



The ordering of marketing theory: the influence of McCarthyism and the Cold War

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Abstract. *Following Peter and Olson's seminal contribution to the marketing of ideas literature, this article explores the marketing of theory. It is argued that marketing discourse should be viewed as an ordering attempt, not as an order. From this perspective it becomes important that we explore how marketing theory comes to be ordered in the way it presently is, and the manner in which certain ideas develop or face extinction, in order to better understand what can or cannot be said in marketing theory. In response, this article argues that a much needed turn in the paradigm debate should be toward the exploration of the institutionalization of marketing theory. It examines the social, economic and institutional logic underpinning theory production in marketing and demonstrates the influence of McCarthyism and the Cold War climate on marketing theory.* **Key Words** ● cold war ● marketing history ● marketing theory ● paradigm politics ● philanthropy

... the things which seem most self evident to us are formed in the confluence of encounters and chances, during the course of a precarious and fragile history. (Foucault in Raulet, 1983: 206)

Introduction

The marketing of ideas increasingly merits the attention of the academy (e.g. Bagozzi, 1976; Bloom, 1987; Davies and Fitchett, 2005; Fine, 1981; Germain, 1994; Hunt and Edison, 1995; Jones, 1994; Kotler, 1981; Murray and Ozanne, 1997; Peter and Olson, 1983; Twedt, 1977; Zaltman and Price, 1984). As Cunningham (2003: 201) remarks, the 'dissemination of ideas' is 'a fascinating area of study' and 'a marketing process' (Fine, 1981: 1) with ideas achieving the status of



products when they become detached 'from the knower' (Bagozzi, 1976: 587). In an important contribution to this stream of literature, Peter and Olson (1983) argue that scientists have to market their theories to their academic communities since, if a theory is to be successful, enough allies need to be recruited who can, through word-of-mouth and co-citation, provide plausibility to new research findings and further the diffusion and potential acceptance of the new theory (Bloom, 1987; Germain, 1994; Murray and Ozanne, 1997; Peter and Olson, 1983). Unsurprisingly, as a form of discursive intervention, the production of theory will bear the traces of 'political processes' involving mutual adjustment and accommodation regarding academic norms, rules and procedures between gatekeepers so that the subsequent acceptance of theory 'occurs through implicit coercion and conflict' (Bagozzi, 1976: 589).

The history of marketing theory is testament to the contestation that occurs between rival paradigmatic communities competing to position their paradigm as the dominant discourse of time (Kuhn, 1970a). Thus, from the 1950s onwards, a number of overlapping epistemological debates have been seen to exemplify this form of epistemic struggle, from the early science debates of the 1940s and 1950s, to the motivation research versus logical empiricist *contretemps* throughout the 1950s to the more recent paradigm debate in the late 1970s and early 1980s (Chakravarti and Staelin, 2001; Kassarian, 1994; Levy, 1996, 2001, 2003a, 2003b, 2005). Upon examination of the literature, however, it is immediately apparent that marketing scholars have devoted considerably less attention to the relationship between regimes of power/knowledge production than, for example, our contemporaries in accounting or organization studies (e.g. Hoskin and Macve, 1986). This seems strange given the continued citation within the paradigm literature of Burrell and Morgan's (1979/1992) important contribution to this debate, especially when considering that the 'imperialistic' tendencies of the 'dominant' paradigm (Jackson and Carter, 1991, 1993) were the motivating force behind Burrell and Morgan's postulation of paradigm incommensurability (Burrell, 1997, 1999); a feature of this debate elided in calls for multiple paradigm analysis (e.g. Combe, 1999; Combe and Botschen, 2004; Davies and Fitchett, 2001, 2005; Tadajewski, 2004).

Lowe et al. (2004) and Lowe et al.¹ (2005) offer us a recent example of this discursive movement and typify the disregard for the relationship between power/knowledge and the incommensurability thesis. Stressing the value of mapping paradigm debate as a 'tool' to overcome the hermetic incommensurability thesis, Lowe et al. (2004) depict the epistemic organization of marketing theory as a form of living system whose essential characteristics require clarification so that greater conversation between incommensurable paradigms can take place. This, of course, is a worthwhile project in itself. The implicit assumption in their thesis, however, is that paradigm debate occurs within a free speech situation, whereby paradigm differences are overcome because all discussants are equally free to speak their own minds. Each has equal, and ample, opportunity to do so with no one member in a position to impose their own will on the others or impose pressures of any nature, with the result that good argumentation prevails



over coercion. Regrettably this assumption is misplaced: there are clear social, linguistic and non-discursive² institutional pressures that delimit what can and cannot be said within marketing theory (Brown, 1995, 1998; Cahill, 1993; Desmond, 1998; Dholakia et al., 1980; Firat, 1984, 1988a, 1988b; Holbrook, 1995; Levy, 2003a, 2003b, 2005; McDonagh, 1995). It is not simply that there are paradigm relative rules of discursive formation restricting any movement beyond the incommensurability thesis, but that there are non-discursive practices: the ‘thoroughly heterogeneous ensemble . . . of regulatory decisions, laws, administrative measures, scientific statements, philosophical, moral and philanthropic propositions’ which support particular types of knowledge and social orders (Foucault, 1980: 194). Within this ensemble, there are only certain statements that can count as serious contenders for the mantle of truth, whereas others are excluded by virtue of the rules of discursive formation that govern debate. As Paul Anderson registered:

editors as well as the editorial review boards have a particular idea of what is a good theory . . . and how to go about presenting them. I think we’ve got to respond to that otherwise we’re simply not going to be able to communicate with our colleagues . . . in the meantime, unfortunately, we are going to have to respond to the criteria as we find them. (Peter et al., 1980: 15)

We should not, however, rush to condemn Lowe et al. (2004) and Lowe et al. (2005) too quickly as their assumption of a free speech situation is one made throughout the marketing literature; more problematic is the lack of interest in connecting the production of marketing knowledge to wider societal relations of power/knowledge which influence how knowledge regimes gain legitimacy and institutional support within a particular socio-temporal context. Commensurate with this emphasis on the non-discursive, it seems advisable that marketing scholars should periodically revisit the development of marketing history in order to establish how forms of knowledge become institutionalized, rethinking – where necessary – the ordering of marketing discourse (Brownlie and Saren, 1997; Desmond, 1998; Jones and Monieson, 1990). This is what I attempt here, using the paradigm debate as a case study. I retrace the epistemological development of marketing theory post 1950, and show how the development of marketing theory was relative to the social conditions of the time and entwined with the ‘positivist’ worldview of the philanthropic foundations that had taken an interest in marketing theory and education at the height of the McCarthy era and the Cold War. In so doing I demonstrate the extent to which the promotion of ‘positivism’³ was politically motivated and exerted a formative influence on marketing theory.

The structure of scientific revolutions

The Structure of Scientific Revolutions (Kuhn, 1970a) is widely hailed as one of the most significant academic publications of the 20th century. Central to Kuhn’s depiction of the history of science is the paradigm concept which is seen to direct and shape scientific research. Registering the level of disagreement between social



scientists regarding which research questions and methods are legitimate, Kuhn notes how such controversy is largely absent in the natural sciences. Attempting to discover the source of the differences between these two communities and the problems this presents for the evaluation of research, Kuhn says, led him ‘to recognize the role in scientific research of what I have called “paradigms”’ (Kuhn, 1970a: x). For Kuhn, paradigms signify ‘universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners’ (Kuhn, 1970a: x) – an ‘accepted model or pattern’ (Kuhn, 1970a: 23) which encompasses a theoretical structure comprised of a network of conceptual, theoretical, and instrumental commitments’ that orient future research (Kuhn, 1970a: 42). This description may make the paradigm concept and Kuhn’s broader thesis sound simple enough but as numerous commentators have noted, Kuhn’s work is highly ambiguous, contributing to the variety of interpretation accorded it since ‘part of the reason for its success is, I regrettably conclude, that it can be too nearly all things to all people’ (Kuhn, 1974: 459). Yet, Kuhn’s work has been read in a surprisingly consistent manner, having been seen to highlight the way in which the social sciences can earn the accreditation of ‘science’ (Sharrock and Read, 2002). This, nonetheless, is a misinterpretation of Kuhn:

I claim no therapy to assist the transformation of a . . . [social] science to a science . . . If . . . social scientists take from me the view that they can improve the status of their own field by first legislating agreement on fundamentals and then turning to puzzle solving, they are badly misconstruing my point. (Kuhn, 1970b: 244–5)

Indeed, the use of the paradigm concept itself raises manifold problems in that paradigms of a discipline can, for Kuhn, only be specified on the basis of the consensus they generate, so that paradigms are identified in terms of their super-theoretical content (Gutting, 1980). The result of this parsing method means that there will be an unlimited variety of ways of dividing a discipline into paradigms since any interrelated set of beliefs, values, methods and instruments held by a given research specialty numbering of ‘perhaps one hundred members, occasionally significantly fewer’ can justifiably be referred to as ‘a paradigm’ (Kuhn, 1970a: 178). On this reading, a scientific revolution need only be viewed as revolutionary by ‘perhaps . . . fewer than twenty-five people’ (Kuhn, 1970a: 181).

Despite the limitations of Kuhn’s analysis his writings remained, for those beginning research careers in the 1970s, ‘very liberating for it established their revolutionary credentials and legitimated them’ as the progressive community ‘to whom the future belonged’ (Burrell, 1997: 33). In adopting Kuhn’s concept of the paradigm but ‘not using Kuhnianism in any faithful way’ (Burrell, 1997: 33) Burrell and Morgan argue that they have broadened Kuhn’s definition⁴ to ‘emphasise the commonality of perspective which binds the work of a group of theorists together in such a way that they can be usefully regarded as approaching social theory within the bounds of the same problematic’ (Burrell and Morgan, 1979/1992: 23). In a further movement away from Kuhn, Burrell and Morgan do not view the paradigm concept as referring to opposing substantive explanations



as Kuhn proposed, but use it to refer to ideal types of opposing meta-theoretical assumptions with each of their four paradigms (functionalism, radical structuralism, radical humanism, interpretivism) developed through two conceptual dimensions related to the nature of society and the nature of science. This allows them to contrast the more conservative, status quo oriented paradigms of functionalism and interpretivism with those of the radical change paradigms.

As the 'crisis of representation' reminds us, this form of cartographic endeavour can never be neutral but is firmly implicated in the politics of the academy. This raises questions of voice and silence in Burrell and Morgan's mapmaking exercise and in terms of providing space for alternative discourses they cannot be criticized (Kelemen and Hassard, 2003). More important here are the issues that they draw attention to when discussing cross-paradigm communication. This is the phenomenon of paradigm incommensurability, which in Kuhn's early writing refers to the idea that each paradigm has its own structures of meaning, methodological standards and relation of theory and the world. Different theories are seen to be incommensurable because there are no direct and conclusive means through which they can be compared against some common standard (Kuhn, 1970a). Burrell and Morgan adapt this thesis by proposing that each paradigm is 'mutually exclusive' since each offers 'different ways of seeing' (Burrell and Morgan, 1979/1992: 25).

Adopted as a means to isolate each paradigm from the others, paradigm incommensurability was promoted by Burrell and Morgan to legitimize the production of knowledge from a wider range of paradigmatic positions preserving this new plurality against the imperialistic aspirations of a disciplinary orthodoxy and thereby ensuring that paradigm differences are maintained, rather than elided (Burrell, 1999). Despite some oscillation between mutual exclusivity and their view that 'some inter-paradigm debate is also possible' (Burrell and Morgan, 1979/1992: 36), inter-paradigm debate appears foreclosed, with the consequence that they reinforce epistemological relativism. This view was, and remains, contested, leading to a burgeoning literature that attempts to subvert the incommensurability thesis by producing studies that draw upon multiple paradigms. However, underpinning this support for multiple paradigm analysis is a vision of paradigm debate whereby 'differences are welcomed and celebrated' (Burrell, 1997: 46). This picture of tolerance is often grounded in the rhetoric of social pluralism (Grafton-Small, 1993).

While social pluralism might be an appealing picture of academic debate it bears little resemblance to the actual conditions within which knowledge is produced and institutionalized (Burrell, 1997, 1999, 2001). The issue is not simply the methodological, semantic or value incommensurability (see Sankey, 1994, 1997) that prevents theory comparison on a point-by-point basis, but the prosaic reason that paradigm debate is shaped by procedures of exclusion: 'We know quite well that we do not have the right to say everything, that we cannot speak of just anything in any circumstances whatever' (Foucault, 1981: 52). Burrell and Morgan were well aware of the potentially disciplinary nature of certain factions toward alternative, more marginal paradigms and yet in marketing theory this awareness



appears to have been subsumed within a general discourse that emphasizes ‘the value of diversity and breathless endorsements of pluralistic research whereby the many, equally valid paradigmatic views can converge upon a “richer”, “deeper”, “fuller” . . . understanding of consumption phenomena’ (Thompson et al., 1997: 151). Each paradigm is positioned as contributing important insights to our understanding, even though the existence of a multi-paradigm plurality does not necessarily indicate a fundamental change in the conditions in which knowledge production takes place; to assume that it does presupposes the existence of an free speech situation and the absence of any apparatus foreclosing debate (Calas and Smircich, 1999; Foucault, 1981; cf. Kilduff and Kelemen, 2003). This assumption is problematic given that paradigmatic pluralism is not ‘some neutral ground which allows any and all speech, but a particular version of ontology and politics’⁵ (Parker and McHugh, 1991: 455).

Perhaps, as some have argued, marketing scholars avoid such politically framed questions due to the scientism underpinning much of marketing theory and the concomitant attention towards the context of justification, rather than the logic of discovery (Willmott, 1999). The implication of this orientation is that important questions related to the relationship between power and knowledge are marginalized (Alvesson and Willmott, 1996; McDonagh, 1995; Morgan, 1992, 2003). While it is apparent that such questions are indeed largely elided, this relationship between knowledge and power has merited the interest of a number of prominent marketing scholars, who have called for attention to be directed toward those institutional pressures that function to constrain what forms of inquiry can be undertaken (Bagozzi, 1976, 1992; Levy, 2003a, 2003b, 2005; McDonagh, 1995; Murray and Ozanne, 1997). Levy is particularly clear on this issue: ‘When entrenched forces manage to stave off change, one has to ask where that power comes from, and why change is failing’ (2003b: 111). This is not to suggest, however, that attempts have not been made to challenge what some might see as ‘dominant’⁶ discourses, as will be demonstrated.

Disenchantment in the academy

In the late 1970s disenchantment with the extant philosophy of science underpinning marketing theory motivated a number of marketing and consumer researchers to question the limitations of logical empiricism. The general argument is well rehearsed by Sauer et al. who maintain that disciplinary-wide subscription to this paradigm ‘tends to limit the areas of inquiry as well as the methods of inquiry in marketing and hence it unintentionally restricts our knowledge about marketing phenomena’ (1982: 15). As a response, they argue that marketing theorists should strive to become philosophically and methodologically multilingual so that the limitations of any single view can be supplemented with the insights available from other perspectives.

In a similar vein, Arndt makes the persuasive case that without alternative forms of inquiry marketing will remain ‘a one-dimensional science concerned



with technology and problem-solving' (Arndt, 1985a: 21). Following Burrell and Morgan (1979/1992) and drawing heavily on Morgan (1980), Arndt proposes the paradigmatic expansion of marketing theory via the use of a variety of paradigmatic positions (e.g. subjective world, sociopolitical, liberating) in marketing research – 'While no paradigm or metaphor is more than a partial and incomplete truth, the *notion* of paradigms should be viewed as an argument for paradigmatic tolerance and pluralism' (Arndt, 1985a: 21; emphasis in original). Not only does Arndt (1985a, 1985b) see pluralism as an end within itself and as an opportunity to be uncritically welcomed rather than subject to scrutiny, but the epistemological expansion he wishes to promote remains largely within the 'objectivistic-consensual' view associated with logical empiricism (Alvesson, 1994). This strategy, however marketable it makes Arndt's support for paradigm plurality, nonetheless lacks any sustained attempt to question the power-politics underlying paradigm debate. Still it is not especially surprising that Arndt's criticism of 'positivism' is almost absent, for, as a number of commentators have maintained, the requirement that knowledge is subject to social certification has potential publishing and career implications for those wishing to promote oppositional discursive regimes to the 'intellectual hegemony of positivism' (Shankar and Patterson, 2001: 485).

Alluding to the idea of scientific debate as a language-game, Anderson (1983) suggests that all marketing scholars are obliged to play by the rules conventionally sanctioned by various, important interest groups (Peter et al., 1980). Yet the very fact that there are multiple paradigms available for marketing scholars to adopt does not necessarily mean that each viewpoint will be seen as equally correct – the very definition of pluralism – as there are explicit normative rules of discursive formation that must be adhered to if the proposed contribution to knowledge is to avoid consignment to the margins of marketing theory. As Greenley points out, marketing theory is bound by 'certain rules and conventions [that] must be followed if [the] acquired information is to be accepted as *bone fide* knowledge by those working within and outside the domain' (Greenley, 1995: 665; emphasis in original).

Stressing his subscription to opening epistemological debate in marketing, Greenley (1995: 668) proffers what he says consensus indicates is the 'right set of rules and conventions' to be adhered to in the production of marketing knowledge. These rules are broadly logical empiricist in tenor and demand attention to 'the rigour, discipline and systematization of scientific research methodology' (Greenley, 1995: 668). Wanting to remain open to alternative paradigms of knowledge, Greenley stresses his subscription to critical pluralism. Yet his call for this critical pluralism extends only so far, remaining firmly within a position he attributes to scientific realism with contributions to knowledge to be welcomed only if they have 'accommodated rules and conventions of scientific realism and critical pluralism' (Greenley, 1995: 668). He continues, the 'controversial and new must be within rules and conventions; if they do not build-on existing knowledge in a justifiable manner then they are outside the doctrine of criticality' (Greenley, 1995: 668). Far from any research question supporting a variety of plausible but



mutually conflicting responses, this 'pluralism' is underdetermined only by research that subscribes to its own axiological tenets.

Given that Greenley's position is broadly consistent with the majority of published marketing theory (McDonagh, 1995), the challenge for marketing remains that, having acknowledged such disciplining forces exist, we should now turn our attention towards the examination and evaluation of those rules of discursive formation which underlie the production of marketing theory: in other words, how particular epistemological positions become legitimated within a specific socio-temporal context and subsequently gain institutional support. This kind of focus would go toward redressing what Willmott sees as a fundamental weakness of marketing theorists, namely our 'studied refusal to acknowledge and explore the relationship between knowledge and power' (1999: 211). What this means is that we must, as a matter of course, investigate the ways in which particular social systems give rise to specific views of the world and our place within it. To better understand how the sentiment espoused so unselfconsciously by Greenley came to be so widespread in marketing, we need to turn to the historical record to identify the myriad of events that contributed to the formation of the paradigm debates as presently conceived. In an attempt to illustrate the kind of analysis that I am proposing, the focus of attention is, in this case, the conjunction of marketing theory and the funding proclivities of those philanthropic foundations that took an interest in marketing in the 1950s which – by way of the agenda underpinning their activities – induced eurhythmic changes in marketing theory.

Institutionalizing logical empiricism

The period between 1945 and 1975 is important for management education and the business school: where once the provision of business education had been limited to a few influential schools, by 1975 nearly every substantial college campus in the United States had entered the burgeoning marketplace for business education (Gleeson and Schlossmann, 1995). Despite these changes, business education was still not held in high regard among the university and business communities alike. This recognition encouraged a variety of actors at the institutional level to reflect upon how this state of affairs could be remedied. Important here are marketing scholars themselves and following the Second World War there were genuine aspirations among marketing scholars to strengthen the discipline through the creation of fundamental theories, the improvement of research techniques and a desire to improve the general level of scholarship via the training of competent researchers (Alderson and Cox, 1948).

This sentiment continued to be echoed throughout the 1950s and was encouraged still further by the publication of the Ford and Carnegie foundation reports into the state of business education. These reports presented the business school as having a serious liability crisis, with the students seen to be lacking the intellectual and scholarly abilities of their peers in other disciplines, and in equal



measure the research conducted by the academic staff was criticized for being largely descriptive and 'unscientific' (Gordon and Howell, 1959; Pierson, 1959). These are features that are usually documented in discussions about the changing nature of marketing theory (e.g. Fullerton, 1987). Less frequently documented is the impact of the social, economic and political environment that served to influence the path marketing theory was to take (Scully, 1996). Yet we cannot, however, expect the genealogical lineage of these debates to imply a simple line of descent, but rather to highlight the confluence of diverse and multiple factors. Nonetheless, it is relatively easy to start from the point of view that appears most promising and it seems clear that the everyday realities of the Cold War (1947–90) impacted heavily upon both lay and academic publics alike (Schwartz, 1997). Indeed what Hirschman (1993) sees as the masculine character of marketing research, with its emphasis on rationality, objectivity and a predilection for quantitative measures, was strengthened by the Second World War and subsequently reinforced by the Cold War.

The Cold War

In this political context, business education offered an important contribution to the standoff between the East and the West as one channel for a diplomacy of ideas between the two sides (Gemelli, 1998): 'the welfare-warfare state that emerged in the Depression and cold war eras created the conditions in which the various social sciences . . . became valuable, if not indispensable, adjuncts of corporate and state power' (Ball, 1998: 76). Not that these were the only conditions contributing to the changing relationship between these disciplines and the wider social environment, with the Cold War creating the conditions for the so-called 'Red Scare' and the anti-Communist fanaticism that flourished in the United States from 1947 until the 1970s (Babich, 2003a, 2003b; Schwartz, 1997).

This was a period where charges of Communist sympathy were used to discredit 'liberals, intellectuals, antiwar advocates, feminists, civil rights advocates, and other citizens who were left of center on the political spectrum' (Schwartz, 1997: 47). As a grotesque spectacle, the 'public humiliation trials' (Garfinkel, 1956) were led by Senator Joseph McCarthy who, as his supporters included administrators at most university campuses, blighted countless academic careers in his search for Communist sympathizers within the social and natural sciences (Lazarsfeld and Thielens, 1958; Schrecker, 1986; Stewart, 1950). As Babich points out: while the Cold War climate in the US was not exactly a carbon copy of Stalinism, 'it nevertheless had exactly repressive parallels. Much of the social and intellectual repression characterizing the 1950s and 1960s expressed this climate of fear' (2003b: 103).

Marketing was not immune to the influence of McCarthyism and the Cold War. Certainly, at this time, being branded a radical or subversive influence was not desirable and the pressures militating against radicalism were influential in dissuading a substantial proportion of researchers away from advertising their



political, sexual, social or epistemological leanings (Lazarsfeld and Thielens, 1958; McCumber, 2001; Stewart, 1950). This was a time of intellectual change in marketing and consumer research and even into the 1960s ‘just after McCarthyism, the Cold War, and Red Baiting, the terms “radical” and “revolutionary” might have been inflammatory if not outright libelous’ (Kassarjian, 1989: 124). Relating this to his own experiences as a doctoral student, Kassarjian recalls how his ‘own doctoral committee chair was ostracized and destroyed’ for not immediately signing the oath of non-membership of the Communist party (Kassarjian, personal communication). These were, he says, ‘bad times to be a liberal, never mind a radical’ (Kassarjian, personal communication) and in a survey of over 2000 social scientists a noticeable segment of respondents ‘felt intimidated by the difficult years – deterred by a fear of attack and of harm to their careers that might result from a free expression of their views’ (Lazarsfeld and Thielens, 1958: 196).

Comments made within the pages of the *Public Opinion Quarterly* are particularly telling, given the overlap between the members of The American Association for Public Opinion Research and the American Marketing Association, with the two research specialities having ‘grown closer together’ (Bogart, 1957: 135; see also Myers et al., 1980: 140–1, 229; Twedt, 1977: 70). For instance, in his Presidential speech delivered to the American Association for Public Opinion Research, Stouffer makes a case for marketing communications researchers to contribute to combating the ‘forces which currently threaten freedom and democracy’ (Stouffer, 1954: 233). Drawing a parallel between the Great Plague that enveloped the world in 1665, he notes how, like the populace of that time, those living in 1954 also lived with a similar kind of threat, albeit from ‘Communist subversion and espionage’ which ‘can erupt into devastating sabotage in case of war’ (Stouffer, 1954: 234). In response, he argues that all social scientists must work to improve their research, especially their measurement instruments, so that the plague of Communism can be challenged ‘with our instruments of truth’ and thereby ‘hold off the forces of darkness which imperil our ancient liberties’ (Stouffer, 1954: 238).

Central to the general atmosphere of the time was the demand for academic research to be geared towards the promotion of American interests, aimed at keeping America economically strong in the face of the ideological appeal of Communism, whose main attraction was seen to be its promise to redistribute wealth more equitably through society. The Ford Foundation, beginning its initial investigations into how best to use its available funds for philanthropic purposes, compiled a report documenting how:

Communism proposes to grant a better standard of living for the mass of the people and asserts the exploitation of the working man by the capitalists; it claims that the capitalistic and imperialistic United States plans to exploit the weaker nations. (Gleeson and Schlossmann, 1995: 13)

In this environment, desirable forms of academic labours were those that could contribute to the undermining of Communist propaganda by promoting American democratic goals in the international arena by bolstering the image of the United States and capitalism against the propaganda produced by those allied



against America and Judeo-Christian values – those ‘ruthless men in the Politburo [who] can press a fateful [atomic] button’ (Stouffer, 1954: 232–3).

It was not simply that researchers had to say that they weren’t affiliated with any un-American activity, but that they were compelled to take part in a more effective distancing strategy: performing their political orientation through teaching and publications that steered clear of ‘the need to probe and question existing political, economic, and social arrangements’ (Robin, 2003: 7). The result of such pressures on academic labour can be found throughout the social sciences and humanities during this period, including: philosophy (McCumber, 2001); mathematics (Nasar, 1999); history (Lazarsfeld and Thielens, 1958); economics (Schrecker, 1986); politics (Stewart, 1950); and communications research (Simpson, 1996). Each of these disciplines tended toward intellectual conservatism and, as a result, were rapidly depoliticized, experiencing a concomitant reduction in theoretical variegation in favour of paradigms and methodological approaches that celebrated the status-quo and eschewed intellectual radicalism (Lazarsfeld and Thielens, 1958). In these circumstances the safest and most common move was to celebrate ‘the status quo and the end of ideology that dominated intellectual discourse’ (Schrecker, 1986: 336). The most effective demonstration that ‘one was not a Communist, or a fellow traveler, or a sympathizer (and so on) was to engage in a quasi-scientific pursuit of truth . . . or if that was too much to stomach, to search for confirmation’ (McCumber, 2001: 146).

These reverberations of McCarthyism, the Cold War and the space race, stimulated by the launching of Sputnik I in October of 1957, all helped translate the status of the social sciences into immediate congressional concern, with the East/West standoff one of the most important structural events that impacted upon the business school (Gemelli, 1998; Kleinman and Solovey, 1995). In a speech delivered at the time, Vice-President Nixon emphasized the need for America to maintain its superiority vis-à-vis the Russians in the social and behavioural sciences: ‘We must not be behind in that field, but we also have to train leaders in government, leaders in business, leaders in the social sciences’ (Alpert, 1958: 683).

The obvious support for the development of the social sciences in the 1950s would have been the newly formed National Science Foundation (NSF), a body established as a post-war prolongation of the linkage between government funding and university science, although this body only supported a very limited number of projects in marketing and consumer research (Bloom, 1987; Staelin, 2005). This was due to a range of factors: the vagueness of the subject matter; the controversy associated with the applied disciplines (i.e. lack of scientific method, the involvement of applied scientists with potentially politically deleterious research involving questions of race and so forth); the ‘softness’ of work undertaken in their name (non-mathematical, non-experimental); debasement of human dignity (i.e. the hidden persuaders and the association of marketing and consumer research with consumer manipulation); partisanship (dogmatism rather than objectivity); the production of applied rather than basic research; the magnitude of needs that would have to be funded; and, finally, the presumed



access to private resources that business disciplines were believed to possess (Alpert, 1958; Robin, 2003). The interest and funding available from the principal philanthropic foundations – the Carnegie Corporation, the Rockefeller Foundation and the Ford Foundation – compensated for the absence of support from federal sources. It is the last of these foundations that is the most pertinent to the present discussion, in as much as the Ford Foundation was the most overt contributor to the development of a scientific basis for marketing, having been seen to ‘prime the more “worthwhile” academic work of business schools’ (Willis, 1971: 357).

Following the death of Henry Ford in April 1947 and Edsel B. Ford in 1950, the potential philanthropic contribution that this foundation could offer was greatly enhanced, with the leadership of the Ford Foundation having \$69 million at its disposal for philanthropic programmes (Berghahn, 1999). The rapid increase in available funding was to affect the development of marketing theory formatively, courtesy of the interest of this foundation in business education. This focus was primarily remedial in that business education was viewed as a ‘wasteland of vocationalism . . . [that] needed to be transformed into science-based professionalism’ (Simon, 1991: 138–9). With these, and similar views, widespread in a tense political climate, the push towards making marketing more scientific was seen as a proactive way to stem the threat from Communism through the impact that an improved ‘scientific’ knowledge base with an emphasis on prediction and control would have on managerial and organizational efficiency; efficiency that would ultimately translate into American national security via the improvement in the balance of payments and other economic measures (Gemelli, 1998; Gleeson and Schlossmann, 1995; Zimmerman, 2001).

There were, of course, other interests driving this funding strategy in as much as these foundations sought to improve the status of the business profession relative to other professional groups. What is clear, however, in this instance, is that the promotion of pro-American values, methods and research institutions was a response to the increasing criticism and allegations of subversion levelled at the foundations from a number of House of Representatives Un-American Committees (HUAC).

Communist subversion

Established in 1938 and continuing to investigate subversive activities until 1975, these committees had a dim view of the social sciences. Speaking in 1946, Senator Fulbright of Arkansas highlighted the political tension surrounding the social sciences most explicitly when he made the case that while he saw the founding of the National Science Foundation as a means to secure America’s world position for the foreseeable future, the fact that this body would concern itself with funding the social sciences was potentially problematic due to the ‘misconception with regard to a study of the social sciences being confused with what we commonly think of as politics, socialism, or some form of social philosophy’ (Miller, 1955:



513). It was not the intent of those seeking to found the National Science Foundation, Miller argued, to promote any such social philosophy and any research attempting to study human relationships on a scientific basis should not be confused with the support for socialism or any related subjects.

Despite these attempts at clarification these HUAC's remained under the misapprehension that the funding of the social sciences was in some way connected with support for socialism. Given this misunderstanding it was perhaps not wholly unexpected that the funding directed by the philanthropic foundations towards the social sciences would garner unwanted attention, with philanthropic funding denounced as supporting 'projects that tended to weaken or discredit the capitalist system in the United States and favor Marxist socialism' (Robin, 2003: 35). These are not views that would dissipate in the intervening years: just six years after Fulbright had attempted to clarify the nature of social science research, a special committee in the House of Representatives returned to examine the conduct of the foundations, the funding of social science and the alleged subversive influence of these foundations policies, subjecting them to great scrutiny (Magat, 1979).

The Cox Hearing in 1952 and the subsequent report issued in 1954 were sympathetic to philanthropic funding motives in that they did not immediately dismiss the funding strategies of the foundations as socialist, as might have been expected in this politically charged environment. The hearing and report did, however, encourage the major foundations to carefully re-examine what forms of research they would fund in the future. This pressure was further exacerbated in 1953 when 'the big three', as Ford, Carnegie and Rockefeller were known, were the focus of a congressional committee led by Congressman B. Carroll Reece. Reece was critical of the length of time given to the Cox committee to investigate the activities of the philanthropic foundations and added one further charge to those already facing these organizations, alleging that 'foundation funds were being used for "political purposes, propaganda, or attempts to influence legislation"' (Magat, 1979: 31). In allocating the blame for the socialist thought apparently saturating the social sciences at the feet of the Ford and Carnegie Foundations, he had placed these foundations in dangerous territory.

Accused of seeking to promote an ideological view critical of the United States and its free enterprise system, the foundations were quick to mount a defence arguing that, in reality, their funding functioned as an effective defence against the threat posed by Communism and the propaganda disseminated by Russia and its satellite states. If there were any sociopolitical consequences of their philanthropic activities, they argued, it was their function as 'cooling-out' agencies that served to regulate the extent to which radical structural change could take place in society in that their previous funding strategies had attempted to research social inequalities and postulate possible techniques for ameliorating such potentially destabilizing tensions (Arno, 1980; Robin, 2003). In spite of these attempts at appeasement, the impact of the focus of committees led to rapid policy changes inside the foundations themselves. Here the case of the Ford Foundation illustrates the influence that this congressional focus had on their funding strategy:



‘The Joseph R. McCarthy era and the congressional investigations took their toll on the Ford Foundation and no doubt helped to account for our caution, especially in the late 1950s’ (Magat, 1979: 31). With the foundations beset by controversy, and the implication that they were perpetuating an ‘un-American’ ideological agenda, ‘the trustees believed it desirable to make a highly visible public impression with safe and popular grants’ (Magat, 1979: 33). In 1953, for example, Thomas Carroll, the Dean of the School of Business at the University of North Carolina, was invited to spearhead the Ford Foundation’s programme for upgrading business education. The committee concluded that if business education was to improve then:

[T]he foundation had to subsidize and encourage substantial, imaginative research in a field where few . . . conducted any research at all, and where much of that undertaken was descriptive, industry – specific (if not company-specific) and generally pedantic. Ford would have to invest heavily in the few researchers and the few institutions capable of generating quantum leaps in knowledge of the business enterprise. (Schlossmann et al., 1987: 14)

Management education and research was an important outlet for the Ford Foundation given that it was not a politically sensitive issue (Schlossmann et al., 1987). Central to this attempt to appear ideologically consistent with both the governmental regime of the time, and the political climate in which they were operating, the business school and business research and education appeared not only to be a safe option for philanthropic funding, but one which would also demonstrate support for America more broadly – ‘enhancing economic well-being in the United States lay in our support of leading business schools’ (Magat, 1979: 60).

Concurrently with the undesirable political focus on the Ford and Carnegie foundations, it was into this politically turbulent environment that both organizations released their critical reports into the business school (Gordon and Howell, 1959; Pierson, 1959). The entrance of these two foundations occurred at an opportune moment: America was on the verge of a major expansion in college enrolments as a result of the large increase in the cohort of people aged 18 to 24 who were willing and able to invest in a college education. Those enrolling at this time found the business school undergoing a radical transformation almost immediately after the publication of these highly critical reports. Broadly commensurate with these shifts was the transformation of marketing theory and education.

Whereas the business disciplines had once been dismissed as overly descriptive and analytically weak, there was a ‘*fundamental change*’ in marketing research and knowledge development, with the ‘academic worth’ of knowledge seen to rest on its scientific basis, that is, the extent to which it was based upon ‘basic’ research conducted in disciplines such as economics, psychology, engineering and statistics (Myers et al., 1980: 261; emphasis in original). It is here that the shift in funding and, more importantly, the shift in the semantic description of the work being funded in the business school, is of most importance.



Behavioural sciences

Rather than proposing to fund the social sciences given the association with socialism, in an effort to purge any association between the Ford Foundation and the promotion of socialist or Communist thought, it was thought to be politically advantageous to use a different label – ‘behavioural sciences’ – including: political science, biological science, geography, anthropology, psychology, sociology and work conducted in professional schools of law, business, medicine and journalism (Berelson, 1953). These disciplines were united by their efforts, directly or indirectly, to study human behaviour. What is interesting for this analysis is that the use of this term was almost non-existent prior to 1950, the key element in bringing this label to the forefront of university life being the Ford Foundation programme in this area. This programme sought to encourage efforts to:

. . . identify topics that constituted the subject matter of the behavioral sciences [such as] . . . behavior in primary groups and formal organizations; behavioral aspects of the economic system; social classes and minority groups; social restraints on behavior; and social and cultural change. (Berelson, 1968: 43)

Important here was a scientific ethos, reflected in the demands by the Ford Foundation, that work carried out with its funding had to produce ‘behavioral knowledge under conditions which, as far as possible, ensure objectivity, verifiability, and generality. It called for conformity to high standards of scientific inquiry’ (Berelson, 1968: 42) as a way to establish ‘scientifically validated generalizations about the subject matter of human behavior – how people behave and why. They are thus interested in motivation, perception, values and norms . . . social institutions and culture’ (Berelson, 1968: 43). Even closer to the disciplinary home of marketing and consumer research, the foundation wanted to encourage scholars to attempt to ‘understand, explain, and predict . . . the behavior of goods and prices in the marketplace’ (Berelson, 1963: 3).

Notably unusual about these proposals was their departure from the traditional development of academic research specialties. Where previously the history of scientific endeavour provided examples of intellectual concepts (e.g. psychoanalysis) becoming steadily administratively institutionalized, the behavioural sciences represent the reverse: an administrative arrangement devised by the Ford Foundation that was intellectually institutionalized. Ford’s support for the behavioural sciences was influenced by the acceptability of the term, with this phrase gaining supporters who wanted to affiliate themselves with it ‘because its neutral character made it acceptable to both social and biological scientists’ (Miller, 1955: 513). This marriage of convenience was especially important for those seeking funding for their research, whose potential financial supporters ‘might confound social science with socialism’ as academics were expected to exhibit extreme caution more generally in relation to the labels they attached to themselves and their work (Miller, 1955: 513). This, after all, was an historical context in which J. Edgar Hoover’s surveillance of academics was reaching its zenith, as he published a guide detailing methods of identifying Communists’ and fellow travellers who appar-



ently drew attention to themselves by using phrases such as ‘peace’, ‘disarmament’ and, most alarmingly to an academic, ‘Restore full academic freedom for students and faculties’ (see Hoover, 1958: 187). The Ford Foundation thus had reason to be cautious. As Berelson writes, it is ‘obvious that the Ford Foundation’s commitment of several million dollars to this program had something to do with the terms acceptance and spread’ (1968: 43). Indeed, given the ostensible political neutrality of management research (including marketing), the direction of funding towards the business school as a focus for the ‘expression of the behavioral sciences’ is hardly surprising (Berelson, 1968: 44).

This criticism of the Ford and Carnegie reports in relation to the state of business education, the political expediency of focusing philanthropic funding on the business school and the large quantity of funding available, meant that the upgrading of these institutions was, it appeared, a non-zero sum game for all. Besides, refusing the advice of the foundations meant losing access to the grant money available (Cochoy, 1998). As was to be expected, in part derived from their quest for legitimacy, marketing academics readily catered to the demands of the philanthropic foundations and produced knowledge that cohered, directly or indirectly, with philanthropic objectives. In addition, prominent business school academics were quick to support the proposed modifications to business curricula and research practice, which meant contributing toward a credible, scientific basis for knowledge development. This was of major importance because the development of a knowledge base was central to the desire of philanthropic foundations to be seen to be using knowledge developed in the business school in the process of social engineering that would propel the United States to global, intellectual and economic hegemony (Berman, 1983). Underpinning this ideological vision, and structuring all philanthropic funding activity, was modernization theory – ‘the most enduring metanarrative in American intellectual life’ (Robin, 2003: 29). When viewed through this discursive framework, knowledge production is valued according to its contribution to the consistent, controlled change and progression of the US economy and those abroad, the meter of which was economic growth. In this context, the dissemination of ‘scientific’, practical marketing knowledge provided the means for more effective managerial decision making in the growing complexity of the American marketplace, by improving management predictions about the behaviours of consumers. This, in turn, enabled the (more) effective alignment of production with consumption, and strategic marketing with consumer requirements (Fischer, 1993; Massey, 1965; Myers et al., 1980).

New educational and research institutions were established and funded on the basis that they contributed to this ideological direction, generating ideas and training the manpower necessary to accomplish these goals (Parmar, 2002; Robin, 2003). As Nugent (2002) argues, the *primary* function of these philanthropic foundations was the creation of an academic elite who could be trained to appreciate and subscribe to the technocratic and utilitarian belief in the value of empirical, practically oriented research, rather than descriptive or theoretical research (Arnove, 1980; Parmar, 2002). This distancing of the production of



knowledge from largely descriptive research was a politically motivated shift encouraged by the funding proclivities exhibited by the Ford Foundation, directed to support logical positivist/logical empiricist interdisciplinary research that was closely affiliated with the image of the natural sciences and its objective and dispassionate nature; support that was a reflection of political necessity as the foundations wanted to distance themselves from research projects that could be construed as ideologically motivated (Robin, 2003).

Given the climate of the time and the political pressures acting upon the Ford Foundation it is not surprising that they were reluctant to provide funds directly to researchers for the pragmatic reason that their funds might be used in enlarging the Foundation's projects without necessarily improving the quality of the research undertaken. In equal measure, concern was voiced that it might be used to fund research that the Ford Foundation, in this instance, did not wish to be connected with (Berelson, 1953). One of the most appropriate courses of action, it was believed, was to control the training and research 'within one of the philanthropies' remade institutions of higher learning' (Nugent, 2002: 11). These institutions were 'remade' in the sense that the philanthropic organization invested large sums of money in order to enhance the facilities, so that the colleges could offer scholarships to promising students and academics alike, training the selected few in a way that supported the ideological beliefs of the sponsoring organization. Specifically, this meant students 'were schooled in the scientific, empirically grounded, practically oriented concepts, methods and techniques that philanthropies believed would make a contribution to the most pressing social problems of the day' (Nugent, 2002: 11).

Axiological redirection

The funding derived from the Ford foundation served to usher in a new age for marketing; their influence on the subsequent epistemological direction of marketing is apparent in a variety of places, including sponsored textbooks, seminars and conferences. In the foreword to John Howard's (1963) *Marketing Executive and Buyer Behavior*, sponsored by the Ford Foundation as part of their 'program to encourage new directions in business education', the Vice President of the Ford Foundation, Dyke Brown, suggests that Howard's publication was funded as a way to encourage the movement of marketing theory towards a more scientific style, drawing upon the 'social sciences, modern mathematics and statistics with a greater emphasis on analysis rather than description' (Brown, 1963: v). To speed this shift, the Ford Foundation sponsored the preparation of a variety of publications 'to provide teachers throughout the country with readier access to recent advances in particular areas of business administration' (Brown, 1963: v). These were not books for the students, but for the teachers, designed to enrich their pedagogy. They were very successful in this regard, functioning as the 'dominant' paradigm for marketing research and teaching because they 'obviated what had been a pedagogic nightmare' (Kernan, 1995a: 554). Moreover, the



marketing of this viewpoint was not limited to the academic community alone, but further promoted by being repackaged and redistributed in a suitably modified form to 'a wider, less specialized audience', as Howard remarked in the preface to his 1965 text *Marketing Theory* (Howard, 1965: vii); a text that would, in turn, be modified and extended 'in a more systematic, comprehensive and rigorous way' by Howard and Sheth in their *The Theory of Buyer Behavior* (1969: viii).

In keeping with their desire to stimulate the transformation of business education called for by their report, the Ford Foundation supported changes in undergraduate and postgraduate marketing education spending: over the period 1957 to 1965, \$46.3 million on business research and educational reforms (Magat, 1979: 106). These reforms took a variety of forms, such as funding provision for individual business schools (e.g. the Carnegie Institute of Technology); the reformulation of doctoral programmes; the use of mathematicians and social scientists for research training in doctoral education; and fellowships to researchers including John Howard for his textbook writing, Philip Kotler in relation to his work on the use of computer simulation for marketing strategy (Kotler, 1967) and Perry Bliss for his study of the behavioural sciences and their contribution to marketing⁷ (Bliss, 1963).

In conjunction with these specific projects, funding was made available for visiting professorships in the social sciences, applied mathematics and statistics. One of these professorships was provided to Wroe Alderson, whose (1957) *Marketing Behavior and Executive Action* had been lauded in the Ford Foundation report as an exemplar of good business research (Carroll, 1958; Gordon and Howell, 1959). Worthy of special note here are the Ford Foundation sponsored seminars that took place at the Carnegie Institute of Technology, the Universities of California and Chicago, and at the Institute of Basic Mathematics for Application to Business at Harvard (Bartels, 1988; Brown, 1963). The influence of these seminars should not be underestimated, having attracted at least 1500 faculty members from 300 business schools, with those held at the Institute of Basic Mathematics for Application to Business seen as a 'Landmark effort in raising mathematical competence for business teachers' (Schlossmann et al., 1987: 77). Although the influence of these workshops on teaching practice is less clear, with many participants reporting difficulties in incorporating advanced mathematics into their curricular revisions as a result of collegial hostility to these new methodological tools, the impact on research was more pronounced and seminar attendees reported greater incorporation of these tools into their own research (Willis, 1971). As a result of the positive response by conference attendees to the material demonstrated, one commentator at the Ford Foundation was moved to assert that those courses run by the Institute of Basic Mathematics for Application to Business at Harvard in 1957 and MIT in 1959 'had more impact than any other single seminar' (Schlossmann et al., 1987: 88–9).

When we consider those marketing scholars in attendance at these seminars, the impact of this focus on mathematics and quantitative techniques seems clear, with some of the most productive marketing scholars of the period, including



Philip Kotler, Robert Buzzell, Frank Bass, William Lazer, Jerome McCarthy, Edgar Pessemier, Donald Shawver, Abraham Shuchman and John Howard, enrolled at the Institute for Basic Mathematics (Bartels, 1988; Kerin, 1996; Lazer, 1970). Indeed, being involved with graduate teaching and also being active researchers were criteria for eligibility (Gordon and Howell, 1959). Recollecting his time at the Harvard-Ford seminars, Buzzell says:

This year affected me greatly. For one thing I learned a lot about math and statistics The program gave me a “quantitative view” of the world that I’ll never lose. The program also exposed me to a group of bright young faculty people from a variety of fields. (Bartels, 1988: 259)

Wilkie is equally effusive regarding the importance of these seminars and their impact on the future development of marketing and consumer research: ‘there’s no question in my mind that even a causal tracing of the participants and their students will reveal a *huge impact* on the course of research in marketing’ (Wilkie, 2002: 144; emphasis in original). Obviously these individuals have been and continue to be tremendously influential in marketing; they are among the most frequently cited contributors to the major academic journals (Myers et al., 1980; Wilkie, 2002). Surely then, it is no understatement to suggest that their influence and academic contributions, formatively shaped by the funding of the Ford Foundation, have fashioned the identity that marketing theory has today by facilitating the quantum leap in the ‘upgrading’ of the analytical sophistication of marketing theory and in redirecting marketing away from naive empiricism towards logical empiricism (Bartels, 1988; Brown, 1963; Howard, 1965; Levy, 2005; Myers et al., 1980; Wittink, 2004).

The influence of the Ford Foundation through their seminars does not, it must be acknowledged, end here. It is this initial cohort of marketing researchers that forms the core of an intellectual network whose boundaries expand further as new generations of marketing scholars are taught and schooled in this paradigm by the first generation of graduates from the ‘revolutionary Ford Foundation program’ (Staelin, 2005: 146). In his recent reflection on this period (1960s) in marketing history, Staelin documents the continuing influence of the Ford Foundation programme at Harvard. He recalls that those taught by first generation Ford Foundation scholars – Frank Bass, Ed Pessemier and Philip Kotler – were influential in training what he calls the second generation of Ford scholars, including Don Lehrman, John McCann, Dick Wittnik, David Riebstein, Abel Jeuland, Len Parsons, Mike Hanssens, Al Wildt, Rick Winter, John Summers, Jim Ginter, Doug Tigert, Ron Turner, Dennis Gensch, Neil Beckwith, Frank Houston, Albert Bemmaor, Bob Leone and Wilfred Vanhonacker, all of whom ‘helped solidify the infusion of scientific theory, methods, and analysis into the field of marketing’ (Staelin, 2005: 146). This movement was further supported by the institutional changes made throughout the marketing academy, as faculties were bolstered by researchers trained in the behavioural sciences, mathematics and statistics moving into major research centres, including M.I.T., Carnegie-Mellon, Chicago, Stanford, Harvard, Wharton, Northwestern and Berkley (Myers et al., 1980). In

this context, to be viewed as producing ‘respectable’ theory, marketing scholars had to symbolically subscribe to a hypothetico-deductive model of science which was philosophically grounded in a form of logical empiricism and associated with quantification, inter-subjective certification and, where possible, taking ‘the form of an equation relating a dependent variable to one or more independent variables in terms of a causal relationship’ (Schwartz, 1963: 134).

Indicative of this turn was the emergence of texts broadly supporting this philosophical and methodological style (Bass et al., 1961; Buzzell, 1964; Frank et al., 1962; Montgomery and Urban, 1969; Zaltman, 1965). This is not to suggest that marketing scholars were unaware of alternatives to the received view of knowledge. Rather, they continued to adhere to the dictates of logical empiricism because this view was supported by the research culture and buttressed by the redoubtable efforts of the American Marketing Association to advance the science of marketing. This scientific style was further supported by the ‘bias’ reflected in the editorial direction of established journals and mirrored in new outlets – including the *Journal of Advertising Research* and the *Journal of Marketing Research* which emphasized empirical data, the use of quantitative methods for data analysis and the use of mathematical modelling (Webster, 2005: 122).

In isolation, these changes could not have supported the ‘upgrading’ of the academy. They required technological developments to support them and the requisite knowledge of how to use the new computer technology. Again, the Ford Foundation was quick to respond, financing conferences and seminars on the potential contribution of computer technology to marketing research and data analysis (Alderson and Shapiro, 1963: acknowledgements).⁸ There were continued demands by business executives as to whether mathematical modelling techniques could be used to contribute to management decision making (Buzzell, 1964). These demands, together with the availability of computer processing power combined with large and reliable databases, made it possible, and expeditious, for marketing researchers to experiment with larger numbers of variables than was previously possible. Consistent with this turn towards quantitative computer modelling, it is at this time that the information processing model of consumer behaviour rapidly gained support as a result of the ease of modelling this view using the latest computer technology (Wilkie, 1986).

Clearly important in continuing this axiological shift was the formation of the Marketing Science Institute – the only major research programme attempting to ‘lay the foundations of marketing as an integrated body of knowledge’ (Willis, 1971: 358). This institution has been, and remains, extremely influential in the development of marketing theory and practice, acting as a bridge between the academic and practitioner communities. As the then Director of the Marketing Science Institute noted, their role, in particular, was to ensure the ‘crash cultivation’ of a new field of science (Halbert, 1965: xxv). It is also one of the few institutions which has been seen to have a direct impact on the genealogical development of marketing by way of its research programmes and projects, most notably the Research Priorities Program established in 1974, which have been influential in relation to ‘academic agendas for many marketing faculty in the



United States and around the world' (Greyser, 1997: 12). These priorities are the 'most wanted' issues that the member companies of the Marketing Science Institute would like researchers to tackle and are 'considered the most desirable for meaningful research' (Greyser, 1997: 12). These trends were further encouraged through the founding of the journal *Marketing Science*, the annual Marketing Science conference in 1979 and the establishment of the 'marketing college' in The Institute of Management Science (TIMS) (Bloom, 1987; Kerin, 1996; Montgomery, 2001).

The results of these philanthropic efforts are hard to ignore. Just 15 years after these changes, almost all graduate-level courses in marketing in the United States taught multivariate statistics, with 'courses on mathematical modeling of marketing phenomena . . . common, and marketing journals are filled with articles reporting the results of complex mathematical analyses' (Stewart, 1991: 28). As might be expected, not all were sanguine about the domination of publication opportunities by a small core of behaviourally focused research departments and their PhD programmes (Cohen, 1995). Engel is perhaps the most critical of all the commentators here, arguing that 'poorly conceptualized borrowing from the behavioral sciences' (1995: 549) and the importation of advanced mathematical techniques into marketing has resulted in research degenerating 'to a substantial amount of number crunching over trivial issues' (Engel in Bartels, 1988: 264). Lehmann is no less critical, asserting that there is a certain degree of statistical sterility in marketing, with the 'blind use of cookbook statistics [having produced] a rigor mortis in our thought process' (1996: 3). Having said this, the confluence of this host of institutional supports coming together at a similar juncture in the history of marketing served to push logical empiricism and the behavioural sciences 'to the fore' (Levy, 2005: 341; see also Bloom, 1987; Kassarian, 1995; Kernan, 1995a, 1995b; Mittelstaedt, 1989, 1990; Myers et al., 1980; Sheth and Gross, 1988: 18). A position in which it should be added, logical empiricism remains today (cf. Brown, 2004, 2005).

Marketing as science

The belief that marketing will one day establish itself as a 'science' remains widespread, and as the above analysis has indicated, the material support of the philanthropic foundations formatively influenced the axiological direction that marketing theory was to take. This reorientation of marketing theory and business education generally was itself a function of the political and economic interests of these foundations. As Berman points out – 'Their vast wealth enables them to articulate programs, set certain agendas' (1983: 176). But lest this be seen as appealing to a crude determinism, those policies that guided the funding provided by the Ford Foundation served not only the interests of the Foundation itself – in that it offered a politically safe and visible project to fund while the political environment was so unstable – but they also provided an influx of money that speeded up the professionalization of marketing and radically overhauled research



conducted by marketing scholars. In this respect, individual career interests, those of the discipline, academic associations and the business school were closely intertwined with the interests of Ford philanthropy.

This funding has, nonetheless, represented a double-edged sword: on the one hand, it stimulated a generation of marketing scholars to enlarge the disciplinary horizons from which they could legitimately draw and placed greater emphasis on logical empiricism and 'scientific method', along with a predilection for research with a quantitative orientation. In equal measure however, the funding provided by the Ford Foundation and the subsequent definition of what a science of marketing might look like was grounded in their view of scientific activity and heavily influenced by a particular view of the scientific style of the natural sciences, thereby institutionalizing certain 'biases and barriers . . . in terms of the theoretical development of the field' (Dholakia et al., 1980: 25). This again is unsurprising. While still in vogue at the time of the publication of the Ford and Carnegie reports, and figuring centrally in the funding strategies used by both organizations, the limitations of remaining attached to logical empiricism alone led to greater interest in alternative paradigms (Arndt, 1985a, 1985b).

Pluralism?

Despite greater calls for pluralism in marketing theory, logical empiricism prevails among the available paradigms, thereby circumscribing the horizons of knowledge production by delimiting what forms of knowledge are seen as legitimate. As one commentator put it, the

behavioral science revolution . . . has led to a decline in many areas that do not fit with their perceived criteria These behaviorists affect the development of new ideas in marketing because who gets published, what gets published, and where it gets published often depend on their standards and values. (Morris, 1996: 4)

The interest in 'purely academic research' stimulated by both the Ford and Carnegie reports has been seen by some commentators to succeed too well, to the extent that practical relevance of research findings is accorded a secondary place in academia, with too much emphasis placed on quantitative methods (Behrman and Levin, 1984; Buzzell and Sisodia, 1997). In a more speculative vein, Desmond (1998) suggests that it may be the historical memory of the Cold War that continues to hamper the emergence of a more prolonged engagement with the two radical paradigms in the Burrell and Morgan (1979/1992) typology, especially among North American researchers (Lee and Murray, 1995). While Desmond's view is plausible, what can be affirmed with more certainty is that the lack of substantive engagement with alternative paradigms continues to be reinforced by the structural impediments that constrain knowledge production. Where the main impact of the Ford and Carnegie reports was undeniable, having 'widespread and lasting effects . . . for the next twenty-five years' (Lazer and Shaw, 1988: 149), perhaps the most important turn stimulated by these reports was towards



the behavioural sciences and the much greater use of quantification and the equally problematic marginalization of qualitative research in marketing. This reorientation stimulated in marketing remains in place partly because researchers schooled in the logical empiricist paradigm have attained important positions within the academy. Likewise, pressures to publish or perish, the time compression of PhD programmes and tenure tracking have been suggested as other possible factors that serve to delimit the paradigmatic style of marketing theory by skewing it towards logical empiricism (e.g. AMA, 1988; Bloom, 1987; Myers et al., 1980). In contrast to Kuhn's (1970a) view that research paradigms are overthrown when enough anomalies accrue, it would seem, therefore, on the basis of the analysis undertaken here, it is rather that specific paradigms remain pre-eminent while they are commensurate with salient social values and remain supported by the wider political, economic and academic environment.

Often explicit but occasionally implicit, marketing and consumer scholars still warn that to step outside the boundaries of acceptable scholarly convention is a dangerous strategy with the following comments typical of those offered to doctoral students: 'Morris [Holbrook] warns those early in their careers – college students, MBAs, PhD candidates, and even young assistant professors' that deviation from the mainstream of marketing theory is a potentially dangerous career strategy (Holbrook, 1995: 23). Brown (1999) thinks this warning might be Holbrook attempting to keep other researchers from invading his turf and perhaps he is right. Holbrook (2000) certainly remained silent on this issue when responding to Brown's (1999) charge. Hackley sums up the present state of pluralism marketing theory well when he registers that despite the 'bewildering variety of methodological wisdom to choose from . . . you have to locate your preferred metatheory within marketing's shadowy ideological paradigm' (Hackley, 2001: 39). Thus, the very fact that there are multiple paradigms available for marketing scholars to adopt does not necessarily mean that each viewpoint will be seen as equally correct – the very definition of pluralism (Rescher, 1995) – as there are explicit normative rules of discursive formation that must be adhered to if the proposed contribution to knowledge is to avoid consignment to the margins of marketing theory (cf. Holbrook, 1997: 146–7). The comments by Greenley (1995) highlighted at the start of this article serve to illustrate this point most vividly.

Conclusion

In this article it is clear that, within the marketplace for ideas, not all ideas are equal. I have demonstrated that the relationship between power and knowledge production has an important and much neglected influence on the development of marketing theory, and presented the development of marketing theory as relative to the social conditions of the time and entwined with the scientific worldview of the Ford Foundation. In sponsoring the development of marketing theory and in training a generation of marketing scholars who have continued to advance, in the main, a paradigmatic position based on logical empiricism, this philanthropic



foundation influenced marketing theory in an important and enduring fashion that has had both positive and negative implications for the development of marketing and consumer research.

Far from intellectual debate being a forum where ideas are judged solely on their merit, the production and effective marketing of theory necessitates that theory is marketed to negotiate the practices of exclusion which admit only certain forms of research into the canon, while pushing other material to the margins of marketing theory. The 'paradigm debates' on this reading are not so disciplined that they totally cease to take place (Kassarjian, 1989). Instead, controversy 'may rage as long as it adheres to the presuppositions that define the consensus of elites, and it should . . . be encouraged within these bounds' (Chomsky, 1989: 48). Such pluralism exhibits what Žižek terms 'para-politics', a pluralism that acknowledges political conflict, but where it is reformulated as conflict within a carefully proscribed intellectual space (Böhm, 2003). This intellectual space was, to a certain extent, circumscribed by the radical changes enacted in marketing by the confluence of a wide range of influences, each of which had a system-reinforcing character that continues to support the discourse of logical empiricism. Despite Kuhn's initial recognition of paradigm debates to become closed, there still remains space within the multiple power relations that constitute the disciplined environs of marketing in which we can rethink what is taken for granted.

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Notes

- 1 Lowe et al. (2005: 197) do offer a certain degree of insight into this issue when they suggest that 'all representations are captive of language-based power relations'. Unfortunately they fail to acknowledge that they themselves are captive of these relations in promoting their paradigm-mapping strategy to overcome incommensurability. Moreover Kuhn himself has long since moved beyond methodological incommensurability – the only form Lowe et al. could be referring to, given their citation of Kuhn (1962) (see note 4). Moreover they also fail to offer any guidelines as to how incommensurability could be overcome by way of theory-adjudication, in other words, the practice of theory-adjudication that Kuhn denies in his 1962 work.
- 2 Here discursive practices are linguistic and consist of a series of statements which, for Foucault (1977/1991), might be represented by the penal code. Non-discursive practices, Deleuze (1988) maintains, are visible entities like prisons or the public



humiliation trials that characterized the McCarthy era (Garfinkel, 1956). Discourse, for Foucault, consists of the expert pronouncements of those who are empowered to speak within a given disciplinary formation. Expert pronouncements, Foucault asserts, have their basis in unwritten rules of appropriate discourse formation and are to be found within the archive; the archive being the sum total of discursive formations in circulation at a particular time. Discursive formation is used by Foucault to refer to how, within a given language community, it is often unacknowledged that language practices constitute and group together the objects and subjects that are talked about, so much so, that these language practices become second nature (Foucault 1972/2002). A discursive formation consists of statements coalescing around a similar topic but not necessarily in agreement, since there will be conflicting statements or representations of an issue. In his archaeological work, his concern with a discursive formation is with acts of expert pronouncement and the constitution of these as learned practice that, in turn, shape the behaviour of those within the disciplinary field. As he moves from his archaeological to his genealogical period, Foucault recognized that his archaeological representations of systems of thought lacked any great consideration of the material arrangement and strategic use of power, knowledge and social practice and failed to effectively mobilize subjugated knowledge. This led Foucault to reorient his analytic focus. While he never abandoned the archaeological 'methodology', from the early 1970s archaeology subordinated itself to genealogy. This should be considered as a widening of the scope of the analytic focus of Foucault's work toward the thematization of the operations of power, which had, hitherto remained largely implicit but not wholly absent from his archaeological works. In relation to power Foucault offers an innovative thesis in that he tries to move beyond thinking of power as a form of repression and constraint by the powerful on the powerless:

we should admit that power produces knowledge (and not simply by encouraging it because it serves power or by applying it because it is useful); that power and knowledge directly imply on another; there is no power relation without the correlative constitution of a field of knowledge, nor any knowledge that does not presuppose and constitute at the same time power relations. (Foucault, 1977/1991: 27)

Power, in terms of the repressive thesis, would question the ability of the individual subject to overcome the dominant ideology espoused by the more powerful in society, in the case of the history of marketing, for example, the taken-for-granted view of the historical development of marketing and the role of marketing discourse in society (see Morgan, 2003). Foucault, as the above quotation highlights, questions the plausibility of the repressive thesis that power is all pervasive, positing: 'where there is power, there is resistance' (Foucault, 1979/1998: 95). There is freedom for the subject, but this is dependent upon the resources they have at their disposal, in terms of individual capacities and those in the structures of society. Nor is power an abstract concept that can be isolated and studied as an object; it exists only in terms of concrete actions and enactment and can be understood as:

the multiplicity of force relations immanent in the sphere in which they operate and which they constitute their own organization; as the process which, through ceaseless struggle and confrontations, transforms, strengthens, or reverses them; as the support which these force relations find in one another, thus forming a chain or a system, or on the contrary, the disjunctions and contradictions which isolate them from one another; and lastly, as the strategies in which they take effect. (Foucault, 1979/1998: 92–93)

The thesis ‘knowledge is power’, or ‘power is knowledge’ is, therefore, an incorrect characterization of Foucault’s interest, ‘studying their *relation* is precisely my problem’ (Foucault, 2000: 455, emphasis in original).

- 3 The scare quotes surround our use of this term in recognition of the philological deficiencies of such a label (e.g. Szmigin and Foxall, 2000). It remains nonetheless current practice in marketing and consumer research to use this or some variant of this term (Brown, 2005).
- 4 Not using the concept of the paradigm in the manner as it was used by Kuhn would, quite naturally, lead scholars to call this interpretation into question as they did (Donaldson, 1985), as we would with Lowe et al.’s interpretation of Burrell and Morgan’s characterisation of paradigm where they appear to suggest assumes a ‘objectivist epistemological notion of cognition as a representation of an independent, pre-given, concrete, ontological reality’ (2005: 190–1). While Burrell and Morgan may metaphorically refer to objectivism (Booth, 2002), they bring their own subjectivity to the fore in their paradigm journey. To paraphrase Lowe et al., their view was that ‘meaning resides in context’ (Lowe et al., 2005: 191). This said, Kuhn himself moved away from the paradigm concept over the course of his writing, from paradigm to disciplinary matrix to taxonomical lexicon, ultimately eschewing the use of the label paradigm post 1980 (see, Kuhn, 1974, 1977a, 1977b, 1983a, 1983b, 1990, 1991). Much more interesting than staying with Kuhn’s (1962, 1970a) work are his individual publications, particularly his later work on disciplinary wide values which heralds a movement away from the paradigm concept but, somewhat paradoxically, is a view that is surprisingly close to Burrell and Morgan’s (1979/1992) suggestion of disciplinary wide values that members of their broad paradigm concept could subscribe to. For further problematization of Burrell and Morgan’s typology, Deetz (1996), Hassard (1985, 1993), Jackson and Carter (1991, 1993) and Willmott (1993a, 1993b) offer relevant criticism. As might be expected, Burrell himself offers interesting counterpoints to those who support commensurability without acknowledging the political intent underpinning the incommensurability thesis in his and Morgan’s (1979/1992) work. It is also worth pointing out that the incommensurability thesis, whether methodological, semantic or values-based, has also been subject to wide-ranging criticism (Sankey, 1994, 1997).
- 5 This view, of course, depends on the conception of ontology and politics that the scholars concerned may hold. Here we subscribe to the weak incommensurability that Anderson (1986) discusses. Of course the distinction that we invoke is a reflection of our own understanding of the philosophy of science, the sociology of science combined with its own peculiar inflexion of social epistemology. Where, for example, analytic philosophy placed the Cartesian individual at its epistemological epicentre, in contrast to the Continental tradition, where the shared value commitments were seen to assume greater importance (Foucault, 1970/2003), we follow Fuller (2002) in moving away from the idealized image of science assumed by either the philosophy of science or the sociology of science and want to question how a particular representation of scientific inquiry becomes emplaced. We are in certain respects more inclined towards the political negotiation that is associated with social constructivism in viewing scientific knowledge as constructed via negotiation, accident, and selective interpretation (Knorr-Cetina, 1981, 1991). Or in terms of the philosophy of science towards Laudan’s views on theory-choice (1977, 1996) – while tempering his arguments regarding the underdetermination of theory by evidence with Quine and Ullian’s (1978) arguments regarding the use of standards for theory



adjudication. We therefore subscribe to a version of rationality, scientific progression and theory choice that operates across paradigms, but only within certain discursive boundaries. With scientific research on this reading not only the process of consensus generation but one in which people do, on occasion, consciously adjust the manner in which arguments are framed to make their claims more marketable (Latour and Woolgar, 1986). Hence the use of citations as symbolic support in this article where comments such as imperialistic paradigms, social pluralism and other potentially polemical language is used; as one reviewer registered, these kinds of comments are, in their eyes, not entirely suitable for an academic article, while another wanted to know what the author really thought. Again these kinds of comments hint at the discursive limits in marketing theory that we must negotiate. Here comments found in any of the following would be pertinent (Brown, 1995, 1998, 2003, 2004, 2005; Cahill, 1993; Hackley, 2001; Kassarian, 1989, 1994, 1995; Levy, 2003a, 2003b, 2005; McDonagh, 1995; Morgan, 1992, 2003; Peter et al., 1980; Shankar and Patterson, 2001).

- 6 Here and in other places 'dominant' is used without any pejorative intent and to indicate general subscription to a certain style of reading, writing and analysis (cf. Arndt, 1985b; Hunt and Speck, 1985).
- 7 The importance of these texts and concomitantly the funding from the Ford Foundation cannot be underestimated. As Mittelstaedt (1989: 4) noted, Howard's and Bliss's texts were "'milestone" books' in the founding of consumer research as a distinct discipline.
- 8 This point deserves due qualification as Professor Stanley Shapiro pointed out to the author. According to Shapiro, this funding was 'very indirect. Ford gave the money to Wharton, the Wharton Dean gave it to Wroe and Wroe spent it' (Shapiro, personal communication 29 October 2005). Despite the indirect nature of this funding, the Ford Foundation was careful about ensuring the consonance of the distribution of their funding to their philanthropic vision. The reader can draw their own conclusions.

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