

# Critical realism and realist research in human geography: a method or a philosophy in search of a method?

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**Abstract:** Recent philosophical debates in human geography tend to misappropriate critical realism as a method *per se*. Drawing upon an extensive review of the realist philosophy and method in social science, this article argues that critical realism is a philosophy in search of a method. It first delves into recent debates about critical realism within the wider geographical discourse. It then suggests three useful guidelines in executing realist research in human geography: iterative abstraction, qualified grounded theory method and methodological triangulation. The article ends with a detailed empirical example for the readers to work through of the ways in which realist research can be practised in human geography.

## I Introduction

In recent years, critical realism has received serious attention in the geographic literature. There are various debates on the practice of human geography (Lawson and Staeheli, 1990; 1991, *versus* Chappell, 1991), flexible specialization (Lovering, 1990; 1991, *versus* Scott, 1988; 1991a; 1991b) and postmodernism (Lovering, 1989; 1993; Murdoch and Pratt, 1993; Sayer, 1992; 1993). There is also an increasing attention to alternative research methods in human geography (Schoenberger, 1991; McDowell, 1992; Healey and Rawlinson, 1993; Herod, 1993; Miles and Crush, 1993). These interesting and fruitful debates provide the discursive context for this article. It aims to clarify the nature of critical realism and to suggest several methodological avenues for realist research in human geography because the 'practicalities of what it means to do realist research are still emerging and ought to be the subject of a wider debate' (Sarre, 1987: 10).

The basic argument of this article is twofold: 1) critical realism is intrinsically a philosophy; and 2) to a certain extent, both realists and their opponents have not adequately resolved the issue of method in critical realist research. The latter issue becomes particularly important when the 'methodological cart' often comes before the

‘philosophical horse’ in geographic research. Previous debates in human geography tend to focus on critical realism as a method, neglecting its essentially philosophical nature. On the other hand, realist philosophers are almost exclusively concerned with further refinement of their philosophical positions, at the expense of the actual ways in which realist philosophy can be practised (cf. Pratt, 1995). The tension between the misappropriation of critical realism in human geography and the lack of methodological development within the philosophical discourse of realism itself has prompted a critical reappraisal of both the philosophy and the method in this article.

This article is structured into four parts. The first section summarizes very briefly the canons of critical realism as a philosophy of (social) science. This is followed by a timely deconstruction of the debates on critical realism and the practice of human geography in the second section. The issue at hand is whether critical realism is appropriated as a method or a philosophy within the (human) geographical community. The third section presents some methodological guidelines, not rigid prescriptions, for realist research. The points raised in this article also have wider relevance to the social sciences at large. The penultimate section offers a detailed empirical example to illustrate how realist research can be practised.

## II Critical realism: a method or a philosophy?

This article draws on critical realism, a distinct version of the realist philosophy originally proposed by Roy Bhaskar (1975; 1979; 1986; 1989a; 1989b; 1993; 1994) and adapted by many other writers (Harré and Madden, 1975; Keat and Urry, 1982; Harré, 1985; Outhwaite, 1987; Layder, 1990; Sayer, 1984; 1992; Collier, 1994). In philosophy, there are many varieties and versions of realism which are not entirely consistent with each other. This is, however, neither the place to engage in a full-fledged review of the realist philosophy of science, nor the place to evaluate different varieties of realism (see Harré, 1986).<sup>1</sup> It suffices to make the claim that critical realism has been widely recognized as the hallmark of the Bhaskarian version of scientific realism in the social sciences (e.g., Bhaskar, 1993; 1994; Collier, 1994) – a scientific *Philosophy* that celebrates the existence of reality independent of human consciousness (realist ontology), ascribes causal powers to human reasons and social structures (realist ontology), rejects relativism in social and scientific discourses (realist epistemology) and reorientates the social sciences towards its emancipatory goals (realist epistemology).

Table 1 presents a summary of the philosophical ideas embedded in two versions of scientific realism proposed by Roy Bhaskar: transcendental realism (for natural science) and critical realism (for social science). One of the many unresolved questions remains: what is the relationship between philosophy, ontology, epistemology, theory and method in the social sciences? This question demands, of course, no simple answer (see Outhwaite, 1987; Sayer, 1992; Collier, 1994; Morrow, 1994). The position of this article is that philosophy deals with the ontological and epistemological aspects of the social sciences (i.e., what is the social world and why do we need to research it?), whereas substantive social sciences themselves address the theoretical and methodological issues (i.e., why do social phenomena occur the way they do and how do we research on them?). Philosophy is thus particularly useful in explaining such ontological questions as what the social world must be like in order for the social sciences to be possible. This is a transcendental argument. Philosophy is also pertinent to the understanding of epistemological

**Table 1** A typology of scientific realism in science

	Transcendental realism	Critical realism
<i>Discipline</i>	Natural science	Social science
<i>Origin</i>	<ol style="list-style-type: none"> <li>1) Rejection of Humean empiricism and Comtean positivism</li> <li>2) A synthesis of the Copernican revolution</li> </ol>	<ol style="list-style-type: none"> <li>1) Rejection of positivist account of science: methodological individualism</li> <li>2) Rejection of empiricism, positivism, structuralism and hermeneutics</li> <li>3) Search for the possibility of naturalism</li> </ol>
<i>Ontology</i>	<ol style="list-style-type: none"> <li>1) Things and materials possess <i>causal powers</i> independent of human agency: their realization is contingent</li> <li>2) <i>Intransitive</i> dimension of science: stratification of the world into different levels of 'ontological depth'</li> <li>3) The existence of <i>natural necessity</i>: secretion of causal powers through generative mechanisms and enduring tendencies</li> <li>4) <i>Open systems</i>: no regular conjunctions of events and outcomes</li> </ol>	<ol style="list-style-type: none"> <li>1) <i>Pre-existence</i> of social structures: transformed and reproduced by social actors</li> <li>2) Human agency with <i>intentions</i>: reasons as real causes</li> <li>3) Continuous process of <i>structuration</i> between structures and agency</li> <li>4) <i>Open systems</i>: no regular conjunctions of social events and outcomes</li> </ol>
<i>Epistemology</i>	<ol style="list-style-type: none"> <li>1) Science is an <i>ongoing</i> process</li> <li>2) <i>Transitive</i> dimension of science: social (re)production of knowledge</li> <li>3) Search for <i>causal laws</i> in science: explanation but not prediction</li> </ol>	<ol style="list-style-type: none"> <li>1) The possibility of <i>naturalism</i></li> <li>2) <i>Subject-matter</i>: internal (necessary) and external (contingent) relations between objects and events</li> <li>3) A <i>material</i> perspective of knowledge</li> <li>4) <i>Practice</i>: immanent critique and emancipation of actors</li> </ol>
<i>Methodology</i>	<ol style="list-style-type: none"> <li>1) Process of <i>retroduction</i>: a posteriori reasoning</li> <li>2) Possibility of <i>experimentation</i></li> <li>3) Use of postulated entities and analogies</li> <li>4) <i>Practice</i>: theoretical and empirical research</li> </ol>	<ol style="list-style-type: none"> <li>1) Process of <i>abstraction</i> and retroduction</li> <li>2) Impossibility of experimentation</li> <li>3) Possibility of <i>direct awareness</i> of structures and mechanisms</li> <li>4) Theoretical (abstract) and empirical (concrete) research</li> </ol>

debates in the social sciences. For example, one of Bhaskar's strongest philosophical critiques of modern social science is of 'epistemic fallacy' which arises from the confusion of the relationship between the nature of objects (ontology) and the social knowledge of them (epistemology) (Bhaskar, 1975; 1979; 1989a; 1994). Recent debates in postmodernist discourses are subject to the critique of epistemic fallacy (see Sayer, 1993).

In summary, while philosophy *can* contribute much to ontological and epistemological debates, it is still up to each substantive social science (e.g., economics or geography) to discover empirically grounded theories and, in the process of doing so, to set up their distinctive methodological apparatus. Critical realism has much to say on the philosophy of the social sciences, but it leaves the theoretical and methodological work to each substantive social science. The real need, as a result, is to achieve a dialectical mediation between philosophy (concerning ontology and epistemology) and the social sciences (concerning theory and methodology). There is much for philosophy to inform research

in the social sciences. Similarly, successful research in the social sciences can shed light on the nature (e.g., ontology) and practice of philosophy itself. Any lack of such mediation in dialectical moments (cf. Bhaskar, 1993; 1994) often leads to a misunderstanding of reality either in philosophy (e.g., Kantian idealism) or social science (e.g., positivist geography). In other words, we need a philosophy to inform our practice and, at the same time, through our practice we would inform our philosophy in dialectical ways. If we could integrate realist philosophy and social practice, realism should no longer be seen as a realist's categorization of social thinking (cf. Vaitkus, 1994).

### III Realist research in human geography

Before presenting methodological suggestions for the practice of critical realism in human geography, it is necessary to take one step backward by evaluating the fundamental flaws of some ongoing debates on the practice of human geography. This article argues that the crux of most recent debates in human geography rests upon a misreading of different moments of critical realism. There seems a lack of proper understanding of critical realism in its own terms. Critics of realism and realist research in human geography rely largely upon cursory readings of different versions of 'realism' presented in the geographic literature. It is not surprising that many of them are confused between treating critical realism as a philosophy, an epistemology, a method, a dogma or just another '-ism'.

The first and most devastating misreading of critical realism is to treat it as an *epistemology*. This confusion between critical realism as an ontology and as an epistemology is evident in the claim by Cloke *et al.* (1991: 134) that 'its main task has been to begin to offer an epistemology: that is, a theory of the nature of knowledge and a justification of belief'. Such a misreading can easily slip into an attack on critical realism on the basis that it is calling for an 'ultimate' truth or theory. Doel's (1991) deconstructionist review of Bhaskar's (1989a) book also suffers from such a treatment of critical realism as a totalizing philosophy. In fact, critical realism argues exactly the contrary. The essence of Bhaskar's (1975; 1979; 1989a) transcendental realism is that if there is an ultimate truth or theory (be they derived from empirical regularities or whatsoever), science will be impossible because it is either perfect or meaningless.

On the other hand, one should not take Bhaskar's realist philosophy as advocating relativism found in Kuhn's (1970) notion of the 'incommensurability' of different paradigms. Critical realism is as much against totalization as relativism. To a critical realist, all knowledge is fallible, but not *equally* fallible. The adequacy of social knowledge, for example, can be established by practice through an immanent critique and human emancipation. A critique is paradigmatically distinguished from criticism in that it isolates the source or the ground of the imputed error (see an example in the penultimate section). As such, an explanatory critique may license a negative evaluation on the causes of the error concerned. An immanent critique isolates a theory/practice inconsistency (see Bhaskar, 1993: 396; 1994; also my example in the penultimate section). An important lesson for human geographers who aspire to critical realism is that it is a philosophical argument about the ontology of reality; it is not just another epistemological prescription, if it has ever been. Missing this crucial point would almost guarantee someone guilty of committing 'epistemic fallacy' by mistaking the relationship between the nature of objects (realism as an ontology) and the social knowledge of them (realism as an epistemology).

A second common misunderstanding of critical realism, as evident in some debates within human geography, is to regard critical realism as a *method per se*. For example, Keeble (1980) has misread critical realism as a method unconsciously (or consciously?) so that he could simply apply an essentially positivist method to analyse the theoretical argument (i.e., industrial restructuring thesis) used by some realists. To cite another example, Gregory (1986: 387, emphasis omitted) has also mistakenly put forward a methodological definition of critical realism as ‘a philosophy of science based on the use of abstraction to identify the (necessary) causal powers and liabilities of specific structures which are realized under specific (contingent) conditions’. The tendency among human geographers, who are either sympathetic or hostile to critical realism, is to regard critical realism as a set of abstractions which are used to ‘tease’ out the causal mechanisms and structures of empirical phenomena. Though this error of misunderstanding is not as serious as ‘epistemic fallacy’, it tends to overlook the philosophical power of critical realism. This article argues that critical realism makes its strongest claims at the ontological level: the independent existence of reality and causal powers ascribed to human reasons strengthen the possibility of reclaiming reality and an emancipatory social science. This strength of critical realism is ironically its weakness – that method in critical realist research has received much less attention in the Bhaskarian version of critical realism. Social scientists are thus confronted with a ‘realistic’ philosophy in search of a method. Potential converts in critical realism often face rather embarrassing difficulties in realizing the power of realist philosophy in their empirical research. So far, only limited methodological works have been conducted (e.g., Allen, 1983; Sayer, 1981; 1984; 1992; Pratt, 1994; 1995).

One consequence of this lack of clarity between critical realism as a philosophy and critical realism as a method is that many of its critics tend to read critical realism as a method of science and attack it on that ground. This article argues that critical realism is not just about methods. Methods are surely important, but their importance cannot be exercised unless they are supported by strong philosophical claims at the ontological and epistemological levels. This is precisely the reason why positivism and empiricism have failed because they offer easily accessible and ‘objective’ methods (e.g., quantification and statistical methods) without resolving some fundamental philosophical problems embedded in Humean empiricism and logical positivism (e.g., successive events as cause and the atomization of human rationality; see Bhaskar, 1989a; 1994).

A third common misunderstanding of critical realism is to consider it, among positivism, empiricism and structural Marxism, as another *dogma* or ‘-ism’.<sup>2</sup> Such is the case in Chappell’s (1991) immanent critique of the realist practice in human geography suggested by Lawson and Staeheli (1990). The former tends to pin critical realism down to specific (Bhaskarian) versions in order to justify his critique of the latter. Their exchange is a typical example of misdirected arguments which are grounded in different axes of reference (philosophical *versus* practical) and different practical preoccupations (predictive *versus* emancipatory). This can be extremely superficial and does much harm to the legitimacy of critical realism than provides new critical insights on its practice in human geography. A similar problem of misunderstanding is also found in Scott’s (1991a; 1991b) reply to his critics. He considers critical realism as just another ‘-ism’ celebrating contingency and messiness of empirical reality. He obviously pays little attention to the philosophical underpinnings of critical realism in broader contexts. All these examples show that misleading stereotypes can be generated as easily as any ‘-ism’ in contemporary social science. Critical realism is not an exception, as a victim, in such a general tendency

to label a philosophical standpoint without much appreciation of its fundamental concern with ontology and epistemology. It is interesting to see how much the word ‘realism’ has contributed to its present misunderstanding.

#### IV Method for critical realism: some issues

This article argues that method in critical realism is underdeveloped and misunderstood, resulting in a methodologically handicapped philosophy. Some 15 years ago, Keat and Urry (1982: 229) complained that ‘we have given no clear indications of how this [critical realism] can be done in practice’. In contrast, the relative easy accessibility of its method is one of the main reasons why ‘positivism in its empiricist guise is alive and well, and winning more than its fair share of methodological battles’ (Allen, 1983: 26). The state of realist methodology has not been much improved since then. In a recent article on the practice of critical realism, Pratt (1995: 61, emphasis in original) notes that ‘[p]recious few attempts have been made rigorously to follow through critical realism from conceptualization to practical research and back again. Those who have done so have seldom reflected upon the *methodological* implications’.

A clear implication of the above review is that a recent development in the realist philosophy is unable to capture its fair share of ‘methodological battles’ largely because of its opaque method. Despite the reputation of critical realism as ‘the flavor of the 1980s in human geography’ (Pratt, 1991: 252), relatively little work has been done on its methodology (Allen, 1983; Sayer, 1984; 1992; Layder, 1993; Pratt, 1994; 1995). For example, most practising critical realists understand the crucial importance of causal powers and generative mechanisms of objects in explanations. Similarly, they believe faithfully in abstraction as a useful tool to reclaim reality. The basic methodological question remains: how can these ‘things’ be abstracted? What is the starting-point of abstraction in the first place? How exactly should a realist conduct (or ‘operationalize’ in positivist terms) a piece of critical research? Realist philosophers (e.g., Roy Bhaskar and Rom Harré) provide no readily made answers to these questions because critical realism originates from a philosophical concern with transcendental ontology and it is still in its embryonic form. It should be noted, however, that methodology was not the primary concern of critical realists in the 1970s and early 1980s because they were mainly involved in engaging with a debate within the philosophy of science in opposition to the positivist position (e.g., Bhaskar, 1975; 1979; Harré 1985).

Today, the time appears ripe for a serious consideration of the issue of methodology in critical realist research. This article does not intend to provide a comprehensive answer or a ‘cook-book’ to this tricky, and yet intriguing, question of method in critical realism (see also Pratt, 1995). It aims modestly to engage in a methodological appraisal of critical realism and to shed light on some issues in the *potential* realist method. Some authors of research method in the social sciences argue that there is no necessary connection between research method and a particular philosophical stance (e.g., Platt, 1986; Bryman, 1984; 1988; 1992; Hammersley, 1992; Silverman, 1993; Morrow, 1994). Instead, a mixed method of both qualitative and quantitative research has been advocated (Brewer and Hunter, 1989; Brannen, 1992) because a ‘selection among these positions ought often to depend on the purposes and circumstances of the research, rather than being derived from methodological or philosophical commitments’ (Hammersley, 1992: 51). This article does not dispute such a timely appraisal of the qualitative–quantitative divide in research

method, but it does contend with such methodological pragmatism by arguing specifically that in the practice of critical realism, certain methodological guidelines are *more* relevant and useful than others. The precise ways in which these guidelines are employed in different research processes are dependent upon, but not necessarily determined by, different research topics and contexts (cf. Layder, 1988; 1993). For example, ethnomethodology may not be very useful to understand the globalization of finance in the capitalist space economy; it fares much better in investigating issues of social interaction (e.g., Boden, 1994).

More generally, qualitative methods such as interactive interviews and ethnography are necessary to abstract the causal mechanisms of which quantitative/statistical methods are oblivious. It should not be expected that these abstract causal mechanisms can explain events directly without any need for empirical research into the contingency of the concrete. To do so is to commit the error of ‘pseudo-concrete research’ that is common in radical structuralism such as Marxism (Sayer, 1992). Quantitative methods, on the other hand, are particularly useful to establish the empirical regularities between objects. Although these concrete regularities are not causal relations, they can inform the abstraction of causal mechanisms. Quantitative methods are also useful in drawing attention to the external and contingent relations between objects. Inferential statistical analysis can throw light on, for instance, the external relations between objects (e.g., employment and poverty) in society from a sample. One should bear in mind that these statistical generalizations are only ‘universal’ at a specific temporal–spatial intersection. A serious problem of reductionism is incurred if one attempts to treat these contingent generalizations as necessary causal mechanisms.

The *nature* of the realist method merits detailed examination because it provides ‘a set of guidelines which outline how to critically analyse and re-work existing conceptions of social processes’ (Allen, 1983: 26). The realist method is basically a posteriori in that given the social reproduction of knowledge, a critical realist seeks to reconstruct causal structures and their properties on the basis of constant reflections and immanent critique. It is, for example, impossible to realize a priori the existence of capitalist social relations without experiencing some of their manifested effects. Casual mechanisms are thus historical and contextual in their realization. The realist method must abstract a posteriori causal mechanisms and stipulate their contextual circumstances. Moreover, if an experiment is considered as ‘an attempt to trigger or unleash a single kind of mechanism or process in relative isolation, free from the interfering flux of the open world, so as to observe its detailed workings or record its characteristic mode of effect and/or to test some hypothesis about them’ (Bhaskar, 1986: 35), then it is not possible to be carried out in social science. Such an impossibility of experiments in social science does relegate it to an inferior position *vis-à-vis* natural science. Social science, however, has an upper-hand over natural science in another sense because the explanation of the social world can be accompanied by direct experience and practice. While no natural scientists can observe atoms, many social scientists are directly aware of the existence of social relations (e.g., marriage) and some have even practised on it.

The following section specifically addresses three methodological avenues and their central issues: 1) the use of iterative abstraction; 2) the ‘grounded theory’ method; and 3) the use of ‘triangulation’ in realist research. As a preamble, these are not the only methods that are compatible with critical realist philosophy. But they may probably be the most practically adequate methods in the practice of critical realism. As far as possible, these methods are evaluated to show how they are applied differently in critical realist research.

Another important point is that these methods are different from specific research instruments which refers to questionnaire surveys, participant observations, personal interviews, action research and so on. These research instruments mainly aim at the collection of empirical data to be analysed at a later stage of the research process. Although they do not constitute methods in their own right, they can be utilized empirically in accordance with the guidelines provided by each of the three methods outlined below.

## 1 Iterative abstraction as a method in realist research

Iterative abstraction is probably the most well-known method in critical realism to discover and to conceptualize generative mechanisms. Abstraction is a central and necessary tool in the realist method for several reasons. First, it is a practically adequate method to mirror social structures and generative mechanisms. Sayer (1992: 86) argues that

knowledge must grasp the differentiations of the world; we need a way of individuating objects; and of characterizing their attributes and relationships. To be adequate for a specific purpose it must 'abstract' from particular conditions, excluding those which have no significant effect in order to focus on those which do. Even where we are interested in wholes we must select and abstract their constituents.

Secondly, abstraction serves as a first sound step towards conceptualizing and theorizing the real essence, power and mechanism of an object (Sayer, 1981). Thirdly, abstraction helps to distinguish external/incidental/contingent from internal/essential/necessary relations between objects and events since '[n]either objects nor their relations are given to us transparently; their identification is an achievement and must be worked for' (Sayer, 1992: 88). If such an attempt is successful, 'rational abstraction' is said to be obtained (Sayer, 1981). Abstraction is thus extremely useful for the identification of causal structures.

The method of abstraction in critical realist social science is unsettled for the time being. So far several authors have contributed to refine the method of abstraction (e.g., Allen, 1983; Horvath and Gibson, 1984; Sayer, 1981; 1984; 1992; Lawson, 1989; 1995). The purpose of abstraction is to isolate causal mechanisms (the 'real') in relation to a concrete phenomenon and 'to obtain knowledge of *real* structures or mechanisms which give rise to or govern the flux of *real* phenomena of social and economic life' (Lawson, 1989: 69, emphasis in original). Any causal mechanisms must therefore be in a necessary relation to a phenomenon in issue and, therefore, the former must presuppose the latter. Concrete objects are constituted by a combination of diverse elements or forces *from* where the abstraction is made. They are something real, but not something reducible to the empirical. Marx, in his *Grundrisse* (p. 101), conceptualizes the concrete as 'the unity of the diverse' (quoted in Williams, 1981: 32).<sup>3</sup> An abstract object, however, is 'a one-sided or partial aspect of an object' (Sayer, 1981: 7). Abstraction necessarily 'isolates in thought a *one-sided* or partial aspect of an object' (Sayer, 1992: 87, emphasis in original) and involves a double movement from the concrete to the abstract and from the abstract to the concrete:

At the outset our concepts of concrete objects are likely to be superficial or chaotic. In order to understand their diverse determinations we must first abstract them systematically. When each of the abstracted aspects has been examined it is possible to combine the abstractions so as to form concepts which grasp the concreteness of their objects (Sayer, 1992: 87).

A realist thus starts an empirical problem and proceeds to abstract the necessary relation between the concrete phenomenon and deeper causal structures to form generative mechanisms. As more empirical evidence is collected, a realist may revise or reaffirm his

or her abstraction so that the process of iteration continues until no further contradictory evidence is obtained and the alleged generative mechanisms are robust and powerful enough to explain the concrete phenomenon. Such is the point whereby ‘realistic’ abstractions have been achieved.

Specifically, two analytical criteria can be applied to adjudicate whether a proposition can be considered as a causal mechanism and whether the abstraction is realistic:

- 1) When this mechanism is activated under appropriate circumstances or contingencies, will the proposed phenomenon occur?
- 2) Can this phenomenon be caused by other mechanisms? If yes, the proposition cannot be a generative mechanism because it is not exclusive.

An example can be found in abstracting the causal mechanism of pregnancy. Dey (1993: 49) suggests that the ‘sexual drive is an integral part of the human make-up – even when it is repressed. As such, it is a power (or a susceptibility) which acts as a causal mechanism in producing particular and identifiable effects (such as pregnancy)’. However sound it appears to be, this proposition is flawed under closer scrutiny because pregnancy and reproduction can be effected through other ‘mechanisms’ (e.g., nonsexual means). The proposition cannot therefore satisfy criterion (2) and sexual drive fails to perform its role as *the* causal mechanism of pregnancy, as compared to, say, human reproductive make-up. But sexual drive may still be *an* indirect cause of pregnancy. The process of abstraction continues in an iterative manner until a point where ‘theoretical saturation’ is reached. Theoretical saturation occurs when further abstraction brings no significant additional theoretical rigour to the generative mechanism and when empirical evidence is strong enough to support the practical adequacy of the postulated mechanism in explaining a concrete phenomenon.

One should be careful in deciding what aspects of an object should be abstracted. The tendency to conceal the constituent of an object by abstraction may result in ‘chaotic conception’ (Sayer, 1981; 1984; 1992). The notion of ‘chaotic conception’ implies a bad abstraction which is based upon a non-necessary relationship, or which divides the indivisible by failing to recognize a necessary relationship. The only way to move away from this ‘chaotic conception’ is neither to increase the sample size or representativeness of sample (as positivists typically do), nor to interpret subjectively the categories and objects of investigations. A realist move necessarily involves a more careful theorization of the objects of investigations and incorporates such a reflection of these categories during the research process.

The broader realist method in which iterative abstraction is embedded is known as *retroduction* in which an argument ‘moves from a description of some phenomenon to a description of something which produces it or is a condition for it’ (Bhaskar, 1986: 11, note 26). In other words, we should move from pure descriptions of the phenomenon to abstractions of possible causes. The discovery of these generative mechanisms responsible for empirical behaviour at that level of abstraction can be accomplished through a postulation of hypothetical entities and generative mechanisms. One such tool of hypothetical entities is the use of an analogy which is ‘a relationship between two entities, processes, or what you will, which allows inferences to be made about one of the things, usually that about which we know least, on the basis of what we know about the other’ (Harré, 1985: 172). Both functional equivalence and plausibility are important criteria to confirm the adequacy of any theory. In order to be qualified as adequate, the hypothetical mechanism at the heart of each theory must behave analogously to the real mechanism

(i.e., functional equivalence) and must be constructed analogously to some known entity (i.e., plausibility). The purpose of using analogies is to locate the real essence of things (i.e., their natures and constitution).

Attributing the nature of relations between subjects and objects during the process of retrodution is 'to indicate that within the relevant context no alternative to that condition, outcome, truth-value or conclusion is possible' (Harré and Madden, 1975: 19). Ascribing tendencies to generative mechanisms requires an understanding of whether the enabling conditions for the exercise of causal powers are satisfied. There is thus a place in the realist methodology for the use of hypothetical or conditional statements to ascribe power based on a dual criterion (Harré and Madden, 1975: Chapters 4 and 5): first, there must be a test or trial show of the behaviour of the thing in the specified way, given appropriate circumstances. This is, nevertheless, more conducive under laboratory experiments in some natural sciences (e.g., physics and chemistry). Secondly, the nature or constitution of the material is then investigated. This second criterion can possibly be met in social science through intensive studies of the nature and constitution of social structures. In contrast to the Humean 'scandal' of induction, there is a possibility of induction because necessary relations can be discovered a posteriori, i.e., by inferring from the outcome of actions:

Explanations are precisely what they appear to be in science and ordinary life – that is, *explanations!* – and do not require shoring up by various external 'vindications' of induction in order to have universal validity *within a given universe* (Harré and Madden, 1975: 70–71, emphasis in original).

Abstractions may likely lead to a number of possible mechanisms responsible for a particular concrete event. We therefore need to set up some criteria to adjudicate what constitutes an adequate abstraction because 'almost contentless abstractions can easily, if unwittingly, be manipulated or "strengthened" in illegitimate ways to yield conceptions that really are no longer abstractions at all' (Lawson, 1989: 72). These useful criteria and guidelines are summarized as the following (see Harré, 1979: 236–38; Bhaskar, 1986: 168; Outhwaite, 1987: 58; Lawson, 1989: 68–73):

- 1) A satisfaction of certain a priori conditions of self-consistency: internal validity of the axioms and logical structure of theory.
- 2) An adequacy in its account of the reproduction and transformation of that system: 'internal access' to structures and mechanisms.
- 3) An a posteriori evaluation of its maximum and revealed explanatory power such that the postulated mechanism is capable of explaining the phenomenon.
- 4) Its acceptance by most competent members if there are good reasons to believe in the existence of the postulated 'real' mechanism.
- 5) Most competent members can be persuaded to adopt it since no other equally good alternative explanations can be thought of.
- 6) Its dialogical engagement with one's subject and intersubjectivity between subjects and objects: feedback from human agency.
- 7) Its ability to make one's analysis as precise as 'the nature of the subject permits' (Aristotle, quoted in Bhaskar, 1986: 168).
- 8) A successful prediction has a role to play, but it should never be the main/sole criterion.
- 9) The formulation of successful policy to emancipate human agency from exploitative 'real' structures: e.g., changing the structures for housing allocation in Bedford (Sarre, 1987).

If more than one abstracted theory satisfies the above stringent conditions for a good abstraction in the social sciences, simplicity becomes an obvious candidate as the final criterion.

## 2 The grounded theory method in realist research

Another potential contribution to method in critical realism is the grounded theory method (Glaser and Strauss, 1967; Glaser, 1978; Strauss, 1987; Strauss and Corbin, 1990; also Layder, 1993). It is a mode of doing analysis for generating and testing theory – ‘a qualitative research method that uses a systematic set of procedures to develop an inductively derived grounded theory about a phenomenon’ (Strauss and Corbin, 1990: 24, emphasis omitted). The following are some rules of thumb in this mode of analysis:

- 1) It is accomplished by enormous complexity: first, interpretations and data collection are guided by successively evolving interpretations. Secondly, a theory must be conceptually dense with many concepts and linkages. Thirdly, a detailed, intensive and microscopic examination of the data is necessary.
- 2) The use of experimental data.
- 3) Induction, deduction and verification are three integral elements of a grounding in data.
- 4) The ‘triad’ of research are data collection, coding of data and memoing of data.

Turner (1981; 1994) has provided an interesting example of how to apply the grounded theory method to qualitative analysis. He has also suggested a sequential series of stages in grounded theory analysis:

- 1) After some exposure to the field setting and some collection of data, the researcher starts to develop ‘categories’ which illuminate and fit the data well. These ‘categories’ are meant to stimulate further theorization and conceptualization. They are not exclusive and certainly are at their most suggestive until the entire research process is completed.
- 2) The categories are then ‘saturated’, meaning that further instances of the categories are gathered until the researcher is confident about the relevance and range of the categories for the research setting. There is a recognition in the idea of ‘saturation’ that further search for appropriate instances may become a superfluous exercise.
- 3) The researcher then seeks to abstract a more general formulation of the category, as well as specifying the criteria for inclusion in that category.
- 4) These more general definitions then act as a guide for the researcher, as well as stimulating further theoretical reflection. This stage may prompt the researcher to think of further instances which may be subsumed under the more general definition of the category.
- 5) The researcher should be sensitive to the connections between emerging general categories and other milieux in which the categories may be relevant.
- 6) The researcher may become increasingly aware of the connections between categories developed in the previous stage, and will seek to develop hypotheses about such links.
- 7) The researcher should then seek to establish the conditions in which these connections pertain.

- 8) At this point, the researcher should explore the implications of the emerging theoretical framework for other, pre-existing theoretical schemes which are relevant to the substantive area.
- 9) The researcher may then seek to test the emerging relationships among categories under extreme conditions to test the validity of the posited connections.

The grounded theory method reinforces iterative abstraction in realist research by providing a mediation between theory and practice. First, theoretical categories must be grounded in empirical evidence so that abstractions do not occur in a vacuum. In other words, they must be abstracted from concrete or empirically observable phenomena. An emerging realist theory can achieve much practical adequacy through such a method because 'it moves the research away from the idea that "producing" theory is something of a sacrosanct activity reserved only for those who have been initiated into the mysteries of some "master" framework or perspective' (Layder, 1993: 53). Secondly, the practice of realist research would be guided by a quest for theory rather than by an utter empiricism. Theoretical sampling, a process through which the choice of subjects for extensive empirical investigation is driven by intensive theorization or conceptualization, helps avoid much decontextualization of objects in statistical methods and facilitates the choice of valid objects for investigation and reproduction. The grounded theory method come in handy for the practice of realist research by complementing the functioning of iterative abstraction and by grounding realist theories of causal mechanisms in concrete phenomena.

The grounded theory method is relatively well received in the qualitative sociology literature (e.g., Layder, 1993; Silverman, 1993; Bryman and Burgess, 1994). Its use in qualitative sociology, however, differs from realist practice in that the former does not take on board any *ontological attributes* of reality. To the sociologists, the grounded theory method is used to discover theories based on empirical facts, not iterative abstractions of necessary relations as in the case of realist researchers. Moreover, despite potential advantages associated with the method, pragmatic qualitative researchers typically pay only lip service to the iterative interplay of data collection and analysis that is at the heart of the grounded theory method.

The use of the grounded theory method in realist geography, however, is not only rare (e.g., Tickell, 1992; Pratt, 1994) but also far from satisfactory. Two reservations about the wholesale import of the grounded theory method into critical realist geography deserve special attention. First, the original method is essentially *inductive*: the same argument also applies to qualitative method as a whole which is more concerned with inducing theory and hypothesis from field research (e.g., Layder, 1993; Silverman, 1993). As it stands, the grounded theory method tends to be phenomenological and therefore antithetical to realism.<sup>4</sup> In qualitative sociology, Strauss and Corbin (1990: 23) define a grounded theory as 'one that is inductively derived from the study of the phenomenon it represents'. In practice, an inductive method such as the grounded theory approach may suffer from many operational constraints: 1) elements of empiricism are just too obvious to be covered; 2) most researchers may be lost in the so-called 'field' because they lack theoretical sensitivity; and 3) it may overlook the broader context of a concrete social phenomenon and/or its generative abstract mechanisms. The net effect of these operational constraints is a tendency towards an 'eclectic empiricism'<sup>5</sup> in which too many empirical categories (what the grounded theorists call 'theoretical categories') are combined in a typically atheoretical framework. Causal relations and tendencies between the referents of these categories and the phenomenon at hand are perpetually lost. What one

ends up with, in the final analysis, is a complex map of empirical categories grounded in the data. While this article accepts that a theory should be grounded in concrete data in order to test its practical adequacy, it disputes the inductive ways in which a theory is grounded (e.g., in qualitative sociology and, recently, in cultural geography).

The realist method for theory construction is neither purely deductive, nor purely inductive. It operates rather simultaneously in a deductive–inductive dialectic. On the one hand, a realist researcher must constantly resort to critical abstraction that is more deductive in nature. On the other hand, this abstraction and subsequent theoretical construct should not determine the entire concrete research that remains open and flexible. The role of the realist researcher is to achieve a harmonious synchronization between deductive abstraction and inductive grounding of generative mechanisms. Under this argument, a realist researcher should not simply ‘borrow’ an existing theory and fit it into empirical data; nor should the theory emerge solely from concrete data. The most practically feasible method of theorization is an iterative process of abstracting theories based on an immanent critique and the grounding of abstractions in concrete data (see an empirical example in the penultimate section).

Secondly, the grounded theory method relies too much on the *subject’s narrative* of concrete social phenomena. To a certain extent, the grounded theory method postulates that both substantive and formal theories can be generated directly from concrete data, implying that generative mechanisms can also emerge directly from data. But three dangers are associated with this over-reliance on ‘direct data’ (i.e., data collected directly from actors). First, social actors (including critical realists!) may be trapped in false consciousness, unable to explain truly and to account fully for their action. Quite often, this happens when social actors are constrained and bound by social structures. An example of capitalism is appropriate here. A worker may not be able to account for the fact that he or she works as a waged labourer. A typical account given would be: ‘I work because I need money to buy food, shelter, health care and so on ...’, instead of ‘I work because under capitalism I have to sell my labour value in order to exchange it for a part of what I have produced’. The consciousness of this waged worker has already been trapped in the capitalist relations of production. He or she does not realize that in the process of doing so, he or she is reproducing (and sometimes transforming) such relations of production. What then is the researcher’s role in helping the social actor? It seems that the realist mission of human emancipation is one such role. By explanatory critique (see Collier, 1994) and research into underlying social structures, critical realists may contribute towards the eventual freedom of social actors from the imprisonment of false consciousness. Secondly, much information on structural context and contingency is not obtainable directly from individual case studies and/or interviews. Sometimes, the researcher must ‘elevate’ him or herself from the data to get a broader and clearer picture. Thirdly, although causal categories can emerge from the data, relations among these causal categories (i.e., generative mechanisms) cannot be ‘read off’ straight from the data. They must be abstracted in conjunction with substantive theorization and immanent review, subject to a posteriori evaluation.

In summary, the grounded theory method can potentially complement iterative abstraction in the realist methodology by grounding abstract causal mechanisms in empirical data. We must be cautious, however, in applying the grounded theory method because it may simply lead to another form of empiricism hidden behind a qualitative mask. One possible way to avoid falling into such an empiricist trap is to use the method in close connection with iterative abstraction. By applying both methods carefully, we

should be able to uncover the causal mechanisms of concrete phenomena through empirical research in its intensive and extensive variants.

### 3 The use of triangulation in realist research

A third methodological issue is concerned with the use of the *triangulation* method (Webb *et al.*, 1966; Denzin, 1979). Triangulation is inherently a call for multimethod in social scientific research. This method is based on the conviction that ‘there is no fundamental clash between the purposes and capacities of qualitative and quantitative methods or data. What clash there is concerns the primacy of emphasis on verification or generation of theory’ (Glaser and Strauss, 1967: 17). Originating from the practice of social research, the use of triangulation is extensive and well accepted in sociology and organizational studies (see Fielding and Fielding, 1986; Brewer and Hunter, 1989; Martin, 1990; Brannen, 1992; Yeung, 1995a). Its applications in human geography have so far been minimal, let alone in realist research. This relative neglect is explained by the general lack of methodological developments symptomatic of excessive preoccupation with theoretical debates within human geography in the past two decades.

Denzin (1970: Chapter 12) suggests four basic ways of triangulation:

- 1) Data triangulation with respect to time, place, person and level.
- 2) Investigator triangulation via multiple observers of the same phenomenon.
- 3) Theoretical triangulation via multiple theoretical perspectives with respect to the same set of objects.
- 4) Methodological triangulation via both between-method (dissimilar methods) triangulation and within-method (variations within the same basic methodology) triangulation.

Triangulation, in particular its methodological form, can do much to improve the validity and reliability of data collected. This contribution is based on the assumptions that the method is well understood and different data complement each other in revealing different facets of the social world. Methodological triangulation is therefore broadly compatible with the deployment of both intensive and extensive methods in realist research.

Some cautions, however, must be given at this juncture to prevent its unqualified intrusion into the realist method. First, triangulation may suffer from the problem of ‘ironies’ in data collection (Silverman, 1985: 20; 1993) when action makes sense only in context. It becomes much more problematical to compare different sets of data. Some varieties of triangulation are sometimes boiled down to merely eclecticism in that data collected from different contexts are combined without proper cross-examination and scrutiny. Secondly, a real difficulty is encountered in locating a common unit of observation for each theory to be applied. Triangulation does not help much in this regard. Thirdly, when data from different methods are in conflict, it may be extremely difficult to decide which should be accepted and who should take the decision. In triangulating his research on the effects of a merger on employees by using standard survey and other unobtrusive observations, Jick (1983) found immense difficulties in weighting the data generated from different sources and methods. Fourthly, there is a tendency to regard qualitative data as more trustworthy than quantitative data should differences in findings arise (Bryman, 1992). This may raise the issue of bias in interpretation. Finally, there is the usual question of time, cost and accessibility to critical data areas, types or levels.

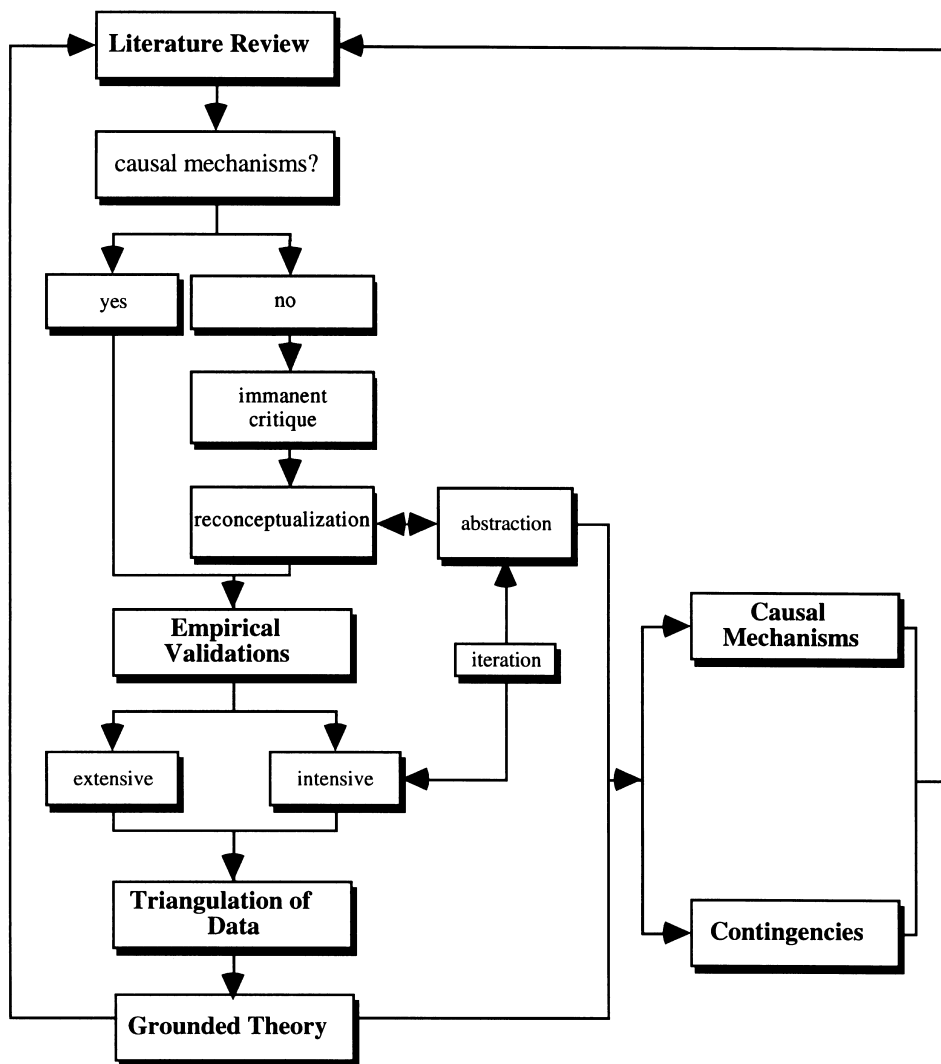
As a final remark, the practical adequacy of causal mechanisms is likely to be better established through triangulation if the methods used are all consistent with realist ontology and epistemology. What is necessary in the process of triangulation is to compare and contrast different sources of findings if they are addressing the same phenomenon. Alternatively, if different methods are used to investigate different facets of the same phenomenon, the resultant findings tend to be complementary. The methodological implication is that triangulation is useful *in so far* as different facets of a concrete phenomenon are researched through the most appropriate combination of methods; it surely is not about replication *per se*, but about making connections within particular cases. This approach to triangulation applies in particular to social research:

In any case, the concept of triangulation in sociological studies operates differently. It is less a case of checking a 'fact' collected by one method, using another method than using one method and then justifying the results by means of another. Usually, it is the survey that is the primary method. Data from informal interviews, or open-ended questions taken on to the questionnaire or data from other kinds of sources are then used either as illustrations of the 'fact' or as an explanation (Porter, 1994: 70).

## V Realist research in practice: an empirical example

This article so far addresses theoretically the ways in which critical realist research *can* be practised. This section aims to present an empirical example to illustrate how such potential methods have been applied in practice. It also serves as a guide to prospective realists on how to conduct their geographical research. One caveat here is that even realists can still get things wrong in their substantive analyses because to critical realists, all knowledge is fallible. Incorporating the key methodological suggestions in this article, Figure 1 outlines the possible trajectories of realist research in practice. It is by no means a recipe for all realist research, but rather it is an illustration of the research process of a particular realist. The following example is drawn from a recently completed study of transnational corporations from Hong Kong (HKTNCs) and their activities in southeast Asia (Yeung, forthcoming). The original aim of that study was to *explain* the spatial organization of HKTNCs in the southeast Asian region. The author sought to apply almost all aspects of the methodological guidelines outlined in the previous sections of this article.<sup>6</sup> Two key dimensions relevant to the practice of critical realism are 1) conceptualization and explanation; and 2) empirical validations.

The study took the theoretical and empirical gaps in the so-called 'third-world multi-nationals' literature as the starting point for reconceptualization. In the realist methodology, any explanatory study must start with conceptualization through an immanent critique of the existing work. This approach does not appear to differ much from the literature review method in any empirical studies conducted under the auspices of empiricism and positivism. It differs from them, however, in the elements of the critique and the ways in which such a critique is used for further reconceptualization (see Figure 1). Going back to the study of HKTNCs, it argues that despite over two decades of academic research on transnational corporations (TNCs) from developing countries, we have only inconclusive evidence and misleading stereotypes regarding their characteristics and processes of transnational operations, coupled with economistic and western-centric theoretical perspectives (Yeung, 1994a; 1994d). A common characteristic of these perspectives is that they employed established economic theories to explain these allegedly 'unconventional' TNCs. While some studies largely relied upon an eclectic approach in



**Figure 1** Realist research in practice

explaining ‘third-world multinationals’ (e.g., push-and-pull factors), others preferred a unilinear view (e.g., technological and developmental theories). Despite their differences in theoretical orientations, these studies agreed upon several key characteristics of ‘third-world multinationals’: small in size; concentration in labour-intensive industries; preference for joint ventures; loose and informal intrafirm control; reliance on home source of capital; low, but appropriate, technology; production of mature products largely for export; and lack of direct interfirm competition.

Today, it is fairly obvious that most of these characteristics are no longer valid for *many* TNCs from developing countries (Yeung, 1994d). On the other hand, these characteristics can also be found in *some* developed country TNCs. The problem the study intended to tackle is whether this ‘conventional wisdom’ of ‘third-world multinationals’ contributes to misleading stereotypes still prevailing in most studies of TNCs from

developing countries (Yeung, 1994a). It is absolutely vital for us to examine not only what the literature has to offer but also whether findings and explanations are generalizable in the context of the *changing* world economy. The study therefore was confronted with conventional economic costs and benefits analysis (e.g., lower labour costs and more government incentives in southeast Asia have attracted HKTNCs). It argued that these ‘factors’ are merely time- and space-specific contingencies; they bear little causal power in explaining the underlying causal mechanisms of transnational operations (Yeung, 1993).

Having pointed out that the ‘third-world multinationals’ literature is shallow in its ontological understanding and lacks deeper theorization of the causal mechanisms of transnational operations, there was a need to search for a theoretical framework to inform the study. A review of different theoretical perspectives in the disciplines of industrial economics, international business, economic sociology and economic geography showed that very few of them are concerned with the central theoretical category identified through an immanent critique of the empirical literature on ‘third-world multinationals’ – business networks, although most of them supposedly explain the TNC and its transnational operations. They suffer, in one way or another, from the problems of 1) historical abstraction and ‘pseudo-concrete analysis’; 2) inadequate theorization and ‘chaotic conception’; 3) an ignorance of the spatial constitution of the TNC; 4) the neglect of the issue of society and culture; 5) paying little attention to diverse forms of organizing capitalist production; and 6) an omission of the role of the state. More specifically, relying upon transaction costs economics and the eclectic paradigm, economic theories offer much less in explaining *why* and *how* transnational operations by ethnic Chinese business firms, for example, are less driven by transaction cost considerations than by network formations and exploitation. Meanwhile, although networks have recently received increasing attention in the international business literature, it tends to *describe* the patterns of networks, rather than to explain their underlying sociospatial organization and embeddedness (Yeung, 1997a). It appears that economic sociology, as a third strand of theoretical perspectives, has the most promising insights to offer. But these various theoretical perspectives are mostly concerned with the firm as a unilocational entity, not as a multilocational organization.

To theorize the organizational nature and the social embeddedness of the TNC, the author needed to develop a network perspective based upon the realist ontology and iterative abstractions (Yeung, 1993; 1994b; 1997a). This network perspective aimed to *explain* the ways in which transnational operations are established. It was argued that networks possess causal powers to enable successful transnational operations. These causal powers are embedded in dense networks of business and personal relationships at three distinct levels: 1) intrafirm co-ordination and control; 2) interfirm co-operation; and 3) extrafirm connections and bargaining. The TNC was seen as a transnational network governance structure which possesses causal power through multinationality and is capable of effecting changes and transformations in the global space-economy. Because of the peculiar ways in which the rationality of this networked organization could be exercised, the TNC was theorized as spatially embedded in specific localities in its conduct of international business through networks of relationships at all levels. At the abstract level, actors on networks (i.e., TNCs in the study) are empowered to operate according to their *modus operandi*. Such an enactment of causal power, however, is subject to time- and space-specific constraints. For example, when markets or cost advantages are available in particular geographical locations at a specific time frame, the network-

embedded TNC will tend to establish operations to take advantage of those economies. There is thus always a continuous interaction between causal powers embedded in deeper structures and changing empirical contingencies.

Given these underlying theoretical insights and propositions obtained through an immanent critique and iterative abstractions, the challenge remained at the methodological level: how should we conduct research which is necessarily concerned with vague and intangible network relationships? An earlier critique revealed that there are too many quantitative and macroeconomic studies in the empirical literature on TNCs from developing countries (Yeung, 1994a). These statistical analyses tend to show empirical contingencies or surface phenomena. They tell us little about the *underlying* processes and mechanisms of empirically observable events (i.e., southeast Asian operations of HKTNCs). To execute a realist research, the study engaged simultaneously in extensive and intensive methods (see Figure 1). While the former method was concerned with the context and empirical contingencies (i.e., patterns), the latter revealed almost exclusively the underlying causal mechanisms (i.e., processes) of any phenomenon via iterative abstractions. Extensive methods were employed, mainly through questionnaire surveys, to collect general information on HKTNCs. Postal surveys were planned for in the initial research design. But real-life situations in Hong Kong and ASEAN countries precluded such possibilities. In turn, personal interviews were used throughout the field survey phase, encompassing both 111 parent HKTNCs in Hong Kong and over 60 of their subsidiaries and/or affiliates in four southeast Asian countries. On the other hand, intensive methods were used for immanent critique, theory construction and empirical validations. During the theory construction stage, substantial reviews of the literature were conducted and a network theory of TNCs was suggested as the basis of reconceptualization. In terms of empirical validations, intensive methods called for a more qualitative orientation, e.g., the use of personal in-depth interviews to probe processes and mechanisms of transnational operations by HKTNCs (Yeung, 1995a).

A sceptical reader may question the extent to which the above methods are different from any other ‘-isms’. There are at least three differences that can be detailed here: first, the study did not start with preconceived notions both at the stage of literature review and at the stage of data collection. Hypothesis testing was simply not the way in which a realist should proceed. While iterative abstractions were constantly under way from an immanent critique to data analysis, the notion of ‘networks’ did not fully emerge until empirical data were triangulated and the ‘network theory’ was grounded in these data (see also Tickell, 1992). Secondly, most empirical studies of TNCs rely on extensive methods. In very few instances, intensive methods are used. Even so, only a handful of them are concerned with abstractions and uncovering underlying causal mechanisms. Thirdly, the study was highly sceptical of crude statistical data which often form the backbone to econometric studies of foreign investments. At best, these foreign direct investment (FDI) ‘numbers’ were used in the study to highlight the general patterns and contexts of HKTNC operations in southeast Asia (e.g., Yeung, 1994c; 1995b; 1996a; 1996b).

Having collected the much-needed data through both extensive and intensive methods, the study now faced a major challenge – how to analyse them in relation to the realist methodology. In the first place, the study began by triangulating the data which existed in quantitative and qualitative forms. It appeared that quantitative data were representative and robust enough to present a general picture of Hong Kong investments (HKFDI) in southeast Asia. They were therefore used to describe the spatial organization of HKFDI in southeast Asia and to illuminate the empirical and descriptive context of the study

(Yeung, 1994c; 1995b; 1996a; 1996b). FDI data, however, were inadequate in explaining *why* these HKTNCs operate in southeast Asia. To complement such deficiency, the study turned to qualitative data obtained through carefully conducted personal interviews with parent HKTNCs and their subsidiaries in southeast Asia. Similar to Pratt's (1994; 1995) research into the development of the form and location of industrial estates, interview data in this study of HKTNCs went through an intensive grounding process in which concepts emerged and data were thoroughly examined. Because all personal interviews were conducted by the author (see Yeung, 1995a), the grounding process started with the interviews. Interview tapes were transcribed immediately after each interview. Conceptual categories (e.g., network relationships, institutional constraints and so on) emerged almost instantly from this grounding process and they were largely consistent with the abstract network perspective developed earlier in Yeung (1994b). This implies that the network perspective was robust enough and could be grounded in empirical data. Interview quotes were also assessed in their contexts and irregular/inconsistent statements were eliminated. The transcripts were then recategorized to facilitate subsequent analyses. By the time the actual analyses were conducted, the transcripts and quotations from interviewees came in handy because of 'prior' categorization.

The ready availability of these qualitative data, often in verbatim quotations, formed the backbone to the validation of the role of networks as the causal mechanisms of transnational operations (Yeung, 1997b; 1997c; forthcoming). The data were not only rich enough to support such an empirical 'grounding' of the abstract theory but also methodologically sound enough to enable the development of a more socially embedded approach to the study of the geography of international business. Because of the robustness of the network perspective on HKTNCs, interview data were presented in accordance with its theoretical categories (e.g., intrafirm networks, interfirm and extrafirm networks). Interview quotations were selected on the basis of their representation of the sample firms and their details. The case study approach (cf. Yin, 1994) was adopted in the analysis and presentation of interview data (see Yeung, 1997b; 1997c). These cases were drawn from secondary firm-specific information and qualitative personal interviews. They outperformed quantitative analysis in illuminating the nature and working of business networks as the causal mechanisms of transnational operations.

The results of the study boldly challenged the received wisdom in the 'third-world multinationals' literature. The study found that both markets and market presence underpin the strategic predisposition of parent HKTNCs. It evaluated the role of three key 'factors' (or empirical contingencies) in the southeast Asian operations of HKTNCs: 1) cost factors; 2) markets and marketing activities; and 3) government incentives. It found that these contingent factors are highly sector specific and context dependent because they are not replicated in all countries. Cost factors and government incentives, championed as the key factors in previous studies, are relatively insignificant motivations (see Yeung, 1996c). Rather, it is markets and marketing activities that have the greatest power in explaining the investment motivations of HKTNCs.

The study also examined the role of business networks and personal relationships in explaining the processes and mechanisms of southeast Asian operations by HKTNCs (Yeung, forthcoming). First, it argued that institutional barriers arising from changing regulatory regimes in southeast Asia pose formidable challenges to HKTNCs and induce them to enter all sorts of extrafirm political relationships to facilitate transnational operations. Secondly, it contended that the social organization of Chinese business provides the 'institutional thickness' (see Amin and Thrift, 1994) to enable HKTNCs to

take advantage of pre-existing or newly formed interfirm networks of personal and business relationships. The role of *guanxi* or personal relationships is particularly important in the formation of interfirm networks (e.g., joint ventures and co-operative alliances between HKTNCs and their host country partners). Thirdly, the study analysed the role of intrafirm networks which are clearly relevant to wholly or majority-owned subsidiaries. It found that HKTNCs internalize ethnic Chinese entrepreneurship through networks of intrafirm relationships. These findings tended to support the theory based upon iterative abstraction that the TNC is always embedded in dense networks of relationships and it is these network relationships that provide the causal powers to drive the national firm into transnational operations.

## VI Conclusion

Critical realism has begun to gain greater recognition within wider scientific discourses. But to achieve a truly realist conception of science, much work needs to be done to reconcile theoretical and empirical research. Critical realism is neither a philosophy without practice, nor an ideological practice without philosophical bases. The realization of critical realism in social science is certainly a contingent matter. But there is no real reason why such realization should not be possible. In today's human geography, critical realism has gained a stronger foothold than it had during the early 1980s. More research in human geography adopts an implicit or explicit realist ontology. Realist philosophy provides a valuable guide to research in human geography because 'it holds out the hope of healing the division between theorists and empirical workers' (Sarre, 1987: 10).<sup>7</sup> Such a diffusion of philosophy is healthy, but should not be overemphasized for critical realism is, in its essence, a philosophy for and of social science (including human geography). If human geography is to establish itself as a practically adequate discipline within the social sciences, it must take into serious consideration the philosophical and practical challenges posed by critical realism.

Having said that, critical realism is still largely a philosophy in search of a method. Both philosophy and method are critical in realist research in human geography. It is one thing to get your philosophy (or whatever) 'right'; it is yet another thing to use a combination of appropriate methods. Critical realism needs such an appropriate combination of methods to conduct concrete research. A philosophy is sound *only if* it guides the selection of methods in carrying out empirical research. This article has shed light on some methodological guidelines and discussed their surrounding issues. These methodological guidelines, in the names of iterative abstraction, the grounded theory method and triangulation, are not meant to be prescriptive; they are targeted towards a better appreciation of the complexity of ongoing research processes in social science. They are also evaluated, in the spirit of critical realism, by an immanent critique. The moment has come to execute some, if not all, of these guidelines in concrete research. What is missing badly in the existing realist practice in human geography is how such concrete research is *actually* conducted to examine generative mechanisms and contextual contingency (e.g., Sayer, 1986; Lovering, 1985; 1987; Sarre, 1987; Morgan and Sayer, 1988; Allen and McDowell, 1989; Henderson, 1989; Massey *et al.*, 1992; Sayer and Walker, 1992; Watts and Bohle, 1993; Pratt, 1994; 1995). The future of a critical and emancipatory human geography is still more of a dream than a reality. Reclaiming such a geographical reality is the ultimate goal of this methodological essay.

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## Notes

1. Because of extremely limited space, this article restricts its discussion to the Bhaskarian version of realism (and Andrew Sayer's version of critical realism in human geography) not only because it is the most accepted version in the philosophical literature but also because it can serve as useful philosophical underpinnings for empirical research in human geography. See Harré (1986) for an excellent discussion of other versions of realism and anti-realism.

2. See, for example, a philosophical article by Magill (1994) who argues that critical realism is useless for the social sciences because it poses itself as an overarching philosophical ontology. I dispute such a 'dogmatic' view of critical realism and its subsequent dismissal of the role of realist philosophy as an 'under-labourer' for the social sciences.

3. See Horvath and Gibson (1984) for a detailed exposition of the four levels of abstraction in Marx's method. Cox and Mair (1989) also offer a non-Marxist treatment of the different levels of abstraction in locality research.

4. I am very grateful to Andy Pratt for this important point. See his work for further examples of how the method is used and the caveates needed (Pratt, 1994; 1995).

5. Silverman (1993: ix) has labelled this failure of the analytic nerve as 'abstract empiricism'.

6. The original study was much more iterative and dynamic than is reported in this article. Given the constraints of textual representation, the study is presented in an unilinear fashion in this article. At appropriate times, however, I will try to link different parts of the study to form a coherent whole. There is also a danger of using reconstructed logic in that much of the messiness of the original study is assumed away. As far as possible, the methodological problems of the study are presented as well.

7. See Lawson (1989; 1995) for a similar defence of realist philosophy in contemporary economics.

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