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Go Forth and Replicate: A Revisionist Account of the Value of Replication in Management Studies

by Kevin Morrell, John Loan-Clarke & Adrian Wilkinson

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Abstract

This paper brings together three different accounts of the role of replication in management studies: replication as 'scientific project', replication as 'socio-cultural artefact'; replication as 'aesthetic practice'. Each of these is developed from within separate reference frames: epistemology, the sociology of science, and the philosophy of art. This offers new scope to revisit a fundamental question in management studies, namely: why is there a gap between the espoused value placed upon replication, and the actual paucity of replication studies? Each reference frame offers different insights into the nature of replication. The paper argues that by integrating all three, and by understanding the potential contribution of the philosophy of art, a more realistic account of theory development is possible; one that explains why successive calls to researchers to replicate fall on deaf ears. Despite the empirical evidence to suggest replication studies are undervalued, and the problems posed by postmodernist challenges to science, we reiterate the importance of replication.

Introduction

Imagine a party. It's 10:30 PM. Pretend every guest represents a scientific discipline. The Mathematician, the Chemist, the Physicist and the Biologist are perched in separate corners of the room, each busily doing their own thing. The Anthropologist is reflecting on how a variety of seemingly strange practices are made legitimate via ritual. The Psychologist is not actually participating, but is watching to see how much fun other people are having. The Sociologist is concerned about those who were not invited, wondering why were they excluded. The Economist is working out what the opportunity cost of coming was. Suddenly (rather late), in walks the Management

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scientist. Invited at the last minute, they weren't sure what to wear. On reflection, the red, frilly tuxedo and yellow dress-shirt was not a good choice. Someone coughs and there is an embarrassed silence. During the silence, each pauses, imagining the next party: what would they do? The Anthropologist thinks, 'I wonder if there will be a pattern to these rituals'. The Psychologist thinks, 'I must find some more interesting people to observe'. The Sociologist thinks, 'maybe I can go to another party instead, perhaps in a rougher part of town'. The Economist thinks, 'it'll be a waste of time, I can stay at home and have more fun developing a theory of parties'. The Management scholar thinks, 'next time I'll wear the red frilly tuxedo, but this time with a *blue* shirt.'

Going to someone else's party is an extended metaphor, or allegory, for replicating theory. It is a useful allegory because it connotes three inter-related spheres of action: the conventional, the social and the aesthetic. In turn, these point to three problems facing the management researcher wishing to carry out replications. Firstly, in terms of convention, there is a profound lack of consensus in management research, and this makes it difficult for management scholars to conduct research in an organised way. This is notable in the paradigm (or 'Pfefferdigm') debate (Hasard and Kelemen, 2002; Pfeffer, 1993; Van Maanen, 1995; Weick, 1999). Secondly, in terms of relations with other disciplines, there is uncertainty as to the status of management research. This arises in part from the broad challenge of carrying out social science research, but also because management research is eclectic and borrows from a number of more well established social sciences. Uncertainty also arises from a recognition of the limits of scientific methods, and scepticism about the claims of science to be value-free, progressive or objective (Allen, 2003; Potter, 1996; Thorpe, 2001). Thirdly, in terms

of aesthetics, the allegory emphasises the significance of emotional and intuitional responses to attempts by management researchers to replicate theory. In part this response can be understood in terms of the appeal of novelty, or uniqueness (Bornstein, 1991, p. 73; Mone and McKinley, 1993), but it also captures the sense that different researchers have described, where in their efforts to replicate theory they encounter, 'a vague sense of disrespect' and their studies 'are often second-class citizens' (Hendrick, 1991, p. 42). This is partly shown in the attitudes of some journal editors, who have (anonymously) commented on them as, 'dull', 'boring', stating that readers 'aren't interested in them', because they 'don't reflect cutting edge stuff' (in Neuliep and Crandall, 1991, p. 88). Setting aside the degree to which these comments are valid, it is important to recognise that these are aesthetic responses.

The problem we face as management scholars is that we were late arriving, no one told us what to wear and when we do arrive we don't know what to do. In this paper, we share our thoughts about the problematic status of replication. For some, replicating theory is a way of earning a living, or a way in which they contribute to knowledge. For us it has (unwillingly) become a diverting, therapeutic exercise. Diverting: because to date it has in all probability been less fruitful than alternative methods would have been; therapeutic: because it has awakened an interest in some fundamental questions that have a bearing on management theory. We have nodded to Popper (falsificationism), winked at Tsang and Kwan (replication) and flirted with Sayer (critical realism). Our nods, winks and flirtations have been unsuccessful, and we remain unsure as to the status of replication. In the process of trying to test and develop an influential theory, we have become dissatisfied with current accounts of what replication means, what purpose it serves, and what its fundamental value is.

This paper is an attempt to address these problems, with a radical overhaul of replication. In doing so we summarise three views of the role of replication in organisational theory, developing the themes of the conventional, the social and the aesthetic. Each of these has its particular insights. Associated with each view is a wider domain of enquiry. Together insights from these fields offer a more comprehensive and realistic description of the status of replication in management studies.

Theory Development as 'Scientific Project'

The first view of replication that we outline - replication as 'scientific project', is the most well articulated to date. The term science is sometimes used pejoratively, or as a basis to undermine claims that research can be value free, unencumbered by ideology, or a source of progress (Allen, 2003, p. 291; Potter, 1996; Thorpe, 2001). Though we discuss these themes in the following section, in this section, we try to use science in a descriptive way that is intended to be non-pejorative, so that it serves an analytic purpose. In describing the 'scientific project' account of theory development, we wish to retain a naïve, or 'folk concept' (c.f. Becker, 1970) of scientific practice; in other words, where replication itself is seen as a technology for developing, 'knowledge as science (in the sense of academic knowledge)', which is separate from 'knowledge as culture' (Delanty, 2001, p. 151). This has the advantage of allowing us to differentiate between the technical and epistemological challenges posed by a technology of replication, and the social or cultural challenges to such a project. An added advantage is that retaining this sense of the scientific allows us to hold on to our belief that replication can be useful as a technical procedure. Lest we be criticised for running with the scientific fox and then hunting with the postmodern hounds, this

is prompted by a desire to offer a wider ranging, revisionist account of replication. This necessitates engaging with the debate not just from within one camp, but from several, in an attempt to escape the current 'figuration', i.e. the existing meaning and value laden frame in which we understand the term replication (Elias, 1970).

A number of organisational theorists have explored the legitimacy of developing theory in management studies, by invoking comparisons with other disciplines (Cole, 1993); where a conventional view of theory development is that empirical data can be called upon to support, or refute theory (Bacharach, 1989; Chimezie and Osigweh, 1989; Eden, 2002; Glass, 2000; Neuliep, 1991; Tsang and Kwan, 1999). For example, falsifiability has long been advocated as a necessary criterion for theory in the natural sciences (Hendrick, 1991; Magee, 1971; Popper, 1959, 1962). This is because no amount of confirmatory evidence can prove a theory holds true for all time, as proof by induction is always open to question. A falsifiability criterion allows comparison between competing explanations of phenomena according to whether they are testable (and hence in principle open to falsification), or not testable. However, adopting falsifiability as a necessary criterion for management studies is problematic because the absence of immutable laws means rigorous standards of falsification are impossible (Tsang and Kwan, 1999). Others have pointed out that the falsifiability criterion itself is not open to falsification, and hence an inappropriate dictum even for the natural sciences (Brief, 2003). Nonetheless, falsifiability is often represented as a desirable, or traditional characteristic of organizational theory (Bacharach, 1989, p. 500; Lee et al, 1999, p. 459; Mitchell and James, 2001, p. 543; Whetten, 1989, p. 486-487; Worren, Moore and Elliot, 2002, p. 1227). According to Sayer (1992, p. 204), the implication of social scientists adopting a falsifiability

criterion is that we may be either unduly pessimistic, or naively optimistic about the possibilities of developing theory.

To accept that replication can be valid offers some of the advantages of a falsifiability criterion, because it permits the accumulation of empirical knowledge. It also implies that theory can be developed, because it acknowledges the importance of observation and testing. However, it does not commit us to such rigorous standards of closure, because whether replication is successful, is more clearly seen as a matter of degree, rather than success or failure (Rosenthal, 1991a). If we believe that replication has some potential to be applied in management studies, this suggests that organizational researchers need to understand the particular conditions that apply in the social sciences (i.e. the ways in which it differs from the natural sciences), before they can put replication to work. A starting point is to acknowledge that accepting the validity of replication means committing to some basic beliefs about the nature of reality. The claim that management theories can be replicated is open to challenge in the same sense in which correspondence and coherence theories of truth are open to challenge. A correspondence theory of truth holds that truth consists in agreement with reality; in other words, what determines whether a statement is true is whether it corresponds with 'the way things are' (Flew, 1984, p. 76). A coherence theory of truth holds that truth consists in agreement with an established system; in other words, whether a statement is true concerns, 'a relation *among propositions*, not a relation between a proposition and … a state-of-affairs' (Hospers, 1973, p. 116, original emphasis). Replication studies depend on a correspondence account of truth, insofar as they assume the researcher is able to discriminate accurately between and compare two different situations in which a theory is tested. Replication studies also depend on a

coherence account of truth, because to incorporate it as a tool of inquiry, replication must be construed as part of a wider framework for sense-making.

If reality is understood as purely socially constructed – an *anti-realist* stance - this denies the validity of any replication study. For anti-realists, there is no sensible way to talk about different situations in terms of an underlying theory, since this assumes a degree of objective knowledge about the ways in which two situations differ. This is a stance that Sayer refers to as 'unfounded pessimism' (1992, p. 204). Conversely, to believe the social world exists independently of our cognition, and that access to it is unproblematic – a *naïve realist* stance - means replication is always valid irrespective of mode of inquiry. This is what Sayer calls 'exaggerated optimism' (ibid). Rejecting anti-realism means committing to the belief that the 'social world has a reality of its own' (Burrell and Morgan 1979, p. 4). Rejecting naïve-realism means committing to the belief that access to this reality is fundamentally problematic and mediated (Sayer, 1992) – a *critical realist* stance.

For critical realists, replication can be valid across a range of settings, though this is contingent, because access to the social world is problematic, and 'social structures and their causal powers operate beyond the immediate perceptions of individuals' (Roberts, 2001: 669). The promise of critical realism is that it offers an epistemological basis from which to divine such structures, and hence it allows for the possibility of developing and testing theory (Bhaskar, 1989; Sayer, 1992; Tsang and Kwan, 1999). So, sharing a set of assumptions about the nature of reality (that it is socially mediated, but there are discernible regularities in structure) allows for replication, at least in principle. This element of consensus can be helpful, and is seen

by some as a necessary precursor for scientific investigation. For example, Chimezie and Osigweh (1989, p. 580) argue, 'imprecise concepts make it difficult to produce knowledge that is cumulative', and this point is made even more strongly by Cole, '[w]ithout agreement on fundamentals, scientists will not be able to build on the work of others and will spend all their time debating assumptions and first principles' (1983, p. 134 *in* Pfeffer, 1993, p. 611). Similarly, Grunow advocates that, in the face of challenges from postmodernism, 'debate on organization theory needs a starting point for communication' (Grunow, 1995). These sentiments owe much to the Kuhnian notion that 'normal science' allows the accumulation of knowledge because of a shared paradigm (Hendrick, 1991; Kuhn, 1962; Pfeffer, 1993). Endless debate is avoided because there are clearly defined limits as to which questions are meaningful. The difficulty for organizational theory is that one result of 'agreement on fundamentals' may be staid consensus, if one of the agreed fundamentals is how to conduct research. Scope for 'style' (Van Maanen, 1995) and the rejection of a clear paradigm may be a source of strength, diversity and creativity (Zald, 1996).

This three-part classification (above) of epistemological stances is an oversimplification since there are many different ways of looking at the world, and few researchers would confine themselves to the naïve realist, or anti-realist camps. However, these two polar types illustrate how at the extremes, there is no question as to the suitability of replication. Since most social science researchers do not hold either of these extreme positions, those of us in between face a dilemma, which can be framed in the following way:

If there is scope to accumulate knowledge about events in the social world, then we should test theory. If it is possible to identify and address limitations via logical critique, theory can be developed. To accomplish both goals requires a type of testing that is flexible enough to allow for theory development, but rigorous enough to preserve the logic of a repeated test. Otherwise, any investigation must choose between 'creating' a new theory, or 'testing' an existing theory. We suggest this tension between theory development and theory testing is an unavoidable, archetypal research dilemma (cf McGrath, 1982). In any replication, there must be a degree of sufficient similarity, otherwise it will not count as a test, yet without a degree of extension, or development, the test will be open to the charge of not making a contribution (Hendrick, 1991). Following Lykken (1968, in Eden, 2002, p. 842), for a replication to be considered sufficiently similar, it must allow for testing of the 'same hypothesized relationships among the same theoretical constructs'. Given these constraints, the most potentially useful tests will be those that differ as much as possible from the prior study or studies, so as to establish the generality of the theory (Rosenthal, 1991b). As well as the *where* and *how*, such differences extend to *who* carries out the replication (Eden, 2002; Hendrick, 1991). Tests by researchers wholly unconnected with the original authors of a theory are preferable, since they are normally regarded as more objective (Rosenthal, 1991b; Tsang and Kwan, 1999).

The degree to which a replication study differs is often described in terms of discrete types: for example, the difference between 'literal', 'strict' or 'exact' replications, and 'conceptual' replications (Hendrick, 1991; Hubbard, Vetter and Little, 1998; Tsang and Kwan, 1999); or in terms of replications that test 'reliability', versus those that test external 'validity' (Rosenthal, 1991a); or 'reproducibility' and 'generalizability'

(Amir and Sharon, 1991); or that establish 'authenticity' (Hubbard et al, 1998). However, it is more sensible to consider replications as lying on a range, given that the number of ways in which two studies can differ from one another is indeterminate. This range can be understood in terms of the tension between maintaining sufficient similarity (to preserve replication status), and maximising scope for difference (to enhance potential contribution). This is a reformulation of the tension between theory testing and theory development (above).

Perversely perhaps, given the value placed on novelty in management studies (Mone and McKinley, 1993) a replication study that is an unqualified success, is also one that tells us nothing new (Bornstein, 1991). If on the other hand, the replication calls previous findings into question, this 'null result' is not valuable unless the original theory has been closely followed in the replication. This is because it is otherwise impossible to know whether failure is to be explained in terms of the inadequacy of the original theory, or because of the changes made prior to replication (Tsang and Kwan, 1999). These dilemmas make carrying out replication research a risky business, implying that researchers need a degree of prescience (or perhaps pre-science).

it may be possible to have, 'surrogates or substitutes or equivalents of replication within the assumption structure of other approaches to inquiry' (Weick, 1999, p. 800).

Theory Development as Socio-Cultural Artefact

There is an established sense in which scientific practices can be seen as culturally embedded, and historically contingent, rather than simply being a search for transcendental truths. This idea is addressed in several strands of literature that examine and problematise the status of science and the nature of scientific knowledge. Below, we have organised these strands into three sections, each of which has implications for an account of replication: science as discourse; knowledge as characterised by production and consumption; and the role of the scientific community. The insights from each of these strands show how it is difficult to retain a folk concept of science, since they each demonstrate the influence of political and cultural norms, values and ideologies on scientific research.

1. The literature on science as discourse, or as discursive practice (Foucault, 1979 / 2002; Latour 1993; Potter 1996), suggests that instead of an ahistorical, transcendent view of science, we see it as something whose:

> social meaning becomes a problem to be understood, both in terms of how it is accomplished as an ongoing practice by particular scientists in particular settings, and how it is understood socially and culturally as a type of resource (Locke, 2001, p. 11).

This analysis shows how scientific procedures such as replication can be recast as rhetorical devices, or linguistic tropes. Instead of being represented as discrete parts of a technology, or abstract system, these can be understood as part of an overarching ideology, or discourse. Calling on themes of rationality, precision and science may be useful in legitimating the practice of management studies as an academic discipline, and enable us to draw comparisons with higher prestige disciplines. The ideal practice of replication can then become represented as

something we should aspire to, and something which advances in technological and statistical procedures, or paradigm development will soon make a reality (Pfeffer, 1993; Tranfield, Denyer and Smart, 2003). This analysis would be one means of explaining the gap between an espoused valuing of replication, as a ''must' for advancement' (Amir and Sharon, 1991), and the paucity of replication studies (Hubbard et al, 1998; Neuliep, 1991). It is easier to appreciate a gap between the normative and actual values of replication in management studies, if replication is seen as a rhetorical resource, rather than a precision instrument.

2. Understanding knowledge as characterised by relations of production and consumption rather than discovery or invention, similarly denies a transcendent status to science (Bourdieu, 1990, 1993). In this way, theories are socially constituted, 'consumption of knowledge fuels the creation of new knowledge while new knowledge acquires its status as 'knowledge' only when selected for consumption by important players' (Hassard and Kelemen, 2002, p. 333). These themes of production and consumption can also be seen in terms of the sanctioning and legitimation of a theory by the research community (Kuhn, 1962). This in turn encourages further research, with the potential development of shared methodologies, and 'schooling' (McKinley, Mone and Moon, 1999; Pfeffer, 1993); less optimistically, this can result in tribalism, or groupthink (Campbell, 1979; Janis, 1982). One perspective is that social and cultural changes have altered the relations between scientific communities and wider society, necessitating new forms of knowledge production (Gibbons et al, 1994; Delanty, 2001; Nowotny, Scott and Gibbons, 2001; Starkey, 2001). The idea that there is, or that there should be, coherence between scientific practices and the social

world hints at a trajectory of progress, where the demands of social change are met by changes in the way the academy operates. In this sense, those advocating new forms of knowledge production make a slightly different point from those identifying patterns of production and consumption, since this process implies a final purpose, or *telos*. However, the notion that theory should be developed and tested so as to remain relevant to the needs of diverse stakeholders (Starkey and Madan, 2001), implicitly invokes the notion of consumption, and consumers. In this light, replication can be understood as one means of producing knowledge that is relevant, or palatable. The gap between the normative and actual value of replication could be a reflection of the particular tastes of the academic community, for whom replication studies are insufficiently novel (Neuliep and Crandall, 1991).

3. There is also an established sense in which scientific communities can be understood as playing a part in constituting social order, since they may be perceived as the embodiment of authority and wisdom (Durkheim, 1957; Giddens, 1990; Merton, 1938 / 1973; Polanyi, 1946; Weber, 1958). Whether for good or ill, the academic community can be seen as standard bearers for the advancement of knowledge, as secular priests, with new moral ideals, such as objectivity. 'Deprived of the shepherding role of the Christian clergy, there is a temptation to look to a new, scientific clergy for moral guidance' (Thorpe, 2001, p. 20), however, since science does not provide such moral guidance, or a sense of certainty, this can lead to disenchantment (Giddens, 1990). The roots of this analysis lie with Nietzsche's exposition of the self-contained, subversive dialectic embodied in the 'ascetic ideal'.

> You see what it was that really triumphed over the Christian god: morality itself, the concept of truthfulness that was understood ever more rigorously, the father confessor's refinement of the Christian conscience, translated and sublimated into a scientific conscience, into intellectual cleanliness at any price (Nietzsche, 1887 / 1974).

Nietzsche's exposition is relevant to the debate on replication, since it shows by analogy how the view of replication as 'scientific project' contains within itself the seeds of its own destruction. The complexities of the social world preclude discovery of truths, and relying on technologies such as replication to do so is misguided, however appealing replication may at first appear in terms of its 'intellectual cleanliness'.

All these illustrate how the procedure of replication can be understood as embedded in the practices of particular communities, so that it can legitimately be interpreted as a socio-cultural artefact, or symbol, rather than a discrete technique that forms part of a scientific project. In turn these different strands suggest potential explanations for the disparity between the espoused and actual value of replication studies. Replication may be analogous to a linguistic trope, which management scholars draw on as a normative ideal, but merely pay lip service to in practice; it may be one form of knowledge production, that results in forms of knowledge that are less readily consumable; it may signify an internally incoherent procedure, that exemplifies the contradictions inherent in pursuing unattainable ideals of objectivity or truth in the social sciences. This wider, political aspect to replication resonates with some of the fundamental problems posed by postmodernist critiques that undermine claims to objectivity and unmediated access to truth (Lyotard, 1984). However, before outlining the final view of replication as aesthetic practice, it is worth noting that there

are ways in which scholars working in what we have called the 'scientific project' camp have identified with and acknowledged these themes. For example, Pfeffer (1995, p. 684) advocates the importance of replication, in a way that he claims is independent of any arguments over method. The ability to extend, replicate and adapt others' work, 'is simply an ecological argument; the ability to readily reproduce gives ideas (just as it does other forms) survival value'. The emphasis on replication and selection echoes Baum's view of organisational ecology (Baum, 1997), and outside the field of management studies other theorists offer explanations for various phenomena ranging from the success of best-sellers (Dawkins, 1998, p. 302), to explanations of language and consciousness (Dennett, 1993), to the origins of life (Dawkins, 1997, pp. 254-271) in similar, ecological terms. Seeing replication in this wider context reinforces the importance of understanding the influence of political and social structures on the development of organizational theory, but it does not rule out the idea that replication can have some intrinsic value.

Paradoxically perhaps, (given that the main thrust of these challenges is to undermine the value of scientific objectivity) the socio-cultural account of replication underlines a key benefit of critical testing. If it is possible to simultaneously test theory, and accumulate knowledge, while allowing for experiment and improvisation, then replication can be a source of development, or critique rather than simply being a vehicle for reinforcement. Appealing though this notion is, it is perhaps unrealistic in the face of evidence suggesting that replication studies are not actually valued (Hubbard et al, 1998). To explore this further, it is beneficial to consider the insights from another perspective, namely replication as an aesthetic practice.

Theory Development as Aesthetic: The Philosophy of Art

Strati and Montoux (2002, p. 757) advocate the relevance of aesthetics in management studies, arguing that the, 'sharp distinction between science and art in organizational studies' has 'lost legitimacy':

> The split between scientific discourse and aesthetic experience has faded (ibid.).

This position is the starting point for our introduction of the philosophy of art to this debate. As the previous two sections show, even researchers working with what we have called a 'scientific project' account of replication recognise the influence of cultural and political pressures (Hubbard et al, 1998; Neuliep and Crandall, 1991; Pfeffer, 1993; Tsang and Kwan, 1999). In this section we try to show that eliding the split between scientific discourse and aesthetic experience may be one way in which replication can be revalued. To do this, we draw on recent work advocating greater recognition of the influence of aesthetic experience on contemporary research (Strati, 1999; Strati and Montoux, 2002).

In a tripartite framework, Strati (1999, pp. 188-190) outlines different approaches to organizational aesthetics in the study of culture: the 'archaeological approach'; the 'empathic-logical approach'; and the 'empathic-aesthetic approach'. Using this classification, the approach we try to follow in this paper is perhaps closest to the archaeological approach, where, '[t]he researcher assumes the guise of an archaeologist or a historian of art to investigate values and symbols… to explore the information yielded by the artefacts or fragments of artefacts about the 'civilizations' that created them' (Strati and Montoux, 2002, p. 756). To some extent, we have

already adopted an archaeological approach in our analysis of the preceding section, because treating replication as a socio-cultural artefact, involved considering processes of legitimation and sanction ('values and symbols'); their associated patterns of production and consumption (creation and use of 'artefacts'); and the relationship between communities and social order ('civilizations'). Here however, we want to widen the scope of our investigation, to draw out some of the more general insights that the philosophy of art can bring, in terms of further eliding the 'split between scientific discourse and aesthetic experience'.

We argue that this split is illusory and that maintaining it results in intellectual incoherence. This makes a realistic account of the status of replication unattainable. This is because the view of replication as a tool to enhance the accumulation of empirical knowledge (the 'scientific project'), is to a degree incompatible with the view that theory development is embedded in a particular context (the 'socio-cultural artefact'). Recognition of socio-historical contingency is in direct conflict with scientific claims (to universalisability, objectivity and generalisability). However, the failure to acknowledge the role of aesthetic responses in theory development, means that adherents to the 'scientific project' view do not have a coherent way of explaining the paucity of replication studies, since replications are a means to enhance universalisability, objectivity and generalisability. Neither do adherents to the scientific project view have a means of solving the problem of the 'second class' status of replication studies (Hendrick, 1991). Existing 'solutions' (e.g. to have replication only journals, to improve the quality of replication studies, to have a certain number of pages in leading journals devoted to replication studies, Hubbard et al 1998; Neuliep, 1991 passim) are incoherent, because they address the problem in a

mechanistic way. Instead, we argue that a revaluation of the status of replication studies can only come about through recognition of the aesthetic dimension to theory development, and through recognition that a scientific discourse / aesthetic split is a charade. Although we do not know what the mechanism for such a widespread reappraisal might be, we offer in this section some ways in which to reappraise this.

There are several ways in which insights from the philosophy of art are relevant to the debate on replication. Firstly, in relation to the 'scientific project' account of replication, it is legitimate to introduce the notion of replication as a skilled craft. For example, Rosenthal (1991a, pp. 17-25) makes a convincing argument for relying on effect sizes (e.g. 'r' and 'Z' values), rather than statistical significance ('p' values); for reporting statistical power. This is not simply a rhetorical trick, since (if we accept the legitimacy of replication) there are sound statistical reasons for prioritising summary statistics other than 'p' values; 'p' values may simply be a function of sample size, which is not usually the most meaningful basis for comparing two studies. Introducing an element of craft, suggests one way in which aesthetic and scientific criteria are already brought to bear on evaluating replication studies. One way of closing the gap between aesthetic and scientific discourse could be to reintroduce the Aristotelian notion of *techne*, which is the root of our modern word technique, but which also has connotations of mastery (Worren et al, 2002, p. 1228).

Secondly, again in relation to the 'scientific project' sense of replication, there is a useful parallel to be drawn between attempts to resolve the question 'what is a work of art', and the problem of what constitutes a replication. In neither case can we rely on a set of necessary and sufficient conditions. Though this is true of other concepts

as well, Dickie's (1974) attempt to resolve this question is noteworthy. Though this theory is called institutional theory in aesthetics, to avoid confusion, we shall refer to it as Dickie's theory. In response to the question, 'what is art?' it states:

> A work of art in the classificatory sense is (1) an artefact (2) a set of the aspects of which has had conferred upon it the status of candidate for appreciation by some person or persons acting on behalf of a certain social institution (the artworld) (Dickie, 1974, p. 34, *in* Hanfling, 1992, p. 20).

There are several parallels with Dickie's definition, and the problematic status of replication. First, it deals with classification, and so avoids the problematic issue of discussing whether something is of superior, or inferior quality. Second, it refers to something that is created, 'an artefact'. Third, it focuses only on the relevant features of the artefact, 'a set of the aspects'. Fourth, it indicates there is an element of sanction 'has conferred upon it' by powerful individual(s) 'by some person or persons'. Fifth, these individuals represent a wider social institution, 'the artworld'. Recognising the significance of the conferral of status illuminates a comment by Eden, 2002, namely that he would bet a year's pay that most studies are in some sense replicative. What is interesting is that so few of these studies are labelled replications. Revisiting this might cause a significant reappraisal of the status of replication. A slightly more subtle point is that adopting an amended version of Dickie's theory would recognise that replication studies are artefacts, and in some sense authentic works, rather than simply copies. This could also be a way of revising the status of these studies as second class citizens.

It is interesting to note the way in which some influential theorists have advocated aesthetic judgment as an integral part of theory building and theory testing in

management research (Van Maanen, 1995a, 1995b; Weick, 1993). For example, Van Maanen argues persuasively against a Pfefferdigm, by advocating consideration of 'style' in conducting research (Van Maanen, 1995a). This is not simply a rejection of homogeneity in design and epistemology, on aesthetic grounds, but also illustrates the effect that modes of theorising have on us as individuals, and on the people, processes and systems we study. Van Maanen illustrates how the gap between the scientific and the aesthetic is elided in the process of building theory:

> By trying to write like everyone else (and not talking about it in public), we not only bore ourselves to tears but restrict the range of our inquiries and speculations (ibid., p. 139).

So, adopting a model of consensus is not simply a technical or scientific agreement on convention, but a choice that involves social regulation, and it is also an adoption of an aesthetic, that in turn influences the way in which we represent the world. Weick also advocates an aesthetic sensibility in theory development, '[w]henever one reacts with the feeling *that's interesting*, that reaction is a clue that current experience has been tested against past experience, and the past understanding has been found inadequate' (1993, p. 525, original emphasis). This also elides the scientific / aesthetic split, by recognising the importance of an emotional response to theorising. It also suggests one way in which the response to replication could be reappraised, namely by revisiting the status of null results. In one sense, 'failed' replications do not contribute because they fail to offer support for a theory. We have argued that falsification is an unrealistic goal for management theory, because we cannot ever 'refute' a theory of management, (instead we can perhaps claim to redefine the limits within which it is generalisable). This implies null results in replication studies do not even have the negative contribution they do in other disciplines. Further, since so few

replications are exact, or literal (given the desire for novelty), a null result is doubly empty. It does not represent a contribution, nor does it seem to offer scope to explain failure. Nonetheless, we would contend that such results are intrinsically *more* interesting either than exact replications (which may support, or critique a theory), or extensions that develop support for a theory. This revaluation is possible if we understand the aesthetic dimension to theory, because, as Weick argues:

> A disconfirmed assumption is an opportunity for a theorist to learn something new, to discover something unexpected, to generate renewed interest in an old question, to mystify something that had previously seemed settled, to heighten intellectual stimulation, to get recognition and to alleviate boredom (ibid.).

This extract also offers a nice summary of the three spheres of action we have discussed: the *scientific* 'opportunity… to learn [and] discover', the *aesthetic* 'stimulation', and the *social* 'recognition'. Well conducted replication studies that disconfirm assumptions offer a basis for learning something new. If they are viewed in this light, rather than as 'failing' to support a theory, then those considering carrying out a replication do not need prescience, since the outcomes of the study will be symmetrical and equally valuable (either extending theory, or a basis for further study).

If we acknowledge that it is legitimate to view theory development as the creation of cultural artefacts, and recognise processes of knowledge production and consumption, this makes the link between scientific and aesthetic discourse even stronger. It also invokes an alternative means of explaining or investigating the problematic status of replication. To explore this, we use an existing data set, from Neuliep and Crandall, (1991, p. 88) of 'editor's comments regarding the problems with publishing

replication studies'. Rather than list all thirteen comments reported in Neuliep and Crandall's survey of 47 social science journal editors, we select six that exemplify aesthetic responses to replications. We code these in terms of three aesthetic qualities that we believe influence contemporary evaluation of replication: authenticity, craft and novelty. We do not claim that the comments reported indicate responses that are purely aesthetic, merely that they have a substantial aesthetic component. As an aside, this procedure is an example of a replication study, using the same data set, but different analysis; in other words, what Tsang and Kwan (1999, p. 766) label 'reanalysis of data'. Though the data well is very shallow, we feel it is sufficient to show how responses to replication studies can be understood as aesthetic:

Insert Table 1

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Maybe put a classificatory diagram or something in here?

Conclusion

This'll be a summary of the above

Table 1: Reanalysis of Neuliep and Crandall, 1991 (selection of comments)

Editor's Comment Regarding Replication Studies	Aesthetic Component
'Dull'	Novelty
'the worst of the modern science/social science publish or perish mentality'	Authenticity
'People aren't interested in them'	Novelty
'When do you stop? Is one rep enough or should we let someone build their career replicating the same study?'	Authenticity / Craft
'Readers feel that replications are redundant and don't reflect cutting edge stuff'	Novelty / Craft
'They tend to be boring and not contribute a lot'	Novelty

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