

# The impact of bad writing in economics.

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**We find significant style differences between articles written by economists for their professional journals and articles by the same economists in the Wall Street Journal. We find no evidence that style influences the professional success of economists, which suggests that the private rewards to improving the quality of writing are low. Indeed, each community of scholars has probably adopted a "professionally correct" writing style, from which its members diverge little. However, scholars do alter their rhetorical style to communicate effectively with audiences other than the professional community. This suggests that intra- and inter-group (written) communication by economists is efficient.**

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"Bad writing does not get read."

--Donald McCloskey(1)

"Technical considerations of publications are at least equally as important in judging the worth of scientific information as the end goal of adding to the existing body of scientific knowledge."

--Janet Chase(2)

## I. INTRODUCTION

In his lead article in *Economic Inquiry* !1985<sup>^</sup>, Donald McCloskey lambasts economists about their rotten professional writing. He believes that the economics profession would be better off if we all wrote "better." Interestingly, in an equally hard-hitting article published in the *Journal of Political Economy* in 1969, Walter Salant took economists to task for their poor writing. He led off his presentation by noting:

In the past several months I have spent much time reading manuscripts written by my professional colleagues. Although this activity has taught me some economics, as one might expect, it has not been an unmixed pleasure. At some times, to be frank, it has been rather trying.

What has made it trying is that too much of the writing I have read is clumsy or worse: nearly incomprehensible. Crimes of violence are committed daily against the English language and the helpless reader is too often frustrated in his effort to understand the message !545<sup>^</sup>.

In the scientific community clarity and conciseness of writing style are regarded as valuable aspects of an individual's effort to convince others of the validity of a knowledge claim. Chase !1970<sup>^</sup> reports that academic scientists rate clarity and conciseness of writing style the third most important criterion used to evaluate the merit of other scientists' research. The ten criteria chosen from

included, among other things, originality, logical rigor, theoretical significance, mathematical precision and replicability of research technique.

The prominence accorded to McCloskey's article and the appearance of Salant's article in arguably the most influential journal published in the economics profession is notable. Journal editors apparently concur with the basic message expressed by both authors: the writing style of academic economists leaves much to be desired.

We find it difficult to disagree with the notion that how you say what you say matters. Knowledge is a commodity that is demanded and supplied in a market; basic marketing principles surely apply. But Salant's reference to readers being "helpless" is as ludicrous as suggesting that sellers force consumers, via advertising, to purchase their products. Consumers (of all commodities) are neither stupid nor helpless. The market for literature, both professional and otherwise, is characterized by free entry and exit. Nothing compels prospective readers to purchase specific literature or to consume (read) it fully once purchased. Any reader continually makes marginal decisions about whether to continue reading. These decisions are presumably based upon an ongoing evaluation of the expected benefits of continued reading as compared to the expected costs. Whenever the former fall below the latter, the reader stops. How the individual's expectations regarding costs are formed is open to speculation. The average difficulty a reader has experienced in the past with an author's work (be it an altogether different publication or previous pages in the current work) probably forms, in part, the basis for his expectation of the costs of continuing to read.

This suggests that each author is residual claimant to the negative effects of his prose. A necessary condition for acceptance of a knowledge claim is that the virtues of that claim become known by individuals other than the author. To become widely known, the claim must be read by at least some members of the relevant academic community. The discoverer of a significant scientific breakthrough who communicates poorly the virtues of his work incurs a loss derived from reduced professional acceptance of his

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ideas.

Given the results of Chase's survey, authors may, on average, improve their chances of manuscript acceptance at professional journals by attending to elements of presentation. Every author, academic or otherwise, invests time and technology in improving the presentation of his or her work, provided the marginal benefit of so doing exceeds or equals the marginal cost. It is presumptuous of anyone to claim unique knowledge of the marginal benefits to authors of investment in better presentation. An author's marginal investment calculus is impossible to evaluate externally. An external observer does not possess precise knowledge of the elements in the objective function being maximized (presumably) by the author. The individual interested merely in reporting the results of his research without regard for the response of others in his field to his work undoubtedly will be less interested in the readability of his report than will the individual interested in maximizing acceptance of his knowledge claims. Similarly, holding constant marginal returns across inputs, the scholar for whom writing is easy will depend more on his writing style relative to other inputs in his knowledge claim production function, than will scholars for whom writing is difficult. Margins matter to scholars, not just to the subjects they study.

Despite his agreement with McCloskey's general thesis that economists write poorly, Jack High !1987^ insists that voluntary efforts to clean up our act are misguided, since bad writing is, indeed, read (and cited). The argument hinges, as so much of economic analysis does, on opportunity cost. Bad writing may be read and cited; the knowledge claims may achieve a certain modicum of professional acceptance. The critical issue is, would the marginal costs to the author of improving the presentation stylistically be more than compensated for in terms of marginally increased readership?(3) High claims that the general answer to this question for academic economists is no. The expected private costs of improving one's writing exceed the expected benefits, for most scholars. This makes it unlikely that academic economists will change their professional writing style.

If we find it difficult personally to disagree with Salant and McCloskey, we find it even more difficult professionally to agree with them. We are willing to consider changing the input mix in our scientific production functions based on evidence that we can expect to profit, broadly speaking, from so doing.(4) The problem is that neither author presents so much as one shred of evidence that writing style influences the professional success of economists. Conjecture is not evidence.

Salant provides limited evidence that Ernest Hemingway

writes with greater clarity than two of the "better writers among economists" !1969, 557^, by looking at the number of syllables per word used by each. Suppose we concede (without proof) the validity of Salant's and McCloskey's complaint that economists write poorly. Why is poor writing of any consequence? Does poor writing among economists adversely affect anyone and if so, to what extent? What is the welfare cost of poor writing and how does it compare against the opportunity cost of the resources required to improve our writing? Salant and McCloskey do not provide us with answers to these questions. Indeed, the characterization of economists' writing as "poor" implies a comparison state that is never discussed. Suppose we concede that economists with established reputations as "good" writers write less well than Hemingway. Are we shocked by their incapacity? Is the economics profession headed toward extinction? The answers are no and no. Hemingway's prose is all-important to the impact of his work on readers. The academic economist's prose is but one ingredient, and not the most important one at that, in producing "impact" or "influence" or whatever it is that the economist produces. We maintain that "poor" writing in comparison to Hemingway is of little or no consequence to scholars. Again, however, conjecture is not evidence. The real issue is whether "poor" writing influences the distribution of "success" among economists.

Jack High argues that for academic economists the cost of bad writing is low; poorly written articles are published regularly in the profession's journals. Moreover, the cost to authors of good writing is high. A scholar's efforts to improve his or her prose are, therefore, likely to be uneconomical. In essence, High argues that poor writing by economists is economical. However, his assertions are also empirically unsupported.

We simply do not understand what the real impact of good writing is, in terms that are familiar to us. It is a subject that has drawn little or no previous study. We propose to begin to fill this lacuna by examining empirically the impact of bad writing on economists. Our findings reveal that, at least by McCloskey's definitions, the quality of writing does not influence (1) citations of published papers, (2) the probability of acceptance at major journals, or (3) the speed with which a manuscript is reviewed. Moreover, influential economists apparently achieve their status for reasons unrelated to quality of writing. Since our empirical measures of "bad" writing come from McCloskey, our findings suggest that either McCloskey is simply wrong--economists' writing is efficient, judged by market as opposed to a biased observer's criteria--or McCloskey's general indictment may be correct, but his definitions of "bad" writing do not capture the essence of the problem.

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### II. METHODOLOGY AND RESULTS

McCloskey provided specific examples of what he regards as bad writing, including verbosity, banal words (which he listed), and distracting footnotes. McCloskey prefers short sentences over long sentences, other things equal, because long sentences obscure the point being made. Footnotes interrupt one's reading and are thus undesirable. It is especially objectionable to interrupt a sentence midway<sup>(5)</sup> with a footnote. Less discomfiting is a footnote at the end of a sentence, especially if the sentence concludes a paragraph. McCloskey provided a long list of objectionable words and phrases, as well as a primer on good writing style. McCloskey also provided a list of twenty-nine economists whom he considers to be excellent writers.

It seemed prudent to test the strength of McCloskey's indictment by employing his standards of writing excellence. This assumes, of course, that his characteristics of bad writing are valid and can be put in some metric. Economy in writing can be measured by number of words per sentence. The degree of objectionable interruption by footnotes can be measured by number of footnotes per unit length of presentation or by number of footnotes that occur mid-sentence, or by some ratio of the two. Similarly, one could count the number of "bad words" in the presentation.

There is independent verification of several of McCloskey's stated elements of style. Several computer programs that purport to measure aspects of writing style have been developed and marketed in the 1980s. The most highly praised (Writer's Workbench and CorrectText) are available in full form only for mainframe computers. Bell Labs developed the former for internal use. The latter is published by Houghton-Mifflin and is available for the VAX family of minicomputers. Volkswriter 4 includes a watered-down version of CorrectText called Perfect Grammar that is apparently about as good as other personal-computer based packages with respect to measuring clarity.

The three main style programs for personal computers are Readability Plus, Grammatik IV and Rightwriter. According to Diamond and Levy !1990<sup>^</sup>, Readability is the program that measures clarity most accurately. Readability draws from four indices of clarity: the Flesch Index, the Flesch-Kincaid Index, the Gunning-Fog Index, and the LIX Index. The Flesch Index examines number of syllables per one hundred words and average number of words per sentence. Flesch-Kincaid measures the grade level required to comprehend the writing. Gunning-Fog analyzes average words per sentence and percentage of words with three or more syllables. The LIX Index looks at average

words per sentence and percent of words with seven or more characters.<sup>(6)</sup> The correlations between the indexes are extremely high-- $.85$  at a minimum. Since these software packages are marketed to would-be writers, the presumption would have to be that their measurement techniques do capture some relevant aspects of style. Otherwise, it seems unlikely that they would survive, much less flourish, in the marketplace.

We employ nine objective measures of writing style. The first is a common feature in computer style programs; the other eight are from McCloskey's 1985 article.

(1) Words per sentence (counted in the first twenty sentences of each paper analyzed), as a measure of clarity.

(2-3) Footnotes and footnotes that interrupt sentences: these interrupt the flow of a presentation.

(4) Bad words including very and respectively (useless adverbs), interesting (pointless adjective), via (misused conjunction), for convenience (miscellaneous phrase), analyze (pretentious verb), data, process, concept, and structure (vague nouns and pronouns).

(5) Five-dollar words (we counted all words composed of at least ten letters in the first twenty sentences of each paper analyzed). These allegedly support a "pathetic" pose of The Scientist or The Scholar.

(6) Is is a colorless verb.

(7) Rhetorical questions chop up paragraphs, provide clumsy transitions to new subjects, are overused, and provide a cheap way of organizing the disorganized.

(8) Table of contents paragraphs are useless boilerplate, "stale drivel".

(9) Excessive introduction and summarizing: see (8) above.

We assume that bad writing on these margins reflects bad writing on all other margins, so that we need not examine them all. Examination of so many characteristics guards against the fallacy of composition. Even so, we acknowledge the possibility that some essential aspect(s) of bad writing may have been overlooked.<sup>(7)</sup>

#### Bad Writing and Subsequent Citations

Citations are, in some sense, the academic market equivalent of dollar voting in product markets.<sup>(8)</sup> Through citations, or lack thereof, consumers in the market for

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ideas can reward efficient producers and discipline inefficient producers. This suggests a natural test of McCloskey's general theme that bad writing hurts authors: examine the impact of specific writing characteristics on subsequent citations of papers. McCloskey's own wording suggests that he would regard this as an unbiased test of the impact of bad writing. In discussing clarity he notes !1985, 191^: "Clarity is a social matter, not something to be decided unilaterally by the writer, because the reader like the consumer is sovereign. If she thinks something you write is unclear, then it is, by definition. There's no arguing."

If the data and proposed model are taken seriously, putatively lousy writing does not affect how often a paper is cited. Table I reports ordinary least squares (OLS) regression estimates of the impact of writing characteristics on unadjusted and impact-adjusted citations (1981-85) of the 60 full articles published in the Journal of Political Economy during 1980 and unadjusted citations (1981-86) to the 251 full articles published in the Review of Economics and Statistics during 1976-80.

Following Laband !1986^, we expect the length of an article and the author's reputation to be positively related to subsequent citations. If McCloskey's assertion about bad writing not being read is correct, citations of a paper should be related negatively to average words per sentence, number of footnotes, number of footnotes that interrupt sentences, bad wording, five-dollar words, table-of-contents paragraphs, rhetorical questions, use of the word "is," and excessive introduction and summarizing.

The results reported in Table I indicate that the measured individual writing characteristics have no empirically discernable, much less negative, impact on subsequent citations. Moreover, the joint impact of these characteristics is zero. The F-statistics for the two simple models:

$$(1) \text{ Cites} = !a.\text{sub}.0^{\wedge} + !a.\text{sub}.1^{\wedge}\text{Pages} + !a.\text{sub}.2^{\wedge}\text{AuthorCites} + e,$$

$$(1\text{!prime}^{\wedge}) \text{ Adjusted Cites} = !b.\text{sub}.0^{\wedge} + !b.\text{sub}.1^{\wedge}\text{Pages} + !b.\text{sub}.2^{\wedge}\text{AuthorCites} + e,$$

are 8.589 and 2.458. In both cases, inclusion of the writing characteristics drives the model F-statistics down. For each model, we can reject the hypothesis that the style variables add significant explanatory power jointly.(9) The expected (positive) impacts of article length and author reputation on citations are observed in both sets of regressions. These normalities suggest that this nonfinding with respect to writing characteristics cannot be explained away easily as resulting from peculiar data. Following

Leamer !1983; 1985^, numerous additional model specifications were investigated, involving different combinations of the variables and squared terms. The reported results are not sensitive to the unreported model specifications.

Of course, this finding of citation insensitivity to writing style is not sufficient to negate the possibility that bad writing may have deleterious effects on the success of academic economists. McCloskey's !1985, 188^ observation that "rotten writing causes more articles to be rejected than rotten t-statistics," if true, suggests that the impact of bad writing may be felt at the acceptance/rejection stage rather than in subsequent citations. Drawing from submissions to the Journal of Political Economy, Laband !1991^ found no significant differences in writing style between accepted and rejected manuscripts. In addition, he found no evidence that the length of time taken by reviewers to evaluate manuscripts is sensitive to elements of writing style.

### The Writing of Influential Economists

McCloskey's list of good economist-writers suggests that a comparison of their writing characteristics with those of a group of similarly famous economists not known for good writing may be revealing. From data compiled by Laband over the TABULAR DATA OMITTED years, we selected a different group of forty economists. Each had accumulated at least 1,000 citations over the period 1971-83, our benchmark for declaring an economist to be "influential." McCloskey's group includes Nobel laureates and past presidents of the American Economic Association; so does ours. We collected average words per sentence and footnoting data for twenty-five of McCloskey's group of twenty-nine and our group of forty influential economists. The information was collected from single-authored articles only, to eliminate possibly confounding effects from coauthorship. Appendix A lists both sets of authors and the articles examined. The comparison is set forth in Table II.

The two groups did not differ significantly with respect to average words per sentence. Although McCloskey's group footnotes less on a per article basis than our group, any differences between the groups with respect to footnotes per page and percentage of footnotes that interrupt sentences are statistically insignificant.(10) This suggests that whatever the criteria for success in the economics profession are, good writing, as measured by average words per sentence and footnoting, is not a critical factor.

Finally, it may be instructive to look at writing in other disciplines. In particular, we suspect that contributors to journals concerned with English language and literature, being more familiar with the characteristics of good writing

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than economists, would write shorter sentences and fewer footnotes than contributors to economics journals. However, the comparison detailed in Table II reveals no statistically significant differences with respect to footnoting, and the difference in average words per sentence favors economists. The averages reported are for the sixty-five papers published by McCloskey's and our groups (merged) as compared to fifty papers published in five journals for teachers of English (ten papers each): Research in the Teaching of English, Modern Language Review, Modern Language Quarterly, English Language Notes, and Language. Note that average words per sentence for the sixty-five economists is virtually identical to the averages reported in Table I for the 60 JPE papers published in 1980 and the 251 REStat papers published during 1976-80. It is hard to argue convincingly that the results stem from our sample of economists being biased towards the best writers, when it is indistinguishable from samples of the more general population.

### III. DISCUSSION

The variety of data examined and the consistency of the results help to ensure that our findings present an accurate picture of the impact of bad writing in the economics profession. We find no statistically significant evidence that subsequent citations of published papers are influenced by a broad spectrum of writing characteristics, individually or in the aggregate. More generally, McCloskey's group of "good" writers is indistinguishable from our group of influential economists with respect to quality of writing. The evidence presented suggests that good writing, at least as defined by McCloskey and operationalized by us, is not a prerequisite for success as an academic economist.

It may be that McCloskey's point is correct, at the margin. If so, he failed to define the relevant margins of writing quality. To the extent this is true, our findings merely help to define the relevant TABULAR DATA OMITTED characteristics of low quality writing and shed no light on McCloskey's underlying complaint. With respect to the latter, his (and our) work may suffer from an omitted variable problem. That is, the subtleties of good writing are hard to define explicitly, therefore difficult to measure.

However, we are not convinced that our work is fatally flawed by the omitted variable problem. Rather, we submit that economists, and perhaps academic scholars generally, share a writing style that has evolved to convey relevant information within the academic community efficiently. There is no question but that this particular writing style differs significantly from the writing style employed by authors whose readership is drawn primarily from nonacademics. For example, the average words per sentence for Forbes and Business Week (drawn from the

first ten cover stories for each magazine during 1986) was 17.953. Compared to these publications the average words per sentence in academic economics journals is some 50 percent greater. Similarly, the average words per sentence for twenty feature stories taken from the Marketplace section of the Wall Street Journal (20.43) is much lower than for articles published in academic economics journals.

Academic scholars have developed what the editors of Economic Inquiry (in a collective review of my manuscript) term "professionally correct" writing to (presumably) communicate efficiently. Professionally correct writing by an academic scholar in an op-ed for the Wall Street Journal implies inefficient communication with that audience. If these margins matter, we should be able to test for them. In Table III, we report diagnostics derived from TABULAR DATA OMITTED Grammatik IV analysis of ten Wall Street Journal op-eds written by economists in recent months and compare them to the diagnostics of the first two pages (excluding headings, footnotes, and mathematical notation) of ten "professionally correct" articles published by these same individuals in economics journals. In addition, we present comparative diagnostics for Newsweek columns and journal articles written by Milton Friedman and Paul Samuelson. The exact citations are provided in appendix B.

The numbers speak for themselves. Scholars alter their rhetorical style to suit their intended audience. The average readability grade level of academic scholars' writing is lower and the reading ease score is higher (easier) when scholars write for the Wall Street Journal than when those same scholars write for professional journals. In addition, scholars employ passive voice less frequently and write shorter sentences, with fewer syllables per word when they address a general audience than when they address their professional colleagues. Across all five measures the hypothesis of equality of means is rejected at better than the 0.05 level of confidence, even for this small sample. The noted differences tell a consistent story: economists' writing varies according to the intended audience.

We reiterate, the evidence suggests that economists communicate efficiently, both within our relevant community of scholars and to non-scholarly audiences. This interpretation implies that McCloskey's and Salant's positions reflect merely their normative yearnings. As the editors of Economic Inquiry noted (in private correspondence regarding this manuscript), "Jargon is not sin but virtue; passive voice not evasiveness but the studied and sober rhetoric of qualification; banal words are not bad but the vocabulary of professional dialect."

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This is not to deny the possibility that writing style really does matter within the scholarly community. Both Salant and McCloskey mention numerous other specific aspects of style that they regard as important. Salant, for example, argues that streams of consecutive nouns, masquerading as adjectives, constitute poor style. In one manuscript he found the expressions: "high risk flood plain lands," "aircraft speed class sequencing," and "terminal traffic control program category." We confess, this is one part of the elephant that we did not attempt to measure. Salant also offers a number of words and phrases that are commonly misused. He also notes that punctuation can be critical to the message conveyed by a sentence. He expounds at some length on the (ab)use of mathematical language. However, both Salant and McCloskey concentrate their attention on clarity. We attempted to measure clarity through the use of average number of words per sentence. Salant suggests that placement of commas and average number of syllables per word are important. Perhaps they are. We suspect, however, that writers displaying excellence on one margin of clarity (average words per sentence) would exhibit excellence on other margins of clarity. This does not, unfortunately, absolve us from a possible sin of omission with respect to our methodology.

This possibility notwithstanding, we have uncovered no evidence that style affects the probability of manuscript acceptance or subsequent citations of published papers. Just as skeptics five hundred years ago were willing to be convinced by the weight of scientific evidence that the world was round, we are willing to be convinced that writing style matters. Unless and until that evidence is presented, we regard the Salant and McCloskey papers as opinionated polemics, not science.

### Interpreting the Time-Trend in Quality of Writing

Diamond and Levy (1990) investigated the issue of whether there has been a noticeable decline over time in the quality of writing in economics. They examined the clarity (as measured by the Readability software package) of ninety-seven presidential addresses to the American Economic Association and found evidence of a decline in Readability scores over time. Their investigation is the only one of its kind that we are aware of. Faced with a dearth of findings to the contrary, we are prepared to accept their tentative conclusions.

What is problematic about the Diamond-Levy findings, taken together with the assertions put forward by Salant and McCloskey, is the implication that our ability to communicate effectively with one another has been degenerating over time. That, in our opinion, is the wrong interpretation. Surely the profits to be made from

communicating with ever-larger audiences have induced scientists to invest (at the margin) in more cost-effective forms of communication, and to divest themselves (also at the margin) of less effective forms of communication. We believe the increased emphasis on mathematical and statistical expression in economics over the past twenty years reflects the former; the possible deterioration in writing style reflects the latter.<sup>(11)</sup> This does not strike us as being inherently bad. More efficient communicative skills are driving out less efficient communicative skills. An economist simply cannot object to that.

### Editors' Behavior

Why have journal editors implicitly put their stamp of approval on Salant's and McCloskey's denunciations? Perhaps there is something to be gained by journal editors from improving the prose of contributing authors. Subscriptions, if not readership, may be a function of how well written, on average, articles published in the journal are. Editors may benefit from both subscription and readership. Improved prose can, in principle, be produced by authors or by editors. The actual distribution of production between these two parties is beyond the scope of our inquiry. Efficiency in production requires that two conditions be met: (1) total costs of improved prose, however the production is distributed between authors and editors, must be minimized, and (2) improvements in prose continue to occur as long as the total costs of making the improvements are, on average, exceeded by (and on the margin just equal to) the total benefits derived from the improvements.

The problem, from the editor's standpoint, is that authors may not find it worth their while to supply the level of prose quality that editors desire. This is not surprising since returns to good prose that accrue to editors (and a community of scholars generally) but not authors do not enter into the latter's decision calculus regarding their private investment in writing well.

Editors could raise submission fees to cover the cost of editing for style the papers that will be published. The rise in submission fees would have an adverse effect on subscriptions, by the first law of demand. All in all, the margins that editors operate on are tight, judging from the conversations that Laband has had with journal editors (and allowing for appropriate discounting of editors' self-interest in making those comments). We suspect that the prominence accorded the Salant and McCloskey papers reflects an attempt by the editors of *Economic Inquiry* and the *Journal of Political Economy* to "jawbone" the profession into supplying higher quality prose. It is cheaper for the editors to jawbone than to supply those editorial services themselves. If our interpretation that

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economists have adopted professionally correct writing is valid, journal editors are wasting their time.

### IV. CONCLUDING COMMENTS

There are numerous margins upon which the professional acceptance and acclaim of an author's research findings may hinge. Writing style is but one of those margins. Other margins include, but are not limited to: the timing of the contribution, caliber of data, innovativeness of statistical technique, theoretical insight, and importance of contribution to the work of other scientists. By definition, a scholar cannot possess a comparative advantage with respect to operating on all of these margins simultaneously. It seems doubtful, if not extremely unlikely, that any scholar routinely maximizes on all margins simultaneously, without regard to the costs of doing so. The scholar who wishes to maximize his expected return per unit of work effort balances off the expected net marginal benefits of devoting resources to one aspect of his work against the expected net marginal benefits of devoting resources to any of the other aspects of his work. The efficient scholar operates at the point where expected net returns to incremental effort are equalized across all relevant margins of productivity.

We reiterate, caliber of presentation is only one productive margin. For some scholars, this margin is more valuable than others. One may have a comparative advantage in technical (i.e., statistical) wizardry. It should come as no surprise that he emphasizes his contributions on that margin at the expense of good grammar. So he loses three readers who can't stand the prose. Suppose the author could have gained the professional acceptance of his claims by those three individuals by investing one more hour in improving the quality of his exposition and one fewer hour on the statistical analysis. If errors of commission or omission stemming from this time substitution cost him the approval of more than three individuals who otherwise would have been adherents, he is a net loser. Is he a fool or irresponsible for not making such a substitution? Hardly.

The individual who makes an important discovery, either theoretical or empirical, balances off the expected gains from having the scientific community recognize his intellectual property rights to that discovery (priority) against the expected losses from poor writing when he submits a relatively poorly written manuscript for publication sooner than he might otherwise have submitted a more finely polished manuscript.

Marginal substitutions between inputs into the publication process are made by the individual, for the individual's benefit. There is no such thing as a universally applicable,

"optimal" production function for all scholars. The province of economists is to seek information about how each individual organizes his/her production and how changes in the relative prices of inputs influence production.

1. "Economic Writing," *Economic Inquiry*, April 1985, p. 188.

2. Janet Chase, "Normative Criteria for Scientific Publication," *The American Sociologist*, August 1970, p. 263.

3. Salant is fully aware of the opportunity cost of poor writing. He argues: "None of us has a right, even at the stage of drafting, to impose on others writing that does not meet the requirement of clarity. He who does so not only irritates the colleagues who must read what he writes, and wastes their time, but also foregoes the larger audience that might otherwise have read what he has to say. He thereby foregoes the influence his work might have" (!1969, 558<sup>^</sup> emphasis added).

4. By defining profits broadly, we include adherence to moral principle as a positively valued return to good writing. Marginal increases in adherence to moral principle are costly. We are not convinced by McCloskey's exhortations that the net return to good writing, on the morality margin, is positive.

5. Like this intrusion.

6. For a more complete discussion of these indexes, the interested reader should consult Diamond and Levy !1990<sup>^</sup>.

7. We will doubtless be called on the carpet for trying to measure the unmeasurable. The general mood of several referees of this paper was expressed succinctly by the one who wrote: "The appreciation for context, for all the subtle interaction of the elements of style, needs to be captured by the measures of the econometrician. Nowhere in Professor Laband's article do I find any demonstration that his measures systematically distinguish good writing from bad." Perhaps the charge has merit. On the other hand, McCloskey was anointed as the guru; his pronouncements are the ones we test. Those who might be tempted to cast the first stone at our method might well consider why they embraced the teachings of McCloskey in the first place, without empirical backing.

8. Neither citations nor dollars measure preferences perfectly, as there is unmeasured surplus. The pros and cons of using citation counts have been aired extensively in previous literature, including Liebowitz and Palmer !1984<sup>^</sup> and Laband !1986<sup>^</sup>.

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9. As a further check on the robustness of our results, we used Grammatik IV to analyze the first page of text from each of the fifty-eight articles published in the Review of Economics and Statistics during 1978 (the midpoint of our sample). The readability grade level score, reading ease score and average number of syllables per word were entered as explanatory variables, with AUTHCITE and PAGES in a separate CITATIONS regression. Estimated coefficients on PAGES and AUTHCITE are consistently positive and significant determinants of subsequent citations. The three measures of readability derived by Grammatik IV demonstrate no impact on subsequent citations. The fact that Grammatik IV has (presumably) met the market survivability test of being deemed useful by some set of writers ought to partially placate those who are tempted to fault our work on the basis of a scandalously large omitted variables problem (see footnote 6). These results are available upon request.

10. The noted differences with respect to footnoting may be due, in part, to differences in the percentage of papers in each group that were Nobel lectures, AEA presidential addresses, papers published in the Papers & Proceedings issue of the American Economic Review, etc. Arguably, papers that fall into this category will have fewer footnotes, on average, than regular published research papers. For McCloskey's group, nearly 50 percent of the papers examined fell into this category. For our sample of influential economists, only 10 percent of the papers fell into this category.

11. Salant discussed several (ab)uses of mathematical language. His insights are thought-provoking at the least. He claimed in passing that the use of mathematics by economists (at the time of his writing) was growing rapidly, a claim verified by Stigler !1949^; Anderson, Goff and Tollison !1986^; Grubel and Boland !1986^; and Colander and Klamer !1987^. However, he argued that, "Until a great many more people speak and read Mathematics as well as they speak English, there is no excuse for using mathematical symbols where words will do" !1969, 546^. With respect to the possibility that the importance of clarity in writing prose relative to the importance of mathematical expression is falling over time in the economics profession, Diamond and Levy note that the decline may be "a sad, but acceptable price to pay for the increased rigor of the profession. Time spent learning mathematics is time that cannot be spent learning how to write well."

### APPENDIX A

Papers sampled from McCloskey's list of Good Writers

Akerlof, G. "Discriminatory, Status-Based Wages Among Tradition-oriented, Stochastically Trading Coconut

Producers." JPE, April 1985, 265-76.

Arrow, K. "General Economic Equilibrium: Purpose, Analytic Techniques Collective Choice." AER, June 1974, 253-72.

Boulding, K. "Economics as a Moral Science." AER, March 1969, 1-12.

Bronfenbrenner, M. "Economics in Dialectical Dialect." JPE, February 1976, 123-30.

Buchanan, J. M. "The Economics of Earmarked Taxes." JPE, October 1963, 457-69.

Caves, R. "Harry Johnson as a Social Scientist." JPE, August 1984, 642-58.

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