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COMMENTARY

The bases of power approach to channel relationships: has marketing's scholarship been misguided?

Shelby D. Hunt, *Department of Marketing, Jerry S. Rawls College of Business Administration, Texas Tech University, USA*

Abstract A recent commentary by Blois and Hopkinson in this journal provides a thoughtful critique of the use of French and Raven's (1959) bases of power approach in channels of distribution research. Their commentary claims that power-base studies (1) suffer from 'weak results' and lack 'psychometrically sound' measures, (2) provide an inadequate 'categorising [of] power sources', (3) suffer from a 'questionable' use of an interpersonal theory of power in 'inter-organisational or B2B situations', (4) suffer from a 'misreporting' of the original French and Raven article and (5) are deficient because they ground channels research on a 1959 theory article that was, itself, 'much less rigorous in its arguments than would be expected', with 'uneven support' from the article's citations. The five criticisms of the power-base approach seem to imply that, throughout the four-plus decades of power-base research, marketing's scholarship has been misguided. This reply argues that the five criticisms are suspect and the power-base approach to understanding channels of distribution has provided an informative theoretical foundation for guiding research.

Keywords channels of distribution; bases of power; franchising

Introduction

Although the term 'channels of distribution' was not in common use until the work of Clark (1922) in the third decade of the twentieth century (Shaw & Jones, 2005), Shaw's (1912) second-decade, 62 page, *Quarterly Journal of Economics* treatise, titled 'Some Problems in Market Distribution', may be considered to be the beginning of the marketing discipline's academic journal literature on channels of distribution (at least in the United States).[1] Among other contributions, Shaw (1912) was the first article to: (1) diagram the, now familiar, box-and-arrow depiction of

[1]An expanded version of Shaw's (1912) article was later published in book form (Shaw, 1915).

channels of distribution, (2) use the channel diagrams to trace the 'evolution of the middleman' from 'remote ages' through the 'medieval period' to the 'modern period' (p. 726), (3) explain the 'modern tendency to reduce the number of successive middlemen' (p. 729) and (4) propose the economic functions performed by channel members, which for him included 'sharing the risk'; 'transporting the goods'; 'financing the operations'; 'selling'; and 'assembling, assorting, and re-shipping' (p. 731). Therefore, Shaw's (1912) article was a major factor in initiating the *economic systems* approach to the study of marketing channels.

The economic systems approach to studying channels of distribution dominated marketing scholarship for over half a century. In the late 1960s, Stern's (1969) edited book, 'Distribution Channels: Behavioral Dimensions', introduced a *social* systems approach to understanding channels of distribution. Among the many social science concepts emphasised by Stern's social systems approach, power and conflict began receiving much attention. As to power in marketing channels, studies have often relied on French and Raven's (1959) classic work on the 'bases of social power' (i.e., coercive, reward, legitimate, referent and expert) as a theoretical foundation.

A commentary by Blois and Hopkinson (2013) (hereafter, B&H) provides a detailed, thoughtful critique of the use of French and Raven's (1959) bases of power approach in the channels of distribution literature. Rather than comprehensively reviewing the power-base research in marketing channels, B&H scrutinise closely five articles that they consider to be 'seminal' in its development: El-Ansary and Stern (1972), Frazier and Summers (1984), Hunt and Nevin (1974), Lusch (1976) and Wilkinson (1979). B&H's commentary is uniformly critical of the seminal articles. It also criticises extensively the arguments in the original French and Raven (1959) article, as well as subsequent, power-base research. Specifically, B&H claim that studies using the power-base approach (1) suffer from 'weak results' (p. 1148) and lack 'psychometrically sound' measures (p. 1156), (2) provide an inadequate 'categorising [of] power sources' (p. 1149), (3) suffer from a 'questionable' use of an interpersonal theory of power in 'inter-organisational or B2B situations' (p. 1151), (4) suffer from a 'misreporting' of the original French and Raven (1959) article (p. 1151) and (5) are deficient because they ground channels research on a 1959 theory article that was, itself, 'much less rigorous in its arguments than would be expected' (p. 1157), with 'uneven support' (p. 1153) from the article's citations. B&H (p. 1157) conclude:

> Fundamental assumptions regarding the application of the theory to this context remain unexamined, whilst the disciplinary origins of the theory are sometimes obscured. These points lead to the naturalising of power-base theory as an adequate and comprehensive explanation of channel power. However, the paper has also suggested that French and Raven's paper is much less rigorous in its arguments than would be expected of a paper which has been so frequently cited and become so influential... The difficulties associated with power-base research lead us to conclude that channel power cannot be fully understood through power-base theory and that the field can be usefully further illuminated for both academics and practitioners through fuller engagement with other theoretic perspectives.

No doubt, there are very few channels researchers who would disagree with B&H's suggestion that 'other theoretic perspectives' might contribute to marketing's understanding of channel power. However, if their five major criticisms of the four-plus decades of power-base studies are correct, a stronger conclusion seems warranted: the use of the power-base approach to understanding power in channels relationships has significantly misguided marketing's channels scholarship. This reply to B&H will argue that the five criticisms are suspect and do not warrant the stronger conclusion.

This writer was a co-author of one of the articles (i.e., Hunt & Nevin, 1974) that was considered by B&H to be 'seminal' and, therefore, an article that was appropriate for B&H's close scrutiny. Because a portion of this reply will argue that qualitative evidence drawn from a large-scale study of franchising supports the truth-content of marketing's power-base research, I begin with a discussion of the source of the qualitative evidence. That is, I begin with the historical background of the data set used in Hunt and Nevin (1974).

Background

In January 1969, I joined the faculty at the University of Wisconsin, Madison, as an assistant professor, and in June 1969, the Small Business Administration (SBA) requested that Wisconsin and eight other universities submit research proposals for studying several types of franchising. The request was forwarded to the marketing department for response. After much departmental discussion, Urban B. Ozanne (also an assistant professor) and I offered to develop a proposal.[2] In July 1969, the SBA accepted our (hastily developed) proposal and awarded us a contract of $50,114 (later increased to $62,114). Urban and I then embarked on a 2-year investigation of franchised restaurants (mostly 'fast food' restaurants), convenience stores and laundry-dry cleaning. Indeed, it is fair to say, for two years we *immersed* ourselves in franchising as a method of distribution.

Our investigation involved three major phases. First, we reviewed the franchising literature (which consisted mostly of trade books and articles, not academic articles), and developed mailing lists of franchise systems in the areas of restaurants, convenience stores and laundry-dry cleaning. The trade literature was extensive, and we found the works of Brown (1969) and Kursh (1968) to be especially informative. In the second, qualitative phase, we conducted field interviews (both formal and informal) with franchisors, franchisees, franchise journal editors, officials of franchisor and franchisee associations, and government officials. In the third, quantitative phase, we developed questionnaires and surveyed approximately 800 franchisors (with about 150 usable responses) and approximately 4000 franchisees (with about 1000 usable responses).

The SBA had requested that we provide answers to questions related to the economic effects of franchising, the characteristics of franchisees, the operations of franchised businesses, the role of minority groups and the nature of franchisee–franchisor relationships. Also, following the advice of senior faculty in the department, Urban and I designed the questionnaires so that we would (hopefully) be able to address specific academic issues that were oriented towards marketing journals, such as the trend towards company-owned stores in franchising, the success rates of different kinds of franchisees, and the impact of 'tying' agreements in franchising.

[2]Translation: no full or associate professor had the slightest interest in pursuing the project.

In the summer of 1971, we submitted the final report to the SBA. Also, shortly before that time, Urban Ozanne accepted a position at Florida State University, and he decided to pursue research topics other than franchising. Therefore, though Urban was a valuable collaborator on the SBA franchising project, he decided not to co-author articles using our database. In September 1971, our study was published in a government monograph entitled *The Economic Effects of Franchising* (Ozanne & Hunt, 1971), which was sponsored by the United States Senate Select Committee on Small Business. In 1972, both Urban and I testified before the Federal Trade Commission concerning their proposed 'Franchising and Business Opportunity Ventures' trade regulation rule. The rule, which dealt with disclosure requirements and prohibitions, was ultimately passed in December 1978. The final rule relied significantly – as shown in the rule's footnotes – on the results reported in *The Economic Effects of Franchising*.

The SBA study provided a launching pad for further research efforts in the area of channels of distribution over several decades. Also, the data from the study provided the basis for several articles on franchising, including 'The Socioeconomic Consequences of the Franchise System of Distribution' (Hunt, 1972), 'Experiential Determinants of Franchise Success' (Hunt, 1973a), 'The Trend Toward Company-Operated Units in Franchise Chains' (Hunt, 1973b), 'Sources of Funds and Franchisee Success' (Churchill & Hunt, 1973) and 'Tying Agreements in Franchising' (Hunt & Nevin, 1975). The data also provided the basis for the article under discussion here, that is, 'Power in a Channel of Distribution: Sources and Consequences' (Hunt & Nevin, 1974).

The precipitating event that led to Hunt and Nevin (1974) was the El-Ansary and Stern (1972) article. Although an earlier work by Beier and Stern (1969) had introduced the French and Raven (1959) bases of power approach to the channels literature, El-Ansary and Stern (1972) was the first article that measured channel power and tested hypotheses concerning power as a function of dependence and sources of power. They measured power as perceived influence over marketing decision variables. The source of power was measured in a proxy manner, that is, it was assumed to be related to the importance of such channel attributes as 'product sales meetings' and 'financial and business advice'. El-Ansary and Stern (1972, p. 51) concluded that their weak findings may have resulted from the fact that 'power was... diffused within the system we studied'.

As I read El-Ansary and Stern (1972), it occurred to me that our franchising data set came from a channel with a well-defined power structure as a result of the franchise contract. Furthermore, the data set had a measure of perceived power similar to that of El-Ansary and Stern (1972). Therefore, perhaps an article on power relationships in a franchised channel of distribution might be feasible. Because I was working on several projects at the time, I asked Jack Nevin if he wanted to join me as co-author on an article targeted for the *Journal of Marketing Research* (*JMR*). He was receptive to the idea. We agreed that, though both of us would work on all aspects of the article, he would assume primary responsibility for the statistical analysis. Jack's recollection of how we developed the article, as published in Nevin (2011), accords with mine. That is, we believed that we had an excellent data set, with a good measure of power and an acceptable measure of coercive power. However, we had a major problem: we did not have direct measures of reward, legitimate, referent and expert power.

In an attempt to solve the measurement problem, we first aggregated reward, legitimate, referent and expert sources of power into a construct we called 'noncoercive' sources of power, which we argued can be distinguished from coercive sources of power in that they involve a willingness on the part of an individual or channel member to yield power to another. We then developed a proxy measure of noncoercive power, which consisted of the perceived quality of several types of assistances that franchisors provide to franchisees, and we argued that high quality assistances would (1) establish the franchisor as an expert in the eyes of the franchisee, (2) legitimise the franchisor's efforts to gain power and (3) help to get the franchisee to yield power willingly to the franchisor. Although proxy measures are commonplace throughout the social sciences, especially economics, we did not know if *JMR* reviewers would find our measure acceptable. When Jack ran the regressions, the results tended to support our two major hypotheses: (1) franchisors in our sample tended to rely primarily on coercive sources of power and (2) franchisors who relied *less* on coercive sources had franchisees who were *more* satisfied with the franchised relationship. We wrote the article and submitted it to *JMR* in February 1973.

In April 1973, we received the editor's decision and the reviews. The editor (Frank Bass) and the anonymous reviewers had no problem with any part of our power base, theoretical foundation. Indeed they had only one significant problem with the entire manuscript: they believed that the use of regression was inappropriate because most of our measures were only ordinally scaled. (In the 1970s, a common research norm was that regression was to be used only with intervally scaled variables.) The editor welcomed a revision, but only if it used a different analysis technique. Nevin (2011, p. 189) describes how we addressed the analysis problem:

> In those days, it was common for our marketing faculty to always be discussing research issues at lunch and in the halls. One day at lunch, Bill Peters, another assistant professor in marketing at the time, mentioned to me that I should look into multiple classification analysis (MCA), a statistical technique developed at the University of Michigan. I talked to the folks at Michigan and they sent me an extensive card deck that was the program. Obviously, we are talking 'stone age' with respect to the development of statistical programs. I reanalyzed the data using MCA and found virtually identical results. In revising the manuscript, we presented both the regression and the MCA results, showing the similarity of our findings.

Jack and I submitted the revision in July 1973. Frank Bass accepted it, without further revision, in August. When *JMR* published our article in May 1974, neither Jack nor I had any reason to believe it would be so highly cited, but as B&H point out, it has been.

With the preceding as background, I now evaluate B&H's critique of the power-base approach to studying channels of distribution. Specifically, I address B&H's five major criticisms.

On measures and results

B&H maintain that channels studies using French and Raven's power-base approach lack 'psychometrically sound' measures (p. 1156) and suffer from 'weak results' (p.

1148). In philosophy of science terms, this criticism attacks the truth-content of the power-base approach. That is, according to philosophy of science, in general, and the inductive realist model of theory status (Hunt, 2012), in particular, theory acceptance in science is influenced positively by empirical successes and negatively by empirical failures. If power-base research relies on poor measures and 'weak' empirical findings, then the acceptance of power-base theory as appropriate for explaining phenomena, predicting phenomena and guiding interventions in the real world is inappropriate.

In response to the charge of poor measures and weak results, I first ask readers to recall that B&H do not do a detailed review of the literature to support their claims. Rather, their analysis examines (1) the measures and results of the seminal articles in the 1970s and (2) the conclusions of subsequent reviews, such as that by Schriesheim, Hinkin, and Podsakoff (1991). In evaluating the evidence supporting B&H's charge of poor measures and weak results, readers should be mindful that the subject of how to measure social science constructs, both in the 1970s and now, is a highly controversial area, with many strongly held and conflicting views. Indeed the recent exchanges among Diamantopoulos (2013), Howell (2013), Lee, Cadogan, and Chamberlain (2013) and Rigdon (2013) on the relative merits of formative versus reflective measures dramatically illustrate the continuing contentiousness of measurement and analysis issues in social science. With the preceding in mind, I believe that there is a good likelihood that a careful review of the measures and findings of over four decades of power-base research in marketing, when compared with the measures and procedures used in other social science areas, would result in a conclusion that empirical research in the power-base research area has been at least as competently executed as research in other areas.

Second, for the sake of argument, let us assume that B&H are correct that the four-plus decades of studies using the power-base approach are characterised accurately as having poor measures and weak results. If this is true, why have so many scholars in marketing, management and the social sciences relied so heavily on the power-base approach? Indeed, at the time of this writing, the original French and Raven (1959) article has received over seven *thousand* Google Scholar citations. To put this number into perspective, no other article in the book in which the French and Raven (1959) article appeared has received more than three *hundred* citations, despite the fact that all the articles focused on the same concept, that is, social power. Does the widespread use of the power-base approach suggest widespread ignorance or something else? I argue for 'something else'.

The inductive realist model of theory status points out that theory acceptance/rejection in science is based on (1) conceptual epistemic factors (e.g., the consistency of a theory with other accepted theories), (2) empirical success factors and (3) empirical failure factors (Hunt, 2012). As to empirical successes and failures, I suggest that, even if B&H are correct (that the power-base approach lacks strong, supportive, *quantitative* evidence), the power-base approach merits the attention/acceptance it has received because of its strong, supportive, *qualitative* evidence.

As discussed in the previous section, for 2 years Urban Ozanne and I were immersed in franchising, with scores of formal and informal interviews with knowledgeable informants. What we observed in the qualitative phase of our research was how franchisors and franchisees interacted with each other. Specifically, we observed how channel members attempted to influence the decisions of others. We observed that franchisors often threatened to punish recalcitrant franchisees (i.e., used coercive power), argued that the franchise contract gave them the right to dictate certain franchisee actions (i.e., used legitimate power), maintained that they had superior knowledge of the marketplace (i.e., used expert power), promised that good things would happen to acquiescing franchisees (i.e., used reward power) and argued that franchisees should be 'team players' (i.e., used referent power). Furthermore, we observed how upset franchisees were when franchisors punished or threatened to punish them (i.e., conflict resulted from the use of coercive power).

As just one example, consider the decision by McDonald's Corporation in the late 1960s to alter the standard design of their buildings so that every free-standing unit would have inside seating. Because existing McDonald's restaurants could not be remodelled to incorporate inside seating, the new design meant that most franchisees would have to invest several hundreds of thousands of dollars to demolish (or abandon) existing units and construct new buildings. In interviews and informal discussions, franchisees told us that (1) many of them were reluctant to make the additional investment and (2) McDonald's representatives used a combination of arguments to attempt to secure franchisees' acquiescence.

First, franchisees told us that McDonald's representatives indicated that the new building design had been tested, and that the tests showed that increased sales and profits would fully justify the increased investment. That is, using French and Raven (1959) terminology, the representatives used expert power. Second, representatives offered to provide franchisees with financing assistance in the construction of the new buildings (i.e., they used reward power). Third, representatives argued that McDonald's (1) had the responsibility of maintaining a uniform appearance for all the units in the system so that customers had a uniform experience when they ate at McDonald's and (2) the franchise contract gave McDonald's the right to require uniform buildings (i.e., they used legitimate power). Fourth, representatives argued that franchisees were all part of the McDonald's 'family' and that everyone had a responsibility to other members of the 'family' to make the additional investments (i.e., they used referent power). Fifth, representatives warned franchisees that their franchise contracts might not be renewed if they did not make the investment in new buildings (i.e., they used coercive power).

When Jack Nevin and I started to write 'Power in a Channel of Distribution' in 1972, I believed the *qualitative* evidence that I had gleaned from the interviews and discussions with franchisees mapped extraordinarily well into French and Raven (1959) classification of sources of power. The power-base classification made sense to me in 1972; it continues to make sense now, after several decades of additional work on channels of distribution. Indeed, I suggest that the congruency of power-base theory with qualitative evidence helps explain the continuing success of French and Raven (1959) approach across numerous scholars in the social sciences. In short, to practicing researchers, the French and Raven (1959) approach makes sense; it is consistent with their field experience.

On categorising power sources

B&H argue that the power-base literature provides an inadequate 'categorising [of] power sources' (p. 1149). Specifically, they argue that (1) the coercive versus noncoercive distinction is suspect because the 'withholding of a reward may be a punishment' (p. 1150), (2) there may be cultural differences 'across national borders' (p. 1150) that imply different categories and (3) though there have been other power sources (e.g., information power) that been suggested in social psychology, channels researchers have been 'reluctant to advance knowledge in line with developing theories of power sources taking place outside the channels field' (p. 1150). Therefore, (4) 'care must be taken in reading this as a cumulative field' (p. 1150).

In response, I first point out that care must be taken in reading *any* social science field as strictly cumulative. The power-base literature is no exception. Second, it is true that in some circumstances, the withholding of a reward may be viewed as a form of coercion. However, in *most* circumstances in channels of distribution, the outcomes of influencing the decisions of channel members by rewards versus punishments are very different: people respond positively to rewards and negatively to punishments. As French and Raven (1959, p. 165) put it in their original article, 'coercion results in decreased attraction of P toward O and high resistance; reward power results in increased attraction and low resistance'. Indeed, the use of coercive power in channels of distribution often results in conflict, whereas the use of rewards normally does not. This is especially the case in franchised channels, in which the threat that the franchisee's contract will be terminated or not renewed might significantly endanger the very survival of the franchisee's business and the livelihood of the franchisee's family (since a large portion of franchised businesses are family-run businesses).

Third, even though there is no requirement in science that one discipline must be, in B&H's terms, 'in line' with another, marketing has historically been a discipline that has been open to potential contributions from other disciplines. B&H provide no evidence that channels researchers have been 'reluctant' to incorporate recent developments in social psychology into marketing, nor have they shown that such developments would, indeed, *advance* channels of distribution scholarship. Perhaps, they would; perhaps they would not. (Perhaps, also, there is an opportunity for additional research here?)

Fourth, the extensive discussions in the channels literature concerning the power-base approach that B&H seem to regard as being a major deficiency (e.g., examining for cultural differences) may also be interpreted as a productive elaborating or extending of the power-base approach. Such discussions, including the commentary of B&H, are a sign of a healthy academic discipline. Note, however, that the starting point for all the power-base discussions is the approach advocated in an article published in a book over half a century ago. Five-plus decades after publication, there are few authors' works that are still topics of vigorous discussion – *very* few.

On using an interpersonal theory of power

B&H point out that French and Raven (1959) were clear that their framework focused on the power of an individual to influence the decisions of another person, and B&H cite Hardin (1993, p. 511) as arguing that marketing has an unfortunate tendency to 'use the easy analogy from individual to institutional issues that abstract from institutional constraints'. Although B&H acknowledge that Beier and Stern (1969) presented arguments for using an interpersonal theory of power in an interorganisational context, B&H argue that the use in this context is 'questionable' (p. 1151) and conclude that 'the assumption that sources of power applicable to interpersonal situations can be transferred to inter-organizational contexts remains largely unnoticed and inadequately argued' (p. 1151).

In response, I concur that at times interpersonal theories may be inappropriately used in an organisational context. However, I argue that no such misuse has occurred with the power-base approach to channels. First, in most franchised businesses the organisation is both a legal entity (i.e., the firm) and an individual (i.e., the owner). Because franchisees (as owners) are people, an interpersonal theory is appropriate. Second, though franchisors are often (but not always) large corporations, corporate policies are carried out by their representatives. In the McDonald's example above, note that it was franchisees who informed us that McDonald's *representatives* had used several different arguments to attempt to get them to construct new buildings. Third, Raven's (1993) review of over three decades of power-base works (across several disciplines) favourably cites two channels of distribution articles, that is, Gaski (1986) and Hunt and Nevin (1974). Tellingly, he makes no mention that these extensions of the power-base approach are possibly inappropriate.

Fourth, firms obviously attempt to influence the policies and decisions of other firms, and they obviously use varied arguments to attempt to get acquiescence. It is difficult to take seriously the view that firms do *not* at times attempt to influence other firms by threatening to punish them, or offering to reward them, or claiming that they have a legitimate right to expect acquiescence, or maintaining that the requesting firm has special expertise in an area, or admonishing the target of the influence attempt that they should be 'good partners' in some joint endeavour. French and Raven's (1959) power-bases may not properly categorise *all* interorganisational attempts to exert influence, but it surely represents a good place to start.

On misreporting others' works

B&H (p. 1151) charge that some of the citations in power-base studies suffer from a 'misreporting' of the original French and Raven (1959) article. Readers should note that academic journal articles use citations for several purposes, including (1) acknowledging the work of others (i.e., giving credit where credit is due), (2) providing authority for making a knowledge claim, (3) positioning an article in a broader stream of literature and (4) pointing scholars towards additional works on the topic that might be of value (i.e., providing a bibliography). An intentional, material misrepresentation of cited works is a form of academic misconduct because it violates what Merton (1968, p. 606) calls the 'ethos of science'. For example, Hirschman (1989, p. 209) made an allegation of academic misconduct when she maintained that some participants in marketing's philosophy of science debates had engaged in 'purposeful distortions' of others' works.

B&H provide four examples of what they allege is a 'misreporting' of French and Raven (1959). First, they state: 'Hunt and Nevin provide an example of such a 'misreporting' of French and Raven in their comment that "Some authorities, however, have suggested that the noncoercive sources of power appear to be better alternatives for enhancing the satisfaction of the weak channel members (Beier & Stern, 1969; French & Raven, 1959)"' (p. 187). B&H's claim of 'misreporting' follows next: 'yet French and Raven's paper does not suggest that the five sources of power that they propose should be grouped as coercive and noncoercive' (p. 1151).

In response, Hunt and Nevin (1974) clearly point out that *we* (not French and Raven) are the authors of the 'coercive/noncoercive' power dichotomy on the very same page that B&H find the sentence that allegedly 'misreports'. Our purpose in citing French and Raven (1959) at the end of the sentence in question was both to give credit where credit was due and provide authority for our second hypothesis. Granted, we could have inserted 'what we label' before the word 'noncoercive'. However, given that we had already claimed authorship of the 'noncoercive' categorisation, there was no good reason to do so. Using the word 'noncoercive' was efficient writing, not a case of 'misreporting'.

B&H's second and third examples of 'misreporting' are the Hingley (2005) and Quinn and Doherty (2000) articles that seem to imply in their citations that the original French and Raven (1959) article was *developed* in an 'organisational context' (p. 1152) or in the 'marketing channels literature' (p. 1152). In response, it is true that the French and Raven (1959) article was in the social psychology literature. Furthermore, the examples French and Raven (1959) used to illustrate their theory were often of the supervisor/employee relationship within organisations. However, my reading of the two cases of alleged misreporting in Hingley (2005) and Quinn and Doherty (2000) leads me to conclude that they are, at worst, innocuous inaccuracies, rather than material misrepresentations that would imply academic misconduct. Indeed, even if readers of the two articles interpret them as implying that the original French and Raven (1959) article was developed in an *interorganisational* (rather than an interpersonal, *intraorgranisational*) context, the inaccuracy seems accidental, not intentional. Furthermore, no academic damage occurs to French and Raven (personally), or to their scholarship, or to marketing's channels of distribution scholarship. That is, I argue, the inaccuracies are not *material*.

B&H's fourth example of 'misreporting' is fascinating and worth reporting in detail:

> Misreporting of the source text even occurs when the original authors discuss their work. Raven has written a number of papers which explore the influence of French and Raven's paper. Unfortunately, by stating that the paper identified six sources, his comments contradict the fact that the original French and Raven paper identified only five sources of power. For example, in one paper, he refers to 'The original six bases of power' (Raven, 1992, p. 239), and in a later paper says 'The original French and Raven (1959) bases of power model posited six bases of power: reward, coercion, legitimate, expert, referent and information (Raven, 1965)' (Raven, 1993, p. 257). Such statements are difficult to understand and may mislead readers who do not have access to the original paper.
>
> (Blois & Hopkinson, 2013, p. 1152)

In response, Raven's statements are not at all 'difficult to understand'. Nor, I suggest, is it the case that they 'may mislead readers'. One of the articles B&H quote is Raven (1993), which is a discussion of the history of the development of the original article by French and Raven (1959). The Raven (1993, p. 227) article was 'based on a presentation made at the Centennial Convention of the American Psychological Association... [and was] in honor of John R. P. French on receiving the Kurt Lewin

Award from the Society for the Psychological Study of Social Issues'. In the section titled 'Conceptualizing the Bases of Power', Raven (1993, p. 232) points out that he and French 'could readily think of examples in which one person, an influencing agent, could convince another with clear logic, argument, or information'. French and Raven decided to call that 'informational influence', even though Deutsch and Gerard (1955) had previously used the term 'informational power' to refer to the same phenomenon.

Consequently, Raven (1993) recalls, the original article stated: 'we distinguish between expert power based on the credibility of O and informational influence which is based on characteristics of the stimulus such as the logic of the argument or the "self-evident facts"' (French & Raven, 1959, p. 164). However, Raven (1993, p. 232) further recalls (with a keen sense of humour) the following: 'I also felt that the person who had the means to exert such influence could be said to have informational *power* – though I was not able to convince French that the term "power" was appropriate. (Apparently, my own informational power was insufficient in this case.)'

The preceding shows why, contra-B&H, it is *easy* to understand why Raven in his later works would often refer to 'six' bases of power, instead of 'five': he always believed that the original article should have enumerated six bases, and in his subsequent works he had the opportunity to show how and why he disagreed with his co-author. Some scholars might agree with French (that informational influence is best considered to be a part of expert power) and others might agree with Raven (that informational influence is best considered as a source of power separate from perceived expertise). In any case, Raven should not be charged with 'misreporting' his own work. Rather, in the work that B&H cite, I believe Raven should be thanked for providing a further elaboration and extension of his work with French. In general, co-authored articles usually require compromises because co-authors seldom agree on everything. Indeed, because published articles must also satisfy the requirements of the reviewers and editor, academic articles often look like they were written by a committee.

In retrospect, I believe that French and Raven's (1959) original article would have been even better if Raven's position on informational power had prevailed. That is, the possession of timely and useful information by a person or firm may be a source of power even though the person or firm possessing the information may not be viewed as being an *expert*. In short, I believe that Raven's 'six' is superior to French's 'five'.

On French and Raven's original arguments

B&H maintain that the power-base approach is deficient because their review of French and Raven (1959) original article finds it to be 'much less rigorous in its arguments than would be expected' (p. 1157), with 'uneven support' (p. 1153) from the literature cited in the article. B&H provide several pieces of evidence to support their claim. First, 'The paper does not justify the claim that these five bases were perceived to be 'especially common and important', nor does it explain from where these five bases were derived' (p. 1153). Second, B&H count the number of references French and Raven (1959) cite in the paragraphs that discuss each of the power bases and report the tallies in their Table 2. B&H then find it 'surprising' that, for example, 'at one extreme, the subsection of their paper which discusses reward power contains no references at all, while the discussion of referent power contains 14 references' (p. 1153). Third, B&H evaluate the content of the references that French and Raven (1959) cite for 'reward and coercive power' and find that the 'five references provide little support for French and Raven's claim that coercive power constitutes one of the bases of power' (p. 1155). Fourth, B&H evaluate French and Raven (1959) arguments in support of 'legitimate power' and claim that French and Raven made a serious 'error'. B&H (p. 1155) state:

> French and Raven define the legitimate power of O over P 'as that power which stems from internalized values in P which dictate that O has a legitimate right to influence P and that P has an obligation to accept this influence' (p. 159). This definition makes the same error as Weber (1968) for whom legitimate power is that power which people regard as legitimate.

On the use of citations in academic works

Before responding to B&H's critique of the citations in the original French and Raven (1959) article, it is useful to keep in mind that, as mentioned earlier, academic articles include citations for, among other purposes, (1) acknowledgement, (2) authority, (3) positioning and (4) pointing readers to other literature.[3] Furthermore, the norms guiding what constitutes acceptable or proper citation practices differ across (1) different types of disciplines (e.g., the natural sciences, social sciences and humanities), (2) somewhat similar types of disciplines (e.g., psychology, sociology, management and marketing), (3) research traditions *within* disciplines (e.g., consumer behaviour, marketing models, channels of distribution and marketing strategy) and (4) different journals within a discipline that share similar research traditions (e.g., the *Journal of Marketing*, the *European Journal of Marketing* and the *Journal of Marketing Management*). Moreover, a discipline's norms for acceptable or appropriate citation evolve significantly through time (e.g., the number of references per journal article in marketing has risen through time).

Consequently, all authors, when writing an article, must make judgements as to which claims in an article represent the 'common knowledge' of their disciplines (and, therefore, require no citation), which claims have been made by others (and require acknowledgement), which claims may be challenged (and require authority citations), and which claims represent the article's original contributions to the discipline (and must be argued for anew). For example, in the first paragraph of this article, 'Clark (1922)' is an acknowledgement citation, whereas 'Shaw and Jones (2005)' is an authority cite. In footnote 1 of this article, the 'Shaw (1915)' cite is a bibliographic cite. Also, as an example of how citation norms evolve over time, the Shaw (1912) article, which is 62 pages long, contains zero citations. Today, it is unlikely that one would find an article of *any* length with zero citations in the *Quarterly Journal of Economics*.

Finally, recall that citation norms evolve over time. One should be careful about assuming that the twenty-first century citation norms of marketing (or psychology,

[3]Citations are also included for nonscholarly reasons. For example, Wilhite and Fong (2012) report that many journal editors coerce authors to add superfluous citations to their journals to increase their 'impact factors'.

for that matter) are directly applicable to an article that appeared in a book on social psychology published half a century ago. Such early works are certainly not immune from criticism; science has no 'statute of limitations'. However, fairness dictates that the maxim in philosophy often referred to as 'charity of interpretation' is also warranted.

On French and Raven's citations

With the preceding discussion of citations in mind, consider B&H's evidence in support of their charge that French and Raven (1959) was not 'rigorous' in its arguments and had 'uneven support' from the literature. First, B&H state that there is a lack of justification for French and Raven (1959) claim that the five bases were 'especially common and important'. In response, French and Raven (1959) is a conceptual/theoretical article in a book on 'social power' in social psychology in the 1950s. An alternative, more charitable, interpretation of their article is that they viewed the substance of each of the five bases of power as having been stated or assumed so frequently that the five had become common knowledge in the psychology literature. Hence, they believed no citations were required. Also, most of their article focuses on the *differences* among the five bases, as to, for example their 'range' and consequences. Also, as do many conceptual/theoretical articles today, they conclude their article with several specific hypotheses for further testing. French and Raven (1959) is an article of *proposals*, not an article of *findings*: they propose five bases of power and provide hypotheses for empirical research.

Second, B&H are surprised that the section on reward power contains no references, while the section on referent power has 14 references.[4] In response, consider reward power, that is, people and firms can influence other people and firms by offering to reward them for acquiescence. Not only is this common knowledge in social psychology, it is also common-sense knowledge as well. An alternative interpretation of French and Raven (1959) is that they saw no need for citations to support reward power. Now consider referent power, which has 14 references and 'has its basis in the identification of P with O, ... a feeling of oneness of P with O, or a desire for such identity' (French & Raven, 1959, p. 161). An alternative interpretation of their article suggests that they believed that what they called referent power required them to, in my terms, 'argue anew', with further elaboration, significant discussion and several references. These arguments allowed them 'to distinguish between referent power and other types of power which might be operative at the same time' (French & Raven, 1959, p. 162). Choosing well the issues that warrant arguing anew is how *good* conceptual/theoretical articles were written in the 1950s; it is also how good conceptual/theoretical articles *should* be written today. As to reward versus referent power, I argue that French and Raven (1959) chose well: reward power was common knowledge; referent power needed to be argued.

Third, B&H charge that French and Raven (1959) lacks rigor because B&H's reading of the original sources cited finds that 'the five references provide little

[4]In B&H's Table 1, 'referent power' has 13 references, not 14, as in the text. My count is that there are 13 different references, which are cited a total of 16 times. I hasten to add that the '13' versus '14' is an innocuous inaccuracy by B&H.

support for French and Raven's claim that coercive power constitutes one of the bases of power' (p. 1155). For example, B&H make much of their finding that two of the five sources cited do not use French and Raven (1959) label of 'coercive power' to describe their work, and one source uses the word 'power' only twice. Also, B&H make much of the fact that two of the five citations are 'in preparation' articles (what would now be referred to as 'working paper' cites). B&H complain that these two cites 'are of no value' because 'no suggestion is given of when or where these two papers might be published' (pp. 1154–1155).

In response, I argue that the key consideration ought not to be whether the three cites use French and Raven (1959) terminology of 'coercive power' to describe their works. Rather, it is whether the three sources provide examples of, or are consistent with, or extend, or contrast with, the *substance* of French and Raven's (1959) proposals concerning the implications of the use of coercive power. Therefore, B&H use an inappropriate criterion for evaluation (i.e., whether the source used the label 'coercive power'). Furthermore, working paper cites are generally not considered to be for the purpose of authority. Rather, working papers are more often used as bibliographic cites that point readers to future works on the same or similar topic that the authors propose might be of interest. Finally, even today, no author would (or should) predict 'when or where' future works may be published (articles 'in press', of course, are exceptions). Thus, the two 'in preparation' articles may also be interpreted as bibliographic cites.

To conclude this discussion of citations, readers should keep in mind that the original French and Raven (1959) article was a conceptual/theoretical article. Its purpose was to offer a classificational schema for the sources of power and to propose differences among their five bases of power as to their consequences. Rather than evaluating the rigour of a classificational schema on the basis of counting the number of citations in the original article, I suggest that the more appropriate 'count' is the number of other scholarly works that subsequently cite the original article as an important part of their own scholarly efforts. On this 'count', the fact that French and Raven (1959) original article has over 7000 Google Scholar citations (as of this writing) speaks for itself.

On French and Raven and the 'is/ought' fallacy

As a fifth major criticism, B&H claim that French and Raven's (1959) definition of legitimate power 'makes the same error as Weber (1968) for whom legitimate power is that power which people regard as legitimate' (p. 1155). In response, readers should recognise that, in essence, B&H are accusing French and Raven as committing, in philosophy of science terms, the 'is/ought' or 'positive/normative' fallacy. David Hume (1711–1776) is generally given credit for being the first philosopher to point out that statements concerning the verb 'is' are different in kind from statements containing the verb 'ought'. In particular, Hume observed that no set of statements containing only descriptive terms and no copula except 'is' can logically yield a conclusion containing an 'ought'.

The positive/normative dichotomy is the version of Hume's 'is/ought' dichotomy discussed in Keynes's (1891) classic work, *The Scope and Method of Political Economy*. There, he defined a positive science as 'a body of systematized knowledge concerning what is' and a normative science as 'a body of systematized knowledge discussing criteria of what ought to be' (pp. 34–35). Keynes pointed out that 'confusion between them is common and has been the source of many mischievous errors' (p. 46). The *Dictionary of Philosophy* (Angelas, 1981, p. 138) clarifies the view of philosophy about this 'is/ought' dichotomy:

> **Is/ought Dichotomy.** Also, *fact/value dichotomy*. Statements containing the verb *is* are related to descriptive or factual claims and are of a different order from those containing the verb *ought* (*should*), which are related to judgments, evaluations, or commands. It is impossible (logically, formally, conceptually) to derive an 'ought' (or 'should') statement from an 'is' (factual) statement, a normative statement from a statement of facts; it is impossible to have a valid deductive argument in which the premises state descriptions and the conclusion states prescriptions or imperatives.

Did French and Raven (1959) commit the 'is/ought' fallacy? B&H (p. 1155) quote the following statement: 'Legitimate power of O/P is here defined as that power which stems from internalized values in P which dictate that O has a legitimate right to influence P and that P has an obligation to accept the influence' (French & Raven, 1959, p. 159). B&H then interpret the preceding definition as committing the 'error' of claiming that 'legitimate power is that power which people regard as legitimate' (p. 1155). That is, for B&H, French and Raven (1959) confuse 'what is' (i.e., P's belief that O has a legitimate right to expect acquiescence) with 'what ought to be' (i.e., whether O's influence attempt is legitimate in the sense of appropriate, proper, or well justified).

I argue that French and Raven (1959) do not commit the 'error' that B&H allege. First, readers should note that French and Raven (1959, p. 156) discuss early in their article how they view all five of their bases of social power, and they state that legitimate power is 'based on the perception by P that O has a legitimate right to prescribe behavior for him'. Indeed, the word 'perception' appears in all five of their bases of social power. French and Raven's (1959) article is a work of explanatory (positive) social science; they are arguing that particular perceptions of 'P' explain the behaviours of 'P'. French and Raven's (1959) article is not a work of normative social science; it is not a work that evaluates whether P's perceptions are well justified. Contra-B&H, at no point in French and Raven's (1959) article do they ever claim that P's *perception* that O's influence attempt is legitimate *makes* (i.e., properly justifies) O's influence attempt as being legitimate (in a normative sense). French and Raven's (1959) article was a work of positive social science; it should be evaluated on that basis. The 'error', if there is one, is B&H's.

Conclusion

Commentaries play a valuable role in maintaining the intellectual health of a discipline. B&H provide a thoughtful critique of the use of French and Raven (1959) bases of power approach in the channels of distribution literature. Contrasted with the critique of B&H, this reply argues that (1) the quantitative evidence (i.e., the measures and empirical results of studies using the French and Raven (1959) power-base approach) is probably comparable to other social science research programmes, (2) the qualitative evidence favouring the use of the power-base approach in channels of distribution is substantial, (3) the coercive/noncoercive categorisation of the French and Raven (1959) sources of power in the channels of distribution literature is appropriate and valuable because the use of coercive sources often leads to conflict, whereas the use of noncoercive sources does not, (4) though the power-base approach was first developed in an intraorganisational context, its use in the channels of distribution area, an interorganisational context, is fully warranted, (5) the (implied) allegations of academic misconduct involving citations that 'misreport' the works of others turn out to be a misreading of the purposes of the citations or innocuous inaccuracies that are not material in nature, (6) the charge that Raven (1993) 'misreports' his work with John R. P. French confuses 'misreporting' with Raven's (1993) efforts to further elaborate and extend his and French's original article, (7) the claim that the arguments in the original French and Raven (1959) article are not 'rigorous' results from a misreading of the purpose of the French and Raven (1959) article, a questionable interpretation of the purpose of the citations in the original article, and an ignoring of the citation 'count' that is most significant, and (8) the charge that French and Raven (1959) make the 'error' of committing the 'is/ought' fallacy with respect to legitimate power is devoid of merit.

In conclusion, the French and Raven (1959) power-base approach to the study of power relationships in channels of distribution, initiated by Beier and Stern (1969) and extended by scores of others, has stood the test of time. This reply argues that the power-base approach also withstands well the critique of B&H. Indeed, the power-base approach, rather than misguiding channels research, has guided it well. Perhaps other approaches to understanding power in channels of distribution, such as those recommended by B&H, will also contribute to understanding power relationships, but that remains to be seen. Hopefully, marketing academics will accord other approaches both a critical and charitable interpretation.

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About the author

Shelby D. Hunt is the Jerry S. Rawls and P.W. Horn Professor of Marketing at Texas Tech University, Lubbock, Texas. A past editor of the *Journal of Marketing* (1985–87), he is the author of numerous books, including *Marketing Theory: Foundations, Controversy, Strategy, Resource-Advantage Theory* (M.E. Sharpe, 2010), *Controversy in Marketing Theory: For Reason, Realism, Truth, and Objectivity* (M.E. Sharpe, 2003) and *A General Theory of Competition: Resources, Competences, Productivity, Economic Growth* (Sage, 2000). One of the 250 most frequently cited researchers in economics and business (Thompson-ISI), he has written numerous articles on competitive theory, strategy, macromarketing, ethics, relationship marketing, channels of distribution, philosophy of science, and marketing theory. Three of his *Journal of Marketing* articles won the Harold H. Maynard Award for the best article on marketing theory. One of these three articles, 'Comparative advantage theory of competition', also won the 2004 Sheth Foundation/*Journal of Marketing* award for its long-term contributions to the field of marketing. His 1985 *Journal of Business Research* article with Lawrence B. Chonko received the 2000 Elsevier Science Exceptional Quality and High Scholarly Impact award. His 1989 article, 'Reification and realism in marketing: in defense of reason', won the *Journal of Macromarketing* Charles C. Slater Award. His 1994 *Journal of Marketing* article with Robert M. Morgan, 'The commitment-trust theory of relationship marketing', was the most highly cited article in economics and business in the 1993–2003 decade (Thomson-ISI). His *Journal of the Academy of Marketing Science* (*JAMS*) article entitled 'Sustainable marketing, equity, and economic growth: a resource-advantage, economic freedom approach' won the Sheth Foundation Award for the Best Article in *JAMS* in 2011. For his contributions to theory and science in marketing, he received the 1986 Paul D. Converse Award from the American Marketing Association, the 1987 Outstanding Marketing Educator Award from the Academy of Marketing Science, the 1992 American Marketing Association/Richard D. Irwin Distinguished Marketing Educator Award, the 2002 Society for Marketing Advances/Elsevier Science Distinguished Scholar Award and the 2010 Marketing Management Association Innovative Marketing Award. In 2011, Sage Publications published the 10-volume set, *Legends in Marketing: Shelby D. Hunt*. The volumes include 132 of Hunt's articles, 41 commentaries on Hunt's work by distinguished scholars and interviews of Hunt by each volume's editor.

Corresponding author: Shelby D. Hunt, Department of Marketing, Jerry S. Rawls College of Business Administration, Texas Tech University, Lubbock, TX 79409-2101, USA.

T (806) 834-5233

E shelby.hunt@ttu.edu