How the emphasis on ‘original’ empirical marketing research impedes knowledge development

Raymond Hubbard
Drake University, USA

R. Murray Lindsay
University of Western Ontario, Canada

Abstract. Empirical research in marketing should focus on the development of empirical generalizations. Marketers do a huge amount of empirical research, but have little in the way of empirical generalizations. This is primarily because most empirical research consists of ‘original’ or ‘novel’ works looking for significant differences, rather than significant sameness, in unrelated data sets, thus exemplifying the ‘cult of the isolated study’. As a result, the marketing literature is made up largely of uncorroborated, fragmented, ‘one-off’ results. Such results are of little use to marketing practitioners or academicians. We discuss a number of impediments to the development of empirical generalizations – preoccupation with the hypothetico-deductive conception of science, preoccupation with ‘statistical’ rather than ‘empirical’ generalization, the ‘publish or perish’ syndrome in academia, and denigration of replication-with-extension research. We conclude that replication-with-extension research must be championed as the vehicle for discovering empirical generalizations.

Key Words • empirical generalizations • knowledge development • original research • replications-with-extensions • statistical generalizations

Unquestionably, empirical generalizations or empirical regularities play a prominent role in science . . . (Hunt, 1991: 117).

We believe that a major goal of empirical research in marketing should be the discovery of empirical generalizations (EGs) among phenomena of interest. This is
marketing theory 2(4)
articles

because we share Leone and Schultz’s (1980) conviction that marketing generalizations are a major component of marketing knowledge. According to Barwise (1995: G30), an extremely weak definition of an EG is ‘any empirical observation which has been found to generalize’. Bass (1993: 2) notes that an EG ‘is a pattern or regularity that repeats over different circumstances and that can be described simply by mathematical, graphic, or symbolic methods’. Later, Bass and Wind (1995: G1) added that an EG is ‘a pattern that repeats but need not be universal over all circumstances’. Ehrenberg (1995: G20) thinks of EGs ‘as merely database regularities’. Finally, Hunt (1991: 113) declares that ‘An empirical regularity is a statement summarizing observed uniformities or relationships between two or more concepts or variables’. What these definitions share in common is the notion of the basic repeatability of empirical results over a wide range of conditions (e.g. different companies, geographic areas, time periods, measurement instruments, researchers).

Empirical generalizations initially constitute descriptions of marketing phenomena, but in addition have the capacity to explain and predict the occurrence or non-occurrence of such phenomena when integrated into a coherent scientific structure (Ehrenberg and Bound, 1993; Hunt, 1973, 1976, 1991). Thus, EGs can lead to lawlike generalizations, laws, principles, and theories (Hunt, 1991). In turn, EGs may pave the way to strategic principles (Rossiter, 2001).

Bass (1995) has observed that science may be conceived as a process involving the interaction between EGs and theory. Indeed, in the interactions between EGs and theory it is more usual for the emergence of EGs to precede theories explaining them, rather than vice versa (Barwise, 1995; Bass, 1993; Ehrenberg, 1993a; Leone and Schultz, 1980). Thus, very often, the inductive process of EG discovery drives theory development. Given this sequence, the overwhelming emphasis in academic marketing on theory development and testing, with its accompanying focus on generating ever more ‘original’ or ‘novel’ empirical results, seems wholly misplaced. In the consumer behavior area, for example, Alba (1999) has recommended downplaying the role of theory-testing in favor of obtaining some reliable ‘data points’ or ‘facts’, a strategy echoed by Hunter (2001). Likewise, Simonson et al., (2001) question the wisdom of the dominant emphasis on theory tests.

What we should be doing is devoting far more intellectual resources to the active search for EGs, rather than the present (and past) misallocation of scholarly efforts on continued theory development and testing. This latter pursuit has resulted in a great deal of ‘original’ empirical research being published in marketing journals. We argue in the present paper, however, that we pay a heavy price for this originality. In making this argument, we address a number of topics. First, we show from a survey of nine leading journals that most published research in marketing is empirical in nature. Second, we indicate that most of this empirical research consists of original or novel works looking for significant differences in unrelated data sets, instead of significant sameness. As a result, our literature is largely made up of uncorroborated, fragmented, ‘one-off’ results — a far cry from EGs. Third, we suggest that one-off results are of little use to marketing practi-
tioners, and that this has contributed to an increasing polarization between marketing practitioners on the one hand and marketing academicians on the other. We examine this polarization in surrogate fashion by documenting how research published by practitioners, and by practitioner-academic alliances, has declined over time in the nine marketing journals. Fourth, we show that not only do practitioners ignore most of the original work published in marketing journals, but academicians do also. This is revealed in a Social Science Citation Index (SSCI) analysis of the nine marketing journals. Fifth, we discuss a number of impediments to the development of EGs (in addition to the search for significant differences rather than significant sameness, the impediment mentioned above). The impediments are preoccupation with the hypothetico-deductive conception of science, preoccupation with 'statistical' rather than 'empirical' generalization, denigration of replication-with-extension research, and the pernicious effects of the academic reward system. Sixth, we anticipate accusations of 'brute empiricism' and therefore we briefly address some philosophical issues associated with the replication research/EG approach to marketing science. We conclude that replication-with-extension research, or 'differentiated replication' (Lindsay and Ehrenberg, 1993), must be championed as the vehicle for discovering EGs.

**Empirical research gets published**

First we estimated the proportion of published empirical work in the marketing literature. After all, the discovery of EGs in marketing is going to be compromised if most of the published research is non-empirical. Table 1 shows the proportion of empirical research published in marketing over the period 1936–2000. This table is based on data gathered from two randomly selected issues of each of the nine marketing journals – *European Journal of Marketing* (EJM, 1971), the British Market Research Society’s *International Journal of Market Research* (IJMR, 1966), *Journal of the Academy of Marketing Science* (JAMS, 1973), *Journal of Advertising Research* (JAR, 1960), *Journal of Consumer Research* (JCR, 1974), *Journal of Marketing* (JM, 1936), *Journal of Marketing Research* (JMR, 1964), *Journal of Retailing* (JR, 1945), and *Marketing Science* (MS, 1982) – for every year indicated in parentheses through 2000.

Table 1 reveals that the proportion of published empirical research in these nine journals increased monotonically from 36.8% prior to 1960 to 80.2% for 1990–2000. Only two marketing journals – the JM and the JR – existed prior to 1960, and their proportions of published empirical work were 38.4% and 32.3%, respectively. For 1960–1969, about one-half (51.3%) of the marketing literature was empirical, with the newly introduced JMR at 58.1%, and the JM at 30.4%. By 1970–1979, the average proportion of published empirical works in the nine journals had grown to 65.2%. The journal with the highest proportion was the JMR (81.7%), and the lowest two were the JM (48.7%) and the EJM (45.1%). The collective proportion of empirical work for the 1980s was 75.1%; no journal published less than 60%. Finally, 1990–2000 saw no journal publish less than 72%
### Table I

**Proportion of published empirical research in nine marketing journals: 1936–2000**

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>No.</td>
<td>%</td>
<td>No.</td>
<td>%</td>
<td>No.</td>
</tr>
<tr>
<td>EJM</td>
<td>—</td>
<td>122</td>
<td>45.1</td>
<td>118</td>
<td>60.2</td>
</tr>
<tr>
<td></td>
<td>(55)</td>
<td>(71)</td>
<td>(90)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>IJMR</td>
<td>—</td>
<td>39</td>
<td>59.0</td>
<td>89</td>
<td>61.8</td>
</tr>
<tr>
<td></td>
<td>(23)</td>
<td>(55)</td>
<td>(81)</td>
<td>(112)</td>
<td></td>
</tr>
<tr>
<td>JAMS</td>
<td>—</td>
<td>149</td>
<td>52.3</td>
<td>184</td>
<td>85.9</td>
</tr>
<tr>
<td></td>
<td>(78)</td>
<td>(158)</td>
<td>(135)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>JAR</td>
<td>—</td>
<td>141</td>
<td>78.0</td>
<td>118</td>
<td>72.0</td>
</tr>
<tr>
<td></td>
<td>(110)</td>
<td>(85)</td>
<td>(111)</td>
<td>(136)</td>
<td></td>
</tr>
<tr>
<td>JCR</td>
<td>—</td>
<td>110</td>
<td>69.1</td>
<td>232</td>
<td>77.6</td>
</tr>
<tr>
<td></td>
<td>(76)</td>
<td>(180)</td>
<td>(175)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>JM</td>
<td>524</td>
<td>38.4</td>
<td>260</td>
<td>30.4</td>
<td>238</td>
</tr>
<tr>
<td></td>
<td>(201)</td>
<td>(79)</td>
<td>(116)</td>
<td>(118)</td>
<td>(113)</td>
</tr>
<tr>
<td>JMR</td>
<td>—</td>
<td>217</td>
<td>58.1</td>
<td>349</td>
<td>81.7</td>
</tr>
<tr>
<td></td>
<td>(126)</td>
<td>(285)</td>
<td>(201)</td>
<td>(190)</td>
<td></td>
</tr>
<tr>
<td>JR</td>
<td>186</td>
<td>32.3</td>
<td>128</td>
<td>50.8</td>
<td>141</td>
</tr>
<tr>
<td></td>
<td>(60)</td>
<td>(65)</td>
<td>(108)</td>
<td>(83)</td>
<td>(104)</td>
</tr>
<tr>
<td>MS</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>88</td>
</tr>
<tr>
<td></td>
<td>(60)</td>
<td>(92)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>710</td>
<td>36.8</td>
<td>785</td>
<td>51.3</td>
<td>1,316</td>
</tr>
<tr>
<td></td>
<td>(261)</td>
<td>(403)</td>
<td>(858)</td>
<td>(1,063)</td>
<td>(1,147)</td>
</tr>
</tbody>
</table>

1. The nine marketing journals, listed alphabetically, are as follows: European Journal of Marketing (EJM, 1971), International Journal of Market Research (IJMR, 1944), Journal of the Academy of Marketing Science (JAMS, 1973), Journal of Advertising Research (JAR, 1960), Journal of Consumer Research (JCR, 1974), Journal of Marketing (JM, 1936), Journal of Marketing Research (JMR, 1964), Journal of Retailing (JR, 1945), and Marketing Science (MS, 1982). With three exceptions, the dates in the parentheses are the initial year the journal was published. It was not possible to locate the first four years of the EJM (then known as the British Journal of Marketing), nor the first seven years of the IJMR (formerly known as the Journal of the Market Research Society). We felt it unnecessary to go back further than 1945 for the JR.
2. No. refers to the total number of published articles and research notes. The numbers in parentheses refer to the number of empirical articles and notes.
3. % refers to the percentage of total published research that is empirical.

Empirical research, while two (JMR, 90.9%; JR, 93.7%) are almost exclusively empirical. We publish a lot of empirical research in marketing.

Despite the vast majority of research in marketing being of the empirical variety, we have little in the way of EGs, the 1995 special issue of *Marketing Science*
dedicated to this topic notwithstanding. Bass (1993, 1995) and Ehrenberg and his
colleagues (Ehrenberg, 1995; Ehrenberg and Bound, 1993; Ehrenberg and
England, 1990) cite examples of their own efforts to supply marketing EGs (e.g.
involving new product diffusion, the Dirichlet model of buyer behavior, and price
elasticities). These are among the finest examples of EGs in the discipline. Yet even
these EGs would likely not have arisen were it not for the foresight and tenacity of
the particular authors themselves.

The above examples of marketing EGs, however, are very much the exceptions
rather than the rule. The rank and file of empirical research in marketing is far less
orderly. Thus, for example, Hunt (1976: 26) asked ‘Are there underlying uniform-
ities and regularities among the phenomena comprising the subject matter of
marketing?’ He answered in the affirmative, claiming that some uniformities had
been identified. He did not, however, give even one example. Sheth and Sisodia
(1999), on the other hand, identified 12 regularities in the marketing literature
that they feel have been sufficiently empirically validated to be classified as ‘law-
like generalizations’. Unfortunately, following Kerin and Sethuraman’s (1999)
scrutiny, 9 of the 12 regularities did not qualify as lawlike generalizations – being,
instead, tautologies, frameworks, concepts, decision rules, or conventional mar-
keting practice.5

Conscious of stating the obvious, Barwise (1995) nonetheless reminded us that
an EG is based on repeated empirical evidence. The implications of this seemingly
innocuous requirement, however, are far-reaching (Barwise, 1995: G30):

This excludes many or even most generalizations in marketing. For instance, much of market-
ing practice – and much of what we teach – is based on beliefs about generalizations which have
not been empirically tested, except at best anecdotally.

Earlier, Leone and Schultz (1980: 11) complained that marketing’s body of
knowledge is ‘more marsh than bedrock’, while Armstrong and Schultz (1993)
suggested that the discipline is without a viable set of ‘principles’. In the consumer
behaviour area, Jacoby (1978: 87) proclaimed most research to be ‘a mass of
meaningless and potentially misleading junk [that is] not worth the paper it is
printed on or the time it takes to read’. Harsh sentiments, these, but the facts are
inescapable: although the lion’s share of research is empirical, there are few EGs
in marketing.

With regard to the lack of discovery of EGs in marketing, then, we may
variously speculate that: (1) by dint of the sheer amount of published empirical
research, they are somehow expected to emerge of their own volition, sooner or
later; (2) they are not an important objective of empirical research; (3) they are an
important objective of empirical research, but are someone else’s responsibility,

hence, few researchers are actively pursuing them; and (4) conducting original or
novel empirical research is a more valuable contribution to the discipline (and
one’s career?). It would seem that this last reason (coupled with number 3) is
extremely influential and potentially damaging, and is examined below.

385
marketing theory 2(4)
articles

The cult of the isolated study

A total of 3,732 empirical papers were published in our sample of nine marketing journals. It has been estimated that only about 3% of empirical work in marketing consists of replications-with-extensions (Hubbard and Armstrong, 1994; Hubbard and Vetter, 1996). This means that about 3,620 of the 3,732 empirical papers in our sample are of the original, but, alas, unsubstantiated kind. Moreover, based on the conclusions drawn by those authors who do conduct replication studies, when such work is undertaken the results are generally at odds with the initial findings. For example, Hubbard and Armstrong (1994) found that 60% of the extensions in their study yielded outcomes that conflicted with earlier results, 25% offered partial support, and only 15% provided full confirmation. Corresponding figures for the Hubbard and Vetter (1996) study were 46%, 33%, and 21%, respectively. Clearly, there are important concerns about the replicability of original research results in marketing's empirical literature.

This excessive concern for originality fuels what John Nelder (1986: 112), in his Presidential Address to the Royal Statistical Society, called 'the cult of the isolated study'. These studies are motivated primarily by the search for statistically significant ($p < .05$) differences among variables of interest in isolated data sets, when we should be looking for reproducible results - which is a search for significant sameness. As a consequence, the empirical literature of any discipline following this strategy is comprised largely of uncorroborated, 'one-off' studies whose validity, reliability, generalizability, and usefulness are open to question.

A single, isolated result is nearly meaningless and useless in itself (Lindsay and Ehrenberg, 1993). It does not tell us whether the result is stable and, more importantly, potentially generalizable to any other set of conditions. This is the case however statistically significant the result is, or however sophisticated the analysis. The result of a one-off study should represent the beginning of an investigation into a particular area, and not the end. Gutman (1985) remarks that accepting the findings of such studies is antithetical to science. They lead to an accumulation of fragmented and isolated results which are of little practical use. For example, why should anyone care about the finding that, say, intrinsic motivation was found to improve sales force satisfaction, if this has been found only once, for 56 salespeople in 2 companies in the UK?

Compounding this problem of an empirical literature made up mostly of unsupported results, and relying on significance testing and low (small) $p$-values as the arbiters of scientific rigor, is the fact that misunderstandings about the capabilities of the tests themselves are rife (Lindsay, 1995; Sawyer and Peter, 1983). A low $p$-value from a significance test indicates only that the null hypothesis of no effect or relationship is not credible, i.e. that random error is unlikely to account for the results. But a low $p$-value does not, as it is commonly believed, tell us anything about the effect sizes obtained in a study, nor does it denote the probability that the null hypothesis is true, that the alternative hypothesis is true, that the result will replicate, that the result will generalize, or that the result is of any practical or even scientific importance. In short, a low $p$-value from a signifi-
'Original' empirical marketing research and knowledge development
Raymond Hubbard and R. Murray Lindsay

cance test tells us little. It is perhaps the least important attribute of a good study (Lykken, 1968) and as such is a very poor methodological criterion for judging the adequacy of knowledge claims (Lindsay, 1995).
Nelder (1999) has bemoaned the 'obsession' with significance tests as the end point of analysis. He calls them non-scientific procedures that obstruct the accumulation of information. In fact, Nelder (1999: 261) warned: 'The most important task before us in developing statistical science is to demolish the $P$ value culture, which has taken root to a frightening extent in many areas of both pure and applied science, and technology'. But as Table 2 shows, it is precisely this 'p-value culture' that dominates the pages of the rank and file of original empirical work found in marketing journals.

Prior to 1960, only 11.5% of published empirical research in the sample of marketing journals employed significance testing. Corresponding figures for the 1960s, 1970s, 1980s, and 1990–2000 are 39.5%, 66.2%, 77.8%, and 81.2%, respectively. For the 11-year period 1990–2000, those journals with the highest percentage of empirical work using significance tests were the $JM$ (98.2%), $JAMS$ (94.1%), and the $JCR$ (90.9%). Those with the lowest were the $IJMR$ (49.1%) and the $JAR$ (64.7%), perhaps reflecting their more balanced academic and practitioner readership. Which brings us to the topic of the relevance of knowledge developed by academic marketers for the marketing practitioner.

The great marketing academic-practitioner divide

Unlike pure mathematics or logic, which requires no empirical content, marketing is very much an applied discipline. It is therefore reasonable to expect that knowledge developed in the field will help inform managerial practice (Webster, 1988). Barwise (1995) states that in addition to having scope, precision, parsimony, and a link with theory, good EGs should also be useful to practitioners. He notes, for example, that they should tell managers the market response to a particular marketing mix.

We maintain that an empirical literature consisting primarily of original, but fragmented and unsubstantiated, findings is of only marginal value for marketing practitioners. Certainly, studies in both marketing (Bloom, 1987; Myers et al., 1980) and consumer behavior (Wells, 1993) attest that managers question the worth of academic research. Even the AMA Task Force on the Development of Marketing Thought (1988: 17) conceded that 'there is little generalizable, accumulated marketing knowledge to be disseminated to marketing's constituents'.

Seeing little of use in the knowledge produced in academe, practitioners turn to other avenues for assistance – for example, trade and popular literature, intuition, business managers, consultants, even organizing their own in-company 'business schools' (see Cornelissen, 2002; Garda, 1988). Academic research is increasingly viewed as irrelevant, and the gulf between academicians and practitioners grows ever wider. The academic response to this polarization can operate to further widen the gap between the two groups, as when academicians reposition a
Table 2

Proportion of published empirical research using statistical significance tests in nine marketing journals: 1936–2000

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>No.¹</td>
<td>%²</td>
<td>No.</td>
<td>%</td>
<td>No.</td>
</tr>
<tr>
<td>EJ M</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>55</td>
</tr>
<tr>
<td></td>
<td>(16)</td>
<td></td>
<td>(33)</td>
<td></td>
<td>(68)</td>
</tr>
<tr>
<td>JMR</td>
<td>—</td>
<td>—</td>
<td>23</td>
<td>30.4</td>
<td>55</td>
</tr>
<tr>
<td></td>
<td>(7)</td>
<td></td>
<td>(19)</td>
<td></td>
<td>(34)</td>
</tr>
<tr>
<td>JAMS</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>78</td>
</tr>
<tr>
<td></td>
<td>(60)</td>
<td></td>
<td>(135)</td>
<td></td>
<td>(127)</td>
</tr>
<tr>
<td>JAR</td>
<td>—</td>
<td>—</td>
<td>110</td>
<td>45.5</td>
<td>85</td>
</tr>
<tr>
<td></td>
<td>(50)</td>
<td></td>
<td>(46)</td>
<td></td>
<td>(74)</td>
</tr>
<tr>
<td>JCR</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>76</td>
</tr>
<tr>
<td></td>
<td>(64)</td>
<td></td>
<td>(169)</td>
<td></td>
<td>(159)</td>
</tr>
<tr>
<td>JM</td>
<td>201</td>
<td>14.9</td>
<td>79</td>
<td>20.3</td>
<td>116</td>
</tr>
<tr>
<td></td>
<td>(30)</td>
<td></td>
<td>(16)</td>
<td></td>
<td>(72)</td>
</tr>
<tr>
<td>JMR</td>
<td>—</td>
<td>—</td>
<td>126</td>
<td>57.1</td>
<td>285</td>
</tr>
<tr>
<td></td>
<td>(72)</td>
<td></td>
<td>(215)</td>
<td></td>
<td>(175)</td>
</tr>
<tr>
<td>JR</td>
<td>60</td>
<td>—</td>
<td>65</td>
<td>21.5</td>
<td>109</td>
</tr>
<tr>
<td></td>
<td>(0)</td>
<td></td>
<td>(14)</td>
<td></td>
<td>(76)</td>
</tr>
<tr>
<td>MS</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td></td>
<td>(35)</td>
<td></td>
<td>(66)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>261</td>
<td>11.5</td>
<td>403</td>
<td>39.5</td>
<td>858</td>
</tr>
<tr>
<td></td>
<td>(30)</td>
<td></td>
<td>(159)</td>
<td></td>
<td>(568)</td>
</tr>
</tbody>
</table>

¹ No. refers to the number of published empirical articles and notes. The numbers in parentheses refer to the number of articles and notes using statistical significance tests.
² % refers to the percentage of empirical research using statistical significance tests.

journal, such as happened with the JM in the late 1970s (Kerin, 1996), from an academic/practitioner journal to one almost entirely aimed at academicians. True, the AMA created a new journal (much later) in 1992, Marketing Management, targeted at practitioners. But separating the literature between 'them' and 'us' tends to reinforce for practitioners the notion that academicians essentially write for themselves.

We use proxy variables – the declining incidence of research by practitioners, as
`Original' empirical marketing research and knowledge development
Raymond Hubbard and R. Murray Lindsay

Table 3

<table>
<thead>
<tr>
<th>Years</th>
<th>Total no. of papers</th>
<th>No. of papers by practitioners</th>
<th>Percentage</th>
<th>No. of papers by practitioner-academic alliances</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Prior to 1960</td>
<td>710</td>
<td>305</td>
<td>43.0</td>
<td>314</td>
<td>44.2</td>
</tr>
<tr>
<td>1960–1969</td>
<td>785</td>
<td>239</td>
<td>30.4</td>
<td>278</td>
<td>35.4</td>
</tr>
<tr>
<td>1970–1979</td>
<td>1,194</td>
<td>141</td>
<td>11.8</td>
<td>239</td>
<td>20.0</td>
</tr>
<tr>
<td>1980–1989</td>
<td>1,356</td>
<td>114</td>
<td>8.4</td>
<td>197</td>
<td>14.5</td>
</tr>
<tr>
<td>1990–2000</td>
<td>1,427</td>
<td>117</td>
<td>8.2</td>
<td>196</td>
<td>13.7</td>
</tr>
<tr>
<td>Total</td>
<td>5,472</td>
<td>916</td>
<td>16.7</td>
<td>1,224</td>
<td>22.4</td>
</tr>
</tbody>
</table>

well as cooperative efforts between practitioners and academicians, published in our sample of nine marketing journals – to partly capture this phenomenon of greater insularity in the academic marketing world. In Table 3, for example, we show that prior to 1960 practitioners were responsible for 43.0% of the published research in marketing. The corresponding figure for the 1960s, 30.4%, is still impressive. For the last two decades, about 8% of the published research in our sample was written by practitioners. These declining numbers stand in stark contrast to the plea made by the AMA Task Force (1988) to publish more articles by practitioners. When considering the amount of work published by practitioner-academic alliances, we see that for the 1970s a respectable 20% came from this group. This number fell to 14.5% for the 1980s and 13.7% for 1990–2000. Table 3 shows that there has been a marked decrease in the proportion of work published by practitioners, and practitioner-academic joint ventures, over the period 1936–2000.

Table 4 reveals a great deal of variation in practitioner/practitioner-academic alliance publication rates by individual journals. Prior to 1960, both the JM (45.4%) and the JR (40.9%) published plenty of these papers. During the 1960s, the IJMR (79.5%) strikingly led the way in this regard, followed by the JAR (58.9%), JM (38.1%), JMR (20.3%) and, counterintuitively given both its numbers in the previous period and its popular perception as a practitioner-oriented journal, the JR (16.4%). This same general pattern has continued to the period 1990–2000. The IJMR (46.8%) and the JAR (37.6%) remained the journals most likely to publish research by practitioners, whether alone or in tandem with academicians. Figures for the JM (7.2%) and the JMR (7.2%) are, coincidentally, identical. Those journals featuring the least amount of practitioner-academic work are the FAMS (2.4%), JCR (2.8%), JR (4.5%), and MS (4.9%).

The information presented in Tables 3 and 4 lends indirect support for com-
marketing theory 2(4)
articles

Table 4
Proportion of papers authored by practitioners and practitioner-academic alliances by journal: 1936–2000

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>No.</td>
<td>%</td>
<td>No.</td>
<td>%</td>
<td>No.</td>
</tr>
<tr>
<td>EJM</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>58</td>
</tr>
<tr>
<td>JJMR</td>
<td>—</td>
<td>—</td>
<td>39</td>
<td>79.5</td>
<td>89</td>
</tr>
<tr>
<td>JAMS</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>149</td>
</tr>
<tr>
<td>JAR</td>
<td>—</td>
<td>—</td>
<td>141</td>
<td>58.9</td>
<td>118</td>
</tr>
<tr>
<td>JCR</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>110</td>
</tr>
<tr>
<td>JM</td>
<td>524</td>
<td>45.4</td>
<td>260</td>
<td>38.1</td>
<td>238</td>
</tr>
<tr>
<td>JMR</td>
<td>—</td>
<td>—</td>
<td>217</td>
<td>20.3</td>
<td>349</td>
</tr>
<tr>
<td>JR</td>
<td>186</td>
<td>40.9</td>
<td>128</td>
<td>16.4</td>
<td>141</td>
</tr>
<tr>
<td>MS</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>88</td>
</tr>
<tr>
<td>Total</td>
<td>710</td>
<td>44.2</td>
<td>785</td>
<td>35.4</td>
<td>1,194</td>
</tr>
</tbody>
</table>

1 No. refers to the total number of published articles and research notes. The numbers in parentheses refer to the number of practitioners plus practitioner-academic alliances.
2 % refers to the percentage of papers authored by practitioners plus practitioner-academic alliances.
3 It was not possible to consistently determine an individual’s employment affiliation in the EJM prior to 1985. Therefore, EJM data are only from 1985 onwards.

plaints by practitioners that academic research is becoming less and less relevant to their needs, and direct evidence that we are increasingly seen to be writing for ourselves. Consequently, there is a serious credibility gap between marketing practitioners and academicians regarding the usefulness of academic research. Does this same kind of gap manifest itself among academicians? That is, do academicians find their peers’ work to be useful in their own endeavours? We investigate this issue below.
Is there an academic–academic divide?

In order to determine whether marketing academicians find their peers’ work to be useful or important, we conducted some Social Science Citation Index (SSCI) analyses. Research possessing the qualities of usefulness or importance is more likely to be cited. Specifically, for selected years we chose a random sample of three journal articles (full-length articles, not research notes and comments) from each issue of our nine marketing journals. Thus, for each of the quarterly journals (IJMR, JAMS, JCR, JM, JMR, JR, and MS) we have a random sample of 12 articles per year, while for the JAR (six issues per year) and the EJM (10 issues per year) we have random samples of 18 and 30 articles per year, respectively. The first random sample of articles from each of the nine journals was selected in 1989, and we tracked the citation counts (self-citations omitted) for these articles over the next 11 years (1990–2000). We then took new random samples of articles from these journals, with citation count tracking years in parentheses, in 1991 (1992–2000), 1993 (1994–2000), and 1995 (1996–2000).

The results of the SSCI analyses are given in Table 5, where we present the annualized average (median) citation rates per journal. These annualized citation rates show how often the ‘typical’ article published in a journal was cited by others in an average year. For example, an entry of 1.00 in Table 5 for any given journal means that a ‘representative’ article published in that journal in, say, 1989 was cited an average of once a year, based on 11 years of citation-count tracking. Accordingly, the value of 0.10 for the EJM indicates that articles published there-in in 1989 were, on the average, cited only one-tenth of once per year. This is not a large number. Indeed, articles published in the IJMR (0.21), JAMS (0.22), JAR (0.39), and the JR (0.68) in 1989 were cited substantially less than once per year based on citation counts averaged over 11 years. Examining the citation rates for 1991, 1993, and 1995, we see that the EJM, IJMR, and the JAR never have attained an annualized average citation rate of even once per year; the JAMS managed this only one time (in 1995, 2.02), and the JR twice (1991, 1.23; 1993, 2.13). These data illustrate that even other marketing scholars are inclined not to cite the work appearing in these journals.

As might be expected, what are generally perceived to be the leading marketing journals — the JM, JMR, JCR, and MS — fared better in this regard. The JM recorded the two highest citation rates for 1993 (4.99) and 1995 (4.38), while the JMR (1.05) and MS (0.93) revealed the lowest, both for 1995. Across all four time periods, the JM (4.05) exhibited the highest annualized average citation rate, followed by the JCR (2.69), JMR (2.05), and MS (1.93).

Because average values are susceptible to the influence of outliers, a few well-cited articles can inflate the annualized average citation rates. Therefore, we also calculated the annualized median citation rates for each journal and time period. Table 5 shows that, with few exceptions, median citation rates were somewhat lower than their mean counterparts. Ranging from lowest to highest, we see that over the four time periods, annualized median citation rates were as follow: EJM (0 to 0.50), IJMR (0.11 to 0.21), JAMS (0 to 1.10); JAR (0.20 to 0.83), JCR (1.80 to
Table 5
Annualized average (median) citation rates per article from each journal

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>EJM</td>
<td>0.10</td>
<td>0.26</td>
<td>0.19</td>
<td>0.42</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
<td>(0.11)</td>
<td>(0.14)</td>
<td>(0.50)</td>
</tr>
<tr>
<td>IJMR</td>
<td>0.21</td>
<td>0.27</td>
<td>0.20</td>
<td>0.57</td>
</tr>
<tr>
<td></td>
<td>(0.18)</td>
<td>(0.11)</td>
<td>(0.21)</td>
<td>(0.20)</td>
</tr>
<tr>
<td>JAMS</td>
<td>0.22</td>
<td>0.11</td>
<td>0.51</td>
<td>2.02</td>
</tr>
<tr>
<td></td>
<td>(0.18)</td>
<td>(0)</td>
<td>(0.14)</td>
<td>(1.10)</td>
</tr>
<tr>
<td>JAR</td>
<td>0.39</td>
<td>0.96</td>
<td>0.42</td>
<td>0.33</td>
</tr>
<tr>
<td></td>
<td>(0.32)</td>
<td>(0.83)</td>
<td>(0.21)</td>
<td>(0.20)</td>
</tr>
<tr>
<td>JCR</td>
<td>2.68</td>
<td>3.57</td>
<td>2.92</td>
<td>1.60</td>
</tr>
<tr>
<td></td>
<td>(2.00)</td>
<td>(4.11)</td>
<td>(2.64)</td>
<td>(1.80)</td>
</tr>
<tr>
<td>JM</td>
<td>4.19</td>
<td>2.65</td>
<td>4.99</td>
<td>4.38</td>
</tr>
<tr>
<td></td>
<td>(4.95)</td>
<td>(2.50)</td>
<td>(5.14)</td>
<td>(2.40)</td>
</tr>
<tr>
<td>JMR</td>
<td>3.34</td>
<td>2.28</td>
<td>1.52</td>
<td>1.05</td>
</tr>
<tr>
<td></td>
<td>(2.32)</td>
<td>(1.56)</td>
<td>(1.36)</td>
<td>(0.60)</td>
</tr>
<tr>
<td>JR</td>
<td>0.68</td>
<td>1.23</td>
<td>2.13</td>
<td>0.62</td>
</tr>
<tr>
<td></td>
<td>(0.59)</td>
<td>(0.39)</td>
<td>(0.86)</td>
<td>(0.60)</td>
</tr>
<tr>
<td>MS</td>
<td>2.58</td>
<td>1.70</td>
<td>2.52</td>
<td>0.93</td>
</tr>
<tr>
<td></td>
<td>(1.45)</td>
<td>(1.78)</td>
<td>(1.02)</td>
<td>(0.70)</td>
</tr>
</tbody>
</table>

4.11), JM (2.40 to 5.14), JMR (0.60 to 2.32), JR (0.39 to 0.86), and MS (0.70 to 1.78).

We don't know what an 'acceptable' annualized average (median) citation rate should be, nor how it should possibly vary over the different marketing journals in our sample. However, the information in Table 5 indicates that for all the emphasis placed on publishing original research in marketing, much of this same work is not seen as being particularly useful or important even by fellow researchers, at least as judged by citations. This is especially pronounced outside of the very top-tier journals. From this perspective, there is evidence confirming something of an academic–academic divide, in addition to the gulf between academicians and practitioners.
Other impediments to the development of empirical generalizations

Earlier we reported how the cult of the isolated study, with its emphasis on obtaining statistically significant \( p < .05 \) differences in isolated data sets rather than searching for significant sameness over many data sets, militates against the establishment of EGs. In this section we briefly discuss four additional impediments to the development of EGs.5

Preoccupation with the hypothetico-deductive conception of science

The hypothetico-deductive conception of science elevates the role of theory and theory-testing over the importance of discovering EGs. As a result of this, a number of misconceptions about the nature of scientific work are perpetuated. These are, first, that hypothesis generation (discovery) and testing (justification) are quite separate and distinct exercises (Suppe, 1977) and, second, that a single study can yield immediate and lasting knowledge. Such beliefs are exemplified in the notion of the ‘critical experiment’, implying that theories can be conceived and evaluated over a short period of time. This, in turn, fosters a ‘been there, done that’ attitude among some researchers, as if the (statistically significant) results of a single study is the end of the matter. A third misconception is that the job of science is to uncover general or universal laws.

But, as Lindsay (1995) and McGuire (1983) point out, modern philosophy of science is completely at odds with this account. Instead, it views scientists starting with low-level theories (e.g. conjectures), which are systematically investigated within a research program. Recognizing this, ‘confirmation’ or ‘refutation’ is irrelevant since theories are only approximations of reality. The goal, instead, is to understand phenomena by examining the conditions under which a relationship does or does not hold over a number of studies. In making the boundary conditions explicit, this process helps clarify the meaning and applicability of theories.

Consequently, we would do well to dispel the three misconceptions surrounding the scientific process mentioned above. Hypothesis generation and testing are not separate activities. Following McGuire (1983), gathering data to empirically evaluate theoretical conjectures should be viewed as part of the discovery process and not as a testing procedure to ascertain the truth or falsity of the original hypothesis. Moreover, the idea of the ‘critical experiment’ that provides instant answers is a myth that helps populate the marketing literature with a host of unverified works. Obtaining EGs is an ongoing process that takes time. Finally, it should be clear that EGs are not universal laws; rather, there are some conditions under which they hold, and others where they do not. A major aim of science is to discover these conditions.
Preoccupation with statistical rather than empirical generalization

Because of the lack of replication research in the business disciplines, some researchers (e.g., Bamber et al., 2000; Hubbard, 1994; Wells, 2001) have argued that too much importance is attached to initial results, with an accompanying tendency to over-generalize early evidence. One reason for this state of affairs is the failure among some researchers to distinguish between the ideas of generalizing to a population as opposed to generalizing across (sub)populations (Cook and Campbell, 1979; Lynch, 1999). To these researchers, external validity is restricted to the former, i.e., to statistical generalization. This explains the preoccupation with increasing the representativeness of the sample via random sampling, using large samples, calculating sample averages, and relying on statistical significance tests to delineate real from chance results. Cook and Campbell (1979) refer to this as the ‘representative model’ of generalization. In science, however, external validity is concerned with generalizing results across subpopulations (what we call prediction or EG) and this is very different from statistical generalization. Predictive studies, of primary concern in science, attempt to determine the scope over which results hold, and to identify other situations where they do not.

Contrast this with statistical generalization, whose validity is predicated on random sampling procedures, which is geared to the estimation of some population parameter(s) of interest. Suppose, for example, that in a worldwide study we discovered that male customers were twice as loyal as females to their supermarkets, a finding significant at the .05 level. How generalizable is this result? Many variables affect this basic outcome, such as, does it hold for males and females in different countries (including further variations, e.g., among developed and developing nations; Muslim, Jewish, Christian countries, etc.)? Do the supermarket loyalty results generalize to men and women of different age, income, occupational and other demographic characteristics? And to other operationalizations of the construct, ‘supermarket loyalty’? Furthermore, do the supermarket loyalty findings hold up over time? All of these conditions (subpopulations) must be explicitly taken into account before we can be confident about announcements that male supermarket shoppers are twice as loyal as their female counterparts. Encouragingly, it should be noted that some of these conditions may be incorporated in the design of the project, and the analyses therein (does the same result - men being twice as supermarket loyal as women - obtain within the various subpopulations?).

A $p < .05$ value from a significance test provides evidence for the statistical generalization that the aggregate measures of central tendency, the sample proportions in our example, differ systematically (i.e., not by chance assuming a true null hypothesis) in the total population. Therefore, our low $p$-value suggests that, on average, men are twice as loyal as women to their supermarket choices throughout the world. But this particular inference does nothing to improve our prediction about the supermarket loyalty of 30–40-year-old females in Germany in the 1990s. This remains so no matter how large the sample size (smaller confidence interval) becomes.
In fact, Starbuck (1993: 889) warned: '... a true statement about averages across a population may be false for every subset of that population, including every individual in that population'. This is precisely what Lynch (1999: 369) means when he speaks of 'aggregation biases' resulting from ignorance of background factors (for instance, age, gender, country, time period, in our example) when attempting to generalize to some super-population. It is also why Deming (1993) calls statistical generalizations useless when it comes to predicting whether a particular relationship holds or not. The key element in external validity is empirical generalizability, i.e. the ability to generalize across subpopulations, as opposed to a superpopulation.

Denigration of replication-with-extension research

It was previously reported that EGs can precede theory or can be predicted by theory. But as Ehrenberg (1993a) remarks, although low-level theories (e.g. conjectures) help researchers determine what data to gather and analyze, theoretical explanation usually comes after a pattern ('stubborn facts', 'data points') has been empirically demonstrated through a sequential process of data and theory interactions over an increasingly broad range of varying conditions. Once demonstrated, the emphasis is then on descriptively modelling the patterns and devising explanations for the findings. By this time much will be known, e.g. that certain conditions do matter, whereas others do not, providing for a deeper understanding of the phenomenon and the possibility of relating the result with those in other areas. Such understandings spur additional theoretical speculations which, in turn, invite more empirical testing. The cycle of data-theory interplay continues and leads to a theory being 'grounded'.

From this account it should be clear that replication-with-extension research is the mechanism for deriving EGs. Studies must be repeated and extended in such a manner as to discover the scope and boundaries of EGs. Yet despite the crucial role of this type of research in the knowledge development process, it continues to receive little attention by academics. This is curious because no one would argue against the principle of replicability as a cornerstone of the scientific method. On the other hand, most researchers, being pragmatists, recognize that a considerable editorial and reviewer bias exists against the publication of replication research (see, e.g. Kerr et al., 1977; Madden et al., 1995), and so opt for submitting original or novel work. Which brings us to the present position of having a huge empirical literature consisting in the main of piecemeal and fragile results that are of limited use for establishing EGs and for guiding marketing practice.

Nor are present meta-analyses as helpful as they might be, suffering as they do from construct validity problems (Bangert-Drowns, 1986). Variation in construct measurement means that meta-analyses must often incorporate only the crudest replications of studies dealing with a particular topic, thus curtailing generalizability. Systematically pursued replications-with-extensions would make meta-analyses more interpretable. But even here, Barwise (1995) cautions that meta-analyses are chiefly concerned with combining the results of several studies
to test qualitative hypotheses. What is really needed are EGs capable of yielding quantitative predictions.

There is, however, some small indication that the stock of replication research is on the rise. In an editorial in the JMR, for example, Winer (1998) indicated a willingness to consider such work for publication. Similarly, Mick (2001) announced a new section of the JCR, titled Re-Inquiries, which aims to publish research corroborating or questioning the robustness of previous findings. The question remains whether these initiatives in two of marketing’s most prestigious journals, welcome as they are, will be sufficient. They seem to be more a function of these particular editors’ personal viewpoints than a sustained commitment to change long-term editorial policy to one explicitly valuing replication research. Will editors of these and other journals follow suit? Certainly not the editor of the JM (Stewart, 2000: 688) who, in commenting on Hubbard and Ryan’s (2000) call for the need to publish far more replication research, observed that ‘most replication research, even with extension, is not very interesting’. But programmatic replication-with-extension research, the key to discovering EGs, goes well beyond being interesting: it’s compelling.

Academic reward structure

The reward structure in academe also stifles knowledge development in the discipline. We teach in class about how companies need to think in terms of long-run performances, and not be too focused on short-run quarterly reports. But we do not practice what we preach. The ‘publish or perish’ mentality which drives the academic reward system is notoriously short-run in orientation.

A brand new PhD recipient typically has a maximum of six years in which to earn tenure. In the ‘research universities’ it is primarily the publication of one’s work in scholarly journals that counts in the tenure decision. To survive in this system, it is imperative that the new assistant professor or lecturer get off to a good start by publishing something out of his or her dissertation, and, by definition, dissertation research has to be ‘original’.

But once tenure is granted, subsequent promotions, salary increases, and other perks such as traveling to conferences, remain critically dependent on research publications. So, even those with tenure are unlikely to invest in replication-with-extension programs aimed at the development of EGs. In the first place, these programs are of a long-term duration, whereas it is the ability to publish ‘this year’ that will be rewarded. In the second place, as we have already pointed out, researchers are aware of the bias against publishing replication-with-extension research and strive to continue publishing novel work.

The AMA Task Force was cognizant of the negative consequences the short-run ‘publish or perish’ scenario has on knowledge acquisition. Indeed, the Task Force (1988: 6) sadly concluded that: ‘It is probably fair to say that the current system has led to conditions under which a significant contribution to knowledge is not at the forefront of most participants’ thoughts as they engage in the research and publication process’.

396
Some philosophical issues

For some readers, particularly those advocating humanistic and qualitative research approaches, the attempt to establish quantitative, factual regularities may smack of 'brute empiricism'. Consequently, some comments addressing this concern are useful in order to reduce the possibility of alienating those who might attribute (incorrectly) a 'logical positivist' orientation to our framework.

In the main, we agree with Cook and Campbell's (1979) response to this same charge and refer the reader to their discussion, thereby allowing us in the limited space available to concentrate on five specific issues. First, the brute empiricism that has been so rightly criticized takes a cavalier and uncritical approach to measurement, whereby the operationalization is considered to be unproblematically equivalent to the construct; in addition, the espousal of such ideals as large-scale sampling and the pursuit of universal laws, the primacy of internal validity considerations and theory testing, the acceptance of 'the critical experiment' and, perhaps worst of all, the mechanization of our research through the ubiquitous use of statistical analysis and inference methods, have, in combination, had a hugely debilitating impact on the marketing discipline. The EG replication-when-extension methodology shares none of these attributes while still attempting to pursue the goal of attaining an inter-subjectively verifiable (although tentative and fallible) knowledge, i.e. a perspective of realism without the ideals of truth and universal generalizations.

Second, some observers question whether it is even possible in the management and social sciences to obtain predictable, recurring patterns given that facts are socially constructed (see the discussion in Ehrenberg and Bound, 1993). The concern here is that since all observation is theory-laden, i.e. based on viewing the world through a particular perspective, there can be no fixed, objective reality. Consequently, the search for immutable facts is considered to be chimerical. However, acceptance of the premise underlying this conclusion does not imply that achieving genuine knowledge is impossible and that one must necessarily fall into a relativist position, wherein all conclusions are inherently non-objective (paradigm dependent) and therefore equally plausible. Instead, we need only to remove ourselves from the positivist's building-block conception of science, whereby each study represents a permanent brick of knowledge which adds to those provided by predecessor studies. In its place, we need to understand that scientific 'facts' are derived from the social phase of 'testing' a solution over an extended period of time. According to Ravetz (1971), in his landmark study of the nature and production of scientific knowledge, scientific facts must pass three tests: a 'social test of significance' (the scientific community believes the result warrants continued research), 'empirical stability' (the result is capable of being empirically replicated), and 'empirical invariance' (the result can be generalized to applications other than those peculiar to its own construction). Consequently, despite the fact that scientific representations are socially constructed, over time it is possible to discriminate between those representations which do or do not possess a reasonable correspondence to the world as we know it, based on
marketing theory 2(4)
articles

science's empirical nature (Giere, 1999). The account described here is entirely consistent with this social view of science.6

Third, while as yet uncommon, meaningful empirical regularities have been discovered in marketing. We mentioned a number of these at the beginning of the paper. We offer one more here, which involves the 'double jeopardy' (DJ) phenomenon. As Ehrenberg (1993b) explains in an article published in Nature titled 'Even the Social Sciences Have Laws', DJ says that small brands are 'punished' twice: they have fewer buyers and their buyers purchase the brand less often than larger brands (low loyalty). This pattern is almost universal, applying to different countries, products, time periods, and distribution channels. Over the course of many years a model has been developed which accurately predicts buying patterns and requires only a single input: each brand's market share. Other potential market influences such as product formulation, level of advertising, price, and availability are not involved and do not affect brand loyalty.

Fourth, Wells (2001) argues convincingly that the burden of assessing the generalizability, or external validity, of findings is by no means restricted to those employing quantitative approaches; it applies equally to those favouring qualitative designs. As Wells (2001: 496) made clear: 'Whether the study is positivistic or interpretive, generalization must be earned.' This is quite right.

Finally, while the emphasis in this paper has been on acquiring routinely predictable results, this is not considered to be the primary goal of science. Like Ehrenberg and Bound (1993), we believe that developing empirically grounded theory (explanations) and causal understanding of phenomena, along with practical applications of theory, are ultimately what is sought in science. However, obtaining predictable regularities is a necessary (though not sufficient) condition for attaining this goal.

Conclusions

We have indicated that the growing empirical literature in marketing is made up overwhelmingly of one-off results based on isolated data sets. As a consequence, it should not be surprising to learn that there are few EGs in the discipline. That EGs are few and far between is because, among other things, we barely attempt to establish them in the first place (Barwise, 1995). Instead of promoting concerted efforts to search for EGs, the reward structure in academic virtually obligates researchers to continue publishing original works if they hope to survive and prosper within the system. It is suggested that much of this original research is of little value to either marketing practitioners or scholars. Nor, according to the AMA Task Force (1988), are the great majority of these original works truly innovative in the first place.

Rather than the current emphasis on original research dedicated to theory-testing, a more productive route to knowledge development in marketing would be to concentrate, and reward, the coordinated search for EGs through ongoing replication-with-extension research programs aimed at uncovering the range and
boundaries of EGs. As Hunter (2001: 157) declares: 'In science, facts are at least as important as ideas. Replication studies are desperately needed in order to determine facts'. Perhaps if the preoccupation with 'originality' were relaxed, we might have more of a chance of establishing 'facts', and the promise of EGs that are reliant on them.

Notes

1 Kerin and Sethuraman (1999) believe that the other three regularities suggested by Sheth and Sisodia (1999), namely, diffusion of innovations, retail gravitation, and the market-share–ROI relationship, would seem to be candidates for lawlike generalizations.

2 In a sample of articles from the JMR and the JCR for 1994–1998, Simonson et al. (2001: 261) observed that some 66% of them were classified as 'applications or minor extensions of established theories and phenomena...'. This figure, down from 94% for 1969–1973, is considerably at variance with the 3% of marketing studies labelled as replications-with-extensions as reported by Hubbard and Armstrong (1994) and Hubbard and Vetter (1996). The discrepancy between these numbers arises because the Simonson et al.'s (2001) results incorporate far more than replications-with-extensions. They would also include what we refer to as mainstream studies dealing with theory development and testing. Otherwise expressed, their data pertain more to theory extensions than to extensions of previous empirical results. In addition, the Hubbard and Armstrong (1994) and Hubbard and Vetter (1996) estimates of only about 3% of published empirical research in marketing being replications-with-extensions is corroborated by similar results from others. Thus, for example, Brown and Coney (1976) estimated that 2.8% of marketing's literature consists of replications-with-extensions, while Zinkhan et al. (1990) estimated this figure to be 4.9%.

3 Rossiter (1994, 2001) claims that it is not in the manager's best interest to use a generalized academic theory to which all competitors have access, unless they are more successful at implementing it. Barwise (1995: G31) counters that: 'The fact that an EG might eventually be known and "applied" by everyone and cease to be a source of competitive advantage is not, to me, a big issue: as with any innovation, the commercial rewards will go to those who adopt sooner and better.'

4 Practitioners were defined as marketers with a non-academic affiliation, such as individuals working in private businesses, government agencies, consultancies, and the like.

5 A fuller account of the first two impediments is given in Lindsay and Hubbard (2002).

6 Tsang and Kwan (1999) provide a 'critical realist' philosophical perspective, which Rossiter (2001) subscribes to, that is commensurate with this viewpoint.

References


Rossiter, J.R. (1994) 'Commentary on A.S.C. Ehrenberg's "Theory or Well-Based


R. Murray Lindsay is Associate Professor in the Management Accounting and Control area at the Richard Ivey School of Business, University of Western Ontario. He has a special interest in developing a theory of replication and generalization, and examining the role modern statistical methods play within such a theory. His methodological work has appeared in such journals as Accounting, Organizations and Society, The American Statistician, Behavioral Research in Accounting, Contemporary Accounting Research, and Accounting and Business Research.

402