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16 May 1996

Dr. P. Rajan Varadarajan  
Editor, *Journal of Marketing*  
Department of Marketing  
Texas A&M University  
College Station, TX 77843-4112

Dear Rajan,

I've finally finished my review of manuscript #96-070, entitled "Can We Replace Perfect Competition Theory for Its Ineptness in Competitive Strategy?" I apologize for my delay in completing this review, but this manuscript puzzled me. Because it's written in a needlessly complex and confusing style, and because several pages of needless exposition often obscure the key issues, I was uncertain about the potential contribution of this manuscript. I remain uncertain, but I suspect that a worthwhile comment is hidden within this manuscript.

As I interpret this manuscript, the counterargument to Hunt and Morgan (1995) is as follows:

1. Neoclassical economic theory is more efficient that CATC. Thus, marketing scholars should prefer Neoclassical economic theory to CATC.

2. If (1) is false . . .

   2a. Neoclassical economic theory is at least as efficient as CATC.

   2b. By Godel's Incompleteness Theorem, formal theories cannot be both complete and consistent.

   2c. Neoclassical theory, because it focuses on ideal types, strives for consistency rather than completeness.

   2d. A preference for theory completeness over theory consistency, ceteris paribus (i.e., equally efficient theories), is a matter of personal taste and convention.

Thus, marketing scholars should not prefer CATC to Neoclassical economic theory.

3. Unlike the physical sciences, the replacement of a foundational theory in the social sciences (e.g., Neoclassical theory) with a new foundational theory (e.g.,
CATC) is undesirable. Instead, marketing scholars should develop connective and transformational theories, which are preferred in the administrative sciences.

I do not believe that the authors have shown, or can show, that (1) is true. However, if they can clarify and further support their arguments for (2) and (3), then it is possible that I would recommend their revised manuscript for publication in JM. I wonder about their ability to write such a revision.

Sincerely,

Michael R. Hyman
Associate Professor of Marketing
Journal of Marketing - Manuscript Evaluation Form

Manuscript #96-070

Reviewer #1: Dr. Michael R. Hyman

Title: Can We Replace Perfect Competition Theory for Its Ineptness in Competitive Strategy?

Importance of the Topic/Issues Addressed to Marketing ....................... Very Important

Conceptual Rigor (Treatment of relevant literature, logical reasoning, etc.) .................................................. Minor/Major Problems

Methodological Rigor (Research design, sample, measures, and analysis) .................................................. Not Applicable

Clarity of Objectives of the Paper (as stated, but the authors chose an objective that is ill-suited for JM, i.e., not highly related to marketing) ............................................. Minor Problems

Clarity of Presentation/Readability of the Paper ...................................... Major Problems

Appropriateness of the Paper for the Journal of Marketing (in its current form) ........................................... Not at all Appropriate

Relevance of the Paper (in its current form) to:

   Educators .......................................................... Marginally Relevant
   Researchers .......................................................... Marginally Relevant
   Managers .............................................................. Not at all Relevant
   Public Policy Officials .............................................. Not at all Relevant

Contribution of the Paper to the Discipline (in its current form) .............. Modest Contribution

Potential Contribution to the Discipline (if revised along suggested lines) .................................................. Important Contribution

Recommendation: Invite revision (major and risky)
Comments to Authors

Manuscript #96-070: Can We Replace Perfect Competition Theory for Its Ineptness in Competitive Strategy?

Reviewer #1

General Comments

I commend your effort to scrutinize Hunt and Morgan (1995). Clearly, marketing scholars should carefully debate Hunt and Morgan's call to replace the current foundation of strategy theory—the neoclassical economic theory of perfect competition—with a new foundation—the Comparative Advantage Theory of Competition (CATC). Furthermore, your manuscript broaches several important domains for that debate. However, additional sharpening of your arguments and exposition would greatly improve your current manuscript.

Substantive Comments

(1) As I understand it, your counterargument to Hunt and Morgan (1995) is as follows:

(a) Neoclassical economic theory is more efficient that CATC. Thus, marketing scholars should prefer Neoclassical economic theory to CATC.

(b) If (1) is false . . .

(b1) Neoclassical economic theory is at least as efficient as CATC.

(b2) By Godel's Incompleteness Theorem, formal theories cannot be both complete and consistent.

(b3) Neoclassical theory, because it focuses on ideal types, strives for consistency rather than completeness.

(b4) A preference for theory completeness over theory consistency, ceteris paribus (i.e., equally efficient theories), is a matter of personal taste and convention.

Thus, marketing scholars should not prefer CATC to Neoclassical economic theory.

(c) Unlike the physical sciences, the replacement of a foundational theory in the social sciences (e.g., Neoclassical theory) with a new foundational theory (e.g., CATC) is undesirable. Instead, marketing scholars should develop connective and transformational theories, which are preferred in the administrative sciences.

In counterarguing for (a), you have ignored all of Hunt and Morgan's argument about
the relative inability of Neoclassical economic theory to explain the global failure of command economies. The superiority of CATC to explain this failure is the heart of Hunt and Morgan's argument for replacing Neoclassical economic theory with CATC. To successful argue for (a), you should first address this argument by Hunt and Morgan.

Furthermore, your anti-replacement argument on pages 16 and 17 often resorts to opinion (e.g., "In the final analysis, there has not been much evidence to refute the conclusions derived from this theory" (p.17)) and suspect reasoning. For example, it does not follow that "There is not much evidence that it [Neoclassical economic theory] is inefficient in the Paretoian sense" because "It [Neoclassical economic theory] is based on explicit assumptions and lends itself to . . . testable conclusions" (p.17). Also, the argument for reason #5 (i.e., "Neoclassical Theory has the potential to fulfill the need of a theoretic-nucleus for more sophisticated views" (p.17)) is missing at least one premise.

I suggest that you focus on subargument (b). In particular, you should try to tighten and strengthen your argument for the heuristic language of CATC. Perhaps you should reorganize your exposition so that your discussion of substantive assertions, specifying assertions, and heuristic assertions (pp.3-5) appears nearer this section.

(2) Much of your manuscript can be deleted without weakening your argument. In fact, deleting the following text should sharpen your argument and thus improve its persuasiveness.

<table>
<thead>
<tr>
<th>Pages</th>
<th>Action</th>
</tr>
</thead>
<tbody>
<tr>
<td>3-5</td>
<td>The focus of the section headed &quot;Fundamental Premises . . .&quot; (p.3) is to define substantive assertions, specifying assertions, and heuristic assertions. Roughly one page of this section is unnecessary to defining these terms. I suggest that you delete (a) the exposition from &quot;For example, in the Arrow-Debreu version of the theory . . .&quot; (p.3, line 11) to &quot;dominant theory cannot be faulted for realism&quot; (p.4, line 3), and (b) the first paragraph of p.5 (&quot;Theories, including Neoclassical Theory . . .&quot;).</td>
</tr>
<tr>
<td>5</td>
<td>In the last paragraph, delete the sentence that begins &quot;Earlier, theory construction . . .&quot; and the last two sentences (that spill onto p.6).</td>
</tr>
<tr>
<td>6</td>
<td>In the second paragraph, delete the sentence that begins &quot;The Arrow-Debreu model . . .&quot; and delete the sentence that begins &quot;Heuristic assertions alone . . .&quot;</td>
</tr>
<tr>
<td>7</td>
<td>In the first full paragraph, delete the first sentence, place a period between time and Godel (line 2), change Because to that (linking the two sentences), and delete the sentence that begins &quot;Like a snake . . .&quot;</td>
</tr>
<tr>
<td>8</td>
<td>Delete the sentences that begin &quot;After specifying assertions . . .&quot; and &quot;Any change in assertions . . .&quot;</td>
</tr>
<tr>
<td>9</td>
<td>Delete the text from &quot;Following Boland's (1971) discussion . . .&quot; to &quot; . . . the Pareto optimal choices is arbitrary, hence irrelevant.&quot;</td>
</tr>
<tr>
<td>9</td>
<td>Change the sentence that begins “Therefore, if we accept . . .” to <strong>Therefore, it is impossible to construct the criteria necessary to select between two equally efficient theories</strong> (or something like this sentence).</td>
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<tr>
<td>---</td>
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<tr>
<td>9</td>
<td>Delete the sentence that begins “Accordingly, there is no way . . .”</td>
</tr>
<tr>
<td>10</td>
<td>Rewrite the first paragraph. Delete the word set (line 4). Place a period after “an easy task”. Delete the text from “against the Neoclassical Theory” to “this glory.” Delete the last sentence.</td>
</tr>
<tr>
<td>11</td>
<td>Delete the sentence that begins “Evidently, our science is eager . . .”</td>
</tr>
<tr>
<td>13</td>
<td>Place a period after “substantive assumption” (line 7) and delete the remainder of the sentence.</td>
</tr>
<tr>
<td>13</td>
<td>Place a period after “best rival is impossible” and delete all text to the end of the paragraph.</td>
</tr>
<tr>
<td>14</td>
<td>Delete the text from “It confronts us with the two dimensional . . .” to the end of the paragraph.</td>
</tr>
<tr>
<td>14</td>
<td>Delete the text “. . . within the theory, must not guide us to dead-ends. They must . . .”</td>
</tr>
<tr>
<td>14</td>
<td>Insert the phrase <strong>authors of the</strong> between “responsibility of the” and “specifying assumptions” and delete the text from “Otherwise, telling the story . . .” to the end of the paragraph.</td>
</tr>
<tr>
<td>15</td>
<td>Delete the text “it will be incomplete if the specifying assumptions cannot articulate its substance.”</td>
</tr>
<tr>
<td>15</td>
<td>Delete the text from “Thus, theories based only on heuristic . . .” to the end of the paragraph.</td>
</tr>
<tr>
<td>16</td>
<td>Delete the text from “They appear at different points in time . . .” to the end of the paragraph.</td>
</tr>
<tr>
<td>16</td>
<td>Arguing that microeconomic theory is irrelevant at the firm level greatly weakens your position; thus, you may wish to delete the last paragraph on this page.</td>
</tr>
<tr>
<td>17</td>
<td>Delete the sentence that begins “Elimination of a dominant theory . . .”</td>
</tr>
<tr>
<td>18</td>
<td>Delete the sentence that begins “Unless we initiate . . .”</td>
</tr>
</tbody>
</table>

(3) On p. 2, you state that your argument will focus on the lack of consistency, lack of completeness, and lack of robustness of CATC. I found your argument concerned the first lack far more than the second lack and failed to address the third lack. Minding this concern, you may wish to rewrite this introductory paragraph.

(4) The discussion regarding the problem of replacing the Neoclassical theory with CATC can be shortened without loss of content. For example, the last paragraph of p.10 and the first paragraph of p.11 could be condensed into a few sentences.
(5) On p. 12 you state that "... absolute completeness or consistency is not a requirement for a new theory." By your aforementioned Godel's Incompleteness Theorem, this should not be a requirement of any theory. Thus, you may wish to revise the subargument contained in this paragraph.

(6) The first two full paragraphs on p. 16 are difficult to follow. You may wish to revise these paragraphs for improved clarity.

(7) As do Hunt and Morgan, I believe that marketing scholars should purge many non-marketing-based terms from marketing science. Please note that the reasons for my belief differ markedly from those expressed by Hunt and Morgan. Clearly, emotional offense is a poor criterion for banning terms from a discipline; if this was the only criterion, then I would agree with your exposition on p. 18. However, the terms used in other disciplines carry the baggage of those disciplines, and that baggage is often forgotten when marketing scholars apply those terms to marketing. As a result, marketing scholars remain unaware that a marketing theory is inconsistent because the underlying premises of the non-marketing notions (i.e., their baggage) used to construct that theory are inconsistent with underlying premises of the marketing notions used to construct that theory. In other words, I believe that Hunt and Morgan are right for the wrong reason. You may wish to consider this argument.

(8) Your Conclusion section rambles, especially pages 18 and 19. You could reduce this text by 50 percent without loss of content. I suggest that you write a new Conclusion, and that the new version focus on a concise summary of your argument (as I did in my opening comments).

(9) Table 1 of Hunt and Morgan (1995) contains the ten fundamental premises (i.e., the foundations) of both neoclassical theory and CATC; your Table 1 (p. 23) contains only eight premises. This difference in premises can only confuse further debate about the foundations of CATC. Furthermore, if you provided more information about your additional references (e.g., which pages most pertain to which issues), you would aid marketing scholars who wish to review writings relevant to this debate. Given its current form and status within your manuscript, Table 1 adds little; thus, you may wish to delete it.

(10) Regarding the footnotes, (a) incorporate footnotes #4, #7, #8, and #10 into the text, and (b) delete footnotes #5 and #9.

Minor Comments

A good copy editor could improve your manuscript markedly. Many of your sentences are needlessly wordy and complex (e.g., "[The] Heuristic side of theorizing language interprets the terms we use in a world where the language is not necessarily cleansed of vagueness (p. 5); "The propensity for confusion... is further enhanced for at least two evident virtues... (p. 6)). Some sentences contain superfluous words and phrases (e.g., perceptively observe rather than observe (p. 1); not necessarily always clear rather than not always clear (p. 5); theory will not be clear rather than theory is unclear (p. 6)). Other sentences contain awkward constructions (e.g., "Second, they accuse it for not faring well with empirical results" (p. 1) or incorrect syntax. Your paragraphs often lack good topic sentences to focus the reader's
attention; the resulting multi-topic paragraphs are convoluted (e.g., second paragraph of p.1; bottom paragraph of p.7). Commas are often misused (e.g., the sentence that begins "The authors assert . . . (p.1); the sentence that begins "Finally, heuristic assertions . . . (p.5)). Which and that are improperly substituted for one another (e.g., . . . substantive assertions are those idioms of a theory that must be considered . . . (p.2); "The empirical study that merely deals . . . (p.16)). Finally, your references, neither in the body of your manuscript nor in your Reference section, are formatted according to JM guidelines.

The following comments are representative rather than exhaustive.

abs.. On the first line, replace Theory of Comparative Advantage with Comparative Advantage Theory of Competition.

Abs.. Perhaps I missed it, but I could find no evidence for sentence #2; i.e., that Hunt and Morgan take 'market orientation' as their foundational premise.

Abs.. You may wish to reconsider the sentence "This manuscripts argues for the deficiencies of CATC as a superior theory" (i.e., it's a bit awkward).

p.3 Substantial assertions seem identical to substantive assertions; if so, you should limit yourself to one term. (Note: This substitution occurs throughout your manuscript.

p.6 The first paragraph requires a better topic sentence.

p.7 Begin a new paragraph with the sentence that begins "No formal language . . ."

p.15 Insert and before uniqueness of firms (line 3).

p.18 Please check the syntax of the sentence that begins "Firm diversity and abundance . . ." (In fact, you may wish to check the syntax of all sentences in your Conclusion section.)

p.22 In footnote #10, change no apparent reason, why to no reason why.

p.26 Please check pages for Hooley et al. (1990) reference (i.e., 7-230).

p.27 Please check the spelling of "Lawrance."
8 August 1996

Dr. Robert F. Lusch
Editor, Journal of Marketing
University of Oklahoma
College of Business Administration
Adams Hall 9
Norman, OK 73019-0450

Dear Bob,

Enclosed is my review of MS #96-070 (R1), now entitled “Does Comparative Advantage Theory of Competition Really Replace the Neoclassical Theory of Perfect Competition?” Overall, this revision addresses all but one of the concerns that I expressed in my previous review. Nonetheless, there is still room for improvement. Specifically:

(1) The authors haven’t addressed my concern that they “ignored all of Hunt and Morgan’s argument about the relative inability of Neoclassical economic theory to explain the global failure of command economies.” Although their argument that CATC and NTPC have the same paradigmatic roots is both interesting and worthwhile, and their arguments about (a) the longevity of comparative advantage as a central tenet of strategy theory and (b) the numerous alternative determinants of firm diversity are without question, none of these arguments directly address this concern (despite their insistence in their comments to the reviewer). None of the discussion on page 2 of the reviewer notes, introduced by the heading “CATC is troubled against questions such as the following,” appears in the manuscript. Furthermore, this discussion is, at best, an incomplete argument against CATC. Until they address—in their manuscript—the key evidence against NTPC offered by Hunt and Morgan, their counterargument is incomplete.

(2) The writing style seems needlessly complex. Although the authors make only a few syntactical errors, and their major punctuation error is an occasional missing comma (both trivial complaints), I suspect that their argument would be far clearer if they’d use a simpler writing style. I assume that JM still relies on a technical editor to revise the final draft of accepted manuscripts. Perhaps you can instruct this editor to focus on simplifying the writing style.

I noted a few additional minor concerns in my comments. The bottom line: I recommend that this manuscript be accepted for publication, subject to the authors success in addressing my attached comments.

Sincerely,

Michael R. Hyman
Associate Professor of Marketing
Reviewer #1

Manuscript #96-070 (R1): Does Comparative Advantage Theory of Competition Really Replace the Neoclassical Theory of Perfect Competition?

General Comments

Overall, your revised manuscript addresses all but one of the major concerns that I expressed in my previous review. Certainly, you've greatly tightened your argument and vastly improved both the introductory exposition on pages 1 through 5 and the conclusion. Nonetheless, you may still wish to consider the following concerns.

Specific Comments

(1) You still haven't addressed my concern that you "ignored all of Hunt and Morgan's argument about the relative inability of Neoclassical economic theory to explain the global failure of command economies." Although your argument that CATC and NTPC have the same paradigmatic roots is both interesting and worthwhile, and your arguments about (a) the longevity of comparative advantage as a central tenet of strategy theory and (b) the numerous alternative determinants of firm diversity are without question, none of these arguments directly address this concern (despite your exposition in your comments to the reviewer). None of the discussion on page 2 of your reviewer notes, introduced by the heading "CATC is troubled against questions such as the following," appears in the manuscript. Furthermore, this discussion is, at best, an incomplete argument against CATC. Until you address—in your manuscript—the key evidence against NTPC offered by Hunt and Morgan, your counterargument is incomplete.

(2) You may find the following reference useful in advancing the arguments that you make on pages 7 through 11: Skipper, Robert B. and Michael R. Hyman (1995), "On Foundations Research in the Social Sciences," *The International Journal of Applied Philosophy*, 10 (Summer/Fall). (Sorry, I don't know the page numbers.) By the way, I'm certain that Hunt reviewed a draft of this manuscript long before he began his CATC article. You may find that he missed some of the methods illustrated by Skipper and Hyman.

(3) On page 11, you argue that "the robustness of CATC is trivial. *Transaction cost analysis* suffers from the same limitation (e.g., "convenient irrefutability qualities). Perhaps another example (other that CATC) would help argue your point.

(4) Your writing style seems needlessly complex. Although you make only a few syntactical errors, and your major punctuation error is an occasional missing comma (both trivial complaints), I suspect that your argument would be far clearer if you'd use a simpler writing style. I assume that JM still relies on a technical editor to revise the final draft of accepted manuscripts. Perhaps you can instruct this editor to focus on simplifying your writing style.
Minor Comments

(1) For examples of missing commas, check the following sentences on page 1:
   (a) the sentence that begins “To address the limitations . . .”,
   (b) the sentence that begins “Their contribution is important . . .”, and
   (c) the sentence that begins “Nevertheless we argue . . .”

(2) On page 5, you write “Theories, therefore, face the predicament of choosing between completeness and consistency.” Theorists, not theories, face this predicament.

(3) On page 6, the third line from the bottom, capitalize the ‘a’ in “An.” Also, why do you use scare (double) quotes in the next sentence?

(4) On page 7, line 4, you state that “theories . . . aspire to be consistent . . .” (See comment #2 above.)

(5) On page 7, check the syntax of the sentence that begins “In Hunt and Morgan’s Table 1, the second . . .”
1 December 1996

Dr. Robert F. Lusch
Editor, Journal of Marketing
University of Oklahoma
College of Business Administration
Adams Hall 9
Norman, OK 73019-0450

Dear Bob,

Enclosed is my review of MS #96-070 (R2), entitled “Does Comparative Advantage Theory of Competition Really Replace the Neoclassical Theory of Perfect Competition?” Overall, this second revision is improved, but its writing style remains seems needlessly complex (a concern also voiced by Reviewer #2).

I noted a few additional minor concerns in my comments. The bottom line, as per my previous review, is as follows: I recommend that this manuscript be accepted for publication, subject to the authors' success in making their manuscript more readily accessible to JM readers.

Sincerely,

Michael R. Hyman
Associate Professor of Marketing
Reviewer #1
Manuscript #96-070 (R2): Does Comparative Advantage Theory of Competition Really Replace the Neoclassical Theory of Perfect Competition?

General Comments

Your revised manuscript addresses continues to improve; you’ve further tightened your argument and vastly improved both pages 2 through 6 and the conclusion. Nonetheless, I strongly suggest that you consider the following concern:

Your writing style remains needlessly complex. Although a few syntactical errors remain (i.e., run-on sentences), and your major punctuation error remains the occasional missing comma (both trivial complaints), I suspect that your argument would be far clearer if you’d use a simpler writing style. You often seem (whether true or otherwise) to be more interested in convincing readers of (a) your knowledge and intelligence, rather than (b) the truth of your premises or the soundness of your argument. (For example, the word mytheopic (p.8) is omitted from Webster’s Encyclopedic Unabridged Dictionary of the English Language and Microsoft Bookshelf 95. Certainly, a more familiar word could serve here.) You may wish to try once again to simplify your writing style (a suggestion that seems consistent with the major problem voiced by Reviewer #2—the accessibility of your work to JM readers).

You may also wish to consider the following minor concerns.

Remaining Concerns

p.2, last ¶ The final clause of the first sentence—"and there is a clear need for a new paradigm with its own constellation of theories"—is unsupported by the previous exposition. Clearly, as you state, the exchange paradigm is in crisis (à là Kuhn and Hunt); however, you have provided no evidence for rejecting exchange-based paradigms. (A few marketing scholars have argued that exchange is not the central notion of marketing. See, for example, Hyman, Michael R. and Richard Tansey (1992), "The Evolution of Applied Marketing Theory as Evinced by Textbook Definitions," in AMA Winter Educators’ Conference Proceedings (Allen, et al., eds.), Chicago: American Marketing Association, 328-338.) As your argument for continued reliance on NTPC requires that exchange be the central notion of marketing, you may wish to avoid the adequacy questions about exchange and delete this clause.

p.2, last ¶ The sentence that begins “The CATC accentuates the impurts . . .” is a bit convoluted.

p.3, 2nd ¶ The phrase “in the past” can be deleted.

p.3, 2nd ¶ Many readers will be unfamiliar with the Chicago School (of Economics) and Bain Type. One or two basic references would provide needed guidance to interested yet naive readers. For a brief, basic, marketing-oriented, and readily
available introduction to the Chicago School, you could refer readers to the following book review:


I'm unfamiliar with Bain Type, so I have no recommendation regarding cites to basic works.

You may wish to check the syntax of your first question.

You may wish to delete the two-paragraph section headed Epiphenoms. Your concluding claim, that "Figure 2 in Hunt and Morgan's essay does not help much in integrating the disconnected statements in their Table 1" (p.10) is merely an opinion (one that I may agree with, but nonetheless remains an opinion). Your current argument, as I could reconstruct it, is as follows:

(a) Amorphous concepts and circularity in a theory produce epiphenomenal concepts.

(b) Exploring epiphenomenal concepts may be helpful, but knowledge development requires that the core meaning of theories be explicit.

(c) Epiphenomenal concepts reveal the "holistic proclivities of CATC" (p10).

(d) A set of "isolated facts" (p.10) (i.e., "an anthology of fragmented and insulated hypotheses" (p.10)), no matter how well collected, cannot yield an integrated theory.

I do not follow how the above argument is sound (i.e., that its premises, although true, argue to its conclusion). It is enough to argue that CATC contains amorphous concepts and circularity (as you do in the preceding section of your manuscript).

You may wish to delete the last sentence. First, this paragraph now has two conclusions (e.g., two sentences that begin "Therefore . . ."). Second, the point about Kuhn's discontinuity hypothesis is tangential to your argument. Third, I'm uncertain that you've provided sufficient evidence for this conclusion.

In the first sentence, you may wish to substitute the word evidence for the word reason. Reason implies causality, and your evidence is interesting but insufficient to establish causality. For example, you argue that "the underlying premises . . . are reasonable starting points for a theory of competition" (p.11). To make this claim adequately, you would need to evaluate each underlying premise for its adequacy, which is clearly unnecessary to further your argument. You also claim that "there has not been evidence to refute the conclusions derived from this (NTPC) theory" (p.11). Do you wish to claim that there is absolutely no evidence (i.e., a strong reason) or little evidence (i.e., a weaker claim, but nonetheless strong evidence)?
17 April 1997

Dr. Robert F. Lusch
Editor, Journal of Marketing
University of Oklahoma
College of Business Administration
Adams Hall 9
Norman, OK 73019-0450

Dear Bob,

Enclosed is my review of MS #97-043-IR, a rejoinder by Hunt and Morgan to the upcoming comment by Deligonul and Cavusgil. As my review suggests, I am deeply disappointed (actually, I am shocked) by the Hunt and Morgan rejoinder. In the phrase of the day, Hunt and Morgan 'generate heat but no light'. Hunt and Morgan clearly ignored one of the primary tenets of civil philosophical debate: the principle of charity. Rather than careful counterargue the points made by Deligonul and Cavusgil, they frequently resort to thinly veiled ad hominem attacks and a seemingly intentional effort to quote Deligonul and Cavusgil out of context. Furthermore, the majority of their rejoinder targets only one (non-critical) page of the Deligonul and Cavusgil comment, and once quoted text from the comment is placed in its proper context, most of this discussion becomes extraneous. Unless Hunt and Morgan can bring both civility and novel content to this debate, I strongly recommend that you reject their rejoinder for publication in Journal of Marketing. I am certain that my notes to Hunt and Morgan will provide you sufficient evidence for such a decision.

Sincerely,

Michael R. Hyman
Associate Professor of Marketing

Enclosure
Notes to the Authors

Manuscript 97-043-IR: Resource-Advantage Theory: A General Theory of Competition?

[Please note that the cited text from Deligonul and Cavusgil (henceforth DC) appears on different pages than indicated in my copy of your rejoinder manuscript. To ease the editorial process, you may wish to reconcile your cites with the third revision of DC's manuscript, dated 9 January. Also, I refer to your original Journal of Marketing article—"The Comparative Advantage Theory of Competition"—as HM95, and I refer to your rejoinder to Dickson's comment—"The Resource-Advantage Theory of Competition: Dynamics, Path Dependencies, and Evolutionary Dimensions"—as HM96. Page numbers sans a cite refer to your rejoinder manuscript.]

General Comments

I was deeply troubled by your rejoinder to DC. Because such a rejoinder, if published, might discourage future dialogue among marketing scholars (i.e., scholars worried about unjust ridicule will avoid authoring comments on previously published articles), I hope that you will consider my concerns carefully.

My concerns about your rejoinder manuscript are fourfold.

(1) You have not heeded your own advice about conducting "a civil, serious, critical discussion" (p.16). Regrettably, many of your statements may be construed as thinly veiled ad hominem attacks on DC. Furthermore, you mock DC for broaching an ambiguity in HM95 that was only later addressed in HM96.

(2) Sometimes you argue that R-A theory is within the exchange paradigm and other times you argue that it is outside the exchange paradigm. In a logically consistent rejoinder, you could take only one of these two mutually exclusive positions.

(3) The heart of your rejoinder focuses on only one page of DC; furthermore, once DC's words from this page are placed in their proper context, much of your rejoinder becomes superfluous. Instead, a rejoinder meaningful to JM readers would

   (a) address DC's arguments about the language of R-A theory, the heart of their comment, and

   (b) more effectively respond to DC's replacement argument.

(4) Some text and two figures are needlessly redundant (i.e., appear in HM96).

I address each of these concerns in turn.
Tone of Your Rejoinder

You accuse DC of “interesting rhetoric” (p.2), careless scholarship (and careless reading of HM95), nihilism, and believing “one of the most thoroughly discredited epistemologies of the twentieth century” (p.4). Then, in your conclusion, you write:

There seems to be something about R-A theory that moves sensible scholars to make insensible allegations. Future commentators who are so moved are urged to calm down, take a deep breath, and read with care the works developing R-A theory (p.16).

In other words, you characterize DC as irrational, overly excited, careless, and ignorant about modern philosophy of science. In response, a careful reader might argue that much of your rejoinder is a thinly veiled ad hominem attack on DC.

It is well known that proper philosophical debate assumes the principle of charity. After all, the scholarly community learns little when one author interprets another author’s argument in a weak fashion and then quickly dismisses this weak interpretation. Instead, philosophers first construct the strongest possible interpretation of their opponents’ arguments and then counterargue these strongest possible arguments. Unfortunately, your rejoinder often quotes DC out of context and then argues against the absurdity of DC’s position. Your introduction is filled with such quotations. (Note: This is not an exhaustive treatment of mischaracterizations.)

Consider your introduction (pages 1 to 2). You strongly suggest that DC took a needlessly strident tone in their comment to HM95, which then seems to justify the strident tone of your current rejoinder. In fact, the tone of DC is civil and consistent with scholarly debate; it is your tone that is needlessly harsh. Furthermore, because many of your quotes are taken out of context, you leave readers with a mistaken impression of DC’s arguments. Some examples from page 1, ¶2 and ¶3:

(1) You state that “For DC, because R-A theory ‘borrows heavily’ (p.4) from several research traditions, it is a ‘tired tenet’ (p.4), ‘hardly a fresh perspective (p.4).’ In fact, DC state:

The CATC model also nearly coincides with the view of resource-based theory... which in turn exhibits striking similarities to the Austria School. ... CATC’s high accord with the other members of the exchange paradigm is readily apparent.... Therefore, we submit that CATC is hardly a new perspective. ... CATC... is not at all epistemologically novel... because CATC borrows heavily from a European version of neoclassical theory that flourished in Austria during the last century. ... CATC includes a list of reasons to explain firm diversity.... The list can easily be expanded... to include factors other than those noted by Hunt and Morgan. This is not surprising because the search for comparative advantage in resources as a motivation for not only enhancing efficiency but also creating new resources is a tired tenet. Versions have appeared in many theory apart from the Austrian School, including
strategic choice theory, the population ecology model, and resource-based models (italics added) (DC, pp.2-3).

In other words, DC never make the attribution that R-A theory is a tired tenet because it borrows heavily from other research traditions. Instead, they argue that (a) R-A theory is not novel because many of its features coincide with the features of other theories, and (b) the search for a comparative advantage in resources that will enhance efficiency and create new resources is a tired tenet of all exchange-paradigm-based theories (i.e., a problem not unique to R-A theory). Clearly, your characterization of DC's words erroneously suggests to readers that DC are careless scholars.

(2) In your next sentence, you state “Worse than tired, [DC claim that] R-A theory borrows unwisely, for it is 'not well grounded' (p.5) and its view of human behavior is ‘inherently implausible’” (p.6). Again, DC make no statements about unwise borrowing and R-A theory. In the first paragraph of page 4, DC criticize “all theories in the exchange paradigm” as “powerless” to explain “the demise of the centrally planned economies.” Thus R-A theory, as an exchange-paradigm-based theory, suffers this common shortcoming. Nowhere do DC single out R-A theory for this shortcoming; instead, they disagree with your claim that R-A theory “is capable of explaining and predicting such phenomena.” In other words, it is your claim about R-A theory, rather than R-A theory itself, which is “not well grounded.” Finally, DC never claim that the treatment of human behavior within R-A theory is “inherently implausible.” Instead, within their discussion of command-based economies, DC claim:

In centrally planned countries, purely 'economic' considerations are not sufficient in explaining the behavior of the central planning authority or state enterprises. Instead, noneconomic considerations—political, social, ethnic, geographic, and so forth—may take precedence. The actors may be much less guided or motivated by economic rationale in their decisions. All these are unaccounted for in the current theories because our paradigms can offer only weakly developed and inherently implausible models of human behavior (pp.4-5).

Again, DC (a) suggest that all exchange paradigm theories suffer this shortcoming, (b) state that current theories “offer only weakly developed and inherently implausible models for human behavior” (italics added). Also, because I assume we should interpret the and in this last phrase as the logical or (again, using the principle of charity to assign the strongest interpretation to DC's words), their claim that our current models of human behavior are weakly developed is debatable, but not absurd (as portrayed in your text).

(3) DC never state that HM95 “offer[s] only ‘dated idioms’” (italics added) Within the context of their argument that CATC cannot replace NTPC, DC state that “Moreover, CATC's qualifiers (dynamic, intangible, and so on) and dated idioms also argue against a new paradigm” (p.11). The “only” in your sentence suggests that DC casually dismissed all your arguments for CATC in HM95.
You state “For DC . . . [b]ecause we ‘adopted a loose argument style’ that is ‘tautological’ (p.11), much ‘like a snake swallowing its tail’ (p.10), it is unsurprising that H&M’s arguments resemble those of ‘astrologers in antiquity’ (p.15). Clearly, given the three different page numbers cited in this sentence, the text you cite from DC does not represent a single notion. Yet, on page 5 of your current rejoinder, you castigate DC for skipping around HM95 to reconstruct your argument for the superior explanations about command economies offered by R-A theory.

The same problems can be found for most of the remaining quotations; thus, you may wish to reconsider their use throughout your rejoinder.

**Thinly Veiled Ad Hominem Attack on DC**

On pages 4 and 5 of your rejoinder, you argue that

DC adopt one of the most thoroughly discredited epistemologies of the twentieth century: Kuhn’s conceptual framework-relativism. . . . In short, DC’s epistemology, as Kuhn came to realize, is neither to adopt a healthy critical attitude toward knowledge claims, nor is it to be tolerant of different claims—it is to adopt nihilism. Marketing, we propose, deserves better than nihilism.

Rather than counterargue specific points made by DC, you first cast them as relativists and then easily reject DC along with other relativists. As a fellow scientific realist, I too reject relativism for many of the reasons discussed in the Hunt (1991) theory text. Nonetheless, DC develop several arguments that a meaningful rejoinder would address.

You also argue that “DC maintain that using reason and evidence for theory choice cannot be relied on because ‘we lack a supracriterion for their contest’ (p.4).” One well-known weakness of Kuhn’s discredited theory concerns incommensurability **between** paradigms. Yet, as point 7 of DC’s Table 1 (a statement of baseline beliefs that justify their arguments) states:

> Although we recognize the characteristics of a good theory by its traits, we cannot establish definitive criteria for superiority within the constellation of efficient theories rooted in the same paradigm (p.15).

In other words, DC argue that there are no supracriteria for choosing among equally efficient theories within the same paradigm. Early Kuhn argued that social forces (i.e., the sociology of science) dictated paradigm choice because incommensurability between paradigms precluded a meaningful dialogue between proponents of different paradigms. There is no evidence that “reason and evidence” (p.4) are extraneous to DC’s arguments regarding selection among efficient theories within a paradigm; instead, their discussion of “Can CATC Replace NTPC” (pp.9-11) contains both reason and evidence. Thus, I believe you have mischaracterized DC’s argument.

Finally, it is interesting that you included DC’s reference to Kuhn, but ignored their reference to Polanyi in the same sentence about social choice settling the score (DC, p.13). Again, either
consciously or subconsciously, you may have omitted Polanyi because readers would find him more difficult to reject without thought.

Inside or Outside the Exchange Paradigm

As my mother would say, "you can’t have your cake and eat it too." Unfortunately, sometimes you argue that R-A theory is within the exchange paradigm and other times you argue that it is outside the exchange paradigm. Clearly, these are mutually exclusive positions and you should select one to make your rejoinder logically consistent. (Note: Your comment on page 2 regarding if “there is such a thing as the exchange paradigm” smacks of the same high definition problem that concerned you in footnote 3.)

In counterarguing DC’s claim that you meant R-A theory to replace NTPC (p.2), you remind readers of this claim in HM96.

‘... R-A theory incorporates perfect competition as a limiting, special case' (HM96, p.113). Because R-A theory incorporates, rather than replaces, perfect competition, it 'subsumes, by implication, the extant predictive successes of neoclassical theory ... [and] preserves the cumulativeness of economic science' (HM96, p.113).

You argue that DC’s replacement argument is unsound (i.e., based on a faulty premise) because R-A theory was not meant to replace NTPC. For your preceding statements to be meaningful, we must assume that R-A and NTPC belong to the same paradigm. Furthermore, as you mention in your discussion about equilibrium (pp.10-13), “R-A theory relates to perfect competition in the same way that Newtonian mechanics relates to Galileo’s Law: the former incorporates the latter” (p.13). Because most physicists would agree that Newtonian mechanics and Galileo’s Law fall under the rubric of classical mechanics, (i.e., the same paradigm), then your example also reveals your belief that R-A theory and NTPC belong to the same paradigm.

Then, on page 3, you argue that R-A theory is outside the exchange paradigm, as defined by DC, because “It is true that perfect competition assumes that economic agents optimize ... [b]ut R-A theory specifically rejects the view that firms optimize (or maximize) anything.” You then make similar counterclaims about agents’ knowledge and equilibrium.

I suggest that you accept DC’s claim that R-A theory belongs to the exchange paradigm because (among other reasons) your arguments rely on an improperly truncated version of DC’s classification scheme (p.4). Had you continued your quote about the properties DC specify for “the constellation of theories rooted in the exchange paradigm” (DC, p.5), your next two sentences would have read “These idioms of neoclassical theory are imperfectly interpreted premises. Each finds it particular function in a given version of the theory” (DC, p.5). In other words, for R-A theory, the notion “agents individually optimize subject to constraints” (DC, p.5) could mean satisfying. Because (a) you refer to a Simon work on satisfying three sentences later, (b) R-A theory could belong to the DC-defined exchange paradigm if optimization subject to constraints equals satisfying, and (c) the principle of charity suggests such an interpretation of DC, then your argument is unsound. (You might also note
that DC later state “Our purpose here is not to defend the maximization principle. Rather, we argue that Hunt and Morgan’s suggestion of ‘gauging performance to the superior referent’ is equally problmatic as a substantive assumption” (DC, p.6). Thus, your counterargument addresses a point which is, at best, tangential to DC’s comment.)

The same argument can be made regarding your next paragraph (about agents having full relevant knowledge). Thus, your conclusion in the next paragraph, that “DC allege falsely that R-A theory is a member of their self-described exchange paradigm” (p.3), is unsound.

Ambiguity Addressed in HM96

On page 2 of your rejoinder, you berate DC for failing to “quote us on the replacement thesis.” Rather, “R-A theory is a process theory that can explain when neoclassical theory will (and will not) predict successfully, because R-A theory incorporates perfect competition as a limiting, special case (HM96, p.113).”

Although you may now believe the aforementioned notion to be true, consider the following text from HM95.

Our central thesis is that the strategy dialogue Day refers to is evolving toward a new theory of competition—one that has significant advantages over neoclassical theory. Our article contributes to the development of this new theory and examines its implications for marketing. . . . We argue that this theory explains key macro and micro phenomena better than does neoclassical theory (p.1).

A theory of competition should be required to explain not only why economies premised on competing firms are superior to economies premised on cooperating firms in terms of the quantity, quality, and innovativeness of goods and services, but also the phenomenon of firm diversity in marketing-based economies. Our analysis indicates that the comparative advantage theory of competition explains these key macro and micro phenomena better than its perfect competition rival (p.13).

A reasonable reader could interpret this text as suggesting that R-A theory should replace neoclassical theory. I could not find the words ‘supplement’ or ‘augment’ in HM95. In fairness to DC, they submitted their comment long before HM96 was published; therefore, it seems unfair to suggest that DC misinterpreted HM95 and are careless scholars because they do not also address HM96. Judging from your decision to discuss this supplement/augment issue in HM96, DC were reasonable in interpreting your claims in HM95.

So, the most interesting (or perhaps problematic) rhetoric is the closing paragraph of your introduction:

Because DC’s argument is based on a demonstrably false premise, it is fatally flawed. Is the rest of their argument equally flawed?
First, you rely on a subsequent work to suggest that DC are careless scholars. Then you hint that such carelessness suggests all of DC’s comments are invalid. Given these facts, wouldn’t you characterize your statements as “interesting rhetoric”?

The Heart of Your Rejoinder

More than half of your rejoinder manuscript (pages 5 to 15) addresses less than 5 percent of DC’s comment (i.e., the section headed “Can CATC Explain the Downfall of Command Economies?”, DC, p.4). As the arguments in the remaining 95 percent of DC’s manuscript do not depend on this 5 percent, your inordinate attention to this material is puzzling. Furthermore, once you place DC’s comments in their proper perspective, many of your counterarguments become superfluous.

R-A Theory and Command Economies

To explain nonmicro phenomena such as “demise of the centrally planned economies,” DC argue that all “theories in the exchange paradigm, including CATC, are powerless” (p.4). Again, R-A theory is not singled out for this shortcoming (see point (2) above, under the heading Tone of Your Rejoinder). Because DC would clearly agree that “Regarding observed differences between command and market-based economies, neoclassical theory is explanatorily impotent” (p.7), you need not make this point; thus, two-thirds of page 6 and the first paragraph of page 7 are superfluous and can be deleted without loss of content.

Four Requirements

(1) In the section headed “Process Theory,” you argue that

DC maintain that R-A theory views the firm as ‘an efficiency seeker in production and distribution, as in Chicago school thinking’ (p.3-4). While it is true that efficiency seeking is the sole focus of neoclassical theory and it may also be true of the Chicago school, it is untrue with respect to R-A theory. Indeed, a major distinguishing characteristic of R-A theory is its dual focus on both efficiency and effectiveness” (p.8).

Again, had you continued with DC’s text, you would have read the following:

The CATC model also nearly coincides with the view of resource-based theory . . . which in turn exhibits striking similarities to the Austrian School. CATC’s high accord with the other members of the exchange paradigm is readily apparent from Table 2. Therefore, we submit the CATC is hardly a new perspective (DC, p.2).

A further examination of DC’s Table 2 clearly shows their belief that R-A theory is similar (in some regards) to the Austrian school and Bain’s Firm theory. DC indicate that R-A theory and the Austrian school posit that the firm is “an innovation seeker” and is “motivated by the desire for supernormal profits as a vehicle for promotion discovery and for realizing opportunities” (DC, Table 2, p.16). Because both these notions
suggest effectiveness, DC do not seem to argue that firms are merely efficiency seekers. Finally, the conclusion of DC’s argument here is that "CATC is hardly a new perspective," yet your response ignores this conclusion.

(2) In the section headed "Organizational Learning," you argue that

DC allege falsely both that perfect competition and R-A theory share the premise of ‘full relevant knowledge’ (p.9).

Again, as discussed in the section above, entitled Inside or Outside the Exchange Paradigm, DC claim that "These idioms of neoclassical theory are imperfectly interpreted premises" (p.5). Qualified in this manner, DC’s claim about “full relevant knowledge” is reasonable. Your discussion on pages 9 through 10 is interesting, but does not address DC’s comment (i.e., it is superfluous).

(3) In the section headed "Equilibrium," you argue that

DC allege falsely that R-A theory assumes that competition ‘must be discussed with reference to equilibrium states’ (DC, p.5). In truth, R-A theory claims that competition is an inherently disequilibrating process (p.10)

In the next 2 ½ pages, you argue that “perfect competition . . . may be viewed as a limiting, special case of R-A theory” (p.13). Again, DC argue that R-A theory is an exchange-paradigm-based theory, so this “special case” claim is not contrary to DC’s comment. Again, your discussion is interesting, and no doubt offers an excellent summary of the unpublished working paper by Hunt (Hunt 1997d), but it fails to address DC’s comment directly.

(4) In the section headed "Socio-Political Institutions," you argue that

DC claim that an understanding of command economies requires a theory that accommodates ‘noneconomic considerations’, for such factors as ‘political, social, ethnic, geographic, and so forth — may take precedence’ (DC, p.4) in the decisions of central planners and state enterprises. DC further allege that R-A theory fails to account for such socio-political factors because it offers ‘inherently implausible models of human behavior’ (DC, p.5) (p.13).

As I mentioned above (point (2) above under the heading Tone of Your Rejoinder), you have taken DC’s comments out of context. (Note: This also pertains to your discussion in the first full paragraph of page 14.)

On page 14, you state that
DC read us as claiming that firms will always act not opportunistically, instead of our actual view that firms will not always act opportunistically. Good grief, no wonder DC thought our view inherently implausible (p.14).

Rhetoric aside, you took one sentence and implied that it served as critical evidence for a claim that DC never made. Instead, DC claim:

Moreover, in the CATC world, since firms are universally not opportunistic (HM95, p.9), but customers are self-interest seekers (tenet 3), it is likely that firms become vulnerable to exploitation (DC, p.7).

Is DC's claim about R-A theory true if the words universally and not are transposed in the previous sentence? If so, then I would apply the principle of charity here.

Questionable Redundancy Between HM95 and HM96

Many readers may question some of the seemingly needless redundancies between HM95 and HM96. Specifically:

1. The text of page 8, starting with "Because superior equates with both more than and better than . . .," and ending with " . . . industry to industry, and culture to culture," appears verbatim in HM96, p.109. You may wish to either delete this text and present your argument in a different fashion.

2. Figures 1 and 2 are identical to figures in HM96. Furthermore, Figure 1 adds little to your exposition (i.e., your discussion of process theory on pages 7 to 8). I recommend that you delete both figures.

3. Footnote 1 in your rejoinder matches footnote 1 in HM96. I recommend that you delete footnote 1.

4. On page 16, your list of what R-A has been shown to do appears—without the references to work by Hunt and Morgan—in the conclusion to HM96 (p.113).

Regarding redundancy (4) above, you merely argue that your own work consistently supports a theory of which you are proponents. Although the creation of such a body of work in so short a time is impressive, the consistency of your work on R-A theory only shows that your recent work is consistent. You cannot show the intersubjective certifiability of R-A theory, which I assume was your goal in presenting this list to readers, until other marketing scholars demonstrate the power of R-A theory. (Note: Please do not assume that the previous comment places me in the dreaded relativist camp; rather, a demonstration of convergent validity typically requires independent confirmation.)