Slide 1

On developing an
understanding of
approaches to research

Dr. Jens J. Hansen
Flexibleplus
www.flexibleplus.com

Frequently people who are involved with research just *hive off* and get on with it. They work at their research in fits, starts and restarts and almost invariably, they show concerned awareness of the ever-present exigencies of time restraints and the sombre realities of resource constraints. Such behaviour is manifest for both seasoned research veterans and neophytes. Indeed, both experienced and novice investigators typically have a reasonable idea of what it is that they think they want to find out about – they know that they want to find out about a particular phenomenon because they are interested in it and they believe, intuitively that researching that phenomenon is essential – to themselves at least! Typically also, they have some pretty good ideas about how they might usefully proceed so that they can complete their research challenge in a timely manner (bearing in mind, of course, the relative absence of resources that will undoubtedly impact upon the calibre of their hastily completed report). They know that they should probably *cobble* together some kind of survey, conduct some interviews and perhaps a focus group or three. And then, *abracadabra*, a set of findings will magically have been achieved!

Reality, however, often meanders a rather different route to that described above. Researchers are frequently reflective of, or at least indicative of, the forms of research apprenticeship they have undertaken or, in some cases, endured. This means that as graduating apprentices, they remain prone for a period of time of indeterminant length to completing research projects in a manner that is pretty much akin to ways demonstrated to them by their teachers. Thus, those who have been taught how to conduct careful observations as a useful means of gathering data, tend, in good faith, to perpetuate that data gathering approach. Similarly, if they emerged from a discipline in which surveys were a preferred tool for discovering the mundane as well as the various facts that an exciting saga make, they adhere to that approach. Equally, if researchers have been produced by a school in which, say, action research is strongly favoured, they tend to champion that approach in their research practice. Only with time and experience do they evolve.

This paper, which accompanies a very complexly layered slide show, proposes that researchers should have at least a passing familiarity with a range of approaches to research and those approaches are <u>introduced</u> here. (Note the term introduced has been emphasized.) Because of the complexity of some of the slides, these notes should be read in tandem with viewing, and reviewing if necessary, the story that literally unfolds within

the slides. They traverse the notion that ideas, even if contestable, become subsumed; they propose that ideas are molded by prior knowledge, and this in turn shapes the ways in which we perceive new knowledge; and they indicate that there are at least four broad approaches that can be harnessed by researchers in their quest for achieving rigor. And then there are other factors to consider which become added to the mix.

At the outset of this commentary, I want to acknowledge Esther Cowley-Malcolm who urged me to construct this presentation and to write it up. Many colleagues and students have proposed this before Esther but her arguments that researchers should have something more definitive than a spoken commentary, no matter how scintilating, finally stirred me into productivity. I also want to acknowledge former colleagues from the Sociology Department within the University of Auckland (Dr. Ivanica Vadonovitch and Dr. Murray Pereira) who between them gave voice during my formative academic years to some of the constructs presented but which have evolved over time in my own thinking.
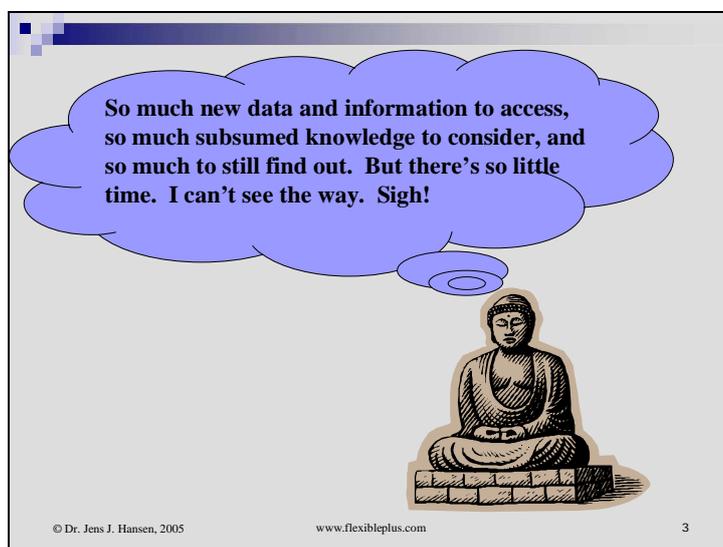
Slide 2
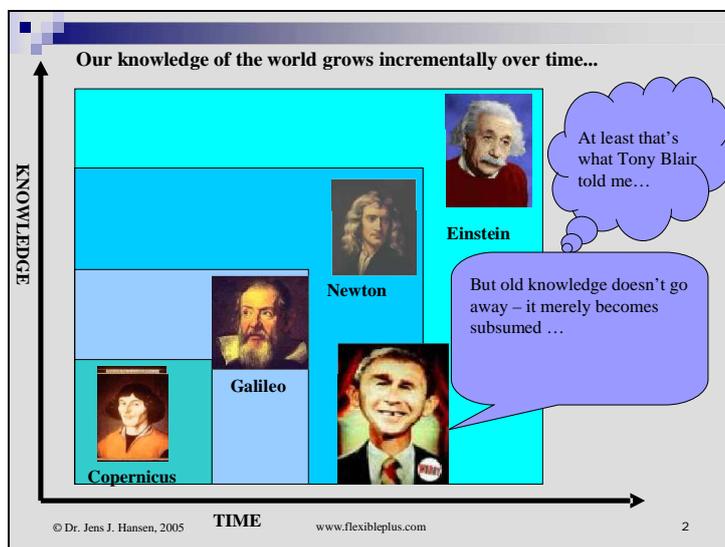
This slide shows how knowledge and some of the core theories about science have *morphed* over time. The point is made, however, that even though one set of beliefs may become overtaken by an alternative or fresh worldview, previously held clusters of beliefs do not disappear – instead they becomes subsumed.

Data are symbols representing facts that exist in isolation. When data items become interlinked, or distilled into meaningful sequences they become transformed into information (see Davidson & Voss, 2002, p.75). In turn, when clusters of information are interpreted and/or applied, generally in order to heighten understanding, they become further metamorphosed - they become transformed into what we think of as *knowledge*.

Nonetheless, whatever we think we know is probably going to be contested at some point or other in time. (This applies even when whatever we think we know is believed to such an extent that we hold it to be an incontrovertible and absolute truth!) And whenever previously accepted tenets of understanding, or even canons of belief, become challenged to such an extent that knowledge and beliefs about knowledge become transformed, a fresh order of appreciation eventually emerges. Thus embryonic paradigms are at once a consequence of reviewed ways of thinking about knowledge and an artifact of altered appreciations of ways of knowing. Paradigms are essentially, therefore, the blended product of knowledge revision/acquisition and thinking syntheses.

Even though it is hackneyed to say so, it remains correct to note that while knowledge is in a state of steady flux, the sum of what we know in the form of data, information and knowledge grows continuously, some would say even incessantly, across time. Simultaneously, however, data and information seepage occurs. This means that knowledge which hitherto may have been thought of as robust (i.e. as normatively accepted as valid and true), becomes jettisoned. In short, data, information and knowledge which become perceived to be somehow wrong, or irrelevant, or even unpalatable, gradually and inexorably become dispensed with. But even though *private* knowledge may terminate only when the individual holder of that specific hush-hush knowledge ceases to exist, public knowledge, however moribund it may become, never actually disappears. It does not become deleted as long as it is public. Instead it becomes subsumed.

Slide 3

The progressions shown in the previous slide provided a severely truncated epistemological account of the evolution of scientific knowledge. Because the account was historical it did not introduce or discuss ways of approaching research which, after all, is what this series of slides and accompanying notes are about. Hence, this slide serves as a bridge and as an introduction into such a discussion.

Knowledge, however it is defined, remains tantamount to blocks of materials which researchers use when plying and applying their craft. Some information and knowledge is informed by <u>descriptive data</u>. Such data provide details that are distilled and assembled into information that provide an explanation about *what* is happening. But while descriptive data yield information that provides an account of attributes, categories or events, such data do not necessarily explain *why* something is happening. Explaining why something is happening requires a <u>causal explanation</u> which, *ipso facto,* attempts to elucidate *why* a phenomenon is, or is not, occurring. And whenever such explanations are not absolute, that is, whenever they are probabilistic in nature, they are posited within what we understand to be the realm of the social sciences. (Of course, causal explanations sometimes derive from false logic or syllogisms. For instance, the Germans and the Japanese drink more beer than the English and yet more English folk die of heart attacks. Also, the French and the Italians drink more wine than the English and have fewer coronaries. Hence it could be concluded that speaking English causes heart attacks!)

So what then, holds those blocks of research explanation together? In a nutshell, I would argue that the mortar of research is a blend of theory and methodology. Theory, in a sense, is the articulation of understandings concerning knowledge configurations. A central task of theory, therefore, is to accord one or more acceptable explanations about *what* and *why* something is occurring. The nub of the matter is to clarify what and why something, is, or is not happening as the case may be. Thus theories provide researchers with a supportive bulwark for explaining their findings. Theory enables them, as we shall see, to either challenge, or extend or even develop innovative explanations concerning whatever it is that they have studied.

Methodologies, on the other hand, have to do with why one or more particular approaches are being used in order to effectively undertake research into a nominated phenomenon or cluster of phenomena. Whereas methods refer to procedures, methodologies are the justification for adopting a particular stance when undertaking research. Methodologies are not only informed by the challenge (or challenges) confronting the investigator, but also by the nature of what we do, or do not know about the matter being researched. In this presentation it is argued that researchers need to address the imperative of making sense of what is known and simultaneously they must devise, with some degree of certainty, ways of establishing what is not yet known. That means they must make a foray into the dominions of theory and methodology although not everyone is terribly *au fait* with these realms.

Slide 4

Is it really the case that researchers are pretty vague about what they are seeking to achieve and equally, that they are unclear about which tools they might deploy in order to facilitate their research endeavors? Is it true that time-lines are frequently extended and/or distorted? And is also true that many investigations are atheoretical? Why do you agree or disagree with each of these contentions? In your view, what is the reality of the research culture within your setting?

Indeed, can you figure out the logic for this slide? (It's hardly rocket science.) Quite clearly, what is being claimed, at least in the initial bullet point, is that researchers are often unclear about precisely what it is that they want to investigate. They generally have what amounts to a pretty vague idea of what it is that they wish to study but being absolutely definitive about what it is that is being explored remains somehow strangely

elusive. Is this a reasonable claim to make in your view? If so, why? If not, why not? Now apply the claim to your own research.

Stemming from this is the second claim – namely – that researchers 'sort of' know what tools to engage irrespective of the outcomes they want to achieve. Think once more about just how reasonable or unreasonable this assertion is? Why is it a fair or an unfair claim? The nub of the matter is that once research objectives/ questions/ hypotheses and rationale have been clarified and justified, the choice of tools which can be appropriately selected will become a pretty straight forward matter. In other words, determining which research tools and investigation procedures will be deployed before the research context, rationale and aims have been elucidated, may lead to an inappropriate choice of methodologies and methods. (For example, if you decide to use a problem centred methodology such as *action research* for investigating how to make a really good process work even better, you might be starting off on entirely the wrong foot. This is so because instead of opting for action research, you could /should more appropriately choose to undertake an *appreciative inquiry*.) The principal question to ask yourself is how do you fare here insofar as your most recent study is concerned?

Raising the issue of time constraints in this slide could be likened to introducing a furphy. But it is not at all absurd to raise the matter of time because, when we seriously plan research, and when we earnestly prepare research budgets, time inevitably looms as an important consideration. Given that this is the case, understanding the overall nature of the research undertaking is especially important which primarily means understanding the various methodological approaches that can be chosen. It is especially relevant whenever investigators are *guesstimating* the amount of time and other resources that should be allocated to a project.

Finally, we come to the matter of theory. If theory can be likened to arrows, how many theories are there in your research quiver? What are the key characteristics of the various broad approaches that are available to us as researchers? If we were to categorize these, what might they look like and how will we know what is useful? Indeed, what, if any research *fashions* or *fads* might influence our ways of critically appraising broad approaches to research? That's what we'll look at next.

Slide 5

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There is an irony to this slide and indeed to the next one as well. The irony is that between the two, the confused reality of approaches to research become quite apparent in the still pictures that are shown. They are a mess and yet, when they are played in sequence, they do actually tell a story. And so it is with research – there is fluidity between the approaches that I have outlined. They are not static.

This slide reinforces the previously made assertion that the social milieux of a period do much to shape the accepted thinking, and even the accepted dogma, of that particular period era. Such thinking and dogma stem from the actions of individuals, and/or political movements and/or organisations. Furthermore, they range from utterly devastating chapters of history through to a recognition that the work of individuals can be momentous.

The slide illustrates both negative and positive examples in contradistinction (viz - the atrocities that occurred during the Inquisition versus the saintly work of Mother Theresa; Adolph Hitler, the man who initiated and sanctioned the genocide that occurred in Nazi Germany as opposed to Paulo Freire, the man who sought to liberate and *consciensatize* and politicise the masses by proliferating adult literacy; Senator Joe McCarthy who led a campaign of persecutions in the USA of anyone suspected of being *un-American* in contrast to Bob Geldoff, the rock star who led a campaign of using music to raise money to alleviate famine and poverty; and, finally, Robert Mugabe, the precipitator of mass murders in Zimbabwe in comparison to Martin Luther King, the Highlander Centre participant who initiated mass civil rights rallies.

There are, of course, many more examples that could be described here, but the point is made that the positive, as well as the negative, the good as well as the bad seem to characterise the behaviours of those who inhabit the human zoo. The habitats of these folk are to be found on all four corners of the globe as well as at points in between. Location aside, it follows that if the knowledge and thinking processes by individuals and/or political movements and/or organisations become reinforced by robust (quality) research, then the power base of those agencies may also have become strengthened. Thus where research is geared towards enhancing power as an end in itself, the matter of research ethics becomes crucial.

There are, at the outset, two broad categories of research, i.e. quantitative and qualitative and, as a third hybrid category, there is mixed mode research which literally is a mixture of the two to a greater or lesser extent. These categories, although useful, are overly simple. Hence, as we will see in the next two slides, a more complete system of categorising approaches to research merits consideration.

Slide 6
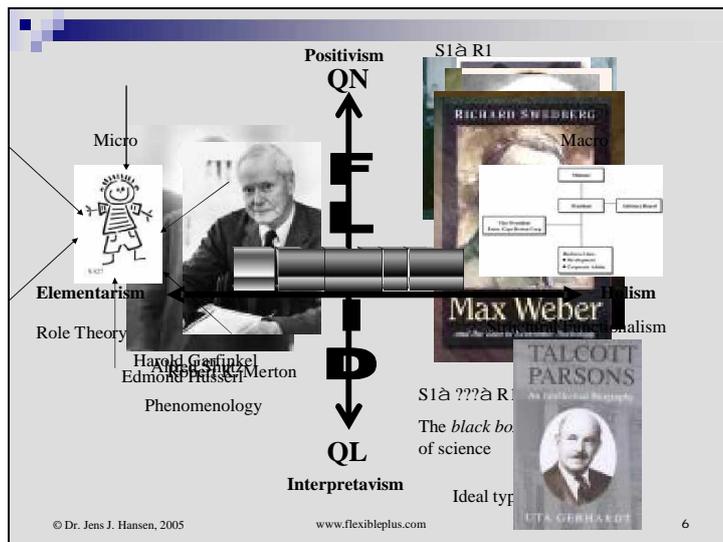
This is by far the most complex of the slides in this series although the story that is being told is relatively straight forward. Quite simply, the overlaid slide elements introduce four broad approaches to categorising research. It further proposes that blends of these are not only possible, but are also commonplace. Finally, this slide suggests that ways in which we perceive a researchable phenomenon precipitate a sort of *research methods determinism* – that is – we view an issue, question or hypothesis to be researched through eyes that are drawn inexorably to our personal academic and/or research creed. And yet, it is also proposed that the entire spectrum is fluid.

In order to understand this complex *mess*, we need to examine the sequences in which the slides are presented. Elements of the first cluster appear around the twelve o'clock setting and identify positivistic approaches to research. This involves gathering and processing numerical or quantitative data. The pursuit of positivism was all about seeking to quantify that which could be observed and measured and proving things empirically drove research throughout most of human history as we know it. Greek, Roman, Persian and Chinese scholars were intent upon proving things mathematically; alchemists from the middle ages were hell-bent upon creating a chemical *golden goose* by devising formulae that would enable them to produce gold from all manner of curious chemicals and substances.

But in conjunction with the seminal treatise by John Stuart Mills on cannons of proof, the probabilistic nature of social sciences gradually became celebrated as legitimately cohering with the still dominant quantitative research paradigm. Thus Frenchmen Auguste Comte, the father of sociology, pursued the *science* of society and his illustrious countryman, Emile Durkheim, sought to measure, or more precisely, sought to quantify, social variables in order to explain suicide. Later, a succession of empirical psychologists (Pavlov, Watson and Skinner) set about measuring and quantifying things psychological. Let's agree here that a complete series of stories could usefully be drawn from the literature about these folk but neither time, nor space, nor the current agenda encourage that. The point is, though, that most early theorists were mainly concerned with measuring *dependent* and *independent* variables.

By contrast, at the six o'clock position the qualitative approach is illustrated. Here, the important slide element to note is that it was recognised by scientists that you cannot measure what is going on inside peoples' minds (the *black box* problem of science). Thus

the approach was to interpret what people said about what they mainly felt (opinion) or remembered as actually having happened (critical instance). The slide shows Marx (conflict theory), Webber (the conceiver the notions of both the *ideal type* and *bureaucracies*) as well as a raft of theorists who were interested in interpreting what was going on beneath the surface (these were the *phenomenologists*). The slide then flits to the nine o'clock position where it unfolds Elementarism, a micro research focus that analyses role theory (Merton) and finally to the opposite pole where a macro approach to understanding organisations is depicted as Holism (Talcott Parsons). These last two poles illustrate content emphases whereas the first two are emphasise processes. Fluidity, though, remains.

Slide 7
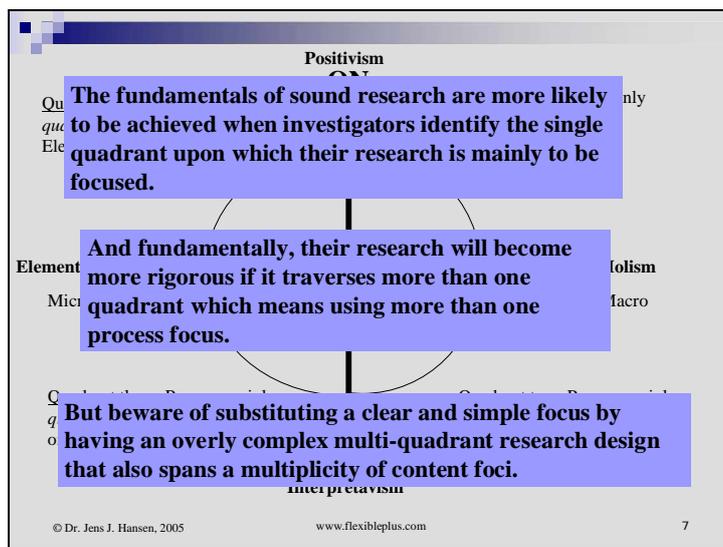
So what it all comes down to, when two millennia have been distilled into a few minutes of time, is that there are four simple quadrants in this diagram/slide which can be thought of as denoting two intersecting continua of research foci. The horizontal continuum denotes the researcher's concern with scale – micro versus macro, elements as opposed to the gestalt. The perpendicular continuum spans processes – measuring the observable and quantifiable in contrast to interpreting and/or reporting on what has been said or has been seen insofar as non-measurable phenomena are concerned.

Two points need to be made very clearly here. First, it is acknowledged and agreed that a simple model such as this can very easily be deconstructed by those who are knowledgeable about the field of theory and research procedures. But the point of a model is that it should serve as a useful heuristic for heightening awareness and thus the model should be seen as a tool for growing our understanding of the broad nature of research. However, at the same time, and as a second point, it must also be noted that any number of blends of process approaches and content foci are possible. The point is that researchers can use a model such as this to understand the broad thrust of what it is that they are doing and that, frankly, affords an improvement to the status quo!

Beyond that, the slide also proposes that rigour, or thoroughness (not completeness) is more likely to be achieved if researchers intentionally invoke a design in which they address more than one of the quadrants shown. However, a word of warning is also given – namely, avoid being too ambitious. There is a risk that by ambitiously seeking to traverse too much content, by admitting too many subtopics, by asking too many

questions, the all important notion of specificity will be lost. Research is *mainly* about determining relative unambiguous investigation parameters and ensuring a *mainly* straight forward and clearly defined focus. Note that the word mainly has been used because in reality, social research tends to never quite be as straight forward as we may wish it to be. But then, if that were the case, we would know it all and we would lose the disputations that are born of contestable research findings!

So this diagram should be construed simply. It is not ambitious. It simply provides a model that researchers can use in order to, broadly speaking, understand the thrust of what it is that they think they are doing. But as the next slide shows, we should avoid being overly complex in our designs…

Slide 8
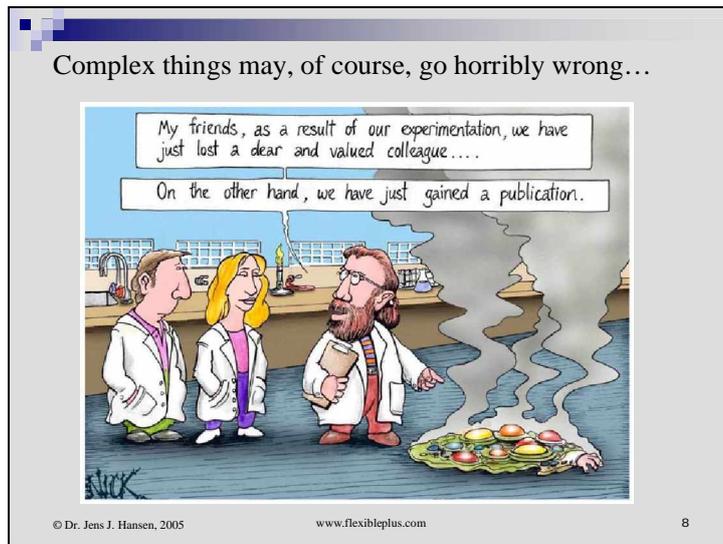
Humour aside, a serious message can also be derived from this cartoon. Quite simply, it is that even though researchers inevitably seek to achieve as near to perfect an outcome (and outputs) as they possibly can, they do not always succeed. Consequently, two things often happen. First, and in my view questionably, researchers are often inclined to modify their questions, aims, hypotheses to suit their results and/or findings. While making such adjustments may ensure that there is a self-fulfilling outcome to the research, the results and/or findings are, strictly speaking not a truthful reflection of reality because the foundation goals of the research have become distorted to suit the researcher. A pall, therefore, shrouds the results and/or findings even though the ethical dubiousness of the practice may have been masked and all that has bee reported about the research appears to be robust. (Note that the term *results,* is perhaps, the best term to link to quantitative end-products whereas the term *findings* is, perhaps, the more salient term to use when presenting qualitative discoveries. Of course, for many writers, there is no distinction because, clearly, it remains a matter of choice. However, the differentiation seems, to me at least, to make a modicum of sense and it is a point that I encourage researchers to consider.)

But second, if we are honest in our research, and in our efforts at representing the reality of investigations truthfully, we should actually celebrate the opportunities to report variabilities. Indeed, it remains incontrovertible that we report on the positive things that our research discloses. But we should also report on what we find that might not be so glossy and we should also comment upon that which we did not find, and yet had possibly

expected to find. It is through such discussion that we can hint at whatever discrepancies may have arisen between our own and previous studies. Through such engagement, we can also comment upon possible incongruities that may exist between what we have discovered and the literature and theory which hitherto has informed us. By engaging with prior scholarship and our own current research, we can contribute at least a little to what should be ongoing dialogue between researchers and scholars about ideas which are, after all, manifestations of knowledge configurations. Furthermore, this process enables researchers to either develop new theory (i.e. to provide *what* explanations as well as *why* explanations and even scientific, *universal law* explanations of a Hempelian nature) or to formulate extensions or revisions to existing theory. It may even enable us to contest established dogma!

But the key point, then, is that we must avoid tickling the research till so to speak – we must not fudge the ledgers. Indeed, it may even be advisable to treat the unexpected, the research results that we did not anticipate, as an entrepreneurial opportunity which is why Alexander Flemming was able to give the world penicillin. And it may be equally wise to consider the absence of the expected as a mandate for applying for support in the next funding round!
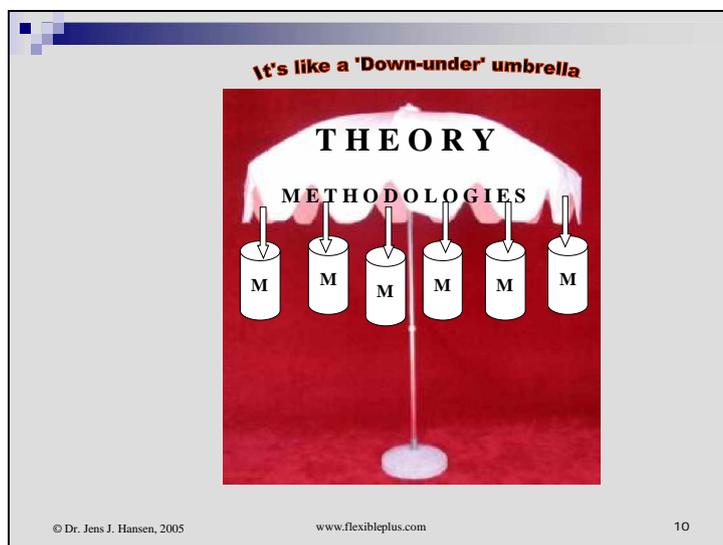
Slide 9

We have now firmly entered the dominion in which research aims, theory, methodology and methods are juxtaposed. That is, each of those dimensions can be, and should be, more or less in union but at the same time, overlaps and intersections will be apparent. These partially merged and fused matters collectively contribute to our emerging understanding of what is being investigated but at the same time, and perhaps even more importantly, they also ignite within us the capacity to ponder 'just what is going on here?'.

This, according to Lyn Richards (2005), is a crucial question to pose irrespective of the actual matter that is being investigated. Explaining that in a more detailed manner means that wondering and asking, "what is going on with data, the information, the knowledge, and the ideas that are involved here?" becomes a fundamental and central question. Indeed, it is contended that this is a question that researchers should adopt as a mantra. Furthermore, it is a mantra that they should repeat often.

Of course, there is more to it than just this. I indicated above that as research evolves, it becomes apparent that there is juxtaposition between a range of research elements – theory, methodologies and procedures. In order to clarify the nature of this proximate interconnectedness, researchers need, ideally, to contemplate separately, issues that pertain to each individual aspect, i.e. they need to focus their thinking on whatever the issues are that specifically arise concerning theory, and methodologies and methods. Each domain should be contemplated separately and in tandem, i.e. they should be reviewed and addressed as discrete elements of the overall research but then, they need to be reviewed with specific regard to the manner in which each separate aspect is linked or conjoined – each to the other – as a whole. In other words, as researchers, we need to cross-tabulate the constituent parts of the research in our thinking about the dynamics that collectively drive the whole.

A word or two about the importance of literature. A critical literature review is absolutely essential if researchers are to be well informed about previous research and thinking. Being familiar with the findings of others; being *au fait* with research commissions and omissions that have occurred within a chosen sphere of interest; and having some knowledge about hitherto unexplored research territories, will almost certainly enable an investigator to add credence to a contemplated research challenge. Moreover, the rationale for a research project almost invariably becomes strengthened when theory permeates. Research which is founded solely on the platitudes and/or the proclamations of those who 'profess' may or may not have merit. But research that is founded on a substantially argued rationale which integrates up-to-date critical analysis of theory is more likely to also devise a methodological approach that is based on substance.

Slide 10

This slide is actually very simple in both construction and conception. It proposes nothing more erudite than to suggest that theory is fundamentally akin to an overarching umbrella that covers all research. (Yes, there are those who believe that they have encountered investigators who are, perchance, atheoretical – but the reality is that practically all academic research can be linked to theory in one way or another. Just how well it is linked may, however, be a moot point!)

It goes without saying that there is reciprocity of influence – just as theory informs ongoing research, so does ongoing research inform theory. Some investigators knowingly,

and sometimes unwittingly, work from an assumed stance whereby their investigation will attempt to give theoretical credence to emergent findings. Their consequent theory (viz. deductive or *grounded* theory) is, therefore, informed by the data. Alternatively, we might operate from the standpoint of intentionally seeking to find answers that conform to a predetermined framework (viz. inductive or *imposed* theory). In such an instance, theory mainly drives the data quest. I say mainly here because increasingly a blend of these two approaches is not only possible, but is also, often consistent with the researcher's intent to demonstrate rigour – or if not that, at least a disposition to flexibility.

In the diagram above, the term methodologies is printed at the base of the umbrella, on the skirt or surround of the main body. This is an intentional location because not only are theory and methodology joined at the hip, they are also inextricably dimensions of the same overall (research) process. Quite clearly, theory informs methodology and equally, methodology should reflect, *a priori*, the logic of whatever theoretical stance the researcher has decided to advance and/or explore. In other words, both the selected methodological approach and the accompanying justification for the engagement of that approach should be consistent with the theoretical framework that has been proposed for the study.

Of course, researchers and research are seldom as 'pristine' as the ideals espoused above promote. More often than not, theory and methodologies seem to assume congruence with the investigator – the researcher merely gives voice to her or his academic predispositions or to the processes with which there is a degree of affinity and a level of comfort. Possibly this even extends to the choice of methods which a researcher makes (e.g. "I like surveys – or focus groups – or whatever – so I use them – often!"). These methods are shown as something faintly resembling corks below the umbrella in the above diagram. This is what makes it consistent with a 'down-under' umbrella! But methods should, ideally, be a logical extension of theory and the accompanying dovetailed methodology. Put as a research adage, it can be proposed that once the aims and attendant theory have been clarified, the methodological approach and the consequent range of possible methods that can be used should become blatantly apparent.

Slide 11

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The previous slide considered the interrelationship, the symbiosis that exists to a lesser or greater extent between theory, methodology and methods or procedures. It was

noted that researchers are naturally inclined towards approaches that fit their comfort zones – they tend to lean towards stratagems with which they are familiar. This, of course, is hardly surprising if it is remembered that research presents as a challenge that transports the majority of scholars beyond their comfort zone. They are not always at ease when contemplating the complexities of how theory and research interact and neither are they always comfortable about ensuring that approaches within their field are rigorous (valid and reliable if you like). It is, after all, a relatively complex matter.

There is another component of variability that must be considered if we are to fully understand how approaches to research intermesh, and that factor is the matter of whether or not research is *applied* or *theoretical*. Pretty well all research is bound to be a blend of both. The extent to which a research project attracts funding can probably be sheeted home to the extent to which applied benefits accrue from the study!

Equally, the degree to which a study is theoretical is probably a reasonable indicant of how likely it is that a study will be published. Notwithstanding this, the issue of quality must not be overlooked and that usually means that some form of appraisal will occur – we live in an era where accountability and peer review have assumed heightened significance and what's more, ours is an era during which these processes have become utterly bureaucratized!

This does not mean, however, that the other approaches to understanding research which have been shown and discussed in this presentation should be cast aside – they remain pertinent because research can be either mainly quantitative or mainly qualitative; and it can be concerned mainly with elements of the whole or with the whole itself. But as we shall see in the next slide, irrespective of the process approach, and regardless of the content focus, and notwithstanding the extent to which the study is applied or theoretical, the research process can be usefully broken down to a series of stages. These, by and large in the social sciences at least, remain pretty much invariant. Despite the constancy of these stages, however, the observation is made that our skills in undertaking and completing each of these phases or stages is variable. We will consider that matter in a while but for now, let me continue to dwell upon applied research and theoretical studies.

Slide 12
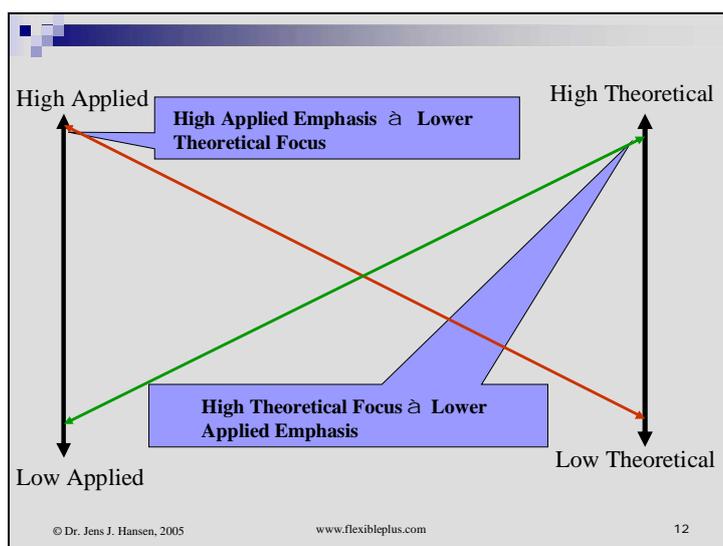
In the previous slide, I made the point that research, quantitative and/or qualitative, can be either predominantly applied or predominantly theoretical and that using that

simple categorical distinction might provide quite a useful aid to appreciating what it is that we are doing as researchers. This slide amplifies that distinction in a very simple manner. The question arises, however, as to why we should even begin to seek to make such a distinction?

There are at least four possible answers here. First, the matter of achieving a more precise research focus might become sharpened if we can provide a reasonable commentary concerning to what extent our goals are grounded in application or steeped in lofty theory. In other words, understanding about the extent to which contemplated research is likely to achieve either applied and/or theoretical outcomes really matters at the point at which that research is moving beyond mere contemplation and into conception mode.

Second and continuing the birth metaphor for a moment, the matter of how proposals are developed during formation and gestation influences their ultimate manifestation – their research persona if you like. If, for instance, a research project looks certain to confirm the benefits of a new health measure, it follows that this is likely to be of obvious practical or applied benefit to a company (i.e. profitable). It is, therefore, more likely (but not entirely improbable) that the company in question will sponsor the contemplated study than were it to be, by contrast, a mainly theoretical discourse on, say, the nature of the health problem coupled with a discussion on why previous approaches might not have not worked. Let me hasten to comment that in this latter instance, it need not follow that the company will be unsupportive – they may very well provide support – but I believe that it remains more likely that such a study will be of interest to an appropriate learned and scholarly journal. In other words, making a judgment call on the extent to which research is either applied or theoretical can help the researcher to divine the most valid target audience to explore.

Third, and very simply, appraising the applied and/or theoretical balances of a project will help the researcher to further heighten their understanding of what it is that they are pursuing. Such an appraisal may inform decisions concerning the selection of sampling strategies, and/or may provide guidance about what scope, if any, their research has for generalisability or for creating a qualitative evidence base that can support a moot or a case.

Finally, appreciating the extent to which a project is likely to be applied in nature or theoretical can provide a useful gauge to the researcher as a writer. The *writing voice* that is assumed should reflect the general thrust of the study but should also, of course, be targeted according so that it has relevance to the audience at which it is aimed.

Slide 13

This slide identifies four relatively invariant phases to the research process and tenders four subjectively evaluative comments for consideration. In particular, the proposition is floated that the weakest link in the research chain pertains to the manner in which we manage data (or rather, don't manage data at all well).

The first comment has to do with research design and here the point is made that researchers should consider the principle of *data before design.* Put another way, this means that when designing an investigation or study, researchers should intentionally and systematically ponder what form the data gathered will take, and even more importantly, they need to determine precisely what they will do with the data. Failure to address this matter typically results in investigators gathering data and then wondering, sometimes pointlessly, just what they will do with those data. Too often, failure to address this matter leads to a surfeit of data – excess information that is not put to practical use but instead, is left to languish in the (unlikely) event that it might be brought to life in future. The principle of *data before design* is, therefore, an important one to remember and even though it may be considered to be more applicable to research that is mainly quantitative, it nevertheless remains relevant for qualitative research as well.

The second stage, data gathering, denotes an activity that most researchers do well – sometimes too well! This means that we do not always curb our enthusiasm for gathering data if only because we are, quite understandably, prompted by curiosity to amass as much information as is offered to us "just in case it is going to be relevant". In reality, it is very difficult to find the balance between gathering focused-only data and collecting data in the hope that they will, somehow, be relevant. In particular, when unbridled enthusiasm operates in parallel with little or no attention being given to what is to be done with the data, the researcher runs into problems. Care and caution are needed so that relevant data are gathered and kept whilst irrelevant data are either not gathered at all, or are not kept for long before becoming jettisoned.

Phases three and four are interlinked (as are all of the phases) but it is phase three, the data management phase that in my experience typically presents as a problem. Just as we need to pay attention at the design stage to the matter of how we are going to treat the data we gather, we also need to take care with our management of these data. While ethics are clearly important here, the main point that is being made is that an actual strategy plan for data management needs to be formulated in tandem with designing the study. We need to

be clear about protocols for handling data, we need to have pre-determined the conventions that are to be followed and this very common sense observation is especially relevant for studies that involve teams. Equally, our writing strategies need prior attention so that we work smarter in the generation of our reports and publications. Marrying multiple styles and conventions is time-consuming and problematic and for that reason alone, pre-determining how a report will be structured, makes considerable sense.

Slide 14
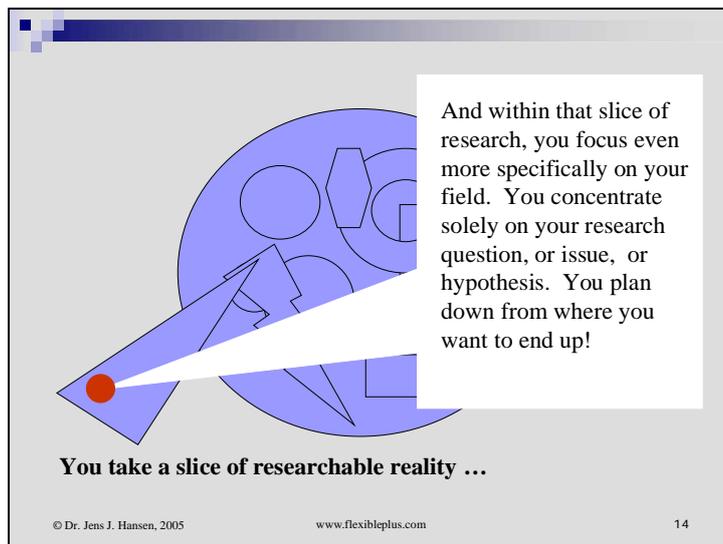
In the previous slide, the matter of research specificity, of being precisely focused was mentioned very briefly – too briefly in fact. It has already been noted, though, that once the research objectives, or the issue, or the hypotheses are clarified, the rest of the exercise ought to become pretty apparent. But finding what is to be the focus can be very challenging indeed. And so this slide attempts to illustrate that challenge.

Basically, within a field that is called knowledge are a suite of academic or scholastic realms that, in one way or another, have become nominated as disciplines (sometimes these have even been self-declared!). It is probably worth briefly considering at this point just what a discipline is. In doing so, however, please remember that in a slide and notes presentation such as this, discussion is only ever cursory at best. At worst, it can of course be quite neglectful by omitting points or by presenting them in an intentionally shallow fashion. That said, let me propose that a discipline is distinguishable because it has a *generally* agreed to study parameters – i.e. it has a specifiable scope. Note that the qualifier *generally* has intentionally been inserted here because often boundaries which nominate what should, or should not be included in a discipline, are hotly contestable.

This is important because the specifiable scope of a discipline impacts upon its knowledge base. Indeed, a second characteristic of a discipline is that it has a body of *relatively* established theory. Again, it is useful to note that the qualifier *relatively* has been added to accent the fluidity of theory. It has also been included if only because a healthy hallmark of a discipline is the presence of ongoing constructive debate between discipline disciples on matters pertaining to theory. In a sense, therefore, the activities of both critiquing and clarifying interpretations about how to make sense of matters that have been studied are a professional responsibility of those who work within a discipline. Hence the term *scholar-practitioner* comes into its own.

How they do that has to do with methodology and methods and so a third criterion for a discipline is that it has a set of accepted procedures for data gathering, data management and data analysis. Note that no qualifiers have been used here because this dimension clearly focuses upon achieving quality whatever that may mean. Indeed, while quality may be a slippery term to define, there is little doubt that any discipline that is worth its salt has a vested interest in achieving rigor insofar as research procedures are concerned. Other hallmarks of a discipline have to do with admission into the hallowed ranks of being a professional (as well as the imposition of sanctions upon those same members for unprofessional conduct) and finally, the vexatious matter of professional charges. However, these matters are not relevant to this discussion. Furthermore, irrespective of these characteristics, it remains paramount, no matter what the profession, that clearly and specifically articulating the focus of whatever is to be investigated remains an ever important constant. It is, in fact, an essential condition for the successful completion of quality research.

Slide 15
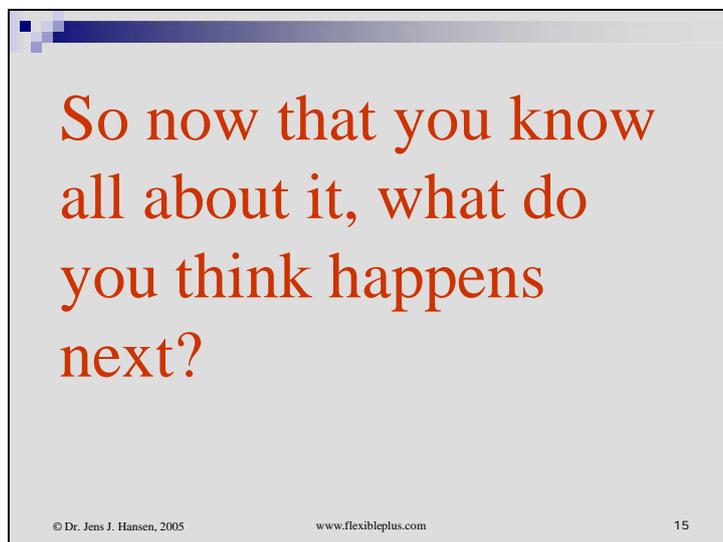
So now that you know all about it, what do you think happens next?

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I've used this slide as a fill-in to get you to review, just for a minute, what has transpired so far. What, in your mind, are the three or four key themes that have emerged? What, if any, relevance might any part of this slide show have with respect to the work with which you are currently involved? What have you learnt? What might you still need to learn?

I suggest that you make an effort to answer those questions. And then try to nominate what, if any, other questions I should have posed. In other words, I'm suggesting that you intentionally complete your personal critical review of this presentation by nominating and answering other questions of relevance.

Slide 16
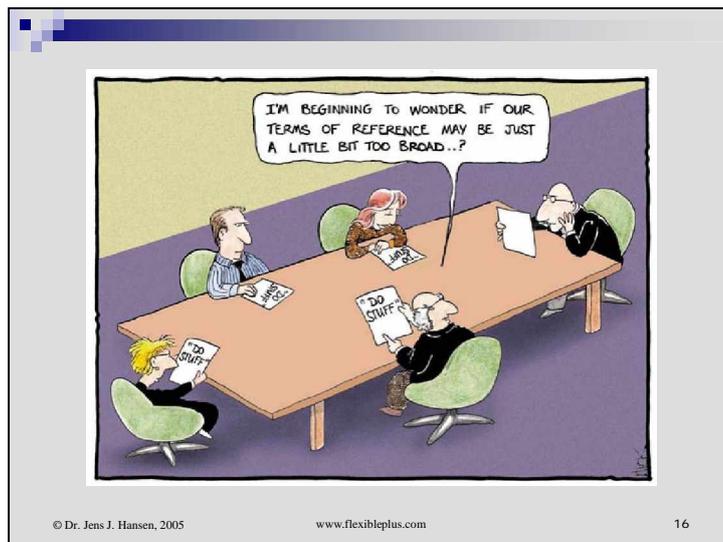
It is tempting to use a slide such as this as a launching pad for discussing, once more, the need to generate research aims, and questions and/or hypotheses that are highly specific. But realistically, there should be little need for this. What could be useful, however, is to encourage you to use the principles that have been proposed in this presentation as a template for reviewing a study you are undertaking. I'm suggesting that you could actually set up a matrix of variables in which aspects of your study become analyzed that you have decreed to be crucial. To achieve this you simply need to nominate key aspects of your study by rows. You then need to create columns for each of the four forms of research process/content presented in slide numbers seven and eight and for the added variables of applied and theoretical. Why not use a piece of paper to trial this approach? And then, when you have completed that exercise, make a comment or two about how useful, or irksome, you found the process to be. What do you conclude?

Slide 17
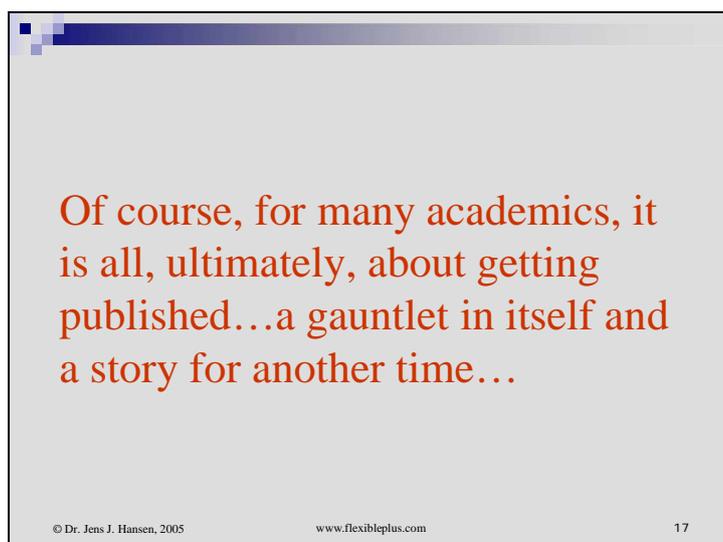
I'm not about to launch into another story here because that really is for another time. However, it may interest some of you to know that I have written a slide show that is, in many ways, similar to this one. It is about preparing and producing a critical literature review. Accompanying that is a one-pager that I have prepared which proposes a model for writing effectively. It is known simply as *Hansen's APT model for Writing* and either or both of these contributions can be obtained for free as email attachments simply by contacting me. Just email me at jens@flexibleplus.com

Slide 18
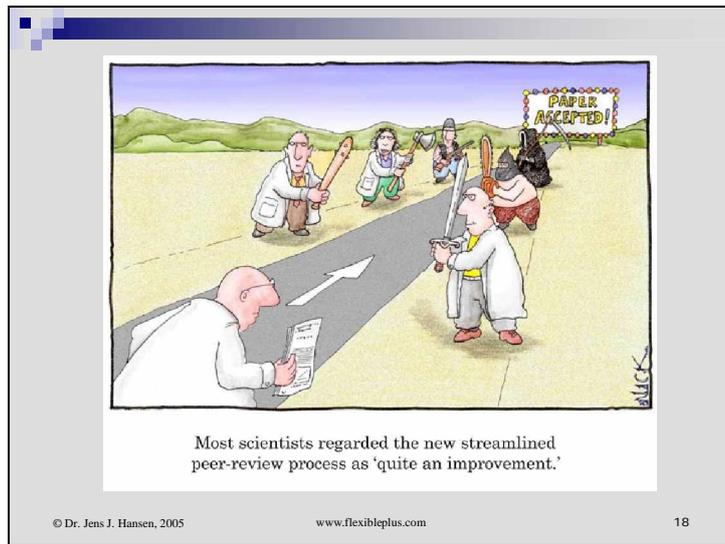
Because this is a first cut at preparing this presentation and accompanying notes, I've not put any citations in here – at least not yet. I intend to do so in the fullness of time. But first, I'd be keen to receive any feed-back that you might feel inclined to provide for me.

Again, please do so by contacting me via email jens@flexibleplus.com