Restoring *Phronesis* and Practice: Marketing's Forgotten P's

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Abstract

Purpose – The purpose of this paper is to examine the evolution of marketing's philosophical conversation over the past 120 years, focusing on the emergent meaning of the notion that marketing should become more "scientific".

Design/methodology/approach – This paper focuses on the US academic marketing literature, primarily journal articles and books published in the first half of the 20th century.

Findings – The Aristotelian distinction between *techné, epistemé* and *phronesis* provides a rich basis for framing philosophical discussion in marketing and should supplant the art – science debate and Anderson's distinction between science$_1$ and science$_2$. Prior to 1959, the marketing journals provided a forum for *phronesis*, though this diminished, as the academic marketing community largely abandonedthe inductive, contextual approach in favour of a deductive, "scientific" methodology. The Ford Foundation played an important role in affecting this change.

Practical implications – The paper highlights the importance of forums where practitioners canreflect on the ethical and social implications of their practices and then work to enhance these practicesfor the greater social good.

Social implications – Questions the value of distinctions between marketing theorists andpractitioners and the consequential focus of marketing journals.

Originality/value – Advances the concept of phronesis in the marketing literature and distinguishesit from epistemé, which has dominated academic marketing discourse over the past 60 years.

Keywords Marketing history, Ethics, Epistemology, Phronesis, Critical marketing, Critical history

Paper type Research paper

Introduction

When I told some colleagues I was writing about marketing's ruminations on philosophy they immediately referred to the 'science-versus-art' debate and specifically mentioned Stephen Brown's (1996) enthralling description of how that dialogue had evolved. However, rather than reprising Brown's excellent piece – or indeed other articles by Kavanagh (1994), Egan (2009), Saren (2010), and Hunt (this issue) – this paper seeks to relate marketing's 'art–science' debate to wider conversations about science, social science, management knowledge and practice, not least because the meaning of concepts like 'science' and 'social science' are always contested, emergent, and situated in time and place.

According to Brown (1996), the debate about whether marketing is an art or a science was 'ignited' by Paul Converse's paper, *The Development of the Science of Marketing* (Converse, 1945). However, as Brown acknowledges, Converse's essay was a rather routine survey of marketing scholars and practitioners aimed at identifying "the more important contributions to the science or art of marketing" (1945, p. 15) and only included "a couple of throw-away remarks about the 'art or science of marketing'" (Brown, 1996, p. 90). Indeed, a study of pre-war marketing discourse in the United States makes it clear that there wasn't really an art versus science debate, but that instead the dominant agenda within the marketing community was to make the discipline more 'scientific'. Hence, rather than structure the debate around the art-versus-science axis, my approach is to try to understand and contextualise what was meant by this idea of making marketing more 'scientific'. The first section of the paper, *Science Outwith Marketing*, seeks to do this by describing the particular understanding of science, and social science in particular, that emerged during the first half of the twentieth century. The paper argues that central to this understanding was the important status accorded to mathematical modelling. Crucially, this understanding was effectively absent from marketing discourse, which is described in the next section of the paper, *Marketing Without Science*. This changed quite rapidly in the late 1950s and early 1960s as marketing academics adopted the mathematical modelling paradigm, curiously, just as it was beginning to lose some of its lustre elsewhere.

The next section of the paper focuses on Paul Anderson's important distinction between science$_1$ and science$_2$, which he first articulated in his 1983 article, *Marketing, Scientific Progress and Scientific Method*. Drawing on Flyvberg (2001), amongst others, I argue that Anderson's distinction is problematic and that it has, in many ways, led the conversation into something of a cul-de-sac. Instead of the distinction between science$_1$ and science$_2$ – and

indeed instead of the art–science dichotomy – Aristotle's tripartite structure of *techné-epistemé-phronesis* is presented as a richer, more fruitful basis for framing philosophical discussion and inquiry in the field of marketing. In particular, I argue that marketing's epistemological debate is deficient because it hasn't engaged with the concept of *phronesis*, which should provide, not only a foundation for marketing thought, but also a vital link between marketing theory and practice. The paper then proceeds to discuss a particular Aristotelian understanding of practice that is associated with *phronesis*, as developed by the philosopher Alasdair MacIntyre and others such as Joseph Dunne. The final part of the paper revisits marketing's early years, through a study of early publications in the *Journal of Marketing* where we find that, while the language may have been different, there were strong resonances with the themes of *phronesis* and practice explored in this paper. The paper concludes with a discussion on how this historical analysis might inform contemporary marketing.

The paper is a study of the history of some of the ideas that constitute marketing thought, and as such it can be subjected to the routine criticisms thrown at 'history of ideas' projects. In particular, such projects can find it hard to justify why certain events were selected from the infinite number of events that have happened in the past, and why particular boundaries were drawn. They can also be criticised for wittingly or unwittingly endorsing existing power structures and for privileging some voices rather than others in the telling of a story. This narrative is centred on the United States because marketing was born as an academic discipline there, and because the major developments during the discipline's early period originated there. The story draws largely on the available academic literature on marketing, especially issues of the *Journal of Marketing* published between 1930 and 1960. One striking consequence of this is that the narrative contains no female voice.[1]

Science Outwith Marketing

The attempt to make marketing more 'scientific' was an ever-present theme in marketing discourse between 1900 and 1960. However, to understand that endeavour we must first contextualise what was meant by 'science' during that period, and, more particularly, what was meant by 'social science', which is why we begin by focusing on debates and practices *outside of* rather than within marketing. In this discussion it is important to remember that 'science' is a concept that emerged through contestation and debate. The meaning of the word was, and is, continually being reinterpreted, and so it might be proper to signal this by always hedging the word with scare quotes, though to do so would be distracting. Such scare

quotes would surely be appropriate in the early twentieth century as a stream of major 'scientific' inventions from the latter part of the nineteenth century began to diffuse through American society – e.g., mild steel, the telephone, lightbulb, phonograph, and automobile. A host of spectacular technologies were also being developed at that time, such as the airplane, helicopter, commercial radio, television, motion pictures, the liquid fuel rocket, aerosols, penicillin, while there were also profound changes in understandings of the individual, society and the cosmos. Importantly, what was understood as science increasingly came to be linked to the application of new developments in mathematics, especially through the advances in probability theory and statistics that were made during the late 19th and early 20th centuries. During that period, Galton introduced the concepts of the standard deviation, regression to the mean, and correlation; Poincaré developed a new branch of mathematics called the 'qualitative study of differential equations', which provided the basic armoury for the mathematical analysis of dynamic systems; Pearson (1857–1936) formulated many statistical techniques that are commonplace today, such as hypothesis testing, Pearson's chi-squared test, and principal component analysis; while the central limit theory, which Tijms (2004, p. 169) describes as "the unofficial sovereign of probability theory", was proved precisely and formulated in general terms in 1901.

These developments in statistics provided a growing arsenal of quantitative techniques ripe for application, and indeed they found ready use in two fields: biology – where a new sub-field of mathematical biology emerged – and thermodynamics, and, in due course, scholars who would now be identified as social scientists looked to both of these fields as they sought to understand and model the social world. A major development occurred in biology in the 1920s, when the biophysicist and statistician Alfred Lotka and the mathematician Vito Volterra simultaneously and independently formulated a set of differential equations that modelled the dynamics of an ecological system with predator-prey interactions, competition, disease and mutualism.

Thermodynamics became influential, partly through the work of Willard Gibbs (1839–1903) and his protégé E.B. Wilson who became a mentor to the American economist and Nobel Laureate Paul Samuelson. Drawing on ideas from thermodynamics, Samuelson depicted a mathematical representation of economics that privileged analogies with physics, biology, and thermodynamic systems at or near equilibrium. Samuelson was an occasional member of Harvard's famous 'Pareto Circle' – named after the Italian sociologist/economist Vilfredo Pareto – which promulgated a mechanical-system model of society as a set of mutually

interdependent and interrelated components tending towards equilibrium, and which had a significant influence on the development of the social sciences during the 20th century (Cot, 2011; Heyl, 1968; Isaac, 2010; Keller, 1984).

In the same year in which the Pareto Circle was formed, 1932, the Cowles Commission for Research in Economics was also founded, and this institute played a defining role in the development of social science for the next three decades. The objective of the Cowles program was to describe the workings of the economy through constructing and analysing a set of simultaneous equations derived from economic theory, mathematics, statistical methods and observed data. It was a relatively small organisation, fluctuating in number between 30 and 50 individuals, but it was hugely influential. Between 1939 and 1955 the Cowles Commission created a "revolution" in econometrics through the work of its 'associates', twelve of whom subsequently becoming Nobel laureates (Christ, 1994). The 'revolution' spread beyond economics into fields like finance, which up to the 1950s had been dominated by *ad hoc* theories largely devoid of systematic analysis (Jensen & Smith, 1984).

In 1955, the Cowles Commission moved from the University of Chicago to Yale University, which roughly coincided with the emergence of another influential group dedicated to the mathematical analysis of social phenomena. This was the newly-formed Graduate School of Industrial Administration (GSIA) led by Lee Bach and Herbert Simon in the Carnegie Institute of Technology. It differed somewhat from the Cowles Commission in that its commitment to inter-disciplinary research meant that it spanned more fields, but it had the same enthusiasm for deductive reasoning and mathematical modelling. It was no bigger than the Cowles Commission but was equally, if not more influential, across a range of disciplines, garnering six Nobel Prizes in the process.

The history and influence of GSIA is well known and documented (Augier & March, 2002; Augier & Prietula, 2007; Crowther-Heyck, 2006; Hosseini, 2003; Tadajewski, 2009), but a key part of the story is that it could not have happened without the financial backing of the Ford Foundation. Set up in 1936 by Henry Ford's son Edsel, it became the largest philanthropy in the world after it was bequeathed the non-voting stock of the Ford Motor Company upon the death of Edsel and Henry Ford in 1943 and 1947 respectively. While its potential remit was wide, it was particularly committed to infusing scientific theory, methods and analysis into U.S. business administration, with the objective of winning the peace-time economic battles much as science was seen as vital to the war effort. To this end, the Foundation's focus was on changing the research agenda, doctoral programmes and teaching

approaches in US business schools, which were seen as too descriptive and 'unscientific'. Beginning in 1953, the strategy was to prototype their ideas in just a small number of schools, one of which was GSIA (Schlossmann, Sedlak, & Wechsler, 1987). As Augier and March (2011) put it, "The Ford Foundation found a poster child in GSIA, and GSIA found a sugar daddy" (p. 124).

In many ways, the GSIA group were continuing the tradition of the Pareto Circle and the Cowles Commission in using sophisticated mathematics and statistics to deductively analyse social phenomena. This pioneering work was being done by a relatively small group of individuals, and the Ford Foundation was the glue that sustained this social network. Herbert Simon's connections neatly illustrate this point. Simon had completed his undergraduate studies in the University of Chicago where he was influenced by the logical positivist philosopher Rudolf Carnap, the biophysicist Nicholas Rashevsky (often seen as the founder of mathematical biology) and Lotka's (1925) *Elements of Physical Biology* which used relatively sophisticated mathematics to model dynamic systems (Crowther-Heyck, 2005, p. 66). Simon was especially attracted to Rashevsky's application of complex mathematics to empirical problems, and much of his subsequent career can be seen as extending Rashevsky's mathematical biology to social phenomena. An early and influential example of this was his reworking of a model of group behaviour first proposed by George Homans (1950), one of the members of the Pareto Circle (Simon, 1952) (reproduced in Simon (1957/1987)). Even though Homan's model was non-mathematical, its origins were in the Pareto Circle and so it was suited to 'mathematisation', which Simon did by converting it into a set of differential equations from which various deductions could then be made.

During the 1950s, Simon had spent some summers working in the RAND Corporation, which shared GSIA's commitment to inter-disciplinary research, 'big science', mathematical economics, mathematical modelling, systems analysis and operations research. At that time the RAND Corporation was transitioning from an original focus on military projects into social welfare research, a transition that was facilitated and funded by, unsurprisingly, the Ford Foundation.

The Ford Foundation also provided the financial backing for the Center for Advanced Study in the Behavioral Sciences, which was founded in 1954. One of that organisation's first activities was to help convene a meeting of the *American Association for the Advancement of Science,* attended by, among others, Ludwig von Bertalanffy, Kenneth Boulding, Ralph Gerard, and Anatol Rapoport all of whom shared an interest in the commonalities between

biological and cultural evolution, in the concept of general systems, and in the application of sophisticated mathematics to understanding such phenomena (Crowther-Heyck, 2005; Gerard, Kluckhohn, & Rapoport, 1956; Rapoport, 1953). From this meeting, the *Society for General Systems Research* was founded.

All of this activity created a commonality of purpose within a relatively small academic community and even smaller clusters, such as the Pareto Circle (1935–1942), the Cowles Commission (1932–1955) and GSIA (1955–1964). The question now, though, is where did the academic *marketing* community fit – or not fit – in this network, given that that community's repeatedly stated objective, during the first half of the twentieth century, was to make marketing more 'scientific'?

Marketing Without Science

It is very clear that, up until around 1960, the academic marketing community showed little interest in developing sophisticated mathematical models of market phenomena through appropriating techniques from biology or thermodynamics. With few exceptions, the particular version of 'science' that favoured deductive analysis and mathematical modelling, which was having a profound impact elsewhere, was effectively absent from marketing. Instead, a quite different understanding of 'science' and 'scientific' seems to have been articulated in marketing, even though many might see this as 'unscientific'.

The German 'historical' school of economics, which emerged in the late 19th century, was especially important in the earliest years of American marketing thought (Jones & Monieson, 1990). The historical school saw history as the primary source of understanding about human actions, and took the view that, since economies are always culture-specific, economic 'theories' were not generalizable over time or space. Consequently, they argued that economic analysis should be done through careful empirical, inductive and historical reasoning rather than through the use of logic, mathematics and deduction. In short, their endeavours were the polar opposite to the type of work being done in GSIA.

During the nineteenth century a large number of American students obtained their higher education in Germany, while many German-trained economists returned to North America in the latter part of that century, including Richard T. Ely who formed the American Economic Association in 1885. One of Ely's students was Edward David Jones who taught the first university course in marketing and who argued that the appropriate methodology for studying markets and marketing was "the inductive form of the scientific method" (Jones, 1913, p.

191). Edwin Francis Gay had also spent five years in Germany during the 1890s undertaking postgraduate study in history and political economy, before becoming the first Dean of Harvard Business School in 1908. His enthusiasm for induction underpinned the School's early and influential promotion of the case method in teaching (Jones & Monieson, 1990).

Gay liked Frederick Taylor's (1911/2010) ideas on 'scientific management' and incorporated them in the curriculum of the new business school (Nelson, 1992). Taylor's book, *The Principles of Scientific Management* reflected and influenced the *zeitgeist* of the United States in the early 20th century as progressives advocated the use of 'scientific' methods to address social, technical and political problems and to improve national competitiveness through increasing efficiency in the workplace. Not surprisingly, therefore, it provided a ready template for the application of 'science' and the 'scientific' method to marketing. An early adopter of these ideas was the engineering graduate Charles Hoyt, who argued in his book, *Scientific Sales Management* (Hoyt, 1913) that the 'old' salesman, whose expertise was based on personality and contacts, should be replaced by a 'new' type of salesman who was "scientifically selected, trained, motivated and directed" (La Londe & Morrison, 1967: 10). Percival White expanded these ideas beyond sales management and his book, *Scientific Marketing Management* (White, 1927), was an explicit attempt to apply Taylor's ideas in the wider domain of marketing (Jones & Tadajewski, 2011; Tadajewski & Jones, 2012). Like Taylor and Hoyt, White was an engineer, which gave him the metaphors and mindset for his analysis of markets and marketing. What he described as a 'marketing engineer' should solve marketing problems using a systematic, evidence-based approach akin to the way Taylor's 'industrial engineer' solved production problems without recourse to heuristics or rules of thumb (White, 1921). White's arguments had an important, if largely forgotten, impact on marketing thought and led to the development of various 'scientific' approaches to marketing practice, such as the application of time and motion study to the work of salesmen (Jones & Tadajewski, 2011; Nolen, 1940; Tadajewski & Jones, 2012). However, in retrospect, the translation of scientific management into marketing was largely unsuccessful if we accept Burger's observation in 1959 that, while manufacturing had produced scientific management over 50 years previously, there was still no equivalent development in marketing: "The essentials of scientific method are observation, deduction, hypothesis, and verification. Marketing, however, has yet to take the first step" (Burger, 1959, p. 246). Moreover, notwithstanding the leveraging of the words 'science' and 'scientific', the introduction of scientific management into marketing scholarship in the early 20th century might be better

seen as a shift from sovereign to disciplinary power, rather than anything much to do with 'science' (Fougère & Skålén, 2013; Skålén, Fellesson, & Fougère, 2006). As Taylor (1911/2010, p. 7) succinctly put it: "In the past the man has been first [sovereign power]; in the future the system must be first [disciplinary power]".

The prominent marketing scholars who founded the American Marketing Society (AMS) in 1931 and the *American Marketing Journal* in 1934 clearly believed that marketing was unscientific and that this needed to change. According to Kerin (1996, p. 1) both AMS and its journal had two goals, one of which was to "advance science in marketing by providing for the systematic study and discussion of marketing problems". This scientific agenda was also manifest in the articles published in the *Journal of Marketing* (the successor of the *American Marketing Journal*), such as in Coutant's (1937) paper, 'Scientific Marketing Makes Progress', in which he wrote that the AMS (of which he was then President) was "an organization devoted to advancing the use of science in marketing" (p. 226). In the same issue, N.H. Engle (1937), then Assistant Director with the Bureau of Foreign and Domestic Commerce, wrote that the Bureau's emerging marketing research objectives were (a) focused on marketing research of national significance; (b) distinct from the type of research undertaken by private research agencies; and (c) concerned with "determining a more scientific marketing procedure" (p. 282). This focus on science and the scientific method is also clear from a number of other early articles in the journal, such as one by McGarry (1936) on 'The Importance of Scientific Method in Advertising' and another by Raymond (1937) titled 'Direct Advertising also Favors Scientific Marketing'.

What is clear from examining the marketing journals between 1930 and 1960 is that marketing scholars were not doing the type of mathematical modelling that had become fashionable in other fields in the social sciences around that time. There were, however, a small number of possible exceptions that are worth highlighting. For instance in a series of *Harvard Business Review* articles, Lydon Brown (1937a, 1937b, 1937c) advocated the use of quantitative methods to estimate market size, though his approach was very rudimentary. Likewise, a number of papers were published during the 1950s explaining the potential application of regression analysis to marketing scholarship and practice (Ferber, 1954; Myers, 1959), though in terms of mathematical sophistication these paled in comparison with the type of work being done in, for example, GSIA.

One person who might have integrated mathematics and marketing was Paul Lazarsfeld who, during the 1950s, became one of the founders of mathematical sociology. Lazarsfeld grew up

in Vienna where he obtained a doctorate in mathematics (on the mathematical aspects of Einstein's gravitational theory) and where he directed a range of market research studies during the 1920s (Fullerton, 1990). He emigrated to the United States in 1933, and within a few years had written articles for the *Harvard Business Review* and the *Journal of Marketing* (Lazarsfeld, 1934, 1935, 1937), and contributed four chapters to the American Marketing Association's handbook on marketing research (Wheeler, Bader, & Frederick, 1937, chapters 3, 4, 11 and 15). Given his mathematical abilities and his interest in understanding social phenomena, he would probably have been at home in GSIA or the RAND Corporation and might perhaps have formed an important link between their activities and the marketing community. However, he drifted away from marketing scholarship after he was appointed Director of the Radio Project at the University of Newark in 1936. His career flourished during the 1940s and 1950s, especially when he moved to Columbia University where the Radio Project grew into the renowned Bureau for Social Research (Jeřábek, 2001). In 1954 he published a collection of papers titled *Mathematical Thinking in the Social Sciences* (Lazarsfeld, 1954), which helped build his reputation as one of the co-founders of mathematical sociology. It is perhaps telling that someone who is now seen as one of the most important sociologists of the 20th century seems to have stayed in the marketing community for a relatively short time, making no contribution to the marketing literature after 1937.

It is difficult to know why marketing was late in catching the mathematical modelling train, but the strategy of the Ford Foundation strategy was certainly crucial, given that it was fuelling the train through its decision to "pour large sums of money into a few reasonably good or promising schools of business which would then be the instruments of change for the rest of the field" (Howell 1966 in Augier & March (2011, p. 111)). Thus, between 1956 and 1961, the Ford Foundation donated $11m to Stanford and Harvard and another $11.5 to Chicago, Carnegie Tech, Columbia, UCLA, UC Berkeley and MIT, with the specific purpose of making business more scientific. But except for Harvard, none of these had vibrant marketing groups, and Harvard marketing scholars were more interested in developing case studies than mathematical modelling around that time (McNair, 1954; Wood, 1963). Yet, the Foundation's trickle-down strategy eventually began to work, not least due to the impact of the faculty training seminars that it ran at the selected schools between 1957 and 1959. These seminars attracted at least 1500 faculty members from 300 schools, including leading marketing scholars like Philip Kotler, Robert Buzzell, Frank Bass, John Howard, William

Lazer, Jerome McCarthy, Edgar Pessemier, Donald Shawver, and Abraham Shuchman. One series of seminars run by the Institute of Basic Mathematics for Application to Business – which was sponsored by the Ford Foundation and launched in Harvard and MIT in 1959 – to upgrade the leaders of doctoral business programmes was "spectacularly successful" (Wilkie, 2002, p. 144). These seminars became a landmark effort in raising the mathematical competence of business educators generally (Schlossmann et al., 1987), and were of major importance in moulding marketing research into the form desired by the Foundation (Tadajewski, 2006). Looking back, Wilkie (2002, p. 144) is clear: "there's no question in my mind that even a causal [sic] tracing of the participants and their students will reveal a *huge impact* on the course of research in marketing" (Wilkie, 2002, p. 144, emphasis in original). Staelin echoed this when he wrote: "I cannot think of a single event that had more seminal impact on our field of inquiry than this year-long 1959 seminar" (Staelin, 2005).

We can identify 1959 as a watershed year because people like Burger (1959, p. 246) were still lamenting that marketing had yet to "take the first step" towards becoming a science, even though this had been an objective of the community for at least 50 years. It was also the year in which the Ford and Carnegie Foundations published their reports on the state of US business education, which they criticised as being too descriptive and 'unscientific' (Gordon & Howell, 1959; Pierson, 1959). Interestingly, the first of these reports, which comes to 500 pages, barely mentions marketing but tellingly observes that "Most of the introductory courses in marketing that we examined spent too much time on descriptive detail" (Gordon & Howell, 1959, p. 189).

The Ford Foundation's pump-priming (at all levels) can justifiably be identified as the main reason for the sea-change in marketing practice, teaching and research after 1959, given the continuing failure to make marketing more scientific up to that time. And the impact was quick and decisive. As Brown (1996, p. 92) observed: "by the beginning of the 1960s, the battle had been decisively won by the scientific wannabes", copper-fastened by the creation, in 1962, of the Marketing Science Institute whose goals were "(1) To contribute to the emergence of a more definitive science of marketing [and] (2) To stimulate increased application of scientific techniques to the understanding and solving of marketing problems" (Buzzell, 1963, p. 33). And while the scientific wannabes might have prudently avoided making causal connections, their endeavours were certainly not diminished by the fact that these new approaches to marketing research, teaching and practice were accompanied by

continuing growth in the US economy during the 1960s. As Day (1992, p. 324) put it, "the 1960s were the era of marketing's widest influence and greatest promise".

But if 1959 marked the point when marketing joined the intellectual bandwagon that had dominated post-war social science in the US, it also marked the start of a more robust critique of the quantitative paradigm outside of marketing. We can illustrate this by recalling the story of James March, who was one of the intellectual powerhouses in GSIA where he worked from 1953 to 1964, during which time he co-authored two seminal books: *Organizations* (with Herbert Simon (1958)) and *The Behavioral Theory of the Firm* (with Richard Cyert (1963)). GSIA was breaking up around 1964 and March decided to move to the University of California at Irvine where he continued to promote mathematical modelling, requiring all social science undergraduates to undergo a large amount of training in mathematics and statistics. But intellectual fashions were changing: ethnomethodology, conversation analysis, and more 'postmodern' approaches to social inquiry were being formulated at that time and, indeed, in that place (March hired Harold Garfinkel and Harvey Sachs, the respective founders of ethnomethodology and conversation analysis, as well as Jean Lave who made key contributions to the notion of situated learning). Looking back, March was of the view that the attempt to advance the mathematical modelling project in Irvine during the 1960s "was poorly timed from the point of view of the flows of enthusiasms within social science … [I]t would have been better timed a decade or two earlier" (personal communication). However, if the 'flows of enthusiasms' were moving elsewhere in the social sciences during the 1960s, this was hardly the case in marketing, where the passion for mathematics was only beginning to take hold. Moreover, the Ford Foundation's deep-root work meant that a paradigm – in the Kuhnian sense of a community holding a shared belief system about its practice – continued to renew itself over subsequent decades, assimilating contrary positions.

Making Marketing Matter

To end the story, we focus on the pivotal contribution made by Paul Anderson (1983) which Brown sees as marking a change from a pro-science era (1945–1983) to a pro-*sciences* era (1983–1999), and which Hunt (this issue) identifies as one of the most important contributions in marketing's philosophy debates. In this article, *Marketing, Scientific Progress and Scientific Method*, Anderson makes the important distinction between science$_1$ and science$_2$ – hence the shift that Brown identifies from science to science*s* – and proposes that the former (which we might refer to as positivism) "should refer to the idealized notion of

science as an inquiry system which produces 'objectively proven knowledge' (Chalmers 1976, p. 1). On this view, science seeks to discover 'the truth' via the objective methods of observation, test and experiment" (Anderson, 1983, p. 26). As an alternative to this non-existing system of inquiry, he then presents science$_2$:

> "The defining element here is that of social consensus. On this view science is whatever society chooses to call a science. In Western cultures, this would include all of the recognized natural and social sciences. *Thus physics, chemistry, biology, psychology, sociology, economics, political science, etc., all count as science$_2$.* This definition bears a resemblance to Madsen's conceptualization of science as a socially organized information-producing activity whose procedures and norms are 'socially established' (1974, p. 27)" (ibid, emphasis added).

In my view, Anderson's distinction between science$_1$ and science$_2$, despite being seen as so important in marketing's philosophical debates, is profoundly problematic and indeed unhelpful for at least two reasons. Firstly, he admits that nothing like science$_1$ (but not science$_2$) "has ever existed – nor is it very likely that such a system will ever exist" (Anderson, 1983, p. 26), which problematizes his definition, as well as biasing and hence weakening his argument. Second, and more importantly, he makes no distinction between the natural and social sciences, and it is this point that I shall now develop. To do so, I turn to the work of Bent Flyvbjerg.

Bent Flyvbjerg's (2001) book *Making Social Science Matter* has had a considerable impact since it first appeared in 2001. Now in its tenth printing, it has been reviewed by more than a hundred journals and magazines, including *Science* and the *Times Literary Supplement*. A central part of Flyvbjerg's argument is that social science has failed in its attempt to emulate the natural sciences and will continue to fail as long as it pursues theory-driven abstract knowledge of universal rationality. Drawing on Giddens' (1982) notion of the 'double hermeneutic', Flyvbjerg argues that there is a critical difference between the natural and social sciences in that "the former studies physical objects while the latter studies self-reflecting humans and must therefore take account of changes in the interpretation of the objects of study" (2001, p. 32). In other words, because social scientists study humans, they are necessarily offering interpretations of other people's interpretations. Furthermore, the people being studied can include the social scientist's interpretations in their interpretations, creating a dynamic, dialogic relationship between the people being studied and the people doing the studying. Crucially, this cannot happen in the natural sciences because "the objects of study are not self-interpreting entities: they do not talk back" (p. 33), which is why Anderson's failure to make this distinction is so important.

Social science fails when it seeks to create time-tested theories of a static social reality, while natural science fails when it tries to offer a reflexive analysis of goals, interests and values in a particular social and historical context. To develop this point, Flyvbjerg draws on Aristotle's distinction between three different types of knowledge: *epistemé*, *techné* and *phronesis*:

> *Epistemé* concerns universals and the production of knowledge which is invariable in time and space, and which is achieved with the aid of analytical rationality. *Epistemé* corresponds to the modern scientific ideal as expressed in natural science…
> *Techné* can be translated into English as 'art' in the sense of 'craft'; a craftsman is also an artisan… Whereas *epistemé* concerns theoretical *know why* and *techné* denotes technical *know how*, *phronesis* emphasizes practical knowledge and practical ethics. *Phronesis* is often translated as 'prudence' or 'practical common sense.'" (p. 55–56, original emphasis).

Table 1 distinguishes the elements of this tripartite scheme, drawing on Dunne (1993):

Insert Table 1 around here

Returning to marketing's philosophical debate, we can see that this tripartite scheme offers quite a different framing device compared to the art–science and $science_1$–$science_2$ dichotomies. Crucially, it makes the distinction between *techné* (art) and *phronesis* (ethics), which is missing from the way in which marketing's debate about matters philosophical is routinely structured as an issue about whether marketing is an art or a science. For instance Vaile (1949), in his commentary on Alderson and Cox's (1948) call to make marketing more scientific, asserts that: "When all is said and done, marketing will remain an art in which innovation and extravaganza will continue to play an important, albeit unpredictable part" (Vaile, 1949, p. 522), a point that Hutchison (1952, p. 289) echoes when he asserts that "marketing is not a science. It is rather an art or a practice, and as such more closely resembles engineering, medicine and architecture than it does physics, chemistry or biology."

Hutchinson's use of the word 'practice' is important here, as is his identification of marketing with engineering, medicine and architecture, each of which is a 'practice' in the understanding of the term popularised by the philosopher Alasdair MacIntyre (1981/1984) who, like Flyvbjerg, draws on Aristotle's writings. For MacIntyre, a practice is

> any coherent and complex form of socially established cooperative human activity through which goods internal to that form of activity are realised in the course of trying to achieve those standards of excellence which are appropriate to, and partially definitive of, that form of activity, with the result that human powers to achieve excellence, and human conceptions of the ends and goods involved, are systematically extended. (p. 187)

Now, while there is a debate about whether or not management (and by extension marketing) is a practice, in MacIntyre's understanding of the term (Kavanagh, 2012), one can at least argue that it is, and that, like engineering, it is constituted by a community of practitioners. Here, we can usefully link MacIntyre and Flyvbjerg because the former emphasises the role of virtues and ethics – which are always situated contextually and historically – within the traditions of particular practices, while the latter sees *phronesis* as the general wisdom that emerges through engaging in particular practices.

Finding *phronesis* in the detritus

One benefit of reviewing the evolution of marketing's philosophical debate is that it makes clear that matters relating to ethics were of more central concern to the marketing community before it passed the watershed year of 1959 on its way to becoming more 'scientific'. In marketing's early years, "the societal domain was an implicit issue in the body of marketing thought" (Wilkie & Moore, 2003, p. 118), probably reflecting the influence of the German historical school, which was very much concerned with addressing contemporary social problems.

There is plenty of other evidence that the concept of *phronesis* was important during the early decades of the twentieth century even if that word might not have been used. For instance, Veblen (1908, 1919) presented an important critique of marketing-related activities at that time, while Percival White adopted a clear ethical stance in his writing, seeing good ethics to be good business and highlighting the long-term downside of treating customers badly (Jones & Tadajewski, 2011).

The same themes are to be found in subsequent decades. For instance, Tadajewski (2010) highlights how Lynd (1936) and Rorty (1934) unpicked "the assumptions that undergirded the legitimacy of business and marketing practice" (p. 779) and he also notes how Paul Lazarsfeld used Marxism as a theoretical sensitizing device in his market research studies around 1930 and his later critique of "promotional culture" (Lazarsfeld, 1941). In addition, a number of contributions to the early decades of the *Journal of Marketing* show that the concepts of practice (from MacIntyre) and *phronesis* (from Flyvbjerg) were integral to the conversation about marketing between 1931, when the American Marketing Society was founded, and 1959. Most obviously, one of that society's two goals was to "formulate standards or principles in marketing" (Kerin, 1996, p. 1) (the other goal being to "advance science in marketing").

This focus on standards and ethical principles is made explicit in a paper by Paul Cherington (1937), first president of the American Marketing Association. Born in 1876, Cherington worked for the US Shipping Board and the National Association of Wool Manufacturers before becoming, in 1922, Director of Research at the advertising agency, J. Walter Thompson Company, where he worked until he became a partner in McKinsey and Company in 1939. During his varied career in industry he also found time to teach marketing in Harvard, Stanford and New York University (Crossley, 1956). While Cherington was committed to making marketing more 'scientific', ethical issues run through his short paper, *Marketing Marketing*, as this extract makes clear:

> In a branch of human activity which is trying to formulate itself into some semblance of a science, there are necessarily the serious problems of maintaining exacting professional and scientific standards, of guarding ourselves and our reputations against the wild doings and claims of the charlatans and the camp-followers, and of sifting out the good and constructive new developments from those which are merely the fruits of misguided zeal. But we have, at the same time, a more serious and urgent problem in getting a world, which has got on a long time without us, to believe that we really do have something to contribute to human welfare. (p. 233)

He exhorts his audience of marketing practitioners "always to focus our selling emphasis on the professional quality of our work" while he warns those with academic connections that "care should be taken to differentiate between individual and institutional standing and reputation. There are some nice questions of ethics involved here which should be frankly discussed" (p. 224). He also asks for respect for those that have "worked out a specialised field" of endeavour and that these should not be subjected to "imitation or price-slashing" by other marketing professionals (p. 224–5). For Cherington, good marketing practice is "not bragging about our virtues, but seeing that they are there, and getting our clients to brag about them for us" (p. 225). While one might take issue with Cherington's points, what is interesting is the degree to which his ideas are situated within the domain of *phronesis*.

Berna's (1937) article on fair trade practices in the machine tool industry takes a somewhat similar tack. As General Manager of the National Machine Tool Builders' Association, Berna represented machine tool companies, which he said were founded and operated by "practical men," who had the "pride of a skilled mechanic in fine workmanship" and are "traditionally scornful of sharp practice, high pressure selling and untested ideas" (p. 129). He also noted a unique feature of his industry, namely that the machine tool is "the only type of man-made equipment that can be used to reproduce itself", which means that "a company may be a competitor and a customer. This tends to make friends of competitors and has created an atmosphere of mutual respect and courtesy that is most constructive" (ibid.). Moreover, because of the relatively small number of metal-working shops, "we sell over and over to the

same customers. We must wear well". He then criticises recent government legislation, partly because it will increase costs that must be passed on to the customer, but more importantly because it displays, for him, a lack of appreciation for the industry's professional standards and ethics, in other words, for its *phronesis*. To illustrate these standards, he includes a list of principles set out by a Mr MacLeod, then President of the National Machine Tool Builders' Association. These ten principles articulated (1) a belief in "energetic but clean and honest" competition; (2) a pledge "to be tolerant in our attitude towards" other industries; (3) a belief "in the advantages of cooperative effort"; (4) support for "our Democratic form of government" and a pledge "to take an active interest in National and Local political affairs"; (5) a pledge to "manage our business [so] that the greatest value will accrue to investor, workman, and consumer"; (6) a pledge "to discontinue products which do not have a promise of showing reasonable profits"; (7) a pledge to "continually strive to improve [workers'] conditions"; (8) a pledge "to do our part to help in balancing production and consumption"; (9) a pledge "to common decency in business as expressed in our own Code of Ethics"; (10) a pledge to "strive for even greater standards" to maintain "the American standard of living and human welfare generally" (p. 131). Again, there is an ethical dimension to each of these principles, and even if they might be seen as self-serving rhetoric, the fact is that Bern saw fit to highlight and write about them, while, in turn, the editors of the *Journal of Marketing* thought his paper warranted publication.

Another paper from that period was by Clarence Francis, President of General Foods, based on his address to the American Marketing Association (Francis, 1938). In this paper, Francis argues that marketing's purpose is to help raise living standards in the United Stateas, whose President had recently admitted that "a third of the nation is ill-fed, ill-clothed, and ill-housed" (p. 27). Francis calls for "a more cooperative spirit between government, labor, and business"; a distinction between laws that seek to make competition fairer and laws that are designed to put someone out of business; more market facts "for all to use in a great program of national betterment" (p. 33); more easily understood language – "talk the language of the people when we go before the court of public opinion and seek to merit the responsibility allotted us" (p. 33); and, finally, to "give youngsters a break" by teaching them best business practice. He ends his talk by asserting that "Active service in the American Marketing Association . . . is one form of *patriotism*, practically applied!" (ibid., emphasis and ellipsis in original).

The American Marketing Association was itself centrally concerned with 'professionalising' the practice of marketing which meant more than making it more 'scientific', and this is why, in 1942, it created a Committee on Professional Standards and Status with the remit of formulating "a set of standards governing professional competence and professional ethics in the field of marketing" (Haring, 1942, p. 334). Part of the reason for this focus on professional ethics was because the Federal Trade Commission, which was founded in 1914, had spent much of its first 30 years investigating false and misleading advertising (according to its chair, Robert Elliot Freer, who was also an occasional contributor to the *Journal of Marketing* (Freer, 1938, 1949)).

In 1958, Lyndon Brown set out what he saw as the necessary steps that marketing needed to take to become a profession. While Brown was in the van of the pro-science programme – and this belief in science ran through much of his manifesto – the fourth of his five steps identified the need for "a continued rise in professional standards and ethics" (Brown, 1948L 29). He also saw ethics as something that was deeply embedded in the practice of marketing: "It is my personal belief that professional ethics and standards cannot be legislated, that they must grow out of *practice*, particularly since our work is inextricably interwoven with a private enterprise economy" (ibid; emphasis added).

In his well-cited article, *Is Marketing a Science*, Buzzell (1963) places himself firmly in the pro-science camp, though he acknowledges that "most managers who are responsible for day-to-day decisions are still typically inclined to distrust generalizations" (Buzzell, 1963, p. 34). He also quotes a speech given by Charles Ramond of the Advertising Research Foundation that strongly echoes the difference Flyvbjerg (and others) make between the natural and the social sciences:

> 'The businessman's practical wisdom is of a completely different character than scientific knowledge. While it does not ignore generalities, it recognizes the low probability that given combinations of phenomena can or will be repeated ... In place of scientific knowledge, then, the businessman collects lore'. (ibid, original ellipsis)

Each of these contributions illustrate – and are a historical record of – a (US) community's concern with *phronesis*, and its reflexive discussion on the right way to conduct the practices that constitute that community.

Conclusion: from *epistemé-techné* to *phronesis*

Another reason why 1959 can be seen as a watershed year is that in that year the *Journal of Marketing* published one of its last '*phronetic*' papers, by which I mean a critical-ethical paper on marketing practice, written by a marketing practitioner. In this case the author was

William Borton, a marketing researcher and management consultant. In his article, titled 'Respectability for Marketing', Borton questioned the usefulness of marketing's mission if it is just "to switch buyers to their particular brand … or to induce people to buy and consume more and still more goods" (Borton, 1959, p. 47). For Borton, a philosophy of simply increasing sales is hardly sufficient given that "problems of obesity and discussions of current automobiles are evidences that, past a certain point, quantity of goods consumed bears little relationship to human welfare" (p. 48).

As the hypothetico-deductive paradigm took hold, this type of essay – by a practitioner, for practitioners – all but disappeared (Twedt (1963) and Blankenship (1964) are rare exceptions) and now practitioners rarely, if ever, contribute to the leading marketing journals. This is quite in contrast to the 1930s to 1950s as is clear from Applebaum's (1947) review of the first ten years of the *Journal of Marketing*. Applebaum, who was himself an executive with the supermarket chain, Stop & Shop, computed that of the 499 articles published in the journal's first ten years, 39 per cent were authored by 'university teachers', 15 per cent by government employees, and 46 per cent by business practitioners. The evidence is that these practitioners used the journal as a forum to discuss and advocate ethical marketing practice, or what we might describe as *phronesis*. Unfortunately, the concerted attempt during the 1950s to make marketing teaching and practice less descriptive and more scientific meant that the practitioners decamped, and with no practitioners there could be no meaningful *phronesis*. Of course the journals did publish papers on marketing ethics (e.g. Bartels (1967)) and a new interest in macromarketing emerged (Bartels & Jenkins, 1977), but by that time almost all the practitioners had left the conversation. This is important in the story about how philosophical discourse in marketing evolved, because the narrative is not just about the flow of ideas, but also about who is – and who is not – participating in the debate.

Tellingly, as the practitioners left the debate, the amount of criticism of business declined, with Tadajewski (2010) observing that there was little, if any, criticism of business interests by marketing academics during the 1960s. This phenomenon appears to have continued to the present day, based on the fact that both MacIntyre and Flyvbjerg have been virtually ignored within marketing discourse – only a few papers in marketing journals reference their work – even though both are highly cited across the social sciences.[2] This is unfortunate as MacIntyre's ideas on practice (and the attendant ethical issues that he brings to bear) and Flyvbjerg's notion of *phronesis* should be worthy of inclusion in any debate about the nature of marketing theory and practice.

Looking forward, it can help to look backward. In his study of the influence of the German historical school on the development of academic life in the United states, Herbst observed that American students studying in Germany in the late 1890s gained "a craftsman's regard for technical expertise, an unfailing respect for accuracy, and a concern for the application of knowledge and skills to social ends" (Herbst, 1965, p. 19, quoted in Jones and Monieson 1990, p. 103). Today, Flyvbjerg's tripartite scheme has similar distinctions and ambitions: *techné*, or the "craftsman's regard for technical expertise", *epistemé*, or the "unfailing respect for accuracy", and *phronesis* or a "concern for the application of knowledge and skills to social ends". And the most important of these is *phronesis*.

Note

1. While women were largely absent from the science debates, they did contribute to the early development of marketing theory and practice (though their influence has tended to be neglected (Zuckerman & Carsky, 1990)). In particular, home economists like Christine Frederick, Hazel Kyrk and Elizabeth Hoyt contributed to consumer behaviour theory, while Pauline Arnold did pioneering work in marketing research. For further accounts of the female contribution to marketing thought and practice see the *Journal of Historical Research in Marketing*, 2013, Volume 5(3).

2. I thank D.G. Brian Jones for introducing me to the work of David D. Monieson (1981, 1988), whose writings on 'usable knowledge' and the 'intellectualisation' of marketing resonates with Flyvbjerg's distinctions between *techné*, *epistemé* and *phronesis*.

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