

## **Theoretical perspectives in purchasing and supply chain management: an analysis of the literature**

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### **Abstract**

The research presented in this paper is work-in-progress and aims to investigate to what extent purchasing and supply chain management (SCM), as a relatively new area of academic enquiry, is ready or able to join the select group of modern scientific disciplines. The analysis indicates that the discipline lacks coherence and exhibits significant and increasingly interdisciplinary breath and is some way off becoming a natural science. Furthermore, it is argued that SCM research has diverse agenda's and therefore it is unlikely that one dominant paradigm will emerge.

**Key words:** purchasing and supply chain management, theory, literature review

### **Introduction**

Purchasing and Supply Chain Management (SCM) started to develop as an area of serious academic enquiry in the early 1990s. Initially, students of the subject drew heavily on the working experiences of Japanese manufacturers for the development of their approach (e.g. Womack and Jones, 1995; Lamming, 1993). The initial dominance of lean supply was clear to see, as was its impact on practitioners. However, by the mid-1990's lean supply had started to face a number of challenges, both from sympathetic critics who saw the model as being broadly correct, but incomplete (e.g. advocates of agile); as well as from less sympathetic critics who regarded its pre-occupation with matters of waste as misdirected (see notably, the work of Cox, 1997). Since that time advocates of what might be called Integrated Supply Chain Management (ISCM) have found themselves competing with writers

from an array of other intellectual traditions, including: economics, in the form of Principal Agent Theory (PAT) and Transaction Cost Economics (TCE); business strategy, in the form of Resource Based View (RBV), the Dynamic Capabilities approach (DCA) and Industrial Organisational (IO); economic sociology in the form of Resource Dependence Theory (RDT); as well as mainstream business management (e.g. IMP).

For some commentators this is seen as a sign of a healthy discipline (Feyerabend, 1975). According to such advocates of intellectual pluralism, no one party has a monopoly on the truth and the vigorous competition of ideas seems more likely to promote useful lines of enquiry than to do damage to a discipline. Others, however, are not so sure. They argue that the proliferation of theory and method leads to a ‘weed-patch, rather than a well tended garden’ (Pfeffer, 1993) (see also Harland *et al*, 2006, for a discussion of Pfeffer’s point). Competition between ideas is only to be welcomed if at some point the areas of dispute are resolved. Fortunately for Feyerabend’s critics, this is usually the case. Plurality is seen as a feature of an immature discipline, and as such only a temporary phase, before the discipline transitions into what Kuhn calls a ‘normal science’ (1970).

The question posed by this paper is to what extent can purchasing and SCM be said to be ready or indeed able, to join the select group of modern scientific disciplines? When Harland *et al* addressed this same question in 2006, they concluded that SCM did not even rise to the standard that would allow it to be categorised as a discipline (let alone a scientific one). In this paper the authors ask what, if anything, has changed?

The structure of this paper is as follows. Firstly, it briefly describes the principal characteristics of a modern scientific discipline. Secondly, it outlines some of the potential competitors for intellectual dominance. In particular, it focuses on the main theoretical contributions from economics, business strategy, economic sociology, and general business management. Thirdly, it looks at the relative progress made by each of these approaches (and as an ancillary measure, looks at the issue of method). Finally, insofar as it supports the argument made by Harland and her co-authors, it questions whether SCM ever has the potential to establish itself as a normal science.

## **A review of the scientific development of disciplines**

Borrowing from Fabian (2000), Harland *et al* (2006), in their original paper, identified three criteria that could be used to judge whether an academic field could be said to have risen to the standard of a discipline. These were coherence, breadth (and depth) of knowledge, and quality. Regarding coherence, Fabian distinguished between unified disciplines (i.e. disciplines with a single paradigm), segregated disciplines (i.e. disciplines with no paradigms), and integrated disciplines (i.e. disciplines that had started to come together but in which no one approach was clearly dominant). For Fabian, a unified (or mature) discipline was one where researchers shared a common understanding of the world that they sought to describe. By

contrast, a segregated discipline was a discipline that in Harland's terms struggled to even merit the description.

According to Fabian's second criterion, a discipline could be said to be broad insofar as it borrowed widely from related disciplines in an attempt gain greater insight into its own. In the case of SCM, the introduction of TCE, PAT and RDT could be taken as indicators of breadth. In their review, Harland *et al* drew no conclusions about the desirability of breadth in a discipline (p.734), other than noting that it might speed up the process of learning, in that interdisciplinary enquiry saved the student from having to reinvent the wheel. However, because Harland and her co-authors were unable to decide whether breadth was, on balance, a good thing, it played no further part in their analysis. The authors of this paper, however, would argue that the impact of breadth on disciplinary development depends, in part, upon whether the approaches being borrowed are ontologically and epistemologically commensurable. Where they are not, and the relative merits of the different approaches cannot easily be compared, this will slow the process of convergence and thus development.

Fabian's final criterion is the *quality* of published research. Like his second point, this too impacts directly upon disciplinary coherence. For quality, Fabian uses methodological rigour as a proxy. It is commonplace when discussing method to distinguish between induction (or empiricism) and deduction (philosophical enquiry). The former relies on observation for the development of theory; the latter on logical inference (usually operating from first principles) (Stoker, 1995, Saunders *et al*, 2007). Much of the historic development of natural sciences like biology rested upon induction. By contrast, neo-classical economics was, and remains to this day, a largely deductive subject. Modern scientific method, often taken to be the gold standard of disciplinary enquiry, represents a fusion of induction and deduction. According to the hypothetico-deductive process, progress in a discipline is achieved through: the development of clear testable (i.e. falsifiable) hypothesis; the testing of these hypotheses; the endorsement, refinement or rejection of hypotheses based upon the findings in the data; and, the subsequent development of clear concepts and theories (Hollis, 1997, pp.38-44).

As far as the coherent development of a discipline is concerned, the most important step is likely to be the first - that the central tenets of any approach or theory be falsifiable. Where this is not the case, the competing theory is likely to exist in a state of limbo. It cannot be progressed since there will be no direct evidence to support it. Nor, for the same reason, can it be discarded. The implication of this is clear. *Ceteris paribus*, disciplines that do not make use of scientific method (or some other method for assessing theoretical merit) remain cluttered with competitors and are less likely to pass Fabian's test of coherence. The fields of politics and sociology are cases in point. The pervasive influence of Marxism in both disciplines meant that for a long time they remained hopelessly mired in introspective debate.

In the short term such fragmentation and friction is not necessarily a problem. Indeed, it is often taken to be a natural part of a discipline's development. In his examination of the emergence of scientific fields, Thomas Kuhn (1970) talked in terms of the phased development of a discipline. Phase 1, or what he called the *pre-paradigm phase*, occurred in

a discipline's infancy. It was characterised by the kinds of intellectual competition described above. In the pre-paradigm phase the emergent discipline was often messy. There was no clear focus of enquiry, or agreement on the key problems that needed to be solved. Concepts were few and used loosely. Theory, or what passed for theory, proliferated, but because the methods used to establish the 'truth' were similarly contested, intellectual progress was slow and often disjointedly incremental.

Gradually over time, however, the nascent discipline would start to come together and progress could be made. Convergence occurred because the relative merits of a particular approach became self-evident. The superiority of the approach lay in its greater explanatory power. Along with the emerging dominance of a single approach (or *paradigm*), came general agreement on three further things. Firstly, concepts – what they meant and how they were to be used. Secondly, the key agendas in the discipline and the outstanding problems that remained to be resolved? Thirdly, agreement on how these outstanding problems were to be addressed (both theoretically and methodologically). It was this second phase that Kuhn referred to as *normal science*.

While the emergence of orthodoxies was a common feature in many disciplines, Kuhn advocated caution. Most disciplines, he argued (even mature ones), continued to contain 'mysteries' even after a dominant paradigm had been established. The dominant paradigm could either shed little light on these problem areas; or, worse still, was at odds with the evidence. Faced with such a dilemma, supporters of the paradigm would continue to address the problem using the methods established by the normal science. However, Kuhn argued that if these strategies proved to be unsuccessful, they might prove in time to call into question the ontological and epistemological basis of the paradigm. Furthermore, the hunt for a resolution to the anomalies might well lead to the development of rival, incommensurable but superior approach, which had the potential to overturn the conventional wisdom. Kuhn described this as a third, or *revolutionary*, phase. He cited physics as an example of a discipline where this happened.

### **Disciplinary convergence in the field of supply chain management**

Not surprisingly, given what has been said, describing even a relatively immature field like SCM is likely to prove problematic. Although a young discipline, SCM is also a rich one. At the time when Harland *et al* undertook their analysis of it, it could clearly be located in Fabian's segregated camp. In addition, to generating its own approaches, most notably in the form of lean, agile and legible supply, it had attracted contributions from economics (PAT and TCE), business strategy (IO, RBV, DCA), economic sociology (RDT), as well as mainstream business management (IMP). Even, where these different approaches talked the same language, they were often focused on different problems. For the purposes of intellectual neatness this paper provides a taxonomy of these different approaches, which must necessarily be incomplete. This is because the variety and incommensurability of even those

approaches still extant is such as to make it all but impossible to categorise them on the basis of a few simple distinctions. What is depicted in table 1 is, therefore, somewhat crude.

**Table 1: Key theories within the discipline**

<b>Theory</b>	<b>Focus</b>	<b>Description</b>
ISCM	Efficiency	Inter-organisational cooperation for the purpose of enhanced customer satisfaction and operational efficiency. Looked at the operational steps required to produce the efficient throughput of product
TCE	Efficiency	Efficiency as an incentive problem. Hold-up is the principal focus of TCE. Safeguards are centred on governance
PAT	Efficiency	Efficiency as an incentive problem. Adverse selection and moral hazard are the efficiency problems analysed by PAT. Safeguards are centred on the contract
IO	Power	Interested in the relationship between power and surplus value, but also considers the welfare implications to third parties. The differences between the relative dependence of contracting parties, confers commercial advantage of the dominant actor. However, this is not to be welcome
RBV	Power	Interested in the relationship between power and competitiveness. Power differentials provide the basis for a firm's strategy. However, these advantages originate not in the transaction but in the internal processes of the firm.
DCA	Power	Interested in the relationship between power and competitiveness. However, these advantages originate not in the transaction but in the internal processes of the firm.
RDT	Power	Interested in the relationship between power and surplus value. Similar to TCE in that it puts the transaction (or exchange process), at the centre of its analysis. It holds that differences between the relative dependence of contracting parties, confers commercial advantage of the dominant actor. Unlike IO this is welcome and forms basis of its competitive advantage

Not only has SCM been a field which, according to Fabian's first test, has lacked coherence; it has also (because of its interdisciplinary nature), been a field that has exhibited breadth and depth of knowledge. However, this has almost certainly contributed to its lack of coherence. The ontology's of ISCM, TCE and RDT are quite distinct from each other. Even in the case of TCE and PAT where there are some obvious similarities; there remain important differences. PAT has its roots clearly located in neo-classical theory. The progenitors of TCE are less clear. Williamson, himself, clearly locates TCE within the economic orthodoxy, positioning it as an extension of neo-classical theory. However, TCE clearly shares important insights with Herbert Simon's Behaviouralism. Indeed, Simon and Williamson were once colleagues at Carnegie-Mellon and Williamson has acknowledged his intellectual debt to Simon's notion of bounded rationality, which sits uncomfortably with the standard model.

Finally, there is the issue of method (or quality). With the exception of IO, neo-classical contributions to the theory of the firm remain largely exercises in deductive thought. There is little or no hard empirical data to support some of its core arguments. Indeed, in the past neo-classical economics has seemed to have made a point of making internal consistency and formal expression the principal tests of its contribution. At the other extreme, ISCM was in its early days an almost wholly inductive approach. Womack and Jones, the authors of the *Machine that Changed the World*, sought to generalise from the experiences of Toyota for

their insights. Similarly, RDT has exhibited a preference for the case method; as did much of the early contributions in the area of the RBV and the DCA.

### Methodological approach to literature review

A review panel of five academics was formed to define the scope of the study, the data to be collected and the data collection process. Three journals were chosen in the field of ‘purchasing and supply chain management’ as identified in the Association of Business Schools journal list (Harvey, Morris, and Kelly, 2009) to find evidence to answer the research question. These were, Supply Chain Management: an International Journal (3 star), Journal of Purchasing and Supply Management (2 star), and Journal of Supply Chain Management (1star).

The review panel, along with four research assistants, were to review papers from every journal volume and issue from 1980 onwards or from when the journal commenced. As shown in table 2, rich data were collected for each paper. Researchers coded the papers independently. However, to ensure inter-rater reliability researchers went through a joint period of ‘learning’. The data collection process included an initial development phase, whereby samples of coded papers were swapped and re-coded by members of the research team to see if there was agreement. Any anomalies or inconsistencies were discussed and resolved. To further ensure consistency of data collection the lead author conducted a final ‘sense check’, taking a sample across the journal analysis.

**Table 2: Example of data analysis table**

Lead Author Initials	Year	V	I	Type of paper	Method	Topic	Lit rev	Ref list	Model	Dom theory	If 'other', what theory?	Country	Sector e.g. Automotive etc	Public / Private	Replication study?
JB	1994	1	1	1. Conceptual	4. None	TQM	5. 2+ pages	4. 1 to 2 pages	1. Yes	14. Other	ISCM	259. None	38. None	4. None	2. No
DM	1994	1	1	1. Conceptual	4. None	Purchasing	2. 0-1/2 pages	2. 0-1/2 pages	2. No	14. Other	ISCM	259. None	38. None	4. None	2. No

### Findings and discussion

The reviewers have partially analysed the Journal of Purchasing and Supply Management (formerly the European Journal of Purchasing and Supply Management). Having completed one of the three proposed journal analysis, the findings and observations presented here should be viewed as work-in-progress. The headline features from the results suggest that nearly a decade-and-a-half on from its development, the field still lacks coherence (Table 2)

and exhibits significant (and increasing) interdisciplinary breadth (Table 3 again) – two points not wholly unconnected. Finally, the survey shows little evidence of establishing norms when it comes to the conducting of research (Table 4), although there is good evidence to suggest that much of the research being undertaken is rigorous and of a good standard. This would seem to support previous research (i.e. Harland et al. 2006) that the field of supply chain management is still some way from being a normal science.

**Table 3: The use of theory in the discipline**

	1994-1999			2000-2004			2005-2009			Total		
	N	%	%-NDIT	N	%	%-NDIT	N	%	%-NDIT	N	%	%-NDIT
Integrated Supply Chain Management	43	38.3	65.2	21	19.1	38.3	13	12.1	23.2	77	23.4	43.5
Transaction Cost/Agency Approaches	2	1.8	3.0	7	6.3	12.6	4	3.7	7.1	13	4.0	7.3
Industrial Organisation	0	0.0	0.0	1	0.9	1.8	0	0.0	0.0	1	0.3	0.6
Sub-Total	(45)	(40.1)	(68.2)	(29)	(26.3)	(52.6)	(17)	(15.8)	(30.3)	(91)	(27.7)	(51.4)
Resource Dependency Theory	4	3.6	6.1	1	0.9	1.8	6	5.6	10.7	11	3.3	6.2
Resource-Based/Capabilities School	1	0.9	1.5	2	1.8	3.6	2	1.9	3.6	5	1.5	2.8
Sub-Total	(5)	(4.5)	(7.6)	(3)	(2.7)	(5.4)	(8)	(7.5)	(14.3)	(16)	(4.8)	(9.0)
IMP/Network Theory	2	1.8	3.0	5	4.5	9.0	4	3.7	7.1	11	3.3	6.2
Contingency Theory	1	0.9	1.5	0	0.0	1.8	5	4.7	8.9	6	1.8	3.4
Other	3	2.7	4.5	6	5.5	11.0	6	5.6	10.8	15	4.6	8.5
Sub-Total	(6)	(5.4)	(9.0)	(11)	(10.0)	(20.0)	(15)	(14.0)	(26.8)	(32)	(9.7)	(18.1)
Intellectually Eclectic	10	8.9	15.2	12	11.0	22.0	16	15.0	28.6	38	11.6	21.5
No Distinctive Intellectual Tradition (NDIT)	46	41.1	-	55	50.0	-	51	47.7	-	152	46.2	-
Total	112	100.0	100.0	110	100.0	100.0	107	100.0	100.0	329	100.0	177

**Table 4: Research strategy in the discipline**

	1994-1999		2000-2004		2005-2009		Total	
	N	%	N	%	N	%	N	%
Conceptual	30	26.7	18	16.3	14	13.2	62	18.9
Literature Review	2	1.8	6	5.5	6	5.6	14	4.3
Simulation	4	3.6	6	5.5	1	0.9	11	3.3
Statistical Survey	34	30.4	34	30.9	39	36.5	107	32.5
Technical	0	0.0	2	1.8	0	0.0	2	0.6
Case-based	33	29.4	39	35.5	46	42.9	118	35.9
Positional	4	3.6	5	4.5	1	0.9	10	3.0
Other	5	4.5	0	0.0	0	0.0	5	1.5
Total	112	100.0	110	100.0	107	100.0	329	100.0

Taking the issue of coherence first, what was perhaps most startling was the absence of theory in much of the work. This was a pattern that did not change much over the sixteen years surveyed. In the period 1994-9 the figure for the No Distinctive Intellectual Tradition (NDIT) category was 41.6%. For the period 2000-4 it was 50.0%, while for the period 2005-9 it was 47.7%. Overall, only 53.8% of the articles could be associated with a tradition of any kind. Furthermore, even where a theoretical approach could be identified, there was no

obvious candidate for an emerging paradigm. Although, over the sixteen year period, the ISCM family of approaches proved to be the most popular of the approaches (43.5% of the articles where an approach could be identified), this was still only 23.4% of the articles overall. Furthermore, the relative popularity of the approach has been in decline over the sixteen year period. In the early days of the field it did look as if lean and agile supply techniques might provide the intellectual basis for the discipline. In the period 1994-9, where an approach could be identified, ISCM (which at that time meant lean) accounted for nearly two-thirds of the published articles in the journal (65.2%). However, since that time this figure has fallen away. In 2000-4 ISCM only accounted for 38.3% of the articles that exhibited an intellectual preference (19.1% of the articles overall). By the end of the period (2005-9), this figure had fallen to 23.2% (12.1% of all the articles overall). Such a finding is open to a number of interpretations, the most obvious being that the approach has fallen victim to the forces of intellectual Darwinism. Either the evidence that has accumulated over the last sixteen years has discredited the approach, or a more efficient alternative has been discovered. A second explanation is that the limits to the perspective have been reached. While ISCM has proven well-suited to providing answers to certain types of problems, these problems have fallen within a fairly narrow range. As the agenda of the discipline has broadened, researchers have had to cast their nets more widely for their explanations.

If this indeed is the case then it would appear that the discipline has yet to find a satisfactory alternative to ISCM. The next three most popular approaches taken together (TCE-PAT, IMP/Network Theory and RDT) only account for only 10.6% of the articles in the journal (or 19.7% of the articles with a clear intellectual preference). The relative decline of ISCM has given way to a disciplinary pluralism; it has not given rise to an intellectual challenger.

Even more worrying is the intellectual promiscuity exhibited by an increasing number of researchers in the field. That is to say, there has been a rise in the number of articles that seek to marry not just distinct but also intellectually incommensurable traditions. In the period 1994-9, 15.2% of the articles made use of theory in this way. By 2000-4 this figure had risen to 22.0% and by 2005-9 to 28.6% (more than the number of articles using the ISCM framework). Typical of this intellectual eclecticism are a number of articles that attempted to fuse the RBV-DCA from mainstream business strategy, and TCE-PAT from mainstream and New Institutional Economics (NIE). The issue of ontological commensurability must clearly be of some concern – not least because it is likely to hamper SCM's progress towards a normal science state.

This point brings us to Fabian's second criterion - the breadth and depth of knowledge in the discipline. From the results it is clear that in its early days, SCM was a relatively inward-looking discipline. The example of ISCM is a case in point. ISCM was developed by SCM specialists on the basis of their observations about Japanese manufacturing practice. The RBV-DCA, TCE-PAT, IO and IMP-Network theory played little part in the early development of the subject. This is despite the fact that all of these approaches were around in the early 1990s. For example, the study of IO is as old as the discipline of economics itself. Adam Smith's work makes repeated reference to the operation of competitive markets.

TCE has its roots in the 1930s. It emerged as a coherent approach in the 1970s and it took off in the 1980s. However, neither tradition was much discussed in the early writings on SCM.

More recently, with the possible exception of IO, all of these approaches have started to make their presence felt. However, just *how* important they are likely to be is hard to say. A number of them have the potential to be quite influential. The RBV-DCA, for example, talks directly to the subject of competitive strategy and has a number of useful things to say about the value-adding nature of relationships. TCE also has an interest in relationships; while PAT holds the prospect of deepening our understanding of the contracts. However, while it is likely that all of these approaches will increase their influence on the discipline in future years, it is unlikely that any of them will provide the basis for the development of a disciplinary paradigm. The research agenda of SCM is simply too broad for RBV-DCA to be a viable candidate. And, while between the two of them, TCE-PAT might offer the range of insight that would be required for the development of a paradigm, they are unlikely to attract sufficient disciples necessary to fulfil the role. This is because the behavioural assumptions that underpin both TCE and PAT sit uncomfortably with most of the researchers currently writing on SCM.

Finally, there is the issue of methodological norms within the discipline – or rather the lack of them (see table 4). Many researchers writing in the area eschew the use of grand theory. They prefer instead to use case material as the basis upon which to draw their lessons. We have already seen that nearly half of the articles published in this period made no use of theory. Other writers, by contrast, sit at the opposite end of the spectrum, preferring instead to derive their insights from deductive reflection. Perhaps not surprisingly, conceptual pieces accounted for a significant proportion of the published work in the early part of the period surveyed (26.7%). In recent years this number has declined. In the last period surveyed, conceptual articles accounted for a little over 13% of the articles published in the journal. Finally, there are a significant number of authors who prefer the type of scientific formalism typically found in the natural sciences. In our survey we found that the numbers of researchers who preferred these sorts of strategy were of a similar order to those preferring the case method. Of course, the existence of this methodological pluralism does not imply that the research being done is of poor quality. However, the absence of clear research norms will make it more difficult for SCM to make progress towards becoming a normal science.

## **Conclusions**

The research aim was to investigate to what extent purchasing and supply chain management can be said to be ready or indeed able to join the select group of modern scientific disciplines. Early indications are that, if we use Fabian's (2000) criterion (coherence, breadth (and depth) of knowledge and quality), then purchasing and supply chain management has some way to go as an academic field to be viewed as a discipline. In addition, as the discipline lends itself to such a broad range of agendas, for example, the internal politics of organisations or the

role of contracts, it can be argued that it is unlikely that just one disciplinary paradigm will become dominant. Indeed, it may not be appropriate to apply Kuhn's phased development of a discipline to this academic field. Finally, further analysis will be conducted on the data gathered thus far and on the remaining two journals. This analysis will aim to identify the key topics (i.e. JIT, TQM, and Partnering), the location (country) and sectoral focus of empirical studies and highlight any interesting trends.

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