carrier. It is important to consider that ECR customers do not necessarily perceive ECR and newspapers or alternative delivery as direct substitutes, since they offer different features and are connected with different forms of valuable content. Also, ECR includes advertising that is directed to as few as ten recipients per carrier route.

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[3] NAA Supplemental Institutional Response to Interrogatory of Val-Pak Direct Marketing Systems, Inc. et al (VP-CW/NAA-T1-11(c)) filed August 4, 2000. See also Exhibit USPS-RT19A.

[4] NAA News Release, *Ad Spending In Newspapers Up 5.7 Percent In 1st Quarter 2000,* available at < http://www.naa.org/about/news/article.cfm?Art_ID=274 > See Exhibits USPS-RT19B and USPS-RT19C.

To the extent the lower pound rate were to generate more volume in this basic tier, it is difficult to imagine that this would come at the expense of the alternate delivery or newspapers, who do not offer such selective distribution. Thus, a decline in the pound rate should not necessarily lead ECR volumes to swell at the expense of other distributors of advertising media.

II. THE OCA'S RECYCLED CEM PROPOSAL SHOULD BE REJECTED.

A. Single-Piece Mailers Already Benefit from Automation.

In Docket No. R2000-1, several parties have introduced discount proposals for single-piece First-Class Mail, including OCA's CEM proposal. One reason provided for these discount proposals is that they would allow the general public to share more directly in the benefits of automation.

It is important to recognize that the general mailing public already benefits from a single-piece rate that is lower than it would have been absent automation. The letter automation projects that have been implemented in the field over the last decade or so have had a direct impact on the rates paid by residential and small business mailers. In Docket No. R97-1, the Postal Service proposed and the Commission recommended an increase of only one cent in the stamp price, which was the smallest proposed increase since postal reorganization. In the current docket, the Postal Service is again proposing an increase of only one cent, or 3.0 percent in the basic rate. These modest increases are well below the overall inflation rate in the consumer price index, and well below the systemwide average increase proposed in this case.

B. Single-Piece Mailers Benefit from an Averaged First-Ounce Rate.

In Classification Reform, Docket No. MC95-1, the Commission stated that "[a]veraging is an integral part of postal ratemaking. It is neither possible nor wise to try to establish separate rates for every piece of mail." PRC Op. MC95-1 ¶ 3063. The Commission also opined that::

> Literally billions of pieces pay the current single piece First-Class rate of 32 cents. There are a myriad of reasons why the pieces of mail within that single cell have varying costs. For example, they are sent different distances; they are sent in different parts of the country; they are to be delivered to rural or urban areas; they are addressed in different ways; the paper used is different; the mailpiece is shaped differently; the list goes on and on. It is accepted that for practical reasons, however, there is a single rate applicable to most First-Class pieces weighing one ounce or less.

Id. at ¶ 3064.

Thus, the Commission has recognized the wisdom and practicality of an averaged single-piece rate. The typical household mails some lower-cost courtesy reply mail and some higher-cost handwritten mail and pays an average rate for all of it – a simple and convenient system. An averaged rate has been relied upon by the general public for decades and is already accommodated by current postal processing methods and equipment.

Any proposal to replace the existing averaged structure needs to be evaluated thoroughly. As a result of Docket No. R97-1, the Postal Service did implement the first deaveraged single-piece rate–the rate for Qualified Business Reply Mail (QBRM). Deaveraging was workable with QBRM due to its specific characteristics. QBRM meets mail preparation standards that ensure its automatability, avoids any revenue assurance issues since it is processed through postage due units with a relatively limited number

of licensed users, and does not have any of the problems created by administering two differently denominated basic First-Class Mail stamps.

C. There is No Evidence to Support as Radical a Change as CEM.

In this proceeding, the OCA has again proposed a discount for CEM. In his rebuttal testimony in this docket, witness Miller discusses the serious administrative, operational, and revenue concerns associated with the CEM proposal. For these reasons, the OCA's CEM proposal is not desirable from the point of view of the Postal Service. Perhaps more importantly, even if all of these problems could be overcome, there is still no evidence of the desirability of a CEM classification from the point of view of users of First-Class Mail.

The OCA has no evidence that the public would prefer a "two-stamp" CEM postal system over the present "one-stamp" system. Interrogatory USPS/OCA-T7-3(a) asked the following:

> Please identify all market research or surveys performed by or for the OCA which seeks to ascertain or otherwise indicates whether the general public prefers one basic First-Class Mail first-ounce stamp or two differently denominated basic First-Class Mail first-ounce stamps?

Tr. 23/10770. In response to USPS/OCA-T7-3(a), witness Willette replied in part:

> The OCA has conducted no research of the type you describe except to speak informally to members of the public concerning CEM when the opportunity arises.

Id. Informal discussions with members of the public from time to time can be interesting. However, the American public, the intended beneficiary of the OCA's CEM proposal, has never shown in any formal, meaningful way that it wants CEM–indeed, it

has never been asked about CEM in any formal, meaningful way by the proposal's proponents.

When asked in interrogatory USPS/OCA-T7-21(e) about whether the OCA had considered conducting any market research in conjunction with its Docket No. R2000-1 CEM proposal, witness Gerarden responded:

> Yes. ... The OCA explored informally the parameters, including cost, of performing market research that could be expected to produce statistically valid results, as well as OMB restrictions on data collection governing the Commission. Given the modest budget on which the Commission operates, including the very modest budget for the Office of the Consumer Advocate, and given the need to commit available funds to other aspects of the rate case, it was not feasible to conduct market research on CEM.

Tr. 29/13607. While the Postal Service is always sensitive to budgeting realities and is aware of the need to prioritize in any rate case, such considerations need to be weighed against the significant impact that CEM would have on the mailing public and the Postal Service. A proposal as significant as CEM[5] cannot be made in a vacuum, apart from the preferences of the very public the proposal is supposed to benefit. There is nothing in the present record indicating the public is in favor of this CEM proposal. If the desire to benefit the public is so strong that it overshadows the Postal Service's administrative and other concerns regarding this proposal, one might expect to see some evidence that this proposal is overwhelmingly embraced by the public. The OCA has provided no such evidence.

[5] Witness Willette estimates that the lost revenue due to CEM could reach $300 million annually. Tr.23/10742.

The Commission is reminded that, when faced with the OCA's CEM proposal in Docket No. R97-1, the Postal Service sponsored market research by witness Ellard of Opinion Research Corporation. Docket No. R97-1, Tr. 35/19508 *et. seq.* This research concluded that the public does not find the two-stamp system attractive. The research showed that 60 percent of the surveyed households preferred a one-stamp system. The remaining 40 percent of the respondents were then asked which system they preferred if their rate for regular First-Class letters could rise. Many respondents changed their opinion, indicating that, in this instance, they would prefer a one-stamp system. The cumulative results from these two questions showed that 86 percent of the respondents preferred a one-stamp system, given a possible "push-up" on the regular stamp price. Docket No. R97-1, Tr. 35/19077.

When asked about the Docket No. R97-1 research in the current proceeding, witness Willette confirmed that the Postal Service may have had a valid point when it used witness Ellard's market research in asserting that consumers do not want two stamps. Tr. 23/10782.

During cross-examination in the current proceeding, witness Willette testified:

> I think that the real point that we're missing by talking about what the rate structure of First Class might or might not look like if we had CEM, is that it's a choice for consumers. And CEM is being proposed as a choice for consumers. If CEM is never offered, then the issue is never going to arise.... And we certainly don't know what kind of use there would be of it. Without it in place, it's not possible for anyone to use it.

Tr. 23/10793 . Witness Willette is apparently concerned about the consumer's choice of whether to use a CEM stamp, assuming the stamp exists. However, the OCA ignores

the threshold consideration of whether the public desires a two-stamp system in the first place.

Exhibit USPS-RT19A

Newspaper Preprint Insert Volume Estimates

U.S. Daily Newspapers

Projected Circulation (In Billions)

Year		Full Run	% change	Partial Run	% change	Total	% change
1997	**Total Retail**	**38.966**	**2.53**	**43.035**	**4.57**	**82.000**	**3.59**
	Sunday	22.313	6.07	25.537	6.60	47.851	6.35
	Daily	16.653	-1.86	17.497	1.74	34.150	-0.04
	Total National	**6.597**	**-3.07**	**1.399**	**0.32**	**7.996**	**-2.50**
	Sunday	5.798	-4.00	0.695	-5.35	6.493	-4.15
	Daily	0.799	-4.27	0.704	6.63	1.503	5.36
	Grand Total	**45.563**	**1.68**	**44.433**	**4.43**	**89.996**	**3.02**
	Sunday	28.111	3.82	26.232	6.24	54.344	4.98
	Daily	17.452	-1.59	18.201	1.93	35.653	0.17
1996	**Total Retail**	**38.004**	**-0.92**	**41.154**	**4.37**	**79.158**	**1.76**
	Sunday	21.036	-0.74	23.957	3.47	44.993	1.46
	Daily	16.968	-1.15	17.197	5.66	34.165	2.16
	Total National	**6.806**	**-8.23**	**1.394**	**21.87**	**8.200**	**-4.21**
	Sunday	6.040	-9.37	0.734	20.83	6.774	-6.85
	Daily	0.766	1.82	0.660	23.04	1.426	10.65
	Grand Total	**44.810**	**-2.11**	**42.548**	**4.87**	**87.358**	**1.17**
	Sunday	27.076	-2.80	24.691	3.91	51.767	0.29
	Daily	17.734	-1.03	17.857	6.21	35.591	2.48
1995	**Total Retail**	**38.358**	**0.37**	**39.429**	**6.44**	**77.788**	**3.36**
	Sunday	21.193	1.20	23.153	5.94	44.346	3.62
	Daily	17.165	-0.63	16.276	7.17	33.441	3.02
	Total National	**7.417**	**-15.13**	**1.144**	**9.91**	**8.561**	**-12.46**
	Sunday	6.664	-16.81	0.608	4.59	7.272	-15.36
	Daily	0.753	3.37	0.536	16.62	1.289	8.50
	Grand Total	**45.775**	**-2.51**	**40.573**	**6.54**	**86.349**	**1.54**
	Sunday	27.858	-3.78	23.761	5.91	51.619	0.45
	Daily	17.918	-0.47	16.812	7.45	34.73	3.21
1994	**Total Retail**	**38.216**	**-4.46**	**37.042**	**15.45**	**72.258**	**4.40**
	Sunday	20.942	0.63	21.855	16.83	42.797	8.30
	Daily	17.274	-9.98	15.187	13.53	32.461	-0.33
	Total National	**8.739**	**-11.41**	**1.041**	**28.29**	**9.780**	**-8.78**

	Sunday	8.011	-11.86	0.581	42.05	8.592	-9.54
	Daily	0.728	-6.00	0.460	58.34	1.188	11.55
	Grand Total	**46.955**	**-5.84**	**38.083**	**16.16**	**85.038**	**2.89**
	Sunday	28.953	-3.17	22.436	17.37	51.389	4.84
	Daily	18.002	-9.83	15.647	14.48	33.649	0.05
1993	**Total Retail**	**40.002**	**1.05**	**32.084**	**9.76**	**72.086**	**4.75**
	Sunday	20.812	-4.45	18.707	17.14	39.518	4.68
	Daily	19.190	7.79	13.377	0.88	32.568	4.84
	Total National	**9.864**	**9.80**	**0.700**	**2.67**	**10.564**	**9.30**
	Sunday	9.089	10.95	0.409	-8.74	9.498	9.93
	Daily	0.775	-2.10	0.291	24.61	1.065	4.84
	Grand Total	**49.866**	**2.67**	**32.784**	**9.60**	**82.650**	**5.31**
	Sunday	29.901	-0.24	19.116	16.43	49.017	5.66
	Daily	19.965	7.37	13.668	1.29	33.633	4.81
1992	**Total Retail**	**39.585**	**6.88**	**29.231**	**13.16**	**68.815**	**9.46**
	Sunday	21.781	25.96	15.970	26.78	37.751	26.30
	Daily	17.804	-9.83	13.261	0.20	31.064	-5.81
	Total National	**8.984**	**13.29**	**0.682**	**-14.00**	**9.665**	**10.80**
	Sunday	8.192	15.01	0.449	-18.81	8.641	12.57
	Daily	0.791	-1.98	0.233	-2.92	1.025	-2.10
	Grand Total	**48.568**	**8.01**	**29.912**	**12.35**	**78.481**	**9.62**
	Sunday	29.973	22.76	16.418	24.85	46.392	23.50
	Daily	18.595	-9.52	13.494	0.14	32.089	-5.70
1991	**Total Retail**	**37.037**	**10.58**	**25.832**	**14.57**	**62.869**	**12.19**
	Sunday	17.292	-3.54	12.597	8.71	29.889	1.27
	Daily	19.745	26.85	13.235	20.78	32.98	24.34
	Total National	**7.930**	**3.47**	**0.793**	**24.29**	**8.723**	**5.07**
	Sunday	7.123	0.04	0.553	20.48	7.676	1.28
	Daily	0.807	48.35	0.240	34.08	1.047	44.81
	Grand Total	**44.967**	**9.26**	**26.625**	**14.84**	**71.592**	**11.27**
	Sunday	24.415	-2.52	13.150	9.16	37.565	1.27
	Daily	20.552	27.57	13.475	20.99	34.027	24.88
1990	**Total Retail**	**33.493**	**10.40**	**22.546**	**4.13**	**56.039**	**7.79**
	Sunday	17.927	15.50	11.588	-0.57	29.515	8.61
	Daily	15.566	5.05	10.958	9.62	26.524	6.89
	Total National	**7.664**	**15.46**	**0.638**	**-43.54**	**8.302**	**6.87**
	Sunday	7.12	13.63	0.459	-47.06	7.579	6.25
	Daily	0.544	46.24	0.179	-31.94	0.723	13.86
	Grand Total	**41.157**	**11.30**	**23.184**	**1.77**	**64.341**	**7.67**

	Sunday	25.047	14.96	12.047	-3.79	37.094	8.12
	Daily	16.11	6.06	11.137	8.56	27.247	7.07
1989	**Total Retail**	**30.339**	**8.91**	**21.651**	**5.80**	**51.99**	**7.59**
	Sunday	15.521	9.21	11.655	3.90	27.176	6.87
	Daily	14.818	8.59	9.996	8.10	24.814	8.39
	Total National	**6.638**	**-3.95**	**1.13**	**-5.44**	**7.768**	**-4.17**
	Sunday	6.266	-3.98	0.867	-18.44	7.133	-6.01
	Daily	0.372	-3.38	0.263	99.24	0.635	22.82
	Grand Total	**36.977**	**6.35**	**22.781**	**5.18**	**59.758**	**5.90**
	Sunday	21.787	5.06	12.522	1.97	34.309	3.91
	Daily	15.19	8.26	10.259	9.38	25.449	8.71
1988	**Total Retail**	**27.858**	**-3.29**	**20.464**	**31.20**	**48.322**	**1.94**
	Sunday	14.212	5.93	11.217	15.44	25.429	9.92
	Daily	13.646	-11.32	9.247	4.12	22.893	-5.67
	Total National	**6.911**	**10.12**	**1.195**	**8.74**	**8.106**	**9.91**
	Sunday	6.526	10.65	1.063	10.16	7.589	10.58
	Daily	0.385	1.85	0.132	-1.49	0.517	0.98
	Grand Total	**34.769**	**-0.89**	**21.659**	**9.96**	**56.428**	**3.01**
	Sunday	20.738	7.37	12.280	14.96	33.018	10.07
	Daily	14.031	-11.00	9.379	4.04	23.410	-5.53
1987	**Total Retail**	**28.805**	**7.33**	**15.598**	**-5.08**	**47.403**	**9.55**
	Sunday	13.417	0.19	9.717	5.22	23.134	2.24
	Daily	15.388	14.45	8.881	23.38	24.269	17.57
	Total National	**6.276**	**-9.89**	**1.099**	**-13.33**	**7.375**	**-10.42**
	Sunday	5.898	-6.87	0.965	-7.48	6.863	-6.95
	Daily	0.378	-40.19	0.134	-40.44	0.512	-40.26
	Grand Total	**35.081**	**3.78**	**19.697**	**11.28**	**54.778**	**6.36**
	Sunday	19.315	-2.08	10.682	3.93	29.997	-0.02
	Daily	15.766	12.00	9.015	21.45	24.781	15.26
1986	**Total Retail**	**26.837**	**7.13**	**16.433**	**20.75**	**43.27**	**11.92**
	Sunday	13.392	9.46	9.235	27.06	22.627	16.02
	Daily	13.445	4.90	7.198	13.52	20.643	7.75
	Total National	**6.965**	**23.01**	**1.268**	**20.08**	**8.233**	**22.55**
	Sunday	6.333	42.54	1.043	31.53	7.376	40.87
	Daily	0.632	-48.15	0.225	-14.45	0.857	-42.17
	Grand Total	**33.802**	**10.05**	**17.701**	**20.70**	**51.503**	**13.50**
	Sunday	19.725	18.27	10.278	27.50	30.003	21.28
	Daily	14.077	0.29	7.423	12.40	21.500	4.17

1985	**Total Retail**	**25.052**	**8.46**	**13.609**	**13.02**	**38.661**	**10.02**
	Sunday	12.235	5.17	7.268	1.54	19.503	3.78
	Daily	12.817	11.80	6.341	29.86	19.158	17.20
	Total National	**5.662**	**7.07**	**1.056**	**26.01**	**6.718**	**9.66**
	Sunday	4.443	2.97	0.793	14.27	5.236	4.53
	Daily	1.219	25.28	0.263	82.64	1.482	32.68
	Grand Total	**30.714**	**8.20**	**14.665**	**13.87**	**45.379**	**9.97**
	Sunday	16.678	4.57	8.061	2.66	24.739	3.94
	Daily	14.036	12.86	6.604	31.37	20.64	18.19
1984	**Total Retail**	**23.098**	**10.15**	**12.041**	**27.86**	**35.139**	**15.64**
	Sunday	11.634	15.21	7.158	45.22	18.792	25.05
	Daily	11.464	5.45	4.883	8.80	16.347	6.43
	Total National	**5.288**	**18.06**	**0.838**	**-2.67**	**6.126**	**14.72**
	Sunday	4.315	22.69	0.694	-4.01	5.009	18.14
	Daily	0.973	1.14	0.144	4.35	1.117	1.55
	Grand Total	**28.386**	**11.54**	**12.879**	**25.31**	**41.265**	**15.50**
	Sunday	15.949	17.14	7.852	38.92	23.801	23.53
	Daily	12.437	5.10	5.027	8.67	17.464	6.10
1983	**Total Retail**	**20.970**	**18.45**	**9.417**	**20.89**	**30.387**	**19.20**
	Sunday	10.098	25.38	4.929	37.41	15.027	29.09
	Daily	10.872	12.67	4.488	6.78	15.360	10.89
	Total National	**4.479**	**19.70**	**0.861**	**17.78**	**5.340**	**19.38**
	Sunday	3.517	1.91	0.723	6.79	4.240	2.71
	Daily	0.962	230.58	0.138	155.56	1.100	218.84
	Grand Total	**25.449**	**18.67**	**10.278**	**20.62**	**35.727**	**19.23**
	Sunday	13.615	18.34	5.652	32.55	19.267	22.18
	Daily	11.834	19.05	4.626	8.67	16.460	15.94
1982	**Total Retail**	**17.703**	n/a	**7.790**	n/a	**25.493**	n/a
	Sunday	8.054	n/a	3.587	n/a	11.641	n/a
	Daily	9.649	n/a	4.203	n/a	13.852	n/a
	Total National	**3.742**	n/a	**0.731**	n/a	**4.473**	n/a
	Sunday	3.451	n/a	0.677	n/a	4.128	n/a
	Daily	0.291	n/a	0.054	n/a	0.345	n/a
	Grand Total	**21.445**	n/a	**8.521**	n/a	**29.966**	n/a
	Sunday	11.505	n/a	4.264	n/a	15.769	n/a
	Daily	9.94	n/a	4.257	n/a	14.197	n/a

Last Update: 11/99

Questions or comments, e-mail: robem@naa.org

Exhibit USPS-RT19B

QUARTERLY NEWSPAPER ADVERTISING EXPENDITURES

		NATIONAL		RETAIL		CLASSIFIED		TOTAL	
Year	Quarter	$Millions	%Change	$Millions	% Change	$Millions	%Change	$Millions	%Change
2000	1(p)	1,821.930	18.7%	4,609.030	0.5%	4,386.880	6.7%	10,817.840	5.7%
	2(p)								
	3(p)								
	4(p)								
1999	1(p)	1,534.90	12.60%	4,586.10	4.50%	4,111.40	3.80%	10,232.40	5.40%
	2(p)	1,792.35	17.40%	5,106.80	1.80%	4,443.97	4.00%	11,343.11	4.90%
	3(p)	1,624.39	17.90%	5,096.89	2.90%	4,600.11	4.50%	11,321.38	5.50%
	4(p)	1,780.40	22.50%	6,117.10	2.42%	5,494.04	4.95%	13,391.54	5.77%
1998	1	1,363.00	10.20%	4,389.90	4.90%	3,959.80	10.60%	9,712.63	7.90%
	2	1,526.70	8.70%	5,016.50	6.80%	4,275.10	7.00%	10,818.33	7.10%
	3	1,378.00	6.20%	4,951.80	5.60%	4,403.70	5.60%	10,733.54	5.70%
	4	1,453.40	5.70%	5,972.70	5.30%	5,234.90	4.10%	12,660.92	4.90%
1997	1	1,236.70	12.20%	4,184.20	5.60%	3,579.60	10.50%	9,000.44	8.40%
	2	1,404.60	13.90%	4,698.30	4.70%	3,995.10	12.50%	10,098.00	9.50%
	3	1,297.30	12.90%	4,689.50	5.90%	4,170.40	10.80%	10,157.31	8.80%
	4	1,375.60	16.20%	5,670.50	3.60%	5,027.70	11.50%	12,073.85	8.20%
1996	1	1,101.80	7.00%	3,960.70	0.40%	3,240.80	10.00%	8,303.27	4.80%
	2	1,232.80	6.50%	4,486.30	0.50%	3,550.10	9.70%	9,269.12	4.70%
	3	1,148.70	18.00%	4,426.60	3.50%	3,763.50	10.70%	9,338.76	7.90%
	4	1,183.40	8.50%	5,470.40	1.10%	4,510.30	8.50%	11,163.98	4.70%
1995	1	1,029.80	2.70%	3,945.00	4.40%	2,946.90	13.10%	7,921.66	7.30%
	2	1,157.20	3.60%	4,464.20	4.10%	3,235.40	8.30%	8,856.79	5.50%
	3	973.5	-0.60%	4,278.90	0.50%	3,400.60	8.70%	8,652.95	3.40%
	4	1,090.60	3.80%	5,411.40	4.60%	4,158.70	11.20%	10,660.72	7.00%
1994	1	1,003.20	8.20%	3,777.60	3.50%	2,604.60	9.10%	7,385.43	6.10%
	2	1,117.10	4.80%	4,287.00	2.20%	2,988.70	12.30%	8,392.76	5.90%
	3	979	10.80%	4,258.90	4.90%	3,129.40	12.30%	8,367.25	8.20%
	4	1,050.20	7.70%	5,172.20	4.40%	3,741.40	12.60%	9,963.76	7.70%
1993	1	927.4	-3.10%	3,648.40	6.90%	2,387.20	4.90%	6,963.00	4.80%
	2	1,066.10	0.00%	4,193.80	3.40%	2,661.80	1.50%	7,921.79	2.30%
	3	883.9	-3.30%	4,061.80	5.60%	2,786.00	3.10%	7,731.61	3.60%
	4	975.4	8.90%	4,955.00	4.90%	3,322.30	5.10%	9,252.91	5.30%
1992	1	956.9	0.00%	3,412.40	-2.70%	2,276.00	0.90%	6,645.31	-1.10%

	2	1,066.50	0.10%	4,055.40	0.30%	2,623.30	2.30%	7,745.16	0.90%
	3	914.4	-2.20%	3,848.00	1.70%	2,702.20	0.70%	7,464.63	0.90%
	4	895.9	-7.40%	4,725.60	4.90%	3,162.10	2.60%	8,783.69	2.70%
1991	1	956.5	-0.80%	3,507.60	-8.40%	2,256.50	-9.40%	6,720.58	-7.80%
	2	1,065.10	-1.90%	4,041.80	-7.60%	2,565.30	-9.70%	7,672.28	-7.60%
	3	934.8	-2.50%	3,782.30	-5.80%	2,684.30	-8.90%	7,401.35	-6.60%
	4	967.2	-13.20%	4,507.00	1.70%	3,080.50	-4.50%	8,554.74	-2.50%
1990	1	963.9	7.80%	3,830.20	6.00%	2,491.80	-11.30%	7,285.82	-0.40%
	2	1,085.70	44.70%	4,373.60	-3.20%	2,841.30	-5.40%	8,300.60	0.40%
	3	958.6	27.40%	4,016.80	1.10%	2,947.20	-5.00%	7,922.68	1.20%
	4	1,114.00	32.40%	4,431.30	-7.30%	3,226.10	-3.10%	8,771.44	-2.00%
1989	1	894.5	1.70%	3,611.70	4.80%	2,810.40	2.40%	7,316.55	3.50%
	2	750.1	-18.40%	4,518.30	16.00%	3,002.50	-5.20%	8,270.93	3.60%
	3	752.6	-13.20%	3,972.20	6.20%	3,103.90	3.00%	7,828.74	2.70%
	4	841.3	-8.50%	4,782.50	0.50%	3,328.10	17.00%	8,951.86	5.10%
1988	1	879.2	6.00%	3,447.60	4.60%	2,745.30	10.80%	7,072.03	7.10%
	2	919.5	-2.20%	3,894.90	1.70%	3,168.00	12.80%	7,982.28	5.30%
	3	867.3	4.50%	3,741.20	4.40%	3,014.40	6.80%	7,622.92	5.30%
	4	919.8	2.90%	4,756.40	5.20%	2,843.60	10.20%	8,519.85	6.60%
1987	1	829.7	2.20%	3,294.80	8.30%	2,478.80	16.40%	6,603.32	10.40%
	2	940.2	3.70%	3,828.50	7.90%	2,809.40	15.60%	7,578.07	10.00%
	3	830.3	8.50%	3,583.70	6.90%	2,823.00	15.10%	7,236.99	10.20%
	4	893.9	0.30%	4,519.60	3.40%	2,579.90	12.60%	7,993.35	5.90%
1986	1	812	1.30%	3,041.30	1.40%	2,129.80	8.10%	5,983.04	3.70%
	2	907	6.80%	3,548.30	7.80%	2,430.30	10.10%	6,886.07	8.50%
	3	765.3	0.60%	3,352.60	7.00%	2,451.80	11.90%	6,569.71	7.90%
	4	891.3	-5.20%	4,369.10	8.70%	2,290.20	14.10%	7,550.53	8.40%
1985	1	801.3	12.80%	2,998.70	7.90%	1,969.50	14.20%	5,769.47	10.70%
	2	849.5	7.20%	3,290.60	1.00%	2,207.00	9.80%	6,347.05	4.70%
	3	760.9	8.80%	3,134.50	4.30%	2,191.60	7.30%	6,086.94	5.90%
	4	940.3	6.90%	4,019.10	7.40%	2,007.60	6.80%	6,966.96	7.20%
1984	1	710.1	7.90%	2,779.40	10.40%	1,724.20	32.10%	5,213.68	16.40%
	2	792.3	11.70%	3,258.80	11.80%	2,009.60	31.70%	6,060.62	17.70%
	3	699.6	15.60%	3,004.80	7.70%	2,043.30	28.20%	5,747.78	15.20%
	4	879.4	-15.60%	3,741.00	3.40%	1,880.10	18.90%	6,500.56	9.00%
1983	1	657.9	6.50%	2,516.50	13.00%	1,305.70	16.70%	4,480.16	13.00%
	2	709.6	6.00%	2,915.40	10.90%	1,525.80	24.30%	5,150.69	13.80%
	3	605.4	21.70%	2,789.90	15.10%	1,593.60	26.40%	4,988.91	19.30%
	4	760.9	14.00%	3,619.50	16.30%	1,581.30	27.10%	5,961.77	18.70%

1982	1	617.8	13.20%	2,226.30	11.40%	1,119.20	2.80%	3,963.19	9.10%
	2	669.5	9.30%	2,628.60	6.50%	1,227.90	2.90%	4,525.97	5.90%
	3	497.4	-2.40%	2,423.90	4.50%	1,261.10	3.90%	4,182.44	3.40%
	4	667.2	13.00%	3,110.90	7.30%	1,243.80	14.40%	5,021.92	9.70%
1981	1	545.7	21.30%	1,998.40	11.30%	1,089.10	9.10%	3,633.12	12.00%
	2	612.4	20.50%	2,467.40	20.20%	1,193.40	14.80%	4,273.14	18.70%
	3	509.8	12.30%	2,320.60	11.40%	1,213.80	10.50%	4,044.21	11.20%
	4	590.5	7.10%	2,899.60	8.40%	1,087.20	0.20%	4,577.29	6.20%
1980	1	449.8	17.00%	1,796.20	14.20%	998.6	9.60%	3,244.66	13.10%
	2	508.1	13.00%	2,053.30	8.50%	1,039.80	-4.00%	3,601.27	5.10%
	3	453.8	6.30%	2,083.30	6.60%	1,098.90	-4.90%	3,636.04	2.80%
	4	551.2	8.30%	2,676.10	10.30%	1,084.70	-1.20%	4,312.03	6.90%
1979	1	384.3	13.20%	1,572.30	9.40%	911	21.90%	2,867.71	13.60%
	2	449.8	9.90%	1,893.00	8.90%	1,083.60	13.90%	3,426.31	10.60%
	3	426.8	25.10%	1,954.30	16.40%	1,155.60	18.50%	3,536.67	18.10%
	4	509.1	12.90%	2,425.40	11.80%	1,097.80	12.60%	4,032.30	12.10%
1978	1	339.4	7.80%	1,437.00	13.50%	747.6	19.50%	2,524.05	14.40%
	2	409.3	8.50%	1,737.60	15.10%	951.4	21.20%	3,098.31	16.00%
	3	341.3	-1.60%	1,678.40	11.70%	975	20.00%	2,994.69	12.50%
	4	451	4.20%	2,169.90	10.60%	975	19.60%	3,595.94	12.00%
1977	1	314.8	8.70%	1,266.60	8.20%	625.7	13.00%	2,207.16	9.60%
	2	377.2	11.20%	1,509.70	9.20%	784.8	16.70%	2,671.60	11.60%
	3	347	11.60%	1,502.30	9.80%	812.6	17.50%	2,661.87	12.30%
	4	433	7.60%	1,962.50	12.30%	814.9	18.00%	3,210.38	13.00%
1976	1	289.6	17.40%	1,170.10	12.80%	553.8	17.60%	2,013.43	14.70%
	2	339.1	18.10%	1,382.70	13.50%	672.5	22.50%	2,394.29	16.50%
	3	310.8	27.30%	1,367.90	15.00%	691.3	22.40%	2,369.95	18.60%
	4	402.6	21.70%	1,747.30	14.90%	690.4	20.20%	2,840.33	17.10%
1975	1	246.7	2.00%	1,037.30	7.20%	470.8	-2.20%	1,754.70	3.80%
	2	287.2	-1.40%	1,218.70	8.20%	549	-6.20%	2,054.97	2.60%
	3	244.2	-4.30%	1,189.50	7.90%	564.8	-2.90%	1,998.46	3.10%
	4	330.9	4.60%	1,520.50	11.30%	574.4	9.30%	2,425.87	9.90%
1974	1	241.9	4.60%	967.8	8.90%	481.4	1.30%	1,691.20	6.00%
	2	291.4	3.80%	1,126.70	5.80%	585.5	2.00%	2,003.69	4.40%
	3	255.3	3.70%	1,102.30	5.70%	581.5	-0.60%	1,939.11	3.50%
	4	316.4	8.80%	1,366.20	9.40%	525.5	-4.90%	2,208.01	5.50%
1973	1	231.2	1.70%	888.4	5.40%	475.4	17.20%	1,595.01	8.10%

	2	280.7	0.70%	1,065.10	9.50%	574	16.70%	1,919.89	10.10%
	3	246.2	1.80%	1,042.70	9.90%	585.1	12.80%	1,874.02	9.60%
	4	290.8	-7.40%	1,248.70	4.10%	552.5	11.20%	2,092.08	4.10%
1972	1	227.3	11.90%	843.2	12.20%	405.7	15.40%	1,476.09	13.00%
	2	278.7	4.10%	972.71	1.40%	491.8	16.60%	1,743.18	11.60%
	3	241.9	7.40%	948.71	0.60%	518.9	18.20%	1,709.45	12.30%
	4	314.2	13.80%	1,199.50	10.80%	496.7	18.90%	2,010.28	13.20%
1971	1	203.2	1.80%	751.3	5.20%	351.5	-4.30%	1,305.89	1.90%
	2	267.6	9.40%	873.2	5.60%	421.7	3.20%	1,562.48	5.60%
	3	225.2	14.20%	858	8.30%	439.1	11.50%	1,522.35	10.00%
	4	276	10.50%	1,082.60	13.00%	417.7	19.00%	1,776.28	13.90%

(p): Preliminary estimates
Last Update: June 2000
Source: Market and Business Analysis, NAA
Email: robem@naa.org

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Ad Spending In Newspapers Up 5.7 Percent In 1st Quarter 2000

Up 18.7 percent, national advertising has largest quarterly percentage

NEWS RELEASE

Debra Gersh Hernandez
Director of Public Relations (703) 902-1737
E-mail: gersh@naa.org

Vienna, Va. – Newspaper advertising expenditures for the first quarter of 2000 totaled $10.8 billion, an increase of 5.7 percent over the same period last year, according to the Newspaper Association of America.

National advertising continued to surge in the first quarter, with a gain of 18.7 percent, reaching $1.8 billion, its largest quarterly percentage gain since 1983. First-quarter numbers show retail up 0.5 percent to $4.6 billion and classified up 6.7 percent to $4.4 billion (see attached table).

"The continuing and phenomenal growth in national advertising this year is a strong testimonial to advertisers' faith in the selling power and brand-building of newspapers," said NAA President and CEO John F. Sturm. "Newspapers are working hard to become easier to do business with, and our progress is evidenced in these numbers."

Within the classified category in the first quarter, automotive was $1.1 billion, up 7.6 percent over the same time period last year; real-estate advertising dipped 4 percent to $667 million; recruitment grew 11.7 percent to $2 billion; and all other classified ads gained 1.7 percent to $556 million.

"The jump in recruitment advertising growth this quarter is another demonstration of this

industry's strength," said NAA Vice President/Market and Business Analysis Jim Conaghan. "Despite speculation about the impact of the Internet in this volatile category, the printed newspaper continues to be the central marketplace for recruitment advertisers."

NAA is a nonprofit organization representing the $57-billion newspaper industry and more than 2,000 newspapers in the U.S. and Canada. Most NAA members are daily newspapers, accounting for 87 percent of the U.S. daily circulation. Headquartered in Tysons Corner (Vienna, Va.), the Association focuses on six key strategic priorities that affect the newspaper industry collectively: marketing, public policy, diversity, industry development, newspaper operations and readership (added February 1999). Information about NAA and the industry may also be found at the Association's World Wide Web site on the Internet (www.naa.org).

CHAIRMAN GLEIMAN: And the witness has left, and that was before I mentioned how many parties want to cross-examine. Just think what is going to happen when I mention that.

Three parties have requested oral cross-examination, the AAPS, NAA and the OCA. Is there anyone else who wishes to cross-examine?

[No response.]

CHAIRMAN GLEIMAN: If not, then Mr. Straus, you may begin.

MR. STRAUS: Thank you.

CROSS-EXAMINATION

BY MR. STRAUS:

Q Thank you. Good evening, Mr. O'Hara.

A Good evening.

Q You must have been here longer than I have, because I just got here.

A Well, only barely.

MR. STRAUS: Okay. Good. So we are both fresh. Not nasty fresh. Should I keep going?

[Laughter.]

CHAIRMAN GLEIMAN: Very quickly, preferably in the same direction that Mr. O'Hara went when he went back to get his bag, but don't stop.

BY MR. STRAUS:

Q Mr. O'Hara, you are my last chance, have you read the SAI reports?

A I have not.

[Laughter.]

BY MR. STRAUS:

Q Well, you know, the Postal Service buys these, right, they don't get them for nothing, is that right?

A Correct.

Q Who reads them?

A I think it must be the people who commission them. The Postal Service is a big organization, and my understanding is that this is commissioned by people in the sales area. It was not commissioned by anybody on the pricing side of the house, and I saw that it would be better, really, in accordance with the way we have long approached our approach to the B4 criterion, where it references the effect on competitors as well as users of the various Postal products, what we have long done there is been very careful not to target any particular competitor or set of competitors.

In practical terms, the pricing witnesses are told, don't tweak your passthroughs to get your rates into a particular relationship with competitors. Don't get them so that they just a little bit lower here and a little lower there. Do your rate designed based on the entire set of

pricing criteria in the act of paying attention, when you get into the rate design area, to rate relationships and costs.

And when it comes time to think about getting a witness to carry through on that, I was not directly managing the pricing function at the time the rate design done, but I was involved in it, having been the rate level witness the time before.

Q Are you through telling me who reads it?

A I am telling you the reason that we did not -- that I have no read it.

Q Oh, I didn't ask. I didn't ask that.

A Okay. Then I will stop in the interest -- yeah.

Q At the beginning of that discussion, you said something about how the act requires that you consider the impact on competitors, and then you said that in rate design you don't intentionally try to hurt your competitors. Now, the law doesn't say thou shalt not intentionally try to hurt your competitors, it says you have to consider the impact.

Isn't it true that these SAI studies explain what the impact is of Postal rates on competitors in the private sector delivering mail matter?

A I do not know.

Q You don't even know what the subject of these reports is?

A I know that the subject is the alternate delivery industry. I do not know that it has anything to do with the impact of our rates on that industry.

Q Were you and the other witnesses in the past several cases that addressed the issue of impact on alternate delivery instructed not to read the reports?

A I know that I never directly dealt with the rate design in that area, but as the rate level witness in the last case, I don't recall a specific instruction not to read the report, no.

Q And you don't know whether the other witnesses who have testified on the impact of rates on alternate delivery have been instructed not to read the reports?

A I don't know, yes.

Q Have you read the testimony of Witnesses Bradpiece Baro and Merriman?

A I have read the direct testimonies. Bradpiece was direct testimony?

Q No, Bradpiece was rebuttal.

A Yes. I have not read that piece of testimony thoroughly.

Q You read Baro and Merriman?

A Yes.

Q Do you know enough about those three to know whether they, with the exception of the company that Mr.

Bradpiece just bought a month ago in the Buffalo area, that everything else that those guys do is in the mail?

A I guess I don't know, I don't recall specifically enough to be sure of that.

Q You were not employed at the Postal Service in the late 1970s, were you?

A No, I was not.

Q Were you doing anything that related at all to Postal rates?

A No.

Q So you wouldn't know then what happened in the late 1970s when the Postal Service reduced what was then Third Class rates on a temporary basis, that is you wouldn't know what happened to the alternate delivery industry as a result of that rate reduction?

A No.

Q Looking at today's ECR rates, do they provide advertisers with an affordable option for the geographically targeted or widespread distribution of high circulation advertising for products and services?

A That sounds like a quote of some kind.

Q From you.

A Yes, I thought it sounded familiar. I think it would still generally apply.

Q Well, I am asking you, you said as a subclass

composed primarily of advertising messages, ECR provides advertisers, et cetera. You also have the words "particularly those serving consumer needs."

A I'm sorry.

Q I don't want to let anybody accuse me of leaving out words. You said provides advertisers, "particularly those serving consumer markets with an affordable option," et cetera. And I just want to confirm that we are talking about now.

A Right.

Q At page 4 of your testimony, line 11, you have what I find to be an interesting phrase, you say "to artificially inflate the proposed ECR rates, as NAA and AAPS urge," and you stuck in the word "proposed" as if we are trying to change something rather than you. Let's just talk now about the present rates.

A That is the rates in effect as a result of R97?

Q That's right.

A Yeah.

Q Isn't it true that AAPS has not proposed to inflate that rate?

A I am having trouble distinguishing the testimony --

Q Of John White?

A Of John White from that of Witness Tye. I know

that Witness Tye has a long section on cost coverages and rate design which would have the effect of raising rates, not only raising the rates that are currently in effect.

I am having trouble remembering exactly --

Q If you don't remember, I mean that's okay. Let's ask something a little closer to home then.

Isn't it true that the USPS, that the Postal Service wants to inflate the present ECR piece rate?

A Yes, That is correct.

Q And it wants to deflate the present ECR pound rate?

A Yes. We believe that that is a better relationship to the cost of those two characteristics that are parts of the mail stream.

Q Please turn to page 6 of your testimony. You did something that one of my high school English teachers told me never to do, and that is use the word "this" by itself. On line 7 you say, "Surely this does not evince evidence" -- can you tell me what "this" means, what "this" refers to?

A It refers to the preceding, subject matter of the preceding sentence, which reads -- I probably have to go even further back in the paragraph.

We are talking about the volume forecast for the high density nonletter category within ECR and about the fact that of all the categories in ECR that the volume

forecasting witness provides separate forecasts for, seven different categories, the high density nonletter category is the only one for which the volume is projected to increase and the footnote goes a little further to explain that what that implies because of the nature of the volume forecasting process is that there is a decline in the average price of all the high density mail, taking into account as Witness Tolley does, a fixed weight price index of the different rate elements for high density mail.

Q You packed a lot into that one word "this", didn't you?

A I guess I did, and maybe --

Q Let me try to focus a little more narrowly.

Are you saying in that sentence, the one that begins "Surely this does not evince evidence", are you saying that the fact that high density nonletter mail which is used by newspapers, sometimes for their TMC products, is not being clobbered -- it shows, to use your words -- shows that the Postal Service's proposal is not an attempt to favor any particular industry over another?

A Yes, that is what I say.

Q Now what, of what comfort is it to the alternate delivery industry to the extent that its membership which Mr. White testified was the majority of its membership is not affiliated with any newspapers obtained from the fact

that your rate proposal not only benefits saturation mailers but possibly benefits some newspapers as well?

A It may be very little comfort to him. My point was that we have not targeted any industry. We have done a rate design based on cost and as a result of that one part of the industry that does compete with the Postal Service winds up with a rate reduction on average.

Q All right, but you understand, don't you, that for an independent alternate delivery company this is sort of a double whammy?

A I am not sure that I follow that entirely but I realize that if an industry does not use this rate category then this is no comfort to them.

Q You testify on page 7 that as further proof of your lack of intention to hurt the alternate delivery industry pound and a half phone books and samples like tubes of toothpaste or packs of potato chips get pretty heavy increases as proposed. Do you see that?

A Yes, I do.

Q Does the Postal Service really want to carry oddly shaped samples like little bags of potato chips?

A We would like to carry them if we could at acceptable contribution, but in fact we have found not just in just case but in the past that those pieces do not cover their cost and we have been proposing and in the last case

getting approval for significant increases in the rates for those.

Q And you have been doing that, haven't you, without really a great deal of concern that you would lose these pieces of mail to the people who could maybe handle, you know, oddly-shaped pieces a little more economically?

A Well, as long as we carry those pieces below cost, and my recollection is that they will still be below cost even with this proposed increase, that is correct. We do not suffer, in fact we would gain slightly financially to the extent that they are carried by other industries.

Q Now let's talk about the 1.5 pound phone book, phone directory.

What does that do to a carrier's day if he has a pound and a half extra for every stop on his route? Isn't a carrier satchel something like a 35 pound limit?

A I don't recall exactly but there are limits, at least in practice for routes that involve walking the park-and-loop or foot routes.

Q Do you have any idea what size that would be?

A I'm sorry?

Q What the pound limit might be? Does 35 pounds sound close to you?

A It sounds plausible, that's all I can say -- whether it is that amount or some other amount I don't know.

Q I'm sorry?

A Go ahead.

Q Wouldn't that carrier be going back to his vehicle about every 10 paces if he was carrying a pound and a half phone book for every house?

A If he was trying to do it for the entire route on one day surely that would be a problem.

My general understanding is that there are work practices in place that avoid that as much as possible but that is one reason this again is a cost-driven increase and in this case the cost coverage is still well above 100 percent but it is a cost-driven increase and so we want to carry those as long as we can continue to get a contribution from them.

Q Page 8 of your testimony --

A Yes.

Q -- you say that both NAA and AAPS witnesses allege that the proposal will divert their members' volumes and hurt their businesses but that those claims are, you say, "wholly without merit" -- does the Postal Service expect to obtain additional volumes of ECR mail above 5 ounces as a result of its reducing the rate for that mail?

A What I can point to specifically is the volume for the entire saturation, nonletter saturation rate category where we do have a rate increase in the fixed weight price

index and as a consequence of that rate increase a decline from the before rates to the after rates volume levels.

Q But that wasn't my question.

Do you expect to increase your volumes above -- at 5 ounces and above where you are reducing the rate?

A If there's going to be any increase, I guess it would be more likely to come there just because of the restructuring of the rates that you point to, but we do not have, as far as I am aware, any specific expectation of that sort.

What we have on the record is the forecast for the rate category as a whole.

Q Is it your belief that those who are now in the mail will stay in the mail no matter what happens to rates and alternate delivery prices and that those in alternate delivery will stay in alternate delivery no matter what happens to rates and alternate delivery prices?

A No. Price clearly matters. We have for our own ECR subclass of price elasticity that we call dot point six.

That price, the price responsiveness that is reflected in that elasticity incorporates movement of all kinds. It can be expansion of mailing or contraction of mailing from people who are now in the mail. It can be expansion or if you will conversion of advertising from non-printed material to mail printed material. It could

also be conversion from alternate delivery to postal delivery.

Q At worst, you are saying that it's wholly unsupported, that what logic would tell you would happen, would happen; that is, that you would reduce your prices, you would get increased volumes.

At least some of those volumes would come from other media like newspapers and alternate delivery, and that losing volume would hurt their business.

Have you heard the term, res ipsa loquitur?

A I'm not familiar with that, no.

Q The thing speaks for itself. I mean, does it need any additional support to say, if you're reducing your prices with your elasticity numbers, you're going to get more volumes, some of those volumes are going to come from others who are now carrying them, and when they lose those volumes, they get hurt?

A Well, I think I would like to see more than just the proper sign on the answer, if you will. I would like to see something that attempts to quantify.

Q Well, would you like Mr. White to raise your prices for you and then go out of business and then come in here and say, well, I'm out of business?

A No.

Q Okay. Didn't both Mr. White and Mr. Wilson

testify about what happened to the alternate delivery of magazines when the postage for mass circulation magazines was reduced in 1996?

A They did, yes.

Q That does not provide even an iota of support here? You say wholly unsupported.

That experience provides no support whatsoever for what they claim will happen if you do the same thing to the pound rate?

A Well, I mean, there may be a difference of opinion here. Periodicals and magazines generally are a different -- not just a different classification for the Postal Service, but a different product, generally.

And I think that the people who distribute magazines are looking for a different set of characteristics in the delivery than --

Q But some were in alternate delivery in 1995.

A Some were, yes.

Q And both Mr. White and Mr. Wilson, who represent the two entities you said provided wholly unsupported claims, documented the loss of magazine business when the postage rate went down.

So, I mean, the fact that they're different products, I mean, some were in the mail, some weren't.

Let me go to another point. Didn't Mr. White also

testify about what happened when the rates were reduced in the late 1970s on a temporary basis by the Postal Service back -- it might have been the only time it exercised its temporary rate authority.

A I do not recall that specific part of his testimony.

Q Well, if, in fact, the record shows that the last time there was a substantial reduction in what was then Third Class rates, there was a serious harm in the alternate delivery business, would that be in contradiction to your claim that the allegations of these witnesses are wholly unsupported?

A I guess it would have to be assumed for that to be the case that the industry and the relationships are still the same, 20 years later.

Q On the same page 8, you say that the prices provided by Mr. White indicate that his prices, the prices of alternative media, you say, are generally below the Postal Service's proposed prices.

Does Mr. White's company compete directly with the Postal Service for the carriage of advertising inserts?

A There is a lot of competition for advertising, generally, and some of that is between the Postal Service and companies like Mr. White's. Some of it is with newspapers, some of it is with other advertising media.

Q How much of the Postal Service's business is a solo, saturation, one-piece advertisement?

A Solo?

Q Yes.

A Meaning?

Q Mailed by itself, not part of someone's shared mail set. It can't be very much, can it?

A Well --

Q Let me rephrase it.

A Well, I don't know, but just from my own mail receipt, I see an awful lot of what I take to be saturation pieces from people like real estate agents, so I just don't know that they are split between saturation, solo, and saturation, shared.

Q But isn't the main competition for those preprints, newspapers, alternate delivery, and shared mailers like Advo and the others in the shared mail business?

A [No audible response.]

Q All right, isn't a great deal of the competition?

A Sure, sure.

Q Well, then, wouldn't at least another relevant comparison not be DSO's prices versus Postal Service's prices, but DSO's prices versus Advo's prices, to see what effect the postal rates really have out there on the people

who use you for delivery instead of their own carriers?

A I guess that would certainly be interesting to look at.

Q Well, why haven't you looked at that comparison with Advo's prices?

A I don't have Advo's prices. I don't know if we have any -- I know Mr. Giuliano put rebuttal testimony in in this phase, but I don't remember there being any prices --

Q I think I can represent to you that since 1970, Advo's prices haven't been in any record before this Commission.

You say at the bottom of 8 and the top of 9 that there is no showing that the industry's prices, and I think you mean the alternate delivery industry's prices, are anywhere near or above those of the Postal Service proposed rates. Are customer decisions based solely on price?

A Not at all.

Q If they were based solely on price, and alternate delivery were cheaper, then alternate delivery would be getting massive volumes and you would lose massive volumes, isn't that right?

A If they were based solely on price, that's correct. Assuming that the price relationships, that their prices are lower than ours.

Q You believe they area?

A I believe they are, yes.

Q Okay. So you believe they are lower, and the fact that they are not getting these massive volumes must be even further proof that people are looking at something other than price in making their selection?

A Yes. And I think both Witness White and Witness Wilson, in the most recent phase, talk about some of those differences.

Q So the mailers are then saying, okay, well, we have a balance scale here, whatever that thing is called, the scales of justice.

A Advertisers?

Q Advertisers, right. And on one side they are going to put -- or maybe even a shopper publisher, you know, like Mr. Bradpiece.

A Yes.

Q On one side, you know, they are going to put Postal Service price, the desirability of using the mailbox, several of them with the lack of, for a better word, the hassle of having to run an alternate delivery business.

A Right.

Q And on the other side, they might put the alternate delivery company's price, they might put the availability of, say, Sunday delivery, if that is what you are interested in.

A Yes.

Q And they will balance all those things, and whichever way it tips, that is which way they will go.

A Yes.

Q Okay. So if we have that scale, and for particular mailers it tips one way, or for particular advertisers, for others, it tips the other way under the present rates.

A That is -- yes.

Q All right. So if we lower the Postal Service rates and change that side of the scale, but don't affect the other side of the scale, isn't that going to tip the balance in the Postal Service's favor, irrespective of whether the alternate delivery rates are lower or higher than the Postal Service's?

A The direction of the effect will be as you suggest, but I have no basis, from their testimony, of assessing the magnitude of the effect.

Q Right. But the fact that their rates, if it is a fact, are lower than Postal Service rates doesn't mean very much, does it, because it is only one of the factors that people look at?

A Well, but it is the factor that we are concerned with at this point. Postage rates at least are what we are concerned with. We don't affect their rates. But it is the

easiest quantitative comparison, it by no means is the only factor in deciding how to get your advertising message across.

MR. STRAUS: Thank you, Mr. O'Hara, that is all I have.

CHAIRMAN GLEIMAN: Mr. Baker.

CROSS-EXAMINATION

BY MR. BAKER:

Q Dr. O'Hara, I would like to start with picking up on something Mr. Straus was covering, which is on page 8 of your testimony where you compared the ECR rates proposed in this docket to the Miami Herald's 2000 rate card.

A Yes.

Q And I believe you told Mr. Straus that you had not compared the Postal Service's proposed ECR rates to Advo's prices, is that correct?

A That's correct.

Q Have you compared the Postal Service's proposed rates to the rates charged by Mr. Baro's "The Flyer"?

A I don't recall having done so.

Q Have you compared the Postal Service's proposed rates to those charged by Val-Pak?

A No.

Q By Carol Wright?

A No, I don't know what those rates are.

Q So your testimony at no point ever considers the prices charged to advertisers by saturation mailers to participate in a shared mailing that is sent at ECR rates, is that correct?

A That is correct.

Q In an institutional response to Interrogatory NAA/USPS-1, we asked whether the Postal Service believed newspapers are major direct competitors of the Postal Service. And in the institutional response, the Postal Service said, and I will read the whole quote, "While newspapers can serve as alternatives from the perspective of advertisers, the Postal Service views newspapers as partners in many instances. For example, newspapers use Postal delivery for total market coverage products. The newspaper industry is also comprised of users of many other Postal services, including First Class mail and periodicals."

Had you seen that response before?

A I believe I had, yes.

Q Do you agree with it?

A Yes.

Q And so in your testimony, though, you compared the proposed ECR rates to those of one partner of the Postal Service, but not others, is that correct?

A Yes. The other partners are, if you will, in support of our rate proposals in this case. And what I was

dealing with in this testimony are the testimonies of Witnesses White and Tye.

Q So you only compare the Postal Service proposed rates to the parties in this case who may not be agreeing with the Postal Service proposal?

A Because that was the basis of the testimony that I was dealing with.

Q Let's suppose, -- but I think you told Mr. Straus that it would be interesting if you could see Advo's prices, is that correct?

A I am always interested in pricing information.

Q Well, if you had that information, would you make that comparison, would you compare the proposed ECR rates to that?

A Since I know that -- at least I thought I knew until I read Mr. Giuliano's testimony quite recently, that they used the Postal Service exclusively or almost exclusively for their products, that would tell me something about their other costs, since I would know from the rate schedules what they pay us, their other costs and profit margins.

Q Well, in your testimony at page 8, you seem to be saying that the rates that you compared -- the prices you compared the proposed ECR rates to seemed to be lower and that seemed to support the pound rate reduction. Let me ask

you this, if it turned out that Advo's rates are lower than the proposed ECR rates, would that be a reason to lower the pound rate?

A It seems incredible to me that it could be, giving that Advo is paying us the pound rate, today's pound rate.

Q So you are suggesting that the rates they charge to the advertisers who participate in a shared mailing are at least as high as your pound rate, your ECR rate?

A We need to be -- and here the whole rate comparison issue gets fairly complex.

We need to be careful about what we're doing. Advo surely has to recover from all the participants in its shared mailing piece, the cost of our postage and the cost it incurs to prepare, address, transport, the pieces that they deliver through us.

Q Well, let me ask it again to make sure I understand it. Is it your belief that the prices that Advo would charge an advertiser to participate in one of its shared saturation mailings is at least equal to or higher than the ECR price rate that Advo pays the Postal Service for delivery of that shared mailing?

A My belief is that the combination of prices that they charge the various participants in the mailing is at least as high as the postage.

Q Oh, so the total revenue they get is enough to --

A Yes.

Q But the price they charge to each individual participant would be much less?

A Yes, otherwise there would be no advantage from a saturation -- from the shared nature of their whole operation.

Q And you think, in general, that newspapers who compete with Advo price for delivery of advertising in a similar manner?

A I'm sorry?

Q Well, do you think that's the same way newspapers go about pricing their TMC programs?

In other words, do you think newspapers charge advertisers a price that, with enough other advertisers, is sufficient to cover the cost?

A I would expect so.

Q Okay.

[Pause.]

Your testimony -- by making the comparison in your testimony, I gather you do think it is appropriate to compare newspaper rates to ECR rates; is that correct?

A Yes. It's really the only way I could do it. I recognize that there are lots of complexities there, but there are, in the two rate cards that I had to work with, a whole variety of pieces of rates for newspaper inserts, and

I compared those with the ECR rates.

Q Do you think that's an apples-to-oranges comparison?

A It could be apples-to-apples for some of the mail, or some of the inserts versus the mail, but there are other things, as we were just discussing, where it's more complicated than apples-to-apples.

I don't know whether apples-to-oranges is quite the right metaphor, but clearly, it's very difficult to know what the combined effects of the prices charged for varying members of a shared mailing or a newspaper insert program are, related to the total cost of preparing the package and getting it delivered.

[Pause.]

EVENING SESSION

[6:00 p.m.]

BY MR. BAKER:

Q I think I heard you say that there might be some instances where you believe that is an appropriate comparison. Can you give me an example?

A I think anybody who is looking at an -- an advertiser looking at various channels for his saturation piece, saturation message, could look first of all at what it would cost him to mail it at ECR rates.

Q As a solo mailing?

A As a solo piece. Then he could look at the kind of blended rate that various people have talked about, related to the newspaper TMC methods of getting that delivered.

He could look, again, at a strictly alternate delivery method, and he could look, again, at what a shared mailer would charge him, so there are at least four possibilities there.

Q Is it your belief that a solo ECR mailing is viewed by an advertiser as the same product as a shared mailing?

A No, I don't think it is. I think a solo mailing, I would expect to have, for most advertisers, greater impact, and to be worth a higher price than any form of

shared mailing.

Q So it wouldn't surprise you then that the prices charged to participate in shared mailings or TMC programs are less than the ECR rates; would it?

A That's correct.

Q You are currently the Acting Manager of Pricing?

A Yes.

Q And we all know that's a government agency. Now, Acting Manager of Pricing sounds like an impressive title to me. Do you have responsibility over all the pricing witnesses in the Postal Service?

A Yes. Well, what I have responsibility for in this acting role is the pricing witnesses that are involved in the current omnibus case.

And many of those witnesses split their time between that set of duties and other duties as time is available, and in those cases, I do not necessarily supervise their other activities.

Q On page 16, line 17, of your testimony, you use the word, troubling, and I just want to make sure. Your definition of troubling is something that bothers you, raises concern?

So I'm asking your definition of the word, troubling.

A Yes, in that context. Could you give me that cite

again?

Q It was page 6, line 17, in the middle of the sentence.

A I thought you said page 16.

Q I'm sorry.

A Page 6, line 17, in my copy, yes. Yes.

Q Something that bothers you, something that raises concern?

A Yes.

Q Okay. As a government official that you are, would you find it troubling in that sense, under that definition, if the Federal Government intentionally took actions to reduce the editorial information available aide the American public?

A That's really, speaking of the federal government, way outside the area. At least there is a possible range of actions which the federal government might take in that area. It is way outside my range of expertise and responsibility.

My reference in that sentence has to do with things that are within my range of responsibility, which is the pricing of Postal products.

Q So you have no opinion on the question I asked?

A Well, you asked a very broad question, and I think that there might be lots of things that conceivably the

federal government could do, whether it is excise prices on paper, or taxes of some kind on paper prices, or environmental restrictions that would raise paper prices, that would have the indirect effect of raising -- of restricting the circulation of editorial content. That, I simply am in no position to express an opinion on.

Q Okay. Well, let me ask you something a little closer to your jurisdiction then. I want you to assume -- did you read Mr. Wilson's rebuttal testimony?

A Yes, I did.

Q Okay. I want you to assume that he was correct and I want you to make the assumption that reducing the ECR pound rate is going to result in reducing the amount of editorial content distribution to the American public. Now, make the assumption, I know you may or may not agree with it.

A Fine. Yes.

Q Would you be troubled by that result?

A Well, in my current position, the extent to which I understand the relevant Postal ratemaking policies of the federal government to apply are within Postal products. That is, we look at the XC content, as you know, of the various subclasses of mail. For example, Witness Wilson is hypothesizing --

Q He wasn't, I was.

A You were. Okay. Thank you.

Q He was testifying, I am hypothesizing.

A He was testifying here. Well, -- thank you.

The consideration is limited to things that are in the mail. ECR makes a contribution, by paying a high markup, to making it possible for there to be a lower markup on periodicals and other classes for which there is recognition of XC value.

Q And when the ECR -- and if the particular ECR mail happens to be a newspaper TMC product, that pays a high markup, too, right?

A Yes. The TMC -- well, yes, the advertising pays a high markup, it doesn't get XC value, it pays the same markup, roughly speaking, at least, as all other mail in the subclass, not that every mail piece has exactly the same markup.

Q I want to change subjects. Mr. Straus asked you some questions about your criticism of Witness Tye and Witness White for, in your words, not offering quantitative data supports the conclusion that volume would shift from newspapers to the Postal Service. But you acknowledge that Dr. Tye did cite to a table in Dr. Tolley's testimony. I am looking at pages -- well, I guess it is the bottom of 1, carries over to the top of 2 there, in particular on page 2, lines 9 through 11.

A Yes.

Q Okay. Did you see Dr. Tye's response to the Postal Service Interrogatory 52 to him where he went into some more detail on that point?

A I do not -- I undoubtedly saw it, but I can't recall it.

Q Well, in that response, Dr. Tye made calculations directly from the ECR elasticities estimated by Postal Service Witness Thress and used data directly from Thress' workpapers to quantify the volume shift. Do you recall that at all?

A I don't recall something specifically in the interrogatory response, no. I recall his actual testimony as filed.

Q Okay. Well, the interrogatory will speak for itself. But do, as you sit here today, have any disagreement with Witness Thress' estimate of the elasticities?

A No. My understanding of that elasticity is, with all elasticities, it reflects the response of the dependent variable, in this case, mail volume of a particular subclass in response to a change in whatever variable the elasticity is related to, holding all other things constant.

So if we are looking at the effect of Witness Thress's elasticity of ECR volume with respect to the price

of newspaper advertising, that tells you how much the effect of newspaper advertising prices have on ECR volume, holding everything else equal, in particular ECR rates constant.

Q Did Witness Thress recently testify that the volume forecasts are on target and do not need to be updated?

A I believe he did.

Q Okay. So does Witness Thress' testimony assert a direct cross-elastic effect between ECR volumes and newspaper ad rates?

A It asserts that the higher newspaper ad rates are, the higher ECR volumes will be.

Q That is a cross-elastic effect, isn't it?

A Yes. Yes.

Q Okay. I want to ask you a few questions about Section II-C of your testimony, where you take issue with Dr. Tye's colorful term "stealth" anti-competitive intents.

A Yes.

Q I want to start by reading you a quotation from a Postal Service document, and asking if you might recognize it.

"Newspapers, particularly preprinted inserts, are the primary and most direct threat to the USPS position in the advertising market in the next five years." Close quote.

Does that sound at all familiar to you?

A That sounds like something out of our marketing plan of a few years ago.

Q Very good, actually. Precisely, that was on page -- I guess at page 2 of that document. That document bears a date October 1997. Do you remember reading it about sometime around that time or shortly thereafter?

A I read it sometime thereafter. I think I probably did have a copy available to me because it was widely distributed at around that time. But it did not really attract my attention until sometime later when it became an object of controversy in the R97 case.

Q Was that marketing -- you did see the marketing plan document. Were you in the pricing division at the time?

A Oh, indeed. Yes.

Q Okay.

A That was during the case in which I was the rate level witness.

Q So that was not one of these documents that the marketing department commissions that's kept away from the pricing people?

A No, that's not a research document; it's a -- I really think of it as a dream book, something that they put all of their hopes in and dreams and I guess fears in this

particular case down for all the products. You can find similar language about practically every product in there.

Q Well, do you think that statement was an accurate statement of the Postal Service's marketing team when it was made?

A Well, it must have been. I mean, they made it. But I have to say, having read the whole document, there are so many statements of that general, in my view, overstated language that I think it needs -- certainly I interpret that with a great deal of piles of salt, if you will.

Q Marketing puffery?

A Yeah. Sort of trying to excite, if you will, the people on the sales side of the house to greater efforts in calling on customers and so on.

Q Do you think it's an accurate statement of the Postal Service's marketing team today?

A I guess I don't have any really up-to-date information on that. I haven't seen a revised plan. I do know that we have had a very substantial reorganization of our marketing function since then.

Q You have not talked to anyone in marketing about that since then?

A About that particular issue, no.

Q Okay. The same document on page 3 had a quote: The third-party intermediaries are a critical part of the ad

mail. These "partners" include shared mail firms such as Advo, Val-Pak and Carol Wright, letter shops and printers, mailing list companies, and direct marketing and advertising agencies."

A Uh-huh.

Q I noticed that that lengthy list of intermediaries contained no mention of newspapers, which, in the institutional response earlier in this case, are now partners. Do you happen to know if that was a deliberate omission?

A I do not.

Q Okay. And finally, on -- well, okay.

So is it your testimony, then, that that marketing plan was basically an internal marketing department-driven wish list?

A Yes. That's my understanding -- interpretation.

Q Does the Postal Service ever act upon any of those wishes?

A They may. I'm sure they do take actions which are designed to pursue at least some of those. I think that is one of the objectives of a document such as that, is to inspire marketing activity in the field.

Q Well, let me ask you one more. On ad page 16, October 1997 of this document, under the heading labelled Retail, the following passage appeared: An indication of

the potential opportunity in the retail segment comes from auto dealers, which as a category grew 68 percent since 1990, mostly in newspapers. If auto dealers were taken out, newspapers would have actually had a negative growth of 17 percent. By providing reliable scheduled day delivery, ad mail could shift a substantial portion of this mail from newspapers.

Do you remember that language?

A I don't remember the language, but the whole notion of trying to act on something like that is familiar to me.

Q And that was an accurate statement of the marketing team's goals in that time.

A Well, at least as far as the individuals who were working on that program, yes.

Q And shortly thereafter, did not the Postal Service conduct a program in Fiscal Year '98 that was an attempt to do just that?

A They started something like that and stopped it very quickly.

MR. BAKER: I have no more questions, Mr. Chairman.

CHAIRMAN GLEIMAN: Mr. Costich.

Before you begin, just let me mention that there is some coffee over there if anybody wants it. Also, it

looks like we're going to go fairly late tonight and there's still some question in my mind about just when the keys disappear out of cars in the garage. To be on the safe side, I think if you've got a car down in the garage with the keys in it, you may want to try and find your way down there before seven o'clock to retrieve your keys.

Mr. Costich.

MR. COSTICH: Thank you, Mr. Chairman.

CROSS EXAMINATION

BY MR. COSTICH:

Q Good evening, Dr. O'Hara.

A Good evening.

Q Could you turn to page 10 of your testimony?

A Yes.

Q And look at lines 13 through 16.

A Yes.

Q Here you say that the general public already benefits from a single-piece rate that is lower than it would have been absent automation.

A Yes.

Q And you go on to say that the automation projects that have been implemented over the last decade or so have had a direct impact on the rates paid by residential and small business mailers.

A Yes.

Q I'd like to explore this statement of yours with the aid of a cross examination exhibit.

MR. COSTICH: May I distribute this, Mr. Chairman?

THE WITNESS: I think I may already have a copy.

MR. COSTICH: Mr. Chairman, could I have this document marked for identification as OCA/USPS-RT19-XE-1?

CHAIRMAN GLEIMAN: You certainly may.

[Cross Examination Exhibit No. OCA/USPS-RT19-XE-1 was marked for identification.]

BY MR. COSTICH:

Q What I've distributed is a graph that displays for first-class -- for the first-class letter subclass attributable costs per piece, revenue per piece, and contribution per piece for the period 1988 through 2001.

Dr. O'Hara, does this exhibit cover the last decade or so --

A Yes.

Q -- that you referred to?

A Yes.

Q Does that exhibit show that the attributable costs for per piece for the first-class letter subclass was about the same in 1990 as it was in 1999?

A Yes. And I think it's important to recognize in looking at this graph that it's for the entire letter

subclass, as you said, and that my testimony in the sentences we're discussing refers to the single-piece portion of that subclass.

It's my belief, and I think it's most dramatically illustrated here around the 1996 to '97 part of it where there's a dramatic decline in unit cost, that a good deal of the cost trend over the whole decade can be explained by changing mix between single-piece and work-shared mail. Reclass, which went into effect in late Fiscal '96, had the most dramatic effect, but throughout the decade, there has been virtually no growth or actual decline in the single-piece portion of the mail and all the growth and more has been in the work-shared portion of the mail.

The work-shared portion has much lower costs on average. And so as the mail mix changes, the average cost can stay constant or decline even though the cost for individual pieces -- individual rate categories within the subclass as a whole goes up.

The mail mix change has really been quite dramatic in first-class.

Q Are you saying that the relatively flat attributable cost per piece is not the result of automation?

A It's the result of several things. Automation is certainly one, but mail mix change is another.

We have -- this is, I would assume because it

doesn't state otherwise and from my own knowledge of the numbers in general terms that this is not corrected in any way for inflation.

This is just the nominal rates, nominal cost, nominal contribution, and I think you can -- what you see if we have level unit costs in a time of increasing nominal wages and transportation costs, it's got to be several factors. One of them is automation, another is mail mix.

Q Does this exhibit show that the revenue per piece for the first-class letter subclass increased about eight cents per piece over the last decade?

A That looks about right, would go from 1990, say, to the year 2000, but that would be 27-1/2 to 35. Yes, it's about eight cents.

Q Could we estimate that for 1988, the cost coverage was about 167 percent?

A I guess I could do the arithmetic. I get on that very approximate basis 162 percent. So whatever -- what was your number?

Q 167. I just did 25 over 15.

A Oh, okay. Comes to the same degree of precision, anyway.

Q And in 1996, it's about 175 percent?

A 1996, you want to tell me what your -- 20 into 35, is it?

Q Right. Actually, I did the contribution per piece and took that as a percentage of the --

A Oh. Well --

Q I had 15 over 20. Three over four?

A Yeah.

Q Seventy-five percent?

A Yes. It's a little less than 15, but if we're going to worry about that, we'll maybe have to come back to it.

Q Around that 175 percent?

A Yes. It's going to be less than 175, I would expect, if we did the arithmetic, but still it's in that ballpark.

Q And then in 1997, it's 200?

A 1997, we're at 17.5 and 35, if you will. Thereabouts, yes.

Q You can see that the contribution and the attributable cost are virtually the same.

A Yes. Another way to get there.

Q And it stays like that from then on, right?

A Yes. I mean, from then on is projected basically, but --

Q Well, '98 and '99 are actuals.

A Yes, but I thought we were looking at 2000. Maybe that wasn't the --

Q Yes.

A -- number you were -- 2001 is definitely projected.

Q Yes. So the first-class letter subclass mailers have benefitted from automation by having their cost coverage jump from 167 to 200?

A Yes, but their rate increases have been well below average. I know that in the last case -- that is, R97 -- and our proposals, this case, and in R94, which is the last time that the rates for the whole subclass were adjusted that I'm familiar with, all of those rates were for first-class below the average increase.

I think the cost coverage increase here is primarily driven by mail mix changes that I was referring to earlier. If you look within the subclass, we sometimes compute implicit cost coverages. The cost coverage on the single-piece part of the mail is almost as a matter of arithmetic lower than on the work-shared portion of the mail.

When you take those same two numbers if they were to remain constant through time and change the weights attached to them as a result of changing mail mix toward work-shared mail, put a greater weight on the higher contribution or coverage, higher mark-up or coverage portion of the mail over time and the average coverage or mark-up is

going to go up.

That is the primary factor going on in this relationship, and I think if you want to look at the benefits of automation, you need to look at the benefits in terms of the rates people pay, and there, the single-piece rates especially in percentage terms have been lower than average and I can't say it's every subclass that they're lower than, but I think over the whole period, they're certainly below average and below most of the individual other subclasses.

So that is at least what I had in mind in looking at -- back at the history of the last decade.

Q When you say below average, you're referring to the rate increase that first-class has received recently?

A Yes. Yes.

Q The rates have increased.

A Oh, indeed they have. They have not increased -- and really, I guess I'm most familiar with the increase in R97 and the proposed increased here -- not increased as much as the CPI, for example, but they have increased. There has been a decline in real terms, but an increase in nominal terms.

Q And the contribution per piece has jumped about 75 percent over the last decade?

A Over the last decade from a dime to -- that is

about right, again in nominal terms.

Q Well, all of the numbers on this chart are nominal.

A Yes. But I guess what I'm suggesting is that from the point of view of the mailer, it would be more meaningful to look at real terms, and there, you would find, at least on the postage rate, I believe a decline in real rates. I don't know about the real contribution.

Q Well, if attributable costs have been essentially flat, they would decline even more steeply than -- in real terms, that would decline more steeply than the rate, correct?

A Yes. Yes. Flat nominal declines more real than increasing nominal, yes.

Q So in real terms, the contribution would still be increasing?

A Yes. And again, I come back to the change in mail mix as the main explanation for that.

MR. COSTICH: Mr. Chairman, I would like to distribute another chart, if I may.

Mr. Chairman, what I have distributed is a figure from Witness Callow's testimony. Perhaps I should give two copies to the reporter for the benefit of the transcript, but it's already in evidence.

I guess before I move on, I should ask that the

cross examination exhibit be transcribed and admitted.

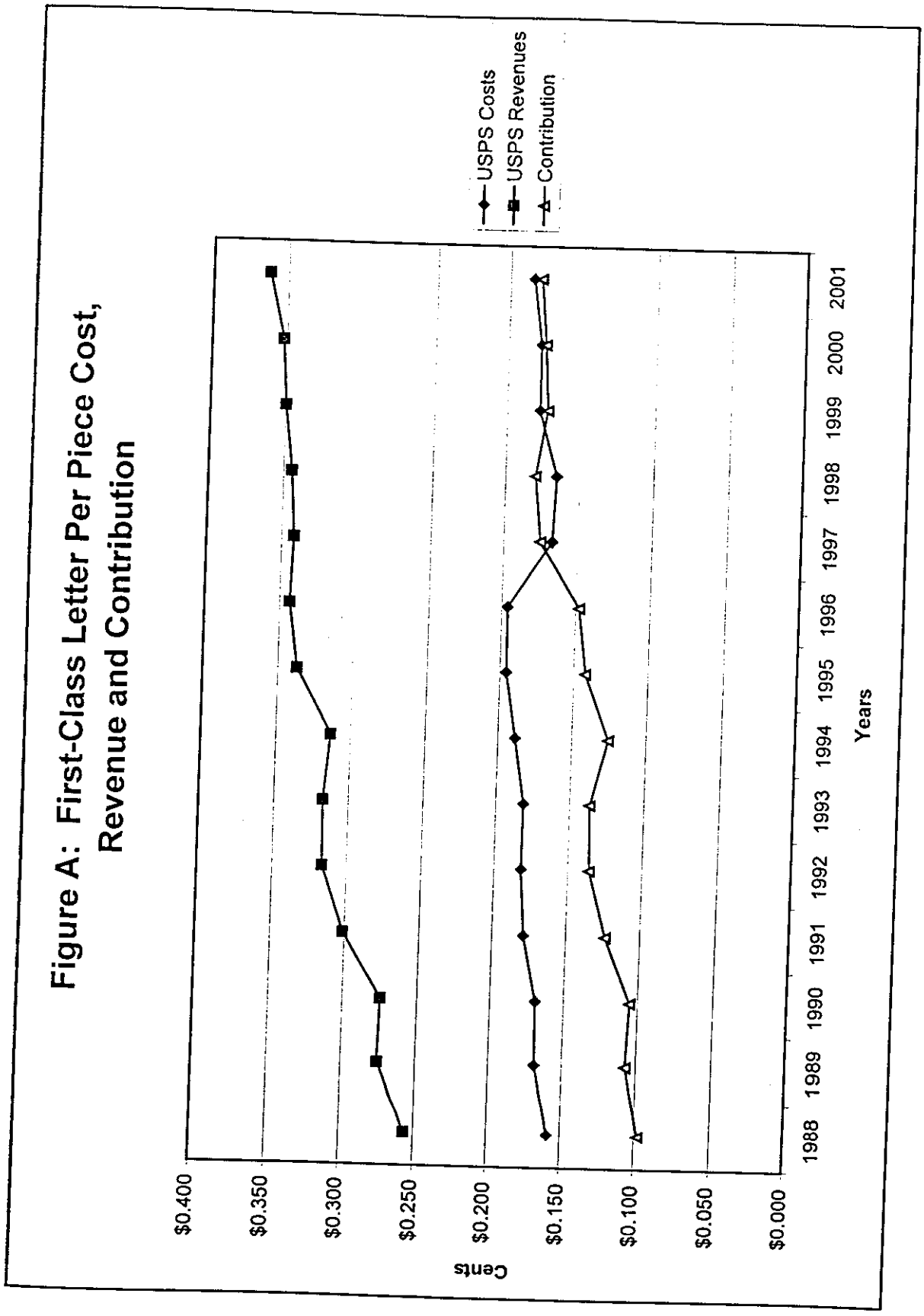
CHAIRMAN GLEIMAN: That's cross examination exhibit number 1, correct?

MR. COSTICH: Yes, letters and numbers followed by a 1.

CHAIRMAN GLEIMAN: It is so ordered transcribed and entered into evidence.

[Cross Examination Exhibit No. RT19-XE-1 was received in evidence and transcribed in the record.]

OCA/USPS-RT19-XE-1



BY MR. COSTICH:

Q Dr. O'Hara, the graph I have just distributed --

A Yes.

Q -- shows a comparison of the first-class letter subclass cost coverage index with the Commission's recommended cost coverage index and the average cost coverage index over the last -- little over a decade.

A Yes.

Q In Docket Number R97-1, were you the Postal Service's pricing witness?

A I was what we refer to as the rate level witness. Yes.

Q And in that case, did you recommend the use of a cost coverage index as a means of comparing relative burdens on subclasses?

A I did indeed. I think it is -- in situations where it differs from the mark-up index, which is in the case I was addressing there, changes in costing methodology, it's superior. If the changes in costing methodologies are not significant as they were in our proposal in that case, then there may not be so much difference between them. But I certainly did recommend a cost coverage index.

Q Would it be fair to characterize this graph as showing, at least for the period '98 through '96, that the --

A '88?

Q Pardon me. Yes, '88 through '96. That the Commission's recommended cost coverage index for first-class, the overall average cost coverage index and the actual first-class cost coverage index all stayed fairly close to each other?

A Yes.

Q After 1996 there was a dramatic rise in the cost coverage index for the First Class letter subclass.

A That is correct.

Q And it at least on this graph continues all the way through 2000?

A Yes, and there again I think we are seeing, and the timing is even more dramatic in this graph than it was in the previous one, the effects of the really dramatic mail mix shifts that followed on reclass.

'97 will be the first year after reclass. You see mail shifting into the workshared portion of First Class letters where the cost coverage is much higher and that shift has continued, although not at quite a rapid a rate, up to the present.

Q I will ask the same question I asked earlier. Is there no effect of automation showing up in this graph?

A I would be hard put to disentangle the effects of automation from everything else that goes into a cost

coverage and especially a cost coverage index.

It is perfectly apparent in the rate increases that we are proposing for various subclasses of mail in this case that letters in general are subject to much more favorable cost trends than flats; First Class letter subclass is almost all letter shape.

We have had great success with automation that is reflected in the unit cost coverage, our unit cost curves that we had on the previous graph in addition to the mail mix changes, but I simply don't know in this graph how to disentangle the effects of automation from everything else that goes into a cost coverage index, so I think I know the main thing that is driving that post-1996 shift in the appearance of the curve and I don't know whether automation is in there or not.

Automation affects both workshared mail and nonworkshared mail so in that sense I am not sure I would expect something dramatic from automation to be extractable from a graph of this kind.

Q Getting back to page 10 of your testimony, the last sentence on that page, you refer to the modest increases for the First Class letter subclass --

A Yes.

Q -- being well below overall inflation rate and well below the systemwide average increase.

A Yes.

Q I read this paragraph and this whole section of your testimony to be suggesting that this is the result of automation. Am I to understand there is more involved?

A No. I think here we are, because we are looking at the percentage increases relative to the CPI, we're taking out indirectly the effects of nominal increases, we are, although it is not at all explicit in the testimony, looking at a penny pretty much across the board for both workshared and nonworkshared mail in this case.

There I think you do see primarily the effects of automation on each of the components of the First Class letter subclass, at least the letter shaped part of it which is the overwhelming majority.

Automation has restrained the growth in nominal cost for workshared mail and also for single piece mail, and so I think that is the case where the effects of automation versus mail mix can be most clearly seen in fact is in the rate increases.

Q Well, granted that the last rate increase for First Class and the proposed rate increase for First Class are certainly lower than inflation and certainly lower than the average rate increase requested.

Shouldn't we be looking at the attributable costs of First Class to determine whether there should be a rate

increase at all?

A Yes. I think that is a reasonable thing to do.

Q And if we look at the cost over the past few years, we see that they have not increased at all, isn't that correct?

A That is correct but there I return to the mail mix changes.

You get mail shifting from a higher cost single piece category to a lower cost workshared category and the average cost can go down even though the cost of each of the two pieces has gone up.

I actually have looked at that for the increase in this case. I did a calculation some time back looking at -- this was trying to explain our rate proposals to mailers, looking at the unit cost for workshared and single piece mail using the PRC methodology in the R97 decision. You have to go way back in the appendix and add up all across the cost segments to do that, and compare that with the unit cost using the same PRC methodology projected for 2001, that is the test year in this case, compared to the test year in the last case, and both the single piece unit cost had gone up and the workshared unit cost had gone up notwithstanding the fact that the average for the subclass as a whole goes down.

As I recall, the number was around 2 percent plus

for single piece and 4 percent plus for workshared.

So in ballpark terms the rate increase for single piece at 3 percent is pretty close to the cost increase, the rate increase for workshared even though it is a penny. In most cases it is on a smaller base so it is a bigger percentage and again it is not that far from the cost increase.

Q When you were the I say "pricing witness" -- what was your term?

A Rate level, in the sense that we have multiple pricing witnesses, so the others are for particular subclasses or combinations of subclasses but yes, rate level witness, if we want to use postal jargon.

Q You did not propose separate rate level increases for workshared and nonworkshared First Class mail?

A I did not propose a separate cost coverage, no.

Q And that is because there is only one subclass here, correct?

A That is correct. I did discuss in my testimony the same mail mix factors that we have just been going over in talking about what the appropriate level of the subclass coverage was, but the whole ratemaking exercise has always treated the letter subclass as a whole.

Q That subclass as a whole has for the last few years been experiencing no attributable cost increase at

all, correct?

A That is what your chart shows and certainly I have no reason to question that.

I think I understand as I have said many times by now that this is a combined effect of dramatic mail mix changes and automation.

Q But it is the subclasses that the Commission sets markups for, correct?

A Indeed.

Q So I come back to the same question. If there's been no increase in the attributable cost for the subclass why should there be a rate increase for the subclass?

A Because once we get into, and here we are talking about the penny increase, the cost for the various components of the subclass have increased.

I haven't done this calculation but I would expect that if you look at the -- no, it's going to be more complicated than I can work through in my head, but at least let me come back to the cost for -- each big piece of the subclass have increased and on that basis I think a rate increase of that roughly equivalent to percentage is entirely in order.

Q Could you look at page 11 of your testimony. At lines 18 and 19 you say that the typical household mails some lower cost courtesy reply mail and some higher cost

handwritten mail and pays an average rate for all of it.

A Yes.

Q When you say "higher cost handwritten mail" how much higher is the cost of a handwritten letter shaped one ounce piece of mail?

A I don't have or know of a particular number. What I do know of and in addition to common sense base that statement on is that the notion that our encode rates are much lower for handwritten mail. We more often have to make use of remote video encoding or even handle the pieces manually and manual operations or the remote video encoding itself has a higher cost, going back to another case I am familiar with, the First Class cost models to some degree, even though I am not a costing witness.

I would also add however that the cost difference I believe has narrowed recently because the most recent advances in automation have actually tended to benefit the single piece or handwritten mail more than the earlier advances in automation which, if you will, hit the easiest targets first, the nearly uniform machine printed mail.

The remote computer reading of even script addresses now permits a large fraction of that mail to be barcoded on our first handling without the intervention of keying, and so while it is still higher cost, the gap I would think in percentage terms is getting narrower without

being able to quantify the degree to which that has happened.

Q Certainly in terms of mail processing costs --

A Yes.

Q -- the difference is getting narrower.

A I have to say that that has been my real background in that last discussion.

Once we get a barcode on it, then the subsequent handlings are I would expect not that different and you would then be left only with the effects of not being able to get a barcode on as high a percentage of the handwritten mail as the machine printed mail.

Q Isn't the lower cost of courtesy reply mail the result of fewer sorts and less transportation and no delivery?

A To the extent those are the case, each one of those factors would tend to reduce cost.

I am not aware of any quantification of that. I know that some reply mail is, say for a national credit card company, shipped to locations that are convenient for them, such as South Dakota and Nevada, where their other costs of operation are low, and so it is not necessarily the case that the transportation costs are low, but some of the other factors probably do work in that direction.

Q And starting at lines 26 on that page you are

talking about QBRM here --

A Yes.

Q -- saying that deaveraging of First Class mail is workable for QBRM due to some specific characteristics and you go on to list them.

When you say QBRM meets mail preparation standards that ensure its automatability, there is no difference there between QBRM and CRM, is there?

A There may be some differences but I think the -- it's supposed to be, the QBRM is supposed to be automatable. It is supposed to have a good barcode on it. We approve the mail piece design when it first starts out, so I think absent some follow-up problems the machinability and automatability, if you will, of QBRM and CEM ought to be close. I don't believe we have quite the same degree of review for CEM.

We have a requirement in place that if the envelope is enclosed in a mailing it goes out at the automation rate, it has to have the delivery point barcode on it but we typically don't examine the pieces for the quality of the print and all of that, and we certainly do examine the QBRM pieces at least at the initial design.

Q You also say that QBRM avoids revenue assurance issues, and is it Witness Miller who discusses the revenue protection problems that the Postal Service believes are

associated with the CEM proposal?

A I believe it is, in this case at least.

Q And you also say that QBRM -- this is over on page 12 --

A Yes.

Q -- doesn't have the problems associated with two differently denominated First Class stamps?

A That is correct.

Q And Witness Miller also discusses this problem at some length?

A Yes.

Q Could you look at page 14 of your testimony? At the top of that page you say that the Commission is reminded that when faced with the OCA's CEM proposal in Docket Number R97-1, the Postal Service sponsored market research by Witness Ellard.

Would you like to remind the Commission of what the Commission said about Witness Ellard's market research in that case?

A Well, among the other things that were handed to me with the two cross examination exhibits was the relevant pages from the decision about that research and I have had a chance before I came up here to look at them.

I think the Commission doesn't need me to remind them of what they said. I did want to remind them of the

Postal Service interpretation of Witness Ellard's research.

We may just have to agree to disagree on the interpretation of that research.

Our belief is, as I summarize in the next few sentences, paragraph -- a few sentences in that paragraph, that the threshold question that Witness Ellard's research deals with is whether people really prefer a two stamp system or a one stamp system and our interpretation of his research is that they prefer, the majority of them, a one stamp system.

I realize that the Commission did not find that research or other aspects of our opposition to CEM convincing. They weighed all the evidence and recommended the classification and I just wanted to take the opportunity here to restate the Postal Service's interpretation of that research.

MR. COSTICH: Mr. Chairman, I'd like to distribute another document before I proceed, and I'd also like to ask that the previous document that I referred to, which is figure 5 from witness Callow's testimony be transcribed in the record at this point.

CHAIRMAN GLEIMAN: We have that one marked as Cross-Examination Exhibit No. 2 for this witness. Is that correct? You can nod, and I'll pick it up.

MR. COSTICH: It's already in evidence, Mr.

Chairman. I don't know that it needs to be marked as a cross-examination exhibit as long as it appears with the cross-examination.

CHAIRMAN GLEIMAN: Well, just so people aren't confused, why don't we mark it as OCA/USPS-RT-19-XE-2? I've got a copy of it.

MR. COSTICH: The reporter has two copies.

CHAIRMAN GLEIMAN: I'll give him one that's already marked, then. He won't have to mark the other.

MR. COSTICH: Thank you, Mr. Chairman.

CHAIRMAN GLEIMAN: Otherwise, someone might wonder what it was doing floating around in the transcript.

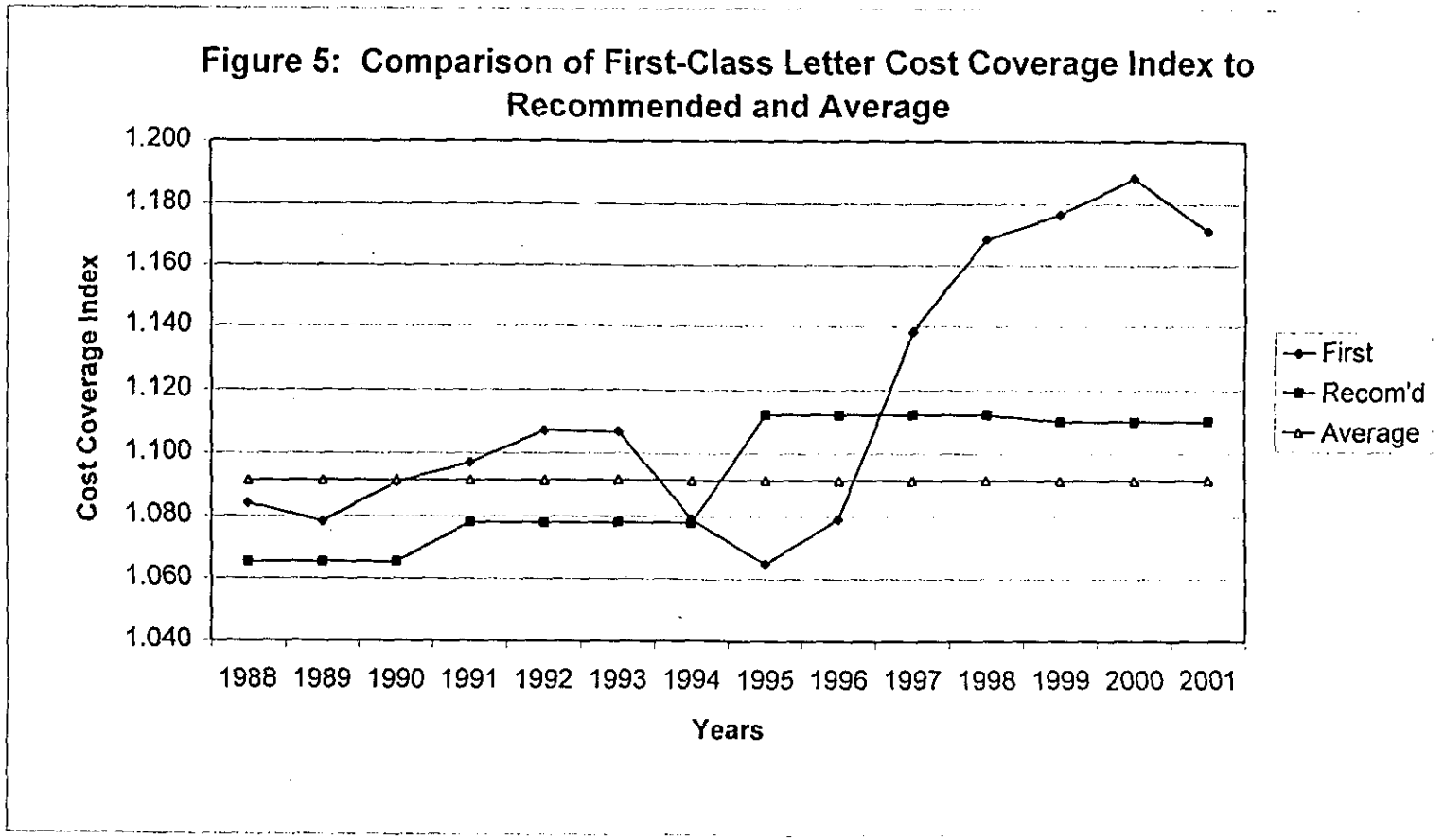
[Cross-Examination Exhibit No. OCA/USPS-RT-19-XE-2 was marked for identification, received in evidence and transcribed into the record.]

OCA/USPS-RT-19-XE-2

Revised 6-29-00

Table 5
COMPARISON OF FIRST-CLASS LETTER COST COVERAGE INDEX TO RECOMMENDED AND AVERAGE INDICES

Cost Coverage Index	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001
First	1.084	1.078	1.091	1.097	1.107	1.107	1.079	1.065	1.079	1.138	1.169	1.177	1.188	1.171
Recom'd	1.065	1.065	1.065	1.078	1.078	1.078	1.078	1.112	1.112	1.112	1.112	1.110	1.110	1.110
Average	1.091	1.091	1.091	1.091	1.091	1.091	1.091	1.091	1.091	1.091	1.091	1.091	1.091	1.091



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MR. COSTICH: What I've distributed, Mr. Chairman, is three pages from the Commission's opinion in R97-1, pages 322 through 324, and I'd like to refer the witness to the last page of that, page 324.

THE WITNESS: Yes.

BY MR. COSTICH:

Q The last sentence of paragraph 5188, and this is referring to witness Ellard's survey, the Commission says when the possibility is raised that a two-stamp system might contribute to a future increase in the basic rate for first class letters, 86 percent of the public say they would prefer to stay with the one-stamp system. Is this the testimony you're referring to in your testimony?

A I'm referring to -- and I think if I don't use the 86 percent number in my testimony; I use the --

Q Well, if you look at line 9 on page 14 --

A Oh, yes; I'm sorry. I had my attention focused on the earlier part of the paragraph. But yes; those are the sort of two sets of results, so one, 60 percent without the issue of pushup being raised and 86 percent with the issue of pushup as part of the question.

Q And the Commission's resolution of the 86 percent question was that the premise of the question was seriously misleading; is that correct? The end of paragraph?

A I presume you're -- that last sentence? So you

must be down in another paragraph.

Q Yes.

A Yes, the end of the next paragraph. Yes; you've read the decision correctly.

[Pause.]

Q Could you look at page 323 of the opinion?

A Yes.

Q In paragraph 5187, the Commission attributes to witness Ellard the finding that about three-fifths of the population say they are very likely or somewhat likely to use two denominations for bill-paying. Do you see that?

A Yes, I do.

Q That is inconsistent with what you've said in your testimony, isn't it?

A No, it's not. There are two separate issues on the table here, and I think it is important to distinguish them. The question that I cite in my testimony, the 60 percent response is the penultimate question on the survey, and 86 percent response is the final question. The questions that are being addressed in paragraph 5187 come earlier, as witness Ellard explained to us at least as we were discussing the research.

It's important to, in order to get a considered opinion of the proposal as a whole, to ask people a series of questions that get them thinking about it, and one of

those questions is would it be more or less convenient to use two stamps than one stamp? I'm not pretending to cite the precise wording of the questions. Another question is would you use it or not, and the over -- or roughly three-fifths, as you said, are either very likely or somewhat likely to use both stamps were they to be available.

After having walked through that kind of sequence of questions, they were then asked the sort of bottom line question: what would you prefer? And on that question, despite yes, if you give it to me, I'll probably use it, 60 percent of the people responding that way, 60 percent said I'd just as soon not bother. I'd rather have a one-stamp system. And that's why I focused on that, that sort of being the bottom-line conclusion of the whole sequence of questions.

Q Do you recall whether that was the infamous question 10 in the Ellard survey?

A I think that was the -- 60 percent is the penultimate question. I don't know the numbering, but the infamous one, the one that is referred to as premise seriously misleading question?

Q Yes.

A If that's 10, the question that I'm referring to, the 60 percent response, is question nine. Eighty-six is

question 10. That's why I had the 60 percent in my head, because I think that is a much simpler way to look at the results of his survey without dealing in a difference of opinion as to whether seriously misleading is really the appropriate way to characterize that. I realize that's the Commission's conclusion, but I don't think we have to go into that. We can just look at the 60 percent.

MR. COSTICH: Thank you, Dr. O'Hara.

I have no further questions, Mr. Chairman.

CHAIRMAN GLEIMAN: Is there any follow-up?

[No response.]

CHAIRMAN GLEIMAN: Questions from the bench, which is getting thinner and thinner, except for me?

[No response.]

CHAIRMAN GLEIMAN: If there are no questions from the bench, then that brings us to redirect. Would you like some time with your witness?

MR. COSTICH: The Postal Service requests 10 minutes, Mr. Chairman.

CHAIRMAN GLEIMAN: You may have 10 minutes. And I want to remind people again about the garage just in case. The other thing, before we take a break, is that I'm going to place on the chair right up at the aisle here in the front row some sets of MODS data that I unfortunately have to refer to in some questions that I'm going to have ask

witness Neels when he's back up on the stand later, and I wanted to make sure that if anyone was interested, they might want to have copies of this material, and certainly, the witness should take a look at it.

So let's take 10.

[Recess.]

MR. ALVERNO: There was a homework assignment that was directed to witness Miller yesterday from the OCA. We were asked to give a status report today.

CHAIRMAN GLEIMAN: Yes, sir.

MR. ALVERNO: I can report that we have every expectation of being able to provide responsive information and filing it tomorrow.

CHAIRMAN GLEIMAN: Thank you so very much.

By the way, I don't intend to have a long break for dinner. We're going to have these 10-minute breaks along the way between now and whenever we finish, either late tonight or tomorrow morning. And, you know, if it really gets pushed, we'll stretch one to 15 minutes so people can run down to McDonald's or something like that.

[Recess.]

CHAIRMAN GLEIMAN: Redirect?

MR. ALVERNO: No, thank you, Mr. Chairman.

CHAIRMAN GLEIMAN: Postal Service understands how to play the game late at night at the end of the hearings.

I think it's an inverse relationship that we have between the length of cross-examination and the weight given to testimony.

[Laughter.]

CHAIRMAN GLEIMAN: You'll have to wait and find out.

Mr. O'Hara, that completes your testimony. We appreciate your appearance and your contributions to the record. And we thank you, and you are excused, and I enjoyed reading your paper. Perhaps one of these days when we're not involved in a rate case, we'll have an opportunity to discuss it. I thought it was very interesting.

THE WITNESS: Thank you; I'd like to do that.

[Witness excused.]

CHAIRMAN GLEIMAN: Our next witness, Ms. Duchek?

MS. DUCHEK: The Postal Service calls Dr. William Greene.

CHAIRMAN GLEIMAN: Dr. Greene, you look familiar because you've been sitting out there all day today, but I don't think that I've had the opportunity to swear you in before during these proceedings. Am I right?

DR. GREENE: No, you have not.

CHAIRMAN GLEIMAN: That being the case, if you would please raise your right hand.

Whereupon,

WILLIAM H. GREENE

was called as a witness herein and, after being duly sworn, was examined and testified as follows:

CHAIRMAN GLEIMAN: Thank you.

You may proceed, counsel.

MS. DUCHEK: Mr. Chairman, before I do, how would you like to proceed? Should I move in both Dr. Greene's rebuttal testimony and his NOI-4 response now?

CHAIRMAN GLEIMAN: Well, if I have my score card correct, he is the first of four NOI-4 witnesses, so I think that it wouldn't hurt if you moved both pieces in, and anybody who wants to look at the transcript can sort out which one we're talking about, unless there is an objection on the part of somebody to doing it that way.

MR. MCBRIDE: Mr. Chairman, Michael McBride. I just wanted to make a suggestion.

CHAIRMAN GLEIMAN: Sure.

MR. MCBRIDE: And just for ease of working with the transcript, it might be better if we did the rebuttal testimony; got that done; and then had all the NOI-4 stuff in one place together rather than two things jumbled here.

CHAIRMAN GLEIMAN: Well, maybe we'll put all four NOI-4 witnesses on the stand at one time since they'll probably be asked a lot of the same questions, and we can just go down the line panel.

MR. MCBRIDE: That may be the best idea yet.

CHAIRMAN GLEIMAN: But I don't think we can get away with that.

I think Mr. McBride is right, as I reconsider what I just said. Let's just get the rebuttal testimony in and get the cross-examination on that out of the way.

Thank you, Mr. McBride.

Mr. McKeever?

MR. MCKEEVER: Mr. Chairman --

CHAIRMAN GLEIMAN: You feel just the opposite; I know.

MR. MCKEEVER: Well, no; I just checked with Mr. McBride, and he indicated that he does not have any cross for any of the NOI witnesses; is that right?

MR. MCBRIDE: That is correct.

MR. MCKEEVER: And I only have, you know, a very few questions from Dr. Bozzo on the NOI and none for Dr. Greene. So I'm not sure -- while Mr. McBride's suggestion may have made sense if there was going to be cross on both pieces of testimony -- but I'm really at the pleasure of the chair.

CHAIRMAN GLEIMAN: Let's get the rebuttal testimony --

MR. MCKEEVER: Okay.

CHAIRMAN GLEIMAN: -- out of the way.

DIRECT EXAMINATION

BY MS. DUCHEK:

Q Dr. Greene, I've handed you two copies of a document entitled Rebuttal Testimony of William H. Greene on behalf of the United States Postal Service, marked as USPS-RT-7. Are you familiar with that document?

A Yes, I am.

Q Was it prepared by you or under your supervision?

A Yes, it was.

Q Do you have any changes to make?

A I do, yes.

Q Would you please indicate what those are?

A On page 10, line 9, the word disturbing should be disturbance; that is change I-N-G to A-N-C-E.

On the same page on line 10, the word ordinary should be narrow.

On page 17, footnote 14, T-15 should be T-14. That's it.

Q With those changes, if you were to testify orally today, would that still be your testimony?

A Yes, it would.

Q And are the changes marked on the two copies that I gave you?

A Yes, they are.

MS. DUCHEK: Mr. Chairman, I'm going to hand two

copies of the rebuttal testimony of William H. Greene on behalf of the United States Postal Service, USPS-RT-7, to the reporter and ask that they be entered into evidence and transcribed into the record.

CHAIRMAN GLEIMAN: Without objection, if you provide two copies to the court reporter, I will direct that that material be received in evidence and transcribed into the record.

[Rebuttal Testimony and Exhibits of William H. Greene, USPS-RT-7, were received into evidence and transcribed into the record.]

USPS-RT-7

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

POSTAL RATE AND FEE CHANGES, 2000

Docket No. R2000–1

REBUTTAL TESTIMONY
OF
WILLIAM H. GREENE
ON BEHALF OF THE
UNITED STATES POSTAL SERVICE

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I. STATEMENT OF QUALIFICATIONS

My name is William H. Greene. I am a professor of Econometrics at the Stern School of Business at New York University and, since 1995, chairman of Stern's Economics Department. I have taught at Stern since 1981. Prior to that I taught Econometrics at Cornell University from 1976 to 1981. I received Masters and Ph.D. degrees from the University of Wisconsin at Madison in 1974 and 1976, respectively. I worked briefly as an economic consultant in the private sector in 1980–1981 at National Economic Research Associates and have also provided consultation to numerous corporations, including American Express Corp, Ortho Biotech, Inc., and The Reader's Digest. I have published numerous works in econometrics, including roughly 40 articles, one of the world's most widely used computer programs for econometric computation, *LIMDEP*, and, notably for this proceeding, the widely used textbook *Econometric Analysis*, which several of the witnesses in this and the prior omnibus rate proceeding, including Neels, Smith, Bradley, Higgins, and Bozzo have all cited in their testimonies.

I do note that this is my first appearance before the Postal Rate Commission. I have no knowledge of the details of Postal Service operations or data systems beyond that contained in the testimonies that I reviewed. The scope and nature of my testimony will be limited to econometric technique and methodology, about which I have written extensively. I will discuss this further in Section II.

II. PURPOSE AND SCOPE OF MY TESTIMONY

I have been asked by the United States Postal Service, the sponsor of Dr. Bozzo's testimony, to comment on the testimonies of Kevin Neels and J. Edward Smith, both of which seek to rebut Dr. Bozzo's testimony and its predecessor by Michael Bradley in the 1997 counterpart to this proceeding. In particular, a number of issues have been raised regarding the econometric techniques used by Drs. Bradley and Bozzo to estimate 'volume variability factors' for labor costs in mail processing. (Volume variability is a measure of the elasticity of mail processing costs with respect to volume.)

I have learned through my reading of the various testimonies that I have reviewed that the Postal Rate Commission has traditionally assumed that this cost elasticity is 1, or 100 percent. So far as I have been able to discern from the work I have read—there is a summary in Dr. Bozzo's testimony[1]—this value is based essentially on judgment, impression, 'common sense,' and intuition. No rigorous statistical procedures were ever used to arrive at this parameter. Drs. Bradley and Bozzo have used quite complex multiple regression methods and a large data base generated within the U.S. Postal Service system to measure this effect, and have found a large amount of evidence that disagrees with the traditional assumption. They found consistent evidence that the volume variability factors for a large number of specific activities in the mail processing chain is considerably less than 100 percent

Witnesses Neels and Smith have raised a large number of criticisms of the data, methods and models used by Drs. Bradley and Bozzo and, by implication, their results. Primary among these issues are:

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[1] USPS–T–15 at 4–13.

- Data quality problems and the issue of measurement error;
- Problems of nonrandom sampling that they suggest arose as a consequence of the methods by which the data were purged of possibly incorrect, misreported, or missing values;
- The issue of functional form relating to the use of certain 'panel data' style models—the 'fixed effects' model in particular;
- Certain other issues concerning the ways in which the regression model was formulated—among them the problem of missing variables.

I intend in my testimony to limit my attention to issues of econometric technique and model building. There is an enormous amount of fine detail in all the testimonies I read, about the specifics of and technical aspects of mail processing procedures and costs, work flow, and technology. Many of these details are advanced by Drs. Neels and Smith as severe complications that cast doubt on the econometric results. Although I believe that some of their comments in this regard are superfluous to the questions at hand, I will nonetheless not be addressing any of this material, and offer no testimony as to their relevance to the econometric modeling. Some of my testimony will be somewhat technical. Unfortunately, this is unavoidable. Some of the issues that the intervenors have raised, such as the problem of 'sample selection,' are, themselves, fairly esoteric.

My testimony will be related to the following general topic areas:

- The criticisms of the methods by which the data set was 'scrubbed' miss some important points about sampling, random sampling in particular, the nature of model building, and, very importantly, the issue of 'sample selection.'
- The discussions in the Neels and Smith testimonies relating to issues of measurement error rely upon some widely held

misconceptions about the topic. Most of their discussion on this issue is incomplete, and some of it is incorrect.

- Much of the discussion of the 'fixed effects' model for panel data, particularly the claim that it is inferior to a pooled regression or a regression based on group means, is incorrect.

My testimony will briefly review the models developed by Dr. Bradley and Dr. Bozzo, and the criticisms of them raised by Drs. Neels and Smith. A more detailed summary appears in Dr. Bozzo's testimony. I will then turn to the specific econometric issues listed above. To summarize my conclusions, I find that while some of the criticisms raised by Drs. Neels and Smith might provide useful guidance for refinement of the data used for estimating models for volume variability, many of the more methodological among their comments are exaggerated and/or misleading. I disagree with the suggestion that virtually all of the flaws suggested by the intervenors would have acted systematically to bias Bradley's and Bozzo's estimates of volume variability downward. On the contrary, from what I have read, I believe that the Bradley and Bozzo studies provide strong evidence that the 100% volume variability assumption should be reconsidered. While I am not prepared to commit to any specific value for any activity, I do believe that the two studies combined provide a strong suggestion that the right results will be substantially less than one.

III. CONCLUSIONS DRAWN FROM MY EXAMINATION OF THE STUDIES

I would not say at this juncture that every econometric or modeling issue that could possibly be addressed by Dr. Bradley or Dr. Bozzo has been addressed. I would definitely conclude that they have provided a substantial amount of evidence that the Commission should take very seriously.

The Commission should have taken a much more favorable view in 1997, and should at this time consider the panel data, fixed effects form of econometric analysis an appropriate platform for continuing work on developing a model for mail processing costs. The aggregate means models and time series regressions advocated by Drs. Smith and Neels discard far more useful information than the data scrubbing operation of which they have been so critical. Dr. Smith is simply wrong that the simple regression of group means on each other is the "least bad" model. Given the data set at hand, the simple regression of group means on each other is not the 'least bad' model; it is the second most bad model. The worst is the grossly aggregated time series regression proposed by Dr. Neels, followed by Smith's site means model, and the best of the lot is the fixed effects model. The arguments advanced by Smith and Neels in favor of their alternatives are based on flawed statistical reasoning, and should be rejected on this basis alone. The same conclusion applies to the visual devices advocated by Dr. Smith. I do not believe that the Commission should accept this kind of visual approximation as a substitute for careful econometric analysis.

The MODS and PIRS data are obviously far from perfect. But, from my vantage point, they appear to be quite good, and in the absence of a well designed and sharply focused data set designed specifically for studying volume variability, are as good as an analyst of mail processing costs could hope for. What is important is for the Commission and other researchers to evaluate these data using appropriate criteria. The criticisms raised in the Neels and Smith testimonies are, in many cases, inappropriate. Likewise, it sometimes happens that intuitively appealing evidence is misleading. For example, the standard deviations of the measurement error cited by the Commission in its Opinion (discussed below), which suggest an alarming amount of measurement error, appear to be much more discouraging than they really are. The intervenors in

this case have thrown up an array of criticisms of the data set that raise a standard that could never be met. Apparently, the MODS data were not created for the purpose for which they were used in this proceeding. But that is usually the case with large micro level data sets. Nonetheless, it does seem reasonable to assert that there is useful information in the MODS data for the determination of volume variability. I would suggest that the Commission take the view that researchers should extract from these data what useful information they contain, not go to great lengths to discredit the data, and then discard them and the analysis based on them.

On the other hand, if the Commission desires to pursue the line of research begun in these studies of volume variability, then continued development of micro level data should be undertaken. In that connection, it is a maxim in econometrics that micro level data are always better than aggregates. The reason is almost self-evident. Aggregation almost always discards information contained in micro level data, and never creates new information. On the other hand, if it is genuinely believed that the micro level data contain no useful independent information, then they can be aggregated. This process cannot be reversed. By this construction, I am unable to agree with Drs. Neels and Smith that analysis of the MODS data should be done using site means of the same data set that could be used in disaggregated form.

Finally, what kind of model should be developed? It is clear that it is appropriate to use multiple regression to model the response of labor costs to output—the appropriate definitions of these two variables and how to measure them is an issue to be settled elsewhere. A simple regression of hours (or its logarithm) on output of any sort (or its logarithm) will surely ignore many other factors that that should be in the equation, including the site specific differences that Dr. Bozzo has analyzed. I also assume that the various models proposed

will be based on the same data that have been used in this set of studies. In this instance, given the availability of micro level data, the fixed effects models proposed by Drs. Bradley and Bozzo are appropriate. At a minimum, they can do no worse, and will generally do better, than the site means ('between groups') models suggested by Drs. Neels and Smith. Aggregation discards information. The more crude the aggregate, the more information is discarded. At the very worst, if the disaggregated data really do not contain information beyond that which is contained in the group means, then a model based on the disaggregated data would simply mimic the model based on aggregated data.

Lastly, there is the question of econometric practice. The worst extreme I see here is Dr. Smith's willingness to rely on gross and misleading, crude two-dimensional scatter plots to defend a specific estimate of a parameter. Between this and the appropriate model lie the pooled regressions suggested by the intervenors, in which they impose restrictions on a regression model, then argue, in direct contradiction to long established results, that these results have improved the estimates. In particular, the suggestion that a pooled regression that imposes the restriction that there are no site specific effects somehow removes a bias inherent in the fixed effects model is simply not true—exactly the opposite is the case. Imposing restrictions can induce biases, relaxing them cannot. At the other end of the scale are Drs. Bradley's and Bozzo's carefully developed econometric models that embody current practice using an elaborate panel data set. The models have been subjected to numerous specification and sensitivity tests, and include features such as dynamic structure, time and site effects, models for autocorrelation, and flexible functional form for the estimated equation. As I noted earlier, I believe that this is the appropriate framework within which the Postal Service should be analyzing mail processing costs.

IV. THE VOLUME VARIABILITY MODELS

A. Dr. Bradley's Cost Equation Model

Dr. Bradley's model was estimated using a data set that provided for a large number of specific installations at numerous points in time. The three crucial variables were:[2]

HRS_{it} = hours of labor at site i in period t

TPH_{it} = total pieces handled at site i in period t

$MANR_{it}$ = manual ratio, a site specific measure of one aspect of the technology at site i in period t.

The specific equation estimated for 'Direct Activities' (sorting, etc.) includes linear, quadratic, and all cross products of these three variables, time effects contained in a time trend which allows for a discrete change in the time effect at a midpoint in the period of the analysis, one period lagged terms for the logTPH variable and its square, and a site specific dummy variable which allows for the site specific constant, or 'fixed effect.' All told, the equation includes 15 variables plus seasonal dummy variables, plus the site specific constants, so it is quite large. Additional lagged effects are introduced into the model through the use of a correction for autocorrelation in the disturbances. A similar, but slightly more involved, model was specified for the 'Allied Activities.'

The data used for the study contained numerous obvious flaws, and as a consequence, they were 'scrubbed' by a procedure that removed from the sample all observations:[3]

(1) that were not part of a continuous sequence of 39 consecutive observations that were otherwise 'clean;'

[2] See Docket No. R97–1, USPS–T–14 at 12–22.
[3] Id. at 30–37; see also USPS–LR–H–148.

(2) for which variables were obviously missing or erroneously coded as zeros;

(3) that were 'outliers,' in that they were in the top or bottom one percent of the distribution of the variable HRS/TPH.

Dr. Bradley subjected this model to numerous specification tests, including tests for whether a fixed or random effects model was appropriate—the latter rarely survives this test—tests for the presence of any site specific effects at all, and a test for autocorrelation.[4]

Estimates of the crucial elasticity of hours with respect to TPH for the direct activities ranged from 0.395 to 0.945; none of the estimates exceeded one.[5] The counterparts for the Allied Activities ranged from 0.720 to 0.829.[6] A number of other regression results were presented for other activities, all with similar results. The consistent outcome was that the volume variability varied across operations, rarely approached one, and almost never exceeded it. The equations were subjected to various specification tests, as noted, and estimated using several different methods, for example without the autocorrelation correction. The elasticity estimates were quite robust to the changes in the estimation methods.

Dr. Bradley conducted an analysis of the effect of measurement error in the TPH variable as well, using a method suggested in Hsiao's monograph on panel data.[7] As he points out, with panel data one can compute two different, albeit inconsistent, estimators of the slope coefficient and, at the same time, two inconsistent estimators of the measurement error variance. Solving two

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[4] Id. at 41–51.
[5] Id. at 54.
[6] Id. at 63.
[7] Cheng Hsiao, *Analysis of Panel Data*, Cambridge University Press 1986, at 63–65.

equations in two unknowns, it is possible to obtain one consistent estimator of each of these two parameters. Dr. Bradley carried out the analysis in a restricted model, and found that the consistent estimator was quite close to the fixed effects estimator. As Dr. Neels pointed out, Hsiao's method can (and in this case, does) produce a negative variance estimator.[8] This is a small sample issue—Hsiao's results are based on infinite sample results. I confess some skepticism of this procedure, not over Hsiao's analytical results, which are correct, but whether this is the best way to approach the analysis. Hsiao's result applies in a very narrow specification, and produces the disturbance variance result in a very narrow circumstance. It is prone to this finite sample problem. I emphasize, the test is not biased and is not misleading. It is simply one possible test and, I suspect, not the most robust one that could be constructed.

B. Dr. Bozzo's Updated Version of the Bradley Model

Dr. Bozzo's model is similar to Dr. Bradley's. In constructing it, Dr. Bozzo attempted to remedy some of the defects in Dr. Bradley's model that were argued by the intervenors and by the Commission, including the use of the data scrubbing procedure, and the absence of several other variables, including one relating to the capital stock and another relating to wage rates. As before, the model fit was a translog (log quadratic) model with site specific intercepts (fixed effects). The translog model was specified with four lags of the logTPH variable and its square, as opposed to one in the earlier study.[9] The data preparation for Dr. Bozzo's model is considerably more elaborate than Dr. Bradley's. The equation is also considerably more elaborate, involving the following variables:

[8] Docket No. R97–1, TR. 28/15637.
[9] USPS–T–15 at 117–118. Note that since Bozzo also changed from AP level to quarterly data, his model embodies a lag structure that is effectively 13 times longer than Bradley's.

HRS_{it} = the log of hours

TPH_{it} = the output (volume) variable (enters with four lags)

CAP_{it} = the capital stock variable

DEL_{it} = deliveries, to capture network and density effects

$WAGE_{it}$ = the wage variable

$TREND_{it}$ = trend variable to capture smooth time effects

$MANR_{it}$ = the manual ratio

QTR_t = specific quarterly dummy variables.

Dr. Bozzo estimated the model without transforming the data to overall mean deviations, unlike Dr. Bradley. The point is important as, in the current case, all relevant elasticities become lengthy functions of the parameters and the variables. The estimated elasticities obtained are similar to Dr. Bradley's, ranging from 0.522 to 0.954. (USPS–T–15 at 119–120; 126). Since considerable attention has been paid to the effects of different methods of estimation and forms of the equations estimated on the quantitative results, it is worth noting that Dr. Bozzo examined his results for sensitivity to different methods of estimation and computing of the elasticities, and found that the various computations produced very similar results. Id. at 130–131, 140-141, 151–160. See also USPS–LR–I–107.

V. ECONOMETRIC ISSUES RAISED BY THE INTERVENORS

As noted earlier, an extremely long list of problems with the preceding analyses was raised by intervenors Neels and Smith. Many of these related to whether the variables used in the analyses were appropriate or accurate measures of the activity being analyzed, e.g., whether total pieces handled

(TPH)[10] was an appropriate volume measure and whether the Postal Service's plant and equipment data really contain useful information about the capital stock. I will not be commenting on these concerns as I do not have the necessary background or knowledge about the specifics of the Postal Service. However, they did, as well raise several issues related to the econometrics.

- Dr. Neels: Most of Dr. Neels's rebuttal focused on the data issues mentioned above. He did make a number of points about the effects of measurement error that he feels persistently bias the estimated elasticities toward zero—that is, toward a result less than one. He was also critical of the screening of the data which produced the estimating sample. I will address this below.
- Dr. Smith: Dr. Smith has raised a daunting litany of criticisms of both the Bradley and Bozzo studies. I will focus my testimony on only a few of these:
  (1) He, as does Dr. Neels, criticizes the data scrubbing procedure.
  (2) He feels that the analysis is 'short run' in nature, and is therefore inappropriate for the phenomenon being studied.
  (3) He feels that observable (with his eyes) evidence contradicts the results of Dr. Bozzo's analysis.
  (4) He is critical of the panel data, fixed effects model that Dr. Bozzo used.

A fair amount of Dr. Smith's testimony is centered on issues of underlying microeconomic theory. Some of this is used to criticize the theoretical underpinnings of Dr. Bozzo's study. It is not my intention in this testimony to address issues of the underlying theory of any of this analysis; I come to this proceeding as an econometrician. However, I am familiar with the economics and econometrics of the estimation of cost and production functions. My doctoral

[10] Total pieces fed (TPF) was used in place of TPH in the automated and mechanized operations. See USPS–T–15 at 50–52.

dissertation and a subsequent paper published in the *Journal of Political Economy* with Laurits Christensen have long been regarded as pioneering studies (they were among the first) of the marriage of theory and empirical estimation of cost and factor demand equations—they are, for example, a standard application presented in microeconomics textbooks. While there are valid points in Dr. Smith's discussion of the theory behind an appropriate cost function, there are also some noteworthy errors. For example, Dr. Smith states that "Dr. Bozzo's treatment of homotheticity appears to lead to incorrect conclusions." Tr. 27/13196. He then states:

> In his testimony, Dr. Bozzo asserts that "... capital and labor variabilities will be identical in equilibrium under the assumption that cost-pool-level production (or cost) functions are *'homothetic'* ... Homotheticity implies that changing the level of output of the operation will not alter relative factor demands such as the capital/labor ratio, in equlibrium (and other things equal). However, the Postal Service testimony is replete with examples of the implementation of major investments designed to reduce costs. ... The focus is on the elimination of major labor costs via capital investment to achieve an overall reduction of total costs. Accordingly, the application of a homotheticity assumption appears to be an inappropriate assumption. (Id.)

Nowhere does the theory state that the firm in equilibrium will never adjust its capital labor ratio in response to changes in relative prices. Even if the technology is homothetic, the firm will respond to a change in relative prices by substituting away from the factor that is becoming more expensive unless it is unable to. This has nothing to do with whether the production function is homothetic or not. It is a question of factor substitution, and I do not believe that either Dr. Bozzo or Dr. Smith argued that the Postal Service operates under conditions of fixed coefficients, in which it would never substitute capital for labor in the face of increasing wage rates. The wages that appear in the labor demand functions estimated by Dr. Bozzo allow for adjustment in response to changes in

wages over time, and are not consistent with a fixed coefficients assumption. Dr. Smith seems, as well, to suggest that the technology of production in the Postal Service is nonhomothetic, which it may well be. But no empirical evidence for this has been presented, and the mere fact that the Postal Service has invested in labor saving capital does not say anything on the subject one way or the other.

A. Sample Selection and the Data 'Scrubbing' Procedure

As noted, Dr. Bradley subjected the MODS data to a screening process denoted 'scrubbing' that was intended to remove observations that were obviously erroneous and unrepresentative of the underlying relationship he was attempting to uncover. This data scrubbing—systematic removal of observations from the sample—is the subject of a considerable amount of discussion in this proceeding. There are two issues that are brought forth by such a procedure. The first is biases. Under certain circumstances, selection of observations based on specific criteria (as opposed to randomly) can induce biases in the estimates obtained with the resulting sample. The second concerns the issue of efficient use of sample data—the problem of 'throwing away information.' In point of fact, efficiency has not been an issue in this proceeding. However, at some points, comments by the intervenors that are related to this issue have nonetheless been made, evidently to cast doubt on the Bradley and Bozzo studies. This section will discuss these issues.

To review, Dr. Bradley's screening procedure involved the following steps:

(1) He removed observations with obviously missing values, zeros coded for certain activities known to be taking place at the facilities observed, and observations for which reported output was below a specified threshold.

(2) He imposed a continuity requirement that the remaining data set for a site contain at least 39 useable consecutive observations.

(3) He removed the top and bottom 1% of observations based on productivity—the ratio of pieces handled to labor hours.

The productivity screen could disrupt the 'continuity' of the data series for some sites, so his data scrub was iterative in that after step (3) it was necessary to revisit step (2).

Among the issues raised by the intervenors was that this screening process removed an extraordinary amount of data from the sample.[11] The response to this point has been made in passing by Dr. Bozzo, but it bears repeating. The samples involved in these analyses are large, even after the data scrub.[12] However, irrespective of the size of the samples, if we are agreed at the outset that the underlying model that we seek to discover applies to all the data points, then absent the problem of nonrandom selection discussed in the next paragraph, the amount of data discarded has no bearing on whether the results obtained with the remainder are biased or not. Under the assumption, Dr. Bradley could have simply randomly thrown away three quarters of the observations, and safely based his results on the remaining quarter. Certainly, intuition would correctly suggest that this waste of information would be costly. But the cost is that of having a smaller sample, which leads to less precise estimates than one might otherwise obtain—i.e., larger standard errors. It has no relation at all to whether or not those estimates are systematically biased in one direction or another. The issue of how many data were discarded is a red herring.

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[11] E.g., Docket No. R97–1, TR. 28/15609–619, 15632-15633, 15853. In the present docket, see Tr. 27/13163, 13172 and TR. 27/12796–12800.

[12] USPS–T–15 at 20–22, 95–102.

There is a substantive issue concerning how the data were removed from the sample. The overriding issue is whether the criteria used to discard data points were themselves related to the quantitative measure being studied, in this case, the log of the hours variable. This immediately raises a consideration that does not appear to have been noted by the intervenors or by Drs. Bradley or Bozzo. In particular, the missing or questionable values in the data set upon which the scrubs were based were the output variable, an independent variable, and the hours variable, the dependent variable. In the former case, once again, removal of data from the sample need not impart any particular bias to the results. Removal of observations from the sample because the output variable is missing or miscoded simply makes the sample smaller. Once again, the underlying relationship still applies to, and is discernible from, the observations which remain. Discarding observations based on values of the output variable is similar in its impact to throwing away observations randomly. On one hand, it may amount simply to wasting possibly useful information. On the other, if the output variable is erroneous while the hours variable is correctly coded, then my rule that the model must apply to all the data points would not hold, and the observation should be discarded. For an obvious example, positive output coded with zero hours makes no sense.

That leaves the missing or badly coded data on the dependent variable, hours. Bradley and Bozzo note a few cases.[13] Zero values recorded within a sequence of positive values are obviously erroneous. These once again violate the assumption that the model applies to all data in the sample, and they should be discarded. Bradley identifies another case, that of a 'ramping up' period, in

[13] E.g., USPS–T–15 at 109–110. See also Docket No. R97–1, USPS–T–14 at 30.

which the hours data would be unrepresentative.[14] As I noted in my introduction, I am not able to comment on specific technical aspects of the production process in mail handling. As such, I leave it to the others involved in this discussion to settle whether this is an appropriate omission. My own reading of the testimony suggests to me that it is.

The final case, and the one that does merit some attention is the trimming operation. Dr. Bradley eliminated the extreme values of hours per piece handled, from his sample, reasoning that these were unrepresentative and should be treated as erroneous data.[15] This is a specific sample selection rule that could, in principle, color the results obtained with the remaining sample. Dr. Bradley removed the top and bottom 1% of the distribution with this rule. Dr. Bozzo used a more detailed screen of this sort.[16] This productivity screen has called forth a criticism about "sample selection." Dr. Bozzo has commented specifically on the issue, but I believe there is a consideration that should be added. First, *sample selection* has become something of a bugaboo in econometric analysis, so we should be precise in our use of the term. What the productivity screen could potentially induce is a *truncation bias.* The distinction is important in this context because not very much is known about sample selection bias—except that it is bad—but a fair amount is known about truncation, and some of what is known has direct bearing on this case.

Dr. Bradley's productivity scrub of the data amounts to a trimming operation. Although the term 'selection bias' has been used in this context, the proper term is 'truncation.' Extracting data based on values of the dependent variable does have the potential to do some mischief. The pure theory of the

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[14] Docket No. R97–1, USPS–T–~~15~~ 14 at 30.
[15] Id. at 32.
[16] USPS–T–15 at 101–102, 110-112.

issue (discussed at various points in Chapter 20 of my text) does suggest that trimming the tails of the distribution would bias the least squares regression estimator toward zero. There are two crucial variables here, the asymmetry of the trim and the amount of the distribution that remains after the culling. The lesser the former and the greater the latter, the less 'damage' is done. In this regard, Dr. Bradley's productivity scrub scores well on both counts, in that he removed a fixed and very small percentage—one percent—from each tail. Dr. Bozzo's scrub was more complicated, in that he did not symetrically cull observations from the tails of the productivity distribution as per Bradley, but rather used cutoffs based on *a priori* information on maximum and minimum TPH per hour. It is impossible to tell what if any truncation bias would result from this. But, in any event, looking at his Table 3 (USPS-T-15 at 107) we see that, with two exceptions, the numbers of observations removed from the sample by the productivity scrub are generally so small that it would be hard to argue that the truncation effect would be substantial. His Appendix A (id. at 140) is suggestive. By foregoing the productivity screen and using "All Usable Observations," he obtains largely similar results. What I find surprising, and encouraging, about these results is that the theory suggests the estimates should rise, not fall, when the suspect observations are put back in the sample. In fact, most of the estimates fall, some substantially. Dr. Bozzo's type of screen does not conform to the assumptions in my text, so I don't see this as a contradiction. I do conclude that concerns about attenuation due to truncation of the data set are probably misplaced.

This leaves an important consideration, which entered both the Bradley and Bozzo discussions, the data continuity issue. Dr. Bradley imposed a 39

contiguous observation threshold on his sample.[17] Since he was fitting models with autocorrelation, this was primarily a practical consideration. Dr. Bozzo used tools (the econometric software package, *TSP*) in which time series with gaps are permissible, so the continuity requirement becomes a nonissue. But, in either case, it would be a question of sample size, not systematic coloring of the sample.

I am reluctant to generalize from narrow results to sweeping conclusions (though, in fact, both Dr. Neels and Dr. Smith have done so, using results taken from my book). But I do believe, based on the considerations discussed above, that the attention paid to the criticisms raised by Neels and Smith concerning the data scrubbing procedures has been exaggerated. Data that contain recording errors and other obvious flaws must be cleaned before being used. The samples used were large to begin with, and remained so after the trimming operations. By and large, the trimming operations were innocent. To the extent they were not innocent, the received theory suggests that the problems created are likely to be very small.

The foregoing is not meant to be glib. Data cleaning of this sort must be done carefully, particularly when the end result of the statistical process will be an input into a process as important as setting postal rates. Moreover, I do not dispute the possibility that the data scrubbing procedures used by Dr. Bradley were less than ideal, perhaps even less perfect than it potentially could have been had it been done differently at the time. Dr. Neels has raised some valid criticisms of the procedures; his observation that "unusual observations ... may also provide the clearest possible picture of how processing costs vary with volume" is well taken.[18] In his update of Dr. Bradley's model, Dr. Bozzo backed

[17] Docket No. R97–1, USPS–T–14 at 31.
[18] See Docket No. R97–1, TR. 28/15613.

away from some of Dr. Bradley's procedures. But in its Opinion and Recommended Decision from Docket No. 97–1 (PRC Op., R97–1, Volume 2, Appendix F), in the discussion of the cost models, the Commission devoted 11 of 45 pages (pp. 24–34) to this issue, and the conclusions it reached were quite dire. I believe that while many of the issues raised were appropriate, the conclusions were unduly pessimistic. After reviewing the procedures, the Commission stated "Witness Bradley's productivity scrub is exactly the kind of data elimination that econometricians try to avoid. Since the scrub eliminates values that are accurate as well as those that are erroneous, it leaves a sample that cripples the econometrics." Id. at 26–27. This is not true. Notwithstanding the truncation issue I raised above, discarding the extreme, though still valid, observations will indeed reduce the quality of the sample; it will do so by producing a model that is less precise (in terms of statistical measures such as standard errors) than it might be otherwise. But "cripples" overstates the case. The screen left more than adequate variation in the sample to allow econometric estimation of the model. Discarding anywhere from a quarter to a half of a sample might seem extreme, but it must be recalled that the sample that remained contained thousands of observations, not dozens, and the analysis attempted to estimate only a relative handful of coefficients. Faced with a need either to use obviously erroneous data or to discard with those data some observations that might have improved his estimates, I feel that Bradley took the right course. In order to argue that this data scrubbing "crippled the econometrics," one would have to argue that all or nearly all the data were bad, not just some of them.

The Commission makes one final argument about the data scrubbing process, that the process did not truly purge the sample of erroneous data. Id. at 33–34. This may well be true, but it is a side issue—the screen was not intended

for this purpose. They cite certain values derived by Dr. Bradley to illustrate the extent of measurement error remaining in the data. Two aspects of this observation should be noted. The first is already made above. The screen was intended to provide complete and appropriate data, not data free of measurement error. Whether or not TPH is an appropriate measure of the output or whether errors are introduced by the conversion of some other measure to the TPH are valid concerns, but they are separate issues from the screening of the data discussed here. The second point concerns two numerical estimates of the extent of measurement error that are given. These measures are interesting, but are prone to misinterpretation, as I discuss in the next section.

B. The Issue of Measurement Error

A large amount of the criticism leveled at the Bradley and Bozzo studies concerns the issue of measurement error. Specifically, Dr. Neels argues that the output measure used, pieces handled in Dr. Bradley's case and pieces "fed" in Dr. Bozzo's case, do not correspond to the true measure of output that should enter the calculation of volume variability.[19] He concludes that the output variable which appears on the right hand sides of both regression models is measured with error. From this, he concludes:

(1) "It is a well established econometric principle that measurement error in an independent variable causes downward bias in coefficient estimates." (Docket No. R97–1, Tr. 28/15604. He goes on to state a quote from page 437 of the third edition of my text.)

(2) "Measurement error in an explanatory variable of a linear regression model renders the estimator inconsistent and frequently biases

[19] Tr. 27/12792–12793, 12802 et seq. See Also Docket No. R97–1, Tr. 28/15598–600.

coefficient estimates towards zero." (Tr. 27/12800. In this instance, he does not invoke my text.)

The statements above are based on a widely cited, fairly simple result from econometric theory. Suppose that the analyst wishes to estimate the slope parameter in a regression model:

$$y = \alpha + \beta x^* + \varepsilon$$

where x^* is the desired independent variable, volume in this instance. Let x denote the observed independent variable, pieces handled, however measured. We further assume that x deviates from x^* by a random measurement error, denoted u, so that $x = x^* + u$. In order to obtain the results that form the backbone of Dr. Neels's criticism, it must now be assumed that (a) the measurement error and the true variable are uncorrelated, (b) all variables are strictly uncorrelated across time and with other observations—i.e., we are using random samples—(c) the variances of u and x^* are denoted θ^2 and λ^2, respectively. With these in place, we obtain the fundamental result that the slope estimator in a least squares regression of y on x (the observable data) will estimate consistently, not β, but

$$\gamma = \beta \times 1 / (1 + \theta^2/\lambda^2).$$

Two important points to note are, first, that if there is no measurement error, then θ is zero and least squares does what it should (it estimates β), and, second, when θ is not zero, least squares estimates β with a persistent downward bias. This is the source of Neels's result stated above.

There are quite a few misconceptions about measurement error in the discussions on the subject that I have seen in this case.

(1) The suggestion that measurement error biases all coefficients downward is generally not correct. The preceding statement is true under the circumstances assumed. However, none of the models discussed in the present docket or the preceding one involve a simple regression of a dependent variable on a single independent variable measured with additive error. In a multiple regression in which one variable is measured with error in this fashion, the coefficient on the badly measured variable is indeed biased downward, though not by the same amount as in the simple regression case. Also, other coefficients in the regression are affected as well, in unknown directions. There is one easy case to analyze. In the preceding example, with measurement error, the constant term is biased *upward*, not downward. The effect of the measurement error is to tilt the regression line, not to push it down. This observation is important in this case because all models are multiple regression models, not simple ones. (This result appears in my text four pages after the familiar one cited by Neels.)

(2) Whether or not the bias in the coefficients carries through to biases in functions of those coefficients, such as the volume-variability factors, is unknown. Any function of the coefficients in a multiple regression in which a variable is badly measured is a mixture of coefficients, some of which may be biased downward and others of which might be biased upward. The end result is not knowable.

(3) In time series data with autocorrelation in the variables, the effect of the measurement error will be mitigated if the underlying variables are correlated across time and the measurement errors are not. This has particular relevance here because lagged values of the output variable

appeared in the model, through the estimation of the autocorrelation model.

(4) In a model in which more than one variable is measured with error, essentially all bets are off. The familiar attenuation result can no longer be shown. The directions of biases, if any, are unknown. Since the models fit by Drs. Bradley and Bozzo are translog, quadratic in the logs of the output variable, this result applies here. In addition, note that the square of the erroneously measured variable appears in the models estimated by Drs. Bradley and Bozzo, so the assumption of additive error which enabled the derivation of the multiple regression case in my text is also lost.

(5) If the original data were measured with additive error, surely the logs of them are not. This means that the blanket statements made by Neels cited above are incorrect. The results would obtain if the logs were measured with additive error, which would be the case if the original data were measured with multiplicative error. Thus, the analytic results above have to be qualified, in ways that are not obvious.

Lost in this discussion is an assessment of the likely magnitude of the quantitative impact of measurement error. Without a large amount of very high quality data, we cannot say much with any precision on this subject. We can form some impressions, though. First, the familiar result on the previous page can be written in the form

$$\gamma = \beta \times \rho^2$$

where ρ is the correlation between the true variable and the one measured with error. As noted, I am not able to make a judgment on this sort of calculation. I do note that the R^2s in the regressions reported by the various authors are

exceedingly high, sometimes above 0.99. Another effect of measurement error is to bias the fit of the model downward. Given values this high, I suspect that measurement error is not a major factor here. There is another way to approach this. Suppose the measure were multiplicative. It is possible to show that in this instance, the result becomes a bit cleaner,

$$\gamma = \beta/(1 + \theta^2).$$

Now, what value do we use for the measurement error variance? Suppose that the pieces handled variable varied in either direction by as much as 20 percent from its true value. This would imply a relative standard deviation of the measurement error of about 0.1, or a relative measurement error variance of about $\theta^2 = 0.01$. This is trivial. While a 20 percent error rate in the reporting of piece handlings seems fairly large, it implies only a 1% bias in the estimated coefficient, since with these values, $\gamma = 0.99\beta$.

All of these results are narrow theoretical conclusions based on a hypothetical situation. But I do believe that they have relevance here. The overriding result, which will fall out of any analysis, is that the damage done by measurement error will be a function of the 'reliability ratio':

reliability ratio = variance of true variable / variance of measured variable.

This, in turn, is a function of the correlation between the true and the measured variables. In cross sections, in which researchers attempt to measure such things as education, the reliability of self reported data can be extremely low. In this setting, by contrast, we are considering very stable flow variables which evolve reasonably smoothly through time. I would strongly predict that the reliability of output data in this setting is exceedingly high. Consequently, I would argue that criticisms of the models based on measurement error, while certainly

to be taken seriously, are greatly exaggerated. Moreover, isolated examples in which an observed flow rate of pieces handled differs noticeably from some known true value are not really informative. What matters is the *correlation* between the observed measure and what we are trying to measure. Even in the face of a few egregious reporting errors, this seems likely to be very high for these data sets.

Interestingly, there are a couple of estimates in the Commission's Docket No. R97–1 Opinion. They state, citing a table in Dr. Bradley's rebuttal testimony (which I have not seen),

> The standard deviations for total piece handlings (TPH) derived from the variances in Table 3 are 0.268 for manual letters and 0.297 for manual flats. The corresponding standard deviations for the measurement error are 0.123 for manual letters and 0.068 for flats. These results do not support the conclusion reached by witness Bradley that large and material measurement errors are absent from the piece handling data for these activities....In the Commission's opinion these results are inconclusive but tend to support exactly the opposite finding, that large measurement errors remain in the sample after witness Bradley's scrubs. (PRC Op., Docket No. R97–1, Volume 2, Appendix F at 34.)

This seems true. However, one must be careful. While standard deviations are in natural units, the crucial variables for our analysis are variances (squared standard deviations). For the values cited by the Commission, the reliability ratios are $0.268^2/(0.268^2 + 0.123^2) = 0.826$ for manual letters and $0.297^2/(.0297^2+0.068^2) = 0.950$ for manual flats. Whether these are large or small is debatable, but the impression one gets from the reliability ratio is certainly different from the raw data.

C. Panel Data Treatments

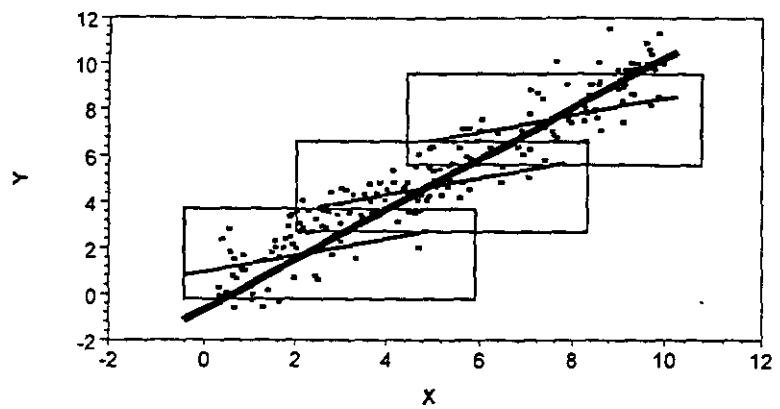
1. The Fixed Effects Model vs. a Group Means Model

For the purpose of this discussion, I will focus on Dr. Bozzo's model and the criticisms raised by Drs. Neels and Smith in this proceeding. Some of the discussion would apply to the earlier results, but it seems more useful to concentrate on the current efforts. Dr. Bozzo estimated a labor demand equation of the form:

$$\log HRS_{it} = \alpha_i + \Sigma_k \beta_k z_{itk} + \varepsilon_{it}$$

where α_i is a site specific constant and the remaining terms are the components of a conventional regression (though one that involves linear, quadratic, and cross terms in logs of the independent variables, time effects, lags of the output variable, and autocorrelation, which make it quite complicated). A central issue concerns the use of the fixed effects regression model.

Dr. Smith takes issue with the fixed effects approach altogether. TR. 27/13163–65, 13189–90, 13207–214. He argues, through the devices of a graphical and logical demonstration, that the fixed effects model is inappropriate, and that the appropriate model is a regression on means that does not have fixed site effects. To focus ideas, I use a caricature of his main figure, his Figure 4 (TR. 27/13210). I assume he would agree with this approximation to his depiction.



The three boxes in the figure represent the observed data for three sites. The line of best fit that passes through each box is that produced by a fixed effects regression. But, so Smith's argument goes, each of these is a short run regression, and the long run regression is the one that passes through the center of the data, which is the heavy line in the figure, and which has a slope much greater than the individual lines. The logic behind the demonstration is that the data inside the box embody a short run relationship that the site obeys, but in the long run the site obeys the steeper sloped long run relationship.

Missing from this demonstration is just when the short run becomes the long run. At some point, so the argument goes, the site in the lower box is transformed to the one in the middle box, as it is then that it climbs up this curve, and obeys the long run relationship. The problem with this discussion is that within each box (at least figuratively—in actuality within the MODS data) the sites' operations are observed for several years. What Smith characterizes as the "long run" regression relationship certainly should manifest itself at some point. Dr. Smith claims that the fixed effects model assumes that the capital stock is constant within a site for the full period of the observations, but this is not

true.[20] Dr. Bozzo's model contains both a measure of the site's capital stock in each period (imperfect though it may be, it does not appear to be devoid of information) and a time trend. There is evolution of the technology built into the model.

Now, let's consider this argument statistically. Dr. Smith argues that the appropriate model is the group means regression. Let's suppose that it is. We'll make the assumption that the site means regression that Dr. Smith advocates is correct. That means that the linear regression model using the site means,

$$\bar{y}_i = \alpha + \beta \bar{x}_i + \bar{\varepsilon}_i$$

is appropriate. Suppose it is. Then it must be true that the disaggregated data obey the same regression:

$$y_{it} = \alpha + \beta x_{it} + \varepsilon_{it}.$$

Note the common constant term. It is there of necessity, because if this were not the case, then the group means regression suggested cannot be right. The only way there can be a single constant term in the group means regression is if there is a single constant term in the disaggregated data. Something is wrong here. Surely the data would have something to say about this. If the group means regression were appropriate, then when the fixed effects regression is fit, the site specific constants would all be the same, at least statistically so. But this is decidedly not the case. All the tests of this hypothesis decisively reject the hypothesis of no site effects.[21] The upshot is that it must be the group means

[20] TR. 27/13190–92.
[21] See USPS–T–15 at 123; see also Docket No. R97–1, USPS–T–14 at 41–43, Tr. 33/18021–22.

regression, which inappropriately restricts the regression model, that is biased—not the fixed-effects model.

There is another point of view here. If Dr. Smith is correct, then surely his group means regression could be fit using the disaggregated data. Disaggregating the data always relaxes restrictions, in this case, the restriction of equal constant terms for each site. It is a fundamental principle of statistics that when one relaxes a restriction, the very worst one can do is what one did before. Here, what this means is that if we don't assume the constants are the same, and they really are, then the regression on the disaggregated data should do no worse than the group means regression, and the site specific constants should resemble each other. Otherwise, the restrictions will appear to be incorrect. In fact, the MODS data are speaking loudly and clearly here. Statistically, Dr. Smith's argument in favor of the group means regression is not correct. Logically, it is weak as well, but whether that is the case or not, his graphical device cannot be used to support it, and his statistical interpretation is incorrect.

I would like to make one additional point at this juncture. The term "between" regression has been used at several points in the discussion, and it has been argued that the "between" regression (using group means) is more appropriate than the fixed effects model. The preceding addresses the site means issue. But it should be noted that there is an important distinction between the group means regression and the "between groups" regression. The fixed effects model is the 'within groups' regression. The "between groups" regression is a weighted regression of the deviation of site means of the dependent variable from the overall mean on the same transformation of the independent variables, *without a constant term*, and weighted by the number of observations made for each site. It is easy to show—it is done on page 619 in my aforementioned text, for example—that the same regression model applies to

this data set as to the original data set. But this is not the group means regression that Dr. Smith has suggested. Dr. Smith's group means regression involves a simple regression of site means on site means, with a single constant term. This regression is only appropriate if the original model with no site specific effects is correct. Otherwise, the between groups estimator and the within groups estimator both estimate the same parameters, while Dr. Smith's regression produces results that are biased.

The preceding has addressed some econometric fine points. There is a substantive conclusion. Dr. Smith has advocated the group (site) means regression, with means constructed from the underlying data used to fit the fixed effects model, as somehow superior to the fixed effects model. Logically, this makes little sense. Statistically, it is simply incorrect. It is the group means regression which imposes the improper restriction, not the fixed effects regression. A fortiori, if Dr. Smith were correct about the means regression, then the worst the fixed effects model could do would be to mimic it. The fact that it does not is strong evidence that the assumption underlying the means regression is incorrect. His statement that the "between model" is the least bad model available is not correct either, even if he had fit the appropriate between groups regression.

2. Measurement Error

Dr. Neels has suggested that aggregating the data to group means helps to ameliorate the measurement error problem.[22] The logic is that averaging tends to average out the measurement error. It's an intriguing argument, and would be a very useful one if it were true. Unfortunately, it is not. Once again,

[22] Docket No. R97–1, TR. 28/15626-15630.

the statistical rebuttal is simple. It is true that in the averaged data, the variance of the measurement error is now divided by the number of observations. However, the variance of the mean of the systematic component is likewise. That leaves the crucial reliability ratio that I discussed earlier unchanged. If there is measurement error, it will exert the same influence on a model fit with site means as it would on the underlying disaggregated data.

3. A Pooled Time Series Regression

Dr. Neels has offered a pooled, aggregate yearly time series regression based on system wide costs and volumes as an appropriate tool for analyzing volume variability.[23] If it could be argued that there were no systematic variation in volume variability factors across sites or activities, no adjustment within calendar years in response to changes in output, and no long run adjustment in response to technological advances, this might be appropriate. None of these assumptions seems warranted. And whether they are or not, assuming them at the outset discards nearly all the useful information to be had from the disaggregated data set.

The (lack of) usefulness of the time series regression suggested is the same as that for the group means regression. Once again, the statistical result is clear. If it were appropriate to aggregate the data—in this case, that would mean no site specific and no period specific effects—then the aggregate and the disaggregated approaches would produce similar estimates of the same parameters. The disaggregated approach cannot make things worse. When they differ substantially, as they do here, the right conclusion to draw is that the

[23] TR. 27/12835–12843.

aggregated approach is causing the problem. To reiterate, the disaggregated data will give the right answer whether or not Dr. Neels' approach is correct.

D. Alternative Estimates Based on a Reverse Regression

In a demonstration intended to show that piece handlings rise faster than volume, Dr. Neels presents a set of results using a technique known as reverse regression (TR. 27/12805–12810). This technique originated roughly two decades ago in the sex discrimination literature. (See *Journal of Business and Economic Statistics*, (April 1984) in a symposium in which I have a contributed paper.) The logic of the technique is suggested by the following: In a regression of wages on qualifications, job characteristics, a dummy variable for sex, and other control variables, if there is discrimination, the coefficient on the dummy variable will be positive and the coefficient on qualifications will be too small. If so, then in a regression of qualifications on wages, the sex dummy, and the other variables, the coefficient on the dummy variable will be too high. In essence, if women are underpaid, then they are overqualified for the jobs they have.

Dr. Neels has extended this methodology, using an alternative approach to regression in the presence of measurement error. In particular, he states, "[t]o avoid the pitfalls of errors-in-variables bias, I estimate the elasticity of TPH/F with respect to FHP using the reverse regression of FHP on TPH/F and other variables..." TR. 27/12806. Then, to obtain the desired result, he takes the reciprocal of the elasticity of FHP with respect to TPH/F derived from the reverse regression. Id. The reasoning appears to be that in reversing the dependent and independent variables, he can pile the measurement error into the equation error on the right hand side and mitigate the measurement error bias that might affect the direct regression. Once again, he cites my text: "It is a well known result that measurement error in the dependent variable is absorbed in the error term and

can be ignored." Id. (footnote omitted). The quotation is right (well, close enough), but the regression result is not. The reason is that, even with the rearranged equation, the measurement error is still in the independent variable, and the estimator remains inconsistent, as I now show.

The prototype for the original regression is:

$$y = \beta x^* + \varepsilon$$
$$x = x^* + u$$

exactly as specified earlier. The thinking in Dr. Neels's reverse regression, apparently, is embodied in:

$$x^* = (1/\beta)y - (1/\beta)\varepsilon$$

so that:

$$x = x^* + u$$
$$= (1/\beta)y - (1/\beta)\varepsilon + u$$

which is a conventional regression which appears to obey the claim from my text that was quoted earlier. We just absorb u in the disturbance, compute the regression, then take the reciprocal of the coefficient. It doesn't work. Relying on conventional regression results, and to avoid technical details, I go directly to the result. The least squares slope estimator in the reverse regression of x on y is an estimator of the quantity

$$\delta = \mathrm{Cov}[x,y] / \mathrm{Var}[y].$$

We can derive this just by going back to the original specification;

$$\delta = \beta\lambda^2 / [\beta^2\lambda^2 + \sigma^2]$$

where σ^2 is the disturbance variance. Neels' estimator would be

$$b = 1/\delta$$

which estimates not β but

$$1/\delta = \beta[1 + \sigma^2/(\beta\lambda^2)].$$

Whether or not there is measurement error—indeed, independently of the measurement error—the Neels estimator will overestimate the true coefficient that he seeks. His elasticity estimates are biased upwards.

What went wrong? What went wrong is that this manipulation simply trades one misspecification for another. Looking back at the rearranged equation,

$$x = (1/\beta)y - (1/\beta)\varepsilon + u$$

what we find by manipulating it a bit is that the 'independent variable,' y, is correlated with the 'disturbance,' $-(1/\beta)\varepsilon+u$; the covariance is $-\sigma^2/\beta$. This violates another assumption of the regression model, and renders least squares estimates from the reverse regression inconsistent. In fact, the accepted result on reverse regression in the presence of measurement error is that the reverse and direct regression estimators bracket the correct result, which is what is shown above.

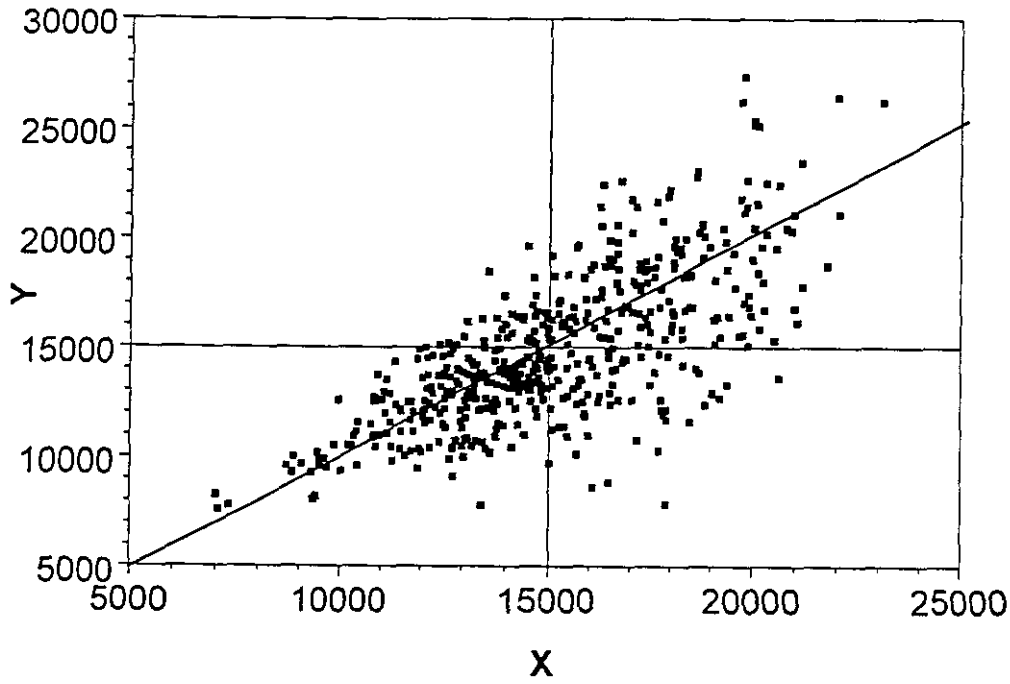
I hesitate to generalize from narrow results. The reverse regression estimator is generally going to be vastly more complicated than I have made it, because it is usually embedded in a multiple, not simple regression model, and at least in this case, we are not completely agreed on the nature of the measurement error in any event. I do believe, however, that a firm conclusion is safe here. Reverse regression is not an appropriate way of "avoiding the pitfalls

of errors-in-variables bias." This method and the estimates presented should not be accepted. Rather, if the problem is to be analyzed and solved, it should be done so directly.

E. Visually Compelling Plots

In both his 1997 and 2000 testimonies, witness Smith has relied on some low resolution figures generated by the SAS computer program to bolster his suggestion that the data and evidence in hand are consistent with 100 percent volume variability. Indeed, in the 1997 testimony he characterizes the figures as "visually compelling in demonstrating a variability approaching 100 percent between labor hours and mail volume."[24] Irrespective of any other evidence or results discussed in this proceeding, I would caution the Commission against relying on any visual devices of this sort. I do not believe that the eye is capable of resolving such evidence at a level approaching "compelling" or even convincing. I offer the figure below as motivation for this belief. The figure

[24] Docket No. R97-1, Tr. 28/15847.



contains a plot of 500 points that, as those in Dr. Smith's graphs do, certainly appear to be consistent with a 100% variability relationship. The solid 45 degree line plotted in the figure is intended to aid the eye in reaching its conclusion. However, they are not consistent with such a relationship—by what appear to be the proportions of interest in this proceeding, not even closely. The points in the figure were cleanly produced with a random number generator so that the values of *X* are drawn from a normal distribution with a mean of 15,000 and a standard deviation of 3000, while *Y* was produced so as to be equal to 2000 + 0.85 times *X* plus a random normal draw with mean zero and standard deviation X/100. In the terminology of this proceeding, the data were constructed with an 85 percent volume variability. (The standard deviation is intended to produce the kind of fanning effect in the figure that is typical in Dr. Smith's figures. This feature will not produce any effect on the slope of an underlying relationship; it will only

produce systematic variation around that relationship.) In the context of the results I have reviewed in the various testimonies, the difference between 0.85 and 1.00 is substantial.

I realize that this little demonstration is simplistic. The data were carefully constructed so as to produce an impression, not to mimic any real outcome that an analyst might observe. The purpose is to suggest that visual devices such as this, which could be based on such real data, could be very misleading. I do not believe that one could rely on a visual inspection of such a figure as this, which is itself of considerably higher quality than Dr. Smith's, to draw a conclusion about a precise quantity such as the slope of a regression. Graphs such as those in Dr. Smith's testimony should not be substituted for careful statistical analysis, and should not be accepted as evidence that casts doubt on any such analysis.

CHAIRMAN GLEIMAN: Three parties have requested oral cross-examination. I don't know at this point whether they've requested oral examination on the rebuttal testimony or on the NOI-4 materials. But the parties can tell me, the Magazine Publishers of America, Office of the Consumer Advocate and the United Parcel Service. I understand that MPA is going to cross on the rebuttal testimony only.

MR. MCBRIDE: That is correct, Mr. Chairman.

CHAIRMAN GLEIMAN: And the other two parties, please?

MR. MCKEEVER: United Parcel Service will do some cross-examination on the rebuttal testimony only.

CHAIRMAN GLEIMAN: Okay.

MR. RICHARDSON: And the same with OCA. We will only be crossing on the rebuttal testimony.

CHAIRMAN GLEIMAN: Sounds good; at least I sort of kind of understand where we are.

Mr. McBride?

MR. MCBRIDE: Thank you, Mr. Chairman.

CROSS-EXAMINATION

BY MR. MCBRIDE:

Q Hello again, Professor Greene.

A Hello.

Q I know you haven't testified here before, so if there's anything about the process that you don't

understand, feel free to ask me.

CHAIRMAN GLEIMAN: At this stage of the game --

THE WITNESS: How it can go on as long as it has.

[Laughter.]

BY MR. MCBRIDE:

Q I'm not going to try any hidden tricks, in other words, here. I just want to ask you really I hope a relatively few number of questions, but I want to begin with your statement of qualifications on pages 1 and 2 of your testimony including the introduction of the purpose and scope.

A Okay.

Q I gather that what happened here was that because of Professor Bradley's testimony in the last case, R-97-1, and testimony that Dr. Bozzo has offered in this case; Dr. Neels and Dr. Smith among others, all of whom, apparently, at one point or another were citing your work. You are viewed now by the Postal Service as the person who is going to straighten all this out for us and tell us what's right and what's wrong about the work that each of those other fine gentlemen have submitted in these proceedings; is that correct?

A Apparently, yes.

Q Okay.

A That's a heavy load.

Q Well, I want to focus then just a little bit, if I may, on the work of Professor Bradley. And I'm going to look at your treatise or textbook -- how do you wish to refer to it? Econometric analysis.

A It's a textbook.

Q Well, okay.

A Well, not everybody views it that way, since it's a reference as well so --

Q All right.

A For most people, it's a textbook.

Q Well, without getting into too many of the details if I can possibly avoid it, I would simply like to ask you about a -- what I gather you experts refer to as a translog equation. Are you with me?

A Yes.

Q All right; and do I understand that you looked into this process or this type of analysis in your textbook, and you concluded that with respect to such an equation that analysts sometimes normalize the measured variables -- I'm on page 229 of your textbook --

A I realize that.

Q -- by dividing by their respective sample means. It turns out that the interesting elasticities in this model are unaffected by the normalization. Did I read that correctly?

A That is correct, yes.

Q You were here earlier, I think, when I examined Dr. Neels briefly about this same subject.

A I was, yes.

Q Yes; and he testifies, in turn, about Professor Bradley's use of --

MS. DUCHEK: If I could interrupt here for a minute, does this have to do with mail processing testimony or transportation testimony?

MR. MCBRIDE: It has to do with transportation.

MS. DUCHEK: Mr. Chairman, I object. I think this is outside the scope of this witness' testimony.

MR. MCBRIDE: Well, I thought he was blessing generally the approach of Professors Bradley and others, and that's why I've started with the beginning part of his testimony.

MS. DUCHEK: Professor Bradley's testimony in R-97-1, which concerned mail processing.

MR. MCBRIDE: And it has been repeated in this record, as I went through earlier this afternoon. He repeated some of that testimony in his testimony here.

CHAIRMAN GLEIMAN: I'm going to allow the cross to go on, and if the witness feels uncomfortable answering the questions because it's not something that he has paid attention to, then -- or is beyond the scope of his

testimony, then, he will let us know.

MR. MCBRIDE: Thank you, Mr. Chairman.

BY MR. MCBRIDE:

Q Is mean-centering an appropriate approach with respect to a translog equation?

A I'm not sure I'd use the word appropriate. It's a reasonable approach; it's a convenience. Researchers sometimes do it to provide a convenient form of the equation.

Q Is it the case, however, that the quote that I read that you agreed I read correctly from page 229 of your textbook suggests that it's not appropriate or it doesn't answer the question of what the appropriate elasticities would be when you're using a translog equation?

A I'm sorry; I don't know what you mean by it doesn't answer the question.

Q Well, if you're trying to calculate what the elasticities are, and you're using a translog model --

A Yes.

Q Are you with me?

A Yes.

Q Then, is it necessary to still determine -- solve for more than one coefficient and that you can't do that by mean-centering? And if I'm not stating that well, you're the author of the book.

A Right.

Q And I would ask you to explain it.

A Well, maybe I can straighten it.

Q Please.

A The coefficients in the model will be different, but they'll be different in such a way that the elasticities, which are not equal to the coefficients, will be the same. Different combinations of the coefficients in the two approaches will produce the same outcome.

Q Okay; and therefore, I take it mean-centering, if I understand what's going on here, doesn't change the results.

A In that regard, mean-centering is innocent.

Q Okay; so it didn't change anything for Professor Bradley to have used mean-centering in his approach.

A No.

Q Is that correct?

A And I wouldn't say it didn't change anything. It didn't change the elasticities --

Q Didn't change the --

A -- derived from the model.

Q Very good; thank you.

Is heteroscedasticity a concept with which you're familiar?

A Yes, it is.

Q Is that spelled with a C in place of a K these days, or it's still spelled with a K, or are those two different things?

A They aren't two different things. My editor insists I spell it with a C. There is a rather obscure article in Econometrica that argues it should be spelled with a K.

Q Okay.

A The obscurity of the article suggests it should be spelled with a C.

[Laughter.]

Q All right; so anybody trying to understand this record, though, would equate the two terms whether with a C or a K; is that correct?

A Well, I don't know if they would, but they should.

Q They should; all right.

In any event, Professor, I hesitate to state what I understand heteroscedasticity to be, but I take it it's a non-uniformity in the data with respect to the line that's been drawn through it. Is that about right?

A No.

Q No? Please explain it, then.

A It's a nonuniformity with respect to variation of the data around the line which has been drawn through it.

Q Very good.

A Or through them.

Q Is it something that is a property of a given set of data or a property of a model that is estimated on the given set of data?

A Heteroscedasticity is a property of the data.

Q Of the data?

A Yes.

Q Does it cause a bias in the values of the coefficients of a model?

A No; well, let me qualify that. It does not cause a bias in the coefficients of -- the linear coefficients of a model estimated by least squares.

Q Okay.

A It depends upon the model.

Q And when it's said that heteroscedasticity makes estimated coefficients inefficient, what does that mean to you?

A It's not the coefficients themselves that are inefficient; it's the way the analysts use the data that is inefficient. In the presence of heteroscedasticity, simple least squares is not necessarily the best way to use the data in hand.

Q Okay.

A And it's that sense in which it causes inefficiency. One can't characterize the coefficients

themselves as being efficient or inefficient. They are simply coefficients.

Q So let me see if I understand this. If a translog model, which includes a squared term and a cross-product, is the model with which one is working, and you're trying to solve for the coefficient of the first-order term -- are you with me so far?

A Yes.

Q Would it be appropriate to use mean-centered data or not?

A You just got away from me.

Q All right.

A In that sense, now, I don't know what you mean by the first order coefficient, because if one centers the data, the coefficient on the linear term would be different from the one you get if you don't center the data.

Q All right; but I was struggling to apply what we've discussed from page 229 of your textbook here, and are you missing something from me that you need to be able to apply the teaching there?

A I may be. I'm not sure --

Q Okay.

A -- where we're going with this.

Q All right; well, if we've got two variables -- Y as a measure of cost and X as a measure of volume --

A Yes.

Q -- and we take the natural logarithms of both and estimate an econometric model in which the log of Y is the dependent variable, and the log of X is the independent variable, is it correct that the elasticity of Y with respect to X, that is, cost with respect to volume, is simply the coefficient that is estimated for the log of X?

A Yes, that is correct.

Q All right; but now, let's assume we have a more complex econometric model, and the log of X also appears on the model in the squared terms and cross-product terms.

A With other variables which were not in the model before.

Q Right.

A Yes.

Q Okay; and this happens in a translog model; is that correct?

A Yes, it is correct.

Q Is elasticity still just the coefficient that is estimated for the log of the X term?

A No.

Q Can you confirm that in a translog model, the computation of the elasticity is a complicated function that involves more than one of the estimated coefficients?

A Definitely.

MR. MCBRIDE: Okay.

Thank you, Professor. That's all I have for now.

CHAIRMAN GLEIMAN: Office of the Consumer Advocate?

MR. RICHARDSON: Thank you, Mr. Chairman.

CROSS-EXAMINATION

BY MR. RICHARDSON:

Q Good evening, Dr. Greene.

A Good evening.

Q I'm Ken Richardson from the Office of the Consumer Advocate.

I would like you to first refer to your testimony, your rebuttal testimony on page 4 and specifically, line 18. The sentence starts with the phrase: "While I am not prepared to commit to any specific value for any activity." Do you see that?

A Yes, I do.

Q And you're referring to the specific volume variability for a particular activity in the MODS pool.

A That is correct.

Q And you indicate you've looked at the evidence in this case in preparing your testimony.

A I've looked at some evidence in this case, yes.

Q And you would agree there are several values of MODS activities for volume variability that have been

presented in this case.

A Yes, I would.

Q And would you agree that there has been a lot of disagreement as to the appropriate value for those MODS pools?

A Apparently so, yes.

Q And would you agree that a specific -- that a specific value for any of those activities has not been conclusively determined from this record?

A I have to say yes, because from what I've read, the testimonies that I was given, the value used historically is 1, and under discussion in this proceeding is a large number of alternatives derived from the MODS data.

Q Would you say that the appropriate volume variability for the activities is identifiable on this record?

A I don't think I can reasonably answer that question. I don't really have the expertise to say a definitive value for an elasticity is identifiable from the data in this record.

Q Is that because there are so many disagreements as to the appropriate approaches that it cannot be determined what is the appropriate volume variability in this case?

A As I read the testimony, and as I thought about

what I was being asked to discuss, it concerned methodology, not the specific values of volume variabilities. Do the data that were analyzed in these studies contain the specific value? I really couldn't say that.

Q I would like you to refer to the end of the sentence which I just referred to that appears, I guess, on line 19 of the same page which concludes: "I do believe that the two studies combined provide a strong suggestion that the right results will be substantially less than 1." Do you see that?

A Yes, I do.

Q And what do you mean there by substantially?

A Well, substantially has got to be in the eye of the beholder here, and as I read the discussion, it looked to me -- and again, this is only my opinion, because I don't know how this feeds into rates -- but a value of 0.9 or 0.85, based on what I was reading, sounded like it was substantially less than 1. And based on what I saw, it looked like that might well be the outcome, and that's what I meant by that statement.

Q I could refer you to your own testimony on page 38, where you do discuss this briefly.

A Yes.

Q At the top of the page, your sentence: "In the context of the results I have reviewed in the various

testimonies, the difference between 0.85 and 1.00 is substantial."

A Yes.

Q Would you also apply the description of substantial to a difference between 0.90 and 1.00?

A I don't know if I could do that, because again, the question is how this feeds into ratemaking and how costs are evaluated on this basis, and I really don't know. I don't have the expertise to say that.

Q But you believe 0.85 --

A Well, when I said that, it reflected my opinion based on the numbers I was seeing in a disagreement, that that looked like a substantial difference. But I really can't testify as to how this would turn out monetarily. I really don't have the expertise.

Q Are you aware that Dr. Bozzo proposes the variabilities for one of the MODS pools, the LSM MODS pool of 0.956?

A I can't testify to a specific value. I'll accept if you tell me he's done that, then --

Q That appears on his table 6, revised January 28 --

A Okay.

Q -- 2000.

A Okay; well, I'll take your word for that.

Q I believe that's the current variability.

A Okay.

Q And would you accept also that the variability for the BCS pool recommended by Dr. Bozzo is 0.897?

A Well, if you're reading to me from his testimony, then, yes, I accept that.

Q Okay.

A I accept that he said that.

Q And do you have any reason to believe that these two pools might be any different from the other pools for which he makes a recommendation as to --

A I really can't comment. I don't know whether these pools are or are not different from others. I don't have the expertise.

Q But the -- would you agree based on your other comments that the volume variability for those two pools is not substantially less than 1.00, because they are higher than --

A I would only agree that the numbers that you cited are not substantially less than 1. Whether those are the right volume variabilities for those remains to be settled, and I really don't know that.

Q Well, you testified that in your view, a variability of -- a variation between 0.85 and 1.00 is a substantial difference.

A That is an impression that I drew from my reading

of the testimonies in this case. Again, it's not a statement about how I believe these things feed into rates or the evaluation of costs and the like.

Q So you can't actually generalize from the model that the variabilities are all substantially less than 1 based on your analysis.

A Are they all substantially less than 1? That would require an expertise about certain specific activities in mail processing, and I really don't have that.

Q And would you agree that some are not substantially less than 1, at least the two that I have indicated for you?

A I can't agree to that. I really don't know.

Q Also on page 4 of your testimony, lines 23 to 25, you state: "I would not say at this juncture that every econometric or modeling issue that could possibly be addressed by Dr. Bradley or Dr. Bozzo has been addressed." Do you have in mind any issues which were not addressed?

A I have in mind one specific datum that caught my attention. That is that there is evidence in the things that I read -- it's from the Commission's opinion -- that gave a specific value, and I don't know whether this is right or wrong, but there's specific evidence about the extent of measurement error. Most cases that talk about measurement error do it with viewing the parameters needed

to resolve the issue as unknowns and largely unknowable. And yet, here was a case where there seemed to be some data.

Now, generally, if it were the case that there were specific data to be had on that issue, it would be useful to make use of it, and probably it could be.

Q And did you feel Dr. Bozzo should have addressed that issue?

A No, I do not.

Q Did Dr. Bozzo address capacity utilization in your view?

A I can't say. Again, I'm thinking of econometric methodology in particular.

Q And so, then, you wouldn't have any view as to whether the key cap variable that he has used to measure capital is performed either at the activity level or at the facility level.

A No, I really couldn't say.

Q Let's move to page 6 of your testimony on lines 2 to 3, and there you're discussing the MODS data were not created for the purpose for which they were used in this proceeding, and you continue on line 6 through 9, you say, I would suggest that the Commission take the view that researchers should extract from these data what useful information they contain, not go to great lengths to discredit the data and then discard them and the analysis

based on them.

Do you see that?

A Oh, yes.

Q Is it theoretically possible that the information a researcher of data who goes to great length to discredit the data might determine that the database is truly fatally flawed?

A It is theoretically possible.

Q And that if the researcher does not go to great lengths to discredit the data, as you say, isn't this the same as stating that you would be willing to accept the data as usable without a full investigation of the data for its suitability or accuracy?

A No, I wouldn't say that.

Q Would you believe particular attention should be taken to the accuracy of the data since it was never collected for the analysis to which it was used in this case?

A Given the purpose for which the data are to be used in this case, I think, it would behoove the researcher to look into the quality of the data and the suitability for the analysis.

Q On page 8 of your testimony, line 8, you list in a list of the terms of the -- or the variables the manual ratio on line 8, and you list the manual ratio as a

site-specific measure of one aspect of the technology at Site "i" and period "t." Do you see that?

A Yes, I do.

Q Now, could the manual ratio be affected by the mix of mail?

A I can't answer that. I'm sorry, I really don't have the expertise.

Q Could it be affected by mail dispatch policy?

A Again the same thing -- I really don't know.

Q And do you have any opinion as to whether it would be affected by equipment decisions?

A I do not have an opinion.

Q Or the volume of mail?

A I can't. I really don't have the expertise to answer these questions about a specific aspect of mail handling.

Q Or -- one last point -- operating decisions?

A No, I really do not.

Q On page 12 of your testimony, lines 3 to 4, --

A Yes.

Q -- as you just indicated, based on your testimony, is it fair to conclude that you don't have a background or knowledge about the specifics of the Postal Service operations?

A Yes, it is fair to conclude that.

Q And does it follow, then, that we may assume that you do not know whether any crucial variables relating to the Postal Service operations are missing from Dr. Bozzo's analysis?

A I could not say that. I would agree with your statement.

Q Okay. And likewise, may we assume that you do not know whether the proposed functional form and modeling approach is suitable for Dr. Bozzo's analysis?

A I wouldn't say that, no. That begins to reach into the realm of what's the right way to build a model more generally, not just for this particular activity, but more generally, and while I am not knowledgeable about how the Postal Service handles mail, I do have some knowledge of how models are built, and so I do think I could comment on that.

Q Isn't it essential that you have some underlying knowledge of the processes which you're modeling?

A To do what?

Q In order to analyze and correctly model an operation.

A It's essential to go into that exercise with some knowledge about how model building is done and what's the appropriate way to proceed, and I think there is a point at which the expertise of people like me enters that appropriately, and then there's a point at which people like

me join hands with people who have specific expertise about the process.

Q May we assume that you do not know how investment decisions, operating decisions, and management decisions at the Postal Service interact over both the short-run and the long-run?

A That's correct.

Q I'll refer you to page 29 of your testimony, at line 1.

A Yes.

Q And you state, "Dr. Bozzo's model contains both a measure of the site's capital stock in each period (imperfect though it may be, it does not appear to be devoid of information) and a time trend. There is evolution of the technology built into the model." End quote.

My question to you is, does the time trend provide some explanation of technological change?

A Generally, time trends are used for that purpose, so yes.

Q Could the time trend provide a measure of anything else?

A Well, the time trend, since it's there by itself, has to embody technical change and other things about the process that might evolve through time.

Q And could it measure things like management

efficiency, changes in operating procedures and the like?

A I think a change in operating procedure may well fall under the heading of technical change. Yes, it could.

Q And so the time trend measures a myriad of variables and trends?

A Yes. It has to.

MR. RICHARDSON: Those are all the questions I have, Mr. Chairman.

CHAIRMAN GLEIMAN: Is there any follow-up? Mr. McKeever.

CROSS EXAMINATION

BY MR. McKEEVER:

Q Dr. Greene, my name is John McKeever, I represent United Parcel Service.

CHAIRMAN GLEIMAN: I'm sorry, Mr. McKeever. It's the hour.

MR. McKEEVER: Same result, Mr. Chairman. It makes no difference. No harm, no foul.

CHAIRMAN GLEIMAN: Thank you.

BY MR. McKEEVER:

Q Do you prefer Dr. Greene or Professor Greene or --

A Either one. Whatever you're comfortable with.

Q Okay. Doctor comes more easily off my tongue, so I'll use that.

A That's fine.

Q In your testimony, you do discuss the time series regression presented by Dr. Neels.

A Yes.

Q Is it your understanding that Dr. Neels has recommended that the Commission should adopt the time series regression he presents as the definitive answer to the question of whether mail processing labor costs vary --

A I'm really not prepared to state what his intent in doing that was. My commentary was merely on the validity of that as a model --

Q Okay.

A -- using those data.

Q Okay. Could you turn to page 8 of your testimony, please.

A Okay.

Q And as Mr. Richardson indicated there, you provide certain definitions of certain terms.

A Yes.

Q For example, HRS you define as hours of labor at Site I in Period T.

A Right.

Q Do you know, and I'm not sure if you have the level of knowledge to be able to respond to this, but do you know if that definition would more accurately be stated as

hours of labor at cost pool J at Site I and Period T?

A I can't say.

Q Okay.

A I don't know.

Q Okay. You used the term there total pieces handled at Site I and Period T. Do you see that?

A I do.

Q Are you able to give me a definition of that term, total pieces handled?

A Not at all. I cannot.

Q Have you heard the term total piece handlings?

A Perhaps that is what I should have said there. I really don't know. I had in mind here a generic statement of what I thought Dr. Bradley was stating in his testimony.

Q Okay. What was your understanding of what he was stating?

A Pieces handled struck me, and maybe I misread it or maybe it was simply my understanding at the time, that that was the output or volume variable that he intended to use, and that's what I meant by that. It's not a specific statement, a technical statement about the process of mail handling, and I have no expertise here.

Q Well, let me just ask you one more question along that line, then.

A Sure.

Q Do you understand that to mean the number -- the total number of pieces of mail that are handled or the total number of times that those pieces of mail are handled?

A I'm sorry, I just can't answer that.

Q Okay.

A I really don't have the knowledge.

Q Okay. Did you look at the data set at all?

A No, I did not.

Q Okay. Bear with me as I skip over a lot of questions I had.

A Absolutely.

[Laughter.]

BY MR. McKEEVER:

Q Could you turn to page 24, please.

A Sure.

Q And I would like to direct your attention there in particular to lines 3 to 11, your paragraph numbered 4.

A Yes.

Q And I will ask you to read that, but here is my question so that when you read that you will have it in mind.

You use a phrase there on line 7. You say "this result applies here."

I just got confused when I got to that point and so if you could read that and let me know what result

applies.

A I'm sorry, where are we here?

Q On page 24.

A I am there.

Q Line 7.

A Line 7? "This result applies here."

Q Yes, and by the time I got down to "This result applies here" I wasn't sure what you meant --

A Okay.

Q So if you could read that paragraph --

A Well, actually, the result that applies here is all bets are off. That is to say, in a model in which there is a variable measured with error but it is a multiple regression and there are other variables in that equation that are correlated with the variable that is measured with error, then the results are splashed all over the place and nobody has ever successfully worked out what the directions and biases are.

Q Okay, thank you very --

A That is what I meant by that statement.

Q Okay, thank you very much.

Now could I ask you to turn to page 31, please?

A Yes.

Q And at the bottom of 31 and top of 32 you have a discussion about measurement error in aggregating the data.

A Yes.

Q Are you saying there that aggregating the data does not help to ameliorate a measurement error problem if one exists?

A That is what that statement says, yes.

Q Okay, so if there is a measurement error problem, aggregating the data isn't going to get rid of it?

A That is correct.

Q Okay. Could I ask you to turn to page 34, please.

A Okay.

Q There at the top, lines 2 and 3, you say the reason is that even with the rearranged -- you are talking about the reverse regression here, correct?

A That is correct.

Q And you say the reason is that even with the rearranged equation the measurement error is still in the independent variable.

A Yes.

Q I have a similar question to the one I just asked. Does that mean reversing the regression does not solve a problem of measurement error?

A No, it does not.

Q It does not solve the problem of measurement error?

A It does not solve it.

Q Okay. I wasn't sure whether you meant it does not solve it or your sentence did not mean that.

A No, I meant to say it does not solve the problem.

Q Okay. Now could you turn to page 35 of your testimony, please?

A Sure. Okay.

Q And in particular I would like to direct your attention to lines 15 through 18, the sentence that begins, "In fact" --

A Yes.

Q You state there, "In fact, the accepted result on reverse regression in the presence of measurement error is that the reverse and direct regression estimators bracket" -- and you have that underlined -- "the correct result, which is what is shown above."

A Yes, that's what I say.

Q Now on that same page you indicate, and this up around lines 5 through 7, I guess, that the Neels estimator will overstate the true coefficient that he seeks.

Do you see that?

A Yes.

Q Now when you say that the reverse and direct regression estimators bracket the correct result, do you mean that the direct regression estimator understates the true coefficient in this measurement?

A In that prototype model, that is exactly correct. The direct estimator underestimates it. The reverse regression estimator overestimates it. It is tempting to average those two, but that would be incorrect. It doesn't bracket it symmetrically, it just brackets it.

Q Okay, so all you know is that the correct value is somewhere between the one and the other?

A Yes. That is correct.

MR. McKEEVER: That is all I have, Mr. Chairman.

CHAIRMAN GLEIMAN: Now I guess we are ready for follow-up. Mr. McBride?

MR. McBRIDE: Thank you, Mr. Chairman.

FURTHER CROSS EXAMINATION

BY MR. McBRIDE:

Q Professor Greene, I would just like to direct your attention back to page 24 of your testimony.

I wasn't sure I was following Mr. McKeever's questions about what appears in paragraph numbered 4 there, and your answers. Maybe it is the lateness of the hour or just my inadequacies but my impression was you were supporting the testimony of Drs. Bradley and Bozzo on the mail processing costs, but I wasn't sure if that is how the transcript would read.

Were you being critical of their testimony there in answer to Mr. McKeever or were you not being critical or

just what was it that you were attempting to say?

Maybe you could explain it for me.

A The intent of paragraph 4 on page 24 to address what really is a common misconception about measurement error.

I think if he were still alive I could convince Griliches that he made the same mistake in that article I was asked to read and that is the common belief is that when there is a measurement error it pushes all coefficients toward zero regardless of what else appears in the model and a lot of people believe that and it is a result that does riffle through the literature.

The fact is it is incorrect. If there is a single variable measured with error in a multiple regression model estimated by ordinary least squares the coefficient on that variable is pushed toward zero and the coefficients on other variables are pushed in unknown directions.

If more than one variable is measured with error, and essentially what I say here, all bets are off, the algebraic results are a horrible looking hash and nobody knows what directions the effects go. It's just never been worked out in detail.

There is a paper that actually follows up from the Griliches paper that I was asked to read by Professors Garber and Klepper, who tried to do this. It is about a

1980 vintage paper and their end result is a horrible hash. Really you can't work it out and that is what I meant by that.

It is a general comment about this issue of measurement error in an equation.

MR. McBRIDE: Thank you.

MR. McKEEVER: Mr. Chairman, I do have a follow-up.

FURTHER CROSS EXAMINATION

BY MR. McKEEVER:

Q Am I correct in understanding, Dr. Greene, then that your answer to Mr. McBride's question was that you were not either being critical or supportive of Dr. Bradley or Dr. Bozzo but rather were just generally addressing a question of measurement error?

A That is correct.

MR. McKEEVER: Okay. Thank you.

CHAIRMAN GLEIMAN: At this point we'll turn out the lights, all join hands and give the witness an opportunity to convince --

[Laughter.]

CHAIRMAN GLEIMAN: Any other follow-up?

[No response.]

CHAIRMAN GLEIMAN: Questions from the bench? Commissioner Omas?

COMMISSIONER OMAS: Yes. Dr. Greene, on page 25 of your testimony you say, and I quote, "In cross sections in which researchers attempt to measure such things as education, the reliability of self-reported data can be extremely low. In this setting by contrast we are considering very stable flow variables which evolve reasonably smoothly through time. I would strongly predict that the reliability of output data in this setting is extremely high. MODS data, MODS reports compile workloads, hours, and productivity data. As the Postal Service describes it, this data is collected and reported by the staff of each individual facility. Although it is used primarily for internal management of that facility the staff is aware that the data is reported, is compiled nationally and used by postal headquarters systemwide analysis."

Is this description of how MODS data is collected and used accurate? Would it give staff at a facility incentive to overreport its own productivity?

THE WITNESS: I'm sorry, I lost sight of the question. Are you suggesting that --

COMMISSIONER OMAS: Is this description which I just described --

THE WITNESS: Yes.

COMMISSIONER OMAS: -- how MODS data is collected and used accurate? Would it give staff at that facility,

postal facility, an incentive to overreport its own productivity results?

THE WITNESS: It might. If they all had the same incentive to over-report, that wouldn't hurt anything. Merely over-reporting, if everybody does it, doesn't affect this outcome. That's not the kind of measurement error that's causing a problem in this study.

COMMISSIONER OMAS: Did the Postal Service discuss with you the potential of self-reporting bias in the MODS data before you reached your conclusions concerning its reliability for econometric analysis?

THE WITNESS: No, I was not in any discussions about the particular dataset or the quality of the data. I drew my impressions from the testimonies that I read.

COMMISSIONER OMAS: If they had, would that have affected your conclusion?

THE WITNESS: I could have been convinced one way or another. I really don't know.

COMMISSIONER OMAS: One final question. Bear with me, these are a little long.

THE WITNESS: No problem.

COMMISSIONER OMAS: When you reached your conclusions concerning the reliability of MODS data, were you aware of the arguments that the Postal Service once made that MODS data could not be used to make reliable

comparisons of one mail processing with another? Specifically, were you aware that in R84-1, in the R84-1 rate case, the Postal Service was asked to provide MODS data to an intervenor who wanted to use it to analyze mail and processing variabilities. The Postal argued then that MODS data was not useful for this purpose.

I will quote from the response of the United States Postal Service in opposition to motion of the United States Postal Service for an Order to Compel filed February 4th, 1984 at page 17. There it said, "The MODS system is not designed to compare one installation with another, but it is intended to provide information on local relationships between workloads and actual versus planned work hours. Given the absence of uniform reporting, coding, editing and auditing procedures and the possibility of local variation in the data measurement, the only arguable meaningful comparison which can be made are between different years for the same office. Even then, it is best not to directly compare in results separated by more than a few since local practices change over time."

THE WITNESS: I have never seen that description before. It sounds reasonable to me. As you read it to me, the thought that comes to me is that that is exactly why, if I were going to be use those data, I would want to use a model that incorporated site-specific effects.

Now, the purpose wouldn't be to compare one site to another, but to compare the activities of a site across time. As I read your description, that's exactly why I would do that, or as I hear your description. I've never seen it before. I've never talked to anybody about that.

COMMISSIONER OMAS: Okay. Has the Postal Service told you of any specific measures that it has taken to improve the MODS data since it issued this warning not to use it to derive system-wide mail processing cost variabilities?

THE WITNESS: I haven't been told of such measures. I think it would be a very good thing to do that.

COMMISSIONER OMAS: And do you still feel as convinced the reliability of MODS data for econometric data is exceedingly high?

THE WITNESS: I drew that conclusion based upon other datasets that I have seen that are often drawn, for example, to study income and education effects, where you ask somebody how much education they have, they tell you how many years. The reliability of such data on how much education one has is very, very low. The Griliches paper that you distributed to me was actually part of the study they were doing of production functions. It was Ringsted's thesis.

He was studying capital data. Capital data are

notoriously erroneous, especially at the aggregate level, especially when you use stocks instead of flows. Those are low-quality data. These are data that I suspect are highly correlated with the actual outcome you're trying to measure, and that's why I drew that conclusion.

What's really important is the correlation between the measure that you have and the thing you're trying to measure, and in comparison to those other studies I'm familiar with, this seemed quite good.

COMMISSIONER OMAS: Thank you, Dr. Greene.

CHAIRMAN GLEIMAN: Anybody else?

Take it away, Commissioner LeBlanc.

MR. McBRIDE: I was just going to do my follow-up as quickly as I could.

COMMISSIONER LeBLANC: That's all right.

Dr. Greene, I think you touched on this a little bit with Mr. McKeever, but let me clarify the record, at least as best I can here.

You make a point in your testimony that the aggregated models are not justified if the true relationship manifests itself at a lower level because aggregation will discard information about the true relationship.

Is that kind of a fair summary of --

THE WITNESS: That's fair.

COMMISSIONER LeBLANC: Okay. If there are daily,

weekly and monthly peaks in mail processing workload and output, would it be reasonable to infer that the true relationship between handlings and hours manifests itself at that level?

THE WITNESS: Yes. A production function or the derivative demand function such as we're talking about here have a time dimension, and this is a flow, it's a flow per unit of time that we're talking about -- hours of labor or hours of -- amount of labor input per amount of output is a flow and it has a time dimension. If it varied through time in the way that you describe and if one wanted to be exceedingly precise at a very, very disaggregated level, that's in the time dimension that, yes, there could be variation across times of the day or times of the week or seasons of the year. There's no doubt about that.

COMMISSIONER LeBLANC: Then I guess the question I would have is, is Witness Bozzo's aggregation of daily MODS data into accounting period and quarterly forms likely to be discarding information about the relationship between handlings and hours and inducing bias in his variability estimates as a result?

THE WITNESS: Well, one has to be careful about what they mean by information. It's got to be in a context. My understanding was from these variability estimates that you wouldn't have any use for a daily variability estimate,

that you're looking for an aggregate measure in these studies. Again, I'm drawing this conclusion from the testimonies that I read, that if you could get accurate measures of volume variability by minute of the day, and it certainly theoretically exists, you couldn't use it, and so information about that is not useful to you.

So in that context, yes, you could discard information by going to a more time-aggregated level, month week or accounting period, but that's what you want.

Does it induce a bias, by the way? Not necessarily. That's a different question.

COMMISSIONER LeBLANC: That was what I was coming back to.

THE WITNESS: Yes. That doesn't necessarily induce a bias because you aggregate across time like that from the minute to minute or hour to hour level up to some other slightly higher -- or lower frequency like the month or the accounting period.

COMMISSIONER LeBLANC: Fine. Thank you very much.

CHAIRMAN GLEIMAN: Mr. McBride.

MR. McBRIDE: Thank you, Mr. Chairman.

FURTHER CROSS EXAMINATION

BY MR. McBRIDE:

Q Following up on both Commissioners' questions, Professor Greene, were you explaining why you thought the

MODS data might be useful notwithstanding, for example, what Commissioner Omas asked you about when you answered the questions of OCA about page 6 of your testimony where you said that one would have to look at the quality of the data and the suitability for the analysis?

In other words, were you testifying that it was appropriate to ask the question whether the MODS data was useful but that you felt that Dr. Bozzo had addressed that?

A It looked like it to me from my reading of his testimony, yes.

Q All right. And then I would like to ask you, if I may, just quickly about the article that you've referred to now that the Postal Rate Commission put out for all of us to take a look at yesterday. This is the economic data issues Journal article that you referred to. Do you have a copy of it there?

A I was asked to read two.

Q Yes.

A One is labelled Chapter 25, Economic Data Issues. I guess that's the one you just referred to; is that right?

Q Yes, that's right.

A Okay. That's a chapter from a lengthy book.

Q Okay. Well, I'm only going to ask you about one small part of it.

A I appreciate that. It's thousands of pages.

Q Right. My page is numbered 1507 under the heading Final Remarks.

A Okay.

Q They're referring to somebody, and forgive me for being ignorant about your field here, but whoever Morganstern was in 1950 was questioning the usefulness of economic data for econometric analysis --

A Morganstern was not an econometrician; he was a famous theorist.

Q Okay. Great.

In any event, I'm sure you're capable of responding to this.

I would like to first just set the predicate by asking you, are you aware from what you've read or is it at least your impression that data that we all have to work with here comes from the Postal Service?

A Apparently, yes.

Q So there were -- it was the conclusion of the author of this article that the Commission directed us to that one could have four responses to criticisms about economists and the data they were using and this sort of thing, and the fourth one reads: That is all there is. It is the only game in town and we have to make the best of it.

Does that seem like an appropriate response to the predicament we're in here when we're all working with --

A Absolutely. If you could put this in context, Griliches wrote this paper while he was working on a study of income and education with somebody named Mason, and that was the situation he was in. Income and education data are just horrible. And yes, sometimes -- usually I would think one is in that situation. Occasionally, a study is designed from ground zero and we go out to collect the specific data for the study, and then the data are appropriate for the thing we want to study. But most of the time, economists and observers such as yourselves are passive observers of a dataset that was generated in some other time period for some other purpose, and one culls from those data what one can.

Q And I take it the data might be very, very useful --

A Yes.

Q -- notwithstanding that it was collected for another purpose, and it all depends on what you conclude about whether it's suitable for the purpose for which it's being put --

A I will agree with that.

Q Okay. And I take it also by the way that although you testified that the number of years one spends getting an education and its correlation to the quality of education is rather low, that --

A No, I didn't say that. I said --

Q I'm sorry.

A I said the amount of education.

Q Amount.

A I went to school for 19 years, but I still go to classes.

Q All right. But I take it that where you go to school and the correlation of the quality of education is rather high, isn't it?

A Well, I would like to think so.

MR. McBRIDE: That was a joke, Mr. Chairman. We both went to the University of Wisconsin.

THE WITNESS: At the same time.

BY MR. McBRIDE:

Q And finally, under the heading of final remarks in that article, Professor, there is a proverb, a Russian proverb: The dogs bark, but the caravan keeps moving.

[Laughter.]

BY MR. McBRIDE:

Q If the caravan is the Postal Service, then who are the dogs who are barking here?

A Is that a question?

Q Forget it.

MR. McBRIDE: Thank you.

CHAIRMAN GLEIMAN: Mr. McKeever?

FURTHER CROSS EXAMINATION

BY MR. McKEEVER:

Q Dr. Greene, in response to a question from Commissioner Omas, you indicated that if all postal reporting personnel, MODS reporting personnel, had an incentive to over-report productivity, that would have no impact on the result, or something to that effect; do you recall that?

A Yes, I do.

Q Suppose some gave into that incentive but others didn't. Would that have an impact on the result?

A It would diminish my conclusion, but I was answering it would seem like a specific -- and it's a curious result because it's counter-intuitive, and that is that if everybody faced the same incentive and acted on it and the data were erroneous but always too high, that that would be fine. That would not --

Q But -- go ahead.

A -- impact the results. But the situation you suggested where the amount of response to that incentive varies, then that would, I think, reduce the impact of my statement.

Q That would create a problem?

A It conceivably could create a problem.

Q Okay. And I think you did testify before that you

have not examined the MODS data yourself.

A I have not.

Q Okay.

A I have not examined the data.

MR. McKEEVER: Thank you.

CHAIRMAN GLEIMAN: Anybody else?

[No response.]

CHAIRMAN GLEIMAN: Would you like some time to prepare for redirect on the rebuttal testimony of this witness?

Is that a yes?

MS. DUCHEK: Yes. Could we have -- five minutes would be fine.

CHAIRMAN GLEIMAN: Sure.

[Recess.]

CHAIRMAN GLEIMAN: Yes, ma'am.

MS. DUCHEK: Before we do redirect, Mr. Chairman, I'm a little bit confused. The Griliches articles were handed out and Dr. Greene and Dr. Bozzo both took a look at those and it was my understanding that there were going to be questions from the Commission on those, and I just wanted to clarify whether there were no questions for Dr. Greene from the Commission on those articles.

CHAIRMAN GLEIMAN: You wanted to clarify that there were no questions?

MS. DUCHEK: Well, I --

CHAIRMAN GLEIMAN: There are questions. I think they relate -- I can ask them now, if you would like.

MS. DUCHEK: You were going to ask them in response to NOI Number 4.

CHAIRMAN GLEIMAN: I was going to do them in the context of NOI Number 4, --

MS. DUCHEK: That's fine.

CHAIRMAN GLEIMAN: -- and I was going to ask them of all four witnesses who --

MS. DUCHEK: That's fine. Thank you.

CHAIRMAN GLEIMAN: Okay.

MS. DUCHEK: That clarifies.

CHAIRMAN GLEIMAN: All right.

MS. DUCHEK: Then I'm ready for redirect.

CHAIRMAN GLEIMAN: Was that one of your redirect questions?

MS. DUCHEK: Well, for you, but not for the witness.

REDIRECT EXAMINATION

BY MS. DUCHEK:

Q Dr. Greene, in response to questions from counsel for Dow Jones, Mr. McBride, he asked you some questions about Dr. Bradley's transportation models. Have you examined Dr. Bradley's transportation models in this or any

other proceeding?

A No, I haven't. I was answering generically.

Q So you don't have any idea of how the issue of mean centering specifically relates to Dr. Bradley's transportation models, is that correct?

A No, I don't. I do not.

MS. DUCHEK: Thank you very much. Now should I move in NOI Number 4?

CHAIRMAN GLEIMAN: Sounds about right to me.

BY MS. DUCHEK:

Q Dr. Greene, I am going to hand you two copies of a document entitled response of William H. Greene to Notice of Inquiry Number 4, Items (B) through (F) on behalf of United States Postal Service.

Are you familiar with that document?

A I am, yes.

Q Was it prepared by you or under your supervision?

A It was.

Q And if you were to testify orally today, would this still be your response?

A Yes, it would.

MS. DUCHEK: With that, Mr. Chairman, I will ask that the response of Dr. Greene to NOI Number 4, Items (B) through (F) be entered into evidence and transcribed into the record.

CHAIRMAN GLEIMAN: Is there an objection?

[No response.]

CHAIRMAN GLEIMAN: Hearing none, if you would provide two copies to the court reporter, I will direct that the material be received into evidence and transcribed into the record.

[Response of William H. Greene to NOI Number 4, Items (B) through (F), inclusive, were received into evidence and transcribed into the record.]

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

POSTAL RATE AND FEE CHANGES, 2000	Docket No. R2000–1

RESPONSE OF WILLIAM H. GREENE TO NOTICE OF INQUIRY NO. 4, ITEMS (b) through (f) ON BEHALF OF THE UNITED STATES POSTAL SERVICE

August 21, 2000

RESPONSE OF UNITED STATES POSTAL SERVICE WITNESS GREENE TO NOTICE OF INQUIRY NO. 4, ITEMS (b) through (f)

QUESTION:

(b) Parties are asked to indicate whether rejection of the hypotheses described in a) establish that Model A is statistically superior to the models nested within it, such as the "pooled" and the "random effects" models. Similarly, parties are asked to indicate whether the rejection of the hypotheses described in a) establish that Model B is statistically superior to the models nested within it, such as the "pooled" and the "random effects" models.

RESPONSE:

(b) Model A is a fixed effects linear regression model. The alternatives indicated are the linear random effects model and a pooled model with no site specific effects. The question first asks whether "rejection of the hypotheses described in a) establish that model A is statistically superior to the models nested within it, such as the "pooled" and "random effects" models."

The random effects model is not nested in model A. That is what necessitates the Hausman statistic which Dr. Bozzo used in his study rather than something more conventional such as an F statistic. As such, it is not possible sharply to answer this question. However, we can say that rejection of the pooled and random effects models by the standard tests (irrespective of the nesting issue) implies that both of them produce inconsistent estimators of the other parameters of the model. By this construction, which seems to be the overriding criterion in this case, the answer is "yes." A is superior because in this instance, model A provides consistent (lack of persistent bias) estimates of the parameters of the model while the alternatives do not. That is the implication of the rejection.

RESPONSE OF UNITED STATES POSTAL SERVICE WITNESS GREENE TO NOTICE OF INQUIRY NO. 4, ITEMS (b) through (f)

The question then asks "parties are asked to indicate whether rejection of the hypotheses in a) establish that Model B is statistically superior to the models nested within it.

The same issue about nesting applies. In addition, the question does not make clear whether the correct model to use as a yardstick for these tests is A or B. Assuming that B is the departure point, the exact same reply applies to B as to A in the previous reply. The issue of "statistically superior" still needs to made clear, but by the consistency rule above, the more general model is better. Model B is more general than the pooled and random (time) effects models. Both of these rejected models impose restrictions, and incorrect (rejected) restrictions produce biased and inconsistent parameter estimators.

RESPONSE OF UNITED STATES POSTAL SERVICE WITNESS GREENE TO NOTICE OF INQUIRY NO. 4, ITEMS (b) through (f)

QUESTION:

(c) Parties are asked to discuss whether Models A and B are nested within one another, and whether rejection of the hypotheses described in a) provide statistical grounds for preferring either of these models over the other.

RESPONSE:

(c) Models A and B are not nested within each other. Both are nested within a Model C which is

$$y_{it} = \beta_0 + \delta_i + \lambda_t + x_{it}\beta + \varepsilon_{it}$$

where the δ_is sum to zero and the λ_ts sum to zero -- this just shifts things so there is an overall constant and the time or site specific effects just show the difference from the overall constant. The term nesting as used in econometrics applies to the situation in which one model, the one which is nested within the other, can be obtained by restricting the parameters of the larger model. In this case, model A is obtained by assuming that λ_t equals zero for all t, while model B results if δ_i equals zero. However, no restriction on model A produces Model B, nor the reverse. The second part of this question asks whether "rejection of the hypotheses described in a) provide statistical grounds for preferring either of these models over the other." This question is a bit ambiguous. I interpret it to ask whether rejection of the random effects or the pooled model in the context of Model A provides a statistical basis for -preferring model B over A, and vice versa. The answer is no. Rejection of the hypotheses provides a statistical basis for preferring the model which was maintained. Thus, in the context of Model A, rejection of the pooled and random effects model provides a statistical basis for

preferring model A, and says nothing direct at all about model B. Indeed, it argues against B, since B would aggregate the site specific effects into a single constant, which is precisely the hypothesis that was rejected. The same argument applies in reverse if we depart from model B. The answer to this question is no.

RESPONSE OF UNITED STATES POSTAL SERVICE WITNESS GREENE TO NOTICE OF INQUIRY NO. 4, ITEMS (b) through (f)

QUESTION:

(d) Parties are asked to discuss whether witness Bozzo's rejection of the hypotheses applicable to Model A is sufficient to establish that Model (A) yields a valid estimate of β, which determines the magnitude of volume variability.

RESPONSE:

(d) The question asks whether witness Bozzo's rejection of the hypotheses applicable to model A is sufficient to establish that model A yields a valid estimate of β, which determines the degree of volume variability. This question contains, unfortunately, a subtle ambiguity. Model A is the most general of the three models suggested, in the sense that if the correct model is

$$y_{it} = \beta_0 + \delta_i + x_{it}\beta + \varepsilon_{it}$$

where β_0 is a common, overall constant while δ_i is a site specific constant, shifted in such a way that the average of the δ_is is zero, then the fixed effects formulation is robust in the sense that it will provide a "valid" estimate of β whether the fixed effects, the random effects, or the pooled model is actually the right model. The pooled estimator will only do so if $\delta_i = 0$ for all i while the random effects estimator will only do so if the values of δ_i are uncorrelated with x_{it}. But, the fixed effects estimator is consistent in all cases. The subtle ambiguity is that it has been assumed at the outset that the model above is already complete. If there is a $z_{it}\theta$ missing from the right hand side of the model, then the analyst might, ignoring this fact, carry out tests which would lead them to Model A, but, in fact, none of the three estimators is consistent in this case. The result here is that to answer the question, it must be agreed upon at the outset that model A as stated

is a complete model already. In point of fact, it seems very likely that for this case, the missing $z_{it}\theta$ would be the time effects discussed in the next question.

RESPONSE OF UNITED STATES POSTAL SERVICE WITNESS GREENE TO NOTICE OF INQUIRY NO. 4, ITEMS (b) through (f)

QUESTION:

(e) Parties are asked to discuss whether rejection of the hypotheses applicable to Model (B) is sufficient to establish that Model B yields a valid estimate of β, which determines the magnitude of volume variability.

RESPONSE:

(e) The answer to this question is the same as that to (d), but the argument is more compelling in this case. Considering the specifics of this case, rejecting the random and fixed effects models in the context of B would only be sufficient to validate the estimator of β in model B if it were agreed that there were no site specific effects missing from the model. Based on the empirical evidence presented, this seems very unlikely. So, once again, the answer is no, it is not sufficient.

RESPONSE OF UNITED STATES POSTAL SERVICE WITNESS GREENE TO NOTICE OF INQUIRY NO. 4, ITEMS (b) through (f)

QUESTION:

(f) Parties are asked to discuss whether, even with the rejection of the hypotheses described in a), there may be theoretical grounds for concluding that a rejected model could provide a better estimate of variability than either Model A or B.

RESPONSE:

(f) Are there theoretical reasons why a rejected model could provide a better estimate of variability than either model A or B? There is one way. Strictly in the narrow context of A or B, both the rejected models, pooled and random effects models, provide inconsistent estimators of the parameter in question, while the parent model provides a consistent estimator. However, in such a case, an analyst might weight the possibility that the inconsistent estimator is more precise in the sense of having a smaller variance than the consistent one. By this construction, the rejected estimator might be preferred. Intuitively, what this means is that the "accepted" (fixed effects) estimator is generally right on average, but has a moderately high probability of being wrong by a fairly large amount. At the same time, the rejected estimator is demonstrably wrong all the time, but not wrong by all that much. So, we trade a small amount of bias for a reduction in imprecision. This phenomenon is called, in fact, the "precision" of the estimator, and it is possible that the biased estimator could be more precise.

This type of tradeoff tends to be worth serious consideration in fairly small samples, and can be deduced when the test statistics that lead to rejection or nonrejection of the hypotheses tend to be borderline—for example, a t statistic for testing the hypothesis that a coefficient is zero comes out at 1.7. With respect

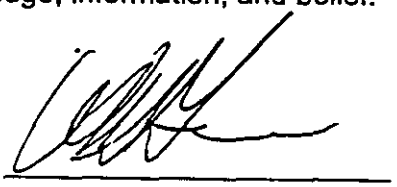
to this specific case, the samples are extremely large and the test statistics are huge. Based on the empirical evidence, I conclude, and recommend, that the possibility is not even close. The effect we would observe here, based on the huge test statistics, is that the rejected estimator is not biased by a small amount. It is off by a very large amount, and under this circumstance, questions of the possibly smaller variance are moot.

Could a rejected model provide a better estimate of a parameter than a maintained one? Yes. Could the pooled or random effects model provide a better estimate of the volume variability in this particular case than the fixed effects model? No.

A final conclusion, I feel that the questions raised in the context of models A and B in this NOI are too narrow. The appropriate model for the Commission to be considering is my model C.

DECLARATION

I, William H. Greene, declare under penalty of perjury that the foregoing is true and correct, to the best of my knowledge, information, and belief.



Dated: 8/21/00

CHAIRMAN GLEIMAN: Is there anyone that wishes to cross examine this witness on his NOI-4 responses?

[No response.]

CHAIRMAN GLEIMAN: I do have some questions for this witness, and as I indicated a moment ago, I am going to be asking the same questions of Witnesses Bozzo, Neels and Smith when they are on the stand with regard to NOI Number 4.

So what I would like to do, even though we handed out some materials yesterday and put them on the web page is provide one page, which I think is the most relevant page, from the much-maligned Z. Griliches --

THE WITNESS: It's Ger-rill'-ik-keys.

CHAIRMAN GLEIMAN: Griliches. I will take your word for it and I will try and just to refer to him as the author from here on in.

THE WITNESS: You could call him Zvi.

CHAIRMAN GLEIMAN: I could call him Zvi. I can get that one off of my tongue.

[Laughter.]

CHAIRMAN GLEIMAN: I would like to say I knew him and, you know, whatever follows after that, but I didn't, so I don't think I can call --

THE WITNESS: He's no Bill Greene.

CHAIRMAN GLEIMAN: We can't do it on a first name

basis.

I would like to hand out one page, which is page 1498.

I am also going to hand two copies of that page to the reporter since I am going to be asking questions relating to the page and ask that it be included in the record as a cross examination exhibit.

I have marked PRC/NOI-4-XE-1, and the reason I have marked it that way is because it is for a bunch of different witnesses and it relates to NOI-4, rather than putting it in each time for each witness.

[PRC/NOI-4-XE-1 was marked for identification, received into evidence and transcribed into the record.]

I have only two cautionary comments on this topic: As is true in many other contexts, and as was noted earlier, solving one problem may aggravate another. If there are two reasons for the z_{it}, e.g. both "fixed" effects and errors in variables, then

$$z_{it} = \alpha_i - \beta\varepsilon_{it}, \tag{6.6}$$

where α_i is the fixed individual effect and ε_{it} is the random uncorrelated over time error of measurement in x_{it}. In this type of model α_i causes an upward bias in the estimated β from pooled samples while ε_{it} results in a negative one. Going "within" not only eliminates α_i but also increases the second type of bias through the reduction of the signal to noise ratio. This is seen easiest in the simplest panel model where $T = 2$ and within is equivalent to first differencing. Undifferenced, an OLS estimate of β would yield

$$\operatorname{plim}(\hat{\beta}_T - \beta) = b_{\alpha,x} - \beta\lambda_T, \tag{6.7}$$

where $b_{\alpha,x}$ is the auxiliary regression coefficient in the projection of the α_i's on the x's, while $\lambda_T = \sigma_\varepsilon^2/\sigma_x^2$ is the error variance ratio in x. Going "within", on the other hand, would eliminate the first term and leave us with

$$\operatorname{plim}(\hat{\beta}_w - \beta) = -\beta\lambda_w = -\beta\lambda_T/(1-\rho), \tag{6.8}$$

where ρ is the first order serial correlation coefficient of the x's. A plausible example might have $\beta = 1$, $\beta_{\alpha,x} = 0.2$, $\lambda_T = 0.1$, and $\hat{\beta}_T = 1 + 0.2 - 0.1 = 1.1$. Now, as might not be unreasonable, if $\rho = 0.67$, then $\lambda_w = 0.3$ and $\hat{\beta}_w = 0.7$, which is more biased than was the case with the original $\hat{\beta}_T$.

This is not an idle comment. Much of the recent work on production function estimation using panel data (e.g. see Griliches–Mairesse, 1984) starts out worrying about fixed effects and simultaneity bias, goes within, and winds up with rather unsatisfactory results (implausible low coefficients). Similarly, the rather dramatic reductions in the schooling coefficient in earnings equations achieved by analyzing "within" family data for MZ twins is also quite likely the result of originally rather minor errors of measurement in the schooling variable (see Griliches, 1979 for more detail).

The other comment has to do with the unavailability of the "within" solution if the equation is intrinsically non-linear since, for example, the mean of $e^x + \varepsilon$ is not equal to $e^x + \bar{\varepsilon}$. This creates problems for models in which the dependent variables are outcomes of various non-linear probability processes. In special cases, it is possible to get around this problem by conditioning arguments. Chamberlain (1980) discusses the logit case while Hausman, Hall and Griliches (1984) show how conditioning on the sum of outcomes over the period as a whole

CHAIRMAN GLEIMAN: I am just going to ramble, roll out here a little bit and lay these on the table and anybody that wants to grab one may.

I take it you have a copy of that page? If not, I will hand you one.

THE WITNESS: No, I am looking at it right now.

CHAIRMAN GLEIMAN: Okay. At the top of the page our friend the author discusses an issue that witnesses are dealing with in all of the mail processing analyses we have been looking at, specifically the likely impact of the presence of both measurement error and fixed effects on the bias of the within estimators and the ordinary least square estimators.

Do you agree that the author's analysis may help us understand the impact that measurement error in a single regressor would have on bias in an econometric analysis of mail processing presented in this docket?

THE WITNESS: Yes. His results in the paper are correct as they stand, and if they help understand the issue, then yes.

CHAIRMAN GLEIMAN: Please briefly review the example presented in the middle of that page.

THE WITNESS: Want me to summarize it or --

CHAIRMAN GLEIMAN: The example seems to present a situation similar to what we have in the analysis of mail

processing operations. In the example is it assumed that the coefficient beta is one but a biased estimate, slightly greater than one results from econometric analyses using an ordinary least squares.

When the site-specific fixed effects model is used, an estimate with a downward bias results that is greater than the upward bias caused by omitting the fixed effects terms.

Could the empirical results obtained in this docket be reflecting the same tradeoff of upward and downward biases?

THE WITNESS: I would say in the broadest sense yes, they are not unrelated to them.

He has got two differences in his model that really do matter here.

The first of them, once again this is a multiple regression that you are analyzing in your proceedings and he's -- in order to get any kind of hard analytical result he is forced to resort to a very simple little model with one regressor.

The use of deviations for means to get a zero mean at the outset is an innocent normalization but then when he goes to the within transformation you learn something about the result at hand, so it is informative.

The other problem in this study is that, or in

this little example, is in order to get his result he had to assume two periods -- a panel as such but only two periods and there are certain hard results one can get if you assume that there are only two periods in the panel that go away when one gets past two periods, and the results for only two periods are quite drastic.

There is a dramatic example in Tsiao's textbook that is referred to. The book itself, his monograph, is referred to in various testimonies here.

He has got a dramatic result that is based on a panel with only two periods and these results are a whole lot less dramatic if you have lengthier time series, so you learn something here but I would be very cautious about drawing any conclusions from the specific numerical results that you see there.

CHAIRMAN GLEIMAN: The court reporter has asked you to spell the name of the author of the text that you just referred to, so I will ask you to do it now and save him --

THE WITNESS: The last name is H-S-I-A-O.

CHAIRMAN GLEIMAN: Thank you.

THE WITNESS: Hsaio.

CHAIRMAN GLEIMAN: Equation 6.7 --

THE WITNESS: Yes.

CHAIRMAN GLEIMAN: -- of the paper gives an

expression for the bias of the OLS estimator of beta.

THE WITNESS: Yes.

CHAIRMAN GLEIMAN: If beta were equal to 1, and the fixed effects and variable measured with the error were positively correlated, is it true that the bias in the OLS estimator could turn out to be zero, despite the presence of measuring error in the regressor X?

THE WITNESS: They could offset; yes; that's exactly the point he's making there.

CHAIRMAN GLEIMAN: Okay; in equation 6.8, it gives the expression for the bias of the within estimator.

THE WITNESS: Yes.

CHAIRMAN GLEIMAN: Is it true that if beta is equal to 1, and there is a measurement error in the regressor, then, the bias in the within estimator is always negative; in other words, will the within estimator tend to be a value less than 1 as the sample size grows?

THE WITNESS: In this context, yes, and again, you see, he needed to have a two-period panel to get that result. I can't assure you of that same result if there is a T greater than 2 in the panel. I couldn't say that. The intimation suggests it might hold up, but certainly, the result is going to be diminished as the panel gets longer.

CHAIRMAN GLEIMAN: The analysis provided by the author -- notice how I deftly avoided mispronouncing his

name that time --

[Laughter.]

CHAIRMAN GLEIMAN: -- involves error in one independent variable. In your comments in your rebuttal testimony on page 23, at lines 6 through 9, you note that the downward bias carries through to multiple variable models so long as only a single variable is measured with error. Would the caution that the author offers and the results he identifies apply to Dr. Bozzo's analysis if, one, THP has significant measurement error, and two, the other variables either have no measurement error or have little impact on the results?

THE WITNESS: I'm reluctant to draw that conclusion. In order to get that hard result, there's a lot of assumptions one has to make to get there, and I'm not going to hold -- in the other article that you haven't asked me about yet, for example, he makes a very special assumption about the lack of correlation between the other variables in the model and the one measured with error, and it's just not going to hold.

CHAIRMAN GLEIMAN: One last question: the error in the variable bias in nonlinear context papers --

THE WITNESS: Yes.

CHAIRMAN GLEIMAN: -- by the author and Ringsted --

THE WITNESS: It's in --

CHAIRMAN GLEIMAN: -- concludes, in short, errors in variable are bad enough in linear models. They are likely to be disastrous to any attempt to estimate additional nonlinearity or curvature parameters. In particular, the author shows that the squared term of the variable with measurement error is also downward biased in a simple linear regression model. Do you agree that this result could be applicable to Dr. Bozzo's model, which includes square of natural log of the TPH term?

THE WITNESS: Again, the result in this paper is a very, very special case. I doubt very much it applies to Dr. Bozzo's model, and I might mention there is another result in this paper which is of relevance here which he doesn't address, and, in fact, I think that his sentence, his last sentence about the disastrous results of the measurement error is not only overstated but irrelevant to what he's actually studying in the paper.

Again, I know something about what was going -- the research around this paper, and he was not interested in the coefficient on the squared term. What he was interested in was the elasticity of the substitution. He mentions the elasticity of the substitution in the paper. There's a formula for it. It is -- it's 1/1+rho. It appears somewhere in there, which is a function of the parameters

that are estimated. And moreover, the estimate of rho itself is a function of other parameters that are estimated, some biased upward, some biased downward.

So what he's saying in the last sentence is true as it stands with respect to the coefficient on the squared term in the equation. But it is simply not true with respect to the thing he was trying to fit in that model. And again, this piece of research is an offshoot -- actually, it was a book by Ringsted that was published a few years later where they were studying the model that you see on the second page there, on 370. The equation there was really the thing of interest, and the thing that they were studying was a very, very nonlinear hash of the parameters of that model, and the reason it relates here is because the elasticities that Dr. Bozzo was trying to fit were really functions of the parameters in the model.

So even if Griliches' result held up, and it doesn't because of the very heroic assumptions he makes, it's not the direct result that you're looking for. It's a very different result.

CHAIRMAN GLEIMAN: Thank you.

Any followup?

Yes, sir?

FURTHER CROSS-EXAMINATION

BY MR. MCBRIDE:

Q Very briefly, Professor Greene, I'd like to refer you to the last page of the Griliches article, page 1509, and see if you agree or disagree with this part of it. I'd like to start with footnote 25. Can you read it? If you can read it, just follow along for the part that I'd like to read to you: "An important issue not discussed in this chapter is the testing of models, which is a way of staying open and allowing the data to reject our stories about them."

Is what he's saying there, the point is that the data are the best we have and that we should try to determine whether they tell us something we haven't previously thought we knew?

A Well, one enters the model building exercise with a set of priors. If those priors were cast in concrete, there would be no need to analyze the data. So one gathers the data in the hope of learning something about the process you're studying. So it's hard to disagree with what he says there in footnote 25.

Q All right; and then, up to the text, the last couple of sentences: "The real challenge is to try to stay open; to learn from the data but also, at the same time not drown in the individual detail. We have to keep looking for the forest among all these trees." Do you agree with that?

A Absolutely.

MR. MCBRIDE: Thank you, Mr. Chairman.

CHAIRMAN GLEIMAN: So do we here at the Commission, in spades.

MR. MCBRIDE: Rare agreement.

CHAIRMAN GLEIMAN: No, frequent agreement.

Is there anyone else?

[No response.]

CHAIRMAN GLEIMAN: Would you like some time with your witness for redirect?

If not, Dr. Greene, that completes your testimony here today. We appreciate your appearance and your contributions to our record, and regarding your comment earlier about 19 years of school, but you're still learning, it's the same around here, and we were able to learn a little bit tonight, and we thank you.

THE WITNESS: Thank you very much.

CHAIRMAN GLEIMAN: And you're excused.

[Witness excused.]

CHAIRMAN GLEIMAN: Our next witness?

MS. DUCHEK: Postal Service calls Dr. Thomas Bozzo.

CHAIRMAN GLEIMAN: This one, I know. Dr. Bozzo is already under oath in this proceeding. He and Dr. Haldi have started to pay rent.

Whereupon,

A. THOMAS BOZZO

was recalled as a witness herein and, having being previously duly sworn, was examined further and testified as follows:

DIRECT EXAMINATION

BY MS. DUCHEK:

Q Dr. Bozzo, I'm going to hand you two copies of a document entitled rebuttal testimony of A. Thomas Bozzo on behalf of the United States Postal Service concerning mail processing volume variability, designated as USPS-RT-6. Are you familiar with that document?

A Yes, I am.

Q Was it prepared by you or under your supervision?

A It was.

Q Do you have any changes to make?

A I have one correction to make. On page 48, in footnote 15 at the bottom of the page, the section reference is incorrect due to a late repagination. What reads the Roman numeral XIII-A should instead read Roman numeral VII-F, and I have made these corrections in the copies that you presented me.

Q With those corrections, if you were to testify orally today, would this still be your testimony?

A It would.

MS. DUCHEK: Mr. Chairman, I'm going to hand two

copies of the rebuttal testimony of Dr. Bozzo, USPS-RT-6, to the reporter and ask that they be entered into evidence and transcribed into the record.

CHAIRMAN GLEIMAN: Without objection, it is so ordered.

[Rebuttal Testimony and Exhibits of A. Thomas Bozzo, USPS-RT-6, were received into evidence and transcribed into the record.]

USPS–RT–6

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

POSTAL RATE AND FEE CHANGES, 2000

Docket No. R2000–1

REBUTTAL TESTIMONY
OF
A. THOMAS BOZZO
ON BEHALF OF THE
UNITED STATES POSTAL SERVICE
(CONCERNING MAIL PROCESSING VOLUME-VARIABILITY)

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Autobiographical Sketch

My name is A. Thomas Bozzo. I am a Senior Economist with Christensen Associates, an economic research and consulting firm located in Madison, Wisconsin. My education and experience are described in detail in my direct testimony, USPS–T–15.

I. Purpose and Scope of Testimony.

The purpose of this testimony is to rebut criticisms of the Postal Service's econometric estimates of volume-variability factors for mail processing labor, and of the underlying economic theory and econometric methods, found in the testimonies of witnesses Neels (UPS–T–1) and Smith (OCA–T–4).

Associated with my testimony is Library Reference LR–I–457, which contains the background material for the analyses reported in this testimony. The accompanying CD-ROM contains electronic versions of the spreadsheets and programs used for the analyses presented herein.

II. Dr. Neels's aggregate time-series models yield Cost Segment 3.1 variabilities well below 100 percent when obvious flaws are corrected.

In this section of my testimony, I review Dr. Neels's aggregate time-series analysis, which he represents as "a conceptually superior alternative to the MODS-level analysis presented by Dr. Bozzo." Tr. 27/12835. As Dr. Greene indicates, Dr. Neels's conclusion that his aggregate time series model is "conceptually superior" is erroneous. USPS-RT-7 at 5. Among other flaws noted by Dr. Greene, Dr. Neels's aggregate time series model imposes a variety of restrictions on the response of costs to technological change and to variabilities at the site and activity levels which are not warranted *a priori*. Dr. Neels's analysis also discards most of the information in the underlying micro data. I concur with Dr. Greene, and by way of addition, note that Dr. Neels's time series analysis is materially identical to the simple regression models that the Postal

Service rejected as a basis for variabilities more than thirty years ago. A number of deficiencies of Dr. Neels's approach are already described in some detail in my direct testimony. USPS–T–15 at 9-12. Chief among these, as was noted by the Cost System Task Force back in the late 1960s, is the inability to identify and control for the effects of non-volume cost-causing factors. USPS–T–15 at 11.

Below I show that, notwithstanding the fundamental conceptual errors in his aggregate time series approach, Dr. Neels's quantitative results—estimates of cost segment 3.1 "volume variability" ranging from 98 percent to 123 percent in his Table 11 (Tr. 27/12840), and 109 percent to 119 percent in his Table 12 (Tr. 27/12842)—are artifacts of errors he committed when building his model. When these flaws are corrected, his models produce aggregate volume-variability estimates for Cost Segment 3.1 that are significantly less than 100 percent, results generally consistent with the results from my disaggregated models.

When performing aggregate time-series regression analysis, it is essential that the data used for estimation consist of observations on variables that are consistently defined throughout the sample period. If not, the analysis is effectively comparing apples and oranges, and produces nonsensical results.[1]

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[1] In time-series modeling the data are regarded as a single realization from an underlying data generating process that governs the values of the variables in each period. If the definition of a variable changes materially within the sample period, the process generating the observations that occur prior to the change cannot be said to apply to those that occur after it. It is therefore incumbent upon the analyst to either correct the data or incorporate the changed definition explicitly into his model. See, e.g., A.C. Harvey, *The Econometric Analysis of Time Series*, Phillip Allan 1981, at 14 et seq.

Dr. Neels estimates several variations on his time-series model using "aggregate, system-level [annual] time series data on volumes and mail processing [labor] costs." Tr. 27/12835. "The mail processing costs data for cost segments 3.1 (Mail Processing Clerks and Handlers), 2.1 (Mail Processing Supervisors), and 11.2 (Mail Processing Operating Equipment Maintenance) [were] taken from the Postal Service's response to Interrogatory UPS/USPS-T11-7-17, Tr. 21/9351-52." Tr. 27/12836. Dr. Neels's first error was failing to account for changes to the definition of Cost Segment 3.1 that occur during the sample period even though he is aware of these changes:

> I have reviewed the documentation on changes in the definition of Cost Segment 3.1 cited by the Postal Service in response to UPS/USPS-T11-8. Several changes in the definition have occurred. Because they do not appear to be of a significant nature, *I have not accounted explicitly for these changes.* Response to USPS/UPS-T1-14, Tr. 27/12940 (emphasis added).

In fact, Dr. Neels makes no effort to account for changes in the definition of Cost Segment 3.1 whatsoever. Furthermore, Dr. Neels was wrong to suppose that the definition of Cost Segment 3.1 does not change significantly during the sample period. In his data set, FY97 and FY98 Cost Segment 3.1 costs include the so-called "migrated" costs from Cost Segments 3.2 and 3.3, whereas the remaining cost observations do not. The implications for the measured segment 3.1 costs are not trivial. FY97 and FY98 segment 3.1 costs in the Postal Service's methodology are, respectively, $801 million and $570 million greater than the corresponding totals from the Commission's methodology, which continues the pre-Docket No. R97-1 definition. It is interesting that he should characterize the

change as "not... of a significant nature" since another UPS witness (witness Sellick) has, ostensibly in response to Dr. Neels's advocacy of the 100 percent variability assumption, opposed the redefinition of segment 3.1 in this proceeding and in Docket No. R97-1. Tr. 27/13126. It is all the more ironic as Dr. Neels has made something of a career out of criticizing Postal Service witnesses who, in his view, fail to adequately scrutinize their data sets.[2] In this case, Dr. Neels fails to perform even a modicum of quantitative analysis to justify his assumption that the changes to Cost Segment 3.1 were "not...of a significant nature." Response to USPS/UPS-T1-48(a) at Tr. 27/13009.

To correct Dr. Neels's mistake, I reran his aggregate time series regressions using a consistent definition of Cost Segment 3.1 costs. Since recasting years prior to FY96 using the Postal Service's Docket No. R97-1 method is difficult, I chose to use the PRC's definition of Cost Segment 3.1 as explained in the Docket No. R97-1 Opinion. PRC Op. R97-1, Vol. 1 at 93-95, 117-118, 126. As I show in Table 1, when a clean cost series is used, Dr. Neels's time series analysis produces lower variabilities than those he originally reported based on the inconsistently defined series.

A second error in Dr. Neels's analysis concerns the exclusion of FY79 and FY80 observations from his time series analysis. He excluded those observations because he claims there is uncertainty as to whether zero reported volumes for First-Class carrier route presort and Third Class 5-digit presort

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[2] In the present docket see, e.g., Tr. 27/12792, 12796-12802; in Docket No. R97-1 see, e.g., Tr. 28/15590-91, 15600-609, 15799-800.

represent "true zeroes" or reporting errors. Response to USPS/UPS–T1–47(d) at Tr. 27/13007. Dr. Neels's error in this instance is one of omission rather than commission. The rate history information provided in USPS-LR-I-118 clearly shows that the rate categories in question did not exist until FY81. Witness Fronk's testimony also references the FY81 introduction of carrier route presort discounts for First-Class Mail. USPS-T-33 at 13. Including the FY79 and FY80 observations in the time series regressions lowers the estimated variabilities by a few points.

The third, and most quantitatively significant, error in Dr. Neels's time series analysis is the underspecification of his model. Dr. Neels freely combines data from the Postal Service's automation and pre-automation eras, and neglects to include any variables to capture the effects of such patently non-volume factors as the network served by the Postal Service. Dr. Neels's justifications for this approach, that his omissions capture a truer picture of the effect of volume on costs, and that there are no likely omitted non-volume factors (Tr. 27/12938-9), are unsupportable on operational and statistical grounds. Omitting relevant variables from a regression leads to bias. Dr. Neels's own model does not follow what he himself calls "basic econometrics." Tr. 27/12939. Furthermore, Dr. Neels concedes elsewhere in his direct testimony that serving its network is costly to the Postal Service, so the argument that non-volume factors that affect costs do not exist strains credulity. Dr. Neels should have employed a more richly specified model.

One way of exploring the effects of the specification error is to split Dr. Neels's sample and reestimate his model. I have done this, and report the results below in Table 1. Splitting the sample has the effect of relaxing the assumption of Dr. Neels's time series model that the same cost relationship applies to all time periods, irrespective of the extent of the network served, the technology employed, and other factors. An obvious choice of the split point is between the period covered by the Postal Service's variability studies (FY88-FY98) and the previous period. This analysis allows for a better apples-to-apples comparison of results between Dr. Neels's time series models and the Postal Service's studies in my testimony and that of Dr. Bradley in Docket No. R97-1. The results from the split sample are remarkably different from those reported by Dr. Neels. The estimated variabilities obtained using the FY88-98 observations range from 67.5 to 84.8 percent, depending on the choice of worksharing parameter. These results are broadly consistent with the Postal Service's disaggregated models.

Dr. Neels expresses concern that there were too few observations to reliably estimate the variabilities in defending his failure to estimate his models over the time period studied by Dr. Bradley and myself. Tr. 27/13060. My analysis shows that this concern is unfounded, however, as the standard errors of the variabilities from this shorter time period are only a couple of percentage points higher than those obtained from the larger sample. The estimated variabilities using the FY88-FY98 observations are lower than 100 percent by a statistically significant amount. Nor is it the case that fitting the time series model

to the earlier observations shows that the pre-FY88 variabilities exceed 100 percent. There, too, the variability estimates are somewhat less than 100 percent.[3]

However, the purpose of this analysis is not to try to rehabilitate the aggregate time series analysis. Rather, it is simply to demonstrate that, when cast on an apples-to-apples basis, and using minimally appropriate data, the time series analysis fails to demonstrate 100 percent variability.

A final point concerns the nonlinear least squares model that Dr. Neels employs to validate the choice of worksharing parameter. While the variability estimate from this analysis is notably high—119 percent—the standard error of the estimate, 0.3, is also extremely high. As a result, not only is the 119 percent variability not significantly different from 100 percent, but at a 90 percent confidence level it is not statistically different from 70 percent. The standard error of the worksharing parameter estimate is also very large. The estimated value of 0.855 is not significantly different from any of the estimates Dr. Neels used for the analysis presented in Table 12 of UPS–T–1. Tr. 27/13064.

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[3] The high standard errors suggest that the simple time series model does a poor job of explaining segment 3.1 costs in the FY79-FY87 period. Again, this suggests the need for a more richly specified regression model than Dr. Neels's aggregate time series approach.

Dr.Neels's nonlinear least squares results are rendered useless by the high standard errors of the estimates.

Table 1.
Sensitivity of Dr. Neels's Time Series Analysis to Modeling Choices:
Estimated "Volume Variabilities" (Standard errors in parentheses)

Model	Neels, UPS-T-1 Table 11	FY79-FY98 Observations, Neels Data	FY79-FY98 Observations, Consistent Data	FY88-FY98 Observations, Consistent Data	FY79-FY87 Observations, Consistent Data
Worksharing parameter = 0.6	.979 (.068)	.930 (.057)	.880 (.053)	.675 (.076)	.781 (.189)
Worksharing parameter = 0.7	1.048 (.073)	1.001 (.061)	.948 (.056)	.748 (.079)	.843 (.199)
Worksharing parameter = 0.8	1.135 (.078)	1.092 (.065)	1.035 (.059)	.848 (.082)	.919 (.212)

Sources: Tr. 27/12840; USPS-LR-I-457.

III. Correcting obvious flaws in Dr. Neels's analysis of the relationship between TPH and FHP yields the operationally plausible result that the elasticity of TPH with respect to FHP is approximately unity, which supports my methodology.

In this section of my testimony, I review Dr. Neels's analysis of the relationship between TPH and FHP.[4] First, I discredit Dr. Neels's claim that I used TPH as an erroneous "proxy" for mail volume, an argument that was also refuted by Dr. Christensen in Docket No. R97-1. Then, I refute Dr. Neels's "reverse" regression analysis: the analysis itself is mishandled sufficiently that the results are meaningless; but even if he had not made hash of the analysis, Dr. Neels clearly has failed to grasp its meaning. Finally, the available evidence, while not conclusive, generally supports the result that the elasticity of TPH with respect to FHP is approximately unity, thereby supporting my methodology.

As he did in his R97-1 testimony, Dr. Neels continues to promote the canard that using piece handlings to estimate volume-variability factors for MODS mail processing labor costs constitutes an erroneous reliance on "a proxy for true [sic] volume." Tr. 27/12791-93, 12802; see also Docket No. R97-1, Tr. 28/15594-600. Under this theory, Neels seeks to estimate the elasticity of TPH with respect to FHP (that is, $\partial \ln(TPH)/\partial \ln(FHP)$) in order to "correct" my volume-variability estimates by a multiplicative factor. Tr. 27/12832; Tr. 27/12902-3.

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[4] In this section of my testimony, "TPH" should be read as "TPF or TPH, as appropriate."

The "volume proxy" issue is a red herring because, as Dr. Neels himself concedes in his testimony, I do not use piece handlings as a proxy for subclass volumes, but rather as an intermediate cost driver. Tr. 27/12802; see also USPS-T-15 at 52-53. Under the "cost driver/distribution key" (or, for short, "distribution key") approach to measuring volume-variable costs in mail processing, piece handlings are taken to be the "outputs" (cost drivers) of mail processing operations, not proxies for volume. The volume-variability factors, which are elasticities of hours with respect to piece handlings in an operation, are combined with distribution keys, which are estimates of the elasticities of piece handlings with respect to subclass (RPW) volumes, to form the elasticities of hours with respect to subclass volumes. USPS-T-15 at 52-56. The distribution key approach constitutes a feasible approach for estimating subclass volume-variable (or, when unitized, marginal) costs because it decomposes the relationship between cost and RPW volume, which cannot be directly estimated, into components that can be estimated. As I discuss in more detail below, the distribution key method is an economically appropriate method to estimate volume-variable costs for rate making.

Dr. Neels is unjustifiably selective in criticizing the application of the distribution key approach to mail processing costs. He finds that the distribution key approach is a reasonable method of measuring volume-variable costs in some contexts—he specifically mentions its use in analyzing Cost Segment 14, purchased highway transportation. Tr. 27/12802; Tr. 27/12999. However, he claims that it should not be used to analyze mail processing costs. Tr. 27/12804.

Dr. Neels is clearly inconsistent on this point: does he claim that cubic foot-miles, the cost driver in Cost Segment 14, is a valid "proxy for delivered volume"? Of course not: it is obviously not that, nor need it be. It is merely a cost driver, as is piece handlings.

Dr. Neels testifies that there are two key assumptions underlying the cost driver/distribution key methodology: the first is "that the cost driver captures the essential cost-causing characteristics of the various subclasses." Tr. 27/12802. The second "is that the cost driver changes in direct proportion to the volume of mail" – the so-called "proportionality" assumption. Tr. 27/12803. Regarding the first assumption, Neels offers no supportable objection to my argument that piece handlings is a valid cost driver in mail processing operations. Instead, he raises the red herring that piece handlings are a poor proxy for delivered mail volume. Tr. 27/12803. As I argued above, this feint is clearly an attempt to distract, since Neels knows that whether or not TPH is a good "proxy" for delivered mail volume is irrelevant and has no bearing on the necessity of estimating elasticities with respect to piece handlings. Dr. Neels's "corrections" are at best superfluous, and should be rejected. Nor is it a requirement of the distribution key approach that there be a single cost driver that captures all relevant characteristics. As Dr. Christensen demonstrated in Docket No. R97-1, the distribution key method can readily be generalized to accommodate multiple cost drivers. Docket No. R97-1, USPS–RT–7 at 6-7, Tr. 34/18222-3.

Nonetheless, without conceding the relevance of Dr. Neels's FHP-TPH analysis or the validity of the "corrections" he derives from it, his analysis of the

statistical relationship should be examined, since virtually every aspect of his analysis seems conceived to misstate or obfuscate the true relationship between TPH and FHP, let alone TPH and RPW volume. Dr. Neels attempts to investigate the statistical relationship between TPH and FHP "as a test of the 'proportionality assumption'" between piece handlings and mail volume. Response to USPS/UPS–T1–3(a) at Tr. 27/12899. However, the proportionality assumption concerns the relationship between TPH and RPW volume, not TPH and FHP volume. Dr. Neels's analysis, at best, simply substitutes one proportionality assumption for another—to be dispositive of the proportionality assumption for TPH and RPW volume, Dr. Neels's FHP analysis must assume proportionality of FHP and RPW volume. Tr. 27/13046-7. Furthermore, citing the Docket No. R97-1 bogeyman of FHP measurement error, he chooses a statistical method—reverse regression—for estimating the TPH-FHP relationship that, for reasons Dr. Greene discusses at some length in USPS-RT-7 at 23-24, would be expected to produce an upwardly biased result. Needless to say, an upwardly biased estimator makes it much easier for Dr. Neels to demonstrate the need for a disproportionality "correction" to the Postal Service's variabilities.

The FHP measurement error motivation for the reverse regression estimator is extremely weak. As Dr. Greene indicates, measurement error needs to be quite severe before even trivial attenuation of "direct" regression estimates would be expected to occur in the classic errors-in-variables model. USPS–RT–7 at 24-26. Accordingly, Dr. Neels should have at least tried to estimate the direct regression equation. But he did not estimate, or even specify, the direct

regression he purported to estimate. The surprising—and operationally implausible—result of "disproportionate increases in piece handlings [TPH or TPF]" (Tr. 27/12805) in response to an increase in FHP volume should be rejected as the erroneous progeny of Dr. Neels's inappropriate estimation procedures.

In what follows, I review Dr. Neels's handling of the problem of estimating the statistical relationship between FHP and TPH, highlighting the major errors he committed. Then I show that when these errors are corrected, the evidence supports the conclusion that the elasticity of TPH with respect to FHP is approximately unity.

Dr. Neels has testified that his purpose in performing his "reverse" regression analysis was to "estimate the elasticity of TPH/F with respect to FHP." Tr. 27/12806. In other words, he wanted to obtain consistent estimates of the parameters of the function relating TPH to FHP (and other relevant variables) and then use them to compute the elasticity, which is a function of the parameters. This implies that he had a model in mind of the regression function relating TPH to FHP and other relevant variables. However, Neels chose not to work with the direct regression of TPH on FHP because he believes that FHP is an error-ridden proxy for volume:

> FHP is known to be a very noisy measure of volume....To avoid the pitfalls of errors-in-variables bias, I estimated the elasticity of TPH/F with respect to FHP using the reverse regression of FHP on TPH/F and other variables....The reverse regression isolates the mismeasured variable FHP as the dependent variable. Tr. 27/12805-6.

Moreover, Neels never explicitly specified this "forward" or "direct" regression model, either in his testimony or in his interrogatory responses. Indeed, he twice refused direct requests to specify what it looked like. Tr. 27/12968, 13015-6. This refusal is telling: were he to have explicitly specified the forward model corresponding to his so-called "reverse" regression model, it would have made clear that his "reverse" regression specification was nonsensical.

Proper econometric practice demands that the analyst explicitly specify the forward regression model of interest, and then derive the reverse regression specification from it – this is the only way to know that the parameter or elasticity estimate obtained from the reverse regression bears any meaningful relationship to the desired statistic from the associated forward regression. If the reverse regression is specified in an ad hoc fashion, one runs the risk of seriously misspecifying the direct regression, which would then yield meaningless results.

This point is important because Dr. Neels claims to have derived an admissible estimate of an elasticity that would be appropriately defined in terms of the direct relationship between TPH and FHP without even specifying the relationship. Indeed, he specified his reverse regression in such a way that he is unable to say what the forward regression function looks like. Tr. 27/12968. He argues that the direct regression equation can only be defined implicitly (Tr. 27/12968), and provides some analysis that purports to show that his reverse regression elasticity formula is appropriate. His argument is entirely circular—change the specification of the reverse regression, and the result Dr. Neels reports at Tr. 27/12802 changes. See also Tr. 27/13055-6. The only logical

conclusion for his arguments is that they allow him to deflect attention away from the fact that one could easily specify, and estimate with reasonable accuracy, the direct regression relationship between TPH and FHP.

Let us therefore ask the basic question that Neels himself should have asked, but apparently never did: what is the relationship between TPH and FHP? Ironically, the information needed to specify a reasonable forward model is contained in Dr. Neels's own testimony and interrogatory responses. "A single piece of mail...will generate a unit increase in FHP volume at each of the processing plants through which it passes and in which it undergoes sortation." Tr. 27/12900. Continuing, "A piece handling, however, is generated each time a piece of mail at a specific site is processed in a particular sorting activity." Tr. 27/12803. Therefore, for a given site, the following identity holds:

(2) $TPH_{it} \equiv FHP_{it} \cdot HPP_{it}$

where HPP_{it} is the average handlings per piece for a given plant and time period. This identity expresses the truism that the total piece handlings in an operation (for a given plant and period) is the product of the number of pieces initially entering the operation and the number of handlings the average piece receives in that operation. This, then, is the fundamental relationship between TPH and FHP.

In logarithms, equation (2) is:

(3) $\ln TPH_{it} = \ln HPP_{it} + \ln FHP_{it}$.

From equation (3), it follows immediately that if handlings per piece are constant with respect to a change in FHP volume, there is "100 percent variability" of TPH with respect to FHP, that is:

(4) $\partial \ln TPH_{it} / \partial \ln FHP_{it} = 1.$

Equation (4) demonstrates that Dr. Neels's results require that handlings per piece must increase with volume, or:

$\partial \ln HPP_{it} / \partial \ln FHP_{it} > 0.$

To flesh this relationship out for statistical analysis, we need to expand the HPP term by understanding that it is a function of other variables, potentially including FHP. Additionally, HPP would be expected to depend on network characteristics, and a trend should be included to account for technical changes and other trend factors not elsewhere specified in the model. Therefore we can rewrite equation (2) as the following general function:

(5) $TPH_{it} \equiv FHP_{it} \cdot HPP(FHP_{it}, SITE_t, NETWORK_t, PERIOD_t)$

where HPP(·) indicates the function defining HPP. Discussion of the precise form and content of the SITE, NETWORK, and PERIOD terms is postponed for the moment. This equation is intended to apply at the shape level. At the operation level, it would be necessary to further complicate the relationship in order to relate TPH at the operation to FHP in all upstream operations where a given piece might have received its first distribution handling.

Taking natural logarithms of both sides of equation (5) yields:

(6) $$\ln TPH_{it} = \ln FHP_{it} + \ln[HPP(FHP_{it}, SITE_t, NETWORK_t, PERIOD_t)]$$

Equation (6) says that the logarithm of TPH is an unknown function of the logarithm of FHP as well as site and network characteristics and time period. Since the form of this function is unknown, current best econometric practice dictates that a fully flexible functional form (including interaction terms, which Dr. Neels inexplicably dropped from his regressions), with site fixed effects and either quarter dummies or a time trend, is the preferred specification for empirical work. I chose the translog form to expand the expression for ln(HPP), and the resulting direct estimating equation remarkably resembles the equation that Dr. Neels could not confirm represented the direct equation corresponding to his reverse regression. Response to USPS/UPS–T1–33(d) at Tr. 27/12968. The translog version of the direct regression model relating TPH to FHP is:

(7) $$\begin{aligned}\ln(TPH_{it}) = {} & \gamma_i + \gamma_1 \ln(FHP_{it}) + \gamma_{11}[\ln(FHP_{it})]^2 + \gamma_2 \ln(DPT_{it}) + \gamma_{22}[\ln(DPT_{it})]^2 \\ & + \gamma_3 t + \gamma_{33} t^2 \\ & + \gamma_{12}[\ln(FHP_{it}) \cdot \ln(DPT_{it})] + \gamma_{13}[\ln(FHP_{it}) \cdot t] \\ & + \gamma_{23}[\ln(DPT_{it}) \cdot t] + \mu_{it}\end{aligned}$$

where DPT is delivery points, t is a time trend, and μ is the direct regression disturbance.[5] The relevant elasticity from the direct regression is the marginal effect of FHP volume processed at a plant on the number of piece handlings at that plant:

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[5] Note that equation (7) includes a time trend rather than individual quarter dummies as Dr. Neels's model does. This was done primarily to simplify the

(8) $$\partial \ln(TPH)/\partial \ln(FHP) = \gamma_1 + 2\gamma_{11}\ln(FHP) + \gamma_{12}\ln(DPT) + \gamma_{13}t\,.$$

In the results I report below, I evaluate the elasticity formula given by equation (8) at the arithmetic sample mean values of the variables on the righthand side.

When equation (8) is compared with the expression that Dr. Neels derived from his reverse regression model using the implicit function theorem (Response to USPS/UPS-T1-52 at Tr. 27/13015), it is clear that they are quite different:

(9) $$\frac{d\ln(TPH)}{d\ln(FHP)} = \frac{1}{\beta_1 + 2\beta_2\ln(TPH)} \neq \gamma_1 + 2\gamma_{11}\ln(FHP) + \gamma_{12}\ln(DPT) + \gamma_{13}t$$

Note in particular that Dr. Neels's elasticity – the middle term in (9) – is a function of TPH, while the correct elasticity is a function of FHP and DPT. His claim that his result "is exactly the inverse of the marginal effect of TPH on FHP from the regression of FHP on TPH...presented in UPS-T-1" (Id.) may be true as a matter of purely abstract reasoning. But it obviously *is not the relevant elasticity derived from the correctly specified forward model shown in equations (7) and (8).* The obvious asymmetry between the elasticity derived from the direct regression and that which Dr. Neels derives from his reverse regression helps explain Dr. Neels's erroneous results.

Even ignoring the lack of correspondence between Dr. Neels's reverse regression specification and the properly specified forward regression shown in equation (7), as Dr. Greene describes in his testimony, Dr. Neels cannot claim

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specification of interactions between time and the other variables and should not be construed as a criticism of the time dummy approach, per se.

that his reverse regression result provides a consistent estimate of the elasticity he is seeking. The most he could reasonably claim to have found with his reverse regression estimates is an upper bound for the true (unknown) value. Tellingly, Neels makes no claims, in testimony or interrogatory responses, about the consistency or unbiasedness of his TPH/FHP elasticities. As Dr. Greene argues, this alone is good reason why Neels's "reverse" regression analysis, and the results in his Tables 6 and 7 that depend on it, should be rejected. USPS–RT–7 at 34-35.

I estimated equation (7) and the elasticities defined in equation (8) for the combined letter and flat shape operations, using the data I provided in LR-I-107 and LR-I-186. I employed the same panel data fixed effects estimator that Dr. Neels used, but did not impose an adjustment for AR(1) disturbances. The omission of the autocorrelation adjustment simplifies the programming somewhat; it does not bias the results. I report my results in Table 2. I did not attempt to estimate elasticities at the cost pool-level. To appropriately do so, as I stated above, it would be necessary to greatly complicate the TPH-FHP models to account for the fact that TPH in one cost pool may, and often will, appear as FHP in another cost pool.

The results in Table 2 contrast sharply with those presented by Dr. Neels. The direct regressions for the letter and flat shapes produce TPH-FHP elasticities between 0.92 and 0.95 for letters, and approximately 0.81 for flats, depending on which observations are used to evaluate the elasticity functions. These results cannot, however, be used as evidence on the proportionality assumption—the

decisive data for that purpose would be the elasticities of FHP with respect to subclass RPW volume, which cannot be estimated given the limited RPW volume data available.

What these data do suggest, however, is that the TPH-FHP relationship is not likely to be grossly different from a 100 percent variability relationship. There is no reason why Dr. Neels's misconceived reverse regression model should produce a reasonable upper bound on the TPH-FHP elasticity. Furthermore, the direct regression results, combined with Dr. Greene's theoretical exposition, strongly suggest that the true value of the elasticities are close to the direct regression results. Dr. Greene observes that an effect of measurement error would be to "bias the fit of the model downward." USPS-RT-7 at 25. But the direct TPH-FHP regression models, like many others based on my data set, exhibit very high values of the R^2 statistic. The FHP, generated through weight conversions, do an excellent job of explaining the variation in the mainly machine-counted TPH and TPF. The FHP data could not do so if they exhibited extreme measurement error of the sort Dr. Neels assumes. The evidence suggests that measurement error is not likely to be a major problem. Of course, without material measurement error, Dr. Neels's pretense for employing the reverse regression technique evaporates.

In summary, the evidence Dr. Neels provides purporting to overturn the "proportionality assumption" does nothing of the sort. Dr. Neels employed an inappropriate estimation method to produce a nonsolution to a nonproblem. The Commission should reject his analysis.

Table 2.
Direct regression estimates of TPH-FHP elasticities[1]

Shape	Letters	Flats
TPH-FHP Elasticity (evaluated with all observations)	0.950 (.015)	0.811 (.008)
TPH-FHP Elasticity (evaluated with FY98 observations)	0.920 (.016)	0.813 (.009)
Adjusted R-squared	0.991	0.995
Number of observations	5,603	4,980
Number of sites	303	276

[1]Elasticities evaluated using arithmetic mean method; standard errors in parentheses.

IV. Dr. Neels's shapes level models, though likely to be biased, support the conclusion that variabilities for mail processing operations are less than 100 percent.

Dr. Neels attempts to improve on the cost pool-level models of the relationship between hours and TPH/F by estimating models on data aggregated to the shapes level. Ostensibly, the purpose of the aggregation is to capture the effects of interactions among operations that Dr. Neels contends are ignored in the cost pool-level models, and to overcome supposed data errors along the lines of the "commingling" of manual parcel and SPBS data that Dr. Neels erroneously believes to occur. Tr. 27/12829.

However, Dr. Neels's shapes-level models fail to establish any indication of bias in the cost pool-level results. I reproduce Dr. Neels's shapes-level results, along with the corresponding cost pool-level results from USPS-T-15, in Table 3 below. For Dr. Neels, the would-be smoking gun appears to be the

result that his flat and parcel shape models yield higher elasticities than the corresponding cost pool level models. Tr. 27/12829-30. However, as Dr. Neels notes, the letter shape model yields a variability estimate over 17 percent lower than that which results from the cost pool model. Tr. 27/12831. Insofar as the letter shape cost pools are much larger than the combined flat and parcel shape cost pools, and the shape-level elasticity for flats is only approximately 7 percent higher than the composite cost pool-level value, the net effect of the aggregation to shape level is a composite variability of 73.1 percent for the pools covered by Dr. Neels's analysis—7 percent lower than the 78.6 percent composite that results from my cost pool-level models. Following Dr. Neels's logic, if my cost pool-level results are biased because of the interactions of operations and supposed data errors that motivate the shapes-level analysis, the net effect is actually a slight upward bias. Furthermore, even the higher flat and parcel shape elasticities estimated by Dr. Neels are still significantly lower than 100 percent, as is the 66.3 percent letter shape level variability. However, Dr. Neels's logic that the differences between the shape level and cost pool level models reflect biases in the cost pool level models is wrong. As with the aggregate time series and group means ("between") regressions, the problem is aggregation. The shapes level models are simply aggregates of the cost pool models. Tr. 27/12829. As Dr. Greene notes, aggregation imposes restrictions on the shape level models that are not present in the cost pool models. Then, if the restrictions of the shape level models are correct, and the disaggregation by cost pool really does not add anything to the model, the cost pool and the shapes level models should produce

the same results, at least statistically. But, as Dr. Neels points out, they do not. Tr. 27/12830. The correct conclusion to draw is that the shape level models impose inappropriate restrictions and that the results reported by Dr. Neels in Table 8 (Tr. 27/12832) are biased. Dr. Neels's interpretation is the opposite of the statistically correct conclusion and must be rejected.

As a final note, Dr. Neels's shape level analysis marks a major change from his Docket No. R97-1 testimony. In Docket No. R97-1, Dr. Neels argued, based on the results of the group means regression or "between" model, that it was not possible to exclude on statistical grounds the possibility that the TPH elasticities were equal to or greater than 100 percent. As I explain in my direct testimony—and Dr. Greene further explains in USPS–RT–7— Dr. Neels's assertions that the group means model is appropriate (and Dr. Smith's claim that the group means regression is "least bad") are based upon badly flawed statistical logic. USPS–T–15 at 122-124 and USPS–RT–7 at 30-31. In his current testimony, one statistical error Dr. Neels does *not* make is to attempt to rehabilitate his previous recommendation of the "between" model's results. The end result is that no econometrically defensible result on the record of this proceeding suggests anything other than that the elasticities of hours with respect to TPH are less than 100.

Table 3.
Effect on BY98 Volume-Variable Costs of Substituting Neels Shape-Level Variabilities
(without FHP adjustment) for Postal Service Variabilities

	(1) Pool Total Cost, BY98 ($000)	(2) Neels Variability	(3) Bozzo Variability	(4) Neels Shapes	(5) Neels Pool Variable Cost, ($000)	(6) Pool Variable Cost ($000)	(7) Neels Shape Variable Costs ($000)	(8) Difference ($000)	(9) Percent Difference
Letter Shape Cost Pools									
BCS	1,043,841	0.897	0.895		936,325	934,238			
LSM	78,765	0.956	0.954		75,299	75,142			
Manual Letters	1,563,964	0.737	0.735		1,152,641	1,149,514			
OCR	219,070	0.752	0.751		164,741	164,522			
Subtotal	2,905,640			0.663	2,329,007	2,323,415	1,926,439	-396,975	-17.1%
Flat Shape Cost Pools									
FSM	1,042,369	0.82	0.817		854,743	851,615			
Manual Flat	459,933	0.773	0.772		355,528	355,068			
Subtotal	1,502,302			0.857	1,210,271	1,206,684	1,287,473	80,789	6.7%
Parcel Shape Cost Pools									
Manual Parcel	60,593	0.522	0.522		31,630	31,630			
SPBS Non-Priority	283,275	0.645	0.653		182,712	184,979			
SPBS Priority	82,446	0.645	0.653		53,178	53,837			
Subtotal	426,314			0.75	267,520	270,445	319,736	49,290	18.2%
Total Composite[1]	4,834,256				3,806,797 78.7%	3,800,544 78.6%	3,533,648 73.1%	-266,896	-7.0%

[1]Composite is volume-variable cost as a percent of pool total cost for all reported pools.

Sources:
(1) USPS–T–17, Table 1
(2) Tr. 15/6386
(3) USPS–T–17, Table 1
(4) UPS–T–1, Table 8
(5) (1) * (2)
[6] USPST17, Table 1
[7] (4) * (1)
[8] (7) * (6)
[9] (8) * (6)

V. Dr. Neels's criticisms of the "distribution key" method, not to mention MODS cost pools, are fundamentally at odds with the findings of the Data Quality Study, and are especially ironic as the UPS mail processing cost method is transparently an application of the "distribution key" approach with 100 percent variabilities.

In this section of my testimony, I revisit Dr. Neels's criticisms of the "cost driver/distribution key" method of measuring volume-variable costs, as described in my testimony, in light of the findings of the Data Quality Study and the testimony of UPS witness Sellick. In his direct testimony Dr. Neels states that "[I]t would be even simpler for the Postal Service to dispense with the whole cost driver/distribution key approach and retain the traditional finding that mail processing labor costs are 100 percent volume variable." Tr. 27/12804. Elsewhere, he criticized my decision to "base [my] analysis on each MODS cost pool in isolation" rather than working with more highly aggregated data. Tr. 27/12793. These views put him squarely at odds with the conclusions of the recent Data Quality Study, jointly sponsored by the Postal Service, the GAO, and the Commission and, ironically, also with UPS witness Sellick, whose mail processing cost proposal is transparently an alternative application of the Postal Service's distribution key methodology using 100 percent variabilities. Response to USPS/UPS–T2–1 at Tr. 27/13133.

The authors of the Data Quality Study are generally quite favorably disposed towards the cost driver/distribution key approach. Moreover, they do not support the continued assumption of 100 percent volume variabilities for mail processing. For instance, in the section discussing cost attribution, they state

that measuring volume-variability factors as the proportional change in a cost pool with respect to a unit proportional change in a cost driver, far from being the suspect practice that Neels would have us believe, is in fact "logical" and "correct":

> The main economic issues arising from data quality problems in the determination of attributable costs are in the modeling of cost elasticities (or "volume variability factors" using Postal Service terminology). As noted in the VVC equation above, these cost elasticities are intended to measure the percentage change in an accrued cost pool in response to a given percentage change in the Cost Driver of the respective pool. *Logically, this is the correct approach.* (*Technical Report #1: Economic Analysis of Data Quality Issues* at 24, emphasis added.)

They go on to describe the Postal Service's method of measuring volume-variable costs as an "economically sound" approach, suitable for rate-making:

> The procedures adopted by the Postal Service of estimating forward-looking economic costs based on extrapolating the results of activity-based causal models of cost attribution is an economically sound starting point for identifying economic costs necessary for rate-making. (Id. at 27.)

They also are critical of assuming that mail processing costs are fully volume variable:

> The Docket No. R94-1 assumption of 100% volume variability for mail processing costs can be traced to Docket No. R71-1 documentation that is based on an analysis of 1953 to 1969 manual operations data. *It is more accurate to actively measure and calculate these elasticities than to continue to assume a 100% variability factor for all mail processing activities.* (*Summary Report* at 40, emphasis added.)[6]

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[6] As I described in my direct testimony, the 100 percent variability assumption has an even more tenuous link to statistical analysis than the Data Quality Study's authors suggest. USPS-T-15 at 128-130. Rather, the statistical analysis

While the authors of the Study note that criticisms of the cost driver/distribution key approach have been raised by intervenors in rate case testimony, they argue that the criticisms "pertain less to the theoretical structure of the Postal Service approach and more to issues of...implementation." Id. at 28, footnote 32. At no point in their study do they suggest that continuing to assume 100 percent volume variabilities for mail processing cost pools would be preferred to measuring the actual elasticities within each MODS cost pool.

The Data Quality Study also strongly supports the use of disaggregated cost pools in measuring volume-variable costs, since this corresponds to the theory of activity-based costing:

> The Postal Service uses an economically sound approach grounded in activity based concepts to determine its sub-class unit volume variable costs (UVVCs) on which Postal Rates are based. The categories of data collected and analyzed are sufficiently detailed and appropriate to arrive at the sub-class UVVCs. Id. at 32.

> As stated previously, the Study team believes the move to using MODS operational activity cost pools for mail processing costs is appropriate given the vast changes in mail processing operations over the past three decades. Id. at 123.

In short, the MODS cost pool approach is economically sound and an appropriate framework to deal with the "vast" and ongoing changes that have occurred in the organization of mail processing operations. Dr. Neels's criticisms of the cost pool/distribution key approach are empty and should be rejected.

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simply convinced the Postal Service's researchers to reject aggregate time series analyses – such as Dr. Neels's – as a basis for volume-variability.

VI. Dr. Neels's and Dr. Smith's criticisms of piece handling data for the manual operations are inapplicable to other MODS sorting operations.

MODS employs three distinct methods to measure piece handlings in sorting operations. For mechanized and automated operations, piece handlings are obtained directly from machine counts. Manual flat and letter piece handlings are derived from weight conversions and "downflows" from other operations. In manual parcel and Priority Mail operations, piece handlings are derived from manual piece counts and container conversions. Consequently, the quality of the MODS piece handlings data cannot be depicted with a broad brush. This has not stopped Drs. Neels and Smith, and Dr. Neels in particular, from attempting to cast doubt on the validity of the entire data set by focusing on a few allegedly egregious examples of data errors.

Dr. Neels, in particular, expended a great deal of effort seeking out possible reporting errors in the MODS piece handlings data.[7] Tr. 27/12797-12800. Neels would have us believe that each of these instances, which he documents in his Table 5 (Tr. 27/12799), is a data recording error. In fact, as I have stated previously, this is not necessarily correct.[8] Furthermore, he makes two subsequent arguments that are entirely unwarranted. First, he attempts to extend these alleged errors beyond the bounds of these two cost pools to other MODS operations. And second, he argues that the alleged errors he identified

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[7] See also Dr. Smith's comments at Tr. 27/13173.

[8] In response to oral cross-examination by UPS counsel, I indicated that the presence of a number of allegedly "suspicious" data gaps had far more prosaic and reasonable explanations. Tr. 15/6432-6436.

necessarily cause downward bias in my volume variability estimates. As I will show below, both arguments are specious and should be rejected.

As an example, let's consider the case to which he devoted the most space in his direct testimony, namely the one-year gap in piece handlings data for the Manual Parcels MODS operation group at a single site. Neels identified "positive piece handlings for Manual Parcels from the first quarter of 1993 to the first quarter of 1994, zero piece handlings from the second quarter of 1994 to the second quarter of 1995, and then positive piece handlings again." Tr. 27/12797-12798. For the sake of argument, suppose that he were entirely correct in saying that the zero TPF values for one year at site #6 all represent data recording errors. Even so, his analysis of this "error" is faulty.

First, Dr. Neels attempts to extend the presence of these alleged errors beyond the bounds of the Manual Parcels MODS cost pool to the SPBS cost pool. But there is no evidence on the record about data measurement errors in the SPBS piece handlings data, other than Neels's unsupported statements. To make his case, Dr. Neels concocted a theory that "Dr. Bozzo indicate[d] that the gaps in the data series correspond to periods where the *data* for the SPBS and Manual Parcels MODS activities were commingled and reported together as data for the SPBS MODS group." Tr. 27/12798 (emphasis added). This is false and misrepresents my comments. In oral cross-examination, I stated "that site [#6] *had handled manual and SPBS parcels together* up to a point prior to separating them according to the mail processing technology that was used to sort them." Tr. 15/6431. In other words, the commingling in question at site #6 represented

the physical commingling of parcels on site during sorting operations, not the commingling of data after the fact.

Even after Dr. Neels had had the error of his theory pointed out to him several times, he continued to cling to it uncritically. Response to USPS/UPS-T1-9, 45 at Tr. 27/12917, 13001-2. The reason for his tenacity seems clear: Dr. Neels doubtless believes that if he can convince the Commission that MODS data collectors are mixing together piece handlings data from manual cost pools with piece handlings data from automated and mechanized operations, then he can cast doubt on all of the MODS data—manual, mechanized, and automated—rather than only a single manual MODS operation at a single site.

Dr. Neels's theory that the SPBS and manual parcel piece handlings were "commingled" at site #6 (or elsewhere) is, quite simply, incorrect and inconsistent with MODS data collection procedures. SPBS is a mechanized sorting operation, and as with other mechanized and automated sorting operations, SPBS piece handlings are obtained from machine counts. Since a piece has to be handled on the SPBS to be counted in SPBS TPF, there is no way for pieces handled manually to enter the SPBS TPF count. By contrast, manual parcels (and Priority) volumes are manually logged. Tr. 15/6387. In fact, after many interrogatories and responses, Dr. Neels has conceded as much. Response to USPS/UPS-T1-45 at Tr. 27/13001.

Dr. Neels's second line of argument—that the presence of these alleged errors in Manual Parcels TPF led ineluctably to a downward bias in my econometric volume variability estimate for that cost pool because of

measurement error, is even more easily dismissed. One can do so in either one of two ways. The first is to refer to the relevant portions of Dr. Greene's testimony, in which he disproves the argument on theoretical grounds. USPS-RT-7 at 21-26. The second is even simpler: in making this argument, Dr. Neels is conveniently choosing to ignore my comments (Tr. 15/6388) indicating "that the manual parcels observations from this site [#6] do not enter the manual parcel regression sample," which makes this specific complaint completely irrelevant to my econometric results.

VII. General appraisal of Dr. Smith's testimony

In my direct testimony, I gave Dr. Smith substantial credit for his observations in Docket No. R97-1 that some aspects of Dr. Bradley's mail processing "cost equations" may have been inconsistent with standard economic cost theory. USPS–T–15 at 31. Addressing Dr. Smith's concerns motivated, in whole or in part, a number of important elements of my analysis, particularly the inclusion of additional variables in the models to ensure consistency with the applicable economic theory.

In his current testimony, Dr. Smith has manufactured a list of "fatal flaws" in my analysis as extensive, if not more, than his objections to Dr. Bradley's analysis. However, his objections to my study are devoid of substance. He offers nothing more than a convoluted mass of cosmetic gripes, misinterpretation of the testimony of several Postal Service witnesses (including myself), statistical

errors, faulty and self-contradictory (and sometimes flatly absurd) theoretical prescriptions, and—since Dr. Smith conducted no independent quantitative analysis of my data or models (see Response to USPS/OCA-T4-9, Tr. 27/13249)—entirely unfounded quantitative speculation about my econometric results. A summary of Dr. Smith's major arguments and the rebuttal follows.

VII.a. Cosmetic Gripes

A number of Dr. Smith's criticisms of my analysis are purely cosmetic, and therefore do not impeach my analysis. Dr. Smith objects to my interpretation of the variability models as "labor demand functions" (as opposed to Dr. Bradley's term of "cost equations"), claims I failed to provide the theoretical derivation of the models, and asserts that my presentation of the facility capital variable is unintelligible. Tr. 27/13167-8, 13180. The complaints are trivial and poorly founded.

Dr. Smith's claim that "we are faced with... cost functions that have become labor demand functions" (Tr. 27/13217-8) incorrectly characterizes both my testimony and Dr. Bradley's. Dr. Bradley garnered some criticism by calling his models "cost equations," which he specifically distinguished from cost functions. USPS-T-15 at 42. I maintain throughout my testimony that my regression models represent labor demand functions; the same would be an appropriate clarification of Dr. Bradley's "cost equation" terminology. There is no metamorphosis of the functions being estimated.

In this case, Dr. Smith argues not that the labor demand functions I estimate are inconsistent with economic cost theory, but rather that I did not explicitly perform the derivations. Tr. 27/13187. Ironically, Dr. Smith cites my response to OCA/USPS-T15-56, in which I explain (verbally) the economic motivation for my models. In that response (at Tr. 15/6358), I note that none other than Dr. Smith confirmed in Docket No. R97-1 the mathematical substance of the derivation of conditional factor demand functions from the cost function. Docket No. R97-1, Tr. 28/15909-10. In short, the mathematical foundation Dr. Smith contends was lacking had already entered the evidentiary record of the Docket No. R97-1 proceeding. To the extent I introduced new concepts, I provided detailed citations to authoritative sources in the economics literature. Dr. Smith does not claim that the derivation cannot be performed (Response to USPS/OCA-T4-7 at Tr. 27/13246) nor does he provide an alternative derivation that demonstrates any error. This critique is consequently without substance.

Dr. Smith's complaint that my "testimony does not discuss QICAP" (Tr. 27/13196-7) is true only in the narrowest of senses—QICAP, the TSP variable name for my facility capital index, indeed does not appear in the text of USPS-T-15. However, I did discuss its data sources and inclusion in the labor demand models. USPS-T-15 at 93-94, 116. I also responded to numerous interrogatories from the OCA and UPS investigating the foundations of the variable. In fact, Dr. Neels was able to use the information I provided to demonstrate the deployment of various types of equipment over the period of time covered by my sample. Tr. 27/12780. Dr. Smith is able to extract such

detailed information about the derivative of the capital index as the depreciation rates by asset category. Tr. 27/13182. Since there is only one facility capital index used in the study, there is no real ambiguity.

VII.b. Misinterpretation of Postal Service testimony.

Dr. Smith bases his contention, that I potentially erred in not using a simultaneous equations estimator to reflect the endogenous nature of capital, on a string of misinterpretations of my testimony as well as those of witnesses Degen (USPS-T-16) and Kingsley (USPS-T-10). Dr. Smith's contentions, that "it is not clear whether capital is an exogenous or endogenous variable" (Tr. 27/13168) and that I indicate "that capital is neither exogenous nor endogenous" (Tr. 27/13201), misrepresent my testimony. I explained that I treated capital as "predetermined." Tr. 15/6414. This term reflects the fact that the investment decisions that determine current period capital occur well in the past, as well as explaining my choice of estimation procedure. In econometrics, "predetermined" variables include exogenous and lagged endogenous variables—the term is used in virtually every textbook treatment of the simultaneous equations problem, including those cited in his response to USPS/OCA-T4-21 (see Tr. 27/13268-9). The significance of the term is that a simultaneous equations estimator is not needed for a regression in which all of the explanatory variables are predetermined. The terminology I used should have clarified my treatment of capital to Dr. Smith.

Dr. Smith attempts to take issue with my characterization of capital as predetermined on operational grounds as well. He states, without citations, "Based on information furnished by the Postal Service, it appears that the current level of capital is related to the current level of activity, though not necessarily on a 100 percent basis." Response to USPS/OCA–T4–21(d) at Tr. 27/13269. Asked to provide supporting citations to the referenced Postal Service information in USPS/OCA-T4-51 (Tr. 27/13310), Dr. Smith cites two of my interrogatory responses, portions of witness Degen's and witness Kingsley's testimonies, and the Postal Serivce's 1999 Comprehensive Statement on Postal Operations. The material he cites does not support his characterization of capital costs. For example, he cites my response to OCA/USPS-T15-14, which does not concern capital costs at all. My response to OCA/USPS-T15-13, also cited, indicates that major equipment deployments usually take more than one year. Witness Degen's cited testimony, emphasizes that

> One reason for this deliberate pace [of new plant construction] is the enormous time and capital commitments involved. From initial proposal to project completion, it may take anywhere from 6 to 9 years to bring a new plant on line. Site acquisition, planning, and approval for a new plant can easily take 5–7 years, and actual construction another 1–2 years. USPS-T-16 at 15.

Likewise, a cited section of witness Kingsley's testimony indicates that the initial phase of AFSM 100 deployment was scheduled to begin in March 2000, with a second phase deployment planned to begin at the end of FY 2001. USPS-T-10 at 11. These responses make it clear that there are long lead times between investment decisions and the appearance of new plants and capital

equipment on the workroom floor. The conclusion Dr. Smith draws from the cited material is virtually the opposite of its plain meaning. The cited material supports my treatment of capital as predetermined.

VII.c. Statistical errors.

In USPS-RT-7, Dr. Greene describes several fundamental statistical errors Dr. Smith commits in his testimony, including the erroneous claim that the between model is the "least bad" among the alternative estimators, and the faulty suggestion that visual analysis is a "compelling" substitute for an appropriate quantitative study. USPS–RT–7 at 31, 37-8. Dr. Smith himself admits that the simple regression analysis corresponding to the visual exercise is "econometrically indefensible." Tr. 27/13215. Dr. Smith's erroneous econometric prescriptions must be rejected. His contention that I could have potentially increased the accuracy of my estimates by considering clusters of sites in lieu of the panel data estimation approach (Tr. 27/13174) is also faulty. A clustering approach would have constituted another type of aggregation procedure. Once again, if aggregation were appropriate, the disaggregated models would produce results consistent with the aggregates. The clustering procedure cannot add information to the variability analysis, but rather only create the potential for bias from imposing inappropriate restrictions on the variability models. Dr. Smith's erroneous econometric prescriptions must be rejected.

VII.d. Faulty and self-contradictory theoretical positions

Dr. Smith's testimony relies on a number arguments that are transparently self-contradictory. Chief among these is Dr. Smith's inconsistent position on the fundamental issue of whether multiple regression analysis is required for the variability study. As Dr. Greene indicates,

> It is clear that it is appropriate to use multiple regression to model the response of labor costs to output—the appropriate definitions of these two variables and how to measure them is an issue to be settled elsewhere. A simple regression of hours (or its logarithm) on output of any sort (or its logarithm) will surely ignore many other factors that that should be in the equation... USPS-RT-7 at 6.

Some of Dr. Smith's criticisms imply that there are additional variables that I should have included in my models but did not. For example, he claims that "Capacity utilization is another potentially important variable missing from Dr. Bozzo's database." Tr. 27/13184.[9] For Dr. Smith's statement to have any practical meaning for the labor demand models, it would have to be that capacity utilization should be added as an explanatory variable to the models. This would make the appropriate model a multivariate regression *a fortiori*. On one hand he suggests that I do not have enough variables in my model, but on the other hand he is unsure whether a multiple regression model is appropriate. Dr. Smith's response to the question of whether a multivariate regression model is

[9] Dr. Smith's statement is, in itself, erroneous. The capital and labor data needed to compute measures of capital (i.e., "capacity") utilization are present in the database. Furthermore, since workhours are endogenous to the models, capital utilization is implicitly determined by the models as well.

appropriate is "I don't know." Response to USPS/OCA-T4-16(a) at Tr. 27/13262. By way of explanation, Dr. Smith offers:

> Two important variables for the analysis of volume variability appear to be TPH and hours. On a bivariate basis they seem to be closely associated. Applying the concept from William of Ockham, *Pluralitas non est ponenda sine necessitate* (this translates as "entities should not be multiplied unnecessarily." Put differently, "keep it simple"), also known as Ockham's Razor, one would look for the simplest explanation, and a simple explanation is that there is a very high degree of relationship between the two variables: it is visually compelling. Id.

One wonders if the only reason why he is unable to say whether a multivariate model is appropriate is because he is unable to figure out how an appropriate multivariate model can be made to produce the 100 percent variability result. In contrast, results from the simple regression model, such as those Dr. Smith presents at page 66 of OCA-T-4, more-or-less do.[10] The catch is that the bivariate models are "econometrically indefensible." Tr. 27/13215. All Dr. Smith can offer is a paean to simplicity—hence the invocation of the maxim of "Ockham's Razor."

Ockham's Razor, however, does not value simplicity at any cost—this is the vital "unnecessarily" in the direct translation. This maxim, as Carl Sagan nicely puts it, "urges us when faced with two hypotheses that explain the data *equally well* to choose the simpler." (Carl Sagan, The Demon-Haunted World, New York: Ballantine Books, 1996, at page 211;

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[10] However, note that Dr. Smith's results show a 19 percent "variability" for the "OCS [sic]" operation—presumably this means OCR. To be consistent, Dr. Smith would have to maintain that there is "visually compelling" evidence that OCR costs are 19 percent volume-variable.

emphasis in original). The bivariate models are definitely simpler, but they do not explain the data as well as the multivariate models. The specification tests that favor the more complicated multivariate models tell us loudly and clearly that the additional complications are necessary. Rather than draw the correct conclusion that the bivariate models are biased, Dr. Smith concludes that the multivariate models must somehow be wrong.

Dr. Smith's testimony incorprates inconsistencies on points of economic theory as well. The Intriligator work he cites in support of his "expansion path" arguments (discussed in more detail below), motivates the "expansion path" in the context of profit maximization.[11] Response to USPS/OCA-T4-2 at Tr. 27/13240-1. However, he goes to some length to argue that the Postal Service is actually an "output maximizer" a la Soviet manufacturing industries. OCA-T-4 at 47, 49. The objectives of profit and output maximization are inconsistent, since "output maximization" would tend to require unprofitable behavior such as selling product below cost. In fact, neither of the behavioral models Dr. Smith offers is particularly applicable to the Postal Service. The Postal Service's statutory break-even requirement interferes with profit maximization, while the requirement that prices at least cover "attributable" costs, among other things, makes output maximization difficult. Its inability to freely choose its prices limits both types of behavior. Indeed, Dr. Smith's "evidence" in support of the output

[11] Since, as I discuss below, the "expansion path" and cost function are conceptually identical, the "expansion path" does not depend on profit maximization for its existence.

maximization hypothesis is extraordinarily thin, consisting primarily of a reference to a speech in which a Postal Service vice president emphasizes the importance of revenue growth. Response to USPS/OCA-T4-13(c) at Tr. 27/13257. It should be transparently evident that the Postal Service operates in an environment dramatically different from Soviet enterprises, and is, in various ways, prevented from exhibiting output maximizing behavior. All Dr. Smith has done in his output maximization argument is to follow a far-fetched claim to its logical but absurd conclusion. He does not provide a useful characterization of the economic framework for mail processing costs.

VII.e. Unsupported allegations.

Dr. Smith makes a number of allegations that my estimates are potentially sensitive to a variety of factors, including structural changes to Postal Service operations and the sample selection procedures. Tr. 27/13169-77. As Dr. Smith performed no analysis of his own (Tr. 27/13249), he offers no evidence in support of the allegations. In fact, in many cases, he simply ignores responsive analysis I presented in my direct testimony. In USPS–T–15, Appendices A and B, I present alternative variability estimates varying the minimum observations screen and dispensing with all of the sample selection screens entirely. The results clearly show that, contrary to Dr. Smith's allegation, the presence of the sample selection screens do not drive my results. Nor did I ignore the issue that the earlier years' data may not be fully representative of future operations. Thus, in Appendix D of USPS–T–15, I presented the results of alternative variability calculations in which only FY98 observations were used to evaluate the elasticity

formulas. Once again, the results are robust to the period over which they are evaluated. Dr. Smith's concerns are not merely groundless, they are contradicted by evidence already on the record in this proceeding.

VII.f. Dr. Smith's "erratum" revising the definition of volume variability introduces an error into Dr. Smith's testimony.

As I demonstrate in this section, the revision of the definition of volume variability in Dr. Smith's erratum to his direct testimony not only introduces an error and contradiction into that testimony, but calls into question Dr. Smith's basic understanding of econometric model construction and interpretation.

Dr. Smith's initial direct testimony correctly defines "[v]olume variability for mail processing...as the percentage change in cost that results from a [unit] percentage change in volume, *holding delivery points and other non-volume factors constant.*" Tr. 27/13153. In a subsequent section of his testimony Smith expounds on the importance of including measures of network effects, including possible delivery points, in the analysis of mail processing variability, noting the possible presence of "three types of network issues" in modeling mail processing labor demand:

> First, there is the intra-plant network of activities that feed mail to each other....A second type of network effect is apparently the delivery configuration of the service territory. Dr. Bozzo measures this network configuration with a variable measuring the number of possible deliveries [in the plant's service territory]. Finally, the position of the plant in the mail flow between other mail processing plants also seems to be a type of network relationship. According to an interrogatory response, the size of facilities and their mail processing operations depends not only on the volume of mail processed, but also their position in the Postal Service's network. Id. at 44 (footnote omitted).

In his testimony, Dr. Smith emphasizes the importance of network effects in models of mail processing labor costs, citing their importance in determining, among other things, "the length of processing windows, the complexity of mail processing schemes, the relative amount of labor required for set up and take down activities, [and] the operation's role as a gateway or backstop." Id. at 45. Indeed, he even expresses concern that my models may have included *fewer* than the optimal number of controls for the various types of network effects:

> The analysis conducted by Dr. Bozzo addressed only the possible deliveries; he did not address the networking of activities at the plant level or the interchange of mail between plants. Both of these types of network effects might have an impact on labor demand. Id. (footnote omitted).

I was therefore puzzled when, over a month after filing his direct testimony, Dr. Smith appeared to have inexplicably changed his mind about the importance of including measures of network effects in the regression. In a revision to Smith's direct testimony labeled "Erratum," the phrase "holding delivery points and other non-volume factors constant" was stricken from the sentence on page 5 cited above.[12] The erratum stated that the deletion was necessary "to eliminate an inappropriate restriction on the volume variability definition as previously indicated in witness Smith's response to USPS/OCA-T4-11(b) and to eliminate any uncertainty as evidenced by...interrogatories USPS/OCA-T4-33 and 34(b)." This was accompanied by Smith's responses to USPS/OCA-T4-33 and 34, which note the need "to remove a statement in my

[12] "Revision to the Testimony of witness J. Edward Smith (OCA-T4)(Erratum)" filed June 28, 2000.

direct testimony that conditioned the definition of volume variability upon holding delivery points and other non-volume variables constant." Tr. 27/13284-5.

All the more puzzling is the fact that while striking this clause, Dr. Smith neglects to remove the above-cited material from pages 44 and 45 of his direct testimony extolling the importance of network effects in models of mail processing labor costs. Tr. 27/13193-4. The net effect of this that Dr. Smith's direct testimony (as amended) is in direct conflict with itself, on one hand asserting that network effects are key elements of the analysis, and on the other insisting that the econometric estimates of the variabilities should not be conditioned on them.

The key to explaining this confusion in Dr. Smith's testimony is evident from a close reading of his responses to USPS/OCA-T4-11 and 34. In his response to 11(b) witness Smith claims that:

> [i]n computing the volume variability, Dr. Bozzo...estimated the multivariate econometric model of hours of labor as a function of TPF and other variables; only the estimator associated with the TPF variable is used in computing the variability. *Accordingly, in order to be precise, the statement should be "the percentage change in cost that results from a [unit] percentage change in volume"* (emphasis added). Tr. 27/13254.

In comparison, witness Smith states in his response to 34(a):

> On further review, it is apparent that Dr. Bozzo has used more than the estimator associated with the TPF variable in computing [the] variability. The appropriate annotation is found in footnote 36 at 76 in Dr. Bozzo's testimony. *I believe it was Dr. Bradley who used only the estimator associated with the TPF* [sic] *variable in computing [the] variability* (emphasis added). Tr. 27/13285.

The apparent problem is Dr. Smith's mischaracterizations of the derivation of the variabilities at Tr. 27/13254 and 13285. The variabilities, in both my study and Dr. Bradley's, are appropriately computed as the partial derivative of the labor demand function with respect to TPH. The resulting formula depends on TPH and the other variables in the labor demand model. Dr. Smith states that since I do not include the "estimator" associated with delivery points in my computation of the variability factor, it would not be "precise" to say that delivery points had been held constant. Tr. 27/13254. Dr. Smith is wrong on this point, as may be verified by examining any econometrics textbook. The correct computation of volume variability (as provided in USPS–T–15) must hold constant (or be "net of") delivery points and the effects of other non-volume factors, otherwise we would not have proper measures of volume variability, but rather a confounding of volume and non-volume effects. One does so by including delivery points and other non-volume factors in the regression model. This does not imply that one should include the coefficients corresponding to these factors explicitly in the variability formula. As I mention above and in my direct testimony, this was well known as of Docket No. R71-1. Dr. Smith's "erratum" obscures, rather than clarifies, the correct definition of volume-variability.

VII.g. The Postal Service's cost methods, taken as a whole, embody the correct "length of run"—which is not the "long run" advocated by Dr. Smith.

Dr. Smith incorrectly claims that the Postal Service's mail processing cost analysis is "fatally flawed" because it is not a "long run" analysis. Tr. 27/13167 et seq. His criticism is hardly new, but unfortunately it has not improved with age. In Docket No. R97-1, Smith claimed that the high frequency of Dr. Bradley's data —i.e., observations every postal accounting period—combined with the use of the fixed-effects model, caused Bradley's variability estimates to be inappropriately "short run." Docket No. R97-1, Tr. 28/15835-41. As I note in my direct testimony, Dr. Smith's arguments about length of run in the previous rate case were specious and without merit, and were successfully rebutted in the record evidence of that case.[13] In the present docket, Dr. Smith makes a similar claim, but has largely backed away from the arguments he proffered last time. Instead, he erroneously asserts that nothing but a "long-run" analysis – by which he means one in which all factors of production, including plant and equipment, are assumed to vary freely – will do for purposes of ratemaking. Tr. 27/13189.

Once again, Dr. Smith is wrong. He claims without substantiation that:

> Postal Service witnesses and management appear to have a time frame of as little as one year to as many as five years in mind when they discuss the longer run, the period over which capital investment varies. The time frame seems to center on two to three years. Tr. 27/13190.

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[13] See USPS-T-15 at 18, lines 16-19, which cites the rebuttals by witnesses Higgins and Bradley to this line of argument in Docket No. R97-1. See also id. at 71-72.

He goes on to state:

> [I]t would appear that there are several time periods relevant to the estimation of postal costs. One time period is a day, the period over which very short-term adjustments to labor are made....A second time frame appears to be the 4 week or 3 month time frame used by Dr. Bradley and Dr. Bozzo....Finally, a longer-run time period, which would appear to approximate the length of the rate effective time period in the neighborhood of two years, seems to be the time frame over which investment, personnel, and equipment decisions are realized. Tr. 27/13191.

Only the first of these "time frames" has any basis in the record evidence of this case. As I testified, the process of assigning the existing labor complement in a plant to various operations to meet immediate processing needs does, indeed, operate "on time scales on the order of hours." USPS-T-15 at 18. This comports with Smith's first "time frame" of a day. Smith's second reference, to "the 4 week or 3 month time frame" used by Bradley and myself refers not to any operational decision-making framework, but rather to the frequency of our data (accounting periods and quarters, respectively). Contrary to Smith's understanding, the periodicity of the data used to analyze costs does not determine the length of run of the analysis. As I have already discussed, that particular argument was rebutted in the previous rate case and should carry no weight.

Dr. Smith's final reference in the above-cited passage, to the "longer-run time period, which...[is] in the neighborhood of two years" and "over which investment, personnel, and equipment decisions are made," has no basis in fact. This is mere conjecture—he cites neither record evidence nor any authority versed in the subject of management decision making. As I have already testified, management decisions concerning long-run labor allocation and

investment are independent of the "rate cycle." USPS-T-15 at 18. Since models of labor demand of the sort I developed in my analysis are properly based on the actual planning practices of actual line managers, rather than abstract theorizing, there is no basis for incorporating Dr. Smith's third "time frame."

Having posited, without evidence, that only a "long-run" model is appropriate for ratemaking, Dr. Smith attempts to discredit my inclusion of a plant-level capital index in the labor demand function. As Dr. Smith notes, I freely admit that my volume-variability estimates are "short run" in the sense of treating capital as a quasi-fixed factor. Tr. 27/13190. I fully intended to do so: my treatment of capital as quasi-fixed is reasonable and comports with the mainstream of econometric cost analysis.[14] My treatment of capital does not mean to imply that my variability estimates assume that the Postal Service never changes its capital stock, or that no new net investment takes place[15]. Nor does it mean, as Smith asserts, that my estimates are "only measuring transitory changes in mail processing." Tr. 27/13190. Quite the contrary: my model incorporates an explicit measure of capital into the model, along with a time trend, to allow for continuous changes to the capital stock, and with it the level of

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[14] For a general discussion, see chapter 9 of Ernst R. Berndt, *The Practice of Econometrics: Classic and Contemporary*, Addison-Wesley 1991. For an application, see Douglas W. Caves, Laurits R. Christensen, and Joseph A. Swanson, "Productivity Growth, Scale Economies, and Capacity Utilization in U.S. Railroads, 1955-1974," *American Economic Review* Vol. 71, No. 5 (December 1981), 994-1002.

[15] Dr. Smith's confusion on this matter may be related to his misunderstanding of the term "to hold constant," as I discuss in Section ~~XIIIa~~. VII.f.

technology.[16] I have included an index of the plant's net capital stock in my regression model, so that my estimate of the volume variability of labor hours in an operation is conditional on the level of capital in place in the current period.

That Dr. Smith should criticize my analysis for including a capital measure is ironic, given that in his R97-1 testimony Dr. Smith criticized Dr. Bradley for failing to include such a measure:

> In my opinion, witness Bradley's translog cost equation is insufficient, for he does not include capital as one of the cost factors. Witness Bradley needs to examine the underlying production function and cost function and the derivation of the cost function. He also needs to examine capital/labor substitutions, scale economies, and the interrelationships of activity processes in conjunction with his estimated cost equation. This will enable an understanding of the impact of changes in capital and technology on the cost in labor hours as TPH varies during mail processing. Docket No. R97-1, Tr. 28/15828. See also Tr. 28/15823, 15825, 15826-7, 15850-52.

In Frank Capra's classic movie, "It's A Wonderful Life," the angel Clarence warns Jimmy Stewart's character, George Bailey, "Be careful what you ask for, George – you might get it!" Dr. Smith could learn a thing or two from Clarence. In the previous rate case, Smith "asked for" a regression model that included, among other things, a measure of capital. Now he has what he asked for, but it has not apparently made the kind of difference to the results that he anticipated. Smith has turned his old argument on its head and tried to use it as a basis for shoring up his previous argument about length of run, which was successfully rebutted in Docket No. R97-1. That is, whereas (according to Dr. Smith) in the

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[16] See Dr. Greene's rebuttal testimony (USPS–RT–7) at 11-13, 31-34 for further discussion of this point.

last case Dr. Bradley's model was no good because it didn't contain a capital variable, in this case my model is excessively "short run" because it does contain a capital variable. At best this is disingenuous. An econometric model should be specified based on economic theory, not on whether the results fit one's own purpose.

VII.h. The theoretical foundations of the Postal Service's mail processing labor demand models and of Dr. Smith's recommended "expansion path" approach are identical

The Postal Service's mail processing labor demand analysis is consistent with Dr. Smith's "expansion path" approach, which he claims is the conceptually correct economic relationship to estimate. Tr. 27/13167.

Dr. Smith himself establishes that the expansion path argument does not constitute a criticism of the Postal Service's variability methods at all, for the simple reason that the cost function and expansion path are conceptually identical. Citing several authoritative texts, Dr. Smith explains, "the set of all possible pairs of output and cost along the expansion path define the cost curve." Tr. 27/13267. He further notes that "[i]n general, one can obtain a system of factor demand functions" derived from the expansion path or cost function. Id. He also confirmed that the short-run cost function simply represents an alternative expansion path, and that the long-run cost function must be below the short-run cost function for every level of output. Tr. 27/13304.

Dr. Smith also confirmed in part the substance of a number of statements, including derivations of the relationship between the expansion path and the

elasticities Dr. Bradley and I estimated. Tr. 27/13304, 13323. It follows immediately from the theory that Dr. Smith cites that the degree of volume-variability along the expansion path is the elasticity of labor demand (workhours) with respect to output. These are precisely the quantities Dr. Bradley and I estimated.

VIII. Conclusion

My review shows that Drs. Neels and Smith have provided no credible basis to challenge the conclusions presented in my direct testimony. Their attempts to sustain the general assumption of 100 percent volume variability for mail processing do not withstand scrutiny.

CHAIRMAN GLEIMAN: Two parties have requested oral cross-examination: the Office of the Consumer Advocate and United Parcel Service, I assume both on the rebuttal testimony, and correct me, gentlemen, if I'm wrong.

MR. MCBRIDE: That is correct, Mr. Chairman.

MR. RICHARDSON: That is correct.

CHAIRMAN GLEIMAN: Do either of you intend to cross-examine on Dr. Bozzo's NOI-4 response?

MR. MCBRIDE: I do have a very few questions on that.

CHAIRMAN GLEIMAN: Let's try and make a distinction at some point on that. We'll operate under the same procedure we did with the preceding witness.

Mr. Richardson?

Is anyone else interested in cross-examining?

[No response.]

CHAIRMAN GLEIMAN: If not, Mr. Richardson, proceed.

CROSS-EXAMINATION

BY MR. RICHARDSON:

Q Thank you, Mr. Chairman. Good evening, Dr. Bozzo.

A Good evening, Mr. Richardson.

Q I would like to start with the testimony on page 34. Just generally, your testimony is structured so that from page 33 to the end of it, you have a series of sections

rebutting Dr. Smith's testimony under various headings: misinterpretations, gripes and things, and the like. Correct?

A That is correct.

Q And I want to focus on the first section, styled cosmetic gripes, and that starts on page 33, but I want to also really discuss the second of the two subjects you discuss under that heading. The first refers to Dr. Smith's objections to your labor demand function not being explicitly derived, but I don't want to discuss that one. I want to discuss the second issue you raise that relates to your qicap or facility capital index.

A Facility capital index is probably a better English term, since the qicap is spelled, for the purposes of the reporter, Q-I-C-A-P, standing for quantity index of capital.

Q And there, you know, Dr. Smith, is critical because you didn't explain it specifically in your testimony, and then, you conclude in your discussion that even though that Dr. Smith and Dr. Neels actually prepared some studies using your capital index, and then, you conclude on page 35, the last sentence of that section: since there is only one facility capital index used in the study, there's no real ambiguity.

A That is what the statement at page 35, lines 1 and

2 says.

Q Now, that sentence suggests that if there is only one index, even if not adequately explained, there is, per se, no ambiguity. That's not correct, is it? That's not what the sentence is intended to say.

A What the sentence is intended to say is that there are descriptions of the presence of the facility capital index; the data sources used for the facility capital index; various economic assumptions used to conduct the facility index in my testimony and the accompanying library reference. And I should add that I also responded to a large number of discovery requests from both the OCA and UPS related to that material.

So, since if nothing else, that material should have made it abundantly clear that there was, indeed, one facility capital index that I constructed, again, I believe that what Dr. Smith has done is make a very narrow complaint regarding the particular style of presentation of the analysis. Again, it is my belief and my testimony that there is ample documentary material supplied related to the capital index.

Q Well, your rebuttal testimony didn't refer to all of those factors which you just indicated, and --

A Well, I believe --

Q Just because Dr. Neels and Dr. Smith used some

studies doesn't necessarily mean that they were confident that the underlying assumptions had been fully explained, does it?

A Well, I would assume that the parties would fully avail themselves of the discovery opportunities that they have to resolve the ambiguities that they actually believe are in the study.

Q Let's move to your second section, misinterpretation of testimony on page 35; that's the heading. And you suggest that Dr. Smith misinterpreted your testimony because he was not certain whether your capital variable is exogenous or endogenous. Now, you go ahead and define or make it clear that your capital variable is predetermined; that it includes exogenous and lagged endogenous variables, correct?

A Well, first of all, I did indeed explain that capital was predetermined, I believe while I was sitting in this chair in response to an oral question which I cite with that cite on page 35, line 12, to page 6414 of the transcript.

Predetermined is a technical econometric term, as I explained, that refers to lagged, endogenous variables.

Q Then, on line 18, you point out the significance of the assumption, of your assumption, that a simultaneous equations estimator is not needed for a regression in which

all of the explanatory variables are predetermined.

A Yes; you correctly represented my testimony.

Q So that means that if it is predetermined, it follows a simultaneous equations are not needed. That's your view, and that's your testimony.

A That is the theoretical econometric result.

Q On the other hand, though, would you agree Dr. Smith has suggested that simultaneous equations are needed?

A He has suggested that. However, I disagree with the suggestion. Again, it's -- the operational facts are that there are long lags between capital decisions and the equipment actually appearing on the workroom floor, and that makes capital predetermined in my book.

Q Your statement says simultaneous equations are not needed where all of the explanatory variables are predetermined. From that statement, am I to understand that in your model, all of the explanatory models are predetermined?

A I consider -- it would be a -- since predetermined variables as a term can include the truly exogenous variables, the ones that are outside the control of the Postal Service as well as the ones that are inside the control of the Postal Service but which simply don't happen to be simultaneously determined with labor, yes, it is my testimony that the variables that appear as explanatory

variables in my regression are all either exogenous, such as the volume which is simply based on the -- for instance, the volume of mail presented to the Postal Service is clearly a function of prices that are preset at any point in time and the amount of mail that the mailers actually enter in the plan.

Issues such as the operating plan by which that mail is processed and turns into TPH is, again, predetermined because all these decisions about facilities and equipment choice and location and investment have to be made long before the mail is actually processed.

Q Okay; thank you. I'd like you to turn to page 38. Another section of your testimony deals with what you view as faulty and self-contradictory theoretical positions of Dr. Smith, and there, you discuss Dr. Smith's criticism that you do not include all of the correct variables in your study. You say he's inconsistent about suggesting that multiple regression analysis is needed. Do you see that? Generally, is that what you're discussing there?

A I assume you're referring to lines 3 to 5 of page 38?

Q Yes, and you cite an example where you suggest that his testimony is self-contradictory, where he suggests that capacity utilization is potentially necessary to be considered, and if it is, it would require multiple

regression analysis. When asked if multiple regression analysis is appropriate, he testified that he didn't know, and that's essentially your testimony there, that you're suggesting that his comments were self-contradictory. And do you see that? I just wanted to ask you a further question about it?

A Well, just to first of all state I do believe that that is an appropriate summary of my testimony on pages 38 and 39.

Q Is?

A It is.

Q Okay; and --

A So what is the question?

Q The question is essentially, my point is that I don't see that it is necessarily self-contradictory, because in your own testimony, you indicate in your quotation of Dr. Smith's comment on line 15 of your testimony, Dr. Smith said capacity utilization is another potentially important variable missing. He used the word potentially, which indicated that it's not necessarily unequivocally within the model, and so that it appears that Dr. Smith is not prepared to state categorically that capacity utilization should be in the model and is therefore withholding judgment as to whether or not multiple regression analysis is necessary.

A Well, I still haven't -- I'm not sure -- let me

see if I have the question, if there is one. The -- in general, the econometric results that we have at hand suggest that there are multiple explanatory factors that are statistically significant in the regression models. So obviously, there's a significant difference of opinion as to the interpretation of that, but indeed, if you just think of the standard formulation of economic and econometric cost functions, basic economic theory tells you that costs will depend on multiple things, and it follows from that that if cost depends on multiple things, you need to include those multiple things in the regression analysis in order to capture them.

So, my point is that when Dr. Smith says that there are other things that are missing from the model, for that to have any operational significance, it must be at least in theory possible that those factors could be statistically significant as well. And again, given that fact that those factors are statistically significant doesn't, in general, make the other factors less significant. As I state on lines 18 and 19, when you add more explanatory variables, you simply are expanding the complexity of the model, so that if there are already shown to be multiple, statistically significant explanatory factors, then, adding more potential explanatory factors simply makes the appropriate model a multiple regression a

fortiori, as I say, and as Dr. Greene states in the methodological discussion from which I quote on page 38.

Q I don't see how that makes -- that confirms that Dr. Smith's testimony was self-contradictory.

A Well, again, it's either self-contradictory, or he was setting up a straw man argument. Again, if he's saying that the factors such as capacity utilization maybe aren't significant after all, I'm not going to disagree with him as my footnote 9 partly explains.

Q Did you consider capacity utilization in your model?

A Well, as I state in footnote 9, capacity utilization is endogenous to the neoclassical economic cost model upon which the whole economic framework here is based. So it is --

Q Is it predetermined in your view?

A Well, since capital is predetermined, and labor is determined within the model, then, if you regard capacity utilization as, for instance, labor per unit of capital or space or something else, then, capacity utilization is determined in the model.

Q So, is it predetermined in your view, in your model?

A Capacity utilization is not predetermined in the model. It is determined by the model.

Q And is it your understanding that Dr. Smith considers that capital -- capacity utilization is potentially necessary to take into account?

A Well, you're suggesting to me that maybe it is, and maybe it isn't.

Q I'm asking you if you understand that that's Dr. Smith's testimony.

A That is Dr. Smith's highly qualified argument, as you've represented it to me.

Q On page 39, lines 12 to 15, I'd like you to refer to --

A I have it.

Q Actually, this is one of three statements which appear in your testimony which speak to Dr. Smith's motives for his testimony and do not suggest any source or basis for your comments. And I want to ask you about them and wonder if you have any underlying reason for indicating that Dr. Smith's motives may be something other than trying to determine the appropriate volume variability. For instance, on line 12 through 15, the sentence: "One wonders if the only reason why he is unable to say whether a multivariate model is appropriate is because he is unable to figure out how an appropriate multivariate model can be made to produce the 100 percent variability result."

And I would also refer you to page 49, lines 21

and 22, where your testimony is: "now, he has what he asked for, but it has not apparently made the kind of difference to the results that he anticipated." And again on page 50, lines 3 to 5: "an econometric model should be specified based on economic theory, not on whether the results fit one's own purpose."

Now, those three quotes suggest that you view that Dr. Smith had a motive other than determining the appropriate and technically appropriate volume variability. Do you have any basis from your own knowledge to support those comments?

A I would not interpret those comments that way. I would interpret those generically as a gloss on what Dr. Greene and I have identified as basic statistical error that is -- that appears repeatedly in Dr. Smith's testimony, which is that, for instance, just to go to the page 39 material, you have a simple bivariate model that produces a given result. That model can be rejected in favor of other models that are more general; that produce a different result.

Q You're not answering my question.

A I am explaining the meaning of the statements, which is, I believe, what you asked.

Q No.

A Unless you want to restate the question.

Q The question was did you have any basis for suspecting that Dr. Smith's motives were anything other than determining the appropriate -- technically appropriate volume variability?

A My response to that is that my statement is a gloss on Dr. Smith's inappropriate sticking to his statistical priors in the face of statistical evidence that contradicts them. That is the correct way to interpret those statements.

MR. MCBRIDE: And, Mr. Chairman, I just want to just state for the record that OCA considered moving to strike these particular sentences, but I don't see much purpose in making a motion of that type and burdening the Commission with that type of motion. However, I do want to register a comment that I just don't think that kind of comment is appropriate in a prepared testimony.

Dr. Bozzo has indicated that he has no particular understanding of the motives of Dr. Smith. That's all I want to say on that subject.

THE WITNESS: Again, I apologize if that impression was given.

BY MR. MCBRIDE:

Q On page 42, we have a heading that you're into another area relating to the erratum filed by Dr. Smith to his testimony, and on page 42, lines 10 and 11, is where you

set out the definition that Dr. Smith had filed initially with his prepared written testimony, and which I believe you also ascribe to, which volume variability for mail processing as a percentage change in cost that results from a unit percentage of change holding delivery points and other non-volume factors constant.

Is that -- first of all, do you see that in your testimony?

A I do see that.

Q And that was the definition that Dr. Smith had in his prepared testimony. Is that also the definition which you ascribe to for volume variability in this case?

A Yes, I do, and it flows from the mathematical definition of marginal cost, which is defined as the partial derivative of cost with respect to volume, and lest there be some confusion as to what the particular meaning of volume, the Postal Service uses a methodology that decomposes that relationship which cannot be directly estimated from the available volume data that is from the RPW system into two components, one of which allows you to estimate the models that I do, which are with respect to piece handlings or intermediate measures of output and then something else that relates those, in turn, to the subclasses.

Q Now, again, I want to just ask generally. A quick reading of your testimony suggests that perhaps you felt

that Dr. Smith had somehow incorrectly modified or inappropriately modified his testimony after he filed written testimony. Is there -- I would point out that this testimony was, although filed as prepared testimony, was modified before he appeared orally and swore to that testimony in open proceedings.

You, yourself did the same type of procedure with your testimony, didn't you? You filed some errata with your testimony.

A I filed some minor errata that were largely nonsubstantive. If you want, strictly speaking, I would -- again, my section here is simply discussing my interpretation of the change and its effect on Dr. Smith's testimony.

[Pause.]

Q In the same section on page 44, line 8, you indicate that Dr. Smith asserts that network effects are a key element of the analysis, but then, on line 9, you say that on the other hand, he is insisting that the econometric estimates of the variabilities should not be conditioned on network effects.

With respect to your statement on line 9, that he's insisting that the econometric estimates of the variability should not be conditioned on network effects, what's your basis for that statement?

A The basis for that statement is Dr. Smith's assertion -- well, there are a couple of bases. Of course, the uncertainty of Dr. Smith's agreement that a multiple regression model is appropriate would enter that. Again, the delivery points in this context would be one of these other explanatory factors in addition to volume that would need to be taken into consideration.

So, if he says I don't know whether -- I believe these factors are important, but I don't know whether they should be included as regressors in the model, he is implicitly suggesting that maybe we shouldn't condition the volume variability estimates on the nonvolume factors that might enter into the analysis.

The other basis for the statement is Dr. Smith's suggestion that eyeballing plots of the data which corresponds to a type -- which also corresponds to a type of simple regression analysis that ignores factors other than those shown in the graph could produce a visually compelling evidence in support of a given volume variability estimate.

As Dr. Greene and I explained in our rebuttal testimonies, from a statistical standpoint, that's wrong.

Q Well, I think you're getting far afield from my original question.

A Well, you asked me what the basis of the statement was, and that's the basis of the statement.

Q In your view, does the network affect the volume of mail processed, the network?

A The network -- how do you define the network?

Q The mail processing network and the network as it's been used by you and Mr. Degen in this proceeding.

A As I understand it, the volume of mail to be processed depends on the amount of mail entered into the Postal Service's system by mailers and that the presence -- again, other than, perhaps, a chicken and egg issue that without a postal system, there can be no mail volume to be processed, I don't believe that the network itself causes volume, and I believe we discussed this back in May.

Q Does the volume of mail affect the Postal Service network?

A I believe that there is testimony that does suggest that over very long periods of time, the Postal Service network may indeed change in some respects with respect to volume. So, it's certainly possible, plausible under certain circumstances.

Q You said over a very long period of time; is that --

A Yes; exactly. Again, this goes back to the issue that it takes the Postal Service years to devise, authorize and implement its responses to changes in its operating conditions.

Q On page 46, you have a section relating to the length of run, and you indicate that Dr. Smith incorrectly claims the Postal Service is in error because it's not a long run analysis. On lines 11 through 13, you say Dr. Smith's arguments about the length of run in opinion R-97-1 are "without merit," and "were successfully rebutted in the record evidence of the case." Do you see that?

A I see that.

Q Now, it seems to me -- and correct me if I'm wrong -- that a measure of a successful rebuttal is that the Commission would reject the testimony that was rebutted. Is that -- but that's not the case with respect to Dr. Smith's testimony in the last case, is it?

A What I'm referring to here is the Commission's opinion that the relevant time horizon is the rate -- what's been variously called the rate cycle or the rate effective period, which is a period of, let's say, a couple to several years. And from an economic theoretical standpoint, that period corresponds to a version of the economic short run so --

Q Well, you agree that the Commission in the last case found the model was too short a run; that Dr. Bradley's model was too short run, don't you?

A I agree that that's what the Commission stated.

Q I just wanted to clarify that one reading your

testimony would have no idea that the testimony of Dr. Smith, at least the result he was testifying to, was approved by the Commission. I just wanted to clarify that, that you fail to mention that fact, don't you?

A Well, again, as I explained, what I mean by the length of run issue being determined is that the Commission, again, appropriately decided that the -- again, this idea of the rate cycle or rate effective period is the appropriate horizon.

Q And that was longer than Dr. Bradley's?

A No, what I'm saying is that that's not the long run.

Q And on page 48, lines 7 to 9, you have a sentence: "I freely admit that my volume variability estimates are short run in the sense of treating capital as a quasi-fixed factor." The sentence focuses on your treatment of capital as short-run. That's also -- your model is also short-run in its entirety, is it not?

A From an economic standpoint, a model is short-run or long-run depending on whether it treats factors of production such as capital that take a long time to adjust as being relatively fixed with respect to the decisions to use other factors of production such as labor that can be more easily varied.

Q On page 49 to 50 of your testimony, at the end of

49, moving over to 50, you say that according to Dr. Smith: "My model is excessively short-run because it does not contain a capital variable."

A Excuse me; what are you referring me to?

Q The sentence that starts at the bottom of page 49 and ends on the top of page 50 that says: "That is, whereas, according to Dr. Smith, in the last case, Dr. Bradley's model was no good because it didn't contain a capital variable." Then, the relevant portion: "In this case, my model is excessively short-run because it does not contain a capital variable."

A I'm sorry; I don't believe that that's what my testimony reads.

Q The statement reads --

A Because I add a negative?

Q Because it does -- you had a negative in there.

A Because it does contain a capital variable.

Q Because it does contain a capital variable.

And my question is rather simple: where does Dr. Smith say that a model containing a capital variable is too short run? That seems to be what you're saying, and it doesn't really make sense.

A Well, what I'm saying is that he says my model is too short-run. He said that Dr. Bradley's model was inappropriate because -- or it was inappropriate in part

because it failed to consider the effect of capital on labor demand, but now that my model does consider the effect of capital on labor demand, he is turning the argument as an argument against my model. That's what I'm referring to.

So again, there's -- the implication is that -- I believe that Dr. Smith has played one side of the capital coin with Bradley and another side with me, and that's an inconsistency across his R-97 and R-2000 testimonies in my opinion.

Q You basically mean to say that your model with your capital variable is too short-run; that's --

A Well, he says my model is too short-run. My contention is that it is appropriately short-run in the context of the Postal Service's base year and roll-forward analysis in which base year costs are adjusted to account for the effect of known capital deployment and operational changes between the base year and the test year.

MR. MCBRIDE: Thank you, Dr. Bozzo.

Those are all the questions I have right now, Mr. Chairman.

CHAIRMAN GLEIMAN: Mr. McKeever?

MR. MCKEEVER: Thank you, Mr. Chairman.

CROSS-EXAMINATION

BY MR. MCKEEVER:

Q Dr. Bozzo, your counsel a few days ago supplied a

number of potential cross-examination exhibits to UPS, and there was one in particular in which you calculated a composite variability from the variabilities that you present on pages 119 to 120 of your testimony. Are you aware of that?

A I believe I recall that there were computations of the composite -- or, excuse me, the composite variability for the sets of cost pools reported in some of the potential cross-examination exhibits.

Q Right; do you recall if the composite variability calculated in that exhibit for the variabilities you present in your direct testimony on pages 119 to 120 was 0.772?

A You would have to refer me to a specific exhibit, but I believe that that number does ring a bell, and again, without seeing the exhibit in front of me, I can't recall whether that number was for the full set of cost pools that I estimated or for a subset, but I will accept subject to check that that is a number which can be derived from at least some subset of cost pools.

MR. MCKEEVER: Well, Mr. Chairman, I would like to provide counsel for the witness and the witness with the exhibit that calculates that composite variability so that we don't have to do anything subject to check, and we're all clear.

CHAIRMAN GLEIMAN: Certainly.

[Pause.]

BY MR. MCKEEVER:

Q Dr. Bozzo, I've provided you with a copy of a document that contains two tables. The table on the left hand side is entitled comparison of variability estimates from alternative regression models, and one of the columns there is model A USPS-T-15, pages 119 to 120. Do you see that?

A I see that.

Q And that relates to nine cost pools?

A Yes, it does.

Q Okay; and the composite variability down there is 0.772.

A That is correct.

Q Is that the composite variability that results from the variabilities presented in your direct testimony on pages 119 to 120 for those nine cost pools?

A Yes, I believe it does. That 0.772 is the ratio of the number 3,931,751, which represents $3.9 billion more or less, since the convention is to report costs of the Postal Service in thousands, divided by the cost pool total for those nine pools in base year 1998 of $5,094,018,000.

Q Turn to page 9 of your rebuttal testimony, please. That's your table 1. Do you have that?

A Yes, I have that.

Q That table contains a column about the middle; FY79-FY98 observations consistent data. Do you see that?

A I see that.

Q Now, am I correct that those results, the results reported in that column are derived from a time series analysis of cost segment 3.1 data that you did?

A That column represents the results of a regression that I performed in which I included, first of all, all of the years of data included in Dr. Neels' work papers, and additionally, I substituted for the FY 1997 and FY 1998 segment 3.1 costs the segment 3.1 total computed per the Commission's method.

Q Okay.

A That is what that represents.

Q I think you may have anticipated me, but let me make sure that I'm clear. You calculated this regression using the years 1979 and 1980 included; is that correct?

A That is correct.

Q And one of your criticisms of Dr. Neels' regression was that he did not include those years; is that correct?

A My -- that is more or less correct.

Q Okay.

A To be precise, my criticism is not a particularly severe criticism as such. He stated a reason which was not

invalid in principle for excluding those from the results that he presented. However, the reason, upon further investigation, turned out to not have any import; that is, the FY79 and FY80 data that he was concerned may have been erroneous were, in fact, correct.

So given that he suggested, I believe, in an interrogatory response related to his choice that he did not have any other reason to exclude those observations, I considered that -- what the effect of including those observations may have been.

Q Okay; and I think you also stated -- the other point you made was that he included the so-called migrated costs for 1997 and 1998 in his numbers; is that correct?

A That is correct.

Q And in this particular column here presented in your table 1, you corrected that.

A That is what is meant by consistent data.

Q You did correct it.

A That means that I corrected it in the sense that the data are computed using the same method for determining the segment 3.1 total for all years. I don't make a comment here on the validity of that method.

Q Okay; am I correct that all of the numbers shown in that column of table 1 are higher than the composite variability of 0.772 calculated from your variabilities?

A Well, again, it depends on exactly what you mean by higher. For instance, that 0.880, if you take a two standard error band around that, the lower end of that band would be right around 0.772. So, first of all, you could argue that at least the lowest of the numbers is only borderline significantly different from 0.772. While I would agree that those numbers are higher, I also argue that those numbers are likely to be biased simply because they don't account for a variety of other factors that have changed in Postal Service operations over the time period between FY79 and FY98, which includes, among other things, the wholesale deployment of the Postal Service's automation equipment.

Q You mentioned the standard error and using a two standard deviations, adding them to the estimate in your answer; is that correct?

A Yes, adding or subtracting two standard deviations would give you approximately the 90 percent -- or excuse me the 95 percent confidence interval more or less for those estimates.

Q And if you did -- go ahead.

A As given.

Q Okay; and if you did that for the 0.88 estimate you mentioned, you would come up with -- on the high side; you mentioned one on the low side; you would come up on the

high side of one of 0.98, a little bit above 0.98; is that correct?

A That is correct.

Q Okay; now, that's for work sharing parameter 0.6; is that correct?

A That is correct.

Q Do you recall that Dr. Neels used a work sharing parameter of 0.855 as his estimate?

A I do recall that in another model, in which he estimated both the volume variability and the work sharing parameter simultaneously. He did come up with an estimate of the work sharing parameter of 0.855. However, I do explain on pages 7 and 8 right before the table that the standard error of that 0.855 estimate -- well, I actually don't state it here, but I believe that the standard error that Neels reported was approximately 0.25, so, in other words, the same confidence band would extend from 0.855 minus about a half or 0.35 to an upper bound in excess of 1, although the way that work sharing parameter is defined, it doesn't have any meaning outside the range of 0 to 1.

So it's certainly the case that that's the estimate he came up with. As I explained, it was not a very reliable estimate.

Q And the closest one in your table is the 0.8 work sharing parameter.

A That is the closest value that he estimated to the 0.855 parameter.

Q Thank you.

Could you turn to page 37 of your testimony, please?

A I have it.

Q There, you indicate that Dr. Smith's suggestion that you could increase the accuracy of estimates by clustering is another type of aggregation procedure; is that correct?

A That is what I state.

Q Okay.

A So, again, since Dr. Smith didn't define technically what he meant by clustering, the grain of salt with which my statement should take is that there could be an unstated meaning that I did not pick up.

Q Suppose there are two different types of MODS facilities with very different relationships between labor hours and volume and that the two groups are roughly equal in size, and there are plenty of data available for each group. Wouldn't it be possible to split your sample and estimate separate variability equations for the two samples?

A You could conceivably split the sample with or without that assumption and estimate variabilities for the subsamples.

Q And there are standard statistical tests, aren't there, for determining whether the coefficients for those two equations would be significantly different.

A That's correct, although by way of clarification, I should mention, as Dr. Greene indicated, the quantities of interest, the variabilities are not those coefficients themselves but rather mixtures of the coefficients, and the appropriate test of the relevant equality would be between the variabilities derived from those models and not necessarily the parameters themselves.

Q Okay; if those tests indicated that the coefficients were significantly different, would that indicate that you could increase the accuracy of the estimates by considering the two clusters of facilities separately?

A Again, it would depend on the exact nature of the test, the procedure that you used to aggregate the results from the split samples. Since if the ambiguity here is that if you split the sample, but there are some common factors that run across the sample, while -- again, you would not bias the estimation by splitting the sample, although again, that's somewhat different from the clustering -- or I should say it's a different and somewhat separate issue from the clustering procedure; again, you could do it. It's an open question as to what the practical relevance of splitting the

sample would be.

Q Well, let's forget about clustering. Am I correct that if you split a sample, used statistical tests for determining whether the results were significantly different, and the tests indicated that they were, could that increase -- couldn't you increase the accuracy of your estimates by considering the two groups of facilities separately?

A Again, as I said, it would be possible.

Q It could happen.

A Yes; again, you would need to recombine the results, since unless your hypothetical means to say that one of the groups doesn't have any bearing on the variability of postal costs. And again, there are -- it's ambiguous. I do not disagree that you could, in theory, increase the accuracy through that procedure.

Q Okay; thank you.

Now, you, of course, don't split the MODS facilities in different groups, right? You treat them all together.

A I treat them as their labor hours being explained by what is, in part, a common function which also allows for site-specific differences among the facilities.

Q But you do treat the sample as one grouping; is that correct?

A That is correct.

Q That's a higher level of aggregation than if you had split the two.

A Well, again, it's not strictly -- splitting the sample is not an aggregation issue. I can split the sample and still use the data at the facility level, for instance, to estimate the models. Again, so, again, I believe that, again, you're discussing generically a possible alternative method, but I believe you're mixing together a couple of techniques that are not mutually exclusive. Again, splitting the sample can be done with or without aggregating the data.

Q Okay; could you turn to page 12 of your testimony, please?

[Pause.]

A I have it.

Q There, you indicate on lines 14 and 15 that Neels knows that whether or not TPH is a good proxy for delivered mail volume is irrelevant; is that right?

A That is the statement.

Q Okay; now, do you say that because you believe that TPH and volume are directly proportional? Or are you saying that because -- you're saying that's true regardless of the relationship between TPH and volume?

A I'm stating it regardless of the relationship

between TPH and volume, and that's, again, in the context of this mathematical decomposition of the relationship between costs and volume into the relationship between costs and an intermediate cost driver and then between that cost driver and volume.

Q And the relationship between the cost driver and volume is an empirical question, isn't it?

A It is.

Q Now, I don't understand -- I mean, I understand that you don't agree with Dr. Neels' empirical results. Actually, I don't understand why you don't agree, but that's not the question I intended to ask. I understand that you don't agree with those empirical results, but if Doctor --

A Are you referring to the results that purport to test the --

Q Yes.

A -- proportionality assumption?

Q Yes; but if Dr. Neels were to address all of your criticisms and correct the problems you cite and found that TPH and volume were not proportional -- it's an if question now, okay? -- would that indicate that a correction to the TPH variabilities would be warranted?

A Not necessarily; it depends on how those data were intended to be used in the cost distribution procedure. In particular, this goes to, I think, a technical subtlety of

the way the Postal Service computes the cost by subclass. As was discussed a bit on Tuesday night, those distribution keys use IOCS data, which are generated from a sampling system whose properties are such that the subclass shares that come out of the IOCS data analysis would represent shares of TPH in the first place.

So it's possible to come up with a three-step distribution procedure that employs an elasticity with respect to TPH that is of cost or hours with respect to TPH; then, in turn employ some estimate of the elasticity of TPH with respect to FHP. You then need a third ingredient, which is an estimate of the elasticity of FHP with respect to RPW volume.

Q Well, you're adding things and making life more complicated than I thought we had to, because I didn't mention FHP at all.

A Well, again, you talked about a correction, and I'm afraid that I assumed that you meant the correction of the sort that Dr. Neels proposed or at least if not proposed discussed in his testimony.

Q Well, I just mean a correction to take account of the fact that a relationship between TPH and volume -- and let's forget about FHP -- TPH and volume were not proportional.

Now, let me ask it with that understanding again

in mind. If one were to empirically investigate the relationship between TPH and volume and find that that relationship were not proportional, wouldn't that indicate that a correction to the TPH variabilities would be warranted?

A Again, it could. I think that really, what you're describing there is something that's quite a bit different from what Dr. Neels discussed in his testimony. Again, if you could directly estimate the relationship between TPH and RPW volumes, that would -- I would agree, I believe, with you that that would be the best of all possible worlds. In library reference I-1, the Postal Service describes that as the so-called constructed marginal cost method, and it does dispense with what we've termed the proportionality assumption in this proceeding and in R-97-1.

It would be a very tall order to address the proportionality assumption using the data that are available. I would be -- I would personally be interested to see how one would go about doing it.

Q I think you stated in your testimony that you don't think the measures of RPW volume are good enough for that purpose; is that right?

A Well, I don't believe that they are voluminous enough for that --

Q Okay.

A -- purpose.

Q Okay; that's fair.

A That is, RPW is a time series of mail volumes by subclass that's available quarterly and at the national level, and, of course, it changes -- its meaning changes comparatively frequently with classification changes and other factors. So RPW itself is, in some respects, a moving target, and having a short, low frequency series of it that's only available as a national aggregate makes the problem all the more difficult.

Q And so, when you're in that situation, you, I guess, try to get the best you can out of the data that you have; is that right?

A I sure agree with that.

Q Okay; now, let me -- Mr. Chairman, with your permission, I'd like to show the witness a copy of page 3 of Dr. Bradley's rebuttal testimony, USPS-RT-8

CHAIRMAN GLEIMAN: Certainly.

[Pause.]

MS. DUCHEK: Mr. Chairman, I will make my objection for the record that this is transportation testimony, and Dr. Bradley was here on the stand and available and answered all sorts of questions about it, and this is not Dr. Bozzo's area.

MR. MCKEEVER: Mr. Chairman, I haven't stated a

question yet, but we are talking -- I intend to ask some questions about the cost driver approach and Dr. Bozzo's testimony with respect to that and contrast it with Dr. Bradley's testimony with respect to the approach of using a cost driver to determine whether there's a consistency or not.

CHAIRMAN GLEIMAN: Well, we'll let the cross go ahead, and I heard your objection, but I haven't heard any questions yet either.

BY MR. MCKEEVER:

Q Dr. Bozzo, could I direct your attention in particular to the section of Dr. Bradley's rebuttal testimony beginning on line 11 and continuing to the bottom of the page? Could you take a look at that? In particular, that states -- and I'll quote it: "Mr. Nelson may be correct that response of cubic foot miles with respect to volume is less than the assumed 100 percent, but this does not imply adjusting existing econometric models. Rather, it implies estimating the correct variability, which Mr. Nelson fails to do, of cubic foot miles with respect to volume and then applying that variability in the costing procedure." Do you see that?

A I see that.

Q Do you interpret that as suggesting an adjustment of an econometrically-estimated variability of cost with

respect to cubic foot miles to take account of a variability with cubic foot miles with respect to volume?

A Well, I will preface my result by stating that this is the first time I've seen this, and since I only see two pages of Dr. Bradley's testimony, I am not certain of the full context of it.

However, if I would turn to the bottom line result on page 4 or at the top of page 4, this decomposition of the elasticity of cost with respect to volume into the product of the elasticity of cost with respect to cubic foot miles and the elasticity of cubic foot miles with respect to volume, there is, in fact, no contradiction between the methods.

The TPH elasticities that I compute have the same role in the mail processing analysis as this elasticity of cost with respect to cubic foot miles term in the equation at the top of page 4. That's the Greek letter epsilon-C comma CFM. Then, the second term is an elasticity of cubic foot miles with respect to volume, which has to be estimated somehow, and again, in the case of mail processing, that elasticity is measured using the IOCS distribution key shares in a method which, as I have explained in my direct testimony, is constructed to be a first approximation to the true result, no matter what the relationship might be.

Again, as far as the particular implementation of

that second elasticity, there, I have to plead ignorance as to the distribution methods for the purchase transportation cost segment.

Q Is it your testimony that the IOCS numbers are a measure of the relationship between TPH and volume?

A That is what they do in the mail processing volume variable costs by subclass analysis, and in R-97-1, there was very extensive testimony by both Mr. Dagen and Dr. Christensen as well as, I should add, Dr. Panzer, just not to leave the econometric or the economic theory contingent underrepresented, that discussed in detail the economic basis for that approach.

Q Was there any regression of IOCS tallies with volume in that case?

A That -- no, there was not, and just to clarify, that would suffer from the same limitations as a regression, for instance, of TPH with respect to the volume data. Again, you don't have a lot of it, and you don't have it for a high frequency. In the case of IOCS, you can get more disaggregated results for the primary sampling units, but again, the basic pitfalls are the same, and it would not be feasible to estimate that regression.

MR. MCKEEVER: That's all I have, Mr. Chairman.

CHAIRMAN GLEIMAN: Is there any follow-up questions from the bench?

[No response.]

CHAIRMAN GLEIMAN: If not, that brings us to redirect. Would you like some time with your witness?

MS. DUCHEK: Two minutes would be fine, although before we break, Mr. Chairman, for our two minutes, I neglected to enter into evidence a category two library reference associated with Dr. Bozzo's rebuttal testimony. It is USPS-LRI-457, and I ask that that be entered into evidence.

CHAIRMAN GLEIMAN: Does Dr. Bozzo sponsor that?

MS. DUCHEK: I will ask him. I'm assuming his answer is yes.

REDIRECT EXAMINATION

BY MS. DUCHEK:

Q Dr. Bozzo, would you sponsor USPS-LRI-457?

A I do.

CHAIRMAN GLEIMAN: That being the case, the library reference in question will be received into evidence but not transcribed into the record.

[Library Reference USPS-LRI-457 was received in evidence.]

CHAIRMAN GLEIMAN: And now, you have your two minutes or thereabouts.

[Recess.]

CHAIRMAN GLEIMAN: Yes, ma'am?

MS. DUCHEK: The Postal Service has no redirect.

CHAIRMAN GLEIMAN: That being the case, I guess we move on to Dr. Bozzo's NOI-4 response.

Whereupon,

A. THOMAS BOZZO

was recalled as a witness herein and, having being previously duly sworn, was examined further and testified as follows:

DIRECT EXAMINATION

BY MS. DUCHEK:

Q Dr. Bozzo, I'm handing you two copies of a document entitled Response of A. Thomas Bozzo to Notice of Inquiry Number 4 on behalf of the United States Postal Service. Are you familiar with that document?

A I am.

Q Was it prepared by you or under your supervision?

A It was.

Q And if you were to testify orally today, would that still be your testimony?

A It would.

MS. DUCHEK: Mr. Chairman, I'm going to hand two copies of Dr. Bozzo's response to Notice of Inquiry Number Four to the reporter. I ask that they be entered into evidence and transcribed into the record.

CHAIRMAN GLEIMAN: Without objection, it is so

ordered.

[Response to Notice of Inquiry No. 4 by A. Thomas Bozzo was received into evidence and transcribed into the record.]

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

POSTAL RATE AND FEE CHANGES, 2000

Docket No. R2000–1

RESPONSE
OF
A. THOMAS BOZZO
TO NOTICE OF INQUIRY NO. 4
ON BEHALF OF THE
UNITED STATES POSTAL SERVICE

August 21, 2000

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Purpose and Scope

The purpose of this document is to provide econometric estimates responsive to item (a) in the Commission's Notice of Inquiry ("NOI") No. 4 Concerning Mail Processing Variability Models. The text of the NOI is attached. Dr. Greene's response addresses the theoretical issues raised in items (b)-(f) in the NOI. I also provide econometric variability estimates based on the specification that Dr. Greene calls "Model C", which is the general model in which the NOI's "Model A" and "Model B" are nested. Supporting materials are provided in USPS–LR–I–461.

I. Introduction

The NOI asks parties to "test the compatibility of witness Bozzo's data with the family of models that lack facility-indexed coefficients." First, I note that I performed exactly such a test to validate my choice of the panel data fixed effects estimator for the results I recommend in my direct testimony. USPS–T–15 at 122-124. The results of the standard specification tests I performed unambiguously reject the "pooled" model, which lacks facility specific coefficients, as well as the model with random facility effects. The implication is that the estimates from the pooled, between, and random effects models, all of which incorporate statistical restrictions that are rejected per the specification tests, are biased and inconsistent. Accordingly, the evidence on the record of this proceeding shows the pooled model (hereafter "Model 0"), without facility-

indexed coefficients, to be rejected in favor of the fixed-effects estimator of what the NOI terms "Model A," with facility-indexed intercepts.

The NOI also defines "Model B," which is a panel data model with time-indexed but not facility-indexed intercepts. Item (a) in the NOI requests that parties test whether Model 0 (the pooled model) can be rejected in favor of Model B, and whether a fixed- or random-effects formulation is appropriate to estimate Model B, for the five largest MODS operation groups covered by my analysis.[1] In Section II, I present the results of specification tests responsive to item (a) of the NOI. In his response to the NOI, Dr. Greene notes that specification tests comparing Model B with Model 0 have no bearing on the fundamental issue of whether there are significant facility-specific effects.

Dr. Greene indicates that Model A and Model B are not nested, but that both Model A and Model B are nested in what he terms "Model C." Dr. Greene's Model C incorporates both time-specific and facility-specific effects. The questions implied by the NOI are whether adding time effects to Model A, or facility effects to Model B, materially changes the results. In Section III, I present results that address these questions. The results show that, taking Model B as the starting point, it is possible to decisively reject Model B in favor of Model C with both facility- and time-indexed intercepts. The result is analogous to the rejection of Model 0 in favor of Model A. Also In Section III, I compare the results

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[1] The five largest MODS operation groups, by cost pool dollars, are (in alphabetical order), BCS, FSM, Manual Flats, Manual Letters, and SPBS. Below, I also present results for the OCR operation group, since my TSP programs produce results for the mechanized and automated letter and flat operations as a group.

I obtained in my direct testimony (Model A) with those of Model C. I show that the variabilities resulting from Model C are essentially the same as those that I present in my direct testimony.

II. Econometric specification of "Model B", and results of the specification tests described in part (a) of the NOI

In this section, I present the econometric specification of the pooled Model 0 and of Model B. I also present the results of specification tests of model B against Model 0. The specification of the pooled Model 0 that served as the basis for the tests of Model A in my testimony is:

$$
\begin{aligned}
\ln HRS_{it} = {} & \beta_0 + (\alpha_1 + \gamma_1 L + \gamma_2 L^2 + \gamma_3 L^3 + \gamma_4 L^4)\ln TPH_{it} \\
& + (\alpha_{11} + \gamma_{11} L + \gamma_{22} L^2 + \gamma_{33} L^3 + \gamma_{44} L^4)(\ln TPH_{it})^2 \\
& + \alpha_2 \ln CAP_{it} + \alpha_{22}(\ln CAP_{it})^2 + \alpha_3 \ln DEL_{it} + \alpha_{33}(\ln DEL_{it})^2 \\
& + \alpha_4 \ln WAGE_{it} + \alpha_{44}(\ln WAGE_{it})^2 + \alpha_5 TREND_{it} + \alpha_{55} TREND_{it}^2 \\
& + \alpha_6 \ln MANR_{it} + \alpha_{66}(\ln MANR_{it})^2 \\
& + \alpha_{12} \ln TPH_{it} \ln CAP_{it} + \alpha_{13} \ln TPH_{it} \ln DEL_{it} + \alpha_{14} \ln TPH_{it} \ln WAGE_{it} \\
& + \alpha_{15} \ln TPH_{it} \cdot TREND_{it} + \alpha_{16} \ln TPH_{it} \ln MANR_{it} \\
& + \alpha_{23} \ln CAP_{it} \ln DEL_{it} + \alpha_{24} \ln CAP_{it} \ln WAGE_{it} + \alpha_{25} \ln CAP_{it} \cdot TREND_{it} \\
& + \alpha_{26} \ln CAP_{it} \ln MANR_{it} \\
& + \alpha_{34} \ln DEL_{it} \ln WAGE_{it} + \alpha_{35} \ln DEL_{it} \cdot TREND_{it} \\
& + \alpha_{36} \ln DEL_{it} \ln MANR_{it} \\
& + \alpha_{45} \ln WAGE_{it} \cdot TREND_{it} + \alpha_{46} \ln WAGE_{it} \ln MANR_{it} \\
& + \alpha_{56} TREND_{it} \ln MANR_{it} \\
& + \beta_2 QTR2_{it} + \beta_3 QTR3_{it} + \beta_4 QTR4_{it} \\
& + \varepsilon_{it}.
\end{aligned}
$$

Model 0 differs from the specification of Model A (see USPS–T–15 at 117) in that the intercept term is assumed not to vary with the facility, indexed by i.[2] Note that it contains trend terms and seasonal (quarterly) dummy variables, which would be expected to capture some (if not most) of the time-specific effects specified in Model B. The Model 0 equation given above is applicable to the letter and flat shape operations. The corresponding SPBS equation omits terms involving the manual ratio variable. See USPS–T–15 at page 118. The corresponding estimating equation for Model B is:

$$
\begin{aligned}
\ln HRS_{it} = {} & \beta_0 + \lambda_t + (\alpha_1 + \gamma_1 L + \gamma_2 L^2 + \gamma_3 L^3 + \gamma_4 L^4)\ln TPH_{it} \\
& + (\alpha_{11} + \gamma_{11} L + \gamma_{22} L^2 + \gamma_{33} L^3 + \gamma_{44} L^4)(\ln TPH_{it})^2 \\
& + \alpha_2 \ln CAP_{it} + \alpha_{22}(\ln CAP_{it})^2 + \alpha_3 \ln DEL_{it} + \alpha_{33}(\ln DEL_{it})^2 \\
& + \alpha_4 \ln WAGE_{it} + \alpha_{44}(\ln WAGE_{it})^2 + \alpha_5 TREND_{it} + \alpha_{55} TREND_{it}^2 \\
& + \alpha_6 \ln MANR_{it} + \alpha_{66}(\ln MANR_{it})^2 \\
& + \alpha_{12} \ln TPH_{it} \ln CAP_{it} + \alpha_{13} \ln TPH_{it} \ln DEL_{it} + \alpha_{14} \ln TPH_{it} \ln WAGE_{it} \\
& + \alpha_{15} \ln TPH_{it} \cdot TREND_{it} + \alpha_{16} \ln TPH_{it} \ln MANR_{it} \\
& + \alpha_{23} \ln CAP_{it} \ln DEL_{it} + \alpha_{24} \ln CAP_{it} \ln WAGE_{it} + \alpha_{25} \ln CAP_{it} \cdot TREND_{it} \\
& + \alpha_{26} \ln CAP_{it} \ln MANR_{it} \\
& + \alpha_{34} \ln DEL_{it} \ln WAGE_{it} + \alpha_{35} \ln DEL_{it} \cdot TREND_{it} \\
& + \alpha_{36} \ln DEL_{it} \ln MANR_{it} \\
& + \alpha_{45} \ln WAGE_{it} \cdot TREND_{it} + \alpha_{46} \ln WAGE_{it} \ln MANR_{it} \\
& + \alpha_{56} TREND_{it} \ln MANR_{it} \\
& + \beta_2 QTR2_{it} + \beta_3 QTR3_{it} + \beta_4 QTR4_{it} \\
& + \varepsilon_{it}.
\end{aligned}
$$

The terms $\beta_0 + \lambda_t$ in the equation above correspond to the term α_t in the NOI. The NOI notes that regressors made redundant by the inclusion of time effects in Model B may be omitted. None of the other regressors should be

[2] I also relabled the intercept term to be consistent with the notation in Dr. Greene's response.

excluded from the pooled model estimated for the specification tests. Since the relevant issue is whether the trend and seasonal variables in Model 0 adequately control for time-specific effects, it is not appropriate to exclude any variables from Model B *a priori*. In order to avoid the significant programming complications that would be required to apply an appropriate autocorrelation adjustment to the random effects estimator for Model B, I estimated Model 0 and Model B without the autocorrelation adjustment.[3] These results are given in Table 1.

The NOI requests in part (a) that respondents test (1) the null hypothesis of a common intercept for all time periods (" $\alpha_t = \alpha$ for all t") against the alternative that the intercepts vary over time—i.e., Model 0 versus Model B— and (2) random effects versus fixed effects applied to Model B. These test are appropriately conducted by using an F statistic and a Hausman test, respectively. Both test statistics are computed by the programs named var(ltr,nl)-(tpf,tph)-by98-noi4b.tsp, in LR–I–461. The test results are presented in Table 1.

The P-values of the specification test statistics are reported in Table 1. The test statistic values and degrees of freedom are presented in the regression output in LR–I–461. The F test for common intercepts over time indicates that Model 0 cannot be rejected in favor of Model B for four of the six operation groups I examined: OCR, SPBS, FSM, and Manual Flats. However, these results do not weigh in favor of Model 0 since, for those cost pools, Model 0 has already been rejected in favor of Model A using the specification tests reported in

---

[3] Failing to adjust for autocorrelated disturbances impacts the efficiency, but not the unbiasedness and consistency, of the estimates (see, e.g., William H. Greene, *Econometric Analysis*, Second Edition, at 418-419).

USPS–T–15 (see USPS-T-15 at 122-124). One other case, BCS, is "borderline"—Model 0 is rejected at the 5 percent significance level but not at the one percent significance level. Only Manual Letters shows strong evidence in favor of Model B over Model 0.

The Hausman test indicates that the random effects model cannot be rejected in favor of the fixed effects model in any of the six cost pools. The results should not be surprising. The pooled model already contains a quadratic trend term and seasonal dummy variables. To the extent that those variables are successful at capturing the period-specific effects, the time-indexed intercepts in Model B should not add much explanatory power to the model. Nor does the presence or absence of period-specific effects say anything about whether facility-indexed intercept components also belong in the model, as explained by Dr. Greene in his response to this NOI. To address that issue, it is necessary to estimate Dr. Greene's Model C, which I do in the next section.

Table 1.
Specification test results for "Model B"

Cost Pool	BCS	OCR	FSM	SPBS	Manual Flats	Manual Letters
P-value, F test: H_0: Model 0 (with no time effects) vs. H_1: Model B (with time effects)	0.0150	0.5458	0.1550	0.5202	0.9739	0.0006
P-value, Hausman test: H_0: Model B (fixed effects) vs. H_1: Model B (random effects)	0.4179	>0.9995*	0.6132	>0.9995*	>0.9995*	0.3393
Reject H_0: Model 0 (with no time effects) vs. H_1: Model B (with time effects)?	Borderline**	No	No	No	No	Yes
Reject H_0: Model B (fixed effects) vs. H_1: Model B (random effects)?	No	No	No	No	No	No

*P-value is 1 to all reported digits.
**H_0 is rejected at the 5 percent significance level, but not the 1 percent significance level.

III. Econometric specification and estimates of "Model C"

Both Model A and Model B can be represented as special cases of Model C, which includes both facility-indexed and time-indexed components in the regression intercept. The estimating equation for Model C is:

$$
\begin{aligned}
\ln HRS_{it} = {} & \beta_0 + \delta_i + \lambda_t + (\alpha_1 + \gamma_1 L + \gamma_2 L^2 + \gamma_3 L^3 + \gamma_4 L^4)\ln TPH_{it} \\
& + (\alpha_{11} + \gamma_{11} L + \gamma_{22} L^2 + \gamma_{33} L^3 + \gamma_{44} L^4)(\ln TPH_{it})^2 \\
& + \alpha_2 \ln CAP_{it} + \alpha_{22}(\ln CAP_{it})^2 + \alpha_3 \ln DEL_{it} + \alpha_{33}(\ln DEL_{it})^2 \\
& + \alpha_4 \ln WAGE_{it} + \alpha_{44}(\ln WAGE_{it})^2 + \alpha_5 TREND_{it} + \alpha_{55} TREND_{it}^2 \\
& + \alpha_6 \ln MANR_{it} + \alpha_{66}(\ln MANR_{it})^2 \\
& + \alpha_{12} \ln TPH_{it} \ln CAP_{it} + \alpha_{13} \ln TPH_{it} \ln DEL_{it} + \alpha_{14} \ln TPH_{it} \ln WAGE_{it} \\
& + \alpha_{15} \ln TPH_{it} \cdot TREND_{it} + \alpha_{16} \ln TPH_{it} \ln MANR_{it} \\
& + \alpha_{23} \ln CAP_{it} \ln DEL_{it} + \alpha_{24} \ln CAP_{it} \ln WAGE_{it} + \alpha_{25} \ln CAP_{it} \cdot TREND_{it} \\
& + \alpha_{26} \ln CAP_{it} \ln MANR_{it} \\
& + \alpha_{34} \ln DEL_{it} \ln WAGE_{it} + \alpha_{35} \ln DEL_{it} \cdot TREND_{it} \\
& + \alpha_{36} \ln DEL_{it} \ln MANR_{it} \\
& + \alpha_{45} \ln WAGE_{it} \cdot TREND_{it} + \alpha_{46} \ln WAGE_{it} \ln MANR_{it} \\
& + \alpha_{56} TREND_{it} \ln MANR_{it} \\
& + \beta_2 QTR2_{it} + \beta_3 QTR3_{it} + \beta_4 QTR4_{it} \\
& + \varepsilon_{it}.
\end{aligned}
$$

I present econometric variability estimates for Model C, as well as the results of specification tests of Model C against Model B, in Table 2. The results closely mirror those from Model A that I present in my direct testimony (USPS–T–15 at 119-120. The variabilities presented in Table 2 are based on the fixed effects estimates of Model C, and adjust for autocorrelation of the disturbances. The F-test, which here tests Model B (without facility-indexed intercepts) against Model C (with facility-indexed intercepts), strongly rejects Model B in favor of Model C for all six cost pools. Furthermore, the Hausman test of random effects versus fixed effects for Model C supports the fixed-effects model over the random effects model.

1 **Table 2.**
2 **Principal results and specification test statistics for "Model C"**

Cost Pool:	BCS	OCR	FSM	SPBS	Manual Flats	Manual Letters
Output Elasticity (Volume-variability factor)*	0.877 (0.030)	0.742 (0.039)	0.840 (0.026)	0.664 (0.045)	0.764 (0.028)	0.732 (0.025)
Auto-correlation coefficient	0.643	0.701	0.627	0.594	0.673	0.699
Adjusted R-squared	0.986	0.972	0.994	0.987	0.988	0.991
Number of observations	5,406	5,097	4,373	1,584	4,891	5,512
Number of sites	298	289	236	95	278	300
P-value, F test: H_0: Model B (with no facility effects) vs. H_1: Model C (with facility effects)	**	**	**	**	**	**
P-value, Hausman test: H_0: Model C (fixed effects) vs. H_1: Model C (random effects)	0.0022	**	0.0007	0.0011	**	**
Reject H_0: Model B (with no facility effects) vs. H_1: Model C (with facility effects)?	Yes	Yes	Yes	Yes	Yes	Yes
Reject H_0: Model C (with fixed effects) vs. H_1: Model C (with random effects)	Yes	Yes	Yes	Yes	Yes	Yes

*Elasticities evaluated using full data set and arithmetic mean method; standard errors in parentheses.
**<0.00005 (P-value is 0 to all reported digits).

I compare the estimated variabilities from Model A and Model C in Table 3. The results from Model A and Model C are very similar, and the results for individual cost pools differ by less than the estimated standard errors of the variability estimates. There is no indication of systematic bias, and the composite variability for the six cost pools examined here differs by only 0.1 percent between Model A and Model C. The result is consistent with the finding above that the time-specific intercepts contribute little additional information over the trend and quarterly variables for most cost pools. Accordingly, the results in USPS–T–15 are not "fragile" when compared to those of the more general Model C.

Table 3.
Comparison of "Model A" and "Model C" variabilities

Cost Pool	"Model A" Variability (USPS–T–15 at 119-120)	"Model C" Variability (LR–I–461)	Percentage difference: "Model A" vs. "Model C"
BCS	0.895	0.877	-2.0%
OCR	0.751	0.742	-1.2%
Manual Flats	0.772	0.764	-1.0%
Manual Letters	0.735	0.732	-0.4%
FSM	0.817	0.840	2.8%
SPBS	0.641	0.664	3.6%
Composite	0.786	0.787	0.1%

IV. Summary

In this analysis, I demonstrate that the specification defined in the NOI as Model B (a panel data model with time-indexed by not facility-indexed intercepts) generally adds little explanatory ability compared to the pooled model already presented and rejected in USPS-T-15. Furthermore, Model B can be decisively rejected in favor of a more general specification, Model C. Model C, as discussed by Dr. Greene in his response to this NOI, incorporates both time-specific and facility-specific effects. The available evidence—both statistical, as presented here and in USPS-T-15, and operational, as described by witness Degen at pages 18-23 of USPS-T-16—overwhelmingly supports the existence of facility-specific, non-volume factors that affect costs. The rejected models, the

pooled and "between" models as well as Model B, inappropriately ignore the facility-specific effects and are seriously biased. Since the biases of the rejected models have no relevant economic interpretation, but simply reflect a confounding of volume and non-volume factors, they do not provide reliable estimates of volume-variability factors for mail processing and should not be adopted. My analysis also shows that the results I present in my direct testimony (for Model A) are essentially the same as those obtained from Model C. The results I present in USPS-T-15 are robust to the inclusion of the period-specific effects that yield the more general Model C, which provides further evidence that the USPS-T-15 results are reliable and should be adopted.

REPRODUCTION OF THE TEXT OF:

NOTICE OF INQUIRY NO. 4

CONCERNING MAIL PROCESSING VARIABILITY MODELS

(Issued August 2, 2000)

In Docket No. R97-1, witness Bradley conducted a specification search for a model of mail processing variability. He tested a family of models that lack time-indexed coefficients, and rejected the more restrictive models in favor of the facility-specific fixed-effects model. In response to Notice of Inquiry No. 4 in R97-1, the facility-specific fixed-effect model was tested and rejected against the general model, which had both time-indexed and facility-indexed coefficients. In Docket No. R97-1, witness Neels commented that this specification search had produced "too fragile and incomplete a set of results." One respect in which Mr. Neels regarded Dr. Bradley's specification search as incomplete was its failure to evaluate a parallel family of models that lacks facility-specific coefficients. See Docket No. R97-1 at Tr. 28/15775-84, 15805. This family of models was described in Docket No. R97-1 at Tr. 15776.

The record in this docket appears to be incomplete in the same respect as the record in Docket No. R97-1. To help provide a more complete record in this docket, interested parties are invited to test the compatibility of witness Bozzo's data with the family of models that lack facility-indexed coefficients. They are also invited to discuss, in testimony or comments, whether these specification test results, or those already performed by witness Bozzo, establish the validity of any particular model or family of models. Responses are due within 14 days of the date of this Notice.

Specifically, interested parties are invited to consider the model tested by witness Bozzo that lacks time-indexed coefficients. It will be labeled Model A and it takes the general form

$$y_{it} = \alpha_i + x_{it}\beta + \varepsilon_{it}$$

Here α_i denotes a facility-specific fixed-effect, y_{it} is the logarithm of hours in that operation, and x_{it} is the vector of variables including the logarithm of total piece-handling. Interested parties are also invited to consider an alternative model labeled, Model B,which lacks facility-indexed coefficients. It takes the form

$$y_{it} = \alpha_t + x_{it}\beta + \varepsilon_{it}$$

where α_t denotes a quarter-specific fixed effect, and all other variables are as defined above. In both of these models, the subscript i denotes facilities, and the subscript t denotes quarters.

a) Witness Bozzo performs a statistical test of the null hypothesis that $\alpha_i = \alpha$ for all i and rejects this null hypothesis. In addition, he tests and rejects the null hypothesis that the α_i are independently, identically distributed random variables with mean zero and variance. He uses both of these hypothesis tests to demonstrate that the facility-specific fixed effect model is statistically superior to the models nested within it, such as the "pooled" and "random effects" models. For the five largest MODS pools modeled by witness Bozzo (in terms of accrued costs), parties are asked to use his data to perform the following two hypothesis tests with respect to Model B: 1) the null hypothesis that $\alpha_t = \alpha$ for all t , and the null hypothesis that the α_t are independently, identically distributed random variables with mean zero and variance σ^2. Any terms used by witness Bozzo that are not needed because of the presence of α_t , such as lagged dependent variables and regressors may be omitted.

b) Parties are asked to indicate whether rejection of the hypotheses described in a) establish that Model A is statistically superior to the models nested within it, such as the "pooled" and the "random effects" models. Similarly, parties are asked to indicate whether rejection of the hypotheses described in a) establish that Model B is statistically superior to the models nested within it, such as the "pooled" and the "random effects" models.

c) Parties asked to discuss whether Models A and B are nested within one another, and whether rejection of the hypotheses described in a) provide statistical grounds for preferring either of these models over the other.

d) Parties are asked to discuss whether witness Bozzo's rejection of the hypotheses applicable to Model A is sufficient to establish that Model (A) yields a valid estimate of β, which determines the magnitude of volume variability.

e) Parties are asked to discuss whether rejection of the hypotheses applicable to Model (B) is sufficient to establish that Model B yields a valid estimate of β, which determines the magnitude of volume variability.

f) Parties are asked to discuss whether, even with the rejection of the hypotheses described in a), there may be theoretical grounds for concluding that a rejected model could provide a better estimate of variability than either model A or B.

DECLARATION

I, A. Thomas Bozzo, declare under penalty of perjury that the foregoing answers are true and correct, to the best of my knowledge, information, and belief.

A. Thomas Bozzo

Dated: 8/21/00

BY MS. DUCHEK:

Q And I won't forget there is a library reference associated with the notice of inquiry as well.

A Yes.

Q Dr. Bozzo, are you willing to sponsor USPS-LRI-461?

A I am.

MS. DUCHEK: And I ask that that library reference be entered into evidence.

CHAIRMAN GLEIMAN: It will be entered into evidence and not transcribed into the record.

[Library Reference USPS-LRI-461 was received in evidence.]

CHAIRMAN GLEIMAN: I believe the only party who indicated an interest in asking some questions regarding your NOI-4 response was Mr. McKeever on behalf of UPS, and I'm going to give you a pass, because I'm not sure how many times I can get through those questions that I asked Dr. Greene so --

THE WITNESS: If I may state, I also, unlike Dr. Greene, can't give the sort of level of depth of knowledge of the subject. So my response to those questions would have been that I agree with Dr. Neels -- or, excuse me, Dr. Greene.

[Laughter.]

MR. MCKEEVER: Thank you, Dr. Bozzo.

[Laughter.]

CHAIRMAN GLEIMAN: Well, we'll see -- I'll ask the questions of Dr. Neels, and then, if his answers are the same as Dr. Greene's, then, you know, that closes the loop pretty well.

It's late; we understand.

THE WITNESS: Thank you, Mr. Chairman.

CHAIRMAN GLEIMAN: Mr. McKeever?

MR. MCKEEVER: Thank you, Mr. Chairman.

CROSS-EXAMINATION

BY MR. MCKEEVER:

Q Dr. Bozzo, I have just a very few questions. The notice of inquiry asked the parties to do certain analyses for the five largest MODS pools modeled by you. That's on -- actually, we can say page 16 of your testimony, since you reproduce it at the end of your testimony -- between lines 20 and 21; is that correct?

A Sorry; I --

Q I'm sorry; let me try it over again.

A Okay; this is --

Q Go to page 16 of your testimony, which is --

A Yes, which reflects the --

Q Reproduces.

A Yes; thank you; it is getting very late.

Q Okay.

A Reproduces the NOI; I see it.

Q And on lines 20 to 21, the request is for analyses, "for the five largest MODS pools," is that correct?

A That is what the NOI requested.

Q Now, you did it for six rather than five; is that correct?

A That is correct, and Dr. Neels did it for nine, as I recall.

Q Well, that was my question. Nine was all of them, right?

A No, nine is all of them except for the cancellation and meter prep operation.

Q Okay; why did you do six rather than five but not nine?

A The reason why I did six rather than five but not nine, I believe I explain it.

Q Well, then, I missed it; go ahead.

A In footnote 1 at the bottom of page 4, the reason why I did six instead of five was because the program that does BCS and LSM -- or, excuse me, BCS and FSM, which I think are numbers two and three although not necessarily in that order, happens to also give you OCR for free, and OCR is not that much smaller than the -- well, OCR is sort of in

-- it's in a next tier of cost pools, which have a couple to a few hundred million dollars in costs but which are quite a bit bigger than, say, manual parcels, which is small, which has always been small, and LSM, which is going away and kind of substantially irrelevant to forward-looking discussions of postal operations.

Q Did you estimate any models for priority or manuals as part of this NOI response?

A I did not; however, if I may comment, in reviewing Dr. Neels' response, I believe that his model B and model C results would be highly similar to what I would have produced under the same circumstances.

MR. MCKEEVER: Okay; thank you.

That's all I have, Mr. Chairman.

CHAIRMAN GLEIMAN: Is there any follow-up?

[No response.]

CHAIRMAN GLEIMAN: There are no questions from the bench.

No redirect, I take it?

[No response.]

CHAIRMAN GLEIMAN: Thank you.

Dr. Bozzo, that completes your appearance here this evening. We thank you for tonight and for your other appearances. We appreciate your contributions to the record, and you're excused.

THE WITNESS: Thank you, Mr. Chairman.

[Witness excused.]

CHAIRMAN GLEIMAN: Mr. McKeever, would you like to call your witness?

MR. MCKEEVER: Yes, Mr. Chairman. UPS calls Dr. Kevin Neels to the stand.

Whereupon,

KEVIN NEELS

was recalled as a witness herein and, having being previously duly sworn, was examined and testified further as follows:

CHAIRMAN GLEIMAN: You can proceed to introduce Dr. Neels' NOI-4 and POIR-19 materials.

MR. MCKEEVER: Thank you, Mr. Chairman.

DIRECT EXAMINATION

BY MR. MCKEEVER:

Q Dr. Neels, I've just handed you a copy of a document entitled Testimony of United Parcel Service Witness Kevin Neels in response to Notice of Inquiry No. 4 and Presiding Officer's Information Request No. 19 and designated it as UPS-NOI/POIR-T-1. Are you familiar with that document?

A I am.

Q Do you have any changes to make to it since it was served?

A I would make one. And it has to do with the section entitled statistical methodology that begins on page 5, and the corrections have to do specifically with page 7. In this section, I present a number of ways of -- for completeness, I tried to present a number of ways of correcting an error I identified in Dr. Bozzo's work, and one of these included a variation in the means -- the de-meaning approach that he used.

I subsequently realized that the formula that I gave in the footnote on page 7 was not correct. I tried to come up with -- I did come up with what I believe is a correct formula, but I didn't have time really to check it, and I thought rather than confuse the record, I would just strike that, that alternative solution from my testimony and concentrate instead on the solution that I knew to be correct.

So the change I would like to make is to begin on line 6 of page 7 and strike out the phrase beginning transformation contained in footnote 5 and continue the strike-out onto line 7 through the word alternative, so that the revised testimony reads: "An alternative that would have avoided the error would have been to express the data in terms of deviations from facility means" and so forth. And I would also strike footnote 5, which hangs from the deleted text, and that way, I leave in only the corrections

that I'm certain are appropriate.

Q Now, you used the word de-meaning. I take it that's D-E-hyphen-M-E-A-N-I-N-G.

A That's right.

Q For the benefit of the reporter.

A Yes.

[Laughter.]

MR. MCKEEVER: Mr. Chairman, with that revision, I move that the testimony of United Parcel Service Witness Kevin Neels in response to Notice of Inquiry No. 4 and Presiding Officer's Information Request No. 19 and marked as UPS-NOI/POIR-T-1 be admitted into evidence and transcribed into the record.

CHAIRMAN GLEIMAN: Is there an objection?

[No response.]

CHAIRMAN GLEIMAN: Hearing none, if you'd please provide two copies of that material to the court reporter, I'll direct that it be received in evidence and transcribed into the record.

[Testimony of Kevin Neels Regarding NOI-4 and POIR-19, UPS-NOI/POIR-T-1, was received in evidence and transcibed into the record.]

UPS-NOI/POIR-T-1

BEFORE THE
POSTAL RATE COMMISSION

POSTAL RATE AND FEE CHANGES, 2000	DOCKET NO. R2000-1

TESTIMONY OF UNITED PARCEL SERVICE WITNESS KEVIN NEELS IN RESPONSE TO NOTICE OF INQUIRY NO. 4 AND PRESIDING OFFICER'S INFORMATION REQUEST NO. 19

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BIOGRAPHICAL SKETCH

My name is Kevin Neels. I have previously submitted testimony in this proceeding on the volume variability of mail processing labor costs (UPS-T-1) and on purchased transportation costing (UPS-T-3). My biography is set forth in that testimony. See Tr. 27/12773-74. I have also submitted rebuttal testimony on the volume variability of purchased transportation costs (UPS-RT-1).

SCOPE AND PURPOSE OF MY TESTIMONY

On August 2, 2000, the Commission issued two requests for additional information relating to mail processing costs and the study of mail processing cost variability submitted by Postal Service witness Bozzo:

1. Notice of Inquiry No. 4 ("the Notice") invited interested parties to submit statistical information and analyses comparing the model specification presented by Dr. Bozzo to other alternative specifications.

2. Referring to my calculations of error rates in the MODS data used by Dr. Bozzo (presented in my direct testimony, UPS-T-1, and in my responses to interrogatories), Presiding Officer's Information Request No. 19 asked me to extend my calculations to encompass types of errors I had not previously considered, and to comment on the extent to which the processes giving rise to these errors may have infected apparently error-free observations.

This testimony constitutes my response to these requests.

NOTICE OF INQUIRY NO. 4

1. Summary

As the Notice states (at page 1), in Docket No. R97-1 Dr. Bradley reported the results of a number of statistical tests comparing his preferred models of mail processing cost variability to a number of alternative specifications. In response to Notice of Inquiry No. 4 in that proceeding, these tests were extended to a broader range of alternative specifications. Noting that the record in the current proceeding does not address the same range of model specifications that were evaluated in R97-1, the Notice invites interested parties to test Dr. Bozzo's models against a number of alternative specifications, and to comment on the appropriate basis for selecting a preferred model from the set of available alternatives. Notice at 1-2. It asks interested parties to conduct and present the results of formal statistical tests, but also asks whether the results of such tests should constitute the sole basis for selecting a preferred specification. Id. at 3.

The Notice identified two alternative versions of Dr. Bozzo's model. The first, which was identified as "Model A," contained a complete set of facility-specific fixed effects. This was the specification preferred by Dr. Bozzo. The second model, which was identified as "Model B," lacked facility-specific fixed effects but contained a complete set of time period-specific fixed effects. The Notice requested for each model a test of the null hypotheses (a) that the fixed effects were equal (and hence could be replaced with a single constant term) and (b) that they were not fixed, but rather independently and identically distributed random variables.

In addition to these formal statistical tests, the Notice invited discussion of a number of related topics. It asked (1) whether the requested test results provided a sufficient basis for the selection of Model A over alternatives such as the "pooled" or "random effects" models; (2) whether analogous test results for Model B provided a sufficient basis for its selection; (3) whether Models A and B were nested within one another, and whether the statistical tests requested provided grounds for the selection of one model over another; and (4) whether, apart from the statistical results, there may be theoretical reasons for selecting one model over another.

2. Hypothesis Testing Framework

The models described in the Notice as A and B are not nested, in the sense that neither is a special case of the other. For this reason, there is no direct statistical test leading to the selection of one and the rejection of the other. It is possible, however, to specify a more general model that includes both Model A and Model B as special cases. In the Commission's notation, such a general model would take the following form:

$$y_{it} = \alpha_i + \gamma_t + x_{it}\beta + \varepsilon_{it} \qquad (1)$$

where the α_i represent facility-specific fixed effects and the γ_t represent time period-specific fixed effects. I will refer to this general model as "Model C."

Tests involving Model C can shed some light on the choice between Models A and B. If it were the case, for instance, that one could reject Model A in favor of Model C but could not reject Model B in favor of Model C, this would suggest that Model B would be the better specification. In effect, in such a situation the testing sequence would start with the general model and lead eventually to the more parsimonious specification provided by Model B. However, if results compel the rejection of both A

and B in favor of C, the clear implication would be that C was the better model and should be chosen over either of the alternatives.

The Notice points out that it might not be possible to include a complete set of time period specific effects in Dr. Bozzo's model because of collinearity with the variables it contains. Dr. Bozzo's model contains five variables that vary only across time and not across facilities: three seasonal dummy variables, a time trend, and a time trend squared. Perfect collinearity can be avoided by omitting these variables from the specification. In the regression runs reported below, I have eliminated them.[1] This means that the OLS model against which Model A is tested differs from the OLS model against which Model B is tested.

Statistical tests of the type requested in the Notice assume that one of the two models under consideration is correctly specified. If these models are misspecified – in particular, if both omit significant independent variables – coefficient estimates for both of the candidate models will be biased, and tests distinguishing between them will be unreliable. In the present circumstances, this precondition places important limitations on the value of the tests that have been requested. I will discuss this point and its implications in more detail below.

1. Dr. Bozzo's data set contains twenty-four time periods, only nineteen of which appear in his regression sample. The first of the other five time periods is dropped from the analysis because it coincided with significant restructuring of Postal Service systems. The other four are used to calculate the lagged values he requires. Thus, adding a full set of time period-specific effects to Dr. Bozzo's model would require nineteen terms if no constant term were present, and eighteen if a constant term were present. Restrictions necessitated by collinear variables require the elimination of an additional five terms.

3. Statistical Methodology

In estimating these models, I have followed the procedures described by Dr. Bozzo in USPS-T-15 and used a feasible generalized least squares (FGLS) procedure that corrects for first order serial correlation. In the course of responding to the Notice, I uncovered a number of errors in Dr. Bozzo's original methodology. Because the programs I had used in my earlier testimony were designed to replicate Dr. Bozzo's results, they incorporated some of the same errors. I was able to correct some of the errors in Dr. Bozzo's work, but time constraints have prevented me from reworking all of his analysis. In the results presented below, I note the instances in which there remain uncorrected errors.

Dr. Bozzo's FGLS procedure consists of three steps. First, he estimates the coefficients of the model ignoring the possibility of serial correlation. Second, from the residual vector produced in this way he computes an autocorrelation coefficient. In the third and final step, he transforms the data to eliminate the serial correlation. This transformation involves multiplication of all variables for the first observation in each run of data by $\sqrt{1-\rho^2}$, where ρ is the estimated autocorrelation coefficient.[2] He transforms subsequent observations by subtracting from each variable ρ times its value in the previous time period. Researchers using this procedure often simply drop the first observation from their analysis samples. Dr. Bozzo describes this as the "textbook

2. A "run" of data is a set of contiguous non-missing observations for a specific facility. If useable data are present for a particular time period for a specific facility, that would represent a single run. A gap in the middle would divide the data into two runs. A second gap could divide the data into three runs. Dr. Bozzo applies the $\sqrt{1-\rho^2}$ transformation to the first observation in each run.

alternative," but states that he chose not to follow it because doing so would have "adverse consequences for the statistical efficiency of the estimates."[3] He uses this general procedure to estimate his pooled, random effects, and fixed effects models.

In estimating his fixed effects model, Dr. Bozzo uses a preprogrammed function in TSP. Rather than including explicitly in the model a dummy variable for each facility, this procedure uses a computational shortcut in which each variable in his model is expressed in terms of deviations from its facility means.[4] Dr. Bozzo first runs this fixed effects estimator ignoring autocorrelation. He then computes the autocorrelation coefficient, applies the ρ transformation described above, and reruns the fixed effects estimator on the transformed data.

The first error that I uncovered affected Dr. Bozzo's coefficient estimates for his pooled and random effects models. In these models, he neglected to apply the ρ transformation to the intercept terms in his models. Had he used the "textbook alternative," his coefficient estimate for the intercept term would have been off by a multiplicative constant, but otherwise his results would not have been affected. However, by using the procedure that allowed him to retain the first observation in each run, he created a situation in which his constant term was no longer constant. His failure to transform the intercept thus means that his results are incorrect. I have corrected this oversight in the results presented below.

Later in my analysis I uncovered a second error in his fixed effects model. His first two steps are carried out correctly, yielding an appropriate estimate of the

3. Response to UPS/USPS-T15-12 (March 22, 2000).

4. Dr. Bozzo uses the fixed effects estimator in TSP, which uses this procedure.

autocorrelation coefficient. He then applies the ρ transformation to his data. When he uses the TSP panel command to express the ρ-transformed data in terms of deviations from facility means and applies ordinary least squares to the doubly transformed data, he arrives at an incorrect result. The transformation that expresses data in terms of deviations from facility means in order to solve the fixed effects out of the model does not work on the transformed data. An alternative ~~transformation contained in footnote 5 could have accomplished this.[5] Yet another alternative~~ that would have avoided the error would have been to express the data in terms of deviations from facility means, and then apply the ρ transformation and use ordinary least squares to estimate the model coefficients.

Unfortunately, I uncovered the second error too late to allow me to rerun all of the models involved in Dr. Bozzo's original testimony and in my response to the Notice. All

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5. In the standard procedure, the mean for some facility i is calculated as $\frac{\sum_{j=1}^{N_i} x_{jt}}{N_i}$, where N_i is the number of observations for facility i. This value is then subtracted from each of the x_{jt}'s. For Dr. Bozzo's transformed data, the correct value to subtract from each transformed variable x_{jt}^* is the quantity $\frac{\theta \sum_{j=1}^{N_i} x_{jt}^*}{K_i\sqrt{1-\rho^2}+(N_i-K_i)(1-\rho)}$, where K_i is the number of runs of data for facility i and θ takes the value $\sqrt{1-\rho^2}$ for the first observation in every run and (1-ρ) for subsequent observations. The denominator in this expression equals the sum of the ρ transformed dummy variables.

of the results reported below involving facility specific fixed effects contain the same error.[6]

Correction of the first error had a substantial effect on the computation of the Hausman test comparing the fixed and random effects models. In most cases when the models are estimated correctly, the difference between the fixed and random effects covariance matrices turns out not to be positive definite, and hence it cannot be inverted.[7] This is something that is known to occur with the Hausman test when the asymptotic properties of the test fail. Hence, in most cases I am unable to use the Hausman test to determine whether the random effects hypothesis can be rejected.

4. Statistical Results

Table 1 presents the results of a series of hypothesis tests relating to Model A, for each of the MODS activities. These results are derived using Dr. Bozzo's procedures, and so they reflect both his failure to apply the autocorrelation adjustment to the constant terms in his models and his error in solving out the site specific fixed effects. The first two columns present specification test results for Dr. Bozzo's preferred specification without a correction for serial correlation of the error term. The second two columns present comparable results with correction for serial correlation. The latter results are preferred by Dr. Bozzo because of low values for the Durbin-Watson statistic, a diagnostic test for serial correlation. Within each set, the first column tests

6. In the models incorporating time period-specific fixed effects, I did not use the deviation from cell means transformation. Thus, these models do not reflect this particular error.

7. In his original testimony, Dr. Bozzo did not appear to have any problem computing the Hausman statistic. However, his ability to do so appears in most cases to have been an artifact of estimating the random effects model incorrectly.

the fixed effects model against the null hypothesis that the fixed effects are equal across sites. The null hypothesis in this case corresponds to Dr. Bozzo's "pooled" or OLS regression model. The relevant test in this case is an *F* test. The first item in each cell gives the calculated F-statistic. The second gives the p-value. The third gives the number of degrees of freedom for the F-Statistic. The second column in each set tests the fixed effects model against the null hypothesis that the site-specific effects are independently and identically distributed random variables. In this case, the relevant test is the Hausman test. The first entry in each cell gives the calculated chi-squared value. The second gives the p-value. The third gives the number of restrictions.

The results shown in Table 1 provide strong support for the fixed effects model. Regardless of whether or not a correction is made for serial correlation, the pooled model is strongly rejected in favor of the fixed effects model. The random effects model is similarly rejected in favor of the fixed effects model. In all cases, the alternative models are rejected by a large margin.

Table 2 presents comparable results for Model A after correcting for Dr. Bozzo's error in failing to apply the autocorrelation correction to the constant terms in his models. These revised results still reject the OLS model in favor of the fixed effects model, although the margins by which the OLS models are rejected are slightly reduced. Correction of the error has a marked effect, however, on the test of the fixed effects model against the random effects model when the two are estimated correcting for serial correlation. Correction of Dr. Bozzo's error results in a situation in which the Hausman statistic cannot be computed. It is for this reason that column 4 is blank. The

same situation arose in all subsequent tests of the random effects specification, and for this reason I have omitted these tests from the results reported below.

Table 3 presents results for a series of hypothesis tests relating to Model B. The first column presents results for models without correction for serial correlation. The second presents results for models with correction for serial correlation. The latter results, and all comparable results reported below, reflect an appropriate autocorrelation adjustment of the constant term and a correction of Dr. Bozzo's first error. Because I estimated the version B models by explicitly including time period dummy variables rather than using the computational shortcut employed by Dr. Bozzo, these results are not subject to his second error.

These results test Model B with time-specific fixed effects against the alternative pooled regression model in which the time period-specific effects are equal across all time periods. They indicate that in a comparison between the pooled model and the fixed effects model, the fixed effects model is preferred.

Since the pooled, or OLS, regression model was rejected in favor of the fixed effects regression model for Models A and B, it is not possible, from the results presented thus far, to choose between Models A and B. For this reason, I have conducted an additional series of comparisons between these models and the more general Model C described above. Results of these comparisons are presented in Table 4. As in prior tables, the first set of columns present specification test results from regressions run without correction for serial correlation, while the second set presents results from regressions run with such correction. Within each set, the first column tests the fixed effects regression model for Model C against the null hypothesis of the pooled

regression model. The second column tests Model C against the null hypothesis of Model A. The final column tests Model C against the null hypothesis of Model B. As noted above, the models involving facility specific fixed effects (Models A and C) are estimated using Dr. Bozzo's erroneous procedure.

The results presented in Table 4 support the fixed effects estimator for Model C over all of the alternatives for all MODS activities, except Manual Flats. The null hypothesis of the pooled regression model is rejected in favor of the fixed effects regression model for Model C in all cases. The null hypothesis of fixed effects for Model A is rejected in favor of fixed effects for Model C for all MODS activities, except Manual Flats. The null hypothesis of fixed effects for Model B is rejected in favor of fixed effects for Model C in all cases. Thus, from a strictly statistical standpoint, Model C with fixed effects estimation emerges as the clear winner for all but the Manual Flats MODS group. For Manual Flats, the Model A with fixed effects is the winner in the sense that this simpler specification cannot be rejected.

Table 5 compares the volume variabilities implied by these models. All of the variabilities are derived from models estimated with correction for serial correlation. Moving from the Model A fixed effects to Model B fixed effects raises the estimated volume variability in all cases except one. In some instances, the changes are fairly dramatic. The estimated volume variability for Manual Parcels, for example, goes from 0.522 to 0.641. That for Priority Mail goes from 0.522 to 0.641. The addition of time period-specific effects to Model A has the effect of reducing volume variability slightly in five of the nine MODS activities.

5. Interpretation and Discussion

The general conclusion, from a strictly statistical standpoint, is that the preferred model among those investigated is the fixed effects regression model for Model C, although the remaining error in the estimation of the fixed effects models leaves me unsure as to how valid or robust this finding really is. From this narrow viewpoint, there is little else to be said. The pattern of results presented raises some questions, however, about just what is going on in Dr. Bozzo's models.

In every instance in which a set of "dumb" variables is added to Dr. Bozzo's models, they appear to take statistically significant coefficients. In his original model, he included time trends and facility-specific fixed effects. In response to the Notice, I have added time period-specific fixed effects, and they also have turned out to be statistically significant. Even with time period-specific fixed effects and time trends, the models show evidence of serial correlation of the error term. One is left to wonder whether other as-yet unexplored possibilities might turn out to be statistically significant. Clusters of facility-time period interactions? Higher-order autocorrelation?

These changes in model specification sometimes have substantively important effects on estimates of volume variabilities. The question of what really belongs in the model thus appears to be an important one.

A clear implication of the tendency of these "dumb" variables to take statistically significant coefficients is that there is much going on in the labor hour data that is not explained well by the substantively important parts of Dr. Bozzo's model. This is hardly surprising, given the parsimoniousness of his specification. In addition to piece handlings, his model contains the manual ratio and his capital index – two variables that

I would regard as endogenous, rather than as independent determinants of labor demand. His wage variables are only weakly related to labor hours. Only the delivery points variable appears to play a strong role in the models.

In my direct testimony, I commented extensively on aspects of the Postal Service's response to volume changes that are nowhere represented in Dr. Bozzo's models.[8] Dr. Bozzo fails to account fully for the interactions among activities within a plant, and his analysis ignores the likelihood that the mix of sorting technologies within a plant will change systematically with growth in volume. From a theoretical standpoint, I thought it likely that his models were misspecified. The pattern of results presented above is consistent with that opinion. If his model is misspecified, it is likely that dummy variables, time trends, and serial correlation coefficients will pick up some of the effects of the omitted variables and, as a result, take statistically significant coefficients.

In this context, it is worth repeating the cautions expressed above regarding the unreliability of these statistical tests in the presence of misspecification. If Dr. Bozzo's models are misspecified, his coefficient estimates are biased and all of the tests reported above are unreliable. I believe that this is likely to be the case.

6. Are There Theoretical Reasons for Rejecting Model A?

The Notice invites discussion of the question "whether, even with the rejection of the hypotheses described in a), there may be theoretical grounds for concluding that a rejected model could provide a better estimate of variability than either model A or B." Notice at 3, ¶ f. Such grounds do exist. They have to do with the appearance on the right hand side of the regression equation of endogenous variables under the control of

8. UPS-T-1, pages 21-23, Tr. 27/12793-95.

the Postal Service. I alluded to them in my response to interrogatory USPS/UPS-T1-13(b), Tr. 27/12936-38. Portions of that response are worth repeating here:

> Many aspects of postal operations are likely to affect the structural relationship between mail processing labor costs and mail volume. However, many such aspects of postal operations -- including capital intensity, choice of sorting technology and the structure and organization of the mail processing network -- are under the control of the Postal Service, and likely themselves to change systematically in response to changes in mail volume. Simply including such explanatory variables in the regression model without accounting properly for their endogeneity is likely to lead to simultaneity bias. Moreover, even if the econometric problems associated with the inclusion of right hand side endogenous variables could be adequately resolved, the resulting structural model would produce incomplete results. It would capture the direct effects of volume on labor costs, holding other decision variables constant. However, it would exclude the indirect effects exerted by volume growth through its influence on these other decision variables.
>
> In such a situation the appropriate econometric model is a reduced form model that excludes from the right hand side all endogenous variables. The estimated coefficient on volume in such a model captures both the direct and indirect effects of volume on labor cost. The result is a more comprehensive measure of the volume variability of labor costs, and one that comes closer to meeting the requirements of the Commission.

The variability regressions presented by Dr. Bozzo contain a number of endogenous right hand side variables. These include the manual ratio, which measures the way in which the incoming mail stream is allocated between manual and automated sorting activities. They also include Dr. Bozzo's capital index, which clearly reflects Postal Service investment decisions. When Dr. Bozzo computes volume variabilities, he relies upon regression coefficients that control for the effects of changes in these endogenous variables and that effectively give the volume variability of labor hours *holding the manual ratio and the capital index constant.*

That said, the manual ratio and the capital index do not play a large role in Dr. Bozzo's analysis. Although they are generally significant in a statistical sense, their

measured effects on labor hours are generally modest. Their modest role is probably in large part an artifact of the way in which they are measured. Dr. Bozzo's capital index is not limited to equipment relevant to a particular MODS activity, but rather represents a comprehensive measure of the amount of equipment present in the entire plant, with the plant thrown in as well. Given that so much equipment irrelevant to the particular MODS activity is included in this measure, it is somewhat surprising that there is a significant relationship at all.[9]

In my direct testimony on mail processing, I identified a number of ways in which the Postal Service responds to growth in volume that are not addressed by Dr. Bozzo's study. These include installation of automated processing activities in plants,[10] as well as expansions and/or modifications of plants, or the construction of new plants.[11] Variables describing these aspects of the Postal Service's response to volume changes do not appear explicitly in Dr. Bozzo's model. Since his analysis looks only at processing activities that are up and running, we never observe the installation and initiation of a new processing activity.

Dr. Bozzo's fixed effects coefficients measure aspects of labor hour demand that do not vary in response to quarter-to-quarter changes in piece handlings. There is disagreement, however, over whether they reflect, in whole or in part, Postal Service design and operational decisions that respond over a longer time period to expectations

9. OCA witness Smith has also criticized Dr. Bozzo's capital index for its reliance on accounting based depreciation rates that may have little or nothing to do with the actual loss of physical productivity that occurs over time. See OCA-T-4, page 34, line 16, through page 35, line 17, Tr. 27/13183-84.

10. UPS-T-1, pages 9-16, Tr. 27/12781-88.

11. UPS-T-1, pages 16-18, Tr. 27/12788-90.

regarding the volume of mail to be processed within a plant. OCA witness Smith noted the Commission's finding in Docket No. R97-1 that "the fixed effects in Dr. Bradley's study may represent effects that are both related and unrelated to volume."[12] Elsewhere in his testimony, Dr. Smith emphasized the importance of accounting appropriately for the characteristics of the longer-run expansion path mapping out the optimal combination of labor and capital for different levels of expected mail processing volume.[13] In my own direct testimony on mail processing costs, I discussed the way in which processing technology might change systematically in response to changes in mail volume.[14] Dr. Smith argues for use of Dr. Bozzo's "between" model on the argument that it is most likely to show the relationship between volume and cost as plant size varies.[15]

It is certainly reasonable to argue that when the Postal Service opens a new plant, it designs the plant to handle the volume of mail that plant is expected to process. It is also reasonable to expect anticipated volumes to trigger upgrading decisions, and to influence the characteristics of the plant that emerges from the upgrading process. It is likely, therefore, that when viewed in the cross-section, the different plants in the Postal Service's network represent different points on witness Smith's expansion path. In other words, they will be designed to accommodate different mail processing volumes. These design decisions are an important part of the Postal Service's long run

12. OCA-T-4, page 16, lines 1-2, Tr. 27/13165.
13. OCA-T-4, page 40, lines 14-18, Tr. 27/13189.
14. UPS-T-1, pages 11-14, Tr. 27/12783-86.
15. OCA-T-4, page 64, lines 7-12, Tr. 27/13213.

response to changes in volume. They will be reflected in plant size, layout, automation strategy, and many other attributes not explicitly represented in Dr. Bozzo's model.

Since volume-related plant design decisions change slowly and infrequently and are not represented explicitly in Dr. Bozzo's model, it is likely that they are captured in large part by his fixed effects. One can think conceptually of decomposing his fixed effects into two parts. One part would represent the truly fixed effects that would never change with volume. An example might be a location within an urban area. The remainder, however, would reflect volume-related aspects of plant design, such as the fact that in an urban area, the Postal Service will tend to build a large plant to process the large volumes of mail it can expect to have to process.

If it were true that volume-related design decisions account for most of the fixed effects estimated by Dr. Bozzo, these could be regarded as endogenous variables that are actually under the control of the Postal Service. In such a case, the argument presented above would apply. The appropriate measure of volume variability would reflect both the effects of long term volume growth on the number, size, and configuration of the plants in the processing network, as well as the effects of short term changes in the volume of mail processed within those plants. In such a situation, dropping the fixed effects could be regarded as the equivalent of running a reduced form model.

PRESIDING OFFICER'S INFORMATION REQUEST NO. 19

1. Summary

Presiding Officer's Information Request No. 19 ("the Request") cites my response to Interrogatory USPS/UPS-T1-10(b) (Tr. 27/12921-25) in which I discussed the

incidence of errors in the MODS data used by Dr. Bozzo. In that response, I pointed out that the discussion of error rates contained in Dr. Bozzo's testimony understates the incidence of erroneous data, by failing to include in his count of errors observations lost because of missing or negative values for the variables key to his analysis. I also presented revised estimates of MODS data error rates that include the errors omitted from his calculations.

The Request notes that in addition to the types of errors described in my response to USPS/UPS-T1-10(b), there are also instances in the data in which Total Pieces Handled ("TPH") are greater than Total Pieces Fed ("TPF"). As explained by Dr. Bozzo, TPF represents the number of pieces of mail fed into a distribution operation, while TPH represents the number of pieces successfully sorted; the difference between the two, if any, consists of pieces jammed, pieces misfed, or pieces which for some other reason (such as the presence of unreadable addresses or barcodes) are incapable of being sorted.[16] By definition, TPF should always be greater than or equal to TPH. This, however, is not always the case in Dr. Bozzo's data set.

The Request asks a number of specific questions. It asks what meaning can be attached to non-positive values of TPH and TPF, and if there is any way to determine if positive values of TPH and TPF are infected by the sources of measurement error that give rise to the observed non-positive values. It asks also whether observations in which TPH is greater than TPF are indications of data errors. If so, it asks for an updated version of the table prepared in my response to USPS/UPS-T1-10(b) that reflects this additional source of error. It also asks whether there is any way to

16. USPS-T-15 at pages 50-52. Note that for manual operations, TPF and TPH are identical. They can differ only for automated activities.

determine whether observations in which TPF equals or exceeds TPH are infected by the same sources of measurement error that cause TPH to sometimes exceed TPF. It asks what the answers to the above questions imply for the variability analysis introduced by Dr. Bozzo, and whether the resulting bias is likely to be greater for the fixed effects model than for other models, such as the between model.

2. Patterns of Error within the MODS Data

In developing his econometric models of mail processing labor hour variability, Dr. Bozzo relies upon three variables drawn from the MODS data files: Labor Hours, Total Pieces Fed (TPF), and Total Pieces Handled (TPH). Logically, one would expect to see positive values for all three variables if a MODS activity were up and running at a site during a particular time period. Moreover, because of the definitions of TPH and TPF, one would expect that TPF should always be greater than or equal to TPH. Conversely, if a MODS activity is not present, values for all three variables should equal zero.

There are numerous instances in which the expected relationships among hours, TPH, and TPF do not hold. Hours are sometimes positive when TPH equals zero. The reverse relationship also holds. TPH and TPF frequently disagree in implausible ways. The source and significance of these errors is not clear. In his response to an interrogatory, Dr. Bozzo noted that manual parcel and priority volumes must be logged manually, and he suggested that gaps in the data for at least one specific site may have arisen because an in-plant support position was not filled.[17] In his direct testimony, he

17. Response to UPS/USPS-T15-13, Tr. 15/6387-88.

states that some sites appear to have systematically underreported TPF relative to TPH, although he gives no explanation of why this may have occurred.[18]

3. Interpretation of Non-Positive Values

In the data set produced by Dr. Bozzo, zero values have an ambiguous interpretation. They can represent either true zeros, or missing values. On the presumption that once activities are in place they tend to operate consistently rather than starting and stopping, I have treated runs of zeros at either the start or the end of the data for a site as true zeros, and runs of zeros that are embedded between positive values as missing values that represent failures of the MODS reporting system. How these missing values occur is not clear. The statements by Dr. Bozzo referred to above suggest that at times the reporting system simply breaks down. Apparently, these reporting failures can affect all of the variables used by Dr. Bozzo, or only some of them.

In principal, negative values have no proper place within the MODS data. However, they appear with some regularity. Their significance is not clear. I have seen instances in working with other data systems in which entries made to adjust prior period errors sometimes show up as negative values in the current period, and I suspect that some similar explanation may account, at least in part, for the presence of such negative values in the MODS data. The MODS manual does refer to procedures for making adjustments to prior period values.[19]

18. USPS-T-15, page 108, lines 4-6.

19. Management Operating Data System, Handbook M-32, Docket No. R97-1, USPS-LR-H-147, Section 432.1.

4. Are Other Observations Infected by the Problems Causing Non-Positive Values?

The problems that give rise to non-positive values for hours, TPH, or TPF could affect other apparently correct observations if the underlying reporting system records data at a finer level of aggregation than that used by Dr. Bozzo. His observations are quarterly and represent aggregations of Postal Service four-week accounting periods. If data were reported on a weekly basis, it would be possible for zero or negative values to appear in one of the four weeks of an accounting period and to be masked when data for the four weeks were aggregated together to produce accounting period totals.

It is clear that this possibility exists within Dr. Bozzo's data. He aggregated Postal Service accounting periods to arrive at his quarterly totals. I note also that the MODS manual appears to provide for the reporting of data at the day, tour, week, or accounting period level.[20] Unless there is some procedure within MODS that checks for errors before aggregating to a higher level, it is highly likely that some apparently correct observations contain hidden errors.

I know of no way from the presently available data to determine how extensive this problem is. The only way to determine the extent of this problem with any confidence would be to start with data at the finest level of aggregation available and check for errors at each stage of aggregation. Even such an extensive effort as this, however, would not necessarily identify the full extent of the problem. Reporting error and omissions could remain even within the finest level of aggregation maintained by the system. It is possible, for example, that at the end of each shift it is necessary to

20. Management Operating Data System, Handbook M-32, Docket No. R97-1, USPS-LR-H-147, Section 131.

enter piece counts from several different machines. For any number of different reasons, there could be a failure to report data for a particular machine that would be masked by the presence of data from the machines whose results were reported.

5. Do Observations in Which TPH Exceed TPF Represent Errors?

Observations in which TPH exceed TPF clearly represent errors. For automated operations, it is clear, even from Dr. Bozzo's testimony, that TPH should not exceed TPF. For manual operations, there is no meaningful distinction between TPH and TPF, and TPF should simply equal either TPH or zero.

In response to the request for an expanded version of "Table in Response to USPS/UPS-T-10(b)" (Request, page 2), I investigated the TPH and TPF data series in both the analysis sample used in the regressions and in the larger sample of observations provided by Dr. Bozzo. A summary of my findings is presented in Tables 6 and 7.

Table 6 reports the percent of sample observations that exhibit MODS data errors. Column 4 presents Dr. Bozzo's calculation of the fraction of observations that fail the threshold and productivity checks. Dr. Bozzo investigates errors only in the sample of observations used in his regression analysis. He ignores the fact that certain observations were omitted from the regression sample because of data errors. Column 5 expands the universe over which the threshold and productivity error rates are calculated to include in the "non-missing" set those observations that would have been non-missing but for bad MODS data. In response to the Request, Column 6 expands the types of errors which are investigated to include instances in which TPH > TPF.

Table 7 reports the prevalence of MODS data errors for each MODS group over the set of all observations that demonstrate the presence of the MODS activity. Detecting the presence of MODS activity is complicated by the fact that Dr. Bozzo's data codes both missing values and non-present (truly zero) activities as zero. The MODS activity is considered to be present if at least one of the three MODS variables (TPH, TPF, or Hours) is strictly positive, or if at least one of the three MODS variables is an intermittent non-positive number, as explained in the workpapers accompanying my original testimony.

6. Are Other Observations Infected by the Problems that Cause TPH to Exceed TPF?

As explained above for non-positive values, the problems that cause TPH to exceed TPF could affect other apparently correct observations. If the underlying reporting system records data at a finer level of aggregation than that used by Dr. Bozzo, as described above, then it may well be that data errors are masked when data are aggregated to produce accounting period totals.

7. Are These Data Errors Likely to Produce Greater or Lesser Bias in the Fixed Effects Model Than in Other Models?

Measurement error in the right hand side variables of the regression model destroys the statistical properties of the panel estimators. While there exists some simulation evidence to suggest that there may be a trade-off in the relative bias of the different panel estimators, there is in general no way to determine which model is likely to produce greater or lesser bias.[21]

21. See E. Biorn, "The Bias of Some Estimators for Panel Data Models with Measurement Errors," Empirical Economics, vol. 17, 1992, pp. 51-66.

Table 1

Specification Tests for Model A

F-statistic comparing OLS to FE, Hausman test statistic comparing FE to RE.

MODS Group	Without Correction for Serial Correlation		With Correction for Serial Correlation	
	OLS vs FE [1]	FE vs RE [2]	OLS vs FE [3]	FE vs RE [4]
OCR	35.044	136.807	7.420	110.223
	(0.000)	(0.000)	(0.000)	(0.000)
	288 | 4762	38	288 | 4761	38
LSM	18.987	90.161	6.569	76.316
	(0.000)	(0.000)	(0.000)	(0.000)
	272 | 3583	38	272 | 3583	38
BCS	35.455	69.768	9.145	72.104
	(0.000)	(0.001)	(0.000)	(0.001)
	296 | 5056	38	296 | 5055	38
Manual Letters	44.211	191.995	10.631	168.657
	(0.000)	(0.000)	(0.000)	(0.000)
	298 | 5163	38	298 | 5162	38
FSM	45.575	172.756	11.660	76.862
	(0.000)	(0.000)	(0.000)	(0.000)
	234 | 4084	38	234 | 4084	38
Manual Flats	39.858	258.642	9.145	123.051
	(0.000)	(0.000)	(0.000)	(0.000)
	276 | 4564	38	276 | 4564	38
SPBS	53.546	60.420	15.917	50.547
	(0.000)	(0.001)	(0.000)	(0.015)
	93 | 1445	31	93 | 1445	31
Manual Parcels	41.583	119.299	12.898	83.131
	(0.000)	(0.000)	(0.000)	(0.000)
	180 | 2812	31	180 | 2811	31
Priority	27.197	108.282	9.642	83.057
	(0.000)	(0.000)	(0.000)	(0.000)
	199 | 3010	31	199 | 3009	31

Notes:

1. Model A is specified as $y_{it} = \gamma_t + \alpha_i + X_{it}\beta + \varepsilon_{it}$, where $\gamma_t = \gamma$ for all t.
2. Columns [1] and [3]: F-statistics shown on first line of each cell. P-values shown in parentheses on second line. Number of restrictions and degrees of freedom shown on third line.
3. Columns [2] and [4]: Hausman test statistics shown on first line of each cell. P-values shown in parentheses on second line. Degrees of freedom shown on third line.

Table 2

Specification Tests for Model A

With Correction to Dr. Bozzo's FGLS Transformation

F-statistic comparing OLS to FE, Hausman test statistic comparing FE to RE.

MODS Group	Without Correction for Serial Correlation		With Correction for Serial Correlation	
	OLS vs FE [1]	FE vs RE [2]	OLS vs FE [3]	FE vs RE [4]
OCR	35.044	136.807	7.354	
	(0.000)	(0.000)	(0.000)	
	288 | 4762	38	288 | 4761	
LSM	18.987	90.161	6.463	
	(0.000)	(0.000)	(0.000)	
	272 | 3583	38	272 | 3583	
BCS	35.455	69.768	9.029	
	(0.000)	(0.001)	(0.000)	
	296 | 5056	38	296 | 5055	
Manual Letters	44.211	191.995	10.561	
	(0.000)	(0.000)	(0.000)	
	298 | 5163	38	298 | 5162	
FSM	45.575	172.756	11.547	
	(0.000)	(0.000)	(0.000)	
	234 | 4084	38	234 | 4084	
Manual Flats	39.858	258.642	9.134	
	(0.000)	(0.000)	(0.000)	
	276 | 4564	38	276 | 4564	
SPBS	53.546	60.420	15.841	
	(0.000)	(0.001)	(0.000)	
	93 | 1445	31	93 | 1445	
Manual Parcels	41.583	119.299	12.865	
	(0.000)	(0.000)	(0.000)	
	180 | 2812	31	180 | 2811	
Priority	27.197	108.282	9.655	
	(0.000)	(0.000)	(0.000)	
	199 | 3010	31	199 | 3009	

Notes:

1. Model A is specified as $y_{it} = \gamma_t + \alpha_i + X_{it}\beta + \varepsilon_{it}$, where $\gamma_t = \gamma$ for all t.
2. Columns [1] and [3]: F-statistics shown on first line of each cell. P-values shown in parentheses on second line. Number of restrictions and degrees of freedom shown on third line.
3. Column [2]: Hausman test statistics shown on first line of each cell. P-values shown in parentheses on second line. Degrees of freedom shown on third line.
4. Column [4]: It was not possible to compute the Hausman statistic in these instances.

Table 3

Specification Tests for Model B

With Correction to Dr. Bozzo's FGLS Transformation

F-statistic comparing OLS to FE, Hausman test statistic comparing FE to RE.

MODS Group	Without Correction for Serial Correlation		With Correction for Serial Correlation	
	OLS vs FE		OLS vs FE	
OCR	5.973		4.963	
	(0.000)		(0.000)	
	18	5037	18	5036
LSM	2.242		4.451	
	(0.002)		(0.000)	
	18	3842	18	3842
BCS	3.188		7.925	
	(0.000)		(0.000)	
	18	5339	18	5338
Manual Letters	4.323		22.734	
	(0.000)		(0.000)	
	18	5448	18	5447
FSM	5.847		15.301	
	(0.000)		(0.000)	
	18	4305	18	4305
Manual Flats	2.489		4.047	
	(0.000)		(0.000)	
	18	4827	18	4827
SPBS	2.294		8.293	
	(0.002)		(0.000)	
	18	1525	18	1525
Manual Parcels	2.177		6.527	
	(0.003)		(0.000)	
	18	2979	18	2978
Priority	2.895		9.891	
	(0.000)		(0.000)	
	18	3196	18	3195

Notes:

1. Model B is specified as $y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \varepsilon_{it}$, where $\alpha_i = \alpha$ for all i.

2. F-statistics shown on first line of each cell. P-values shown in parentheses on second line. Number of restrictions and degrees of freedom shown on third line.

Table 4

Specification Tests Comparing Models With and Without Time-Specific Effects and Site-Specific Effects

MODS Group	Without Correction for Serial Correlation			With Correction for Serial Correlation		
	OLS *vs* Model C FE	Model A FE *vs* Model C FE	Model B FE *vs* Model C FE	OLS *vs* Model C FE	Model A FE *vs* Model C FE	Model B FE *vs* Model C FE
	[1]	[2]	[3]	[4]	[5]	[6]
OCR	33.825	2.863	35.246	7.190	1.877	7.367
	(0.000)	(0.000)	(0.000)	(0.000)	(0.028)	(0.000)
	301 | 4749	13 | 4749	288 | 4749	301 | 4748	13 | 4748	288 | 4748
LSM	18.415	3.231	19.051	6.566	4.023	6.544
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	285 | 3570	13 | 3570	272 | 3570	285 | 3570	13 | 3570	272 | 3570
BCS	34.697	6.341	35.938	9.069	6.179	9.073
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	309 | 5043	13 | 5043	296 | 5043	309 | 5042	13 | 5042	296 | 5042
Manual Letters	43.338	7.284	45.009	10.248	6.494	10.192
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	311 | 5150	13 | 5150	298 | 5150	311 | 5149	13 | 5149	298 | 5149
FSM	43.471	2.273	45.737	11.241	4.903	11.473
	(0.000)	(0.006)	(0.000)	(0.000)	(0.000)	(0.000)
	247 | 4071	13 | 4071	234 | 4071	247 | 4071	13 | 4071	234 | 4071
Manual Flats	38.160	1.327	39.893	8.801	1.290	9.163
	(0.000)	(0.189)	(0.000)	(0.000)	(0.211)	(0.000)
	289 | 4551	13 | 4551	276 | 4551	289 | 4551	13 | 4551	276 | 4551
SPBS	48.365	3.318	54.735	14.702	3.045	16.285
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	106 | 1432	13 | 1432	93 | 1432	106 | 1432	13 | 1432	93 | 1432
Manual Parcels	39.497	3.625	42.004	12.385	4.514	12.955
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	193 | 2799	13 | 2799	180 | 2799	193 | 2798	13 | 2798	180 | 2798
Priority	26.242	4.802	27.685	9.604	6.329	9.945
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	212 | 2997	13 | 2997	199 | 2997	212 | 2996	13 | 2996	199 | 2996

Notes:

1. The general model, denoted as Model C, is specified as $y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \varepsilon_{it}$, where α_i is a site-specific effect and γ_t is a time-specific effect.

   Model A, or Bozzo's model, is specified as $y_{it} = \gamma_t + \alpha_i + X_{it}\beta + \varepsilon_{it}$, where $\gamma_t = \gamma$ for all t.

   Model B is specified as $y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \varepsilon_{it}$, where $\alpha_i = \alpha$ for all i.

   The OLS model is specified as $y_{it} = (\alpha + \gamma) + X_{it}\beta + \varepsilon_{it}$.

2. F-statistics shown on first line of each cell. P-values shown in parentheses on second line. Number of restrictions and degrees of freedom shown on third line.

Table 5

Estimated Volume Variabilities

MODS Group	Bozzo's Results Model A Fixed Effects FGLS	Corrected FGLS Transformation Model B Fixed Effects FGLS	Corrected FGLS Transformation Model C Fixed Effects FGLS
	[1]	[2]	[3]
OCR	0.751	0.847	0.735
	(0.038)	(0.038)	(0.039)
LSM	0.955	0.932	0.970
	(0.021)	(0.026)	(0.022)
BCS	0.895	0.919	0.867
	(0.030)	(0.028)	(0.030)
FSM	0.817	0.926	0.837
	(0.026)	(0.022)	(0.026)
Manual Flats	0.772	0.833	0.766
	(0.027)	(0.025)	(0.028)
Manual Letters	0.735	0.825	0.733
	(0.024)	(0.024)	(0.024)
SPBS	0.641	0.742	0.654
	(0.045)	(0.043)	(0.046)
Manual Parcels	0.522	0.641	0.513
	(0.028)	(0.032)	(0.028)
Priority	0.522	0.641	0.507
	(0.025)	(0.026)	(0.025)

Notes:

1. Random effects estimation for site-specific error component.
2. Standard errors shown in parentheses.

Table 6
Expanded Table in Response to USPS-UPS-T1-10

MODS Group	Non-Missing	Threshold	Threshold and Productivity	% of Observations Exhibiting Data Errors		
				Ignoring Non-Positive MODS Data	Accounting for Non-Positive MODS data	Accounting for TPH > TPF
	(1)	(2)	(3)	(4)	(5)	(6)
OCR	6642	6637	6493	2.24%	3.19%	3.40%
LSM	5155	5149	5126	0.56%	6.94%	7.59%
BCS	6882	6880	6777	1.53%	1.54%	2.98%
FSM	5441	5441	5423	0.33%	1.00%	9.46%
Manual Flats	6910	6910	6416	7.15%	7.16%	
Manual Letters	6910	6910	6820	1.30%	1.32%	
SPBS	2241	2236	2210	1.38%	8.45%	10.85%
Manual Parcels	5831	5621	4709	19.24%	28.07%	
Priority	5713	5640	4992	12.62%	22.04%	

Notes and Sources:

1. Data from USPS-T-15 (revised 3/22/00) and Reg9398.xls in USPS-LR-I-107.
2. "% of Observations Exhibiting Data Errors" columns show the percentage of observations exhibiting gross data errors when properly accounting for true missing value and bad TPH or work hours data.
3. Column (5) counts as bad data observations with complete non-MODS data, but non-positive values for either TPH or HRS.
4. Column (6) counts as bad usable observations (after the threshold and productivity scrubs) with TPH > TPF.

Table 7
MODS Data Quality

Description	OCR	LSM	BCS	Manual Letters	FSM	Manual Flats	SPBS	Manual Parcels	Priority
Sample Size	7140	6132	7472	7570	5963	7556	2771	7274	6908
TPH > 0, HRS ≤ 0	0.08	0.08	0.03	0.03	0.07	0.01	0.29	3.18	0.84
TPH ≤ 0, HRS > 0	0.77	6.21	0.24	0.21	0.62	0.21	7.33	7.23	9.87
TPH ≤ 0, HRS ≤ 0	0.53	3.02	0.16	0.23	1.58	0.09	5.52	2.63	1.84
TPH > 0, HRS > 0 Threshold failure	0.08	0.13	0.15	0.03	0.10	0.04	0.18	2.98	1.09
TPH > 0, HRS > 0 Productivity failure[1]	2.10	0.42	1.55	1.59	0.59	7.28	1.16	16.00	10.54
TPH > TPF	0.41	0.83	1.57		8.47		2.17		
TPF > 0, TPH =/ TPF				21.10		18.69		3.46	4.91
Overall % of MODS Data Exhibiting Error	3.95	10.62	3.57	22.84	11.34	24.38	16.46	32.05	27.26

Notes:
1. Productivity defined using original MODS data. Productivity bounds taken from USPS-T-15.
2. Threshold failure defined as hours greater than zero, but less than 40.

CHAIRMAN GLEIMAN: The United States Postal Service is the only party that has indicated that it wishes to cross-examine Dr. Neels. Is there anyone else?

[No response.]

CHAIRMAN GLEIMAN: Ms. Duchek?

MS. DUCHEK: Thank you, Mr. Chairman.

CROSS-EXAMINATION

BY MS. DUCHEK:

Q Good late evening, Dr. Neels.

A Good evening, Ms. Duchek.

Q Would you turn to table 5 of your testimony, please? And it's an unnumbered page, but I think it's about the third page from the end.

A I see it.

Q My questions just concern your labeling. The column headings for model B and model C include the words fixed effects; is that correct?

A That is correct.

Q But I'm a little concerned by note 1. Does that note below the table indicate that the site-specific effects were estimated with the random effects models for the results you report?

A I can see why you're confused, and it doesn't mean that. And I apologize for not having caught that. When we were testing the model, there were certain MODS pools in

which it appeared that you could not reject the random effects model. This would be the version of model C where the site-specific effects were fixed, and the time period specific effects were random. And I think that this corresponded to versions of the model when we were still kind of getting some of the bugs out.

And I think in an earlier version of this table, there were certain rows that had random time specific effects, and I didn't catch that in the final edit. I apologize for that. That should really be deleted. Thank you.

Q So the model B and C results are fixed effects.

A The model B and C results are fixed effects; that's right. I should mention also by way of clarification that the corrected FGLS transformation in that heading really applies to model B, because this is the first of the two corrections that I cited where the error arose from failure to transform the constant term in the model. So that applies to model B. And model C, where there were fixed effects, there was no constant term. So that -- we didn't actually get to the correction on that one.

Q Would you turn to page 11 of your testimony, please?

In particular, I'm looking at the discussion lines 15 through 22.

A Okay. I see that.

Q You indicate that your model B results in higher variabilities than model A for most cost pools, correct?

A Yes.

Q And then, turning back to page 10, lines 7 through 10, and based on your recent clarification, are your model -- is it correct that your model B results are not subject to either of the computational errors you identify?

A That is correct.

Q And are any of those model B variabilities greater than 100 percent?

A No.

Q Is it correct that the feasible generalized least squares or FGLS estimation procedure implemented by Dr. Bozzo consists of several stages?

A It is.

Q And are those stages summarized on page 5, lines 11 through 14 of your testimony?

A The -- yes, although I think that's -- in those particular lines, I guess there is -- it's implied that in the third and final step, it transforms the data and then actually runs an additional regression.

Q Understood.

A Okay.

Q Is it your understanding that any FGLS estimation

procedure generally would consist of those same three steps?

A That is correct.

Q And is it your further understanding that here, the FGLS procedure was implemented by Dr. Bozzo to adjust for auto-correlation of the regression disturbances?

A Generally, that's -- yes.

Q Would you also agree that the first stage or step could be characterized as estimating the models by ordinary least squares without the auto-correlation adjustment?

A That is correct.

Q Okay; for the following series of questions, I want to make very clear that I'm not asking you to endorse Dr. Bozzo's overall modeling approach. I'm just asking you about the technical implementation of his models.

A Understood.

Q And is it your testimony at page 6, lines 20 to 21, that this first stage was correctly implemented in Dr. Bozzo's calculations?

A In the first stage, page -- what page and lines again?

Q Page 6, lines 20 to 21.

A Okay; in the fixed effects model, the first two steps, which I guess would involve what you described earlier as the ordinary least squares estimation and then the calculation of the auto-correlation coefficient, yes.

Q Correct.

And do you implement the first stage for your models B and model C FGLS estimates in table 5?

A I do.

Q Suppose we were to stop just at this stage. Given Dr. Bozzo's modeling approach, would the unadjusted ordinary least squares parameter estimates that enter the variability equations be statistically consistent or unbiased whether or not there is auto-correlation?

A They would be; their standard errors would be misstated, but the coefficients themselves would be unbiased and consistent.

Q So would it be fair to say that the reason one adjusts for auto-correlation is not the need to obtain consistent or unbiased estimates but rather the desire to obtain statistically more efficient estimates?

A That is correct.

Q And again, given Dr. Bozzo's modeling approach, would the results from models without the auto-correlation adjustment be both statistically consistent or unbiased and free from the technical errors you identified?

A His fixed effects models would be. The ones where he had a constant term, they would be affected by the first error -- well, no, excuse me; I take it back. Even in his first models, the ordinary least squares estimates would be

unbiased and consistent.

Q Dr. Neels, is it your understanding that variability estimates from model A unadjusted for auto-correlation were provided in Postal Service Library Reference 107? Do you recall?

A I believe they were.

Q And do you also recall that Dr. Elliot provided model C results without the auto-correlation adjustment for some cost pools in his testimony responding to NOI number 4?

A I don't remember whether or not he used auto-correlation.

[Pause.]

Q Have you estimated models A, B and C without the auto-correlation adjustment in the programs you supplied in your work papers?

A They -- that would have been a step along the way, so yes.

Q And do you know what the variabilities resulting from those models were?

A I don't recall as I sit here.

Q Okay; and do I understand what you just said that they would have been estimated along the way, but the results weren't reported in your testimony or work papers?

A I did not report the ordinary least squares, you know, the first stage results as you characterized them

before. I didn't report those in my testimony. I know that the regressions would have been run. I don't recall whether the program that calculates the first stage also calculates variabilities for the first stage. It wouldn't be needed really at that point.

[Pause.]

Q Dr. Neels, I'd now like to ask you some questions about potential methods to correct the errors you say you found in Dr. Bozzo's analysis. Could the errors you identified by avoided by using what has been termed the textbook rho transformation; that is, where the first observations from each run of transformed data are dropped from the re-estimation stage?

A That should eliminate the problem, yes.

Q Okay; do your estimation programs allow you to identify the first observations in each run of data?

A They do, because in applying -- the nontextbook approach, you have to treat that first rho differently.

Q Okay; so, you could rerun models A and C excluding those observations.

A Yes.

Q Okay; is it your understanding that Dr. Bozzo's programs could be modified to allow a regression sample excluding the first observations?

A I would imagine that would be possible, yes.

Q Okay; Dr. Neels, I'm going to provide you with a copy of a cross-examination exhibit, which I believe was supplied to your counsel on Monday, and it is entitled variability estimates, correcting for errors identified by Neels in NOI-4, textbook approach. Do you have a copy of that? If not, I certainly can supply you with one.

A I think maybe I do.

Q Okay.

A I have that.

[Pause.]

MS. DUCHEK: Okay; Mr. Chairman, I'm just distribution copies. I'll give three to the bench.

BY MS. DUCHEK:

Q Do you have that in front of you, Dr. Neels?

A I do.

Q And have you had a chance to review that exhibit?

A I have.

Q And did you also have a chance to review the backup material for that exhibit that we also provided to your counsel on Monday?

A I did not. I have not had an opportunity to go through the programming that produced these.

Q Okay; so, would you or would you not be able to say that the programs from which the data in the table were derived exclude the first observations from the run of the

data?

A As I said, I haven't had an opportunity to go through the programs in detail. So no, I couldn't really say that.

Q Would you accept that they do subject to check?

A Subject to check, yes.

Q Would you accept subject to check that the table contains the auto-correlation adjusted variability estimates using the textbook approach to the rho transformation?

A Again, subject to check.

MS. DUCHEK: Mr. Chairman, I've marked this as USPS-Neels-NOI-XE-1, and I'm going to hand two copies to the reporter and ask that they be transcribed and entered into evidence.

MR. MCKEEVER: Mr. Chairman, I have a dilemma about the entered into evidence part, because Dr. Neels has not checked the program. I mean, he's accepted really for purposes, I guess, of subsequent questioning certain descriptions of it which, you know, may very well be true. I'm not questioning them. We just don't have a way of knowing one way or the other. I certainly have no problem with them being transcribed, but I'm not sure at that point in time there's been a sufficient foundation laid for accepting them into evidence.

MS. DUCHEK: Mr. Chairman, this morning, Mr.

Strasser was presented with a cross-examination exhibit by the OCA which contained numbers. He was asked to accept them subject to check. Some of them, he even disagreed with. I remember there was a disagreement concerning total expenses versus operating expenses. There was a quite extensive disagreement on that. Those were still allowed to be entered into evidence, and I don't see why a different standard should apply to these.

CHAIRMAN GLEIMAN: I don't either. And the same option that I gave to the Postal Service this morning is available to UPS this evening. If someone wants to appeal the decision to enter it, they have until Tuesday, and the replies would be due by Wednesday. I think that's the time frame that we used this morning. And as is always the case, whether it was this morning or this evening, when we review the evidentiary record, we make some judgments about how much weight to give to evidence, whether it's a cross-examination exhibit or straight testimony.

So having said that and wanting to be equally unfair to everybody --

[Laughter.]

CHAIRMAN GLEIMAN: -- I guess, we're going to transcribe it into the record and enter it into evidence.

[Exhibit No. USPS-Neels-NOI-XE-1 was transcribed into the record and

received in evidence.]

Variability Estimates Correcting for Errors Identified by Neels in NOI 4
"Textbook approach": eliminates first observations to compute fixed effects estimator ("Model A")

Cost Pool	Variability	Adj R-squar	Std. Error
BCS	0.762	0.933	0.034
FSM	0.472	0.955	0.034
LSM	0.93	0.964	0.023
Manual Flats	0.582	0.908	0.033
Manual Letters	0.619	0.92	0.027
OCR	0.573	0.827	0.043
Composite	0.618		

Composite Variability Calculation
BY 1998

Cost Pool $	Volume-Variable $
1,043,841	795,407
1,042,369	491,998
78,765	73,251
459,933	267,681
1,563,964	968,094
219,070	125,527
4,407,942	2,721,958

Variabilities from fixed-effects model, adjusted for autocorrelation
Evaluated using arithmetic mean method, all observations in regression sample

Sources:
BCS, FSM, LSM, OCR: varltr-tpf-by98-rev-noi4n.out
Manual Flats, Manual Letters: varltr-tph-by98-rev-noi4n.out

MS. DUCHEK: Thank you, Mr. Chairman, and actually, for a variety of reasons, that has shortened my cross-examination much to my relief I'm sure as well as to everyone else's.

BY MS. DUCHEK:

Q Dr. Neels, would you turn to your testimony at page 12, lines 20 to 21?

A I see that.

Q Okay; you state there, and I'm quoting: "There is much going on in the labor hour data that is not explained well by the substantively important parts of Dr. Bozzo's models." Is that correct?

A That's what it says, yes.

Q Could you please clarify your use of the term substantively important parts?

A Well, I'd contrast it with what I call the dumb variables. And here, I'm talking about variables which are indicators or trends and don't have specific information about specific sites. I think they're described in the paragraph above where we indicate there is time trends, facility-specific fixed effects and time period specific fixed effects, and I would contrast that with variables such as the capital index, which does describe what's going on at a particular facility at a particular point in time. So, does that help -- does that answer your question?

Q I think so. Would it be the case, then, that the pooled model includes only what you term the substantively important parts of the models?

A In the pooled model as it's been put forward, I think it also includes the time trends, which I did refer to in the paragraph above.

Q Did you estimate a pooled model that eliminated the time trends?

A I didn't.

Q And does the pooled model by definition exclude site-specific effects?

A It does.

Q Approximately how much of the variation in labor hours is explained by the pooled model?

A I don't recall.

Q Would you say 80 percent to 90 plus percent for most of the cost pools?

A That sounds about right.

Q Would you characterize that as a good performance for a model estimated on a large data set whose parsimoniousness -- and I'm quoting -- you note on page 12, line 22?

A Well, good performance in that it certainly indicates that some of the variables there are related to the dependent variable. I think in a very large sample of

this sort, to have 20 percent of the variation unexplained leaves a lot of room for other variables to come in and take significant roles and also to alter the, you know, relationships that appear in the included -- among the included variables in the model. So I think that yes, there certainly is some explanatory power there, but I think there's still a lot of room for other variables.

Q Is it your contention that it is not substantively important to appropriately account for the effects of non-volume factors in determining volume variabilities?

A No, it is not.

Q On page 17, would you look at lines 6 to 7?

A I see that.

Q I read that that you are indicating that there may be truly fixed effects that would never change with volume, for example, location within an urban area. Is that a fair reading of your testimony?

A That's correct.

Q Would a model that drops the fixed effects be able to reflect those truly fixed effects that you are talking about?

A It could if it included appropriate explanatory variables to capture them.

Q Would you now refer to page 15, lines 13 through 15, of your testimony?

A I see that.

Q And I am quoting, you state, "We never observed the installation and initiation of a new processing activity," is that correct?

A That is correct. Within the context of one of the variability regressions, by definition, to be included in the regression, the activity has to be up and running and to pass all the data screens, so you would never seen in a FSM variability regression the first installation of, you know, or the transition from all manual sorting to mechanized sorting.

Q But do you recall UPS asking Dr. Bozzo some interrogatories inquiring about several sites where operations started in the FY '93 to FY '98 time period?

A Yes.

Q Did those responses indicate that there are operations at newly open sites represented in Dr. Bozzo's data set?

A They do, and that is one reason why I tried to clarify the last answer. I think if you look in the data set, you can certainly see facilities opening up and you can see activities starting up. If you look in the variability regressions, you know, then if you are looking at a regression on a barcode sorter, you only -- you know, the barcode sorter regression only includes facilities and time

periods that have barcode sorting up and running. That was the sense in which I said you never see these things start.

Q Okay. So you also wouldn't dispute the fact that Dr. Bozzo's data set includes sites where operations closed or ceased in the FY '93 to FY '98 timeframe, correct?

A No, it does.

Q Would you turn to page 12, line 23, of your testimony, please, and read through that to page 13, line 1?

A I see that.

Q Are you stating there that you would consider the manual ratio and capital variables to be endogenous?

A I think, yes, they are endogenous in that they are basically under the control of the Postal Service. The Postal Service determines the level of capital investment, and the Postal Service also has a huge influence over the processing plans and how mail gets allocated between manual and automated processing.

Q Thank you, Dr. Neels. I would now like to turn briefly to the part of your testimony which covers your response to POIR Number 19. In preparing that section of your testimony, had you reviewed the data sections of Dr. Bozzo's testimony, USPS-T-15?

A I had.

Q And in preparing your direct testimony in this case, UPS-T-1, had you also reviewed those data sections of

Dr. Bozzo's testimony?

A I had.

Q Do you recall Dr. Bozzo's discussion of TPF edits?

A I do.

Q Okay. And specifically, that was at pages 107 to 108 of Dr. Bozzo's testimony?

A I don't have his testimony in front of me.

Q Well, would you accept subject to check that that is where that discussion occurs? I can give you the pages if you would like.

A Okay. What were the pages again?

Q Pages 107 to 108.

A Yes, I see that.

Q So you were aware of this for both your direct testimony and your NOI testimony, correct?

A Yes.

Q And you did not raise this issue in your direct testimony, correct?

A I did not.

Q Dr. Neels, would you now please turn to Table 7 of your testimony? It is the very last page.

A I have it.

Q You include a line there where you identify observations in manual operations where TPF is greater than zero. Do you see that line towards the bottom?

A TPF is greater than zero. Yes.

Q And then you have a symbol that I don't recognize for the relationship between TPH and TPF.

A That is intended to mean not equal.

Q Okay. It is the equal sign and then a slash next to it, correct?

A That's right.

Q Okay. And you include those observations in the total, right, in the bottom line MODS observations exhibiting error?

A That's right.

Q Do you recall whether Dr. Bozzo makes any use of the TPF variable for manual operations?

A I don't believe that he does.

Q If you would turn back for a moment to page 12, lines 22 to 23 of your testimony. And I am specifically looking at the part that reads "in addition to piece handlings."

A Yes.

Q Does that portion of your statement indicate that it is your understanding that TPF or TPH, as appropriate, is an independent variable in Dr. Bozzo's mail processing models?

A They are included, that is correct, they are included as explanatory variables.

Q And therefore, would a screen on piece handlings constitute a screen on an independent rather than a dependent variable in the analysis?

A It would.

Q Did you estimate any mail processing models using alternative treatments of the observations with erroneous TPF data?

A I didn't.

MS. DUCHEK: Thank you, Dr. Neels. I have no further questions.

CHAIRMAN GLEIMAN: Any follow-up?

[No response.]

CHAIRMAN GLEIMAN: I have a whole bunch of questions for you, Dr. Neels. I am going to try and do them real, real fast if I can. First, I want to ask you some of the questions that I asked Dr. Greene earlier on. The Griliches -- I won't be able to pronounce it, the articles from the Handbook on Econometrics, and, again, I want to focus on page 1498. The author discusses an issue that witnesses are dealing with in the mail processing analyses, specifically, the likely impact of the presence of both measurement error and fixed effects on the bias of within estimators and ordinary least square estimators. Do you agree with the author's analysis that -- do you agree that the author's analysis may help us understand the impact that

measurement error in a single regressor would have on bias and the econometric analysis of mail processing presented in this docket?

THE WITNESS: I think it is helpful.

CHAIRMAN GLEIMAN: You have looked at the example in the middle of the page?

THE WITNESS: Yes.

CHAIRMAN GLEIMAN: This examples seems to present a situation similar to what we have in the analysis of mail processing operations. In that example, it is assumed that the coefficient beta is one, but a biased estimate slightly greater than one results from econometric analyses using an ordinary least squares. When the site-specific fixed effects model is used, an estimate with a downward bias results that is greater than the upward bias caused by omitting the fixed effects terms.

Could the empirical results obtained in this docket be reflecting the same tradeoff of upward and downward bias?

THE WITNESS: I think that is certainly possible. I was here when Dr. Greene was testifying on this point, and I think he is right in that when you get into more complicated multi-variant models, the situation becomes more complicated, and people have been able to derive results under some strong assumptions. And I think just as people

here have said many times we have to work with the data we have, I think we have to work with the econometric guidance we are given, too.

I think that this, the result shown here certainly indicates that it is possible in a panel model that errors in variables can impart bias to the coefficients, and I think that that is a good reason for exercising some caution in going forward in situations where we think there is a significant amount of measurement bias.

I mean it is, I will admit that the situation is more complicated, but people have tried to work through to such results as are shown here, and they indicate that caution is warranted, I think.

CHAIRMAN GLEIMAN: Equation 6.7 gives an expression for the bias of the OLS estimator beta. If beta were equal to 1 in the fixed effects and the variable measured with error were positively correlated, is it true that the bias in the OLS estimator could turn out to be zero despite the presence of measurement error in the regressor X?

THE WITNESS: Yes, that appears to be the case.

CHAIRMAN GLEIMAN: The next equation, 6.8, gives the impression for the bias of the within estimator. Is it true that if beta is equal to 1 and there is measurement error in the regressor, then the bias in the within

estimator is always negative? In other words, will the within estimator tend to a value less than 1 as the sample size grows?

THE WITNESS: That is what it indicates. Now, I mean, again, strictly speaking, I would have to say this is subject to the assumptions that are laid out here, but that is as much guidance as we have.

CHAIRMAN GLEIMAN: Now, you have to remember that whatever you say here, Dr. Bozzo has agreed to agree with.

[Laughter.]

CHAIRMAN GLEIMAN: Just kidding. We all understand what was said earlier.

The analysis provided by the author involves in one independent variable, and your comments -- I am not sure that that is a relevant question, so we will skip that one.

The analysis provided by the author involves one error in the independent variable and Dr. Greene, in his rebuttal testimony at page 23, lines 6 through 9, noted that downward bias carries through to multiple variable models so long as only a single variable is measured with error. Would the caution he offers and the results he identifies apply to Dr. Bozzo's analysis if the TPH has significant measurement error and the other variables either have no measurement error or have little impact on the results?

THE WITNESS: Well, there are -- the TPH enters

into a number of different terms of the model, and it would seem that the other Griliches' article that I was provided with is relevant in this context, which indicates that when there are high order terms, again, under the assumptions laid out here, there is the potential for bias.

So I think, you know, that, -- again, the other articles suggests that where, you know, there is measurement error in one variable which appears in sort of a nonlinear form, and specifically here in linear form, in the squared form, that there is the potential for bias.

And I will admit that these are results derived under specific assumptions, but they are the only guidance we have at this point.

CHAIRMAN GLEIMAN: Okay. Now, a little bit earlier this evening I put out a set of 10 tables that compiled MODS data by operation for 10 MODS facilities that covered the period analyzed by Witness Bozzo. I take it you had an opportunity to look at that material?

THE WITNESS: I did.

CHAIRMAN GLEIMAN: All right. If I can find two copies of that material, I would probably want to mark it as a cross-examination exhibit at this point.

Thank you, Mr. McKeever, I appreciate your help on that. You are faster at this hour of the night than the Commission staff is. I had one for myself.

Each of the tables shows a set of figures for TPH, TPF, FHP, and HRS extracted from USPS worksheets by operation, group and facility code number. The worksheets they come from are REG9398.XLS in USPS Library Reference 107, and FPH9398.XLS in USPS Library Reference 185, and the values are shown for all 24 Postal quarters from 193 to 498. Some of the observations appear to be anomalous. Some of the observations that appear to be anomalous are enclosed in boxes on those tables.

Would you accept, subject to check, that these tables contain extracts of data that you used to compile the counts and error rates exhibited in Tables 6 and 7 of your response to POIR 19?

THE WITNESS: I would.

CHAIRMAN GLEIMAN: As I said, I am going to mark several copies of these as cross-examination exhibits and offer them in a bit, but let's get on with the questions.

Postal Service Witnesses Bozzo and Greene have argued that measurement error in the MODS data are not likely to cause significant bias in variability models. They argue that measurement error in the handlings data was largely cured by scrubbing outliers. Witness Bozzo argues that any measurement errors remaining after scrubbing are systematic over time and affect his estimates of fixed effects without biasing the variables.

To better evaluate these claims, I would like you look at the MODS data for selected operations at the 10 facilities covering the time period analyzed by Witness Bozzo. Now, looking at Tables 1 through 3, Table 1, 2 and 3 contains observations with negative values for piece handlings or work hours. Table 3 also contains several examples of positive work hours matched to zero piece handlings. Do gross errors like this appear with high frequency in the MODS data?

THE WITNESS: I think errors of this sort are fairly common. I will also note on the first page, I happened to notice that the TPF numbers there are less than the TPH numbers for the manual flats I think throughout the entire series, even though those are not boxed.

There are many instances in the MODS data where you have either positive piece handlings and zero hours or vice versa. There are a fair number of negative observations whose significance I don't understand. I have speculated about that, but I don't have a good understanding as to why they are there, or why you would get reporting only for one side of the relationship, hours or piece handlings, but not the other.

This is common in the MODS data.

CHAIRMAN GLEIMAN: I take it then that you really can't explain, or don't have any idea about the causes of

the errors like this in the data.

THE WITNESS: There are some anecdotes that have come in. I have never heard a satisfactory explanation of negative numbers. I have speculated that these may be after the fact corrections, but when I look at the first couple of pages, I see that these are very -- substantially, you know, large negative numbers. It would seem unlikely that there would be a correction of that magnitude so it must be something else.

Dr. Bozzo indicated, in response to one interrogatory, that there was at least one situation where it sounded as though the person whose job it was to log some of the manual data -- well, that position hadn't been filed, and so the job wasn't done. I don't quite understand why only part of it would be logged but not all of it. So I don't have a good explanation for much of this.

I would note one thing, I have been looking at the exhibit that was provided to me, which shows the variabilities that have been reestimated using one of the correction procedures that I indicated in my NOI testimony. And one thing that struck me about these is that these variabilities are very substantially different from the ones contained in my testimony and in Dr. Bozzo's testimony.

I have been scratching my head wondering why elimination of the first observations would make such a

difference. For example, I notice that the variability for flat-sorting machinery is .472, whereas in the -- I think previously the estimate of variability for that MODS pool was .817. And I wonder if deletion of the first observations affects not just the first observation in the data for a site, but also the first observation after a gap, because Dr. Bozzo had indicated that he would -- when there was a second -- if there was a hole in the data, he would transform the first observation after the hole. And I am wondering if the fact, these very substantial changes in variabilities indicate that there may be something very significant about the gaps in the data, that the observations after the gap are very different from the others in the data set.

I have not had time -- I mean, as I indicated, I haven't looked at the backup programming, so that is just a speculation at this point.

CHAIRMAN GLEIMAN: Are such errors likely to remain in the data after Witness Bozzo's scrubs?

THE WITNESS: I think it is certainly possible that that is the case, and I indicated in my response to the Presiding Officer's Information Request that one critical issue has to do with, you know, at what level are the data reported and how much aggregation has taken place before we get to the observations that are presented in this table.

I know, I mean if only because in R97 Dr. Bradley was using data at the four week accounting period data, that at a minimum, there has been an aggregation to the quarterly level. And I noted in the MODS manual there is provision for reporting at the weekday, I think, or even tour level. And if the underlying data are reported at that level, it could well be that there are similar errors in reporting that occur at a lower level that get masked when the data are added up.

So that there may be other kinds of problems, negatives that have been added with positives such that you can't see that there has been a negative number put into the aggregation. That is certainly a possibility, that numbers that apparently look reasonable and pass the screens still have some problems in some of the components they have been constructed from.

CHAIRMAN GLEIMAN: In Tables 4 and 5, the figures for FHP are all larger than the corresponding figures for TPH, is that correct, as best you can tell?

THE WITNESS: Looking at Table 4 of 10, that is the case, and the TPF are larger than the TPH here by a substantial margin.

CHAIRMAN GLEIMAN: Well, looking at the FHP and the TPH on those two charts, and assuming that I am correct and that the FHP figures are larger than the corresponding

figures for TPH, does this imply that there are errors in one or the other, or both of those measures?

THE WITNESS: It does. As I understand it, a piece of mail gets an opportunity to be counted as FHP only once in a facility, in the first pool it is handled in. And so if that is the case, then the FHP associated with a particular MODS pool has to be less than or equal to the number of pieces handled. And in this case, there is the reverse.

Now, this could be a result possibly of -- well, it could be a measurement error in either one, under-reporting of TPH or errors in FHP that lead to an inflated estimate of the piece counts.

CHAIRMAN GLEIMAN: In Table 5, the hours figure at the top of the table seems high in relation to the corresponding values for TPH and TPF. If this is so, is there any way to tell whether the errors are likely to be in TPH, TPF or the hours values?

THE WITNESS: Well, strictly speaking, no. It looks as though, just eyeballing the numbers, they appear, the TPH numbers, the TPF numbers are more constant than the hours numbers, which start at very high levels and then drop substantially. So that leads me to -- well, I guess no. I mean I would be inclined to say that the piece counts are more likely to be accurate because I would be surprised to

see a contraction in workforce of the magnitude shown for the hours. But from the data themselves, you can't tell which is erroneous.

CHAIRMAN GLEIMAN: Are there likely to be other errors from the same causes that you cannot readily identify by an impossible relationship or magnitude?

THE WITNESS: Well, I think so. I mean I think the comment I made before is a general one. If the underlying reporting of the data is that it comes at a detailed level, and if those detailed numbers are aggregated up, you don't know how many errors may have been masked in the aggregation process. Just simply put, as I said, you can add together negative numbers and positive numbers and come up with a reasonable looking positive number, or at least one that is not unreasonable enough to fail a screen.

And similarly, if particular values are greatly inflated, if they are averaged together with enough good data, it may not be apparent that there are some problems in some of the components.

CHAIRMAN GLEIMAN: Are such errors likely to remain after the data, Witness Bozzo's data scrubs?

THE WITNESS: It is certainly a possibility. I would feel more confident saying that they do remain if I understood more about the processes that generate these errors and at what level they happen. You know, I think the

information I have, I guess it limits me to say only that it is certainly a very strong possibility that errors remain.

CHAIRMAN GLEIMAN: In Table 6, the values for TPH, TPF and FHP for each of the quarters up to 394 seem to have been reported in millions of pieces rather than thousands. The quarters after 394 are not affected this way. However, the piece handlings for Quarter 394 appear to constitute a transition. The quarterly values for TPH, TPF and FHP in Witness Bozzo's data are sums taken over three or four accounting periods, as I recall. Witness Bozzo's minimum productivity cutoff for the LSM sorting is 150 TPH per work hour. Is observation 394 an example of a data error that is likely to escape Witness Bozzo's productivity scrub because good and bad data were combined?

THE WITNESS: It appears to be. It does look as though that transition quarter represents an averaging of two different reporting methods.

CHAIRMAN GLEIMAN: Is it possible that there are other observations that combine good and bad data in this manner?

THE WITNESS: Yes, it is.

CHAIRMAN GLEIMAN: In Table 7, the values of TPH exceed the values of TPF in Quarters 296 and 498. Also, the values for TPH and TPF for Quarter 497 seems to be large. Is this evidence of an error in TPF or TPH or both? Is

there any way that you can tell which is incorrect?

THE WITNESS: You can't tell which is right and which is wrong from this. You know that one or both have to be wrong.

CHAIRMAN GLEIMAN: As I understood his approach, when TPH exceeds TPF, Witness Bozzo takes the value of TPH as his estimate of TPF. Wouldn't such a substitution be about as likely to introduce an error as a correct one? I'm sorry -- as to correct an error?

THE WITNESS: It could do -- it could either correct an error or it could introduce an error. I don't know what the frequency is, when you have a disagreement like this, you really can't tell which is right, so I couldn't offer a quantitative assessment.

CHAIRMAN GLEIMAN: I wasn't asking for one, but I mean it could --

THE WITNESS: It could be either way.

CHAIRMAN GLEIMAN: It could solve a problem or create one.

THE WITNESS: That's right.

CHAIRMAN GLEIMAN: In Table 8, many of the hours values for processing Priority Mail are large in relation to the corresponding values for TPH. Is it possible that the high hours value may include work hours for other activities that were misclocked? Is it also possible Priority Mail

piece handlings may have been incorrectly included in the piece handlings for other activities at this facility?

THE WITNESS: Either explanation could certainly be the case. If I -- I am hesitating. Some of the MODS operations are manually logged, and Dr. Bozzo indicated they were more subject to error for that reason. I think Priority might have been one of them, if I recall correctly.

CHAIRMAN GLEIMAN: In your opinion, are there likely to have been compensation errors in either HRS or TPH values for other activities of this facility?

THE WITNESS: If they've been -- if the data have been reported in the wrong place, there would be. If they've simply not been reported, then perhaps not.

That is certainly a possibility, and, again, from the information, I can't say what the causes are.

CHAIRMAN GLEIMAN: Are witness Bozzo's scrubs likely to catch compensating errors?

THE WITNESS: No, I wouldn't think so.

CHAIRMAN GLEIMAN: Table 9: Witness Bozzo uses maximum productivity cutoff of 700 pieces per work hour for manual parcel sorting. This cutoff eliminates all but the quarter designated as 297 from the manual parcel sample for Facility 130.

Are you with me?

THE WITNESS: I am.

CHAIRMAN GLEIMAN: In your opinion, is it likely that all of the hours values are gross errors except for Quarter 297?

THE WITNESS: It looks as though there is an error in 297 -- well, I'm frankly not sure what to make of this. Certainly, 297 sticks out from everything else going on here, and the fact that it sticks out makes it look like it's an isolate error.

You see, in looking at the piece handlings, from 197 to 297, there is very little change in volume, but you're looking at suddenly almost a tenfold change in hours. I wouldn't expect that would be right.

CHAIRMAN GLEIMAN: Is it possible that Witness Bozzo's productivity scrubs are deleting good as well as bad data from his sample, and, if so, do you believe that this will affect his estimates of volume variability?

THE WITNESS: I think it's possible, and if he's doing that, it certainly could affect his results.

If the errors are not random; if there is some systematic component to them, such that it's eliminating particular kind of situations, that could alter his coefficient estimates.

CHAIRMAN GLEIMAN: In Table 10, positive values for TPH and TPF occur for the first twelve quarters, however, these values are unequal, even though this is a

manual operation.

Is this evidence of error in TPH or TPF, or both, and if so, is there any way to tell which might be incorrect?

THE WITNESS: No, I can't tell which is correct or which is incorrect.

CHAIRMAN GLEIMAN: Beginning with Quarter 196, TPF for manual letters is not reported for this facility. Is TPF omitted in much of the data for manual letters?

THE WITNESS: It is.

CHAIRMAN GLEIMAN: If TPF had always been reported, how do you believe that the overall error rates for manual activities would have changed?

THE WITNESS: If it had always been reported?

CHAIRMAN GLEIMAN: Yes.

THE WITNESS: If a screen were put in place which required that TPH be greater than or equal to TPF, I think the more extensive reporting of TPF would have led to many errors, because there seem to be many situations like this one where the TPF falls short of the TPH.

And so if there were more TPF around, I suspect it would generate more disagreements of this sort.

CHAIRMAN GLEIMAN: Generalizing from this kind of apparent error, and the frequency with which they occur, do the mods data given to Witness Bozzo by the Postal Service

seem to have been meaningfully checked for errors at the time it was collected or aggregated?

THE WITNESS: Not initially. I mean, there is a lot of negative values, very large anomalies, breaks in the series, gaps, things of that sort, still in the data when they were transmitted to Dr. Bozzo.

CHAIRMAN GLEIMAN: In your opinion, can any of the piece-handling variables for any activity be regarded as approximately error-free after Witness Bozzo's data scrubs?

THE WITNESS: I wouldn't be prepared to conclude that they were error-free, and partly that is because if I had a better understanding of why these anomalies arise, I might be able to form some judgments about how extensive they might be, and how extensive the problem of undetected errors might be.

But, you know, I see lots of things that I find puzzling, and they seem to be very common, and there seems, certainly, to be a possibility that they could have been masked in an aggregation process.

And think, as I indicated, it's -- I can't put a probability on it, but it is certainly a likelihood that there are many errors that passed by Dr. Bozzo's screens.

CHAIRMAN GLEIMAN: Is it plausible to generalize that errors remaining in piece-handling variables are un-correlated?

THE WITNESS: I wouldn't think so. I think there would be more likely to be some systematic relationships. There is some process that's giving rise to the errors, and we don't understand what that is. It's unlikely to fall equally on the just and the unjust.

CHAIRMAN GLEIMAN: In your opinion, do Witness Bozzo's scrubs remove enough erroneous data for any activity -- from any activity to allow him to estimate volume variabilities without an error in variables bias?

THE WITNESS: I think there are still some in his coefficient estimates.

CHAIRMAN GLEIMAN: Thank you, sir. Followup questions? The Postal Service seems to have some.

MR. McKEEVER: Mr. Chairman, may I request a brief recess?

CHAIRMAN GLEIMAN: You certainly may, and before we do that, since I asked a whole bunch of questions based on those charts and promised to mark a couple of copies, let me offer for the record, the Cross Examination Exhibits so that they will be transcribed into the record, if that's not objectionable.

[No response.]

CHAIRMAN GLEIMAN: I'm going to hand two copies to the Court Reporter. They have been marked PRC/UPS-POIR-XE-1.

[Exhibit Number PRC/UPS-POIR-XE-1 was marked for identification, received into evidence, and transcribed into the record.]

PRC/UPS - POIR 19 - KE1

MODS DATA FROM SELECTED FACILITIES

Reference Material to Assist Examination From the Bench

Each of the 10 tables attached presents MODS data for a specific facility covering the time period analyzed by witness Bozzo.

Each table shows a set of figures for TPH, TPF, FHP and HRS extracted from USPS worksheets by Operation Group and Facility Code Number.

Each table includes observations that exhibit apparent errors and other anomalies in the MODS data used by witness Bozzo. These observations are enclosed in boxes.

Values are shown for all 24 Postal Quarters from Q193 through Q498.

Values for TPH, TPF and FHP are in thousands. HRS are in workhours.

Source: Data from USPS worksheets reg 9398.xls in USPS-LR-107 and fhp9398.xls in USPS-LR-I-185.

NOMENCLATURE

First Line: Operation Group Name, i.e., Manual Flats, LSM or OCR.

Columns:

DNO – Facility Code Number (1 to 321).

QTR – Postal Quarter and Year (297 = 1st quarter of Postal Year 1997).

TPHop – Total pieces handled in Operation Group <op> during QTR.

TPFop – Total pieces fed in Operation Group <op> during QTR.

FHPop – First piece handlings in Operation Group <op> during QTR.

HRSop – Total clerk and mailhandler labor hours recorded in Operation Group <op> during QTR.

Table 1: Example of TPH > 0, HRS < 0

		Manual Flats			
DNO	QTR	TPH05	TPF05	FHP05	HRS05
89	193	2536	2491	515	3056
89	293	3206	3149	3149	3997
89	393	3305	3245	3245	4371
89	493	4136	4061	4061	5927
89	194	3769	3704	3704	5262
89	294	3464	3402	3402	5029
89	394	3709	3643	3643	5095
89	494	4389	4311	4311	6803
89	195	4017	3947	3947	6170
89	295	3584	3519	3519	5388
89	395	3838	3769	3769	5169
89	495	4571	4490	4490	6371
89	196	3966	0	3896	5322
89	296	3075	0	3020	4354
89	396	3406	0	3344	4631
89	496	4191	0	4113	5961
89	197	4120	0	4045	5234
89	297	3714	0	3646	4874
89	397	2864	0	2811	*-24610*
89	497	3965	0	3891	5699
89	198	3665	0	3600	5176
89	298	3275	0	3213	5170
89	398	2864	0	2810	4461
89	498	3606	0	3538	5323

Table 2: Example of TPF< 0, HRS > 0

DNO	QTR	SPBS TPH12	TPF12	FHP12	HRS12
145	193	0	0	0	0
145	293	0	0	0	0
145	393	0	0	0	0
145	493	0	0	0	0
145	194	53	53	0	315
145	294	666	666	0	2464
145	394	1643	1643	0	6278
145	494	4216	4216	0	20189
145	195	6926	6926	0	25173
145	295	4697	4697	0	19874
145	395	5659	5659	0	14819
145	495	6239	6239	0	16656
145	196	5498	5498	0	13428
145	296	4599	4599	0	12427
145	396	4557	4557	0	13187
145	496	5481	5484	0	17906
145	197	4803	4803	0	17017
145	297	3845	3882	0	15491
145	397	3751	3751	0	14157
145	497	4939	4939	0	18931
145	198	4436	4436	0	17312
145	298	3397	3397	0	16914
145	398	3431	3444	0	17073
145	498	3779	*-41668*	0	19507

Table 3: Examples of TPH<= 0, HRS <= 0

		Manual Parcels			
DNO	QTR	TPH07	TPF07	FHP07	HRS07
85	193	0	0	0	0
85	293	0	0	0	0
85	393	0	0	0	0
85	493	0	0	0	*528*
85	194	0	0	0	*3274*
85	294	0	0	0	*1925*
85	394	0	0	0	*42*
85	494	0	0	0	*14*
85	195	0	0	0	*-200*
85	295	0	0	0	*-116*
85	395	0	0	0	*-759*
85	495	0	0	0	*-597*
85	196	0	0	0	*-108*
85	296	0	0	0	*98*
85	396	0	0	0	*590*
85	496	0	0	0	*286*
85	197	0	0	0	*1273*
85	297	0	0	0	*2592*
85	397	0	0	0	*1808*
85	497	0	0	0	*3238*
85	198	0	0	0	*1899*
85	298	0	0	0	*430*
85	398	0	0	0	*-15*
85	498	0	0	0	*179*

Table 4: Examples of TPF > 0,TPF < FHP

DNO	QTR	OCR TPH01	TPF01	FHP01	HRS01
3	193	60251	*84414*	*99878*	19109
3	293	65557	111305	104835	19480
3	393	60375	*65946*	*105080*	16614
3	493	77975	131197	127771	22035
3	194	57410	96142	95067	16246
3	294	54061	*101659*	*102738*	17099
3	394	56266	100970	99539	17062
3	494	74080	126213	121679	20463
3	195	57529	100174	97173	16328
3	295	50510	99171	99058	16119
3	395	52009	91578	91087	15822
3	495	72839	136591	112659	21399
3	196	52386	91749	88524	15092
3	296	49471	*94268*	*110311*	16763
3	396	49070	*85308*	*95793*	17012
3	496	61914	*105368*	*118348*	23428
3	197	51249	*85055*	*94230*	20420
3	297	47056	*86168*	*100463*	22495
3	397	46753	*73256*	*89012*	19049
3	497	56464	*82213*	*98038*	21154
3	198	49050	*67128*	*75139*	15349
3	298	45209	*63319*	*77637*	15080
3	398	43925	*59009*	*66057*	14510
3	498	54867	*72456*	*82130*	20199

Table 5: Examples of Threshold/Productivity

DNO	QTR	OCR TPH01	TPF01	FHP01	HRS01
42	193	*2853*	*2853*	*4414*	*19612*
42	293	*2277*	*2281*	*4874*	*61045*
42	393	*1872*	*1872*	*4423*	*3587*
42	493	*2083*	*2083*	*5532*	*1444*
42	194	*1651*	*1668*	*4998*	*3255*
42	294	1242	1242	5283	292
42	394	1602	1602	4594	414
42	494	2875	2875	7873	664
42	195	2391	2391	10430	489
42	295	1939	1939	9127	400
42	395	1920	1920	7730	231
42	495	2590	2590	10960	656
42	196	2359	2359	9008	471
42	296	1876	1876	9071	374
42	396	2110	3677	7933	398
42	496	2000	3902	11555	480
42	197	1482	2910	9039	289
42	297	1766	3410	10013	269
42	397	1601	2970	8626	329
42	497	43	198	5580	205
42	198	0	0	1094	291
42	298	0	0	748	185
42	398	0	0	200	89
42	498	0	0	364	204

Table 6: Example of Aggregation of Errors

DNO	QTR	LSM TPH02	TPF02	FHP02	HRS02
35	193	12	12	5	8980
35	293	13	13	4	8852
35	393	12	12	5	9016
35	493	14	14	7	9110
35	194	10	10	5	7999
35	294	11	11	5	7153
35	394	*2278*	*2278*	*1216*	*7683*
35	494	11997	11997	6732	9069
35	195	11787	11787	6929	8809
35	295	13609	13609	7880	9425
35	395	8976	8976	5656	7314
35	495	10902	11394	7252	8491
35	196	8764	9171	5581	7308
35	296	9857	10291	5468	8078
35	396	8693	9137	5251	8314
35	496	8666	9076	4111	6428
35	197	6059	6320	2756	4255
35	297	7496	7841	2990	5430
35	397	5974	6236	2322	4284
35	497	737	773	105	581
35	198	0	0	0	8
35	298	0	0	0	0
35	398	0	0	0	0
35	498	0	0	0	0

Table 7: Examples of TPH > TPF

IDNUM	QTR	FSM TPH11	TPF11	FHP11	HRS11
133	193	38755	38817	33055	52269
133	293	39870	39958	33541	51479
133	393	42475	42704	37261	62654
133	493	52554	53263	43191	84950
133	194	43448	44129	37060	71398
133	294	42201	42736	35368	65127
133	394	44274	44691	38413	70338
133	494	56355	56863	41715	79324
133	195	45142	45613	35493	71126
133	295	44416	44951	31777	61995
133	395	49313	49861	37279	66550
133	495	59021	59747	43131	81901
133	196	47498	48239	35544	66077
133	296	*41996*	*41980*	32274	55869
133	396	45289	45440	34142	60345
133	496	53506	54257	40535	74327
133	197	45351	47424	33079	61962
133	297	40955	41215	28073	58976
133	397	44192	45059	4375	70183
133	497	*138440*	*139881*	101	88226
133	198	44190	46046	93	79265
133	298	42103	43600	37	71834
133	398	46604	48283	2	82118
133	498	*55980*	*54415*	0	99995

Table 8: Examples of Large HRS

DNO	QTR	Priority TPH08	TPF08	FHP08	HRS08
11	193	4927	0	4895	60998
11	293	5024	0	4979	61906
11	393	4904	0	4875	65291
11	493	6274	0	6218	78154
11	194	4975	0	4941	64705
11	294	4637	0	4604	57256
11	394	3664	0	3626	45881
11	494	4758	0	4712	75205
11	195	3714	0	3675	60475
11	295	3809	0	3770	65598
11	395	456	0	456	14317
11	495	-27	0	-13	188
11	196	1	0	1	44
11	296	*0*	0	0	*1077*
11	396	*0*	0	0	*5599*
11	496	*0*	0	0	*4515*
11	197	*3*	0	3	*2327*
11	297	811	0	790	9739
11	397	*-11*	0	-11	*8276*
11	497	*105*	0	105	*10614*
11	198	*97*	0	97	*7133*
11	298	595	0	595	13439
11	398	1658	0	1658	22379
11	498	864	0	864	14423

Table 9: Examples of Large TPH

Manual Parcels

DNO	QTR	TPH07	TPF07	FHP07	HRS07	TPH/HRS
130	193	922	0	922	1534	*601*
130	293	830	0	830	608	*1365*
130	393	707	0	707	171	*4135*
130	493	904	0	904	369	*2450*
130	194	718	0	718	790	*909*
130	294	739	0	739	563	*1313*
130	394	778	0	778	665	*1170*
130	494	1033	0	1033	607	*1702*
130	195	931	0	931	213	*4371*
130	295	750	0	750	635	*1181*
130	395	827	0	827	523	*1581*
130	495	1122	0	1122	704	*1594*
130	196	873	0	873	560	*1559*
130	296	840	0	840	525	*1600*
130	396	768	0	768	577	*1331*
130	496	837	0	837	704	*1189*
130	197	613	0	629	457	*1341*
130	297	648	0	648	3379	*192*
130	397	706	0	706	598	*1181*
130	497	1829	0	1829	801	*2283*
130	198	3113	0	3113	603	*5163*
130	298	3712	0	3712	2335	*1590*
130	398	3665	0	3665	591	*6201*
130	498	5448	0	5448	810	*6726*

Table 10: Examples TPF > 0, TPH =/ TPF

DNO	QTR	Manual Letters TPH06	TPF06	FHP06	HRS06
86	193	*4590*	*3019*	925	6093
86	293	*5543*	*3727*	3727	7124
86	393	*3655*	*2710*	2710	6261
86	493	*4734*	*3719*	3719	8542
86	194	*5751*	*4837*	3237	7009
86	294	*7474*	*6418*	4075	6611
86	394	*6648*	*5842*	3560	5854
86	494	*9656*	*8617*	5441	8174
86	195	*7881*	*7083*	4327	6300
86	295	*8766*	*7867*	4824	6900
86	395	*7545*	*6755*	4326	7319
86	495	*9336*	*8333*	5402	9233
86	196	7776	0	4282	7175
86	296	8388	0	4038	7283
86	396	6822	0	3685	6882
86	496	8621	0	4644	8659
86	197	7102	0	3681	6592
86	297	7569	0	3546	6966
86	397	7941	0	3306	6450
86	497	10344	0	4073	9709
86	198	7931	0	3550	7320
86	298	6236	0	3477	6226
86	398	5929	0	3257	3810
86	498	5939	0	3811	4915

CHAIRMAN GLEIMAN: Let's take 12 minutes and come back on the hour.

[Recess.]

CHAIRMAN GLEIMAN: Ms. Duchek, you have decided that you don't have any follow-up?

MS. DUCHEK: Unfortunately, I still do. The break didn't dissuade me.

CHAIRMAN GLEIMAN: You can proceed as soon as you are ready.

MS. DUCHEK: I am. Thank you.

FURTHER CROSS-EXAMINATION

BY MS. DUCHEK:

Q Dr. Neels, would you turn back to the Griliches article, page 1498, please.

A I have it.

Q Specifically, on the line 6.8 --

A I see that.

Q -- do you agree with Dr. Greene that that result is specific to the case of two time periods?

A Yes, it does, although there is some commentary about what is going on in here that in fact suggests that the same problem would exist to a lesser extent in longer time series and Dr. Greene I believe said that.

The problem was reduced but not eliminated as the time period was lengthened.

Q Do you recall whether your R97 response to Dr. Bradley's errors in variables analysis was based on results from the Hsiao monograph that included the case of more than two time periods?

A My response -- which part of my R97 testimony are you speaking of here?

Q The part that responded to Dr. Bradley's errors in variables analysis.

A Where I calculated the negative measurement error variance?

Q That is correct.

A I actually -- I don't recall.

Q In your discussion of the MODS data with the Chairman, several times you commented that errors in the MODS data -- I believe you used the terms were "reasonably common," "fairly common," did you do a count of the number of errors or do you have a percentage to apply?

A I think on Table 7 I give a count by MODS pool and it ranges from a low for barcode sorting of 3.6 percent of the data exhibiting some sort of error that is detectable from the reported numbers up to a maximum of 32 percent in the case of manual parcels.

Q Do you know if negative hours are scrubbed from Dr. Bozzo's dataset?

A They are.

Q And how about negative TPH?

A They are.

Q Does Dr. Bozzo use an FHP variable?

A No, he doesn't.

Q What is your understanding of TPH, TPF, and FHP for the OCR operation and for the OCR measurement procedures?

A The OCR -- the -- well, I don't know specifically about OCR.

In general my understanding is that FHP is a count of pieces in their first distribution operation, that TPF is a count of pieces fed into the operation and TPH is a count of pieces successfully processed.

Q What I was specifically asking was for each of those -- I realize you have just given me a general definition -- the methodology by which those are collected and recorded in the OCR operation specifically

A Well, I believe that the TPF and TPH would come off of machine counts and the FHP would I believe be generated by weighing the mail and applying conversion factors, although some of the statements in the MODS manual are a little ambiguous about that, but that is my general understanding.

Is that the answer you are -- okay.

Q Dr. Neels, do you know whether any of the

erroneous observations you have been discussing have actually entered Dr. Bozzo's regression sample?

A I haven't -- well, some of them do because Dr. Bozzo alludes to situations in which he finds TPH greater than TPF and he applied an edit and went ahead and used the observation, so I know that at least some of them do.

Q But in general?

A In general I don't know how many of them do, if that is what you are asking.

Q Have you done any analysis indicating that the errors in the MODS data are correlated with volume variability?

That is, is there any evidence that the observed errors create any bias in the estimated volume variabilities?

A Well, we have been discussing I mean somewhat extensively tonight the issue of, the general issue of measurement error, and I think that it certainly indicates there is a possibility that there is bias, although the magnitude and direction are difficult to discern.

I haven't done any testing to see what happens when you change sample definitions or selection criteria to see what effect it has on variability, so I don't have any evidence of investigations of that sort.

Q Okay, so you don't have that evidence and you also

have no evidence of what direction -- if there were any bias what direction the bias would be in?

A No, I haven't worked out what direction the bias would be in although as I indicated some of the results that were discussed earlier in connection with the Griliches article is all we have to go on.

They give some examples that admittedly with assumptions used to allow one to derive conclusions, but these are examples that are pretty directly related to the models we are discussing.

Q Are those assumptions consistent with Dr. Bozzo's models?

A Well, not all of them certainly, in that he -- the specific result that is based upon N of 2 doesn't correspond to his situations, but --

Q Is that the only example that you can give, or are there others?

A I haven't -- I would have to analyze the article in more detail. I couldn't give you an answer right now.

Q Do you have -- you have indicated you haven't done any analysis indicating whether the errors are correlated with volume variability. No analysis indicating what direction the bias might be in. Do you even have a theory of what that bias might be?

A Well, as I indicated, I disagree with Dr. Bozzo

that the relationship between TPH and volume is irrelevant. I think it is actually very important, and I think failure to consider that is a source of bias.

I think also that the models are misspecified in that they don't adequately account for interactions between activities. As I indicated, they don't take in account a number of important decisions like the installation of new equipment or the initiation of activities, you know, in an existing plant or, for that matter, changes in the number of plants. I mean that has been the thrust of my testimony in this proceeding.

Q Where did Dr. Bozzo say that the relationship was irrelevant?

A I believe under questioning with Mr. McKeever, he indicated that the need to adjust the variabilities of labor hours with respect to TPH, you know, there was no need regardless of whether or not TPH and volume were proportional. And I believe he did use the word "irrelevant" in his testimony in characterizing this issue.

MS. DUCHEK: I have no further questions.

CHAIRMAN GLEIMAN: Mr. Richardson.

MR. RICHARDSON: Mr. Chairman, may I ask just one question?

CROSS-EXAMINATION

BY MR. RICHARDSON:

Q Mr. Neels, given your experience with the record in this case and the many estimates that have been made for volume variability for each of the MODS pools, and given the errors in the data which you have seen and the issues that have been raised with respect to the validity of the models proposed by all parties, in your opinion, do you believe the Commission would be able, or is able to determine on this record the correct and reasonable volume variability for each of the MODS pools?

MS. DUCHEK: Objection. I don't think that is legitimate follow-up, at least the second half of the question wasn't, and it is definitely friendly cross-examination.

CHAIRMAN GLEIMAN: That would be the first time today that I have seen any friendly cross-examination in this place.

Mr. Richardson, do you want to try and limit your question a little bit in the interest of helping us get out of here at a more reasonable hour than is otherwise going to be the case?

I would note, though, that, you know, in all seriousness, that we are at the end of the hearings for this case more likely than not, but I saw some very peculiar things going on in the way of preparation for follow-up on cross-examination of Postal Service witnesses along the way

during the course of these proceedings, and actually this evening. And I get a little bit concerned when people call everybody else on -- call others on friendly cross and perhaps, you know, don't see it that way when it is their person up on the stand.

Just see if you can narrow the question. The first half of it seemed to be okay.

BY MR. RICHARDSON:

Q Dr. Neels, it is rather simple question. Do you believe the Commission will be able to determine the correct and reasonable volume variability for each of the MODS pools in this case if it determines that it would like to move away from the 100 percent volume variability that it has historically applied?

A Well, as I indicated earlier, I have substantial issues with the specification of the models and the approach and the things that they don't take into account, and I think that those criticisms are somewhat independent of the quality of the data.

However, as we have indicated tonight, there still are questions about data quality and there are some questions about how these models have been set up and about what you might need to do if you want to use TPH as a cost driver. And I frankly don't think that there is a good, solid consensus estimate out there, or something that is

robust enough that it would warrant a decision to move away from the 100 percent volume variability.

MR. RICHARDSON: Thank you.

CHAIRMAN GLEIMAN: Anybody else?

[No response.]

CHAIRMAN GLEIMAN: If not, would you like some time with your witness to prepare for redirect?

MR. McKEEVER: One minute, Mr. Chairman, or less.

CHAIRMAN GLEIMAN: Thank you.

[Pause.]

CHAIRMAN GLEIMAN: Yes, sir.

MR. McKEEVER: We have no redirect, Mr. Chairman.

CHAIRMAN GLEIMAN: That being the case, then we want to thank you, Dr. Neels, for your participating in the proceedings, your contributions tonight and on other occasions. And we thank you, you are excused.

THE WITNESS: Thank you.

[Witness excused.]

CHAIRMAN GLEIMAN: Mr. Richardson, I believe that you have the next witness.

MR. RICHARDSON: Thank you, Mr. Chairman. Mr. Chairman, the OCA calls its next witness and, hopefully, perhaps the last witness in the proceeding tonight, J. Edward Smith.

Whereupon,

J. EDWARD SMITH,

a witness, having been called for examination and, having been first duly sworn, was examined and testified as follows:

CHAIRMAN GLEIMAN: You may proceed to introduce his testimony.

DIRECT EXAMINATION

BY MR. RICHARDSON:

Q Dr. Smith, you have before you two copies of your prepared written rebuttal testimony entitled "Rebuttal Testimony in Response to Notice of Inquiry Number 4 of J. Edward Smith on Behalf of the Office of the Consumer Advocate," dated August 21st. Was this prepared by you or under your direction?

A Yes, it was.

Q And do you have any additions or corrections?

A No, I don't.

Q And if you were asked the same questions today, would your responses be the same?

A Yes, they would.

MR. RICHARDSON: Mr. Chairman, with that, I will hand two copies of Dr. Smith's rebuttal testimony to the court reporter and ask that it be transcribed into the record and admitted into the record as evidence.

CHAIRMAN GLEIMAN: Without objection, it is so

ordered.

[Rebuttal Testimony in Response to NOI No. 4 of J. Edward Smith, OCA-RT-4, was received into evidence and transcribed into the record.]

OCA-RT-4
Docket No. R2000-1

REBUTTAL TESTIMONY IN

RESPONSE TO NOTICE OF INQUIRY NO. 4

OF

J. EDWARD SMITH

ON BEHALF OF

THE OFFICE OF THE CONSUMER ADVOCATE

AUGUST 21, 2000

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UNITED STATES OF AMERICA
Before The
POSTAL RATE COMMISSION
WASHINGTON, D.C. 20268-0001

Postal Rate and Fee Changes, 2000) Docket No. R2000-1

REBUTTAL TESTIMONY IN RESPONSE TO NOTICE OF INQUIRY NO. 4 OF J. EDWARD SMITH

I. STATEMENT OF QUALIFICATIONS

My name is J. Edward Smith, and I an econometrician with the Office of the Consumer Advocate of the Postal Rate Commission. I have previously provided a Statement of Qualifications in my Direct Testimony OCA-T-4 in this case.

II. PURPOSE AND SCOPE OF TESTIMONY

I am testifying in response to the Notice of Inquiry No. 4 Concerning Mail Processing Variability Models, issued August 2, 2000. The Notice seeks input from participants on the comparison of four models: the facilities-based fixed effects model presented by Dr. Bozzo in his direct testimony, USPS-T-15, and denoted by the Commission in the Notice as "Model A"; a time-based fixed effects model with time as the dummy variable to estimate the fixed effects, denoted as "Model B" in the Notice of Inquiry; a random effects model, one of which was generated by the Panel command by Dr. Bozzo and presented in his testimony; and a pooled model, one of which was generated by the Panel command by Dr. Bozzo and presented in his testimony. The question to be addressed is which model (if any) is superior for estimating the volume variability of labor.

More specifically, the Notice poses questions relating to whether statistical testing of the Commission denoted Models A and B using null hypothesis tests establishes their statistical superiority over the models nested within them (Notice, part b). It also asks whether Models A and B are nested within one another and if there are statistical grounds for preferring one model over the other (Notice, part c). The Notice also requests a discussion of whether passing the statistical tests establishes that either Model A or B yields a valid estimate of the volume variability (Notice, parts d and e). Finally, it further seeks discussion of whether a rejected model might nevertheless provide a better estimate than another model (Notice, part f).

For the reasons set forth below, my testimony today does not include statistical analysis of the models presented. I do not discuss the relative merits of Models A and B with respect to each other or the relative merits of the models nested within these two models prescribed by the Commission, except to note that on a theoretical basis neither Model A nor Model B is nested within the other.

III. IN MY DIRECT TESTIMONY I DISCUSSED THE DEFICIENCIES IN THE MODELS PRESENTED.

In my direct testimony I discussed the deficiencies associated with Dr. Bozzo's models. I believe that the econometric relationships propounded by Dr. Bozzo are basically incorrect from a theoretical viewpoint, regardless of whatever statistical properties are propounded as being achieved. If a hypothesized economic relationship is incorrect, the fact that the relationship can be estimated with a high degree of accuracy and precision is irrelevant.

The Notice also posits as Model B an equation not presented by Dr. Bozzo but which relies for its underpinnings upon the variables found in Dr. Bozzo's model. Model B, regardless of its statistical properties, is based thus upon an incorrect theoretical framework and should be rejected as having inadequate theoretical support.

A. The Analysis Presented by Dr. Bozzo Continues to have Many of the Errors Identified by the Commission in the Work Previously Presented by Dr. Bradley.

The major problems in Dr. Bradley's work identified by the Commission were associated with the accuracy of the underlying database, the theoretical structure of the

modeling effort, and the appropriate estimation approach. Since these problems are carried over into Dr. Bozzo's work, his equations are also wrong. Accordingly, the adoption of any of Dr. Bozzo's equations is inappropriate.

B. The Underlying Database Continues to be a Problem, as it was in Dr. Bradley's Direct Testimony in Docket No. R97-1.

There does not appear to be a high degree of quality control at the field level in the collection of the data. Neither Dr. Bozzo nor Dr. Bradley mentioned any data collection controls associated with the initial collection of the data or implemented during or immediately following the on-site data collection in order to assure accuracy. Instead, both Dr. Bradley and Dr. Bozzo focused on statistical analysis to eliminate data errors. However, statistical scrubs can eliminate correct data, can fail to eliminate incorrect data, and provide no first-hand experience or insight as to why data items are recorded in the form reported.

C. The Underlying Theoretical Assumptions of the Study are Poorly Specified.

In Docket No. R97-1, the Commission found a number of theoretical problems with Dr. Bradley 's study; many of the problems have carried over to Dr. Bozzo's work. The use of the proportionality assumption in an attempt to use total pieces handled or total pieces fed as a measure of output is wrong; the two variables grow faster than First Handled Pieces (FHP).[1]

[1] Direct testimony of witness Neels, UPS-T-1 at 60, lines 5-8.

The equations are incorrectly specified; some variables that are treated as exogenous should be treated as endogenous. In the case of the manual ratio, the Commission in Docket No. R97-1 discussed the problem, but the problem has been carried over into Dr. Bozzo's work.

Both Dr. Bradley and, subsequently, Dr. Bozzo have incorrectly assumed that mail-processing facilities are fixed in number.[2] The Postal Service adds mail-processing facilities and renovates and expands existing facilities on an ongoing basis. A reasonable conclusion from the evidence is that the number of facilities varies with volume. However, this issue has been inadequately addressed. If the number of facilities varies with volume, then witness Bozzo's elasticities are flawed because they do not correctly represent the variability of mail processing labor.[3] Both Dr. Bradley and Dr. Bozzo fail to model variations in mail processing costs in response to sustained volume increases at the system level.

D. Dr. Bradley's Analysis was Short Run, as is Dr. Bozzo's Work.

Economists define the long run as the period of time over which all inputs are variable. By treating capital, the manual ratio, and facilities as predetermined or exogenous, Dr. Bozzo fails to model mail processing costs as a function of capital, labor, and other relevant inputs. Accordingly, the analysis is short run. By failing to

[2] The number and size of facilities is discussed in the *Appendices to Opinion and Recommended Decision*, Volume 2, Docket No. R97-1, May 11, 1998, Appendix F at 21.

[3] *Ibid.*

explicitly model capacity utilization, he eliminates a key variable that affects labor demand and theoretically biases his model to be one of short-run analysis.

The Commission has highlighted the problems associated with a short-run analysis:

> The cyclical nature of mail volume over a rate cycle implies that the relationship between input use and mail volume across adjacent accounting periods will primarily reflect seasonal variation in mail volume. On the other hand, staffing levels and therefore hours would be set to reflect sustained annual or rate cycle volume levels. Therefore, large changes in volume across accounting periods can occur with little change in labor hours across accounting periods. [4]

E. Dr. Bozzo does not Use the Correct Theoretical Econometric Model.

Dr. Bozzo assumes that the modeling effort should be conducted at the activity level, and that mail-processing activities should be modeled independently of each other. However, both assumptions are of dubious validity; neither has been tested, and both assumptions appear to be wrong. Accordingly, none of the equations developed by Dr. Bozzo provide a correct analysis of mail processing costs.

Dr. Bozzo has also estimated a conditional labor demand model; the relevant model, which should have been estimated, is a labor demand model. Dr. Bozzo has *modeled capital as exogenous even though it is clearly endogenous and is* simultaneously determined as a part of the labor and sorting plans.

Finally, the modeling should have been performed on a long-run basis, focused on the facility expansion path. The conditional labor demand function presented by Dr.

[4] *Appendices to Opinion and Recommended Decision*, Volume 2, Docket No. R97-1, May 11, 1998, Appendix F at 13.

Bozzo is not such a solution, being predetermined on the basis of capital and being of a conditional nature.

IV. THE DEFICIENCIES IN THE MODELS MEAN THAT THERE IS AN INADEQUATE BASIS FOR CONCLUDING THAT ANY MODEL IS CORRECT.

I conclude that none of the models presented by Dr. Bozzo, or which could be based on his approach as is Model B, are correct, and believe that adoption of any of them would be inappropriate. Important issues focused on the variables, data, and level of modeling (activity level, facility level, or system level) need to be resolved. In seeking a "least bad" solution for my direct testimony, I focused on the "between model", and Dr. Neels focused on models at the system level, corrected to eliminate the proportionality assumptions. None of the models presented in this case are in close agreement with their alternatives (*e.g.*, none of them serve as a basis to "split the difference"). Accordingly, at this time all models need to be rejected.

I have previously advocated and I continue to advise that the Commission recommend the formation of a working group in order to resolve these technical issues and bring the modeling effort to closure.

V. CONCLUSION

As indicated, Dr. Bozzo's model is fatally flawed, and because Model B is based upon Dr. Bozzo's underlying assumptions, I do not recommend adoption of a time-based version as reflected in Model B.

The modeling effort needs to be modified. Correct variables for output (measured in terms of pieces of mail processed, not pieces handled or fed), capacity utilization, and capital (measured in terms of the processing operation with which it is associated) and other variables are needed. Capital, capacity utilization, and the manual ratio need to be treated as endogenous in a simultaneous equations system in order to allow for the long-run nature of the process. The analysis needs to be conducted at the plant or system level, not the unit activity level. If the analysis were conducted at the unit level, then there would need to be a modeling of the interrelationships of activities. The appropriate variables should be used in estimating labor demand, not conditional labor demand. The analysis should give careful consideration to the fixed effects approach correctly modeled for facilities and time.

CHAIRMAN GLEIMAN: One party requested oral cross-examination of this witness, the United States Postal Service. Does anyone else care to cross-examine?

[No response.]

CHAIRMAN GLEIMAN: If not, Mr. Koetting, when you are ready.

MR. KOETTING: Thank you, Mr. Chairman.

CROSS-EXAMINATION

BY MR. KOETTING:

Q Good evening, Dr. Smith.

A Good evening, Mr. Koetting.

Q Do you happen to have a copy of Notice of Inquiry Number 4 handy?

A No, I don't, I just have my response.

Q Okay. Let me just read to you Question F. It says, "Parties are asked to discuss whether, even with the rejection of the hypotheses described in A, there may be theoretical grounds for concluding that a rejected model could provide a better estimate of variability than either the Model A or B." Do you have that question in mind?

A I have that in mind, sir.

Q Okay. Now, in response to a Postal Service attempt to exclude a portion of your response, your counsel filed a pleading that stated, specifically with respect to the subpart F that I just read, "Dr. Smith's testimony

responds directly to this request with a resounding 'no.' There are no theoretical grounds for concluding a rejected model provides a better estimate than Models A and B." Is that your testimony in this proceeding?

A

Yes, sir, it is.

MR. KOETTING: We have no further questions, Mr. Chairman.

CHAIRMAN GLEIMAN: Follow-up?

[No response.]

CHAIRMAN GLEIMAN: Questions from the bench?

[No response.]

CHAIRMAN GLEIMAN: Would you like some time to prepare for redirect?

MR. RICHARDSON: I don't think so, Mr. Chairman.

CHAIRMAN GLEIMAN: That being the case, Dr. Smith, that completes your testimony here today. We appreciate your appearance, your contributions to the record. We thank you and you are excused.

THE WITNESS: Thank you, Mr. Chairman.

[Witness excused.]

CHAIRMAN GLEIMAN: We thank the Postal Service for its brevity.

MR. RICHARDSON: Mr. Chairman, I apologize for not making a comment, however, you had indicated you were going

to ask the questions about the other articles.

CHAIRMAN GLEIMAN: If I didn't ask them, it is because I have decided not to ask them.

MR. RICHARDSON: Thank you.

CHAIRMAN GLEIMAN: Thank you.

That concludes our hearings. I want to thank counsel for their assistance in developing the record and for putting up with me getting a little weary and tense these last few evenings. But I thank them for maintaining a positive and cooperative attitude, that is what I have come to expect from everyone who is involved in the proceedings here before the Commission.

I do indeed look forward to reading your briefs and your reply briefs. I hope that nothing happens between now and then that causes us to have to come back into the hearing room.

The hearings in Docket R2000-1 are, I hope, closed.

[Whereupon, at 11:16 p.m., the hearing concluded.]