UNIVERSITY OF VAASA

Faculty of Philosophy

Communication Studies

Antonín Kadlčík

Farewell to Media Studies as a Science

An Analysis and Critique of Media Studies, Human Sciences and their Methodology

Master's Thesis

Vaasa 2011

CONTENTS

VAASAN YLIOPISTO
Filosofinen tiedekunta

Tekijä:	Antonín Kadlčík
Pro gradu –tutkielma:	Farewell to Media Studies as a Science An Analysis and Critique of Media Studies, Human Sciences and their Methodology
Tutkinto:	Filosofian maisteri
Oppiaine:	Viestintätieteet
Valmistumisvuosi:	2011
Työn ohjaaja:	Tarmo Malmberg

TIIVISTELMÄ (ABSTRACT):

Ihmistieteissä on jatkunut jo pitkään kahden tiedenäkemyksen vastakkainasettelu. Dikotomiat positivismi–hermeneutiikka ja kvantitatiivinen–kvalitatiivinen ovat tuon vastakkainasettelun muunnelmia. Niiden ydinajatukset ovat säilyneet samoina, vaikka ne aika ajoin esiintyvät ikään kuin uusina. Tämän työn keskeinen kanta on se, että hermeneuttinen ja kvalitatiivinen tutkimusote sekä niiden uudemmat muunnelmat kuten postmodernismi ja sosiaalinen konstruktionismi ovat tieteenfilosofisesti ja käytännöllisesti kestämättömiä. Kysymys ei ole niinkään siitä, mikä on ”ainut ja oikea” kanta, vaan siitä mikä toimii paremmin ja mikä on paremmin perusteltu.

Tarkoituksena on tarkastella, miten mediatutkimus sopii tähän kuvaan. Tämä tapahtuu vertailemalla mediatutkimusta suhteessa muihin ihmistieteisiin sekä niissä harjoitettuun tieteenfilosofiseen argumentaatioon. Tällainen laaja tarkastelu on sikäli oikeutettua, että mediatutkimuksen metodologia ei ole ainutlaatuinen eikä se ole syntynyt itsestään itseään varten. Se on päinvastoin läpikyllästetty niin ihmistieteiden kuin yleisen tieteenfilosofian ja tieteen käytännöillä. Mediatutkimuksen tarkastelussa on siis välttämättä otettava huomioon laajempi kokonaisuus.

Mediatutkimuksen analyysi perustuu tässä tutkimuksessa kahteen osaan: mediatutkimuksesta alana otettuihin esimerkkeihin ja Vaasan yliopiston mediatutkimuksesta tehtyjen pro gradu -töiden tarkasteluun. Osittain on siis kyse case-tutkimuksesta, jossa Vaasan yliopisto on tarkasteltu tapaus. Testattavana hypoteesina on oletus, että pro gradu -töiden tieteellinen taso ja arvosana korreloivat keskenään. Vaikka eroja löytyi siinä, miten esimerkiksi eri ajatuksia kehiteltiin ja käsiteltiin, ei tieteellisen argumentaation, tieteellisten periaatteiden, toisin sanoen tieteellisyyden sinänsä tasolla eroa pystytty havaitsemaan. Lisäksi pro gradu -työt eivät yleisesti ottaen täyttäneet tieteellisyyden vaatimuksia, vaikka arvosanan perusteella olisi syytä olettaa toisin. Jos yhtäältä pro gradu -työt eivät täyttäneet ”perinteisen” (positivistisen, kvantitatiivisen) tiedenäkemyksen kriteerejä, ne toisaalta vastasivat hermeneuttis–kvalitatiivisen jne. ihmistiedesuuntauksen ihanteita.

AVAINSANAT: tieteenfilosofia, postmoderni, positivismi, sisällönanalyysi, feminismi

1 INTRODUCTION

Brubacher (1977: 1) described the state of American education with the following words:

> At present, to borrow a phrase from Arnold Toynbee, American higher education seems to be in a "time of troubles." From one standpoint this is nothing new. Higher education has faced grumbling demands for change since the beginning of this century—and even further back. Most of the changes demanded have been for new means to achieve old and well-accepted ends. Recently, however, there has been disagreement about ends, about the underlying philosophy of higher education itself. When there is doubt about the fundamental frame of reference, there is real trouble. Indeed, some have gone so far as to speak of an "identity crisis"...

I think that we can substitute the words “higher education” with “social sciences”, “humanities”, or “media studies” and the message, its importance, would remain the same. This is nothing new, of course, as the philosophical discussion, especially the “rivalry” between the two opposing camps, hermeneutics (or qualitative) and positivism (or quantitative), has shown. Unfortunately, lately, the discussion has not really been a discussion as such, rather it has been a one-sided “name-calling” where the core issue, doing good science, has been forgotten. Science has been replaced with different fashion trends and “epistemological flavours of the month”. The origins of this discussion – as mostly anything philosophical – can be traced to the Ancient Greece, especially Aristotle and his teleology which Fearn (2001: 41) describes as the

> notion that the present could be understood by reference to the future. The nature of a thing - be it an acorn or a man - was inextricably linked to its *telos,* its goal or final end. The final end of an object informs its nature, and that nature subsequently drives it towards its goal. ... Human beings too had a final end and, if we could understand what it was, we would be all the better equipped to achieve it.

The Aristotelian view of epistemology dominated science, at least officially, until the Renaissance when natural sciences, mathematics, and philosophy of science witnessed a decisive transformation. This change did not come “out of nowhere”, of course, but the changes of scientific conduct cannot be overlooked. Such names like Galileo, Descartes, Bacon, among others, became the driving force of “new” science; one distancing itself from teleological explanations of the world.

This turning point in science, however, did not lead to a "unification" of all scientific endeavour. On the one hand, there were the "practising" scientists who at least implicitly followed what is traditionally considered the "scientific method". This is the sphere of history of science which tells what has been done. On the other hand, there were the philosophers of science – of whom many were also practicing scientists – who wrote about what ought to be done, i.e., what science should be like, limitations of science, and what is the nature of this or that scientific field.

Natural sciences went their own way while most of the then, and present, controversies concerned the then emerging social sciences, history, or simply, the non-natural sciences that more or less tried to study the societal, whether it was the past, present, or future. Traditionally English philosophy of science was empirically oriented. Empiricism, in this sense, did not mean a mechanistic observation or experience. Rather, experience was considered as necessary "inspiration" for our ideas. This is evident, for instance, in Hume's writings about cause and effect. According to Hume (1955: 27), "no object ever discovers, by the qualities which appear to the sense, either the causes which produced it, or the effects which will arise from it; nor can our reason, unassisted by experience, ever draw any inference concerning real existence and matter of fact." This means that, according to him, there are no synthetic a priori judgements. He continues (ibid.) that "Adam, though his rational faculties be supposed, at the very first, entirely perfect, could not have inferred from the fluidity and transparency of water that it would suffocate him..." Instead of being a naive, and one-sided opposition to rationalism, at least Hume's version of empiricism is quite similar to Comte's (the father of positivism) idea of the "core" method of science: "reasoning and observing, correctly combined" (Töttö, 1996: 77). Herbert Spencer, The English sociologist, or social philosopher, continued this line of thought. For him sociology did not differ from other sciences in spirit, rather, in approachability of the object, for instance, to what extent social phenomena can be "measurable" (Turner, 1989: 18, 22). This general approachability was contested during Spencer's time as it is being contested even today by many literary critics, historians, academic feminists, and people generally not categorisable as practitioners of the "hard" sciences. While this kind of opposition is certainly possible, in the same sense as jumping from a cliff is possible, it would lead to a difficulty that Spencer noticed and which is rather awkward if or when the present state of the humanities or social sciences are honestly evaluated. Spencer's position, Turner (ibid.: 19) writes, was that

> there are many who refuse to believe that the social world reveals regularities that can be understood an stated in lawlike ways. If such is the case, sociology is unnecessary, and those who do not believe that the world reveals regularities should retire or write opinion columns for newspapers.

There can be no misconceptions about this point: if a scholar, academic, or anyone else who claims to do research, denies the tenets of science, then the only sensible thing to do is to withdraw from scientific activity altogether. Anything else would be hypocrisy, charlatanism, or any form of dishonesty. Nonetheless, despite the claims opposite to the established – and established for very good reasons - canons of at least 300 years worth of science, their argumentative power has been somewhat lacking. Thus, the questions of whether or not society contains lawlike regularities, if they can be researched, and with what methodology, are the main issues of the debate today as they have been in the 1930s and during Spencer's time.

But it is, in fact, the German speaking part of the world where the "scientific method" broke into three different approaches, or traditions. Natural sciences and mathematics continued to proceed along the lines of Galileo. From the philosophical point of view, perhaps the most important evolution regarded mathematics and logic which through Leibniz, then (much) later, Schröder, Hilbert, and Frege, culminated in what was to become the most important philosophical thought of science, logical positivism. This was a combination of both German and English "mathematical logic" or "logical mathematics", Comte's positivism and empiricism.

The third tradition, which we can call social sciences from the present perspective, can be further separated into two paths which overlapped each other in many ways, although, in many other, they were distinct. One was German historicism – including such names as Droysen, von Ranke, Savigny, Niebuhr, etc. – which, according to Kusch (1986: 46), contained two phases. In the first, proponents of historicism opposed, especially regarding the societal, everything ahistorical, i.e., lawlike regularities of society that were thought to be independent of history. The supposed “uniqueness” of the historical objects and, especially, that “history was taken as the normative basis of thought and action”, led to the second phase of historicism, a form of relativism. (ibid.: 46-48.) Popper (1966: 5) summarised the idea historicism similarly, that it “denies that the regularities detectable in social life have the character of the immutable regularities of the physical world. For they depend upon history, and upon differences in culture … and on a particular historical situation.”

The other, overlapping, tradition was German hermeneutics which shared many qualities with historicism and, of course, added new ones. For example, Dilthey – perhaps the most used example of the hermeneutic tradition – can be seen as both continuing the historicist tradition and criticising it for being incomplete. To repair this incompleteness, Dilthey wanted to show that, firstly, social sciences (Geistesswissenschaften) are independent and differ from natural sciences. He stated three reasons for this: “1) social sciences research a unique object, the historico-societal reality; 2) they contain normative statements which are absent in the natural sciences; 3) when natural sciences can claim only hypothetically the relations between elements, in social sciences the relations are originally experienced in psychical reality.” Because of this, psychology was supposed to be the primary means of understanding this psychical reality. “Understanding” in this sense was connected to psychology; the idea was to “understand” how societal (Vergesellschaftung) processes happen. (Kusch, 1986: 52-53.)

If we compare, for instance, Dilthey’s hermeneutics to contemporary qualitative research, we find that the latter, to a large extent, is a watered down version of both historicism and hermeneutics, where on the one hand, researchers claim that objectivity does not exist and that everything is, more or less, relative and, on the other hand, want to do science despite the fact that the possibility of those things that make science as “science” have been denied. The former, Dilthey’s hermeneutics, wants (or rather wanted) to find a “firm basis” for social sciences which, as an idea, was much closer to the traditional idea of science than to the contemporary qualitative trends.

Unfortunately Dilthey's solution to this "firm basis" failed. The historicist in him took the separation of natural and societal as a given and the resulting difference in method is even today accepted in qualitative research. These are: 1) the study of what is unique, individual, and different rather than, what according to him characterised natural sciences, similarities and lawlike regularities; 2) that as opposed to the natural scientist, social scientist cannot distance himself from the researched object; that if he wants to understand the object then the researcher must be "brought" into the research. (Kusch, 1986: 58, 61.) The solution was a vague concept of "lived experience" or "Erlebnis"; a some kind of ambiguous mixture of a holistic understanding of the self and an "empathic" ability to "step into shoes" of others. Neurath writes about Dilthey that "although he himself wished to avoid metaphysics, he inspired it greatly" (Empiricism and Sociology, 1973: 355-356).

Dilthey's "method" was criticised and rejected even by his contemporaries, most notably by Rickert, Windelband, and later, by Weber. The former two, although, rejecting the method, supported the basic duality of sciences (natural sciences vs. social sciences). In this, they also continued the historicist tradition. Weber's – by many incorrectly included in the "qualitative camp" – solution was "the middle path". The basic division of sciences was still present but this division and the resulting methodological differences seem forced and artificial. Weber's sociological method includes the aspect of "understanding" but this is further extended with the aspect of causality. Both are necessary in social research in the sense that human conduct must be causally adequate, i.e., that people do similar things and that we can make that causality meaningful, or understandable, by attaching to it the interpretation of motives of that conduct. (Weber, 1975: 125-126.) From a positivistic point of view, this division seems artificial because natural sciences are not limited to the kind of "meaningless" and mechanistic observation of events – that whenever X then Y – as Weber makes it look like. The idea of adding meaning to causality essentially means asking the question "Why this or that happens?" and then giving an explanation. For Weber, the explanations of human conduct are the motives of that conduct. But natural sciences act precisely the same way. Granted, they do not ask the objects of their research anything (because they cannot) but they do try to offer explanations why this or that happens.

What tied Weber to historicism was his acceptance of the “unique”, especially the uniqueness of European capitalism. Its methodological effect can be seen from the following event chain: the societal contains both a causal and meaning element -> we make the causally established human conduct understandable by explaining it through human motives -> motives are the product of culture -> cultural epochs change -> each epoch is essentially unique -> the contents of human motives will, therefore, change with time. To this we must say that Weber was only partially right, that some motive contents might have changed with time, while many have remained essentially the same and that not even the natural sciences, or positivism, have remained unchanged. (The fact that not all motives, or human traits and dispositions, have changed becomes particularly apparent when it is understood that being a human means being a physical and psychological totality. If the former is denied, then it may chime well with mostly ideological or moral preconceptions, however, it will be extremely lean on factuality. And if, on the other hand, the former is accepted, many anti-traditional-science claims turn out to be quite ridiculous.)

The present discussions, or lack thereof, in social sciences – I mean with the term “social sciences” all non-natural sciences, excluding mathematics – builds heavily on the events from roughly 100 years ago (plus the lingering teleological component). The last “undecided” round happened during the 60s and 70s., the core issues being the same as before and as they are now. The disheartening thing is that much of the present discussion revolves around straw man attacks, and arguments that have been rejected ages ago. Yet for some mysterious reason they are taken from the grave, slightly cleaned up, and presented as “new” in favour of this or that view. Although this can make me sound biased – and to a certain extent I am – I fault mainly the qualitative camp of these sorts of shenanigans. (While positivism is not the monolithic monster that it is claimed to be, it is, nonetheless, much more conservative in its argumentation. The justification for positivism is essentially the same as it has been since the philosophers of The Vienna Circle, or even since Hume. Although the older ideas of absolute and universal requirements of positivistic science have given way to slightly more reasonable, the principle ideas have remained the same. These are, more or less, that: we ultimately can know things through our senses (and thinking); in this sense, then, there is no difference between nature and man; different sciences can have different methods as long as they meet certain basic criteria, or conditions (for instance, things requiring a causal analysis must produce it, no amount of empathy, understanding, or some other metaphysical nonsense can be an acceptable substitute); scientific results are not necessarily eternal, they are subject to modification and change; this does not mean that we should abandon the scientific in favour of the ambiguous, truth remains still as the ideal; for if things cannot be known then science is unnecessary and to seek justification where there is none becomes pointless; history of science has shown that scientific endeavour has been fruitful (despite many mistakes); and that if we want to know something, science has been, eventually, our best bet.

Why is this situation important? The fact that there are people writing different things is, in itself, harmless; there have always been people writing “different things”, and this will, at least to a certain extent, continue even in the future. A clash between these different things is, as a principle, necessary since, as Popper (1994: 34) wrote, “orthodoxy is the death of knowledge, since the growth of knowledge depends entirely on the existence of disagreement”. This means that in an environment where everyone is busy congratulating and agreeing with each other, no truly new thing can evolve. But the total opposite is also to be avoided; a principal disagreement about everything will lead nowhere as well, especially if the objects of that disagreement are age-old facts. For instance, a sudden disagreement about the basic concepts of electricity would be silly, especially if it was referred to as “the sign of the devil”. Now, as long as this irrational disagreement goes on “somewhere else”, science can continue to function. However, if that disagreement becomes part of science, especially in an even more twisted form, for instance, a principal disagreement about already established facts and a subsequent agreement of that disagreement, then science will be impossible. And if this non-science attitude becomes the main content of methodology-literature, the same literature that is supposed to teach future scientists and researchers how to do science, then what will be the most likely outcome? Kuhn (1996: 80) wrote that “science students accept theories on the authority of teacher and text, not because of evidence”; adding, rightly, on the same page, that “what alternatives have they, or what competence? The situation, then, becomes the following: a student, who, by the definition of a student, does not have knowledge enters the environment of higher education to receive it by those who supposedly have it, and is, subsequently, being taught that “Z is good”, “we commonly do Z”, or “in our field Z is being done”, then this is exactly what will be learned. In this sense, then, “the sins of the fathers will become the sins of their sons”.

Töttö, among the few, has noticed this; not only that there exists a dichotomy between philosophies of science – between positivism (quantitative) and hermeneutics (qualitative) – and that this dichotomy is based on various misconceptions and straw man arguments, or that this dichotomy is present in much of the methodological literature – especially the critique of positivism seems to come from those who have very little to no knowledge about the matter – but it has, as to be expected, found its way into the vocabulary of the students. (see, for example, the introduction in Töttö, 2000; 2005: 12.)

Now, there have been some attempts at closing down this dichotomy but, unfortunately, in many cases it has amounted to a “in theory unification, in practice dichotomy”. For example, Lehto (1998), instead of bridging these opposites (where possible), continues to maintain the false arguments, old and rejected methodology (including the claim that empathy is typical of the humanities methodology, ibid.: 211), in short, it is mostly the “same old, same old” ideology rather than critical analysis of methodology; Kakkuri-Knuuttila & Heinlahti (2006) also want to go ”beyond” by creating a ”pluralist epistemology” (p. 13). But how can this be achieved when they do not seem to be adequately aware of the philosophical contents of at least one of the camps; according to them, for instance, naturalism is in opposition to positivism (ibid.). They continue that “according to pluralistic epistemology, research must offer argumentation about results, selection of research problem(s) and method(s)” (ibid.: 22). But there does not exist any version of positivism (or traditional scientific approach) according to which this kind of argumentation would not be necessary.

In the literature that is not intended as “going beyond”, the situation is, of course, no better. The old arguments are repeated with the addition of new ones, equally bad. So, for example, Karlberg et al. (2002: 22) write that

> public health research needs qualitative research methods to find the “meaning behind the numbers” and to help us to improve our understanding of public health concerns. ... Qualitative methods could be used when the research questions intend to answer questions based on the how, the what and the why.

Based on this, positivism or quantitative research is then left only with the “how much” option. If this is the case then how is it possible that, for instance, in natural sciences – but not only in them, the case is the same in experimental psychology, sociology, social sciences in general and, yes, in media studies as well – quantitative methods, or more generally, positivistic attitude has managed to produce answers not only to the “how much” but also to the how, what, and why questions? To continue, according to Hesse-Biber & Leavy (2004: 5-6)

> What distinguishes the field of qualitative research is its diversity. It encompasses a wide range of epistemological positions and theoretical frameworks while offering many distinct research methods. Qualitative inquiry, then, allows researchers to ask different kinds of questions than its quantitative counterparts. ... Qualitative inquiry is characterised by multiple research methods and multimethod approaches. ... This allows for not only a wide range of researchable topics, but also a wide range of approaches to the same topic. This lends a depth to qualitative research.

Again, this more or less says, that quantitative approach (or positivistic) is limited in the scope of questions and approach, and if this was not enough, it is also “shallow”. It must be, after all, that is the option left if qualitative approach has been labelled as “deep”. Not only is positivism left with only one type of possible research question, and being shallow, it is also quite easy. At least if we believe Jari Eskola (2001: 133) according to whom “quantitative research, compared to qualitative, is in a more advantageous position because there the researcher does not necessarily encounter any truly challenging situations.” Many more examples could be given. If this is the kind of literature that is being used by the students; if the false arguments are then adopted, presented, and accepted by the educational staff, then not only the particular scientific field, or the whole university institution (which is already showing signs of ”wear and tear”), but future of science as such is in deep trouble. Now, it is not that science as such will vanish but by twisting and/or demonising it beyond recognition will create a hostile, and possibly even directly dangerous, environment where only the foolhardy will continue to do real research. On the other hand, this might actually be good because this would filter out the all those who do ”research” as a means to a particular end: namely, bringing bread on the table. In the older days, commitment to science usually meant loss of money rather than having a regular salary. (Then again, I am not advocating that a scientist must remain poor in order to produce credible science.)

Be as it may, the core issue here is about polarisation in the sciences, or as the second part of the title of Gross’s and Levitt’s book (1994) says: The Academic Left and Its Quarrels with Science. The polarisation is not only about certain views but also about from which fields the representatives of these views come from. C. P. Snow recognised this rift, or ”The Two Cultures”, as between the humanities and the (natural) sciences. Of course, in reality, this polarisation is not complete; for instance, the social sciences as well as the humanities contain more traditionally oriented researchers who either have not abandoned the rationality of science or who have returned to it after realising that the so called alternatives lead or have led to a dead end. But regardless of how complete or incomplete the separation is, the critics of science do mostly come from the humanities and social sciences. Yet the interesting thing is, as Gross and Levitt (ibid.: 6) noticed, that

> it would seem to follow, then, that the last eight or ten years [in reality much more] should have seen a flock of earnest humanists and social critics crowding into science and mathematics lecture rooms, the better to arm themselves for the fateful confrontation. This has not happened. A curious fact about the recent left-critique of science is the degree to which its instigators have overcome their former timidity or indifference toward the subject not by studying it in detail but rather by creating a repertoire of rationalisations for avoiding such study. Buoyed by a ”stance” on science, they feel justified in bypassing the grubby necessities of actual scientific knowledge. ... The assumption that makes specific knowledge of science dispensable is that certain new forged intellectual tools – feminist theory, postmodern philosophy, deconstruction, deep ecology – and, above all, the *moral authority* with which the academic left emphatically credits itself are in themselves sufficient to guarantee the validity of the critique. (brackets added)

One of these rationalisations is the already mentioned separation between “deep” and “shallow” science where the former is the new and improved whereas the latter is attached to the old and in every way bad. But it is a mere sloganeering that certainly has not been able to show how, say, the approach of natural sciences is shallow or superficial. On the other hand, and to the disadvantage of the ”in-depth” researchers, it can be easily shown that it is actually their ”deep-approach” that suffers from superficiality: by demonising and denying an approach (positivistic or traditionally scientific) without understanding what is it about, how it can be used, and in what situations, they necessarily limit their sphere of research possibilities and related sources of information. In other words, to close one's eyes and stick fingers in one's ears may guarantee ideological purity of method, although I am not at all sure how this is supposed to be a sign of "depth". Positivism can certainly explain why, for instance, empathy does not offer any reliability to claims of empirical nature of the world (people included). And if this wasn't enough, there is the success-rate of science, and so far the results of various "alternative" approaches are not exactly stellar.

Additionally, even those few critics that seem to know what they are talking about (which doesn't mean that whatever they say is correct), like Feyerabend, for example, are being misunderstood and their message distorted. For instance, in *Against Method* (1975), one of Feyerabend's central themes is that to take any method, call it "The Science", and then demand total adherence to it, is actually counterproductive to science. Then again, we must realise that science has never been about one particular method and that Feyerabend is not really criticising science as much as he is criticising totalising and unrealistic demands of some of its representatives. The second misunderstanding is about the concept of "anything goes". Many critics of science seem to think that this means there is no need to legitimise arguments. Feyerabend points out in *Farewell to Reason* (1987: 284) that this is not the case. The point of "anything goes" is simply that science is an opportunistic enterprise. However, I think Feyerabend made en error in the way he emphasised this anarchistic nature of science. For, even if it can be shown that in this or that instant in the history of science some, or even many, things were done rather anarchically, this doesn't explain why many theories and things that are now accepted as common knowledge eventually evolved from the shaky origins to as close to absolute knowledge as is possible. What in the end saved these theories or damned them was not that anything goes, rather, it was evidence, or lack thereof. The anarchistic nature of science merely means that at some particular point in time, the supposed scientific standards were compromised. However, it doesn't follow logically or empirically from this that science is 100 % anarchistic, void of any phase that can be referred to as scientific rationalism. Maybe this is or was one of the things that led Feyerabend (1992: 28-32), to write:

> How can an enterprise [science] depend on culture in so many ways, and yet produce such solid results? Most answers to this question are either incomplete or incoherent. Physicists take the fact for granted. Movements that view quantum mechanics as a turning-point in thought – and that include fly-by-night mystics, prophets of a New Age, and relativists or all sorts – get aroused by the cultural component and forget predictions and technology.

Perhaps the physicists are doing the right thing after all, to take the situation for granted, for the precise answer would have to lead to what Feyerabend rejects (and rightly): namely, that it is this or that explicitly formulated method which leads to correct answers. There are certain conditions that have to be met in any serious scientific enterprise but they are not a "method", and one can certainly produce wrong results despite adhering to these conditions as best as one can.

The third misconception is based on the false idea that all belief-systems or traditions are equal; that, say, African animism is no better or worse than Western (scientific) rationalism. There are two things that need to be separated here: (any) tradition 1) as a way of life and 2) as the means to arrive at the truth (or as close as possible). What Feyerabend stresses, for instance, throughout *Farewell to Reason*, is the former point. He asks "By what right can the rest of the world be forced to live according to the Western way of life?" Of course, there exist no such right or legitimisation for this kind of action. But if we are interested in what is true and how to reach it, i.e., if we are interested in what the world is like, then, so far, science has been able to "beat" any other alternative, be it religion or any "other way of knowing" – one only needs to compare the so called anarchistic science with any other supposedly equal belief-system.

Another widely cited author among the critics of science is Thomas Kuhn (1996), especially his book, *The Structure of Scientific Revolutions*. Three concepts have been referred to above else – again, in an attempt to show how science is just a subjective belief-system among many. These concepts are: 1) incommensurability of theories or paradigms, 2) the theoryladennes of observations, and 3) the element of subjectivity that ultimately chooses the future path during a scientific revolution (i.e., when an established theory or a whole research program is beyond repair. Of the first Kuhn (ibid.: 112, 148-149) writes that

> at times of revolution, when the normal-scientific tradition changes, the scientist's perception of his environment must be re-educated—in some familiar situations he must learn to see a new gestalt. After he has done so the world of his research will seem, here and there, incommensurable with the one he had inhabited before. In the first place, the proponents of competing paradigms will often disagree about the list of problems that any candidate for paradigm must resolve. Their standards or their definitions of science are not the same. Within the new paradigm, old terms, concepts, and experiments fall into new relationships one with the other. The inevitable result is what we must call, though the term is not quite right, a misunderstanding between the two competing schools.

The second point is a logical conclusion of this. That is, if there is a gestalt switch, then even the same things will be viewed differently, hence the theoryladennes of observations. Regarding the third point, the element of subjectivity, Kuhn (ibid.: 155-156) continues that

> there is also another sort of consideration that can lead scientists to reject an old paradigm in favour of a new. These are the arguments, rarely made entirely explicit, that appeal to the individual's sense of the appropriate or the aesthetic — the new theory is said to be "neater," "more suitable," or "simpler" than the old. The early versions of most new paradigms are crude. By the time their full aesthetic appeal can be developed, most of the community has been persuaded by other means. Nevertheless, the importance of aesthetic considerations can sometimes be decisive. Ordinarily, it is only much later, after the new paradigm has been developed, accepted, and exploited that apparently decisive arguments ... are developed. Producing them is part of normal science, and their role is not in paradigm debate but in postrevolutionary texts.

However, there are many things, in Kuhn's text, that do not make it the "proof" of traditional science having run its course. Simply put, it is not the ammunition that the anti-science brigade might think it is. First of all, the whole idea of science as an oscillation between normal and revolutionary science has been – quite successively, I might add – criticised by Feyerabend in his article, *Consolations for the Specialist* (1980) – though Kuhn's model can be easily criticised on its logical difficulties alone. About incommensurability Hintikka (2002: 253) says:

> Kuhn, for instance, speaks as if his famous concept of incommensurability was totally impossible to approach through the logical or empirical consequences of these theories. Incommensurability means, according to Kuhn, the conceptual difference of theories. I have showed, however, that this kind of impossibility exist nowhere except in Kuhn's own head... At least in certain clear cut examples we can, on the contrary, show that the conceptual distancing between theories is directly dependable on the difference of their consequences.

And on the part of theoryladennes, Hintikka (ibid.) more or less dismisses it, as contradictory with incommensurability, on the grounds that Ptolemy, Copernicus, as well as Brahe worked with exactly the same observations, yet arrived at different conclusions; how is this to be explained with theoryladennes? As to the subjective element of science, well, Kuhn never said that science is subjective (and relativist) only that these happen from time to time (mostly during crisis). It is both a logical as well as an empirical error to assume that because something happens (or has happened) it must happen exclusively all the time.

As the reader can see, this could be carried on for ages, and, indeed, it has continued for ages – despite the fact that many issues are now, really, non-issues. By now the debate has turned into a self-sustained "perpetum mobile" which may serve more personal academic careers rather than actually improving scientific endeavour. This has become predominantly evident during the present "sausage-factory" styled scientism carried out at the modern factory university. Like with any production, research is now being treated as a product which must be churned out at an ever increasing rate to keep the corporation (university) afloat and "profitable" – or at least "academically respectable". For instance, the saying "publish or perish" is a direct symptom of this disease which forces academics to say something when there is really nothing to be said. In this kind of atmosphere text is produced for its own sake while the content becomes inconsequential; a total opposite if or when compared to the older days of science. I fail to see why otherwise nonsense is being written and published (about the same things) by the tons.

This insignificance of the content – for the anti-scientific criticism is either plainly wrong, repetitive, politicised, etc. – is perhaps its own biggest enemy. But not only that, even the fields themselves have it difficult to shield themselves from being, in the end, unimportant. The customary defence of the humanities and the social sciences against demands to be scientific is to invoke the rheumatic mantra of the two worlds: that there is nature and culture; the latter is ontologically and epistemologically different; the humanities and the social sciences are interested in culture; therefore they cannot adopt the traditionally scientific way of doing things. But we, the people, are not only cultural beings; we are also biological beings which means that nature is an inseparable part of us. This involves practical problems, i.e., the questions about shelter, food, and health, for instance. Their solution is a technical matter and so far the humanities and the social sciences haven't been able to offer any answers.

But these fields are not faring any better even from the cultural part point of view. We have had writers, composers, painters, for example, who have produced absolutely marvellous pieces of artistic work. I know of no-one literary critic, media scholar or a sociologist who has been able to create such immensely affecting melodies or deeply moving streams of words. Would anything bad happen if, say, the government cut all its financial support of the humanities? Well, although I am speaking for myself here, I simply fail to see what would be missed; what and where is the impact of the "softer sciences". A politicised discourse that is not only lean on facts but doesn't even recognise the importance of them will hardly be missed. Then again, we already have this type of discourse already produced by actual politics, there is no need to do the same under the banner of "science".

So far this introductory part has been going on at a fairly general level, and it is, to a large extent, unavoidable. There cannot be an epistemological/methodological/scientific discussion without references and remarks of a wide-ranging nature. For instance, the methodology of media studies is not a creation of media studies, it is an import from the methodology of social sciences as well as attempts at a more "artistic" approach, again imported from elsewhere. And of course, all of this – whether we say "there is truth", "there is no truth", or "inductive reasoning fails because of X" – connects at an even higher or more general level: the philosophy of science. On the other hand, this general level is made possible only because it is connectible to and arises from the actual instances of doing science. I have so far, then, introduced the general idea that there are many fields that suffer from confusion of what is science, what are its methods, and other related issues. In the next chapter I intend to illuminate the matters somewhat by talking about aim and structural issues, i.e., by particularising the discussion.

1.1 Aim, Structure, and Preliminary Remarks on Method

The aim of this thesis is simple: to conduct a basic analysis of the nature of media studies. By "nature" I mean the level of "scientificness" which refers to such things as basic tenets of sciences, methodological issues, and so on. I am not so much interested in what is being done right and how, but, rather, what isn't. In other words, the intention is to locate and criticise what, exactly, is wrong. As was said in the previous chapter, discussion about science must be by necessity general which means that the discussion about media studies will be, where necessary or otherwise appropriate, compared to a general level of thought.

Evaluation of media studies not only can but must be split further: 1) first of all, there is the established field itself, with its authors and published material; 2) however such a field is not a closed system. Streams of information enter and leave it. Such information flow is especially important because it connects any particular field – media studies in this case – to its surroundings. Thus the formation of a field depends on a combination of both internal and external factors – as evident from the fact that, for instance, the methodology of human sciences (to a large extent) has been implemented in media studies. But it is not only human sciences as such that have affected the field. The same can be said about natural sciences and, particularly, philosophy of science in general. Therefore, if one wishes to speak of media studies, it is at the same time necessary to speak of the external factors; 3) this can be further divided into what happens in these fields as such and how they are being taught. Of course I am talking about the part of higher education, the "initiation rites" that are supposed to turn raw material (students) into new researchers that will, if not come up with revolutionary results, then at least continue renewing the existence of the field. It is about what the students are being taught and what, scientifically speaking, is required of them in order to become “academically adequate”, i.e., what the students need to do before they get the magical paper that determines their knowledge, competency, abilities of the mind, etc.

Structurally, the content will be divided into three main sections (chapters), each dealing with one of the above mentioned themes: A) some general comments about science. This will not be intended as an “introduction to science”, dealing with all the traditional concepts like modes of reasoning, problem of induction, comparing empiricism and rationalism, and the like; there are plenty of such works available already. Nor will this be a historical tracking of the development of various scientific thoughts; again, there are better books of this sort in existence than this thesis would ever be and, also, there is simply no need to make every inquiry historical. It is not that the content will be completely “new” and “different”, just that I will apply “creative” selection (just like any other researcher in his or her work); emphasising what I feel either deserves a special mention in a general sense or what is otherwise underrepresented in the literature. In addition, there will be two examples of “alternative approaches” to science and why these either fail or turn out to be no different than traditional scientific approach. These are: postmodernism, and feminism. Although to traditional science these approaches are “alternative”, they are, nonetheless, common in media studies, or social sciences and the humanities in general;

B) superficial analysis of media studies. I say superficial because to present a “complete” summary of the field is either impossible or it would require such amount of work that it would be unfair to demand a master’s level student to produce it. As I mentioned previously, I am interested in the negatives rather than the positives, so I am perfectly aware of the fact that there are or can be well conducted scientific inquiries in the field. But the idea that the negatives are not representative of the field at all; that these are mere case examples would be, I think, unjustified; for there should be dissimilarity between these negative cases and pretty much everything else. However, if similarity can be shown between these cases, the way students are educated, and an even wider method-discourse carried out in the humanities and the social sciences, then it will be justified to consider that these examples are, in fact, quite representative of the field. The negativity (or positivity) of the cases shall be approached from a basic methodological point of view, i.e., not if this or that research used absolutely correctly analysis X, Y, or Z but whether the whole approach was justifiable; whether the results or methodological recommendations/advice were justifiable on logical and/or empirical grounds. For instance, to say "positivism is bad" without any explanation whatsoever as to why this is the case will be considered as a negative example;

C) analysis of the education. This will be partly a pure case example, for it is, after all, the University of Vaasa that I will be analysing. Furthermore, the concrete object of analysis will be a randomised sample of media studies theses. The same pros and cons can be raised about this point as well; that the case might not necessarily be representative, that things can, overall, be totally different. While the representativeness of a case study is always more difficult to ascertain, its justifiability is, however, intended to be increased – as was already mentioned – through the combination of the different factors included in this thesis. In other words, while the sample of media studies theses might not by itself produce reliable results about how education is being carried out in general – at different universities, countries, etc. – it would be, nonetheless, very difficult to explain it with a mere coincidence if the case example of education, the analysis of the field (albeit superficial), and the other examples of, say, the methodology of social sciences and/or the humanities would happen to correspond. I think that if taken as a part of a larger picture the educational case example is – or potentially can be – surprisingly accurate (not to mention, very important).

The method, or methodology, of this thesis can be divided into two main parts: 1) the theoretical and 2) both theoretical and empirical. The analysis of the media studies theses can be said to belong to the latter category and that the rest belongs to the former. Then again, it is difficult to draw an absolute line between theoretical and empirical work. If we are precise, the various books that are read in order to help us to construct the argument are equally empirical as any other material which is referred to as "empirical" – in this case media studies theses. Both are intended to be used as sources of information that go beyond the level of the strictly personal and subjective. One possible way of differentiation is by the purpose of the material. For instance, the books of the theoretical part can be intended as offering a proof of an existence of a certain idea, or some specific piece of information; it can also inspire to see things in a certain way. The empirical material, on the other hand, is mostly used to see if things are the way they are supposed to be, or to find a pattern, etc. This kind of differentiation, although in some cases useful, is in the end artificial, for at its core there is only one function: to see if there is something of a certain kind; that an object of certain qualities is either present or not. Nor will there be an attempt to keep the analysis of media studies theses "literature free", i.e., without making any references to other literature. So, again, I want to emphasise that the way I differentiate between the theoretical and empirical part is an purely an instrumental, and subjective, step.

I will discuss the methodology of theoretical research and continue with the methodology of empirical research in a later section of this thesis, the one which will deal about the analysis of media studies theses. Anyway, regarding the former (the theoretical), the existing literature offers tiny amount of description or explication of method(s). To put it more bluntly: there is no method involved when one does theoretical research. For instance Töttö (2005: 10, 12) writes that research is either theoretical or both theoretical and empirical which means that, firstly, the former is always present and, secondly, it is a combination of the researcher's thinking and the use of references (literature). Even though Töttö's description of theoretical research seems somewhat lean, it is actually one of the richest in this regard. Now, this doesn't mean that theory is not discussed in the literature at all; it is, and quite extensively, however, not as a method. Theory, when one reads about it, is always assumed. It is there, it is important, for instance, as an "organiser" of scientific knowledge, as a guide that directs the research process, or as an explanation of causal connection (Eskola, 1981: 164; Toivonen, 1999: 84). Sometimes some representatives of qualitative approach have a tendency to forget that theoretical research – especially the part about researcher's own reasoning – is or can be independent activity and that there is no automatic equal sign between theoretical and qualitative research. An example of this activity can be found, for instance, in Robson and Foster (1989: 3) in the form of the following: “Qualitative research is best used for problems requiring insight and understanding...”. (On a side note, based on this, a quantitative research must be then best fitted for problems that don’t require insight or understanding.)

1.2 Theoretical framework

Many “how to write a thesis”-guides, and method books do write about theoretical frameworks. The general idea here is that a theoretical framework is supposed to conceptualise the object of research, among others, by 1) limiting and specifying the researched object(s) and 2) by developing a specific, concept(s)-dependent research theme (Alkula, Pöntinen & Ylöstalo, 1994: 34). In qualitative research, there are grounded theory and its many variants that adopt a reversed stance; data first, theory second. The “theory first, data second”-model can be referred to as the traditional one. But is this model even possible; or even if this is the case, is it desirable? And should we choose a theoretical framework, what is it supposed to do? Rakitov (1978: 153, 160) writes that

> to be precise, sciences begins, as a matter of fact, with theory. A scientific theory contains the most precious substance of science. ... the decisive factor which separates a theory’s system of causal statements from mutually unrelated group of descriptions is that, in the former case, a theory enables the explanation and prediction of phenomena while, in the latter case, with descriptions [with singular or group of singular descriptions] this would be impossible to do. (brackets added)

How can a theory have predictive or explanatory force? The only criterion, of course, is by being true. Philosophical arguments of the impossibility of proving a universal statement aside, a theory needs to contain a high level of plausibility, at least in a pragmatic sense, that its usage will “produce workable, non-random results”. To the extent that a theory is not yet “workable”, its usage will be a combination of theory testing – of those parts that are testable – and theory formation – correcting and improving those parts that the ongoing research shows should be corrected and how.

If a theory, or a part of it, is found to be true – in the sense, that it produces workable results, it is highly corroborated, and it is even widely accepted by the scientific community – it ceases to be "just a theory", and it becomes a "first principle". In other words, such theory becomes accepted truth and its inclusion in the theoretical framework becomes unnecessary; it will be used in the same way as the ability to walk, an "automated" routine. (We do not explain, for instance, the "theory" of subtracting and adding whenever we use them – it already has been tried and tested.)

We are, then, left with – regarding theoretical frameworks – two things, namely that, accepted theories will not be really part of the explicitly formulated conceptual framework. They may provide much, if not most, of the terminology, concepts, and even guidance but this will not be specially introduced, it will be more or less assumed. The real object of research, the "unknown" part will be and can be conceptualised with any kind of terminology that will be "clear enough", i.e., so that others can understand what is going on (what is research, the how part, etc.). Here, quite likely, everyday language might even prove more fruitful than the usage of the different "theories", for instance, in the social sciences or the humanities, which seem to offer nothing more than an image of "being scientific". Due to the onslaught of postmodernism, constructivism, simply put, all the different versions of strong relativism where there is no truth, only opinion (and all opinions are supposed to be equal), these so called theories are rarely more than a mere play with words; at best an exercise of giving old things new names. And they will never become more, since they lack (purposely) one of the basic criterions of science: testing. But if we become interested in the truth-value of a theory, it will have to be turned into a hypothesis (no matter how simple or complex) and, subsequently, tested.

Nor does this mean, as the grounded theorists would like us to believe, that research can be "theoryless". However, this is perhaps the crux of it all: what really is a theory? What are its limits? When are we justified in talking about theories instead of hypotheses, or a collection of ideas? It is obvious that Comte's idea of theory guided observation as well as observation guided theory is true in the sense that every research is precluded by a some kind of guiding research idea; but when does an idea transform into a theory? The concept of theory-laden observation is not really helpful here at all, for it does not explicate what theory, to what extent, or what combinations of theories “construct” the observation(s). The observation of white swans, for instance, can be considered to include the theories of “whiteness”, “swanness”, “birdness”, “the human perception apparatus”, “colours in general”, “reflection”, ad nauseam. I dare anyone to “explain” the theoretical framework behind the observation of white swans, although, again, we can talk about that there are some ideas involved; ideas that carry a certain meaning which is then connected with a feedback loop to the real object.

I suppose a following distinction can be made: if by theory or theoretical framework we mean any kind of idea that leads the research process then it follows that any research will contain a theory or theoretical framework, for there cannot be research that wasn’t or isn’t guided by some kind of idea. However, to the extent that we don’t know if this or that idea is true, there cannot be any categorical distinction between a theory and an idea; the complexity of the idea can vary but that is all, and so far I am not aware of any authoritative sources that states at which complexity level an idea transforms into a theory. Furthermore, the guiding idea doesn’t need to be strictly “interpretive”, it can direct the inquiry through questions, certain curiosity, etc., and take the results (the different empirical and non-empirical material) at more or less their face value. For instance, a logical fallacy, if or when we find one, doesn’t need to be subsumed under a specific theoretical framework, it is what it is because we happen to recognise one based on our everyday experience; we happen to live in a world where things have their order. If theory is understood like this, then firstly, this thesis contains one and, secondly, its clarification cannot be anything else than making explicit the interests and aims that are guiding the research. And the aim is simply to see what this or that is like, to write down some of the things that happen to occupy one’s mind.

If, on the other hand, we mean by theory or theoretical framework something else, for example, a systematised (or axiomatised) body of knowledge, i.e., a collection of claims whose truth-value is already established (otherwise it wouldn’t be knowledge), then such a framework is not to be found in this thesis; it would be impossible. There simply aren’t any such “theories” available for the “interpretation” of, say, the epistemological state of media studies, or the humanities. There are ideas, claims, statements that, for example, academic feminism is non-science, which are then backed up by an actual examination of such literature. However, we are talking about an extremely simple idea. In fact, it could be also understood as a hypothesis. And to the extent that we don't know yet; if we are supposed to know only after the analysis (as is usually the case), then, regarding this thesis, we can say that insofar as it contains a leading idea or ideas, this or these can be described as having a hypothetical nature. (More about an actual hypothesis in chapter 4, where media theses are going to be analysed.)

2 SOME NOTIONS ABOUT SCIENCE

As noted earlier, this chapter is not about what, precisely, science is about; all its concepts, and their meticulous explanation. In this sense, this chapter can be accused of being incomplete, fragmentary, or something similar. The included concepts should, firstly, give some kind of idea about science – or rather, necessary conditions for doing science, etc. – and, secondly, they should help us understand why or what problems trouble, say, the so-called alternatives to traditional science, whether in themselves or as parts of media studies.

2.1 Science and the problem of knowing

Explanations to "what is science?" are always in some way misleading, even if they managed to capture the bulk of the issue. There is always some point with which we disagree or that we would wish to see clarified more. The situation is no different with, for instance, Wynn and Wiggins' (2001: 2) simplified version, according to which "science can seem mysterious, especially when presented in great detail. In essence, however, it is remarkably straightforward. Scientists simply try to gain a fundamental understanding of natural phenomena."

For positivists or traditional scientists – sometimes to be also found among the representatives of the humanities and social sciences – the above description probably will not sound categorically problematic. In other words, for them there is no fundamental difference between "nature" and "culture", and, therefore, "understanding of natural phenomena" is an all-encompassing term that refers to the "reality", "world, or "universe" around and including us.

The meaning of the word "understand" needs to be slightly expanded. I think it should be quite obvious that this word is closely connected to the word "knowing" or "to know", i.e., that it would be difficult to understand something without knowing what it is that we are supposed to understand. (By understanding I don't mean some kind of "empathic act" where we "relate" to how someone else might feel. Though even in this sense we "understand" better when we have experienced similar events ourselves: we "know" how this or that event makes us feel.) We can now slightly re-formulate the description of science: Scientists simply try to know what certain natural phenomena are like, how they behave, and why they are the way they are.

The central issue, then, resolves around knowing; what can be known and how. (Some, or even many, feminists (for instance, Harding 1987: 3; Ronkainen 1998) have also asked the question "who can know" which from a scientific point of view is a pseudo-problem that will be clarified later.) A fitting and simple way – at least not any worse than others – to approach the problem of knowing can be done with the help of the "classical conditions of knowledge". These conditions are in themselves very simple and, for example, according to Lammenranta (1993: 79) the "formula" is as follows: Someone (S) knows something (P), if and only if 1) P is true (i.e., the proposition corresponds to reality and, hence, its truth-value is true); 2) S believes that P (i.e., the proposition is given in good faith; there is no intentional lie involved); 3) S is (epistemically) justified in believing P (i.e., in addition to that the proposition is true and that the person believes in it, there should also be some compelling reason as to why the proposition is true and why it should be believed. This will usually take the form of an explanation.)

Points 1 and 3 are particularly important. The former simply emphasises the correspondence between an object (phenomenon) and the statement (or theory perhaps) about that object. Some philosophers, humanists, etc., have argued that correspondence, as a criterion, is flawed and unusable. However, this is a mistaken opinion and I will mention two ways how it can be countered. Firstly, (and this is the "common sense" approach) how can something be true, i.e., how can we honestly claim to know some part (phenomenon) of reality if that part of reality differs from our statements; if for instance the phenomenon doesn't even exist? This is so common sense that even court proceedings have adopted such a view – even though sometimes in practice this principle is violated. (If we are sending someone to prison, it would only seem fair that the accusation matches – corresponds – with reality; that the sentenced murderer, for instance, really committed that murder.) Not only is the claim against correspondence counter-intuitive in a general sense, it also violates, in particular, our sense of justice and morality. Second example comes from Bonjour (2002: 36), who writes that

> the mistake that is made ... is thinking that the intelligibility of the correspondence theory requires a generally applicable specification of the relation of correspondence ..., at least if such a specification is supposed to be more than utterly straightforward and trivial. Any intelligible proposition, after all, says that reality ... is a certain way or has certain features that the content of the proposition specifies. And the best way to understand the correspondence theory ... is to construe it as saying no more than that such a proposition is true if reality is *whatever way* or has *whatever* features the proposition describes it as having. In some cases, the content of a supposed proposition may be less than fully clear or intelligible, but that is a problem for that supposed proposition and not for the correspondence theory.

The main issues, then, are the following: 1) no "higher" philosophical or other kind of proof for correspondence can be given other than the trivial "X is Y if and only if X is Y". It is similar to the proof that reality exists: no more can be given than demonstrating that a case is such and such – if more is expected than this then, perhaps, one should devote his energies to religion rather than science; 2) the difficulties of propositional content are caused by, firstly, hypothetical nature of that content and, secondly, of linguistic limitations, which means that, although we are interested in a correspondence relation, it can never be ultimately achieved because that relation is between a collection of words and a phenomenon; 3) because the linguistic component refers to reality only through actual use, a test of correspondence – that of demonstration - becomes necessary; 4) a test of correspondence cannot be of analytic character, it must be synthetic which means that insofar as science is interested in understanding (knowing) reality, it must produce empirical evidence in support of its knowledge-claims, hypothesis, or theories. (Some have argued that, for instance, the truths in mathematics are analytical, i.e., not empirical but rather logical and that in this sense it differs from the natural sciences. Even though there is no pure consensus on mathematics as being purely tautological system, to explain this further would be sidetracking the issue here. Fortunately the humanities and social sciences try to tell us something about reality. Hence, we don't need to deal with the required evidence of tautological systems.)

At this point, the pseudo-problem of “who can know” or rather “who can be the knower”, as the feminists ask, becomes utterly inconsequential: anyone who satisfies the conditions of knowledge can be the “knower”. And because this is directly dependent on or equal to the conditions, the issue of the “knower” can be dropped altogether. Either the conditions are satisfied or they are not.

We can, then, sum this up by saying that the problem of science is the problem of knowing. The legitimacy of a knowledge-claim rests principally on two things: a correspondence between the claim and reality and the empirical evidence for that claim. Empirical evidence doesn’t appear magically by itself, it needs to be actively pursued and produced.

2.2 Causality

Hempel (1965: 297) writes that

> empirical science, in all its major branches, seeks not only to describe the phenomena in the world of our experience, but also to explain or understand them. In physical sciences all explanation is achieved ultimately by reference to causal or correlational antecedents, while some argue that, for example, in social sciences, psychology, or even in biology the establishment of causal or correlational connections is not enough.

Empirical science here refers to all those non-tautological (scientific) fields that make claims about the reality, hence, including the humanities and the social sciences as well. When, for example, Jameson writes about the logic of late capitalism, he is making an empirical claim about the reality even though the reader might have not the foggiest idea what he is actually saying. The part where Hempel writes that “some argue that, for example, in social sciences, psychology, or even in biology the establishment of causal or correlational connections is not enough” can be interpreted at least in two ways.

The first interpretation is the idea of generative theory of causality which

> holds that there is a real connection between causes and their effects, and that in many cases this can be identified with a causal mechanism which on being stimulated by the cause produces the effect..

On the other hand, the succession theory of causality "finds nothing empirical to answer to the connection between cause and effect." (Harré 1972: 116.) "Science follows the generative rather than the successionist theory of causality", Harré continues (ibid.: 118). This simply means that it is not enough to establish "A causes B" but, in addition, why it causes, i.e., what is the causal mechanism connecting A and B. Similar criticism – explicitly directed at Hempel – is presented by George and Bennett (2005: 132) who write that "the first flaw of the D-N model is that it does not distinguish between regularities that might be considered causal and those that clearly are not." (The D-N model refers to the covering law model of explanation.)

The flaw of these criticisms is that they assume a working scientist to be a complete idiot who claims victory if or when B follows A. (Such a "scientist" would claim that cranes deliver babies if cranes were spotted with regularity before a new child appeared in the house. This is another non-issue, and even basic statistics says that correlation (succession) is not causation.) It would have been nice if, for example, Harré could have pointed to some serious research where only succession (A -> B) model of causality was followed. However, no examples were given which means that science (those that actually do it) follows the generative model in one way or another. Why George and Bennett are wrong is because they have plainly misunderstood Hempel. The D-N (Deductive-Nomological) model of explanation simply tries to describe the logical structure of any causal explanation (Hempel 1965: 412). The concrete content of an explanation is based on the laws and/or theories used in that particular case (theories of a particular field and of a particular phenomenon). This first interpretation of what has been directed against Hempel's model turns out to be ineffective. The claim that "explanation is achieved ultimately by reference to causal or correlational antecedents" still stands: Hempel's model is not successionist nor is it generative, it is a logical model of causal explanation where the concrete causal model is supplied by the actual laws/theories used. And although Harré is right in criticising the succession model of causality, he has failed to demonstrate that there exist rigorous scientific endeavour that is successionist – of course, other than a brief phase which basically can be successionist, for, instance, when research about a phenomenon is in its infancy.

The second interpretation (or rather type of criticism), why referencing causal and/or correlational connections is not enough, is based on the idea of two worlds or two basic sciences, as already mentioned in the introduction. We are, of course, talking about the world of nature and culture, or natural sciences on the one hand and humanities with the social sciences on the other. For some bizarre reason – which is never really explained, let alone demonstrated – there is supposed to be a categorical difference between the two. But just as the feminists have failed to produce evidence in favour of the supposed "woman's way of knowing", the defenders of the two-world argument have ended up in the same situation: strong claims yes, evidence (of any kind) no. The idea of the two worlds (or two sciences) rests on the assumption that nature and man are qualitatively so diverse that it warrants a completely different scientific approach; methods, necessary conditions, and interpretation. This claim is, in many cases, taken as its own proof; no more than poor argumentation then. It has the following form (especially in the two-world case) "qualitative difference = ontological difference, therefore methodological difference" (For example, Routila (1986: 10-11) uses the words "different ontological structure" even though he is, as far as I can understand, talking about a qualitative difference. On the other hand, and strangely so, Routila (ibid.: 10) believes the contrast between the natural and "human" sciences "is based on many preconceptions, a stance which shouldn't be considered as a healthy one".)

But even if we ignored the words "ontological difference", it is trivial to show that qualitative difference doesn't automatically lead to a different methodology: chemistry and physics (or biology) are essentially interested in an area of phenomena and qualities that are "uniquely" theirs. Despite the qualitative differences of their scientific interest, i.e., what phenomena or which of their qualities are researched, there is no basic difference in their approach; the "logic" of science remains the same. Now, there is probably no-one who would deny that the there are qualitative differences between the objects of chemistry and objects of, say, sociology. However, because a qualitative difference does not equal a methodological difference (the logic of doing science), it must be shown why these or those particular differences justify a categorical methodological difference between sciences, let alone two ontologically different worlds. Then there are the results, or lack of them: whether we are talking about “verstehen”, empathy, women’s way of knowing, etc., these have remained at the programmatical level only, failing to produce single credible piece of satisfactory research. Or to put it differently: when the alternatives produce credible results, they do so based on the logic of traditional conception of science – regardless of rhetorical burdens to the contrary. So far, then, Hempel’s basic idea is generally applicable.

When we say “A causes B”, we usually refer to causa efficiens (efficient cause) and not the other three (causa formalis, materialis, and finalis) of Aristotle’s four causalities for the simple reason that over time the other three’s explanatory power has suffered from severe inflation. Causa finalis basically means teleology or teleological explanation which, for a long time, hasn’t really been popular in the sciences. If or when it still occurs, it does so mostly in the humanities and social sciences, because of the two-worlds idea (or even extreme relativism). It has been suggested that teleological explanations are being used even elsewhere, for instance, in physics, the idea of heat “death” of the universe is thought of being a teleological explanation, the ultimate thermodynamic state. But before undue rejoicing commences, it should be emphasised that not all teleological explanations are created equal. The world of difference lies in the way the explanation handles the first and third classical conditions of knowing.

First of all, the case of thermodynamics, for example, is backed up by empirical evidence and logical consistency, i.e., the requirement of correspondence and evidence are met. Secondly, many causal laws, or let’s say natural laws, can be reformulated in a teleological ways. In fact, any natural law states that this or that thing “ultimately” happens in certain way. For instance, we can say of gravitation that ultimately dispersed matter will coalesce (again, we have the empirical proof and logical consistency); we irrefutably know that, on earth, dropped objects fall down, they don’t levitate up. Thirdly, teleological explanations of this sort are not really explanations, they merely re-state what is already known; we don’t say things fall down because things fall down. A real teleological explanation of gravity would be Aristotle’s idea that “all bodies move toward their natural place". Not to delve into this too deeply, we can notice the problematic nature of this explanation; what is, exactly, the "natural place of bodies"? The gravitational model is quite straightforward, yet statements like the "natural place of bodies" do not fit to it at all, how could they do so in more complex systems, with higher level of contingency (like human activity)?

There have been attempts to make causa formalis meaningful in studying art (see Routila 1986: 12–16). Routila refers to causa formalis as structure law or style, say, that of Baroque. For some reason, according to Routila, structure law ought not to be confused with causal law, even though he writes that "within a certain stylistic framework, it is impossible to see just any kind of solutions; that the solutions are determined by style" (ibid.: 16). What this means is that during the Baroque era the architecture, for example, was what it was because it was determined by the Baroque style. (The only thing needed to make this argument purely circular is to say that the Baroque style is what it is because that is how things are done.) Then again, Routila contradicts himself when he states that the style isn't so deterministic after all (ibid.: 16, 19). It is apparent that the problem of structure law, or style, is very similar to Kuhn's idea of normal science, or the dominant paradigm. The problem, then, is this: if a style (or a paradigm) is supposed to be deterministic, how can it be explained that there have been both different artistic styles and "prevailing" scientific notions? Well, the only possible explanation is that: because such a thing is not causal or deterministic in the real sense. It certainly can affect but it doesn't cause, which is a case of the basic tenets of statistics, i.e., that correlation is not causation. This means that style is comparable to a fashion – a certain kind of social norm – which we, justifiably so, think of affecting our behaviour, such as artistic choices. But simply as that, as a correlation – that A affects B – it is exactly what Hempel wrote about; that "explanation is achieved ultimately by reference to causal or correlational antecedents". The idea of style certainly fits in the latter category (not causal but correlational).

There are different criteria for establishing causality. Töttö (2005: 120), for instance, lists four criteria: 1) contingency, 2) temporal succession, 3) eliminating seeming correlational effect from a real one, and 4) mechanism. In actuality, it is not merely causality that Töttö writes about, we find that with the inclusion of mechanism comes also an explanatory element, i.e., it is about causal explanation. (Establishing a causal connection is possible without knowledge about underlying mechanism if it can be shown that from the various possibilities only A causes B – that without A there is no B – then a causal connection is established.)

The requirement of contingency means that B doesn't follow logically from A, that the connection is not definitional. For example, there is no causal connection between "a bachelor" and "being unmarried", the latter is already included in the concept of a bachelor. Contingency is therefore an empirical matter, which can be theorised and hypothised about, but one ultimately needing a verification, a test. Furthermore, such empirical tests increase our knowledge over that of what is included in a definition. Again, some say that in the realm of culture, contingency doesn't work or that it should not be required. However, this can be easily shown to be a wrong opinion: without contingency we are, as a result, left with logical necessity which should mean that the world of culture should be predictable; for instance, it should be child's play to tell in what kind of state the economy will be in, say, two years – economy is, after all, a "cultural product". But no such prediction is possible. Contingency can be understood as the possibility of being wrong and regarding human action this is certainly the case. Since there is no logical necessity in human action, and if we would still want to cling to the no contingency idea, then the only solution would be to claim that there is no need to seek causality in human action at all – which in humanities and social sciences many times happens. But this denial is constantly broken by references to norms, other societal effects, and so on. The only possible way to avoid causality regarding human action is to completely refrain from asking why we do what we do and, instead, concentrate on making such authoritative insights as "people do what they do". While some academics are happy to do exactly this – of course camouflaged with the right jargon – it is still in our nature to be curious; to want to know why. This is, by the way, what separates a scientist from an "academic worker": the former wants to know, the latter wants tenure.

Temporal succession is a straightforward idea; a cause has to preclude its effect. It is possible that there can be a feedback loop from B back to A but the process must be started by A first then B (ibid.: 125). Moving along with the third point then. Even though correlation is not causation, the latter presupposes the former. However, what is visible to us is the correlational part as causation doesn't wander around in nature, waiting for us to point at it. The scientist must find out – as best as he can – whether the correlation between A and B is real or if it is a case of third factor C affecting both A and B. This latter case is when there is a seeming correlation. Needless to say, if a seeming correlation is not recognised, the results of research can be pretty much thrown out of the window. A lax or even hostile attitude towards causal analysis can become downright dangerous – not to mention unethical – when wide ranging policies are planned and implemented. Examples of this can be found in nursing, childcare, etc., where if "research" is based on "in-depth" interviews, and other qualitative "methods" then the results are in danger of being most bizarre and misleading. A default no-thank-you attitude towards causal analysis might score points among like-minded ideologues but I dare not speculate to what extent it helps the patient who has told some researcher his life story – the researcher tries to "understand" how the patient feels – and as a result receives treatment which might make him feel "happier" but in reality, in a medical sense, causes a deterioration of health – should such a researcher ever be taken seriously.

A typical basic attitude of this "oh let's not bother ourselves with this thing called science" can be found, for example, in Eskola (2001: 146-147) where he praises Freud's ideas of the origin of society; apparently these ideas cannot be taken as historical facts or causal explanation of something that really happened; a "fruitful" delving into what culture might mean and/or to forget empiricism and substitute it with going on a walkabout in fantasy land seems to be more than enough. And lastly, the third point, mechanism adds to the credibility of causal explanation by reducing the likelihood of a seeming correlation. But again, causation (or correlation really) can be shown to exist even without an underlying mechanism, although, the explanation is nicer with it. It should be further emphasised that much of the elaboration (the attempt of finding a real causal or correlational connection, including the evaluation of a mechanism's fit) is the bread and butter of empirical science which cannot be solved based on statistic criteria alone; a theory and assumptions of the real world – external to the tested model – are needed (Töttö 2005: 130-131).

We can, then, think of causality and causal analysis as the most important element on which both correspondence and evidence rest. In this sense, correspondence and evidence are strongly interconnected: because the search for causality is an actual activity, demanding empirical research, testing, etc., the two conditions of knowledge "grow" simultaneously. When a better mechanism is discovered, the better or stronger is correspondence. On the other hand, the more accurately we can show that there is a causal or correlational connection (a correspondence), the better we can devise mechanisms for this connection. Any empirical scientific field that wants to be taken seriously – where scientific claims exceed the trivial "what is, is" – must resort to causal analysis. Granted, the analysis can fail but the attempt must be made. Even in the human sciences we are more interested in the causes of the way we feel rather than just simply saying that we feel the way we feel. Especially in the human sciences there are attempts and claims in favour of rejection of causal analysis which not only grinds these fields to a halt but it still leaves them with the obligation to produce correspondence and evidence; even a so-called descriptive "science" must show that the claim of "what is, is" really is. I will end this chapter with a lengthier example of what kind of monster is created when causal analysis, and more generally the classical conditions of knowledge, i.e., the basic tenets of traditional science are dropped. The example comes from the "visionary" Marshall McLuhan (1994: 267):

> The telephone demands complete participation, unlike the written and printed page. Any literate man resents such a heavy demand for his total attention, because he has long been accustomed to fragmentary attention. On the other hand, our habit of visualising renders the literate Westerner helpless in the nonvisual world of advanced physics. Only the visceral and audile-tactile Teuton and Slav have the needed immunity to visualisation for work in the non-Euclidean math and quantum physics. Were we to teach our math and physics by telephone, even a highly literate and abstract Westerner could eventually compete with the European physicists. This fact does not interest the Bell Telephone research department, for like any other book-oriented group they are oblivious to the telephone as a form, and study only the content aspect of wire service. As already mentioned, the Shanner [maybe a print error, but should be "Shannon"] and Weaver hypothesis about Information Theory, like the Morgenstern Game Theory, tends to ignore the function of the form as form. Thus both Information Theory and Game Theory have bogged

> down into sterile banalities, but the psychic and social changes resulting from these forms have altered the whole of our lives. (brackets added)

(The text would be probably more accurate if pronouns were replaced by references to self; that, for instance, “My habit of visualising renders the literate me helpless in the nonvisual world of advanced physics”.)

2.3 Monism, reductionism, and determinism

Raatikainen (2004: 11-12) separates monism into two categories: ontological and methodological. Both of these contain some amount of confusion. According to Raatikainen (ibid.: 11) the central question regarding the “sameness” or difference between natural and “human” sciences is the existence and nature of the research objects of these fields. He asks (ibid.) if

> human sciences have their own distinguished object. Are, for instance, social relations, societal structures, human culture, mental states and feelings, or meanings in themselves real, or can they be fully reduced to natural sciences (reductionism)? Positions that wish to deny the real existence the research objects like these of human sciences, and to reduce reality to how natural sciences see it, represent *ontological monism*.

There are two problems that should be sorted out. Firstly, ontological monism doesn’t deny the existence of research objects of any fields. The mistake made is comparable to that of Routila’s (who wrote of ontologically different worlds, although he apparently only meant qualitative differences, or rather, emphasising that this object may have these interesting qualities while that object might have other interesting qualities.) When it comes to positivism, Comte, for instance, didn’t deny the variety of qualities of reality, he only denied that reality could be divided into two worlds, where one is law-like (nature) and the other is not (culture) (Töttö 1997: 38-39). The same can be said of the Vienna Circle: whatever can be known, conforms to the basic conditions, that it is one way or another observable; that it is material in nature - whether one wants to research “the mind” or something else. Research objects of the non-natural sciences do not require ontological dualism or pluralism; they can have – and they do have – a distinguished objects based on difference of interest, i.e., which qualities of an objects happen to be interesting.

The second error that Raatikainen makes is the assumption that ontological monism leads to reductionism. It doesn't, at least if by reductionism we mean an absolute version of it where, for example, human feelings are explained through subatomic particles. (Considering that Raatikainen wrote "...fully reduced to natural sciences", I take it that he meant an absolute version of reductionism.) The ontological monism of Comte, for instance, presupposed only a partial reductionism where the different sciences form a continuum. Mill (1973: 37) wrote of Comte's idea that

> the relation which really subsists between different kinds of phaenomena, enables the sciences to be arranged in such an order, that in travelling through them we do not pass out of the sphere of any laws, but merely take up additional ones at each step. ... that each science depends on the truths of all those which precede it, with the addition of peculiar truths of its own.

If by reductionism we mean what Comte had in mind, then it would be quite difficult to deny its accuracy. We can summarise this line of thought in the words of Levitt (1999: 20), who gives the following example:

> Zoologists, after all, study zebras, not the quarks and leptons of which zebras are presumably composed. The laws and regularities they observe are laws and regularities of zebra anatomy and behaviour, not laws of physics. No sane person would suggest that it should be otherwise. Nonetheless, I venture that there are few zoologists who won't cheerfully concede that zebras are, in fact, constituted of quarks and leptons, and that their properties, including those of most interest to zoologists, are ultimately determined by what goes on at the quark-lepton level (or whatever level might turn out to be even more fundamental).

While it would seem strange that there are academics who deny the "lower" level truths and make claims totally incompatible with them, it is, nonetheless, exactly what is happening. Granted, they are not to be found among zoologists, however, there seems to be an unlimited supply of them in the "human" sciences. Let's take an example from feminism – for it is an extremely bountiful source of all that is done wrong in science, which doesn't mean that other fields have anything to be happy about. Hartsock (1987: 163) has written the following gem: "Thus, the fact that women and not men bear children is not (yet) a social choice, but that women and not men rear children in a society structured by compulsory heterosexuality and male dominance is clearly a societal choice." (Just a small observation here: where would we, as species, be if it wasn't for heterosexuality? And if heterosexuality for us is – or at least was – a necessary prerequisite for existence (we are mammals, after all) then isn't it only logical that society is structured by it?) I have no idea why universities shelter "scientist" who think that their claims can be in total contradiction with other (empirically well supported) sciences. Ultimately, of course, the conditions of knowledge decide who is justified in saying what and at this point the "hard" sciences have the supporting results and theories while the "soft" sciences do not – and never had, as far as "alternative" methodologies go. Then again, as long as nonsense will be academically supported and awarded, nonsense will continue to be produced, in the guise of feminism, postmodernism, or any other fashion trend.

From ontological monism, then, follows methodological monism; if there is one reality with a certain set of underlying laws – which doesn't mean that all of them are known – the methods, or the "logic" of science, must be essentially the same no matter what parts or qualities of the one reality are being studied. (Methodological monism really leads us back to Comte's "method of science", i.e., observation and reasoning which is, commendably, the "main method" even in Routila's (1986: 20) approach of how to study art.) On the other hand, according to Raatikainen (2004: 12), it is possible to accept methodological monism while, at the same time, rejecting ontological monism; "to allow the real existence of the research objects of human sciences, but also to demand that they be approached as the natural sciences approaches their objects." But this raises a difficult question, namely that why on earth should we have same methodology for ontologically different objects; why, despite a categorical difference, they should be approached the same way? Raatikainen (ibid.) talks of "similar goal" that is the unificatory element, although this doesn't solve the difficulty in the least: there must be something same if "similar goal" – which I presume is the goal of knowing – is to be achieved. It simply doesn't make sense to have one kind of monism without the other. The most intuitive, economic, straightforward, etc., similarity is the ontological one. But if this isn't enough then, again, we can ask what results has been produced by monistic science compared to the alternative, and it must be concluded that "monistic" science has been doing quite well, while the alternative has been producing at best only promises.

Regarding determinism, the mistake – well, at least I think it's a mistake – made in scientific discussions now, as well as in the past, is that it is an extreme position; that either it is 100 per cent or none whatsoever. Instead of what seems almost to be an ideologically heated discussion, an example of the approach of a working scientist might be the best solution: to take into consideration those aspects that obviously are deterministic and leave out those that aren't or of which we have no knowledge. Of course, determinism in the sense of the zebra example above is total determinism, but the main question for a scientist is that of fit, not necessarily that of philosophical principle. Especially when we are dealing with causal explanation in science, the only thing required is that it actually works, whether or not the world is deterministic or some other "–istic" (Töttö 2005: 90). Reformulated differently: the question of determinism is an ontological question, while causal explanation is an epistemological issue (ibid.). Naturally, epistemological questions are not entirely autonomous from the ontological nature of the world, for instance, causal explanation works as long as the constitution of the world makes this possible. But once that level of "workability" has been reached, everything that goes on or might go on beyond that is unnecessary.

If we necessarily want to problematise determinism, it should happen by mapping where it is to be found and/or to what degree and how it could help science – if at all. The problem can be separated into two levels. (This is just for conceptual clarity.) If we think of physical systems, where determinism is the equivalent to a time evolution of that system, then the underlying mechanism or principle is the following: once we know the position of points (of matter), their velocity, and the forces affecting those matter-points, then we should be able to predict the future state of that system. This is an ideal case of a deterministic system. In reality we don't really know with absolute precision the initial state of the system. And the more sensitive the system is to its initial state, the stronger will be the effect of the lack of precision on the possible future state. Furthermore, the more complex the system, the more difficult it will be to estimate the effect of the lack of precision. (Ruelle 1991.) One reason, then, not really against determinism but against a strict philosophical stance regarding it, is that the necessary precision of measurements, etc., is beyond that of what we are capable. This doesn't mean that every physical system is beyond our capability of prediction, only that some are. And the fact that some systems are predictable is evidenced by our everyday life: cars, for example, are possible to be manufactured and used only insofar as the system of manufacture and the workings of a car (as a system) are predictable.

Some of Popper's (1982: 4-11, 28) arguments against determinism can be summarised in the following way: 1) metaphysical notion of determinism doesn't assert that the events "are known to anybody, or predictable by scientific means"; 2) the fact that we can ask why-questions does not depend on determinism but (same argument as Töttö made), rather, on the fact that satisfactory causal chain can be produced. Not only determinism requires the possibility of absolute precision – which Popper, like Ruelle, rules out – such precision, in causal explanations, is not even necessary. In fact, beyond certain point, precision doesn't really add anything to explanatory power. This is one thing why Popper considers causality and determinism as separate things: the former is about fit and purpose which accommodates ± tolerances, while the latter is about absolute precision. Since the former works as it is, it cannot be the same as the latter; 3) the indeterminancy of human action (including the issue of free will) of which Popper (ibid.: 28) writes:

> If determinism is true, it should in principle be possible for a physicist or a physiologist who knows nothing of music to predict, by studying Mozart's brain, the spots on the paper on which he will put down his pen. Beyond this, the physicist or physiologist should be able to anticipate Mozart's action and write his symphony even before it is consciously conceived by Mozart. Analogous results would hold for mathematical discoveries, and all other additions to our knowledge.

Empirically and intuitively speaking, we have no reason to believe that, in fact, it should be possible to "predict" the next invention before it is invented – even if it was possible to measure the mass-point with absolute precision. Popper's Mozart example is not only intuitively acceptable, it can also be explained by the "law of conservation of information", at least, as formulated by Medawar (1984: 79): "No process of logical reasoning – no mere act of mind or computer-programmable operation – can enlarge the information content of the axioms and premises or observation statements from which it proceeds." This simply means that we cannot "deduce" more information out what is already included in this or that information set. Yet determinism, in an absolute sense, demands precisely that. Compared to the positions and velocities of certain mass-points (and the forces affecting those), the creation of a symphony is an increase of information, impossible to predict based on purely physical data. But, most importantly, the creation of a symphony doesn't require determinism in the same sense that causality doesn't require determinism or other ontological commitments for it to work. The question of fit or purposefulness applies here as well: when interested in a physical system from the classical mechanics point of view – as the motion of physical objects – it makes sense to consider it as deterministic because it happens to work. In other cases determinism is a non-issue and to force philosophical discussion that direction would be detrimental to science – this is evidenced by the methodological debate between sciences where, say, the fields of the humanities claim they don't need to conduct causal analyses because the human mind is not deterministic.

We can conclude, then, few things. First, to say that there is one reality, or that the world is ontologically monistic, in no way contradicts the existence and fruitfulness of studying particular (limited) objects of that reality. In other words, the objects of, say, sociology or media studies are there and justified. What separates these objects is not ontological difference but, as Comte noted, our interest in certain aspects or qualities of certain objects. For instance, both chemistry and sociology can study human beings; the former is interested in the chemical constitution of people while the latter studies how those "combinations of matter" actually act. Second, although we can think of sociology – at least in this case – as a "higher" order analysis, it cannot contradict what goes on at, say, the chemical level (or even more constitutive level). Applied to reductionism, this means that research objects at least partly can be "reduced" to lower level phenomena. Third, the question of reductionism (or determinism) in science is that of fit and purpose, not a philosophical standpoint; i.e., that if there are good reasons to think that some phenomenon is reducible to or can be explained by a lower level phenomenon (or elements) – when, for example, some human behaviour can be explained through biological factors – then it simply makes sense to direct research that way – it would be totally antiscientific to discourage that kind of endeavour just because it could endanger our philosophical stance. Fourth, determinism, as the enabling or disabling element of science, is a straw man argument. Scientific explanation, causal analysis, etc., goes on quite happily without any need whatsoever to first establish whether the world is deterministic or not. It is like saying: "Before we can do science, we must establish whether or not Bob's car is green", and this is nonsense. Fifth, what really matters is that this or that activity works. If it is in-line with some philosophical notion then all the better, but if these two contradict each other then that what works must win. Traditional science does have the results and they do happen to support its philosophical thoughts – determinism, however, is not one of them – while alternative claims have nothing except for those claims themselves. Sixth, if it is reasonable to think that there is one reality (it certainly is intuitive to think so) and if certain requirements can be shown to be universal, like causality, the need for evidence, correspondence – again, there can be "philosophical" differences of opinion, although, they are trumped by the actual work of real scientists and, yet again, by the results – then it makes sense to assume that methodology will also be monistic. However, methodological monism should not be confused with one highly particular and concrete method, it is about the basic "logic" of science.

2.4 Craftsmanship

This topic is not necessarily more important than any other significant element of science, however, it is – I think – grossly overlooked, not only in science but in other spheres of life as well. We have come a long way from the times when the results of one's work were the source of pride for the craftsman. It was dishonourable to do "a bad job". Today "a bad job" or lack of quality is "normal". Not only is this tolerated more and more, the ability to even recognise quality – in those rare cases where it still exists – seems to have all but disappeared. To think that higher levels of learning would be safe from this general deterioration of standards is highly naive. Not only higher levels of learning can become affected, they have already. We have people like Chomsky (1976, 5th Ch.) essentially saying that university has basically become a sausage-factory – and producing mediocre sausages at that – or Lakatos' (1980: 216, 1. fn.) remark that during Newton's time less than stellar manuscripts were put aside to "wastepaper-baskets" where they awaited for further work, yet in today's publication frenzy the function of these baskets is taken over by scientific journals. Levitt (1999: 44) refers to this as "the abandonment of intellectual craftsmanship".

Although it is contestable to what extent it is true, I, nonetheless, think that Riesman's et al. (1961) concept of inner-directed vs. other-directed person, or rather, the change from the former to modern day's latter, is quite fitting. Strongly simplified, the former is a highly individualistic person, striving for technical excellence (whether in art, science, in everything one does). Whereas the latter is a "peoples-oriented" person, whose actions are guided, to a large extent, by group approval; that it is not the content of the actions, products, etc., that are decisive; worth is decided by what others think. Riesman et al. also refer to this change in character – and societal structuring – as the change from the invisible hand (of the market) to the glad hand of modernity's entanglement of work and pleasure. Instead of personal effort, dedication, and, simply put, personal sacrifice, the other-directed person, above all, expects things to be "fun". (Ibid.) This "fun" mind-set manifests itself with the countless variations of "You can do it!" attitude. It's not that "You cannot do it!" (although frequently this is the case), it is rather that a slogan of positive support misses the other half of the full expression: "You can do it if you have the talent and are prepared to work hard!" Confusion like this can be found, among others, in Eskola (1966: 325-330) who, on the one hand writes that being a scientist is basically just a profession that deals with facts, i.e., that the traditionally expected exceptionality of the person who becomes a scientist is highly exaggerated, but, on the other hand, continues to list certain qualities (creativity among others) that seem to be typical of a scientist and which diverge from other "professions". He essentially writes, then, that a scientist is really "just a regular guy" (or guyette) who just happens to have such personal qualities that are considerably different from the other regular guys of the society – not really different but different after all.

It really isn't the case that anyone (or everyone) can be a good scientist (and not merely employed as one), or a great artist for that matter. (Nor do we even need that many, which is the total opposite of the higher educational policy of Finland, for example, which seeks to "produce" researchers by the metric tons, even if such a "scientific greenhorn" is manufactured only to decorate the unemployment figures.) In music, for instance, there was only one Mozart or Beethoven, and for a very good reason: they were simply miles ahead in talent and competency than the rest of the population. But it was not only talent; all these greats (exceptional scientists included) put hard work into their particular disciplines. Niccolo Paganini was forced to practice for hours and hours by his despotic father and, although, we can (and should) disagree with the morality of such an action, it would be difficult to claim that Niccolo would have become what he was without such strict work. Then again, these examples are from a time when real virtuosity and genius were strived for; as opposed to present day's celebration of ignorance and diluted dilettantism. A less than a flattering account of the "mood of the times", of the sphere of academia, is given by Levitt (1999: 44):

> First of all, there is the current claim, widely echoed in literature departments, the Modern Language Association, and so forth, that literature criticism, once a dilettantish, impressionistic, low-key enterprise, has been transformed by the advent of what postmodernists are pleased to call "theory" into a deeply serious discipline, fraught with rigor, intellectual density, and philosophical complexity. The claim rests on the fallacy that verbal clutter and the interminable jangle of empty neologisms signify intellectual exactitude and authentic insight. My own experience in wading through this stuff is not extensive, but I have scrutinized enough examples to verify that this is a world where raw nonsense is more often rewarded than punished, provided it be presented in sufficiently jargonistic form. Praise, prestige, and perquisites have been lavished on the creators of work that, when examined coldly, dissolves into a slurry of errors and confusion. As it turns out, what has been widely touted as scintillating intellectual fireworks consists largely of damp, pathetic squibs. This is evidence not of resurrected virtuosity in thought and argument but of its dismal opposite.

The result of all of this is that, on the one hand, people who have zero competency to write about certain topic choose to do so anyway, and with the claims of authority for that matter. Levitt (ibid.: 45) continues that "these days academics have discovered that significant brownie points can be had by writing tomes on quantum mechanics and chaos theory (for instance), despite having less grasp of those matters than a freshman physics major." And it's not that people without formal credentials should be barred from writing about this or that topic; stating opinions, for example, should be possible to anyone, regardless of "competence". But the products of human sciences are not intended as opinions – or even more fittingly, literature in general – they are intended as products of "science", as something more than literature; something more credible. But not only is most or even all credibility missing, it simply cannot be present or even aimed at. Whether we call the modus operandi of the human sciences (though found also in philosophy) as relativist, post-modernist, qualitative, etc., it clearly is against all the aspects and requirements that make science what it is and what it has been the last three hundred years or so.

No wonder, then, that humanists frequently end up writing nonsense when the field(s) is ripe with ideas of science like Ang's (1996), for example, who not only attacks positivism with straw-man, or other completely baseless arguments; she is even unhappy about qualitative approaches if or when they aspire to produce legitimate (read: scientific) results. Apparently this is a big no-no and, instead, science – qualitative kind, that is, since positivism is by definition some kind of horrible monster – should be about politics rather than facts or truth (ibid.: 2nd Ch.). Of course, this approach is nothing more than a plea for the right to produce nonsense under the heading of science as long as one remembers to include the disclaimer like Ang (ibid., p. 21) did, namely, by saying that "much of what I am to say will not be more than (theoretically informed) speculation, which will need further refinement."

Ang is not alone in this "not only anything goes, everything is acceptable" approach. Pietilä (1996: 13) in his interpretation of television news writes that he has proceeded along the lines of literature research where the aim is to uncover "the message that is not visible to the eye". He continues (ibid.: 15) that this kind of interpretation has traditionally been treated with suspicion and that "it is true that it would be a miracle if two researchers would reach the same interpretation." In this kind of research, then, there can be no discussion about correspondence, and the question of reliability will remain that, a question. At least these scholars are upfront about the fact that their works don't need to be taken seriously. No wonder, then, that equipped with this kind of attitude, one can write truly anything, like referring to Gödel's incompleteness theorem à la Jameson (1991: xi-xii) like this:

> Whether, as with Gödel's proof, one can demonstrate the logical impossibility of *any* internally self-coherent theory of the postmodern – an antifoundationalism that really eschews all foundation altogether, a nonessentialism without the last shred of an essence in it – is a speculative question

First of all, I am only guessing that Jameson is referring to Gödel's incompleteness theorem; it isn't clear what is being referred to by the term "proof", nor do I have the slightest idea what Jameson actually means in general – perhaps I am just simply missing that which is invisible to the eye.

A progress was made by the recognition that there are no absolutes in science; after all the history of science is the proof of this. We simply cannot know beforehand how long the results of science will last. Some are changing, some have changed recently, but it should not be forgotten that some have remained the same for a long time and it would be difficult to even imagine that we have had it wrong all this time. It has been a healthy and self-critical sign to acknowledge that it is not only this or that method through which we can reach the ultimate truths; that chance (or luck) plays a vital role (see, for instance, Beveridge 1980: 18-20; Medawar 1984: 49-51). But as both Beveridge and Medawar point out, chance (or luck) happen only to those that have made themselves "discovery prone". This means, and on the contrary to what some or many humanists may believe, that having zero idea about a topic while making strong claims about it or gathering material without knowing what to gather in the first place, is not what counts as making oneself "discovery prone". Nor is the grounded theory approach, in the strict sense, any better, for if the mind is purged from the effects of any previous theory (the mind turned into tabula rasa so to speak), then how is the researcher supposed to even recognise a (favourable) chance event.

Craftsmanship is where everything what makes scientist a scientist comes together. It is the realisation that there are limits to knowing, yet it doesn't become its total opposite; that of finding refuge in the safety (both from the intellectual and radical point of view) of the human sciences, where the correctly formulated, and quite meaningless, jargon is the cheap way out. Likewise it is not radicalism at all. For instance, academic feminism with charges of androcentrism, oppression by only white male's, etc., are really quite harmless, I mean to those who utter them. The society tolerates and has tolerated (some even celebrate it) for quite some time now. Radicalism is something where the one being radical risks of losing everything, life included. The academic feminist or postmodernist doesn't really risk anything. On the contrary, he or she is even rewarded by "research" positions and professorships. In this present age, it is actually the traditional scientist who is being a radical. A craftsman strives for what is right (methodologically speaking) and not what is fashionable, or politically correct. Although a craftsman knows the limits to knowing, he realises nonetheless that knowing is possible and that there are certain conditions that have to be met – there are no alternatives. He is also consistent; for instance, a question or a claim of causality is followed by an attempted causal analysis, not by some obscurantist nonsense. And as was mention earlier, though human sciences are not completely void of being scientific (really good work is being done there), it is, however, above all in those fields, that not only nonsense is tolerated, it is even supported – future charlatanism is reproduced by the present one.

2.5 An extremely short overview of postmodernism and its faults

Because this is a highly abused and ambiguous concept, it is difficult to say what is the "core" of it. Therefore, the aspects or approaches that I will mention can be accused of being "handpicked". Nevertheless, their inclusion will hopefully clarify certain "ideas" that are present in "postmodern" discussion. Bignell (2000: 3, 5-6) offers three dimensions to the question "What is postmodernity?": it is, firstly, a term that denotes a historical continuation with separable segments; secondly, it is a contrasting point to the modern (a comparative aspect); thirdly,

> it is a loose set of ideas critiquing Enlightenment reason, and having a Neitzchean flavour, combining Nietzsche's questioning of the categories of thought and of the status of theory itself, with a legacy of Marxian political engagement that stresses the relationship between cultural activity (or the lack of it) and politico-economic power structures.

Bignell also makes the reference to Lyotard's "grand narratives" which can be categorised as the opposition to the modern. Although this tells us very little, there is nonetheless a "hint" that can propel us further in the methodological direction. The hint is, of course, the critique of Enlightenment reason, i.e., truth, materialist philosophy, and the scientific knowledge as the highest or most reliable form of knowledge. A following summary of the critique is given by Ronkainen (1998: 237, 239-240):

> Postmodern thinking and critique of knowledge break two central assumptions of modern epistemology. Firstly, the realist assumption of knowledge is abandoned. No-one and nothing has the possibility to present statements that represent reality because there is no reality that is separable from its representation. Statements of reality do not describe it, they constitute it. At the same time truth-statements in general also become impossible, as well as the idea that scientifically gathered knowledge could better reach the truth about reality than knowledge acquired in some other way. To speak of knowledge means to speak inside a particular discourse, renewing and legitimising it. Scientific knowledge is but one possible local discourse about truth and reality. … Survey research is a form of practice which constructs reality. The way the data is gathered and the accepted conduct of analysis condition what can be asked… Postmodern critique prefers to examine knowledge as rhetoric or a discursive construct rather than knowledge of reality.

We can pick two main themes from this: 1) relativism and 2) linguistic (social) constructivism. We can dispose of the previous right away by, firstly, mentioning that it is an age-old issue traceable to Greek philosophy (not to all philosophers but to some) and, hence, there is nothing "postmodern" about it; secondly, we dismiss it by asking "Whether or not it is so, what particular effects it has or it should have on the actual conduct of science?" This is the main thing, after all, it is science we are interested in. Two possibilities emerge: a) science continues along merrily as it has done so far with all of its concepts of truth, knowledge etc. as legitimate courses of inquiry – this, by the way, seems to be Lyotard's (1985) view, after all, according to him "it is not obsolete to ask what is true or just; what is obsolete is to present science as positivistic and to condemn it as illegitimate knowledge, half-knowledge as the German idealists did" (ibid.: 86); b) we deny that science was ever possible, is, or will be, and, as a result, stop doing it altogether – maybe settle somewhere in the mountains and begin writing poetry. Sane people choose the previous (despite any potential difficulty contained in it) while, well, I am not sure at all that even the "postmodernist" themselves are choosing the latter, despite all the talk.

We can, rather easily, define relativism of the "postmodern" science as epistemological scepticism. It denies, according to Lammenranta (1993: 14),

> the possibility of "knowledge, justified beliefs, or some other knowledge-attitude about something. Scepticism of this kind can be local (concerning particular thing(s)) or global. If a weak form of global scepticism means the denial of absolute guarantees of truth, we call it fallibilism. It says that all believes can be erroneous, not that knowledge and justified beliefs are categorically impossible.

It seems that when some of the postmodernist are denying the basic premises of science, they are, in fact, talking about fallibilism – again, not a particularly postmodern invention – which, if this would be the case, is being and has been recognised in even such abominations of science as positivism. For instance Neurath (1997: 98) writes:

> The process of change of the sciences consists of the fact that at some particular time, certain sentences [but we could, in the postmodern sense, even use the term "discourses"] are often erased and substituted with other. Sometimes the form remains the same while the definition changes. Every law and every physicalist statement, in unified science or its real sciences, can be subject to this kind of change. The same holds true even for every protocol-sentence [which can also be understood as atomic-statement]. (brackets added)

This merely says, and it is difficult not to believe it, that there are no a priori guarantees of truth. On the other hand, if some of the postmodernists would actually be referring to the strong version of global (epistemological) scepticism, it would lead to impossibility of any form of science or even its critique because, if such a case is justified, the glasses used by both scientists and postmodernists would work only randomly, not any better than, say, stone pebbles. But not only glasses work every time, they work because the science of optics works to the extent that we are justified to refer to its results as being true. In a world where every "narrative" is equal, i.e., if nothing was true or, at least, truer, it wouldn't make any sense to go to the doctor. After all, in a postmodernist conception of the world the science of medicine and self-medication by eating only cookies should produce equally valid a remedy. If a postmodernist was truly practising what he preaches, the only reason he would go to the doctor should be random, for example, based on a coin toss. However, I suspect that the connection between a postmodernist's visit to a doctor and a real or perceived decline in health is closer to certainty than being random. And is it faith that compels us to go to the doctors, or is there something more to it? Certainly the element of faith cannot be excluded but this wouldn't explain the results of medicine which, after all, give much higher level of confidence than faith alone. And, the only reason the results are there – despite a healthy dose of charlatanism going on in medicine – is that they work by being true or truer than others. Transportation is another example. If what postmodernists say is true, it simply wouldn't make any sense to use a car or a train, for instance, since a broomstick should produce equal transportative potential. We should see postmodernists fly to work on a broomstick, or why not even on a carpet, rather than using a bicycle. But we don't. Why is it that some of the equal discourses of transportation are to be found only in fairy tales and some others have been actually put to use? A hint: the answer doesn't lie in Foucaultian powerstruggle over discourse dominance.

The fact that bicycles, cars, and trains work is that there is sufficient body of scientific knowledge underlying these modern day conveniences. Although Newtonian mechanics cannot explain everything – from large objects to subatomic particles - it is adequate now and will remain so in the future for the purpose it is being used. Certainly it explains better and gives working knowledge for transportational needs than, say, *One Thousand and One Nights* with its flying carpets. Considering that the postmodernist hardliners do, with a high probability, go to the doctor, use mechanised transportation, and read with the help of glasses, they must believe that something is not 100% relative, that something can be true and that we can have knowledge of this; that there is a very good reason to connect an expected functionality (theory) with empirical events such that they correspond and thus give us justification to call that relation as "being true".

Since there really are no hardcore postmodernists (relativists or skeptics) in the wild – personal claims of being one don't count if the person's concrete and empirical actions speak differently – and since they have not committed themselves to writing poetry, it must be concluded that they 1) conduct some kind of "research" which can only happen if 2) certain kind of unchanging and objective reality (in the sense that we can have knowledge about it) is assumed. I call this approach to science "the fake way" – and it can be considered as the c option. This "fake way" of doing things is a combination of options a and b, i.e., doing science as before and not doing science at all, only with a twist. This twist can be found in the following sentence: “Postmodern critique prefers to examine knowledge as rhetoric or a discursive construct rather than knowledge of reality.” So, the postmodernists – of course, not all of them – begin by denying the possibility of doing science (b); but reality, truth and knowledge are not really abandoned, for to claim how “statements constitute reality” implies, rather strongly, that there is a reality (created by god, nature, speech, postmodernists themselves, etc.), that it is true how this reality is created, and that through examining this process of creation (the discourse), we can have knowledge about it; what actually then happens, is that the traditional conception of science (a), is “sneaked” back into the “postmodern” conception while hoping that no-one will notice; the result, then, is the third way of doing science (c) which, after the dust has settled, is really science as before – at least at the level of basic assumptions – with only one change, or more rightly, one limitation: where traditional science sets no bounds on what can be researched (impossibilities like “the world spirit” don’t count) , postmodernism wants to, so it seems, limit research only to discourse, speech, or language, hence the term “linguistic” shift.

Of course, I am not the first one to notice this. The same or similar problem was observed already (in ancient Greece) by, at least, Plato and, in contemporary Finnish methodology discussion, by Töttö, who deals with this issue quite extensively. Both offer the same counter argument(s) that I formulated previously, namely, that by denying truth you assume that this is the true state of things (Plato); and that “if you claim that objectivity is but a mere illusion, does this mean that your belief is just a subjective opinion that doesn’t need to be taken seriously?” (Töttö 1997: 37). This is really a case of “talking about new things while really discussing the old” and is subsumable under the more general methodological discussion, the basic (socially constructed) dichotomies of quantitative vs. qualitative or positivism vs. hermeneutics.

It seems, then, that the issue of postmodern science revolves around the same division between the two worlds, which was present in the 100-year-old German philosophical discussion, i.e., that “we explain nature” and “we understand people”. It further seems that it is possible to locate a basic divide inside postmodernism on this issue. I am afraid that none of them are really new or unique. Firstly, there is the camp – I suspect that this is the “mainstream” – that has sworn allegiance to this dichotomous worldview. Whether we are dealing with the “general malaise” or “moody attitude”, for instance, as a form of art criticism inside the humanities, or the “quali-people” emphasising “rich thick descriptions” of personal narratives gathered, without the contaminating positivist thinking, we are dealing with the believers of the basic dichotomy. The second camp adopts an opposite view: that this separation is ill-founded. However, we can see that, as a scientific attitude, this has been done already by the positivists, whether we are talking about Comte or the later Vienna Circle.

This second way, then, is the rejection that it is possible to separate the two worlds now or that it ever was before. I think that we can count Lyotard as belonging to this category. (I say “can” because Lyotard, as a true French intellectual, writes in a highly confusing manner. From the combination of the lost of legitimacy, Brownian motion, and derivative functions, it is sometimes difficult to extract what is actually meant.) Husa (1997: 56) writes, drawing on Latour, that “the central idea of modernity’s foundation is the separation of nature and society [culture] so that the only possible entity classes are human and non-human” (brackets added). Now, when Husa or even Latour write about modernity, they are, in fact, writing about the Enlightenment. When Lyotard, then, speaks of Enlightenment as a narrative that has lost its legitimacy, it must be interpreted that Lyotard is also rejecting the accompanying concept of the two worlds. The way Latour dismisses the division, according to Husa (ibid.: 55), is through the recognition of “hybrids”, entities that are no more purely human than they are non-human. Latour (2006: 13-14) gives some examples of these hybrids: these are, among other, “the ominous growth of the ozone hole”, “the accusation of Monsanto’s and Atomchem’s CEOs from committing crimes against the humanity”, “the right to keep frozen human embryos”, etc. It is the entanglement of politics with science (or vice versa) that makes these examples “hybrids” (ibid.). This line of thought is a continuation on his previous work(s), most notably *Laboratory Life* which he co-authored with Woolgar. In that work, the dysfunctionality of the division is already explicitly formulated. According to Latour & Woolgar (1986: 281), the term “social” (as in social construction)

> no longer has any meaning. "Social" retained meaning when used by Mertonians to define a realm of study which excluded consideration of "scientific" content. It also had meaning in the Edinburgh school's attempts to explain the technical content of science (by contrast with internalist explanations of technical content). In all such uses, "social" was primarily a term of antagonism, one part of a binary opposition. But how useful is it once we accept that all interactions are social? What does the term "social" convey when it refers equally to a pen's inscription on graph paper, to the construction of a text and to the gradual elaboration of an amino-acid chain? Not a lot. By demonstrating its pervasive applicability, the social study of science has rendered "social" devoid of any meaning…

According to Husa (1997: 58) Latour "doesn't accept the semiotic or linguistic attempt to solve the basic problems of modernity" because these attempts end up renewing the basic dichotomy that is or was the result of modernity (Enlightenment). The basic classes of human and non-human are, then, idealised extremes of the same continuum. Husa (ibid.: 59) continues – still referring to Latour – that explanations of nature and society must rest on the same ontological assumptions. How does this differ from "traditional science" or from positivism, if at all? If we are talking about the basic ontology, then there is no difference.

We can, thus, see that unification between the two worlds, or, say, the denial that there ever was a separation in the first place, predates similar thoughts presented as part of postmodernism (at least if compared to Latour and Lyotard). And now, we should ask, what implications postmodernism, in any form, might have for science. The line of thought that subscribes to the view of strong global scepticism (relativism) makes science, in any form, impossible. This counterproductive approach can be scratched because, obviously, science has been possible, it is being carried out now, and it has given us reliable knowledge. The second approach, i.e., the "fake way" where all the denied premises are nonetheless "sneaked" back in, and which is after all only about directing attention at certain part of reality, can be scratched as well. Because it is internally confused about even the basic premises, whether methodological or ontological, it cannot serve as a reasonable guide for conducting science. Those that support "unified ontology" end up repeating, as new perhaps, the same principles that have already guided "traditional" science. Husa (1997: 57) writes that this new situation can be solved by adding a reflective dimension to (natural) science(s). Anyone can decide for themselves whether even this "new" reflective dimension is really that new.

To evaluate what postmodernism might mean – a little bit more concretely – or how it can impact the field of media studies, we need to partly start from the beginning, i.e., what is postmodernism. In addition to Bignell's rather meaningless categorisation we can add Jensen's (2002) views on how he sees the central elements of postmodernism. According to him (ibid.: 33) the term postmodernism was originally "introduced into literary and cultural research to refer to an antimodernist style in various arts." Although it can be found in literature, "it has been particularly associated with architecture" (ibid.). Postmodernism as a stylistic difference – a real one, of course, and not imagined – poses no principal problem; not for media studies, nor for science as such. A study of styles in art is actually exactly what Routila's book is about, i.e., how to study art, of which stylistic aspects form the most important issue. Scientifically it follows the basic logic of "observation and reasoning" in the sense that if or when claims about a distinct style are being made, there simply must be a correspondence between the two (claims and a style in the real world). Claims of a distinct style are, in principle, the same as, for instance, the claim that the moon is made of cheese: whether it is or it isn't must be decided empirically, armchair philosophising is no substitute here.

The two other elements of postmodernism, mentioned by Jensen (ibid.), are comparable to what already has been mentioned: that is, the so called crisis of Enlightenment with its loss of "grand narrative" and the stance that (scientific) knowledge is impossible. The former is or can be considered as having two dimensions, sceptical and moral. To deny the possibility or reason, rationality, or rational science is the sceptical stance, one that, hopefully, I have managed to show as totally baseless. (After all, making an error doesn't equate, logically and/or empirically, that only erring is possible.) This sceptical part affects science insofar as the claim of no possibility of knowledge is applied to research which is, at its core, quite paradoxical: something is researched without really being able to know anything through this research, or otherwise.

Moral overtones of the modern/postmodern discussion are visible when the questions of ought, should, could, etc., are being discussed. In this sense, the dimension of morality is not particular to postmodernism only, the object of discussion is that of the promises of Enlightenment, whether or not it has succeeded, and where are we heading or should be heading. Habermas (1984, 1989) defends “the project of Enlightenment” in the sense that, first of all, it is not yet over and, secondly, the rationality promised by Enlightenment can still be, in principal, achieved. For science in general and media studies in particular, the moral question of “ought this or that be done...” is beyond their reach. As Hume argued, we cannot infer how things ought to be from what they are now; science (of any kind) cannot give solutions to moral questions.

However, even this moral part of the postmodern discussion contains a part that is more or less scientific, and particularly central to both sociology and media studies: that is, the historical evolution of society regarding rationality. To the extent, then, that tracing this evolution is basically a fact based research, it will be compatible with scientific principles. (We can, for example, agree or disagree about the morality of witch-hunts, but insofar as they happened, is an empirical question answerable by empirical science.) So, for instance, Habermas (2004) traces the evolution from the old monarchy to the present (unfinished) modernity in three phases: 1) during monarchy private was public and public was private. This meant that the private life of the monarch was transformed into a public spectacle, while the real issues concerning the country were dealt behind closed doors and out of the reach of those it affected; 2) Enlightenment brought a change to this, private became private and public was opened to the discussing public – in principle that is. This change was never completed though because 3) during modernity, the spheres were again turned to resemble the situation of the old monarchy. Only this time, the responsible forces are commercialisation, mass culture, and basically, more or less, what the critical theory calls “culture industry”. Or as Curran (2002: 34) puts it: “the new mass media encouraged consumer apathy, presented politics as a spectacle and provided pre-packaged, convenience thought. The media, in short, managed the public rather than expressed the public will.” In *The Theory of Communicative Action*, Habermas (1984, 1989) can be seen as continuing on the same topic, for the idea of extending rationality (a promise of Enlightenment) to all spheres of life – and not only a particular version found in the systems of economics and law which force their “ways” onto the “lifeworld” (or habitus in a more Bourdieuan sense) – is the old idea of “enlighted” public discussing rationally about public matters.

According to Jensen (2002: 33-34) scepticism finds its theoretical foundation in the third element of postmodernism, “poststructuralism” or “deconstruction”:

> The analytical strategy is to expose internal contradictions in texts and to undermine their apparent intentions. The theoretical premise is that no textual meaning is stable, nor is any genuine human insight into oneself or others a possibility. ...the poststructuralist agenda is an emphatic scepticism and relativism. The aim is not merely to show that knowledge is uncertain ... knowledge as traditionally understood is said to be literally impossible.

At the risk of excess repetition, this theoretical part of postmodernism is a simple impossibility. It, like the denialist stance on the possibility of reason, are in the same way paradoxical at the core of their arguments and approaches. It is the same difficulty that was countered by Plato, i.e., that when one denies truth, one happens to assert “it is true that there is no truth”. How can it be possible to research texts and undermine their “apparent intentions” if we can have no knowledge of basically anything? Likewise, if no textual meaning is stable – or let’s say, stable enough – then there is simply no meaning to be found and all attempts must end in futility. But if there is no meaning then communication would be impossible, yet we seem to understand each other reasonably well which means that the postmodernist denial of everything must be wrong. In fact, the only unstable meaning is to be found in the texts produced by the scholars who continue to insist that the theoretical mess that is postmodernism is somehow correct. It is ironical that instead of “the world”, it is the academic jargon that has become meaningless. But as has already been mentioned, any kind of science based on strict postmodern thinking would be impossible. Some or many scholars in the human sciences, rather than fully admitting this, have taken the dishonourable way out of this difficulty by “sneaking” elements of traditional science back into “postmodernist research”. Unfortunately in media studies, as well as other human sciences, this attitude can go on unsanctioned – and it does.

2.6 Feminism

Feminism is, in many respects, equally problematic a concept as is postmodernism. But as there is a certain core to postmodern "thinking", the same can be said about feminism: under the many variations of feminism lurk identical principles. To the extent that this is not the case, it ceases to make sense to speak about feminism as something distinct. For instance, it makes no sense to speak of feminism, as an academic field at least, if the only thing that is supposed to make it different is that women are being studied; if we study the physiology of women it will still be "regular" medicine, or if we are interested in women's role in society, then it will be the same sociology, psychology, and the like., as always before. Made by a woman about women is not feminism. There is, additionally, a strong overlap between postmodernism and feminism, in their structuralist, contructivist, etc., orientations which result in the same things: denying reason and truth, support of total relativity, pushing ideology in the disguise of science, and, ultimately, leading to nonsensical claims.

We can find the answer to "What is feminism?" from Niemelä & Tammisalo (2006: 10-11), according to whom, feminism, at its core, is based on political and utopian ideologies fulfilling all the features of pseudoscience. Although their work is highly critical of feminism, the issues they have raised are not only difficult to counter, it would be impossible to do so. Instead of supplying the arguments with facts, objectivity, and other good scientific practices, feminism finds the power of legitimacy in moralising: once oppressed people must be from hereafter not only heard, they are necessarily right, in whatever they do or say (ibid.: 16). (This is where, from the outset, feminism collapses: it succumbs to the Humean ought-to impossibility, only in reverse. If Hume wrote that it is impossible to infer what ought to be from what is, the feminists are essentially trying to infer what is from what ought to be. One has to wonder, why such a disaster is cultivated in the echelons of academia.)

They continue that feminism is based on four pillars or main strategies (ibid.: 19–22): 1) full denial which simply "sweeps under the rug the patriarchal culture and science created by white heterosexual Western male, and substitutes it with brave new woman-science"; 2) word magic which is based on the belief that we can make disappear or solve difficult phenomena by simply renaming them. The authors give as an example the concept of socially constructed gender which is supposed to replace the biological concept of sex and, thus, solve the problems that biological factors might have on our behaviour, society, and so on. An obscurantist postmodern conceptual system is created which, should the need arise, can always be substituted with something equally obscurantist; 3) ideological control which aims at securing special privileges and treatments for "once oppressed people". It not only demands that feminist work is accepted by simply being feminist, but to achieve the aims various "dirty" manoeuvres are used, such as ad-hominem attacks, indoctrination, making threats, and discrimination; 4) bio-denial is to fully ignore all biological knowledge which results in the claim that behaviour and sex-differences of people are completely "socially constructed". To change those, one only needs to create other constructs (changing social conditions). But there is a fifth strategy that should be included – which isn't explicitly listed by the authors, though it is pointed out on many occasions – and that is the "women's way of knowing". As if, for instance, the classical conditions of knowledge work differently depending on what sex the "knower" might be. I suppose that since white males have formulated these classical conditions, women must have some other set of conditions. So far, unfortunately, no alternative set has been created – at least not one that wouldn't collapse due to its own impossibility.

Similar praise of feminism is to be found in Gross & Levitt (1994: 5th Ch.). The articles and books they analysed are equally nauseating, so any example of the articles will give a good idea why Gross & Levitt have such warm feelings for feminism. One would think that, for instance, the world of numbers is as neutral as anything can be. But, apparently, it isn't, at least according to an article, "Toward a feminist algebra" by Mary Ann Campbell and Randall K. Campbell-Wright. (This was the first of the more thorough analyses conducted by Gross & Levitt.) According to the authors (ibid.: 113):

> What passes for the idea behind this piece is that women and other disempowered groups are discouraged in the study of mathematics because most of the concrete problems they encounter – "word problems" or "narrative problems" of the "if-a-man-and-a-half-makes-a-dollar-and-a-half-in-a-day-and-a-half" variety – refer to situations that are sexist, racist, class-bound stereotypes.

> ...[Campbell & Campbell-Wright] disapprove of a particular problem in which a girl and her boyfriend run toward each other (even though the girl's slower speed is carefully explained by the fact that she is carrying luggage) because it portrays a *heterosexual* involvement. [Campbell & Campbell-Wright] object to a problem about a contractor and the contractor's workers (sex undeclared), because they assume that the student will envision the workers as male. On the other hand, they offer for our approval a problem about Sue and Debbie, "a *couple* financing their $70,000 home." Their general maxims call for problems "presenting female heroes and breaking gender stereotypes" and "analyzing sex similarities and differences intentionally" and "affirming women's experiences." All this, mind, is to be done in an *algebra* class. (brackets added)

The authors continue (ibid.: 114–115):

> The empirical basis for such an assumption is, as we say, dubious in the extreme. Generations of Jewish kids have done quite well at these problems, despite having to concern themselves with Johnny's Christmas money, rather than Menachem's Chanukah gelt; and in recent decades, an even greater cultural dissonance has done little to trip up vast numbers of young algebraists of Chinese, Korean, or East Indian background. ... However, even if we grant the pedagogical efficiency of feminist-approved terminology, and concede that it might help some reluctant young women to handle simple algebra [the article is supposed to deal with college algebra], the fact remains – and it is a fact – that anyone beyond the age of twelve or thirteen who has real difficulty with such problems, no matter what the social connotations of their wording, is simply not destined to be any kind of mathematician. A young lady who makes a game stab at "Maude and Mabel" problems but balks at "Joe and Johnny" versions of the same is almost certainly without the knack for abstraction that is an indispensable ingredient of mathematical talent. (brackets added)

It is not only these particular examples that are the target for criticism, it is the whole underlying premise that mathematics is "saturated with sexist ideology" that is so disheartening. Although further examples – dare I say "mind-blowing" – could be given but, nonetheless, it is best to close with Gross & Levitt's (ibid.: 116) summary of "Toward a feminist algebra" which, by the way, is an accurate judgment of feminism in general:

> Metaphor mongering is the principal strategy of much feminist criticism of science. It is invoked to accomplish what analysis of actual ideas will not. "Toward a Feminist Algebra" is a particularly childish example of this... The worst thing about this paper, however, is not its shoddy theory of mathematical epistemology. It lies, rather, in the fact that the ultimate aim of the authors is *not* really to advocate devices for improving the mathematical education of women and other disempowered classes. Rather, one finally discovers, the purpose is to justify the use of mathematics classrooms as chapels of feminist orthodoxy. The purpose of the carefully tailored feminist language and imagery is not primarily to build self-confidence of woman students, but rather to convert problems and examples into parables of feminist rectitude. It is, at bottom, not different from an imaginary Christian fundamentalist pedagogy requiring that all mathematics problems illustrate biblical episodes and preach evangelical sermons. Campbell and Campbell-Wright really want mathematics instructors to act as missionaries for a narrow, self-righteous

> feminism. Sermonizing – Christian, Muslim, Buddhist, or feminist – is not the function of science instruction.

It is, of course, easy to dismiss certain idea or theory by presenting only the critique of that idea or theory – though this doesn't mean that the examples presented by Gross & Levitt aren't despicable. A critique can always be accused of "handpicking", i.e., choosing only the bad examples while ignoring the rest, which can be even the large majority. If bad examples are particularly chosen, while knowing that they are not representative, a more or less any different sample should show different results – that is, unless ideologically motivated charlatanism accuses every bad example, even if it meant a whole field, as "handpicked". Because this thesis is not about feminism, I will have to keep the amount of my examples, and their scope, to a minimum. The first example is Helen Roberts' (1988) article "*Women and their doctors: power and powerlessness in the research process*". It can be said that the central point of this article is "the problem of the invisibility of women in sociological inquiry".

Now, I will admit, directly and openly, that I am not quite sure what Roberts is talking about; there are different streams of "thought" that are partly overlapping each other and partly make very little sense when taken together. It is basically written in such a way as to enable the disposing of any critique by saying: "Well yes, I meant something else..." I will, therefore, pick some things that I consider sensible (there is very little of this though) and those that are mostly, well, less sensible. Roberts (ibid.: 9–10, 28) begins by stating that women basically visit the doctor more often than men; that, it is assumed, what ails these women is "sociosomatic" (refers to physical conditions which are attributable to social determinants rather than to psychological states in origin); and that the doctor is, to a large extent, used as the "source of attention and sympathy as well as a source of compensation for the frustrations and inadequacies of their daily lives."

The disadvantaged state of women – the sociosomatic ailments – is based on the following statement (ibid.: 9):

> We held that the social and economic structure of modern industrial society systematically causes women to be disadvantaged educationally, occupationally, and in other ways. This

> disadvantaged position ... may have as its result vague feelings of dissatisfaction and minor worries and complaints.

Unfortunately, the claim that the "economic structure ... systematically causes women to be disadvantaged" is not evidenced in any way. This is extremely important because whether women were exploited in the 19th century has very little bearing on the working of contemporary society. It is an example of the "once a victim, always a victim" argument which may work in political rallies but not in science. When I say that the argument is not evidenced doesn't mean that Roberts did not try to legitimise it in any way, just that what she did is not even close enough. She basically tried to add credibility to the argument by (ibid.: 11–12, 14): 1) conducting a limited number of interviews with some women; 2) doing content analysis on medical literature (i.e., how that literature portrays women); 3) by comparing that claim with other feminists' similar claims – but, as we all know, such a comparison can only establish a matter of coherence, not a matter of fact. With the claims like "All men are bastards" one should be above all interested in a matter of fact, i.e., that of correspondence between the claim and reality.

From here a jump is made to the claim that sociology is patriarchal which means that it is saturated with male sexism which begins by male experience and tries to generalise that experience to the women population without actually bothering to find out about the experience of the "disadvantaged" (ibid.: 14–15). Roberts continues (ibid.: 15):

> The ideology is pervasive and largely unarticulated, but it is expressed within sociology by methodologies which ignore sexual divisions and do not 'see' the experience or situation of women. The symptoms of this are familiar, such as the assumption that statements about social class can be made on the basis of male occupations, and that generalisations can be made about all participants from an all-male sample.

This statement exhibits the same fallacy as was the case with "feminist algebra" example: namely, that as mathematics is not about Joe and Susan or Mabel and Margaret but about sexless numerical abstractions, similarly in sociology the different methods are meant to be used in a "sexless" way, to study what all people share rather than what separates them. But even if we were interested in the particulars of some group, women or men, what would be the methodologies that do not ignore “sexual divisions”. Frankly speaking, I cannot think of any and neither, for that matter, can Roberts. For some mysterious reason, the methods of traditional science are equally usable no matter how large, small, similar, or distinct group(s) – any object really – we are trying to study.

However, not everything Roberts writes is nonsense. For instance, she may be partly or even fully right by claiming that it would be a mistake to make generalisations about a population (men and women) based only on the sample of men. She would be right in the sense that if we were interested in certain qualities in which men and women differ then it would be necessary to study both the sexes. This is the sensible part. Unfortunately, there is nothing “alternative” or “feministic” in this proclamation. The requirement that a sample should represent a certain population is a basic principle of empirical science; there is nothing new, revolutionary, or alternative about this notion. And if a study fails in this respect, then it is simply an example of bad science, which is fully criticisable and solvable through traditional science. Furthermore, Roberts is wrong if she thinks that we cannot generalise based on only male or female sample. She seems to forget that actual process of research is one of fit, i.e., that whatever is being done, must fit the purpose or goals. In this sense, then, it is perfectly legitimate to “generalise” from a strictly male or female sample as long as we can show, in any way, that we are justified in doing this.

Considering that Roberts, already at this point, has nothing much to stand on doesn’t come as a surprise. Simply put: this article is a perfect example of what is wrong with feminism, as a science that is. At least Roberts makes it perfectly clear that her article, or feminism, has really nothing common with science (ibid.: 15, 17):

> Feminism is in the first place an attempt to insist upon the experience and very existence of women. To this extent it is most importantly a feature of an ideological conflict, and does not of itself attempt an 'un-biased' or Value-free' methodology. ...we may as feminists allow ourselves to criticise as biased those sociologists who continue to produce work which is sexist in its theories, its methodology, its practice and its application

And because the bulk of sociology – or any other science apparently – is sexist in, well, everything, feminists are allowed to attack it in any way whatsoever. Of course, the all present sexism is not really shown, it is alleged. But because once oppressed is always right, there is no need of actual evidence. Once the requirement of evidence is disposed of – after all, it is probably also patriarchal, phallic, sexist, you name it – what we are left with is a default situation where sociology (or any other science) is sexist and feminist writers are allowed to write any kind of nonsense that they manage to dream of.

The end of the article sums up nicely the goulash of what is feminism (ibid.: 27): "feminist sociologists, in arguing that gender should be taken into account in theory and in practice, are arguing for more and not less vigorous methods." As was already mentioned, inclusion of gender (or sex, really) is not a sign of a more or less vigorous methodology or science, it is a question of fit: included where it makes sense, omitted where sex is not an issue. It may come as shock but not everything is about "gender". To see gender everywhere is a bit like seeing a communist everywhere; a fixation like that can easily lead to concrete human suffering, as was the case of McCarthyism and as is the case of, for instance, sex-quotas in educational, administrative, or corporate positions which is nothing else than a return to the old aristocratic birthright and disregard of personal achievement.

The second example is an article written by Rolin (2006) – a female philosopher of science, who has mostly written about gender issues in science – which was a "review" and critique of Niemelä & Tammisalo's book. This was not so much a review as it was an attempt at refuting the presented evidence for and the arguments of Niemelä & Tammisalo. However, instead of successfully refuting the critique directed at feminism, Rolin's article ends up more or less conforming with the kind of feminist literature that fuelled the disagreement in the first place.

Rolin (ibid.: 57) tries to refute two points of criticism raised against feminism: 1) that it is contradictory with established natural science(s) and that 2) it is pseudoscientific. What Rolin has in mind with the contradictory status is the issue of "bio denial", i.e., that feminism, allegedly, totally disregards the effect of biological factors and thus, simultaneously, the fields of biology, medicine, etc., and claims instead that "gender is socially constructed". As to the first point, Rolin's counterargument is divisible to subparts of which the first one is basically the claim that Niemelä & Tammisalo are essentially wrong because some of their examples didn't contain an explicit and word for word content that would have claimed sex being entirely a socially constructed phenomenon, or that biological factors do not at all affect behaviour (ibid.: 58).

As a true feminist, Rolin's argument is a perfect example of obscurantism and "word magic". Why is this? Well, Rolin, as a philosopher of science, must be familiar with the fact that language contains both words and combinations of words that are synonymous; that, simply put, we can say the same thing by using different words. If we say 2 + 3 or 3 + 2, we are saying the same thing since both will result in 5. Similarly, if two or more sentences or larger combinations of words will lead to the same result, whether as another combination of words or as a particular action, then they must be considered as having the same meaning. Therefore, one doesn't have to say explicitly that "sex is being entirely socially constructed", it can be stated in any way that produces that same result. One of the examples that Niemelä & Tammisalo (2006) used – and with which Rolin didn't agree – contained the following part (p. 25):

> Sex doesn't ... particularly have an origin, nor does gendered being [unfortunately, I couldn't come up with a better translation] express any core sex emanating from inside the subject. ...heterosexuality, from this point of view, doesn't represent any one and only natural sexuality, instead it is just one form that is being produced, reproduced, and normalised through enormous cultural resources. (brackets added)

Maybe Rolin has read a different book by the same title and by the same authors, but the above citation seems awfully lot like bio denial – and it is not the only example of its kind. Anyway, she continues that "studying gender as socially constructed is not contradictory with the fact that gender [sex] is also a biological phenomenon and that it affects behaviour in a certain amount." What she means by gender, or being gendered, is all the meanings attached to, well, "gendered issues"; these are, among others, "physical features, clothes, profession, and sports. Rolin (2006) adds, rather diplomatically, that “what meanings are attached to what, by whom, and in which contexts, is essentially a question for empirical research. (p. 58)

It is remarkable that Rolin, who is interested in the different “gendered meanings”, is suddenly requiring a literal statement that “biology doesn’t matter”. What makes this remarkable is the fact that the “meaning-seekers” try to find meaning anywhere else but from the literal. This is not surprising, since the meaning-seeking journey would be somewhat short-lived if the scholars simply restated what is/was already written, said, or how this or that person acted. No, a true voyage for meaning is performed, in the postmodern spirit, in what is essentially a relativist and non-empirical sphere of pure conjecture.

But the absolutely best bit is that Rolin basically performs a “denial of a denial of a denial”. So, first Niemelä & Tammisalo write that feminism commits a “bio denial” which Rolin denies, but then she denies this denial by the simple fact that she – as other feminists, postmodernists, etc. – separates a human being into two mutually independent spheres; in the end those of nature and culture where it is legitimate to study only the cultural which in reality must only happen at the expense and by denying the natural. If the natural or biological affects the cultural then by what bizarre logic shouldn’t it affect the different “gendered meanings”, or any meanings for that matter? Of course, biological factors may not be that important in everything cultural, as is the case in mathematics, for example, but to hand wave away the biological factors in such central issues as sex/gender is not only bad argumentation, it is precisely an example of “bio denial”. If heterosexuality is a “cultural product” then we must ask what produced it. Feminists would probably say something along the lines of “from or by oppressing patriarchy”. Biology would say something like “because without sexual reproduction, which is necessarily heterosexual, there would be, for instance, no mammals, including humans”. After what has already become a failed argument, Rolin muddies it even further by invoking von Wright’s concept of intentional action and the age old refuge of human scientists and philosophers, that of the worlds of “understanding and explaining”. When the human scientist gets into a difficult situation, her or she victoriously states that none of their mistakes are actually mistakes since they don’t try to explain anything and that “understanding” doesn’t require any of those things anyway. Unfortunately, it is never really made clear what this “understanding” is supposed to mean concretely.

As to the attempt at refuting that claim that feminism is, ultimately, pseudo-scientific, Rolin is not able to produce much better counterargument(s). First of all, she claims that Niemelä & Tammisalo have not produced any example of feminism being relativist. Either Rolin is, yet again, demanding a literal and explicit formulation of this – in which case she would probably be right, i.e., that no such explicit formulation was given – or she is reading some other book which, surprisingly, has the same name and authors but totally different content. Rolin is also trying to mystify the concept of “objectivity” by claiming that it is not clear what Niemelä & Tammisalo mean by it, since in feminism and epistemology there have been discussions about how this concept is to be understood (ibid.: 59). Although epistemology has been mentioned, the references are only about “feminist” writers which validates a further criticism of feminism: that it is by the feminists for the feminists. Furthermore, there is very little to be discussed about objectivity, considering that serious science has been conforming to it for quite some time now. There is no shortage of literature about what is objectivity. (The fact that we can find “philosophers” who are prepared to deny anything and everything does not give much support to arguments. But most importantly, the non-objectivists have not managed to explain how certain (confirmed) results in science could have been achieved through subjectivism. Although the choice of problem, qualities, metric, etc., maybe “subjective”, their repetition and establishment is not, which relativists of all sorts are only too eager to forget.)

Although Rolin’s counterargument (against feminism being pseudo-scientific) is composed of five sub elements, I will mention, additionally to the already presented, the second one. Here Rolin tries to refute the claim that feminism, or the claims of feminism, are in principle untestable. Niemelä & Tammisalo (2006,: 109) have written, for example, that

> the field's untestability, uncritical approach, and stagnation is easily noticeable from the fact that [feminism] continuously holds on to the same claims disregarding counterevidence and criticism. Some of the claims are formulated in such obscurantist fashion that is impossible to test or even refute them.

Rolin argues against the claim of untestability by stating that Niemelä & Tammisalo do not present evidence (or example) what so ever, where a feminist writer would stick to her claims even in the face of counterevidence and criticism (2006: 59-60). Again it has to be concluded that Rolin must have been reading some other book where, it is possible, there are no such examples. And, again, she expects the same strictly literal examples as before. It is a shame that she needs to resort to such tactics, though, on the other hand, it verifies the anti-feminist critique.

Of course, one reason why there will be not much explicit and literal counterevidence from natural scientist is the fact that feminist-articles are not really published in the spheres of natural sciences – for the simple reason that feminism, in all its incarnations, doesn't correspond to any of these fields of natural sciences. Therefore, not only the natural scientist have no obligation to read feminist material – and comment it – they don't even come into contact with it. To the natural scientist feminism is as a distant thing as is, say, geography to a music historian. Furthermore, Rolin is mistaken if she expects that feminist writers can write anything they want, i.e., making claims that contradict established scientific knowledge, and that it is the duty of others to react to this (preferably favourably of course); that first the feminist does something and then, second, comes to reaction from natural sciences. The counterevidence is not coming after for the simple fact that it exists already before the feminist output. If a feminist makes a claim that contradicts or refutes, say, an established biological fact, it is the responsibility of the feminist to produce necessary evidence, not the other way around. So far, however, feminist writers have not produced any credible evidence, though moralising and sermonising has come in abundance. In this sense, then, Niemelä & Tammisalo's book is full of examples that show how feminist thinking "sticks to its guns" despite available counterevidence.

As to the part of feminist claims being untestable in principle, Rolin (2006) offers the following “defence” (p. 60): “obscurantist claims may be found in feminist research but this is not, according to my experience, a problem only in feminism.” Not only Rolin leaves the accusation of untestability more or less unchallenged, but one has to ask, what or why does it matter if something similar is happening elsewhere too. This is basically the case of the “two wrongs make a right” argument. However, such an argument does not work in the court of law, nor does it work in science – and for a very good reason. Also Niemelä & Tammisalo specifically critiqued feminism and, whether they are right or wrong, it is completely irrelevant if they didn’t condemn, say, literal criticism with the same vehemence as was the case with feminism. So, in a nutshell, Rolin’s article is not much of a book review; it is, however, a good example of what feminist writing is about. The positive thing, though, is the fact that there are much more horrible examples than her article.

The third example – representing communication research and, hence, media studies – is an article by Rush & Grubb-Swetnam (1996), titled “Feminist approaches to communication”. As the title promises, the article should be about an alternative approach to what already exist out there. If anything, one hopes to find out something about communication. So lets see whether it is your “typical” feminist action pack with the compulsory phrases and jargon, while at the same time managing to completely avoid the actual topic, namely communication research. Although one shouldn’t “judge a book by its cover”, or an article by its beginning, it is difficult to keep one’s mind “open and neutral” when the article opens like this (pp. 497-498):

> The thesis of this chapter is that the integration of theory and research ... may have large parts of the scholarship missing, distorted, or coopted—and that it is our responsibility, all of us, as scholars to have the awareness, knowledge, and ethics to at least note, as in footnote, that this may be the case. We have made a conscious decision ... to use women's full names rather than initials, when possible, as a first mention in the text. Women's voices are silenced in ways that are covert and overt: In this small way, we want to indicate that we are trying our best to get out of that black hole of nonrecognition through gendered lack of acknowledgement.

The opening of the article jumps, commendably, to the point: that women are oppressed by nothing else than the great evil of patriarchy in science, and it is their responsibility to change the situation. They continue that the primary purpose of feminists "is to add to and enhance an academic discipline by contributing and establishing new, inclusive, and different perspectives and approaches" (ibid., p. 498). It is difficult to take this seriously when the authors have already established without any evidence that there is a categorical oppression of women going on in science. It would be strange to think that the wrong doing of men didn't reach the androcentric methodologies as well. Because if it didn't, there would be very little point in creating "new, inclusive, and different perspectives and approaches." Naturally, then, the overthrow of male oppression also includes the overthrow of male epistemology. The best bit of the article must certainly be the following (ibid.):

> Myths, philosophies, theories, and research have perpetuated male standpoints for some time. Marija Gimbutas (1989) and other archeomythologists note that the repeated disturbances and incursions by the Kurgan people (who Gimbutas views as proto-Indo-European) put an end to Old European culture between 4300 and 2800 BC, changing it from gylanic to androcratic, and from matrilineal to patrilineal. "The Aegean and Mediterannean region and western Europe escaped the process the longest. . . . Old European culture flourished in an enviably peaceful and creative civilization until 1500 BC, a thousand to 1500 years after central Europe had been thoroughly transformed" (p. xx). Gimbutas summarized, "We are still living under the sway of that aggressive male invasion and only beginning to discover our long alienation from our authentic European Heritage—gylanic, nonviolent, earth-centered culture" (p. xxi). Attitudinal and behavioral adjustments take time, and the swing back to a gylanic culture, a social structure in which both sexes were equal, has begun. But that change will be difficult if particular areas of scholarship are silenced, ignored, or disregarded.

Not only the beginning of this quote is a repetition – more explicit – of the same dogma with which one is usually greeted in feminist writing, but what on earth have the Kurgan people to do with contemporary communications research? Of course, the "Kurgan people" have nothing to do with communications research what so ever, though, they have very much to do with the myth of "ancient matriarchal society". It is a shame, really, that the authors have failed to mention that Gimbutas' claim according to which Europe was matriarchal, or "goddess-centred" has not been much supported beyond those already with the desire to believe in such myths, e.g., feminists. Hence, there is no "swing back" to the "good old days" as the authors wish. On the other hand, the fact that, at least Western society, has become "more inclusive", which means that despite the "androcentric" past of human kind, there is a trend to greater equality, means that men are "not as evil as feminists portray". Ironically, the situation has, in some cases, become the total opposite: it is the man who is oppressed. But it is, of course, impossible for a feminist to admit that a positive change could have come from men as that would demolish the categorical claim that “men are despots and women are the victims” and which is used as holy water to crush all dissent. Anyway, the argument of the authors so far is based on the “the victim is always right due to being a victim” fallacy, that the only reason some women science is disregarded is because of male oppression. But since, obviously, some women science is accepted one has to wonder, couldn’t it simply be that the rejected science deserved to be rejected in the first place? Today that evaluation is difficult to be made since the human sciences accept everything, and it is particularly here where “women science” takes place.

The remedy, according to the authors (ibid.), is that the students “must not be put off or put out by the terms feminist, feminist theory, feminist research, or women's issues.” But this raises more than one question. First of all, if the students are “put off” by feminist thought, could it be, even slightly, possible that it is not necessarily because of male oppression but, rather, that feminist thinking hasn’t been able to produce the kind of content that ought to be taken seriously as scientific input. Despite what feminists might think of themselves – which is obviously that they can do no wrong – in the field of science, only the content should decide the worth of it, not who wrote it. Secondly, if the content is “scientific” and that the only thing which holds women back is “male oppression”, then two things must be shown: 1) that the content is scientific which means that political propaganda or purely faith-based proclamations (no matter how politically correct they might be) simply will not do. But feminism depends on the ideological and even fraudulent, otherwise it wouldn’t be feminism. If it was really about, say, criticism of science, i.e., what has been done wrong or something similar, it would simply be science as it would rest on the established principles that not only enable science to exist but also to criticise it when these principles have been abandoned in this or that particular research; 2) even if the content would be scientific, the obligation to show evidence in support of “male oppression” would still stand. And no, referring to other feminist’s damnation of androcentric science – based on equally non-existent proof, mind you – will not pass for evidence, at all. High-fiving each other can be fun, at a political committee meeting, for example, but science doesn’t work or isn’t improved by mutual agreement alone, no matter how emotional that agreement might be.

If this was the case, then we would still be drilling people's heads as a remedy to demonic possession, in order to release the evil spirit. Fortunately for science, but especially for ordinary people, there were physicians who, eventually, disagreed with such "medication". To repeat myself: a claim is not valid just because it is coherent with other similar claims, nor does the validity of a claim depend on its popularity; science is not a democracy. Because, then, feminism lacks substance – purposefully it seems – it can only find support through popularity. However, that can be only achieved among those whose political zeal clouds their judgement, or those who have no real idea about science in the first place and who show great talent at suppressing their common sense.

But let us move to another inspiring point of this particular feminist creation. The authors continue that (ibid.: 498–499):

> Reading or conducting research about how women and men "are portrayed in stereotypical ways that reflect and sustain socially endorsed views of gender" (Wood, 1994, p. 234) is not only interesting but necessary to an enlightened social scientist in a world where women and men are transforming gender roles. The mass media, for example, distort reality by underrepresenting women in ratios to white males by 3:1 in prime-time television and 2:1 in children's programming, or including men in newscast stories 10 times more often than women...

What do the authors mean exactly by stating that men and women are "transforming gender roles"? Are fathers, in fact, transforming into mothers? Or does it mean that we have reached a higher level of "gender equality" where it is "okay for men to bake pies at home while the women work in coalmines and steelworks"? But all this is really irrelevant from the scientific point of view: science will be what it is even if men gave birth to children and women grew thick beards for the simple reason that science is asexual or "gender-neutral" if one likes to use newer terms. The fact that this or that particular female scientist's work was overlooked doesn't make science sexist or discriminatory against women; male scientists overlook, degrade, and ridicule, even other men's works. Of course, the authors, like many other feminists, are not really interested in science as such, it is the political call to arms, to change society, to make it pure according to the feminist image. And this image has, in many place, already taken its first steps, for instance, in the form of women-quotas which seems to be the desire of the authors as well. After all, they are unhappy that women are "underrepresented" in the mass media. Considering that much of the mass media are private businesses, isn't it their right to choose as employees – actors and news anchors are exactly that – whomever they like? Or that if "guy-shows" bring revenue, why should they be changed to "women or women-and-guys shows"; by whose moral superiority? I suppose that to truly achieve "gender-equality" old literary works are to be rewritten since, for example, they contained too many male characters: *The Good Soldier Švejk* obviously misrepresents all the female soldiers of the first World War which will be remedied by a new version called *Loretta Bravely Dismantles Male Chauvinism in the Bleak World of European Patriarchy.*

After five pages of your typical feminist "thought", the authors finally seem to have something to say about communication research. They open with strong confidence when they say that "contributions to the communication discipline by feminist thinking are extensive" (ibid.: 504). I suppose it will be an exercise in male oppression when I say that "feminism having brought extensive contributions to communication research" is a total surprise to me as it is, quite likely, to actual communications/media researchers – for instance, a feminist manifesto is not a contribution to any particular scientific field, no matter what scientific field is included in the title. The writing of these authors is a case in point; the same article could have been published in any field of science. But why stop there, the same could be done in art as well under the title: Feminist Approaches to Art. Of course, there would be nothing about art (or anything else) really, just the same tirade about how oppressed women are. To continue, the authors now invoke the power of the high priestess of feminism, Sandra Harding, when they write that (ibid.)

> Theories of feminism and feminist research have provided communication scholars insight about how the scientific model has figuratively and literally paled through comparison with other frameworks which indicate sexist, racist, homophobic, classist social projects (Harding, 1991).

What are, exactly, these “alternative” frameworks compared to which the scientific model pales; and what exactly is supposed to be the scientific model? These are, of course, pointless questions because they aren’t and never have been intended to be actually answered. (It is clear what is meant by the scientific model: androcentrism, white male oppression, and the likes. Naturally, then, such evil pales in comparison with the alternatives.) The problem with ideology is that once one tries to seek answers and explanations – no obscurant “understanding” – the empty machinery of sloganeering will turn out to be just that, empty. This fabulous article can be summed up with the following (ibid.):

> Through three decades of current feminist scholarship, we have learned that women's communication, along with minorities of both gender, have been "othered" or silenced in mainstream research. Gender theories and feminist research in communication have helped to reveal that we must be mindful in future research of actively refusing to continue the silencing, drawing out instead and making visible those who have been silenced, revealing their voices in social and historical context.

As a lesson in propaganda, this article is perfect. So what if it actually doesn’t say anything useful about feminist approaches to communication.

What these articles have, hopefully, shown is what utter failure they are scientifically. Although a further analysis of the different mistakes could be carried out, I will point only one which, coincidentally, covers the rest: lack of professionalism. And it is not that only absolutely perfect results will do; everyone makes mistakes – even the best. But there is a huge difference between a genuine attempt at learning from one’s mistakes – and in a long term science’s ability to correct itself seems to have been happening – and not bothering at all with all the pesky requirements of science, just because moralising, sermonising, and politicising has been successful in diverting and suppressing genuine criticism and concern for the future of science. Furthermore, if obscurantism and, simply put, all out bad science has been able to bring “recognition”, fame, and a position in the ranks of academia, why should the “scholars” even try?

3 MEDIA STUDIES

I will admit from the start that this, a sort of review of media studies, is going to be superficial, and it should be not too difficult to point out that "if only you would have read this or that book or article you would have known..." and present similar counter arguments. While a feminist text, or even postmodernist, will show surprisingly little deviation from their underlying assumptions, styles, etc., such is not the case in media studies. This doesn't mean that it has managed to get rid of these counterproductive influences – which presently can be even considered as the dominant ones – but that in addition to these, no matter how little, there are alternatives. Ironically, by "alternatives" I mean either remnants or "conversions" to that of traditional science.

A problem for a review like this – even if it was more thorough – is that of categorisation. This doesn't mean whether or not any kind of categorisation is possible in the first place but whether it suits the purposes. It is, then, a question of fit. For instance, we can divide the different works by the sex of the author which would make sense if we were interested in the amount (in percentage) of works written either by men or women. But the same categorisation would be completely useless if we were interested in what kind of arguments in favour of or against positivism are being made.

The standard way of classifying media studies – if, indeed, it can be called a standard – is usually presented in a historical form, i.e., what questions have occupied researchers during, say, the last hundred years or so. This also includes the mentioning of shifts in philosophical arguments, or if one wants, the paradigm changes in philosophical assumptions – and accompanying actual research. A historical development like this could be presented as a change from positivism to postmodernism or even from hermeneutics to positivism to postmodernism. The "point of origin" depends on whether one counts the field as already established, its precursors, or if one wants to start with, say, the beginnings of modern science in general, and so on. Then again, no historical analysis is necessary if one is interested in what is going on now or in the last 30 years or so. But even a mix of both, a little bit of history combined with contemporary situation can also be done. A success here is not and cannot be prefixed beforehand; only based on the results can we say if the choice of approach was justified or not – providing, of course, that the aim and approach have been “connected” in some sensible way already.

I will start the overview of the field by 1) sketching, on a very basic level, the main themes or evolutive steps of the field; 2) giving some examples of what has been done. This will, mainly, include work about newsgathering and publication. Now, there are categorisations even about news research. For instance, according to Hjarvard (2002: 91–92), news research can be divided into four types: a) gatekeeper tradition which is about what is selected and by whom; b) news flow analysis; c) empirical studies of news content such as researching the coverage of some event by different news outlets (newspaper, television, radio). Such work is, then, strongly comparative; d) more theoretically laden approaches. The examples that I will be using, in my view, do not fit well in any of these categories. One reason is that there are strong normative and moral overtones present in such works – Chomsky is a perfect example. These are a mixture of all four approaches, and, perhaps, to consider them simply as continuation in Lippmann’s footsteps might be better; 3) but it is the scientific nature and status of the field that is most important here, and nothing speaks more clearly than the explicitly philosophical and methodological contributions written; for instance, works dealing with the methodological side of communications research (or media studies). (I use “communications research” and “media studies” interchangeably.)

3.1 The main phases and central ideas of the field

There is something that can be considered as a “school book” categorisation of media studies. Naturally this comes in endless permutations where each author or a group of authors give a “new spin” to what is essentially the same thing. This is, by the way, one of the dilemmas of science: a new researcher must publish; but to publish what already has been said doesn’t make sense; only a few have something truly new or important to say; therefore whatever is being published must at least give the appearance of being new or important. Unfortunately the endless stream of “new” has the habit of silencing the old and in many cases higher quality publications. The re-spinning is certainly evident in media studies – as in any other field – and when the layers of novelty are peeled away what remains is essentially the "same old, same old". Additionally, a lot of the differences are due to taste rather than anything else. Below I will mention some examples of the different approaches to what is, essentially, the same thing, namely the temporal evolution of media and thusly what media studies have been and are about.

So, for instance, there are some introductions (categorisations) to media studies which start the historical tracing from Aristotle's rhetoric (see, for example, Nordenstreng 1975: 237–238) while many do not. Curran (2002), on the other hand starts from the 17th century press, its subsequent evolution, and ending with radio and television. Although Curran's "twist" on the matters is the democratically enabling potential of media or, conversely, its use in an exercise of power, it corresponds to the media-effect type of literature – despite the fact that Curran concentrates on earlier periods, a lot of what he writes about overlaps with many other sources, i.e., that the same or similar categorisation is to be found. One of the themes that join Both Nordenstreng and Curran to other sources, is Marxism. The former incorporates it in a modified version, i.e., Marxism-Leninism as an argument in favour of the possibility that media studies (or communications research) can and should be objective. The latter (Curran) incorporates Marxism from the class-struggle and political economy point of view, i.e., media as potentially a weapon used by those in power exacerbating the class-conflict and, on the other hand, to what extent the question of money (profit, required start-up cost, etc.) affected the possibilities of establishing competing news outlets – not necessarily competition in the sense of revenue, but as offering an alternative to the mainstream (or, if we want, an alternative to the "ruling ideas").

Perhaps one of the most neutral overview – or as close to "school book" version as possible - of media studies (or more widely, communications research) is offered by Pietilä (1997), who divides the field into three main phases; three in the sense that it is possible to speak of them as not completely heterogeneous. During the first phase one of the leading ideas for theorising and research was the question of propaganda in the media and by the media. Names such as Lippmann and Lasswell belong to this era. The second phase, known as mcr-tradition (mcr = mass communication research), distinguishes itself with names such as Katz and Lazarsfeld. During that time two themes emerge: firstly, a shift from theorising (perhaps even unsubstantiated) to empirical and scientifically more rigorous research; secondly, this "scientific approach" was intended to show that the old "hypodermic needle" model of media was incorrect, i.e., that the idea where media unilaterally and directly "dictated" what its consumers ought to do and think simply didn't work; that the situation was more complex. Critical theory (and theorists) paralleled this in some ways and diverged in others. The idea of mass society was central for the critical theorists, though more from the Marxist point of view. Main elements were those of class struggle, the effect of capital, and deterioration of "high culture" as it is being substituted more and more with mass produced "low cost" pop-culture.

If the idea of mass society was to be found in both – though the origins of that idea might have come from different sources – the general "method" of research was completely different: critical theorists favoured, well, theory over actual empirical science. This is not a surprise as one of the major components of critical theory was the critique of Enlightenment, especially by Adorno and Horkheimer. In this sense Habermas, also categorised as a critical theorist, is different: he doesn't condemn Enlightenment. For him it is a sound ideal that has a possibility to happen. Adorno is a cultural pessimist while Habermas is an optimist. But where Habermas continues the tradition is in the critique of postivism, especially its epistemological notions. The third phase began as an opposition to the mcr-tradition, although not necessarily to the Marxist critical theory. Again two trends are discernible: 1) a re-emergence of Marxist thinking and 2) cultural studies approaches which included also such influences as feminism and postmodernism. (It should be pointed out that these things didn't happen strictly one after another; for instance, semiotic analysis – whether or not explicitly connected to hermeneutics – was not only already visible in the cultural critique of critical theory, but also the different cultural approaches including postmodernism.) Similarly, relativist notions can be found in the works of different thinkers and during different times. The Marxist side of the matter still can make sense if or when research deals with the "effect" of media as a tool for hegemony or renewal of class differences, or the political economy side of the matter (to what extent money influences the creation of news, for example). As to the culturalist trend, it is difficult to say much. This is due to, as was mentioned earlier in relation to feminism and postmodernism, the basic obscurantist and contradictory nature of those approaches.

During these three examples the word Marxism was strongly present which is not surprising as Corner (1998: 11–12), for instance, identifies three central elements in development in media studies: Marxist, linguistic, and ethnographic perspectives. In this sense, Marxism in media studies is the result of influences by newer thinkers, such as Althusser, as well as the "rediscovery" of critical theory (ibid.: 12). One of the two main questions of Marxist approach, that of political and cultural power, is often formulated as the question of ideology. Corner (ibid.: 13) writes that

> the most developed form in which the 'ideology' question was posed, that of Marxist structuralism as exemplified in the writings of Louis Althusser..., quite quickly became established as canonical within the new field, both in teaching and in research. This influence was part of a more general structuralist influence, most notably that of a Semiotic analysis of images and of language, coming through from writings such as those of Roland Barthes ... and Umberto Eco..., which themselves referred back to the ... writings of the linguist Ferdinand de Saussure. ...the 'Marxist-structuralist' focus became the guiding paradigm in the broad sector, in different ratios of mix...

The above already shows that Marxism was closely collaborating with the linguistic tradition. To the extent that the linguistic part has been pushed into the front, especially in its strong social constructivist and relativist form, we can speak of a "linguistic shift" not only in media studies but in the human sciences as a whole. The questions about language

> were not simply questions of applied linguistics, asking how the media used language, they were questions of a much broader kind about the linguistic ordering of society and consciousness. The structuralist anthropology of Lévi-Strauss, neo-Marxian concepts of ideology, Freudian analysis of the unconscious and, more directly still, the Semiotics of Barthes and Eco, all posed question about language structures or language-like structures. Language was seen as a key, perhaps the key, to the understanding of cultural and social organisation. Semiotics ... fitted into this intellectual perspective as a practical analytic system which could be immediately brought to bear on the products of the media and a wide range of contemporary cultural expression... (ibid., p. 14)

Although semiotics seemed to provide a flexible analytic tool even for the analysis of images, its results were not particularly convincing – which is exactly the case even today. Corner gives an example of this in the form of Barthes' essay on the photograph:

> the difficulty with images was that of finding an analytic unit equivalent to words and a combinatory convention equivalent to the sentence. ...analysis, far from achieving scientific precision, was hard put to get beyond the socially impressionistic... These problems were compounded by the extent to which Semiotics was put to the service of ideological analysis, revealing the political and social shapings and purposes of a text in ways not made explicit in the text itself. The identification of 'myth' stems not so much from close attention to the levels beneath it as from the analyst's own political sense of what constitutes the 'mythic' in contemporary culture. The procedure is dangerously circular, and whatever results by way of political insight is more a result of the prior political knowledge and intelligence of the writer than of any method or procedure of textual study. (ibid.: 15-16)

Related criticism is given by Barrat (1986: 118), according to whom

> Semiologists, anxious to uncover the complex coding of media messages, suggested that certain audience interpretations were more likely, certain readings of the text were 'preferred'. From the first phase of media research they took the view that the media played a prominent role in shaping ideas, while recognizing the criticism, raised by the later 'two-step flow' [the second phase] researchers, that audiences were capable of putting forward alternative readings. Even so, the question of how certain members of audiences are able to produce alternative interpretations of media texts remains unanswered. What evidence is there that the 'preferred reading', carefully unravelled by the semiologist, is indeed the view that audiences actually take? (brackets added)

A concrete example of the difficulties inherent in the "semiotic method" is Danesi's (2002: 25) analysis of a "magazine ad for Airoldi men's watches that was common in Italian lifestyle magazines published in the early 1990s." Danesi states these properties of the ad: 1) An Airoldi watch has apparently been 'stabbed' by a woman's hand holding a dagger; 2) the woman's fingernails are painted with nail polish; 3) she is wearing a man's ring on her thumb; 4) a finger-less leather glove covers the woman's palm; 5) a diamond-studded handcuff is discernible on her wrist. These are then followed by their "interpretations", in cultural forms: a) The stabbing suggests some form of violence, perhaps of the 'prey hunting' variety; b) the woman's painted fingernails suggest sensuality; c) the man's ring is probably that of her lover; wearing it on the thumb suggests that it is one of the spoils of the 'hunt'; d) the finger-less leather glove is suggestive of sadomasochism; e) the diamond-studded handcuff reinforces the sadomasochism imagery, implying 'capture' and 'captivity'.

The first part of Danesi's argument in favour of his interpretation is that "the 'female-as-huntress' image that this ad generates has a mythic etiology in Western culture". He continues that "the image of a fierce and sexually powerful female surfaces in all kinds of popular narratives - from ancient myths such as that of the Greek goddess Diana to contemporary female movie characters seen in Hollywood films such as Fatal Attraction (1987)." (ibid.: 25-26.)

Now, should semiotic analysis work, as a functional tool of science, it should be possible for anyone who uses it correctly to arrive at the same conclusion like Danesi. Provided, of course, that semiotic analysis is an analytic tool and not merely a euphemism for projecting the researchers own emotions at the picture. But semiotic analysis is not a tool; there are no steps to be followed, just as Corner mentioned. There are no units to be combined nor is there a method (or methods) how these units should be combined. This is evident in the different books or articles where semiotics is presented as a method or part of it – especially when intended as something particular, and not as some vague truism like "semiotic analysis demands a strong intellectual effort from the researcher" as if there are ways of doing science without this effort. (It should be emphasised that the "preferred" reading, or a reading that is as close to a universal one, is a compulsory requirement if semiotic analysis is to be even remotely regarded as a method. If texts, pictures, etc., are something which anyone will, eventually, "interpret" in any way whatsoever – that one "symbol" could have a never ending stream of meanings – then not only will it be an euphemism for one's own mental projections, it will also be a term void of any actual content.)

Anyway, Danesi's first part of the argument, the etiology of the "female-as-huntress" image, is, I guess, supposed to be a "Well, everybody knows that..." type of argument. Unfortunately everybody does not know. Furthermore, it is a complete mystery how the Airoldi ad can be connected, in such an obvious way as Danesi writes, to, for example, the Greek goddess Diana. (It certainly didn't emerge in my thoughts in any obvious way.) Not surprisingly, then, Danesi (2002: 26) offers the second part of his argument (in favour of his interpretation), according to which

> In order to establish the above interpretation of the ad as a plausible one, clearly, the context in which it has been fashioned is a key factor. The term context in semiotics refers to the real-world conditions - physical, psychological, historical, social, etc. - that ultimately determine how a sign is made and what it means. The interpretation that I fleshed out of the Airoldi ad was made possible by my own knowledge of the fact that it was directed towards a female audience, and by my knowledge of the mythic themes that were available to the ad-maker.

The only thing that is missing is the piece of information where Danesi would have written that the ad-maker actually said what was the intention of the ad. I mean, what Danesi refers here to as “contextual information” is something that people do not necessarily have while “interpreting” images, for instance. There is very little semiotic interpretation going on when the interpreter has virtually all available information about the symbol.

Since the linguistic approach where only the researcher does the interpreting part – followed by a hefty chunk of theorising – it was only natural to extend it in the ethnographic direction. Although ethnography is usually connected to anthropologic research, there is no reason why it couldn’t be used in communications research. It is, after all, about observing (participatory or not), interviewing, etc., human beings as individuals and larger groups. The emphasis is on the cultural side of individuals and societies. One thing that makes the ethnographic approach sensible – in principle, not necessarily in practice - is that it goes beyond the interpretations of a single person, the researcher. David Morley is said to have produced one of the pioneering works of ethnographic research with his study of the audience for the British news magazine programme “Nationwide”. According to Barrat (1986: 125) Morley initially conducted a semiotic analysis which was then followed “up by field research by interview in which Morley tried to establish whether audiences did in fact read the programme in this way.” Unfortunately, at least based on Barrat’s description, Morley’s research, although better than wild “interpretations” of single scholars, raises as many questions as it may have provided answers. First of all, there were only four groups doing their alternative interpretations which cannot be considered as a representative sample of the society. Therefore, this would only say that there can be, indeed, other interpretations – which is not necessarily a breakthrough result. Secondly, a considerable amount of the difference in interpretation was actually a difference in agreement; that the groups "understood the message" in more or less the same way, just that some agreed with that message while others did not.

A shift towards ethnographic type of research has, as already mentioned, certain advantages. But in the end, even ethnography – especially its modern day variants emphasising meaning and which, ultimately, shares the shortcomings of the incorporated hermeneutic core – has its severe weaknesses. It is not so much a question of ethnography as such, rather, the issue is what, mostly artificial, limitations are thrown upon it in the name of postmodernism, anti-positivism, "understanding", and the like. Thus, what plagues the single interpretation paradigm is to be found in the multiple interpretations paradigm as well. By this I mean the relationship between an interpretation and how or with what purpose it is being used. If a researcher is interested in meanings, and nothing else, then there is not much cause for complaint. However, almost every work that supposedly deals about meanings, i.e., those that try to "understand" and not "explain", slip into making claims that demand much more than mere interpretations or what meanings these or those people might have constructed.

The problem can be illustrated by the following imaginary example: if we were interested in what meanings different patients attach to, say, their medical treatments, then the hermeneutically oriented ethnography can be useful. It will offer, naturally, less dependent results than a large-scale questionnaire-based study because nowadays "quality" with "thick descriptions" and "deep interviews" are preferred. As time-consuming procedures, they have to be limited to what is usually only a handful of people. On the other hand, if we were actually interested in how effective, if at all, the treatment is, the meaning-creations of the patients will turn out to be a very poor source of evidence indeed. This is because, say, cancer, its mechanism, and possible treatment have nothing to do with the meaning-constructs of the patients; they involve different questions and different ways in how these are answered. The point is then that ethnography as a universal ideology, where voice is given to the voiceless – where the objects of research "are finally allowed to speak" – is simply unsuitable for the many questions and claims that demand much more robust, more "scientific" approaches. We will return to this matter in chapter 3.3.

3.2 News research

This chapter is intended as something of an interlude, though not in spirit. Again methodological (or scientific) interest prevails here. News research serves as a "case" example of certain methodological choices, whether explicit or implicit (though mostly it is implicit), made in that kind of research. Although news research can be divided in different ways, I like to divide it into, firstly, a "plain vanilla" or common sense approach. This can, and in many cases does, contain normative, moral, etc. overtones though the core lies in the empirical investigation of news. It can be considered as combination of historical and comparative approach were news articles are chronologically listed and compared to other news articles but also to any other sources, such as monographies, statistical information, and so on. There is or can be a question such as "Is the news media lying?" or "How war influences what is supposed to be non-partisan news reporting" which is then evaluated through the empirical findings of what the media have actually done. The second way of doing news research, although it has quite a lot in common with the first, is one that is much more affected by constructivism, relativism, or, what can be generally regarded as postmodern leanings. These are not necessarily completely bad studies but, unfortunately, they tend to go overboard in the sense that the claims are simply not matched by the proposed evidence – a phenomenon that troubles all the human sciences. My interest lies primarily in the common sense approach.

I would like to start the case-examples with Lippmann. He is relevant even today not necessarily for his scientific insights – a fact-oriented attitude was not special, for instance, when he wrote *Liberty and the News*, in 1920, though that same attitude might be today, in human sciences, something of a novelty – but for pointing out what an important role is played by news outlets which for many are the opinion-forming sources. And, of course, opinion – whether based on facts or wishful thinking – underlies (public) action. His concern was the safeguarding of democracy which would be impossible in a world where people are being fed lies. He wrote (2008: 2) that

> Everywhere today men are conscious that somehow they must deal with questions more intricate than any that church or school had prepared them to understand. Increasingly they know that they cannot understand them if the facts are not quickly and steadily available. Increasingly they are baffled because the facts are not available; and they are wondering whether government by consent can survive in a time when the manufacture of consent is an unregulated private enterprise. For in an exact sense the present crisis of western democracy is a crisis in journalism.

(It should be added that though he was speaking about private enterprise, history has shown that state-governed/owned news outlets have not fared any better of which the former eastern block is concrete evidence.) It wasn't only corruption that was seen as the underlying reason. Additionally, and perhaps even more,

> Since the war, especially, editors have come to believe that their highest duty is not to report but to instruct, not to print news but to save civilization, to keep the nation on the straight and narrow path. Like the Kings of England, they have elected themselves Defenders of the Faith. (ibid.: 2–3.)

Moralising of all sorts, then, is one of the most important factors, according to Lippmann, that erodes the factuality of that information on which, eventually, we build our worldviews. It is at its worst when it happens from top to bottom; when an "enlightened" elite thinks it knows what is best for the rest, or as Lippmann puts it (ibid.: 4):

> It sometimes seems that after the armistice was signed, millions of Americans must have taken a vow that they would never again do any thinking for themselves. They were willing to die for their country, but not willing to think for it." That minority, which is proudly prepared to think for it, and not only prepared, but cocksure that it alone knows how to think for it, has adopted the theory that the public should know what is good for it. The work of reporters has thus become confused with the work of preachers, revivalists, prophets and agitators. The current theory of American newspaperdom is that an abstraction like the truth and a grace like fairness must be sacrificed whenever anyone thinks the necessities of civilization require the sacrifice.

The importance of this is, then, the following: "The news columns are common carriers. When those who control them arrogate to themselves the right to determine by their own consciences what shall be reported and for what purpose, democracy is unworkable" (ibid.: 5–6).

Compared to, say, present day feminists or postmodernists, Lippmann was (and still is) a visionary, representing the same androcentric "evil" that feminists are hell-bent on fighting, or the kind of basic "positivist" attitude that postmodernists would eagerly like to see go away. Feminism, actually, is the same kind of moralism to which Lippmann was opposed; one that happily sacrifices truth and fact as long as it serves the "higher purpose", no doubt the only true purpose, at least in the minds of its supporters. As Lippmann (ibid.: 5) observed, this is just another "the end justifies the means" doctrine; It is nothing but the doctrine that I want what I want when I want it. Its monuments are the Inquisition and the invasion of Belgium [here Lippmann is talking about the first world war]" (brackets added). The postmodernists are seemingly in a better position than their feminist counterparts because there is, on the whole, less moralising. However, it is equally destructive in its approach due to extreme relativism and the claims that there is no truth or validity outside, or course, these claims themselves. But if there is no truth, there cannot be facts, and in such a case there is no need to report them or in any other way mention them. In such a case, news reporting would be indistinguishable from fiction and fantasy. But as Lippmann (ibid.: 6) noted, "no one can manage anything on pap. Neither can a people. Statesmen may devise policies; they will end in futility, as so many have recently ended, if the propagandists and censors can put a painted screen where there should be a window to the world."

To correct the situation, Lippmann suggested certain things that should be done. Above all lies factuality or, at least, the aim to reach it. Because reporting is not easy – facts don't drop readily from the sky – it is imperative for the journalist to adopt a scientific attitude for his work. To put it differently, journalists should adopt exactly the same basic "logic", basic approach to how things are being done which is the method of "observation and reasoning". For the scientist as well as the journalist this means that things are critically searched and questioned. (Lippmann's suggestions were more concrete than this, but for our purposes their underlying core idea suffices.) This leads us to Lippmann's epistemological principles, which were never explicitly mentioned. However, they are easily extrapolated from his critique of journalism and the proposed remedies. But not only this, further evidence can be found even in his following work, *Public Opinion* (1965), originally published in 1922, where his tone was considerably more pessimistic. Anyway, the four main epistemological assumptions of Lippmann's work are: 1) empiricism. The journalist, like a scientist, works in the field, digging up information, and not by arm-chair conjectures; 2) facts. At the end of the day what decides quality of reporting are the obtained facts. The present day "objectivism" of journalistic process where "both sides of the quarrel" should be heard is really no substitute at all. Although it is nice if those involved can voice their "story" but it doesn't do much if that story is a false one; 3) objectivity. This goes hand in hand with the previous point: it is simply impossible to have a non-objective fact. On the other hand, it is quite possible to have a subjective conjecture. People can and do act on both but it is much better and honest to act on the former rather than the latter. (It is worth repeating that, for example, a national policy which is based on fables has no chance whatsoever of doing any good. Policies in the name of made up security threats are oppressive whereas economic measures that rest on "information and estimates" obtained from the private banking sector end up, in the long run, being even more damaging than their many alternatives.); 4) the common sense notion that there is an independent real world. For instance, in *Public Opinion*, Lippmann (1965) writes about "the two worlds": the real one and the one that exists as a distorted image we created in our heads. This is not intended in a Kantian sense where we cannot have knowledge about the real world; we certainly can. But there are certain reasons why the picture inside doesn't correspond with the world outside (ibid.: 18):

> artificial censorship, the limitations of social contact, the comparatively meager time available in each day for paying attention to public affairs, the distortion arising because events have to be compressed into very short messages, the difficulty of making a small vocabulary express a complicated world, and finally the fear of facing those facts which would seem to threaten the established routine of men's lives.

It was Lippmann's thought (or a moral requirement) that the journalist – or basically anyone in a "fact-production" position – should bring these two worlds as close to each other as possible. Formulated like this, it is basically the same idea as modern science has: we may not know the "universal truth" but it is our responsibility getting as close to it as humanly possible. So far science has done exactly that. (What Lippmann became pessimistic about was whether the "common man" (anyone really) was competent and ready to sacrifice the dream world for the real one.)

I call this epistemology "plain vanilla" in the sense that it is not explicated, not by Lippmann nor by many others, yet it is clearly present in their work. (Though, scientifically speaking, a precise and robust explanation of one's work is to be preferred to one where the reader must infer from the text the implicit methodological assumptions; it doesn't mean that a somewhat unclear work should be automatically discarded. As long as the researcher or author has done the correct things, even if they have to be "dug up", well, the researcher has done the correct things.) Below I will introduce some examples that, I think, can be thought of as a continuation of Lippmann's work, in spirit rather than as a particular kind of content.

Phillip Knightley's (2003) *The First Casualty*, is a detailed exposition of how truth, especially during wartime, is the first thing under attack. Journalists, despite their code of "objectivity", seem to end up doing everything except the kind of reporting that tries to uncover the truth. It is a sad story about what the combination of censorship, lack of journalistic principles, and partisan ideologies can do to news reporting. In Knightley's work, many of the issues that Lippmann pointed out and criticised – and wanted to improve – are, not so much brought out to daylight, but re-established; that the same things that were done wrong before, are being done even today. This isn't so much a case of the news consumer not being able to abandon his dreamworld as it is about the fact that news reporting is still filled with moralising, corruption, and the like; that men still cannot base their action and knowledge of the world on facts.

Now, Knightley's methodological solution was rather simple: compare what actually happened in history with what was reported (or purposely left out) in the newspapers (or radio, tv). Furthermore, there was no specific theoretical framework involved. Nor was the work "theoryless" in the extreme grounded theory sense, as there was a strong, though silent, idea-foundation without which the whole work would not have even started. There were the ideas, among others, that 1) truth is possible; 2) it is the journalist's responsibility to write truthfully rather than deceivingly; 3) censorship has a strong effect on reporting; 4) reality exists and is principally knowable; 5) there is an adequate amount of existing evidence which can be used in reconstruction of historical events. Obviously Knightley also must have had the assumption – regardless what might have influenced that assumption – that news reporting is far from the "tell it how things are"-notion. In fact, such is the general approach that it is possible, quite painlessly, to fit it into Hempel's covering law model. After all, we have the observations (how events are reported and under what conditions); we have the law-statements (that patriotic or any moralist notions tend to affect negatively factuality); and, finally, we have the result, the explanandum, that the factuality of news reporting is, in a general sense, going to be diminished especially whenever news reporting finds itself under the influence of moralims of any kind. Based on this we could proceed to make a prediction that whenever news reporting is going to be is similar situations, we are going to see misleading news.

Now, if it was the case that there is no truth, no objective reality, etc. – or that if at least Knightley believed so – there would be no reason to write the book; it would simply have been impossible. But similar impossibility would have been reached if Knightley adopted the politicised moralising of the feminist or any other fashionable style of the human sciences: although there could be facts, these would have ended up being overlooked because they wouldn't fit with the dogmas of the ideology.

Andersen's (2006) work is almost a mirror image of Knightley's. The type of content, its spirit, and underlying assumptions are that same in both these works. We can also find the same problem of the two worlds as described by Lippmann, i.e., the objective one with factual events and our distorted images of it. For instance, Andersen (ibid.: xvi) writes that

> Most civilians experience military conflict through the signs and symbols of its depiction, their impressions derived not from the battles in distant lands but from the manner they are rendered at home (television, newspapers etc.). War stories are constructed from the bits and pieces of favoured myths and stories of past battlefield heroics.

This leads to Andersen's central theme of her work: propaganda and manufacture of consent or, in her own words, "how to make war, which at the most basic level is defined by suffering and death, an acceptable practice in contemporary democratic society" (ibid.: xvii). While the notion of "manufacture of consent" may point to a constructivist/semiotic direction, such is really not the case. No-one denies that it is possible to substitute a factual world-view with a fake one. As Lippmann pointed out, it is impossible for us to personally gather all the information that exist and as such, we have to rely on (un)professionals who gather that information for us. We have no choice but to rely on the information which gathered, packaged, and presented by others. No wonder, then, that propagandist or otherwise inaccurate information is used as the building blocks of our world image. But this certainly doesn't mean that all voices, discourses, etc., are equal. In Andersen's work we can see the same emphasis on narrowing down the difference between the real and the imagined world by better journalism: "conventional news narratives that present one view, then another, all too often fail to provide enough background information so that viewers can understand the situation and evaluate both claims from an informed perspective" (ibid.: xiv). This is nothing else than the common sense approach according to which there is reality and we can have knowledge of it. The difference is that in Andersen's work this notion is extended by the fact that not only can this be done, it ought to be done.

Chomsky's more politically oriented works, again, fall in the same category that are being dealt with. It can be said that the news research present in Chomsky's writings is something of a sideshow rather than the main course. And it is not that the news research part is not voluminous or in other way detailed, it's just that it serves to legitimise the key moral argument: that democracy ought to be protected. However, this cannot be done if our actions "rely on pap". But it is not only the "trash reporting" that is problematic; regardless of the quality of news outlets, the democratic right of the people to act, to participate in the democratic process, has been denied. It is not only a question of national policy; there have been many "thinkers" whose opinion was that common people don't know anything and their access to decision-making should be barred, rather than improving the matter so that they could know. According to Chomsky (1991: 359)

> The ideas that common people should be excluded from policy-making, ... have ample resonance until today. This doctrine remains a basic principle of modern democratic states, now implemented by a variety of means to protect the operations of the state from public scrutiny: classification of documents on the largely fraudulent pretext of national security…

We find, as already discovered by Lippmann, that censorship is one of the main obstacles of factual reporting and democratic process as well. And yes, when a government acts in secret, it immunises itself from scrutiny, legal proceedings, simply put, from all sorts of challenges directed against its domination. Other, equally detestable, means of public control is propaganda which has time and again come in the form of "it's for the national security" argument. According to Chomsky (ibid.: 2, 5)

> As if by reflex, state managers plead "security" to justify their programs. The plea rarely survives scrutiny. We regularly find that security threats are contrived – and, once contrived for other purposes, sometimes believed – to induce a reluctant public to accept overseas adventures or costly intervention in the domestic economy. The factors that have typically driven policy in the post-war period are the need to impose or maintain a global system that will serve state power and the closely linked interests of the masters of the private economy, and to ensure its viability by means of public subsidy and a state-guaranteed market. To a significant extent, the threat of Soviet Union and other enemies has risen or declined as these ends require. The tacit assumption is that the public welfare is to be identified with the welfare of the Western industrial powers, and particularly their domestic elite.

This is quite evident, for example, in the economic "restructuring" of the former eastern block where foreign companies were given what only can be considered as completely ridiculous and unfair advantages; all, of course, supposedly serving the nations' needs. But what about Chomsky's epistemological assumptions? Well, it is certainly possible to infer them from his texts and they would correspond, more or less, with the tenets of traditional science, as was the case of the previous examples. However, in this case it is better to let Chomsky (2003: 93) himself formulate his own thoughts about science, especially of postmodernism, post-structuralism, post-everything:

> I have spent a lot of my life working on questions such as these, using the only methods I know of; those condemned here as "science", "rationality," "logic," and so on. I therefore read the papers with some hope that they would help me "transcend" these limitations, or perhaps suggest an entirely different course. I'm afraid I was disappointed. Admittedly, that may be my own limitation. Quite regularly, "my eyes glaze over" when I read polysyllabic discourse on the themes of poststructuralism and postmodernism what I understand is largely truism or error, but that is only a fraction of the total word count. True, there are lots of other things I don't understand: the articles in the current issues of math and physics journals, for example. But there is a difference. In the latter case, I know how to get to understand them, and have done so, in cases of particular interest to me; and I also know that people in these fields can explain the contents to me at my level, so that I can gain what (partial) understanding I may want. In contrast, no one seems to be able to explain to me why the latest post-this-and-that is (for the most part) other than truism, error, or gibberish, and I do not know how to proceed

As a last example, we can mention McManus (1994), whose work represents the political economy approach. The concern for democracy is present in the work, as well as a certain kind of "the common man vs. financial elite" struggle, but it is the former combined with an empirical study of how market factors, i.e., the "bottom line", affect the accepted journalistic standards, those of objectivity and factuality. McManus' concerns for democracy, for the possibility of knowledgeable public who can act upon that knowledge, etc., are more or less the same that already Lippmann wrote about, and what the previous examples also exhibited. We do not repeat them again. There is also very little difference in underlying "basic logic" of how things are done, though, on a more detailed level, McManus' work represents a more "scientific" approach in the sense that he tries to actually explain the methodological part of his research. This is the more interesting part and it also serves as a transitioning device between this and the next chapter.

As any "real" research, McManus' work can be divided into two main parts: 1) what has been actually done (methodologically) and 2) how this has been explained to the reader, i.e., what methods were chosen, why, and how they were used. This latter part can delve more deeply into principal philosophical thoughts but it is not, strictly speaking, necessary. There are two main reasons why it makes sense to clarify methodological choices: a) so that the reader doesn't have to guess what has been done and why and 2) to provide a schema or a "building plan" of the research so that anyone could try to replicate it and, thus, give more weight to the results.

What McManus has done is, scientifically speaking, adequate – more of this later. However, some of the provided explanation could have been better. He states (ibid.: xiii), for instance, that the collected data was followed by

> the theory-building tactics of Barney Glaser and Anselm Strauss ... in what they call the Constant Comparison Method. The approach is iterative. Research begins with a theory explaining some behaviour that's available from past scholarship. The researcher tries to find the most likely point of breakdown of the theory and collects data there. The theory is amended over time with its weakest link continually subjected to test. When it has passed all of the "devil's advocate" tests that the researcher can devise, the resulting theory may be offered to others.

Compared to the overall work this is really a minor issue but, I do feel that it represents quite many studies in the human sciences and, so, certain issues should be pointed out. First of all, grounded theory is, to use Chomsky's words, a combination of "gibberish, truisms, and error". What McManus refers to as "the constant comparison method" originates from Glaser and Strauss' (1967: 103) who have this interesting bit to say:

> the constant comparative method is not designed (as methods of quantitative analysis are) to guarantee that two analysts working independently with the same data will achieve the same results; it is designed to allow, with discipline, for some of the vagueness and flexibility that aid the creative generation of theory.

This raises several issues. Firstly, one has to ask, what kind of method allows x number of different researchers to arrive at x number of different results? For a scientific mind the answer is easy: a method that produces random results has nothing to do with being a method. For instance, it wouldn't be much of a method if, say, adding 2 and 2 resulted in person A's calculations as 4, person B's as 5, person C's as 78,2, etc. Either these people would be bad mathematicians or there would be no real method of addition. And the same applies to the "constant comparative method". This part is clearly erroneous. Secondly, the claim that, on the one hand, this method has something to do with being "designed" and, on the other hand, to allow "disciplined vagueness and flexibility that aid the creative generation of theory" is clearly a contradiction in terms. What on earth can "disciplined vagueness" mean? How are we to proceed vaguely, though with a discipline? This part, then, falls under the category of "gibberish". And thirdly, "creative" generation of theory. If anyone could have possibly devised a method for the generation of theory, they would probably get annually Nobel prizes for it, just as a show of appreciation. Although this may come as a shock to the supporters of grounded theory, there hasn't been any method of discovery, there isn't one now, and there won't be in the future. (A side-effect of such a method would be that all scientists became unemployed; a computer could create all the theories, etc., simply by following the algorithm of this method.) This part is, again, erroneous. Now, obviously the generation of theory contains vagueness and flexibility but there is simply nothing methodological about it; it just happens, somehow, in favourable conditions. An additional issue, found in the excerpt from McManus, is that of a truism – though equally true for what Glaser and Strauss wrote additionally about this so called method. What McManus writes, for example, that “research begins with a theory explaining some behaviour that’s available from past scholarship” or that “the theory is amended over time with its weakest link continually subjected to test”, he (or Glaser and Strauss) is merely stating how science works in general; this is in no way exclusive for “the method”. What Glaser and Strauss effectively did is comparative to feminists and many other non-natural scientists: the use of word magic. In this case it means to come up with a catchy name and “market” it as something new when, in fact, it is only repackaged ancient knowledge.

Fortunately McManus manages to redeem himself with the help of his actual conduct of the research. In modern terms the approach could be referred to as “triangulation” or as “transgressing the boundaries between quantitative and qualitative research”. Yet, after closer examination, these terms turn out to be as hollow as the contemporary jargonistic nonsense tends to be. (Luckily, McManus doesn’t use these terms himself.) Triangulation simply means that instead of being a purist regarding the data set, method, or theory, the researcher combines these into a more robust framework (Eskola & Suoranta 1998: 68–69; Anttila 2005: 212). The idea behind triangulation is, quite straightforwardly, that by multiple X approach – where X can be a theory, method, and so on – the results will be more valid than, say, in a single method approach. But this is nothing new. Science has done this even before the term was coined. For instance, when people and their action is studied, an all-out eclecticism makes much more sense than an ideologically “pure” approach. This “purity” is more of a problem for qualitative research because quantitative science has never lacked those aspects of research that the “alternative knowers” try to deny from it. Especially if we refer to the quantitative approach as simply “traditional science” we find that such things as interviewing, observing, theorising, etc., are an internal part of it. In this sense traditional science uses, by nature, triangulation.

McManus, then, is a traditional scientist because, in this work, he combined at least the following things: multiple case study, ethnography, interviewing, and journalistic as well as economic theories. The first of the list refers to the selection of “four local television stations located in the western United States, each affiliated with a major network" (McManus 1994: xiii). As an example of the ethnographic part we can mention the following observation (ibid.: 100):

> [at the mid size station,] reporters were observed covering 16 stories. None involved such active and time-consuming processes as developing sources or searching documents, or sitting through government meetings. Taken as a whole, the case studies describe a minimal commitment to actively examining the doings of local government and business. The business model of news discovery prevailed.

The interviewing part finds support, for instance, in McManus' statements that "every television journalist interviewed said the ideal newscast should cover events in abbreviated form. Television's job is to "boil down" the information available and capture the main points of a story. Secondary details should be left to other media." (ibid.: 177.) And, lastly, the use of different theories can be exemplified by the uses of market model and journalistic model of news discovery where, according to the former,

> it's reasonable to assume that passive discovery of events – when television journalists read about them in local and regional newspapers or wire services or in press releases – is less expensive than more active means, such as hiring and deploying reporters or field producers. So if a station acts rationally, a business model would predict largely passive discovery, or at least as passive as competing stations permit. [Where as according to the latter, to] maximise public understanding of its environment, the fundamental mission of journalism, news departments must actively and independently scrutinise their environments. A journalistic model would predict largely active discovery, or at least as active a discovery process as the station could afford. (ibid.: 96.) (brackets added)

Such a "multimethod" approach, then, enables McManus to reach the conclusion (ibid.: 197) that:

> in fact, rational market journalism must serve the market for investors, advertisers, and powerful sources before – and often at the expense of – the public market for readers and viewers. To think of it as truly reader- or viewer-driven is naïve. …the stations in this and other studies did not add entertainment to information creating "info-tainment" so much as they displaced and often distorted information in favour of whatever they believed would attract attention at the least production cost. Most of the time, market journalism is an oxymoron, a contradiction in terms.

The point of this chapter was not to show what is the dominant type of research done in media studies (or communications research) but, rather, to give an example that, firstly, things can be done right, and also how. This doesn't mean that the mentioned examples are perfect; that they represent the absolute best scientific research. They do not. But they do represent the same common sense approach to doing research as is the case in traditional science. Above all what separates these examples from many of their fashionable contemporary counterparts is the “connectedness” between claims, statements, or questions and how these are, subsequently, supported. For instance, both good and, well, worse research can write about Gramscian hegemony but by being an empirical claim, it simply must be supported empirically; personal beliefs and strong “feelings” are no substitute. But in addition to being empirical, the support must also fit. Many works fail because, while being “empirical”, it turns out to be wrong kind of material; one that might support something, though, not what has been claimed.

3.3 Media studies (or communications research) methodology

While different examples of particular studies can say a lot – and it does – about what goes on in a field, the most telling source of information is to be found in the form of actual books or articles that specifically deal with methodological issues (of the particular field). Of course, actual studies can offer equally illuminating collections of thought when or if they contain as explicit formulations of the issues as is to be found – one expects at least – in the methodologically oriented literature. This chapter will be written similarly to the previous ones in the sense multiple cases are going to be presented following with either praise or condemnation.

Same kind of criticism can be directed against the chosen examples as with everything presented so far: that it is not representative of the field; that I have carefully hand-picked the kind of examples which support the conclusion that I might have decided upon from the very beginning. Personal assurances are a cheap currency in science, so nothing of the sort will be offered. However, there are two things that add support to the possible conclusions, things that do not depend on subjectivity: 1) media studies (or communications research) methodology is not unique. There is nothing in the epistemology of this field that doesn’t have its origins – and, hence, the underlying idea of how this or that works – somewhere else, be it natural sciences of the last 300 years, ancient Greek philosophers, or, say, the variations of hermeneutics. Thus, methodology of media studies represents a wider – or older – philosophical perspectives. If, then, there is a book on methodology that deals or recommends a "qualitative approach" in dealing with the "media messages", my possible criticism of it is justified insofar as that work corresponds to a whole body of literature that deals with same problems. In theory the presented examples could be unique, however, what makes or could make them unique is not the words "media messages". Beyond these words, what we are dealing with, is the world of empirical objects which has been discussed long before media studies emerged; 2) there is a world of difference between bad results and bad results. Bad results are justified when we don't know any better, or that in that particular moment or era there wasn't better information available, nor were there the means to produce such information. Ignorance is not when something is unknown. However, when the necessary information exists and we choose not to use it, that, then, is ignorance. Bad results are part of science; it is unavoidable due to the nature of scientific knowledge. The only way to correct this is if we had the necessary information always beforehand, always ready-made (and neatly catalogued). Obviously this is impossible. But ignorance – the intentional disregard of existing information – does not belong to science. When that happens it is politics, religion, or whatever, but it is not science. Therefore, even if my selection could be accused of not being representational, such examples shouldn't have been published in the first place – or if yes, then at least not as scientific texts.

I will begin the examples, perhaps unexpectedly, with a methodological work which, although attempting to cover the whole spectrum, concentrates mostly on quantitative research rather than qualitative or, equally both. The intention is not to write a repetition of the contents – for instance, to explain what is a median, etc. – rather, the aim is to point out what can be considered as crucial mistakes. These can be, for example, non-critical attitude towards "methods" that clearly do not work or, say, a complete ignorance of the history and "schools" of philosophical thought. From the actual research point of view, the non-critical listing of the various dubious methods – as if they worked – is most unforgiving. Anyway, the first example, then, is that from Frey, Botan & Kreps (2000), *Investigating Communication: An Introduction to Research Methods*.

The quantitative part of this work is "ok" in the sense that it offers a nice introduction to the basic concepts – for students – but in no way can it be considered a handbook on how to really set-up and carry through a research project. However, things begin to go haywire when they stray from this purely quantitative element and attempt to elucidate methodology from a wider perspective. This wider perspective begins by pointing to a methodological dualism where, on the one hand, there is positivism and, on the other hand, as an opposition to it, naturalism; or as the authors write "positivistic and naturalistic paradigms" (ibid.: 18). Yes, that's right, not positivism and hermeneutics or quantitative and qualitative, but positivism and naturalism. The authors (ibid.) continue about the "naturalistic paradigm" that it

> can be defined as the family of philosophies that focus on the socially constructed nature of reality. The naturalistic paradigm, again as applied to the social sciences, is essentially concerned with the development of methods that capture the socially constructed and situated nature of human behaviour. Perhaps the best way to thing about the difference between these paradigms is that while the positivist paradigm stresses the word science in the term "social science", the naturalistic paradigm stresses the word social.

What the authors obviously mean by "naturalistic paradigm" is hermeneutics or qualitative approach to research – at least that is what one usually finds in the methodology literature. There is, of course, such a thing as "naturalistic observation" which refers to observing the object in its natural environment but this doesn't mean or lead to a change of methodological paradigm. Naturalistic observation is fully compatible with positivism. However, it ceases to be so if it is claimed that only "naturalistic observation" is allowable. Furthermore, naturalism as such, i.e., as not only observation, can be considered being a part of positivism, or corresponding to it over the important parts. There are differences, of course, among the naturalists and, hence, between positivism and naturalism, but this is because none of the philosophical schools are monolithic structures; there is a certain shared hard core and a shifting cloud of fog surrounding it.

The book plunges into methodological mess in chapter 9 where it deals about “textual analysis”. According to the authors (ibid.: 225, 227),

> textual analysis is the method communication researchers use to describe and interpret the characteristics of a recorded or visual message [which can be anything, for instance, text, picture, sound, etc.]. Describing the communication embedded in a text is not as easy as it might seem because there isn’t a single meaning of a text, nor is there a single perspective from which to interpret it. Communication scholars also often function as qualified interpreters of texts. They are trained in the methodologies discussed in this chapter, which means they study texts using rigorous and systematic procedures. (brackets added)

From a methodological perspective, not much is offered. Then again, there really isn’t much to be given anyway, other than the many variations on the same semiotic idea, i.e., the thousand and one different ways to search for meaning which, at the end of the day, boil down to the same one thing: the researchers subjective feelings about what might lie “hidden” beyond the immediately visible while not forgetting to remind the reader how that visible is positivistic and, hence, it should be disregarded. Not only is the listing of researchers feelings void of any methodology, if falls prey to the dangers Corner was talking about (the Barthes example). However, the interesting part is the contradiction contained in the above quote. So, on the one hand they say that “there isn’t a single meaning of a text” or an interpretative perspective but, on the other hand, somehow communication scholars are competent interpreters. Not only are they competent, they use rigorous and systematic procedures. But semiotic analyses are as close to being rigorous and systematic as fire is to water. What can be rigorous, is content analysis if understood as a simple counting exercise. Of course, content analysis as simply counting something is as methodological as counting pebbles or grains of sand, i.e., not at all. But this seems to be exactly what the authors have in mind, though even this ends up being contradictory.

They divide content analysis into two categories: quantitative and qualitative where the former is to be understood as a mechanistic procedure. According to the authors (ibid.: 237)

> most content analyses are quantitative in nature, which involves counting the particular instances of certain types of messages in texts. There are, however, qualitative content analyses, where

> researchers are most interested in the meanings associated with messages than with the number of times message variables occur.

Although I can only speak for myself but I have not yet seen content analysis as a method, quantitative or qualitative, where analysis (interpretation) was not somehow present. Deciding what to count necessarily involves interpretation. No one in his or her right mind would be interested only in how many times something occurs. Frequency is important when it is related to the leading idea which, by the way, triggers the whole activity of "counting". So, yes, (quantitative) content analysis does involve counting but it also involves reasoning, interpreting, and so on. Quantitative content analysis is actually an unnecessarily confusing construct, for it is nothing else than applying statistics to media messages. The way it is being written about in a large body of methodology literature gives an impression as if it was something else, an autonomous method. (For instance, in Hansen (1998) we can find the same confusion. Perhaps even more, for he writes of it firstly (ibid.: 91) that "content analysis is [a] method for the systematic analysis of communications content" but, secondly (ibid.: 98) that

> much of the criticism which has been directed at content analysis touches on problems more to do with the potential and actual (mis)-uses and abuses of the method, than to do with any inherent weaknesses of this method as a method of data-collection.

This gives an impression as if content analysis can be considered a data-collection method. However, there is, again, nothing particular in the way data is collected from media messages and, thus, no unique "data-collection method" is warranted. Every data-collection is informed (guided) by some idea – theory, hypothesis, etc. – and as long as we want to have a representative sample – even one case if we are interested in that one case – certain rules have to be followed. (For example, if bias is to be reduced and so on.) These rules are of a general nature which means that they are used always when data is gathered. A new method is not created whenever the object of data-collection changes as long as the process remains the same. Content analysis hasn't been shown to be predominantly different in this sense.)

But the principal error is not that some used terms and concepts are unclear or even false, or that the statements are contradictory; the most serious offence is that a book that is intended to raise a new cohort of researchers does not really evaluate what works and what doesn't, what is justified and what is totally unsupported. (For instance, in contemporary medical books one tends not to find prescriptions on how to tap a hole into a skull as a method to cure migraine for a very good reason: it doesn't work.) It is one thing to write about what happens or exist in a world, but methodological debate should go beyond that, if we want to have science that is.

As has been mentioned several times, in the methodology literature – no matter whether we mean that of human sciences as a whole or specifically communication research – positivism has been mostly considered as the designated negative comparison point; a methodological and philosophical punch bag without much consideration if those punches and kicks are justified. One of the most popular accusation directed at positivism is that it is "shallow". Not only is this the result of misunderstanding positivism but also basic scientific principles.

Tervonen & Hemánus' (1980) *Objektiivinen joukkotiedotus* (Objective mass communication) is one such example – though this is a more philosophical than strictly methodological work. The basic structure of the book can be divided into four parts, either as units or underlying ideas: a philosophical contemplation of objectivity, whether objectivity could be reached, at least in principle, that objectivity ought to be a goal of the journalistic process, and what obstructions to and/or misconceptions about objectivity there have been. In principle, the underlying intention sounds good. The fact that there is an attempt at creating a sort of philosophical foundation makes this book quite distinctive, and in a very positive sense. However, the philosophical part falls somewhat flat, unable to produce that bedrock on which a further argument could be built; it remains disconnected and, quite frankly, pointless. The good part of it is that the writer (Tervonen) considers objectivity as correspondence between the message and reality, though not as identicality between the two (ibid.: 19). Although this connection (correspondence) is problematised further, without really producing a coherent solution, the basic idea of correspondence, no matter how affected by subjectivity, etc., remains as the decisive quality of objectivity.

As to why, say, journalism ought to be objective, we get more or less the same justification as given by Lippmann. Hemánus (ibid.: 135–136) writes that

> mass-communication should aim at objectivity because if it succeeds in this – as much as is possible – mass-communication has the potential to fulfil its noblest duty: to further maximally realistic, in an ideal situation, scientific worldview. Only based on the maximally scientific worldview can a man learn the laws of reality, use them where possible and otherwise adapt to them, and thus be better equipped to build the kind of society that can best satisfy his needs.

But it is the misconceptions – supposedly positivistic for that matter – that are the most interesting here. They are interesting because they rest on nothing but air; a serious methodological/philosophical error that has become all too frequent in the human sciences. According to Hemánus (ibid.: 89–90) positivistic supposition of objectivity is wrong because of two main reasons:

> firstly, the underlying assumption of reality is shallow as is the case for positivism and, secondly, such suppositions [of objectivity] transform the concept of objectivity into a relatively easily operationisable and thus observable, even measurable phenomenon so that the concept of objectivity is distorted. These reasons are intertwined; because positivistic view of reality is positivistic it follows that observational methods and measures are developed which can be validly applied only to the surface of reality while they remain inadequate, even misleading if applied to the deeper, scientific examination of reality.

Yes, according to the manifesto of the Vienna Circle (The Vienna Circle: 306), there are only surfaces in science, and not some metaphysically burdened nonsense about depth. It seems that Hemánus is a believer in the Kantian "things in themselves" and "things as they appear for us" where the former is unknowable for us, only the latter. But firstly, how can we know that we cannot know things in themselves and, secondly, even if this was somehow true, why bother with it in the first place? "Things in themselves" is an unnecessary metaphysical burden that the positivists wanted to get rid of – it doesn't add anything to a scientific process. These "unfathomable" depths are comparable to concepts like the "spirit of history"; things that are unobservable in principle and, thus, there is no place for them in science. One would imagine that those who criticise positivism for being interested only in surfaces would have suggested alternative methodology that allows us to go “deeper”. Unsurprisingly, no such methodology has been so far produced and neither does Hemánus – however, he does go on, with an admirable insistence, for several pages about how shallow positivism is. We do not have to repeat all the points that he lists, as a critique of positivistic assumptions, a couple will suffice. He writes (ibid.: 95) that positivism – in journalism – sees objectivity as fairness, i.e., that “both parties” can voice their “side of the story”. But this has nothing to do with positivism. It is not some political system that tries to establish a universal democratic and fair world order where everyone will have an equal say in the daily matters. Additionally, he says (ibid.: 102) that positivism sees objectivity as a presentation which doesn’t affect the receivers. Hemánus (ibid.) explains this by saying that positivism sees that only value-judgements can affect people while stating or relaying facts will not. Again, it would have been nice if Hemánus gave an example of a positivism that makes such claims. No such examples (not even one) were given. It is interesting that on the one hand positivism is seen as being simply wrong but on the other hand Hemánus & Tervonen sees objectivity as a correspondence between the statement (message) and reality, which happens to be one of the core features of positivism – even though there were, among positivists, leanings towards coherentism as well.

Even the already mentioned Nordenstreng had his quarrels with positivism, against which Marxism was posited as the obviously correct position – then again, that book was written in the 70s, a time when Marxism-Leninism was more fashionable in the human sciences, in Finland. For Nordenstreng the big battle between philosophical approaches centred on the question which has more merit, materialism or idealism. Naturally, in the Marxist-Leninist spirit, Nordenstreng chose materialism as the basis for communications research. The choice of materialism is not what should be criticised, it is his categorisation of positivism as idealism and his stated arguments (or their lack of). (This was, actually, the first time I heard or read anyone speak or write of positivism as a form of idealism.) According to Nordenstreng (1975: 46)

> a positivist does not cease to be, in the end, an idealist when he remembers, on the side of his linguistic-logical analyses, also to notice objective reality and societal practice. Positivism's relation to philosophical traditions doesn't become clear based only on to what extent thinking operates with material and to what extent with phenomena of the mind – from this point, many a positivist (behaviourist), using quantifying methods, would turn out to be "materialist". But the real touchstone of positivism is its position on values: are values to be based on reality or is their origin somewhere else – such as in the human consciousness.

Now, it can be that my translation to English is not necessarily the best one; I can only say that Nordenstreng's original in Finnish makes as little sense as my translation of it. The argument is nonsense up to the point where the issue of values is raised, and that also turns out to be rather weak attempt. Considering what positivism was about, it doesn't make sense to even ask whether positivism sees values as based on reality – if the origin was supposedly somewhere else, a positivist would dismiss that as a metaphysical and mostly meaningless statement that is impossible to verify even in principle. To speak of non-materialistic phenomena simply does not fit into the positivistic philosophy. The mistake in Nordenstreng's thinking lies in the fact he sees human consciousness as not being based on reality. But if it is not based on reality, i.e., if it isn't material or at least have material consequences, then in what sphere of existence or non-existence it is to be found? Is it, perhaps, also somewhere in the Kantian sphere of "das Ding an sich"? If that was the case, how is Nordenstreng qualified to talk about objects of that level? Of course, for a positivist it doesn't make that much sense to speak of the mind's origins. A positivist simply holds that "everything we can sensibly talk about is spatially and temporally ordered. What appears in statements as 'mental', 'personality', 'soul', must be expressible as something spatio-temporal or else vanish from science" (Neurath 1973: 325). According to Schlick to talk about whether or not something is real is pointless as that cannot be established through philosophical analysis. The only thing we can do, according to him, is to clarify what we mean by saying that something is real; and that, whether this is so or not, can be settled only through everyday life and the ordinary methods of science, i.e., through experience (Schlick 1997: 74). It must be said again: it is easy to construct straw-man arguments about positivism (or traditional science) – which demands that no actual examples of the evil philosophy can be offered in favour of the argument – and then present one’s own insights as the clever solutions to a problem that never existed in the first place.

Moving on to the more “linguistic” approaches. Bell & Garrett’s (1998: 2) introduction to critical discourse analysis points out that in media studies there

> has been [a] change in perspectives on where the meanings of texts reside. ‘... Text-as-meaning is produced at the moment of reading, not at the moment of writing ... [this] takes away from that text the status of being the originator of that meaning’ (Fiske, 1987: 305;...) Since meanings are now seen to be more a product of negotiation between readers and texts, text takes on more of the interactive qualities of discourse. (brackets added)

The absurdity of the “linguistic shift” in human sciences is nothing short of bombastic; that is, if the principle of meaning as a sole result of reading is proposed seriously. Such a position could be defended only if the Saussurean separation between langue and reality was not utterly broken. As Giddens (1979: 16-17) writes: “The dam that Saussure established to protect the system of langue from semantic and referential ties to the world of objects and events is continually and necessarily breached”. Although the relation between, say, a word and its material counterpart is based on convention, once that convention becomes adequately established – and we do happen to live in a world where the majority of words or expressions are established – to treat those “signs” arbitrarily means that either a person deliberately tries to breach the linguistic system or that person is, in fact, someone who does not know the system in the first place. Texts, then, are written with a certain intended meaning. If not, why write meaningless texts? And as long as the receiver of that text masters the same linguistic system on which it is based, then the meaning cannot be arbitrary – at least without breaking all semantic and syntactic rules. This doesn’t mean that words have absolutely precise meaning; especially those of everyday language. (One reason scientific terms and vocabulary exists and is developed is to decrease the level of ambiguity as much as possible.) However, even if the boundaries (of the words) are somewhat foggy, there is a solid core that allows and directs communication (meaning) in a certain way rather than some other. Of course, the best evidence for the non-arbitrariness of language (even if based on convention) is the fact that at least most of the time people seem to understand each other. In an arbitrary (random) environment understanding would be impossible. To put it differently, we may not be able to perfectly define a policeman – to the last atom – but we certainly point and say “this is a policeman” when we see one.

According to Bell & Garrett (ibid.: 6) critical discourse analysis “has produced the majority of research into media discourse during the 1980s and 1990s, and has arguably become the standard framework for studying media texts within European linguistics and discourse studies.” Of course, what is meant by discourse analysis is nothing else than the semiotic analysis of various “texts”. Every now and then there comes a scholar who sees it as his or her duty to “invent” a new approach which, however, after some scrutiny, turns out to be the same thing in new clothes. Some use the word “story” or “narrative” instead of text, or they speak of narrative or conversation analyses, but in the end it is the same thing. Structural and/or formal analyses can potentially be different; if aimed at describing structure/form, they are different, otherwise they fall to the already mentioned category. The other problematic of the multitudes of semiotic analyses has also been dealt with. Bell & Garrett (ibid.) write that

> [critical discourse analysis] has an explicit sociopolitical agenda, a concern to discover and bear witness to unequal relations of power which underlie ways of talking in a society, and in particular to reveal the role of discourse in reproducing or challenging sociopolitical dominance. [critical discourse analysis] also offers the potential for applying theoretically sophisticated frameworks to important issues, so is a natural tool for those who wish to make their research socially activist. (brackets added)

It is the political nature of the “analysis” that raises concern. A researcher can certainly be an activist but when that activism overrides good scientific conduct then nothing else but propaganda remains. And in a case when a flawed methodology, or unfitting for the task, is being used, the results are everything but scientific. Then again, even the “politics” side becomes suspect, as there is a tendency for it to be supported with fairytales rather than facts.

One of the variations on critical discourse analysis is Bell’s (1998) discourse structure analysis. As the name implies that aim of this approach is, well, to analyse structure of the text (although Bell insists a text in this case is a story). By structure is meant such things as who, what, where, etc., their order, the source, time of event, and so on. It is really a deconstruction of the "story" into its constitutive elements. So far this would fall under the structural/formal category of analyses. Such an approach is relatively problem free, I mean if formal description, comparison, etc., is aimed at. And one would certainly expect that, especially when Bell (ibid.: 65) writes: "My approach to news discourse focuses on the question 'what does this story actually say happened?' It is not a question – at least initially – of whether these reports represent what 'really' happened." But if we are not interested in reality, it becomes trivial to say what the story claims to have happened. Whether the piece starts with the who part or ends with it makes no difference. Such formal analysis and comparison can reveal, say, stylistic differences between eras, newspapers, or journalists – which can be interesting or even valuable – but not much else. However, this approach doesn't lend itself to answer questions of much wider effect. For example, according to Bell (ibid.: 64–65)

> media 'discourse' is important both for what it reveals about a society and because it also itself contributes to the character of society. Linguistic research on the media has always emphasized this last concern, focusing where issues of ideology and power are closest to the surface. But prerequisite to all such questions is a sound discourse analysis...

Sounds nice, but revelations about society are an empirical matter which belongs to the, as Bell puts it, "what really happened" category. Society cannot be reduced to news discourse, nor to the real facts newsmen supposedly report; it is much more. Unfortunately, instead of giving an explanation how it is possible – validly that is – to move seamlessly between the limited domain of news structure (while disregarding the actual facts) and the practically infinite sphere of society, Bell decided it is better to drown us in a sea of trivialities, for instance, that "only after we are clear what the story says will we be in a position to see what it does not say" (ibid.: 66). It is not that difficult to say what this or that story doesn't say. In fact, those things that are not being said come in an endless stream. For example, news article about Gaddafi is not about a race-horse called Fred – one doesn't need to be a media scholar to do that, nor is it necessary to use discourse structure analysis to reveal such deep insights.

Surely there must be better examples of discourse analysis (critical or not) – as a concrete and scientific methodology that is. According to Schrøder (2002), one of the chief contributors to critical discourse analysis is Norman Fairclough. Schrøder continues that it (critical discourse analysis) "represents a significant theoretical as well as methodological contribution to the ... study of media discourse." (ibid.: 106.) Considering how promising all this sounds, lets see, then, what Fairclough has to say. In one article Fairclough (1998: 142) describes a framework for studying political discourse; a variation on critical discourse analysis. (I presume that the variation will be minimal.) He continues that in his approach "political discourse is seen as an 'order of discourse' ... which is continuously changing within wider processes of social and cultural change affecting the media themselves and other social domains which are linked to them" (ibid.). We begin to see what Fairclough has in mind based on the intended wider scope of analysis (ibid.: 143):

> While I think that 'internal analysis' in the sense of close textual analysis is essential if we are really to develop an understanding of political discourse, Bourdieu is right to insist that internal analysis of political discourses or texts which does not place them with respect to the political field and its wider social frame is of limited value. I propose to partially meet this criticism by arguing that analysis of media political discourse ... should aim to simultaneously illuminate particular communicative events, and the constitution and transformation of the political order of discourse. By the political order of discourse, I mean the structured configuration of genres and discourses which constitutes political discourse, the system – albeit an open and shifting one – which defines and delimits political discourse, at a given point in time. ...discourse analysis also needs to be properly integrated with other forms of social analysis.

The good thing is that a necessity to integrate discourse analysis with other types of analyses is recognised. However, it does not completely solve the problem if this wider analysis disregards research done based on the traditional science paradigm. What I mean is, insistence on a semiotic analysis only, is an unnecessarily restrictive approach, one that cannot solve the inherent limits of the semiotic "method". From the above quote it is actually impossible to say what Fairclough concretely means, for instance, by genres and discourses which constitute political discourse ... which defines and delimits political discourse, etc. Though I take it that it is, in fact, the semiotic approach to which Fairclough's words refer. Unfortunately, not much is offered what can be considered methodology proper. What started as a sensible idea – combining discourse analysis with other forms of inquiries – quickly turns into a collection of trivialities, clad into the "correct" jargon. And so, Fairclough (ibid.: 145) writes that

> my focus here and more generally is on intertextuality: on how in the production and interpretation ... of a text people draw upon other texts and text types which are culturally available to them. The claim is that texts have a dual orientation to 'systems' in a broad sense: there are language systems, and there are orders of discourse. The text-system relationship in both cases is dialectical: texts draw upon but also constitute (and reconstitute) systems. An order of discourse is a structured configuration of genres and discourses (and maybe other elements, such as voices, registers, styles) associated with a given social domain - for example, the order of discourse of a school. In describing such an order of discourse, one identifies its constituent discursive practices (e.g. various sorts of classroom talk and writing, playground talk, staffroom talk, centrally produced documentation, etc.), and crucially the relationships and boundaries between them. The concern, however, is not just with the internal economy of various separate orders of discourse. It is with relationships of tension and flow across as well as within various local orders of discourse in an (open) system that we might call the 'societal order of discourse'.

This is a perfect example of what is wrong with the "linguistic shift"; it is nonsensical, despite the sprinkled crumbs of trivialities. First of all, by invoking the sacred powers of dialectics nothing at all is explained. The fact that things affect each other is perfectly known in science: ultimately everything affects everything in the universe. But there is no actual research that can be based on such a truism. One has to be much more concrete. It is the what, where, and especially how that are decisive, not some uninformative notions that, say, objects are dimensional. Second, the given example of "an order of discourse as a structured configuration" of a school simply means "how people talk in a school". Even laymen understand this latter aim while the previous needs a "professional" to decipher it. Again, it is obscurantism at work. Thirdly, it remains unanswered what is meant by "internal economy of various separate orders of discourse". The same can be said even about the "relationships of tension and flow across and within local orders of discourse..." If it simply means the various interactions individuals and groups engage among each other, then it is a triviality, otherwise we are left in the dark as to what it could mean.

When one reads Fairclough's analytical framework, it is difficult to shake off the nagging feeling that it is an example of what Niemelä & Tammisalo coined as "word magic" – or obscurantism if one wishes, "word magic" just sounds better. For when Fairclough (ibid.: 146–147) proceeds with, say, concepts like the sphere of the political, orders of discourse of the political system, and so on, we eventually find out that: 1) the political is not really delimited in any way; 2) but the political cannot even be delimited for it is, in the end, supposed to be the concatenation of all societal spheres, i.e., economy, politics, media, etc. Although the words like “economy” give an impression of something with boundaries – and on a conceptual level we can think so – in reality, such is not the case as it permeates all aspects of society. It is equally so with politics or any other just as wide a concept. This leaves us with two options: either a) one tries to do “holistic” research of the “totality” of the societal – which is really impossible and one usually ends up selecting, uninformatively and randomly, bits and pieces here or there – or b) an additional layer(s) of delimitation is added and, as in traditional science, the fact that everything affects everything is broken down to more constitutive elements. However, in Fairclough’s framework such steps and their justification is not given: it remains ambiguous and impressionistic. According to him (ibid.: 150–151) “discourse analysis cannot simply focus upon the texts and talk of mediatized politics; it needs also to analyse the practices of political discourse both on the side of production, and on the side of reception/consumption.” Although this may sound as some concrete steps the researcher should follow, such is not the case.

Ultimately, in Fairclough’s model, everything is discourse; what in older sociology was referred as simply “societal” is now transformed into “societal discourse”. But if by discourse is meant only that which is textual (written, spoken), then an act of reduction is performed which surpasses even the supposed evil “positivist reductionism”. On the other hand, if no such reductionism is aimed at – and this remains somewhat unclear – then Fairclough’s model has added absolutely nothing to what already is. If by discourse we simply end up adding a certain word after every other word (concept, or object), then it can be easily removed as it obviously has no concrete meaning. We can, for instance, say that everything is “boinky”, but if it doesn’t contain any specificity, or if our actions towards each other and the reality remain unchanged even after disregarding all “boinkiness”, then it is nothing but a gimmick. It could be interesting in poetry, political party documents, etc., but not in science. Fairclough (ibid.: 161) concludes that “the value of this approach is that it avoids particular discursive events and texts being treated in isolation from the orders of discourse and the wider social fields and processes they are embedded within.” While this is true it, unfortunately, ends up at the other opposite so that one doesn’t really know what to study, how, or even why. But this is not all as, according to Schrøder (2002: 108), “Fairclough’s rejection of empirically studying audiences and other social agents of discourse tends to limit the applicability and explanatory value of the approach”. It is, then, the repetition of Barthes’ case, i.e., the subjective constructions of the analyst.

Now, more examples could be given – about the different variants of discourse analysis, critical or not – but not much new could be offered. It all revolves around the question, how such methodology positions itself in relation to traditional science. If there is conformity between the two, then there is very little reason to treat discourse analysis differently, from traditional science that is; it simply would be an application of already existing scientific procedures which are the same in principle even though the situation of usage might vary. (Quantitative) content analysis is a case in point: it is not a unique method (or methodology), rather, it is an application of already existing methods of traditional science. On the other hand, if discourse analysis – and the whole semiotic approach for that matter – is intended as an alternative, or even an opposite, to traditional science, then even in this case to speak of methodology would not be fitting. Whether the “alternativists” like it or not, traditional science does have a pretty good idea of what ought to be done in science, what works and what doesn’t. The fact that the alternative approaches have produced “research” is meaningless when or if that research is based on ambiguity, triviality, and outright impossibility. If, say, medical research and medical researchers adopted similar approach, we would spare no words in their condemnation. Why should other ranks of the academia/science get a special treatment? So far, from the alternativists camp, we have got promises, substitution of fact-based research with ideological sentimentality that is comparable to religious dogmas, and studies that, when analysed based on merit rather than political fit – turn out to be more or less unsupported fluff.

4 WHAT THE STUDENTS HAVE LEARNED

In this chapter we will take a brief overview of what the students do; where on the traditional science vs. hermeneutic-postmodernist-feminist-semiotic spectrum their work can be located and what is found as acceptable work. Also, a word or two will be given on evaluation of the situation, its consequences, and what ought to be done – if anything at all.

4.1 What material is used, how it was selected, and hypothesis

The used material (appendix 1, selected cases are marked with bold font) consists of accepted media studies master's theses written at the university of Vaasa over the period 2001-2009. Although the sample was selected roughly one year ago, the possible addition to that list will have only negligible effect to the overall results because the number of finished (and accepted) theses is rather low. Now, the reason why we choose samples is to eliminate the need to study the whole population. The central idea of a sample is representativeness: if it can be shown that the sample represents dependently the whole population, the need to study the latter is eliminated. (Sometimes it may be a question of resources, i.e., that due to financial reasons a sample over the whole population must be chosen whether we want or not.) A perfect representativeness is, perhaps, never reached

> as two types of error are regularly encountered: those arising from biases in selection, etc., though avoidable, frequently occur, while those due to chance differences between the members of the population included and those not included are virtually certain to be present. The former is termed the *error due to bias*, and the latter the *sampling error*... (Madge 1953: 207.)

A remedy – as close to one as possible – to this is the randomisation of sampling. But, as Wright (1979: 36) writes, "random does not mean capricious or haphazard; *random assignment* or *randomization* means that each subject or unit has an equal or known chance ... of being allocated ... and that the allocation of each unit is done independently of the others."

This brings us to the size of the sample. As to the question "How much is enough?", the answer depends on what is the purpose of the study. A research can be either extremely empirical, in theory or hypothesis testing sense, extremely theoretical (as is the case, for instance, with many themes of theoretical physics), or something in between. Töttö writes (2005: 52) that if we are interested in the first option, the empirical material should be as robust and representative as possible; theoretical research, as the second option, might not require any samples (here Töttö mentions Einstein's theory of relativity), and the in between will be, well, somewhere in between. The question of size of the sample is similarly explained by Chalmers (1985: 16), that

> For example, based on the dropping of the atomic bomb on Hiroshima the understanding that atomic bombs cause widespread death and destruction needed only that one observation. [On the other hand] we would not ascribe supernatural powers to a fortune-teller on the basis of one correct prediction. (brackets added)

For the purposes of this thesis, there are two aims: to describe the population and carry out a rudimentary hypothesis test. With these aims in mind, more "robust" material is to be preferred. But the initial question, of size, is still not answered. And, I am afraid, not much of a concrete answer will be given. For instance, Madge (1953: 213) writes that

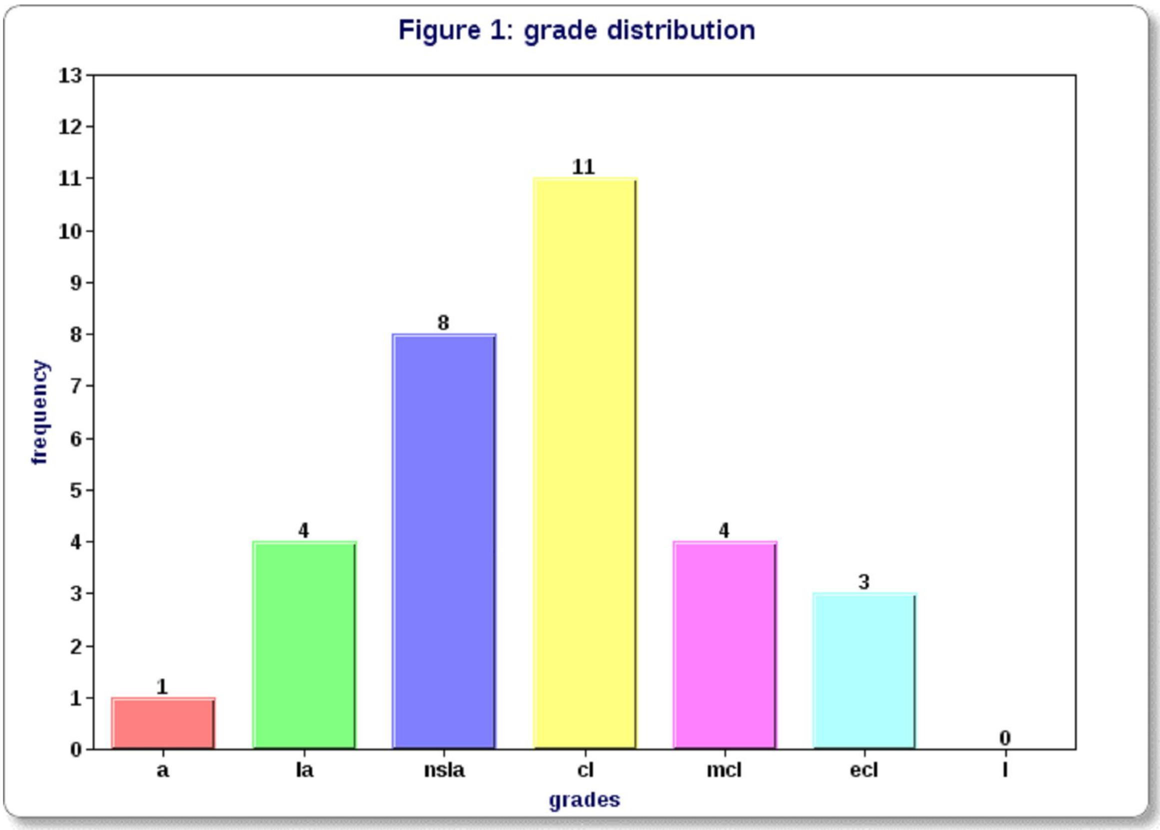
> there are various common misconceptions about the necessary size of sample. One is that the sample should be a regular proportion (often put at 5 %) of the 'population', and another is that the sample should total about 2,000. No such rule-of-thumb method is adequate. The size of the sample is properly fixed by deciding what level of accuracy is required, and hence how large a *sampling error* is acceptable.

And, according to Wright (1979: 30)

> [...that in addition to the limiting factor of available funding, or other resources] sample size depends on the nature of the analysis to be performed, the desired precision of the estimates one wishes to achieve, the kind and number of comparisons that will be made, the number of variables that have to be examined simultaneously, and how heterogenous a universe is sampled. (brackets added)

But there is one more thing that should be realised: the size of the target population. The problem here, then, is that the smaller the population, the more difficult it is to calculate an estimate. This can be illustrated with an example of a population of two units; we can choose randomly at minimum and maximum 1 unit; lets say that the selected unit is a white table-tennis ball (these come in either white or orange versions); how confident can we be that even the next ball will be white? Considering that we have a case of one or the other, the probability will be 50 % which is completely random chance. No mathematics can change the fact that based on one unit of two, the unknown unit's quality may or may not be this or that at the 50 % level of probability. So, the smaller the population, the more will a statistical inference resemble a pure guess. This can be remedied by increasing the sample size but, again, in small populations this could mean including more and more samples until almost or indeed every unit gets sampled.

In the end 7 cases ended up being selected. Originally the intention was to select 12 theses but due to availability limitations at the library and "empirical saturation", the sample was limited to 7. By empirical saturation I mean that the sample showed remarkable similarity of those aspects that are the objects of interest here, i.e., additional material was very unlikely to produce something "surprising". The distribution of the theses' grades is shown in the figure 1 (the grades are ordered from lowest to highest: a = lowest and l = highest. The frequency refers to the total amount of instances of a particular grade in the population.)



The graph in figure 1 is not that far away from a normal distribution which means, for instance, that the measures of central tendency (mean, median, and mode) are close to each other – which in this case are represented by the grade cl (in a normal distribution these measures would be exactly the same). Such a distribution is supposed to simply say that most students have written a thesis that was rewarded an average grade (cl in this case) and also that it is reasonable to assume that most theses in the future will fall into this category. It is expected that the further we get from the average, the smaller the frequency of such instances. Such common expectation certainly is supported at least in the case of the media studies theses. Lastly, a remark about the missing l-grade(s). There are several potential explanations why no-one has been granted the best grade: 1) common sense tells us that this is so simply because there were no theses worthy of that grade; 2) however, it is possible, in a more general sense, that the requirements are set too high – that no "mortal" student can achieve it; 3) (a variation on the previous point) some teachers, professors, supervisors, etc., don't give the highest grade due to a mistaken principle according to which they might think that a student should not receive the highest grade simply because a student is a student.

These last three points differ from field to field, university to university, and supervisor to supervisor. And when one adds to this the fact that the formal thesis requirements – and how they are graded – are thoroughly subjective, it becomes difficult to pinpoint exactly why such and such grade has not been awarded, or why, exactly, the grade distribution is skewed one way or the other. There is no metric for how to evaluate, say, the overall level of l-grade thesis if or when the requirement goes something like this: "Overall, the work is distinctive; it shows the writer's own judgment, the results are interesting and novel." Intuitively this makes sense, however, the evaluation procedure is ambiguous at best. For instance, what does novel in this case mean? Is it novelty from the viewpoint of what students generally write, for the supervisor, or, perhaps, a completely new discovery in the whole field? The same can be asked of everything and the answer will be equally vague. In this sense, I think, it would be better to mostly evaluate the "scientific attitude" of a thesis. Although still this would remain vague at the positive end of the scale but, at the lower end, it would not be that difficult to ascertain whether these and those requirements have been met. For example, it is not that difficult to find out whether a research aim that requires a causal analysis actually does contain one. This analysis could be better or worse but at least it shows that the student grasps the basic logic of science. Unfortunately, these days, there are strong stylistic requirements of the faculty and individual supervisor which seem to take higher priority than actual content – with the faculty requirements this is certainly the case.

Furthermore, it is, I think, safe to assume that the grades have not been issued randomly; that the actual content correlates with the grades so that the better the grade, the better the content (or the other way around). Considering that the content of a thesis is not standardisable in the same sense as, say, some hardware product, the evaluation by the thesis supervisor will contain an element of subjectivity which may have a considerable effect the closer to or further away from the supervisors "pet theory", whims, etc., the to be evaluated thesis happens to be. At minimum, however, one expects that even the lowest grades will show at least a basic scientific competency; that the aim and intended evidential support match in principle, that the methodological choices are basically correct, logically coherent, and so on. There shouldn't be a categorical difference in basic scientific competency between the best and worst, only that the better works should show more skill, higher comprehensiveness, attention to detail, and, well, higher quantity of all the things that are considered as evidence of good science. Why, for instance, stylistic issues shouldn't matter much? Because, after all, these works are created and evaluated at a university and not at a summer camp for writing poetry. Again, the evaluation of a final test at a vocational training centre would most likely concentrate on other things than "matters of scientific importance". A university can be, of course, changed into anything at all, for example, a cooking school, but since officially a university is still supposed to offer scientific education and conduct scientific research, I take it that the deciding factor in thesis evaluation ought to be the scientific competency, or the lack of it.

Keeping this in mind we can formulate our hypothesis: if even the lower graded theses should show at least a basic scientific competency then the higher graded theses should be even better in this regard. By conducting the sample selection in such a way as to make sure that a high proportion of the better graded theses will be included, we can get a better picture what they are like; how good they are. This has the advantage that even if the lower graded theses were, how to put it, absolutely terrible, we would still have the positive comparison point which would show not only the good that is produced but also, that there is a correlation between the content and grades, at least at the higher level. In short, if there is a correlation between a grade and thesis' content, then such a link should be present especially at the higher grade level. An automatic side-effect of this will be that we will obtain a description of the theses' methodology – the scientific principles – and, thus, a comparison can be performed between the methodology literature (general or specifically media studies). If strong correspondence between the two is found then this will indicate that not only students are being thought non-science but also, that the non-scientific argumentation is accepted and, perhaps, even rewarded. In such a case, a thesis and its evaluation will resemble works of general literature and less science.

The selection, then, proceeded in a two-stage manner. First the population was divided into two groups: that with above average grade (7 total) and that with average or below average grading (24 total). As a next step, a random selection was performed from both groups even though the total amount of instances of the higher grade group was kept intentionally disproportionately higher. But as was already mentioned, due to availability and other reasons, I ended up with a sample of 7 theses where 4 were graded above average and 3 below average – mcl, mcl, ecl, ecl, and la, la, nsla respectively.

4.2 The method(ology)

As I have been trying to say, not many methods are really "methodological" – most of them, at least in the qualitative/hermeneutic tradition, are nothing but vague constructs void of content or they are simple truisms. Then again, even in the "hard" sciences what is considered as a method concerns mostly statistical analysis. In experimental research, for instance, to devise and arrange an experiment is a creative process, one that is even more important than any possible "methodology". Of course, serious research tries to be as much as possible valid, factual, true, contain causal explanation, and so on, but much if not most of the procedures leading to the basic principles will vary, even considerably – that is, in a research that studies something new. Much of the "methodology" is actually an explanation of what has been done in this or that particular research rather than mechanical repetition of an "off-the-shelf, one-size-fits-all" method. The used method of this thesis can be described as a combination of comparative analysis, "layman's descriptive statistics" both in scope and presentation, use of empirical material, descriptivism, and the use of theory and other available knowledge to analyse and/or explain whatever there is to be explained or analysed. The same holds true for causal analysis: it is one thing to show a statistical strong correlation, another is to inject this relation with such knowledge – any kind that can be considered as knowledge – as to firstly, elevate the status of correlation into causality and, secondly, offer a plausible mechanism (or explanation) why things are as they are. In other words, the "method" is the kind of common sense empiricism which, in principle, underlies all research.

Whatever choices made may be rudimentary and simple compared with the professional researcher's approach, but the underlying approach is the same.

Considering the "layman's" statistics part, fancy illustrations and what not will be kept to a minimum since, as Frey, Botan & Kreps (2000: 292) write, "when one has a relatively small data set ... one can verbally describe the entire data set by referring to each individual entry..." And the data set used in this thesis is certainly not overwhelming. No so-called qualitative methods will be used since, as by now I hope it has become clear, there are really no qualitative methods, i.e., actual and concrete procedures that should lead to certain type of results. (Thinking, writing, and observing are demanded in every research but they are as methodological as the existence of the researcher: necessary yes, an actual method no.) This means that no critical or uncritical discourse analysis, nor any grounded theory approach where a purified mind will throw itself at an unknown reality and witness the miraculous rise of theoretical skyscrapers (as if by magic) will be used.

But not even (quantitative) content analysis – as a statistically oriented method – has much of relevancy for this thesis. The frequency of any parts of the analysed content are beside the point: a thesis will either show a sufficient scientific competency or it will not, regardless of how many instances there are of this or that sentence or idea. If a student explains only once his conduct and that conduct turns out to be insufficient, confusing, or in any way untenable, then that one instance is enough. If, on the other hand, there are many but contradicting instances of methodological argumentation, the result will be no different no matter what the frequency of these arguments. Contradictory arguments are not the correct recipe for sound methodology. Although the reason behind a possible methodological contradiction may vary, from incompetence to a simple human error, for the reader the result is the same; the reader sees a contradiction and has very little means of knowing why such a mistake happened. Then again, it depends what kind of mistake was made: when the "methodology" is written in an overwhelmingly incomprehensive way, it is usually a sign that the writer didn't have much idea about the issue in the first place. And, lastly, if there is only one instance of "good methodology" (without any contradicting ones) or several (consistent) of them, it is to be understood as a sufficient command of the logic of science. (By "good methodology" I mean that the writer has grasped the basic scientific components, i.e., that her or she does principally the correct thing.)

Why discourse analysis – and their semiotic variants – has very little usage is because it tries to go "beyond" the literal meaning (or the given). It tries to "uncover" things that are impossible to establish – hence, going beyond literal meaning. Even such things as exposing hegemony, discrimination by the ruling class, etc., must be based on the "literal", text or other kind of events where literal simply means the empirical of which we can have knowledge; something which cannot be further "interpreted". Setting aside the fact that such uncovering the invisible cannot possibly work as a method, in this case it isn't even wanted; for we are very much interested in the literal, i.e., the concrete arguments that the students have written. It would be ludicrous to base the evaluation not on what has been written but on what might have been intended. First of all, how can we possibly know what might have been intended and, secondly, why wouldn't the student write according to his intentions? (Again, by intentions I mean those aspects of the content that pertain to the logic of science. It is possible that the topic, the empirical material, the questions, etc., might not be those about which the student really wanted to write but that is irrelevant for the purposes of this thesis and its methodological approach.) It is, then, assumed that the student writes according to his intentions in which case it makes much more sense to treat that text literally. If a text can be taken literally – and there is very little reason, at least in this case, to do otherwise – it can be approached "unmethodologically", by verbally describing what has been written and/or omitted.

And, thirdly, why different versions of grounded theory are mostly useless here is that – disregarding completely the idea that a sand castle is supposed the somehow materialise itself as long as we keep pouring enough of sand on one spot – we are, here, interested in "uncovering" what students actually do, i.e., we try to describe a certain part of reality. If it turns out that what the students do corresponds to media studies particular but also to methodological notions of human sciences in general, then we already have a likely theory to explain it, which is, in a simplified form, that to a large extent, the scholars of yesterday do train the scholars of today. Hence, the good as well as bad habits and practices are most likely going to be renewing themselves.

Although this explains most of the things, it still leaves open the crucial parts: why certain breaks happened in the history of science. The case of transition from old science to the new is only to be expected since the old science simply could not offer not only answers but a general approach – one that was not bound to the bible, scholasticism, etc., to finding out new things about the world we inhabit as well as ourselves. What is more difficult to explain – and here grounded theory offers zero help – is why, for instance, the so-called alternatives to positivism have been institutionalised. Considering that these “alternatives” are, partly, a repetition of hermeneutics but, surprisingly, also partly the exact philosophy that is criticised (positivism) – and sometimes neither of them by being a pure political program (feminism) – one has to wonder how they can, in a supposedly technologically and otherwise advanced contemporary society, not only to barely survive in the academia but to actually flourish and entrench themselves as a part of the establishment. Weber’s idea of the charismatic leader and his followers could explain the initial state but not necessarily the phase after the initial euphoria has disappeared. The alternative explanations are, perhaps, better left for the reader to contemplate as they are not that flattering of human nature and their explicit formulations might be considered as an outright hostility on the part of the writer. In a nutshell, then, this part of the thesis is an attempt at a methodologically common sense approach to science that rests on describing and explaining a certain part of the reality which, in this case, is the seven theses.

4.3 The theses and their analysis

In this chapter I will list each sampled thesis and give a short overview: positive if the thesis is principally correct (in its scientific approach) or negative if the analysed thesis shows serious shortcomings regarding the basic grasp of what is science. I am not interested in an overall critique of the thesis, i.e., how much literature was used, what kind, does the work show that the writer is familiar with the field, whether it is stylistically pretty, etc. The object of interest is simply whether or not the analysed theses can be considered scientific in the traditional sense of the word. Each thesis will be introduced by its title, written in italics. An English translation will be offered in parentheses, also written in italics. For the most part, the titles will give an adequate idea what the theses are about.

Ideally the assessment of any research (methodologically) is a comparison between the stated goals (as well as intended solutions) and what was actually done. Quite a lot can be said based on this kind of comparison, and while a consistency here may not necessarily guarantee good results, the lack of it certainly leads to failure. When methodological "clues" are scattered here and there, and if the evaluation of whole work is required, in order to grasp what has been done, it can be a sign that the writer was not sure himself and that, perhaps, the work was somewhat rushed. Now, it can be the case that the author of any research knows perfectly well what he is doing, but the reader does not. It is the writer's responsibility to state his aims and methods as clearly as possible. This is not a mere question of style, it is an absolute necessity.

Kuusitoista elonmerkkiä: Vuosien 1993–2004 latvialaisten näytelmäelokuvien kerronnan analyysi (*Sixteen Vital Signs of Survival: An Analysis of Latvian Cinema's Narrative, of the years 1993-2004*) The aim of the author is to analyse and describe the narratives of post-socialist Latvian films, i.e., what is similar, what is not, and so on. His empirical material is formed by 16 Latvian films which were produced between the years1993 and 2004. (p.3.) What is not clear, however, is the relation. He mentions, for example, that "societal perspective and contemporary viewpoints ... form a background context for the analysis of these films" (ibid.). In which sense is this context supposed to matter? If the analysis is supposed to describe the empirical part of the films, the visible, then this context is not necessary. If, on the other hand, the context is supposed to help in some kind of semiotic analysis, especially if the aim is to show why the films are what they are, then without a further empirical evidence the context can be useful but only to the one doing the analyses and not to the reader.

The aim seems to be the explanation of the films' nature since the author introduces the idea that the films are what they are due to the changes in post-socialist Latvian society and that the films are or could be a kind of a mirror of that society. However, this leads to a dual purpose not originally stated; that on the one hand, the interest is in analysing the films and, on the other, exploration of the Latvian society and linking it to the films. On page six, this duality of purposes is perpetuated when the author states that "the aim is to approach Latvian independence through the films' narratives and that, finally, the aim is to classify the particular qualities of those narratives". These same intentions are stated also in his conclusions on page 145. We may therefore find three aims: 1) the already mentioned analysis of the films, of their empirical content; 2) explaining how the societal changes etc. have affected the narratives of the films. This implies a causal connection; 3) how the films (or their narratives) mirror the surrounding society. This can mean three things: a) same causal structure as in the previous point, where society is the cause and narratives the effect with the difference that here the causes are inferred (or more likely guessed) through the effects; b) reversed causal structure, i.e., to which extent the Latvian cinema (cause) affects the society (effect); c) a simple correspondence, i.e., not a causal relation, just a comparison between two objects.

The author categorises his work simply as qualitative in nature (ibid.: 9). Now, any attempt that tries to go beyond the first aim – analysis of the visible content of the films – will be impossible for the qualitative approach. The central problem here is causality or, rather, the convincing "proof" of it. Causality can be talked about, different guesses can be suggested, but above all, the relation must be shown to exist. For example, excessive drinking of alcohol does not cause a higher risk of cirrhosis simply because someone thought of it. A synthetic statement is true or false not based on that statement alone, it must be proved or disproved empirically. We can think of the link between society and films as a logical one, i.e., analytic but it would turn out to be quite uninteresting, for it would be a mere tautology. Stating that "what happens in a society is societal in nature" might be true but meaningless in the sense that we already know it. To say, more or less, that "what is, is" is of course true but it does not have much of explanatory power. There are good reasons to believe that there is not a one to one correspondence between films (as a whole) and society (in Latvia or any other country).

Therefore, a rigorous empirical investigation is required, especially if we are interested in causal relations. Qualitative research which, at least in the literature, is mostly described as one that tries to "understand" the "meaning" of human action, what meanings are attached to the action, etc., is ill fitted for this task. According to an extreme version of constructivism, reality is created by talking about it, which means that it could be possible to show that if there exists speech (written or spoken) of society causing particular films, then the reality must be so. I do assume, however, that this variant of qualitative research can be ruled out.

Can qualitative approach be sufficient for proving a correspondence between films content and the surrounding society? Obviously a one to one relation is out of the question, but what about some parts of the films, say, the attitudes of the characters? Can a parallel be drawn between the film and society on a psychological level? Again the answer is mostly no. On the one hand any person, as an individual and as a part of his society, can make a comparison between a seen film and reality. That person has certain feelings, aims etc., and he lives and has lived among other people who also have feelings, aims etc. and so has certain kind of anecdotal knowledge about himself and his surroundings. Based on that a comparison point can be made. But this is something that is open to almost anyone and qualitative method does not add anything extra to this. However, to go beyond the anecdotal, no matter how much sense it seems to make, requires rigorous empirical testing which is not present in this work, and for which qualitative research does not offer tools. The author can guess, as a Finn or as a Finn who has spent some time in Latvia, about the connection between the Latvian films and Latvian society, or he can make an "educated" guess based on what to him seems like relevant literature, but that guess, if it is to be taken seriously, will need something much more. And this "much more" is absent from this work.

There is one author's aim where the qualitative approach, or some version(s) of it, can work; namely the first aim, i.e., analysis of the films (or their narratives). For example, the films can be described as having these or those characters, the plot can be explained, all or some of the films can be classified according to existing genres or new ones can be made up. In short, anything that has something to do with the description and/or the interpretation of meaning can be at this point classified as qualitative. I say "at this point" because a description or the act of describing is not something that is immanent to the qualitative approach. In fact, it is not a prerogative of any method. Rather, it is something that we as humans do in everyday lives. There is nothing scientific about it. But a description must be in any case "good enough"; it should be shown why this or that conceptual framework is particularly good or useful.

The most important classification is both the sjužet and fabula which are, according to the author, formulated as how "the concrete narrative (sjužet) exposes the story as a whole (fabula) in a temporally continuous process. There are five additional classificatory categories: exposition, narrative voids, causality, narrative strategies, and classical narrative. (ibid.: 3.) Now, the concepts of sjužet and fabula do not seem to be that simple, at least the author presents them in a rather difficult manner. So for example on the page 39 the author explains that

> fabula can be illustrated in the following way: The story [or narrative] contains two events A and B of which one is present and the other is not. When A is present, B is not. A relation can be imagined between them which can be causal, temporal and/or spatial; for example, A can be the cause and B the effect. According to the formalists [here the author is referring to the Russian formalism] imaginary or idealised connection corresponds to the fabula, i.e., the story [or narrative]. Fabula also means the imaginable narrative totality which contains both A and B. The fabula-level thus forms an imaginable chronological chain of events and a world which is based on causal relations. ... Fabula corresponds to the ideal [or idealistic] level to which the concrete sjužet relates; according to Šklovski (1965b: 57) ... the fabula serves as the material for its' sjužet-shaping [or forming]. Sjužet, or the concrete storytelling, exposes this narrative totality in time – by slowing down and delaying. Sjužet can be translated as plot but, for example, according to Bacon (2000: 26), the concept of plot is too limited to accurately describe sjužet-narrativeness to which everything that is presented connects – including so called "having no plot". Sjužet is above all the transmission of fabula-related temporal, spatial, and logical relations. According to Tynjanov (2001: 311-315), fabula is not given, it does not cover the sjužet-level, instead fabula must be formed based on the sjužet-narrative [or sjužet-story telling]. (brackets added)

There are even more "definitions" of these concepts but, frankly speaking, it is difficult to understand what is actually meant by all this. (Heikkinen (2001: 128) describes fabula as all those themes that more or less every human being shares, e. g., birth and death, love, struggle, freedom, etc., and sjužet as the concrete manifestation of those themes, for instance, as this persons love, in this particular place and time. Although this makes more sense, it does lack that esoteric ring to it.) When one reads the actual analysis it becomes quite apparent that all these concepts are more or less equivalent to already existing, everyday-language counterparts and that, in the end, we are dealing with a case of “word magic”.

So what the author does is a description of films where it contains elements that 1) are visible and knowable to everyone who can potentially see the films but also elements that 2) are not visible to everyone else, i.e., elements which – without further evidence – are the products of the author’s imagination. These latter elements are quite obvious when one reads the actual analyses of the films, i.e., opinions of not what is given but of supposed meaning. For example, on page 75, the author describes the acts of “Eriks” (a character from the film “Drosme nogalināt”) who, after witnessing his own father raping his beloved, ends up stabbing his father. According to the author, “Eriks’ actions can be compared to (in a psychoanalytic interpretation) a symbolic act, to killing his father, which releases him from his father-related past and thus frees his own future” (ibid.). Although quite fitting for the qualitative approach, this does not offer the required necessity which enables to go beyond what is subjective, in this case dependent on the author (his moods, feelings, expectations etc., which more than likely would vary when compared with someone else and thus which would likely cause different interpretations).

The third ingredient – in addition to the theoretical part and the films themselves (with their respective interpretations) – is formed by interviews. The author has interviewed 17 directors and/or other experts and tries to form (based on the interviews) the context of Latvian cinema (ibid.: 9). Unfortunately no further elucidation of the interviews, as to what was asked, how they were conducted etc., was given. The author only mentions that the interviews were conducted according to recommendations of “Hirsjärvi & Hurme (2000: 17, 22, 24–26, 105)” (ibid.). Clearly this is inadequate, for the purpose of a methodological clarification concerns the author’s own work. Any author can add legitimacy to his claims by an appeal to an authority (although this is principally an incorrect form of argumentation), otherwise known as referencing, but whether there is a reference or not, the author must explain what he is doing in his own work. To write a book, an article or a thesis where the author states that he has done something, and that something is to be found completely somewhere else, is simply unscientific.

But there is also an additional problem concerning the interviews, namely the intended proof which these interviews should give. Considering that the interviewed are directors and other experts (whatever these might be) it is more or less obvious where their field of competency lies. Or in other words, it is obvious what they represent. And what they represent can very well be Latvian cinematography, not only the present but also the socialist one, but what they cannot represent is the Latvian society as a whole. Their memories and general knowledge can give great insights into the development of Latvian cinema during the post-socialist era (and here the author seems to be quite successful) but any further linking between the cinema and the society as a whole would be quite difficult. Again, guesses can be made, but these guesses can be, to the extent possible, assessed only with the help of much more robust material, both theoretical and empirical. Unfortunately, this material is absent. The problem, then, is the following: on the one hand, these interviews cannot give and explain the necessary connection between the cinema and society and, on the other hand, they are not necessary for the act of describing the individual movies.

We can say, then, that the source of methodological problems lies in the mismatch between aims and supplied evidence: the type and qualities of the objects of interest cannot be answered with the type and qualities of the evidential objects. On a more "positive" note, the initial mistake is at least carried on consistently and coherently, i.e., that the author sticks to what he is doing and not sailing here and there.

Viestintäteknologia ja utopiana ja dystopiana (*Communications Technology as Utopia and Dystopia*) The author states that the aim of the work is to "study the relation between communication technology and both utopia and dystopia" (pp. 3, 5). On the same pages the author explains that this relation is to be approached from a historical perspective (i.e., what different "thinkers" have thought), by making a comparison between these thinkers and religious accounts, and that there will be an attempt at finding reasons (or causes) for why attitudes towards technology can reach such overly optimistic (utopian) or pessimistic (dystopian) levels. A suggested tentative explanation, for the cause(s), is that of technological determinism, or where it is being thought that technology is the primary "mover" behind changes in society and culture (ibid.).

The first categorisation of this work is to be found on page five, where the author classifies his study as theoretical. He continues, on page six, that the used method can be described as philosophical. This is immediately followed by stating that "the method of my qualitative research is philosophical" – because he finds the character of contemplative approach (the philosophical part) as best fitting (ibid.). The confusion, then, concerns the description of the work as, theoretical, qualitative, and philosophical. There is an important "qualitative" difference: theoretical work is theoretical while qualitative is both empirical and theoretical. In reality, based on what the author has actually done, the work can be tentatively classified as theoretical, mostly in the literary review sense since the different theories are more or less taken "as they are" without doing any interpretation of what the authors "really" meant or if they made any sense, i.e., lack of analysis. It is possible to describe a work as philosophical but it should be clarified what it means. After all, there are metaphysics, ethics, epistemology, aesthetics, logic, philosophy of language etc., to choose from. It seems, though, that what the author had in mind is really to contemplate, or to think. However, we cannot accept the act of thinking as a particular (philosophical) method. So, at this point, we can say that the description of this work as philosophical is wrong and that there also seems to be some amount of discrepancy between the used terms "theoretical" and "qualitative".

If we return to the work being theoretical, the author explains it as something that can be characterised as, inside media studies (or research), "historical systems research" (ibid.: 5). By "historical systems research" the author means a direction inside media studies "that tries to combine societal aspect and the media attempting at an overall historical perspective". This includes, in addition to the history of technology, media and communication, also historical review of society and ideology, not to mention cultural philosophy. (ibid.) First question here concerns "historical systems research". What is it exactly? There is systems theory (with all of its derivatives) which can be studied as it is now and/or how it has evolved over certain time (historical perspective). But systems theory has nothing to do with this particular thesis. And, on the other hand, the combination of societal with communications/media/technology is nothing new or out of the ordinary. In fact, the societal part is strongly present in media studies; so much so, that sometimes it is very difficult to differentiate it from the other various social sciences. Media studies, for instance, could very well be categorised as media oriented sociology. The author elucidates also his "philosophical method". On page six he writes:

> Niiniluoto (1984: 66-67) describes the core of philosophy as being an activity based on critical thinking and debate [or argumentation]. It does not consist of ready and final knowledge but, rather, it is a continuous attempt at clarification ... of concepts and thoughts. Philosophy can be as "scientific" as any other scientific field, however, the problem of value-free science is connected with theoretical philosophy. According to Niiniluoto (ibid.: 328), values cannot be logically inferred from knowledge [or facts] and knowledge [or facts] cannot be inferred from values. But there can be interaction between them (ibid.). (brackets added)

What is presented here is a philosophical "cocktail" of different aspects of not only philosophy but also science in general. But the real question here is "what does this mean?" What is this supposed to explain? We can say the same thing of any scientific field, or even everyday life, that "it does not consist of ready and final knowledge"; and science in particular tries to "constantly clarify". There is an obvious connection between "what" is knowledge and "how" it can be reached. However, hopes of "final" knowledge have been long since abandoned, therefore, an explanation of method should be an explanation of said method. Final or absolute knowledge is irrelevant from methodological point of view; being better suited or giving more reliable results compared to some other method(s) is all that is needed. Nor does it help much, from methodological point of view, to suddenly jump to the question of values. What is meant by value-free or the lack of inferential capabilities between facts and values? Perhaps this is meant to say that the different theories of different "thinkers" are value-laden and, hence, represent not so much what "is" but only opinion or preference. But even if this interpretation would be correct, the existence and the usage of those theories (as in: this person wrote this and that person wrote that) would be a matter of fact, not value. In any case, it is really impossible to say what the author meant by this.

We can now turn attention to, what is perhaps, the most serious methodological inconsistency. The author stated that one of his aims is finding reasons for the overly optimistic or pessimistic attitudes towards technology, i.e., establishing causes and their explanations. Nothing in the stated methodological content suggests, even remotely, how this is to be done. Yes, the author mentioned technological determinism as a possible cause but, unfortunately, this does not even begin to resemble a sufficient solution – both analysis of and empirical evidence of such a relation is missing. One certainly can make guesses about anything at all – and in science guesses are made – but the work cannot remain exclusively at that level. Otherwise any outrageous claim should be considered as science. Fortunately real science doesn't work like this. Unfortunately, there is a lot of pseudo-science that does. (Furthermore, technological determinism, even as guesswork, is not convincing since the author briefly manages to introduce the idea that technology might be neutral after all. On page 97, he writes: "Huxley (1983: 59) reminds that mass communication, in itself, is not good nor bad, instead it can be used for both". This is a considerable anticlimax since the chapter of technological determinism comes after this.) The shortcoming of this thesis is, then, the same as was the case of the previous one: the type of result cannot be, and is not, supported be the offered "evidence". But, additionally, there is a healthy dose of inconsistency added on top of the initial problematic. (If a claim is made – no matter how weak – one should stick to it. Otherwise it leads to a "both yes and no at the same time" contradiction which is pointless.)

Valokuvan suhde todellisuuteen digitaaliajassa (*Photograph's relation to Reality In the Digital Age*) Although I wrote that stylistic issues don't concern me, here I make an exception. Now, style in itself - any style that is considered as "proper" – is not a guarantor of the overall quality of results. But despite this, there are some things that, rather than being included, should better be left out. One of these stylistic issues is the "dear diary"-approach. The purpose of introductory chapter is to raise interest and to perform a "fade-in" operation, to build an opening for the main work. The beginning of the introduction says that "photographs are very important in my life and that is why I want to study the topic more in depth. I am an enthusiastic photographer... ...Digital image processing is my personal passion." (p. 3). While it may be true that the writer has deep interest for photographing, it does not mean that the same applies for the reader. Therefore some other way of approaching the topic might be more appropriate. The fact that this work does not contain a methodological chapter is but a moot point. It is impossible to reconstruct from the whole thesis what has been done methodologically. Based on the actual text it can be said – very generously indeed – that the work is simply theoretical in nature no matter how meaningless this description in this case actually is.

The author states her aims as “an interest in a photograph’s relation to reality”. Particularly, the author attempts to describe a “photograph’s relation to reality, what is this relation now, and what it has been before”, and if “digital technology changed this”. She wants to “especially delve into questions regarding a photograph and reality...” (ibid.: 3-4.) These aims, in themselves, are not problematic; after all, this relation has been and still is debated. The problem is that the author tries to extend this relation to cover everything but the kitchen sink: the result is a random list of quotations and references rather than a structured and themed development of thought. The only thing that can be considered as a methodological reference or a clarification is the following remark on page 7: “The structure of my work is strongly dialectic. I will construct a continuous discussion, from different perspectives, about photographs and their relation to reality in digital times.” From a strict methodological point of view, describing a work as “dialectic” is not really saying much. Furthermore, there are, for example, the Socratic, Hegelian, Marxist etc., dialectic but it does not seem that the author had any of these in mind. There is really no Socratic dialogue which refines and clarifies different concepts, nor is there a Hegelian construct of a thesis, anti-thesis and synthesis. If the author means by a dialectic structure that there will be a dialogue of sorts then that is sorely missing. However, if a list of quotations and references can pass for “being dialectic” then yes, this work is dialectic.

The work can be summarised by the author’s reference to Juha Suoranta when she says that “being digital in itself does not change the form of messages nor does it make the photographs more false [or deceitful] or true. Instead, it opens new possibilities regarding the production and manipulation of photographs.” (ibid.: 64.)(brackets added.) It is difficult to see how the author's closing remark is something that needed scientific (even as an attempt) study. For laymen living in the same society as the author, and who are thus expected to have some basic experience with photos in general, the offered "grand finale" will contain very little of substance. For the most likely audience, i.e., those studying media, communications, or students in general, the final chapter offers even less. In other words, the author has done something which, in science, should not be done, namely, saying the obvious, or studying and concluding what is already known. It is difficult to say what is the most serious methodological offense. The work is too incoherent for that. It can be said that all the things that make science what it is are missing here. No amount of random references can be an adequate substitute.

Myöhäiskapitalistinen kulttuuriteollisuus: Mytologinen analyysi postmodernistisista kulttuuriteollisuustuotteista. (*Late Capitalist Culture Industry: Myth analysis of Postmodern Culture Industry Products*) The author's aims are 1) to show that critical theory (Frankfurt school) is still useful as a tool for analysis of the present day media culture; 2) to introduce an "updated" version of the concept of culture industry by combining it with Fredric Jameson's ideas of postmodernism and Barthes' myth; 3) to proceed from the general to the particular, i.e., to begin with Frankfurt school and ending up with a case analysis of a song (Vihma by the band Värttinä) and to show a general ideological continuum in media research from the beginning of 20th century until the present day and ending with 4) a myth analysis, based on the "updated" concept, of a contemporary culture industry product. (p. 3.) It is not exactly a straightforward task to categorise this work – for reasons that will be mentioned later. Let us call it provisionally as "theoretical".

Since the author has not really offered any methodological explanation, we may just as well conduct the "deconstruction" according to the author's intended aims. In the introductory chapter the author, among other things, criticises the "linguistic shift" in science and the followed methodological preference for discourse analyses. This critique is justified, and supported by two categorical examples: 1) if all is about discourse, and everything is just a linguistic (social) construction, then, for example, the discrimination of poor people could be solved by changing the use of language (the author here, p. 6, referenced Naomi Klein); 2) that media is part of a system (society) which is affected by economical issues, it would, therefore, be rational, when investigating media, to also investigate the affecting economy (the author, p. 7, references Ampuja). What creates some confusion, though, is the fact that the author speaks of this economical aspect as that of “political economy” but states that he is not going to follow this route and that, rather, he is more interested in another, namely, Frankfurt school and its critical concept “cultural industry” (ibid.).

Now, it is true that Horkheimer and Adorno’s depiction of cultural industry includes many elements regarding the “aesthetics” of culture industry’s products, as well as use of language that many times is closer to general literature than science, but one thing is for sure: the inheritance of Marx, which is present in their work, is about critique of capitalism which in essence is about political economy. Many of the concepts that Marx used, for instance, the fetishist nature of things, lack of choice, alienation etc., are also to be found in the critical theory. In other words, while critical theory adds, for example, the aesthetic element, a great deal remains Marxist in the “political economy” sense. If the author, then, wants to take into consideration the economic aspect of media, he has to deal with political economy in one way or another, especially so if he is interested in the concept of “cultural industry”. Now, both Adorno and Horkheimer can be accused of overlooking the kind of analysis carried out by Marx – and I think this would be a justified accusation – but that is a fault which should be corrected, not further perpetuated. In other words, critical theory (or the concept of culture industry in this case) took one part of Marxism while leaving out others without severing the connecting points. The links to economy are left open without delivering the actual goods. The author also mentions (ibid.: 22-25) that there exists critique against critical theory, especially the concept of cultural industry. It is not clear if the substance of this critique is shared by the author as well, or if its function is simply “to be”; to show that there is critique, regardless of its merits. At this point, however, the usefulness of the culture industry concept remains open.

Next, the author delves into postmodernity and Barthes' myth (analysis) which, roughly, comprises two thirds of the overall theoretical part. This is also the most problematic part, which really shouldn't come as a surprise considering the track record of "postmodern thought". For instance, the author does not limit the use of "postmodern" only to refer to a particular artistic style, he, so to speak, tries to capture the "ethos" of contemporary times in a linguistic fashion that is difficult to "decode". Referring to Morley he states, p. 26, (brackets added)

> that postmodernism can be seen as a new era of social life, an era that delays [the author uses the Finnish word "myöhentää" which could be translated into a more poetic "latens"] the modern; secondly, postmodernity can be understood as a cultural sensitivity; thirdly, the topic can be characterised as an aesthetic style, as the ethos of the times; and finally, postmodernism can be understood as a way of thinking, a sort of theory that fits contemporary analysis.

Except for a reference to particular (aesthetic or artistic) style, the other usages are highly contestable; the question is not one of fit but of meaning, i.e., what does, for example, the phrase 'new era of social life that "latens" the modernity' mean? The author does mention Habermas and his idea that the project of modernity is not yet finished, but to say that, on the one hand, there are certain things that have not happened and, on the other hand, that these things will surely happen, it's just what we are doing now "latens" them, are two different things. The previous makes sense as it is a simple observation while the latter is either a more or less nonsensical jargon or a claim of future state of affairs which, regrettably, has no evidence (empirical or logical) in its support. On page 27, the author continues that "the death of grand narratives" is perhaps the most distinguishing mark of postmodernity and as an example mentions the fall of Soviet Union, the crisis of welfare state etc. However, the human history is filled with "big events" such as these, for example, the end of the Roman or British Empire, the black death, the Russian revolution of 1917, just to name a few. In this sense, then, we have been living in postmodernity at least since Ancient Athens lost her independence.

If the postmodern remains somewhat vague a concept, the Barthesian myth makes it certain that clarity and validity are not the aims of this analysed thesis. The author writes (ibid.: 46) that according to Barthes, myth is simply speech and thus everything can be myth. There are some limitations but these do not apply to content, only form (ibid.). Myth does not arise from the nature of things, instead, it is discourse chosen by history. Myth steals language in order to naturalise an object, to make it ahistorical. In Barthesian sense myth's endeavour is to wipe out history." (ibid.: 47.) On pages 47-50, the author describes myth as a semiotic system (based on Barthes) which, as is/was the case with postmodernity, is more a projection of the analyst's mood rather than clarification. On pages 50–52, the author explains how to read or interpret a myth but there is nothing procedural about these procedures, i.e., any "interpretation" will be equally valid and procedural if evaluated according to the Barthesian "method". And if really "everything goes" then the system is defunct for there are no criteria for evaluation.

The author continues that in order to expose myths, the mytholog has to be able to explain concepts. This is done with the use of neologisms, since the concepts found in dictionaries lack historical spectrum. (ibid.) Two problems emerge: 1) neologisms (as concepts) may have meaning to the person using them but not necessarily to anyone else; 2) the definitions of concepts in a dictionary do contain historical continuation, for they are put together based on the wider linguistic usage. Common acceptance or implementation of a word or a concept does not happen overnight. This process can take time but once a concept becomes common usage, it can remain so, relatively unchanged, for a long period of time. Therefore, it is more likely that a definition of a concept from a dictionary will be clearer and having "wider historical spectrum" than any neologisms produced by this or that individual.

Moving along, the author is ready to put together a decoding mechanism which then can be used to analyse cultural products. It has been mentioned before, and on page 58, the author mentions it again, that "Horkheimer's and Adorno's model does not include a theory of decodification [or interpretation]. However, a suitable one can be found from Barthes. (ibid.: 58.). The possible unclarity, according to the author, mostly concerns which role (especially that of the mytholog) is reserved for whom. (ibid.) Not anyone can be a mytholog for he needs (here the author references Jameson) to have "the political will to hold on to the truth of postmodernism, its fundamental object, the universe of multinational capital" (ibid.: 59). However, as we have learned from Corner, semiotic analysis is broken as a method. We can talk of it, if we like, as a theory of decodification, but that doesn't change the fact that it is a useless tool. To see contemporary culture as schizophrenic or perfectly sane are both equal interpretations under the Barthesian model. As if this wasn't enough, the author decided to fuse it with Jameson's wisdom. What on earth does Jameson mean with the above quote? And how does it affect the "decodification model"? Unfortunately, no answer is given. So, on the one hand, we have the author saying that there is a decodification model that is useful in someway or another and, on the other hand, we have no idea what that model is supposed to be; how it should be used, what are its limits, and so on. In reality, then, this so-called useful model has no use whatsoever.

As to the third point, we can certainly ask if the one particular song by a particular band "represents" the culture industry enough. Though, it should be added that it is not only culture industry, it is the whole culture which is supposed to be more and more commercialised. So, then, does this one song represent adequately the whole culture? The answer must be a strong no. Additionally, the things that ought to be signs of commercialised culture products are, among other, certain "standardisation" and manipulativity of those products. However, if we begin, let's say, from the Renaissance, we will find that most of the art, whether painting, music, or theatre (and later cinema), has been structurally "standardised". Even the great works of Mozart and Beethoven can be (or could have been) manipulated, i.e., substituting any part with something else, or cutting off some of its material. Victor Borge, the Danish pianist, in his numerous comedy acts, performed a sort of pastiche where he would "glue" together parts of different classical compositions (from different composers) making the shift (from one part to another) fluent and "fitting". This "manipulativity" is not a sign of lack of quality; every meaningful composition is a certain melodic "construction" containing different parts, in certain key and tempo. In addition, both tempo and key can be modified which makes "substitutability" of different parts even more straightforward. Naturally, there is a difference between a symphony and a three-minute pop-song. The latter one, being based on perhaps three or four chords and simpler melodic/rhythmic progressions, will be easier to manipulate but if we take, for example, the older folk music, the difference between then and now becomes less evident. So if a pop-song is analysed and concluded that it is "substitutable", it not only cannot prove that critical theory is correct, it cannot even support the feasibility of the "new and improved" decodification mechanism.

Based on the above, it can be said, then, that the thesis contains serious flaws: 1) feasibility of the old concept of culture industry is left open. The author simply doesn't explain what he means by this feasibility (or usefulness). For instance, the quality of being repetitive or, to a certain extent, standardised, cannot be used to differentiate between "high" and "low" culture as all cultural creations share these qualities. It is, then, the personal preference of the analyst what he considers as high or low culture, or commercialised and non-commercialised respectively. Adorno certainly didn't conceal it in any way that he disliked jazz. On the other hand, Adorno had high respect for Arnold Schönberg whose music certainly isn't comparable to jazz. Then again, even Schönberg's music suffers of all the commercial culture's qualities since it is repetitive in the sense of a pure cacophony – one certainly can substitute any part of the music with, say, the initial tune-up by the orchestra; it is also a wonderful sing-along for the completely tone-deaf; 2) due to the initial critique of the so-called linguistic shift in the human sciences, one would have expected a development of the political economy side of things. Surprisingly, however, the author, for whatever reason, didn't step out of that linguistic shift at any point of the thesis; 3) the updated model (culture industry + decodification theory + postmodernity) is useless. After peeling off the meaningless postmodernist jargon, we are left with culture industry and Barthes. But since the latter's model is unmethodological as anything can be, no correction or improvement can be achieved over the initial state of affairs, i.e., the concept of culture industry; 4) the case example remains uninformative as no valid generalisations can be made. For that more is needed: wider sample and more precise theoretical knowledge that would explain why this or that quality makes cultural creations commercialised. Although the concept of culture industry contains such qualities, these are shared by all culture as such and, therefore, it has extremely limited analytic value; 5) because the analytic model is so loose and all-encompassing and conducted by only one person (the author) the thesis only shows what the author thinks of this or that but that is all; 6) as such the work suffers from two basic problems: incoherence and a mismatch between aims and intended evidence.

Kriittisen mediatutkimuksen alkulähteellä: Theodor W. Adorno ja teoria kulttuuriteollisuudesta (*At the Fountainhead of Critical Media Research: Theodor W. Adorno and Theory of Culture Industry*) This thesis represents what can be called ”plain vanilla” approach. Not only is the stated methodological part cut down to an absolute minimum, but the author also stays true to it and does not start to “wander about”. What little there is about methodology, it is to be found in the abstract and introductory chapter as simply: “my thesis is theoretical in nature” (ibid.: 3, 5). And after reading the work, it has to be concluded that, yes, this is the case. Now, the author could have added some minor details, such as, is the theoretical review historically oriented, is it contemporary, or both perhaps etc., but the lack of these details, from the “grand scheme” point of view, is really irrelevant.

The author states, as his aim, that, firstly, to present an overview of Adorno’s thinking and, secondly, to defend critical theory (especially the concept of culture industry) from perhaps too hasty and one sided critique that has been put forward in media research, especially in its culture or linguistically oriented branch. (ibid.: 3.) In short, the author aims to review mainly Adorno and to a lesser extent also other representatives of the Frankfurt School and to show that critical theory is not dead. One thing remains unclear though, namely, that on page five, the author writes: “It is my aim to find out what Adorno really wanted to say and what he meant when he talked about culture industry”.

There are two basic ways how to approach this, one is relatively problem free, while the other, relatively problem ridden. Alternative number one can be described, to a certain extent as positivist, but even better description would be the “common sense” approach, where whatever someone means is to be found out from what that person said or wrote. In this case, then, what Adorno meant was simply what he wrote. Some “interpretation” (or approximation) will be present because there is no 1:1 correspondence between a concept (or a term) and reality. And since it is mostly reality we talk or write about, the non-perfect correspondence will leave some things in need of “interpretation”. On the other hand, every meaningful concept (or term) will have a certain "core" that is more or less clear. Alternative number two is more esoteric and, therefore, fit of being labelled as qualitative or hermeneutic. It will try to go beyond the empirical evidence (written text) and to show the "real meaning" (whatever this "real" may mean. This would be, of course, utterly impossible for what Adorno really meant is known only to Adorno, and he has been dead for some time. Fortunately, the author has mostly chosen the first alternative.

Quite frankly, on the basic level there is very little to be criticised. I mean, sure, the attempt to show that critical theory is alive and kicking has strong resemblance to the previous example's myth analysis in the sense that the fit of the theory will be based mostly on personal preference – unless, of course, one raises the level of precision, narrows down the scope of application, and produces convincing empirical and/or theoretical evidence which shows that such and such must be the case. Also, it would have been nice if the author was more verbal about his aims and intended course of action, but, if we concentrate on the theoretical nature of the thesis, in the literary review sense, then it must be repeated that the author has basically proceeded correctly. It is, quite frankly, disheartening to see that only 1 out of the 4 sampled higher grade theses got it more or less right.

Elokuva unten mailla – Unen ja elokuvan analogia (*Sleeping Cinema – Analogy Between Dream and Film*) What sets this thesis apart from the previous ones is that this one contains a chapter on methodology. However, the attempt remains as perhaps the only redeeming factor of the work for the contents of that methodological chapter make little sense a) in themselves, b) in relation to the aims, and c) in relation to the overall work.

Ad a. It is difficult to understand what is/was the purpose of the stated methodological considerations. Different concepts are mentioned but they do not create a meaningful whole. They seem to be put together without checking if they fit and what they amount to. Secondly, some of the stated concepts are either wrong, from the scientific point of view irrelevant, or obvious (i.e., necessary but not really scientific as in, for example, breathing is necessary but it is not a particular scientific method nor a philosophical consideration). According to the author, who is referencing Algulin, the scientific approaches of the humanities can be divided into five areas: 1) observation, 2) description, 3) analysis, 4) interpretation, and 5) total experience. Further, that the first and last points do not really belong to a scientific research. Explanations are mostly given in the analytic phase. This phase consists of various analytic methods, for example, linguistic, stylistic, genetic, structuralistic, and semiotic. (ibid.; 6-7.) First of all, what does "total experience" even mean? And if it does not really belong to science why has it been mentioned? Also, how come the first point (observation) does not belong to scientific investigation? When the most fundamental aspect of science (even in the scope of the humanities) is claimed to be unscientific, something has gone terribly wrong. There exist no (empirical) scientific field that does not rely on observation. The difference between the various scientific fields affect how observation is conducted, i.e., how controlled the situation is, how the observed material has been chosen etc. Observation as such is of course self-evident, but much of discussion concerns what kind of observation is adequate, what can or cannot be observed, what must be observed and so on, and this part is quite at the heart of science.

On pages 7-8, the author (referencing Routila) writes about different ways of reasoning: deduction, induction, and abduction. She continues that according to Routila abduction is the primary form of reasoning in studying art. However, Routila didn't exactly write that. What he did write is that: "to construct abductions and using both deduction and induction is a central task even for the study of art" (Routila 1986: 28). Furthermore, Routila emphasised that these three forms of reasoning are "tightly connected with each other, so that none of them can be even temporarily bracketed without destroying the essential nature of this totality" (ibid.: 26). Then again, considering that abduction basically means "having a hunch", well, there isn't anything methodological about that. I mean having a hunch or inventing hypothesis is, of course, a central feature of science but, so far, that part has not been formalised into a method. And it even cannot be, since it would literally mean to "discover the logic of discovery" or "the method of invention".

The author continues, on page 8, that “reasoning plays an important role when interpreting films. When films are compared with dreams, the starting point is an assumption for which, at first, a basis is built by reasoning and interpreting films.” Also, the author claims (ibid.), that she is going to incorporate the previously mentioned five approaches, apparently even including the “total experience” whatever it may mean. She continues (ibid.), that she is going to use this in interpreting the film *Mulholland Drive* and, also, to choose as the theoretical framework Freud’s and Jung’s psychoanalysis. It is good to know that “reasoning plays an important role”. This is certainly better than if reasoning did not play an important role. As to the second point, well, reasoning and interpretation doesn’t really strengthen the assumption; an assumption is a result of the two. However, if by assumption is meant a completely unfounded and wild guess, then as long as we are talking about empirical matters, it is the empirical evidence that decides. But even this is irrelevant because to say that one intends to compare films with dreams is equally uninformative as saying that one is interested in comparing leprechauns with unicorns. Let us hope that the author will explain in higher detail what this comparison should entail.

At this point it seems that the author considers her work as qualitative for she writes (referring to Eskola & Suoranta) that “the purpose of analysing qualitative material is to bring clarity to this material and thus create new knowledge about the researched object” (ibid.). Apparently this is to be done, in her thesis, “through combining the thoughts about Mulholland Drive’s dream-likeness by analysing it as a dream in multiple ways” (ibid.). Immediately there are several points that demand examination. Firstly, how does qualitative material differ from quantitative? According Dey (1993: 11), for instance, “quantitative data deals with numbers, qualitative data deals with meanings”. Dey (ibid.: 12) continues that

> qualitative data embraces an enormously rich spectrum of cultural and social artefacts. ... By comparison with numbers, meanings may seem shifty and unreliable. But often they may also be more important, more illuminating and more fun.

Now, Dey's statements may seem as typical of those of the qualitative camp, although, in reality, he seems to be a "soft" positivist in disguise as he continues (ibid.: 28) that

> It is more useful to define qualitative data in ways which encourage partnership rather than divorce between different research methods. In suggesting that quantitative data deals with numbers and qualitative data deals with meanings, I do not mean to set them in opposition. They are better thought of as mutually dependent. Number depends on meaning, but in a sense meaning also depends on number. Measurement at all levels embraces both a qualitative and a quantitative aspect.

In other words, films are not qualitative material (from methodological point of view), nor are they quantitative. They do have certain discernable "qualities" though. We can discuss and think what these qualities mean and, equally, we can also measure them. The difference, then, between qualitative and quantitative is not at the level of material. Rather, the difference is "in our heads", i.e., what we are interested in and what we end up doing with the material. Secondly, the idea of "bringing clarity" is surely not limited to qualitative research or analysis. It would be strange if the purpose of quantitative analysis would be to "bring fogginess". The fact is that all (scientific) analysis aims at clarification. Any form of analysis that does not do or attempt to do so, has no place in science. Thirdly, although it is possible I have misunderstood the author, it still seems to me that she is talking about brining order or explaining the material. Insofar as this is the case, it would be unsatisfactory (see more, for example in Töttö 2000: 125-126). What Töttö is essentially saying is that by explaining the data, and not the phenomenon which the data is supposed to represent, nothing has been explained. This is because every empirical data can be "explained" in any way whatsoever; every thus created "explanation" will be "true" because the data will ascertain any claim made about it.

On page 9, the author writes that empirical data can be studied either with or without theoretical assumptions. Now, it is true that empirical research can build on existing theoretical knowledge but it is absolutely false to claim that empirical study can be carried out without a guidance of a theory, at least if by theory is also meant any leading idea-structure. (The idea here is that a theory is formed inductively by sifting through empirical material. Considering the complexity and abundance of qualities that the various objects and phenomena have, one really has to wonder how anyone could arrive at a meaningful theory.) This idea of a theory-less research can be found among some or many supporters of qualitative research. Some, for example, Holloway (1997: 5, 153-154), think of it as being a general feature of qualitative research, whereas some think of it as belonging to grounded theory, for instance, Dellve et al. (2002: 141). On the other hand, for example, Kiviniemi (2001: 72) concludes that the mind of the researcher is not "tabula rasa" and that there is at minimum some kind of theoretical guidance. This is more or less admitted even by Holloway (1997:. 6) which makes it difficult to know what she really means. Eskola & Suoranta (1998) offer the same "perhaps or perhaps not" for, on the one hand, they say that quantitative research can be theory-less and even easily so and, on the other hand, that theory is more important in qualitative research (ibid.: 81). However, on page 83, they write that in qualitative research, theory can be built from the empirical data. This "first data then theory"-approach is repeated again on page 196 in relation to discourse analysis. Empiricism as a complement to reasoning – in the Comtean sense – is what science is about; Empiricism as a 100 per cent inductive system, i.e., as "empirical data first, theory second" is impossible.

Ad b. What the author essentially tries to do is to show that there are or can be similarities between dreams and films. Although she writes of "analogy", she ends up doing a comparison of qualities; for example, a dream can have a certain kind of visual content or a temporal progression and if something similar can be found in films, then there is a similarity between the qualities of a dream and film. It is something like making a connection between a carrot and a car based on the same colour, for instance. The theoretical framework of psychoanalysis turns out to be "the wrong tool" for the purpose because psychoanalysis tries to give explanation to visible qualities (behaviour) – well, technically it is the "invisible" qualities which manifest themselves in a material way. Psychoanalysis as just a description of (empirical) behaviour has very little to offer. To say that "Tom was hostile", "Sarah violently tossed around her food", etc., is something that can be described with the use of everyday language by regular people in all situations. So to say that "a dream can be hazy" and that "film x is hazy" is not particularly theory-laden. Furthermore, even in a comparative study, representativeness must be maintained. Do the films mentioned (or analysed) truly represent cinema as a whole? An explanation of the films' possible represenrativeness has not been given by the author and, so, it must be assumed that they are singular cases. But this raises the question of what does it matter if there is a connection of sorts between dreams and this or that particular film? How does this further science? Or is this, perhaps, another case of “art critique”, or postmodern thinking perhaps?

Ad c. What happened in this thesis is something similar to the previous ones. It unfortunately seems like the authors have misunderstood what really is the empirical material and what is being researched. In these confused theses the empirical material is in fact the authors themselves; it is about what meaning(s) they give to whatever it is that they write about. We, as in readers, can not know if the meaning is really contained in the objects of their analysis because it goes beyond the material, i.e., beyond what could be perceived by the human senses. What is available, though, to the senses – for us readers – is the text contained in the theses. Therefore anything about the meanings of the films, that go beyond the literal, is available only as written statements by their authors; and it is these statements that are open to evaluation, not the meanings that may or may not be contained in the films. These are, then, really case studies where the cases are the authors and/or the meanings they attach to these or those objects. Based on the evidence we can take seriously the fact that the authors have certain opinions and believe in their meaning constructs. But the links from the content of these opinions and beliefs to the real world are missing, i.e., no necessary correspondence. It must be concluded, then, that even this thesis fails at the very basics of scientific conduct: the aims are vague, methodology incoherent, and the type of evidence is unsuitable.

Urheilujournalismi Etelä-Pohjanmaan maakunnan imagon rakentajana. Esimerkkinä sanomalehdet Ilkka ja Pohjalainen. (*Sports journalism as the image builder of Southern Ostrobothnia. Case newspaper examples: Ilkka and Pohjalainen*) The confusion starts immediately in the abstract part of the thesis where the author states that it is his aim “to examine the relationship between sports journalism and Southern Ostrobothnia’s image” (ibid.: 3). He continues that what makes his thesis peculiar is “a mismatch between the title and his closing remarks”; “that sports journalism is not so much an image builder as it is more of a ritualistic” process which “produces, maintains, and changes reality through communication”; “it is then the reproduction of Southern

Ostrobothnia's collective identity." (ibid.) But on the sixth page, fifth paragraph (a sort of a "in a nutshell" part), the author says that the media builds (constructs) both identities and images.

What is this supposed to mean? Is this the author's "updated" or preliminary belief which then gets transformed in the end? The author basically managed to state a contradiction as the aim: that both X and not X. Furthermore, it is simply false, or illogical, to claim that (any kind of) journalism constructs identities but not images. Although these terms may somewhat differ in meaning, there is nonetheless a strong bond and similarity between them, a dependency. Based on the seventh page, it is not clear if the author thinks of the media's role in image construction as "one among many" or if it is supposed to be the most important. He also speaks, on the one hand, of sports journalism and, on the other hand, of the particular newspapers as totalities, covering more than just sports journalism. Based on this, it is difficult to conclude in what kind of journalism is he interested. Is it sports journalism in particular or journalism in general. Lastly, to say that sports journalism constructs reality through communication is, first of all, a constructivist claim. Such a claim is precisely the kind of example that I have been criticising throughout these pages as its basis lies in impossibilities. Anyway, the analysed thesis contains a contradiction even on that level because the claim of constructing and reproducing an identity – a collective one at that – are two different things: construction can contain reproduction but the opposite relation is more difficult.

As to the nature of the thesis, the author mentions that it is theoretical. He continues that the work includes "illustrative empirical ingredients which are meant as additional elucidation. (ibid.: 8.) Interestingly, the author writes that his "aim is not to create new knowledge ... but enrichment of already existing research" (ibid.). The million dollar question is, how research is enriched if not by coming up with new knowledge. (I assume here that the author is not talking about an elaborate deductive inference which is principally nothing more than a certain way of connecting already known things. Of course, even though the inference "only" combines the already known, the result can still be de facto new.) Nor is it mentioned how the work is theoretical – is it, for instance, a literary review type or, perhaps, developmental. The author states that the theoretical framework is based, among others, on James W. Carey's views of communication as a ritual which focuses instead of image construction on reproduction and reflection of collective identity (ibid.). Again, what is the author after; is it construction, reproduction, or both?

As to the explanation of the empirical data (articles) – which, by the way, changes the type of the work – well, the same style continues. In addition to omitting the explanation of the selection process, there is only one thing that can be conceived of as being methodological. On page 9, the author writes that what the articles say is not that important, rather, it is the way things are said. This "how"-part refers to such things as on what page is the article (or story), how the articles are laid out, etc. It seems quite odd that the actual contents (of what is said or written) do not play an important role in identity or image formation. For instance, the author says that "according to Moring, Southern Ostrobothnian identity consists of patriotism, the will to defend, entrepreneurship, and a lifestyle that emerges from rurality..." (ibid.: 45). But if form is more important than the content, how is it possible to arrive at the statement quoted by the author; surely not on visual layout alone. The claim of form over content, at least in this case, remains unsupported.

But perhaps the most important thing is what the author has completely left out. Namely, that the work, in reality, tries to establish causality. This is evident from the title and aims. If sports journalism or the newspapers as a whole are thought of as constructing identity or image, then we have a basic causal relation where X (journalism or newspapers) cause Y (the effect, in this case identity or image). According to Töttö (2005: 94), establishing causality demands two things: firstly, it requires theoretical guesswork about what X could cause Y and, secondly, empirical evidence that X really does cause Y. It should be added that, at least sometimes, a "guess" about X implies a "guess" about mechanism. There is plenty of material about how media does or can affect perception; this applies to propaganda, perception of self and others, etc. However, the author has not chosen this type of literature, choosing instead a random and unrelated mix of concepts. Another possibility is that the author thinks of media as "one of many" which affects identity and image. But be as it may, even these possibilities require empirical proof, one that is more specific than simply saying "it does affect" or that "everything affects everything". This other option of a combined causality has not been pursued by the author.

One of the necessary conditions of causal relation is temporal succession; such that X precedes Y. This is particularly important, for the difference can mean a 180 degree change of the research question. In this thesis the hypothesis is X (media) and Y (society's identity or image). Again, there is a plethora of literature which points out that media do not operate in a societal vacuum; that the media is affected by many societal aspects which can be political (for instance, the domination and control of the media by a political party) or financial (i.e., the effect of money on the media, the content and also "financial" censorship), or perhaps, if there is peace or war, and so on. So now we have the situation where X (media as part of society) and Y (identity or image as something societal). If the temporal succession is in reality reversed then the situation becomes one where the media do not create images nor identities but where the media "merely" reflects those aspects which are already present in the surrounding society. The thesis contains hints and clues of this scattered around here and there but they are not developed further; such things as having a certain different mentality (or a perception of different mentality) of the region already existing before the case example newspapers (Ilkka and Pohjalainen) were even established or the possible effect of catering for the popular demand (pp.47-48, 39-40), i.e., the financial gain.

These mistakes – and certainly not limited to these – show that two principal mistakes were made which affected the whole thesis. Firstly, the initial confusion about, well, pretty much everything. Secondly, this was followed by a very strong claim of causality, i.e., media constructing a collective image or identity (or both, difficult to say) without any actual causal analysis. The failure – if we are allowed to talk about such a thing – is of the most basic kind: the logic of science is successfully absent.

4.4 Remarks and results

It can be said, then, that methodologically six out of the seven analysed theses failed. And not at some higher, nuanced level, but at the very basics. It is impossible to commit even more rudimentary mistakes. Actually, no, it is possible to make worse mistakes. A thesis can be so incoherent that whether aims and evidence match becomes irrelevant. The level of incoherence in some of the analysed theses was certainly thought-provoking.

Now to some interesting results. Based on the sample, there seems to be no correlation between the grade of a thesis and the level of command of the logic of science, as the better graded theses were equally at a loss compared with the lower graded theses. The fact that one better graded thesis "passed" is no indication of correlation as, firstly, it is only one case of the total sample and, secondly, other higher graded theses do not share this quality. In addition, considering that it is only one thesis, it doesn't really matter that much if it was a better or worse graded thesis, as this singular instance could be explained by accidental reasons; luck, for instance. Based on the methodological content alone (of the "better" thesis) there is very little reason to think that all the choices, etc., were made with deliberation and in a methodologically informed manner, as the methodological content of that particular thesis was extremely lean. Furthermore, a theoretical work in the literary review sense is considerably less prone to mistakes than a more complex empirical/statistical research. Thus a relatively error-free work is more a result of the nature of the work itself rather than due to the excellent methodological capabilities of the author. This is not an insult, nor am I belittling the author's work, it is simply a matter of fact. (We can find a similar situation in a morally correct action: we don't particularly value a person's action if that person didn't have any other choice but to do the "right" thing.)

On the other hand, at least two (first and fifth, graded mcl and ecl) of the higher graded theses showed higher level of consistency and in a certain sense even coherence than the rest. And although I am generally unhappy about the other two higher graded theses (second and fourth, also graded mcl and ecl), regarding consistency and coherence, these were better than the remaining three lower graded theses. Whether a thesis is graded as mcl or ecl seems to make no difference as both are in this regard equal. Thirdly, and although this hasn't really been discussed, the higher graded theses showed a higher level of development of their ideas whereas the lower graded theses seemed more "rushed". So, all and all, there are some things that seem to correlate with the grade although, and unfortunately, the most important part, doing and understanding science, does not.

If we compare the theses, science wise, with both media studies and human sciences in general, we find strong similarities; that is, if we limit them both to the postmodern/hermeneutic/constuctivist/etc. part. Such a limitation doesn't necessarily change much the overall nature of these fields. For instance, the methodology of human sciences was once dominated by positivism which gave way to the so called alternatives (whether new or old). And even if the trend changed once again, or if the trend is changing already, the fact remains that right now positivism (or traditional science) is not fashionable. Likewise, in media studies there are certainly examples of rational research, again, one that is rests on traditional premises. However, what seems to be fashionable now is the so-called linguistic shift which is nothing else than a combination of the "alternatives" found in human science in general. We have already noticed that although these "alternatives" can be verbose, there is actually very little content. Hence, there is talk of methodology without there being any proper methods; jargonistic "discourse" that is either meaningless or a repetition of truisims and things already known, contradictory and confusing statements, and so on. With such methodology and/or science the analysed theses (except for one) certainly correspond.

The main reasons, then, that speak against the possibility that the thesis being what they are is just accidental: 1) they match the human sciences (in their present state) in being non-scientific. It would seem strange (though not impossible) and commendable, if the university of Vaasa was somehow able to resist following fashion trends and instead, concentrate on doing science. That not only is the educational policy not resisting the fashion trends but actually actively supporting it, is exemplified by the fact that a student of the humanities is not required to take any course related to statistics. Also, other courses (number, content, etc.) in methodology are cut to an absolute minimum. It seems strange that in an institution supposedly devoted to science, it actually seems to be the lowest priority issue; 2) the sample should be particularly representative of the higher graded theses part of the population, especially when the majority of that part was selected. Furthermore, although the situation is more difficult with smaller population, the proportion of seven theses is, after all, 22,6 per cent of the total – quite high then; 3) the probability of the event that, for any reason, only the sampled theses just happened to be – science wise – what they are (except for the one thesis), i.e., that the rest of the population is "scientifically adequate", is extremely low. (It is: (6*5*4*3*2*1*25) / (31*30*29*28*27*26*25) = 10^{-6} * 1,36 = 0,00000136) Represented numerically, even more probable events will look unlikely. For instance, the probability of an event where the six out of seven theses are unique but are not selected is: (25*24*23*22*21*20*19) / (31*30*29*28*27*26*25) = 0,18. On a scale of where 1 = happens with certainty and 0 = does not happen with certainty, even the latter result looks like somewhat unlikely. But the main point here is the fact that the latter result is orders of magnitude higher than the previous. (A difference of five orders of magnitude is certainly large.); 4) the theses were not analysed semiotically, i.e., no attempt to expose "hidden meanings" was made. Whatever methodological argumentation, or the lack of, the students made is contained in those theses as an empirical fact and not as a projection of the analyst. In other words, anyone who might be interested can go to the university library and verify the results for him or herself.

It seems, then, that my analysis of the theses may be a bit harsh – as only one of the seven got a favourable review. However, I do think that the critique is justified. First of all, no student should be expected to perform as a full-fledged researcher – I certainly do not expect that. On the other hand, it is fair to expect that a student will have at least a rudimentary understanding of science. After all, five years (on average) of university education – supposedly scientific – should amount to something more than random list of references.

5 CONCLUSION

We may no return to the beginning and ask: “Is the field of media studies in trouble?” The question, however, leads automatically to ask the same thing about human sciences in general and this ultimately leads one to speculate whether the whole university institution is up to task. (These things are interconnected: it is impossible to treat media studies as an isolated case.) There are few basic positions which depend on how we would like to understand science. They do, however, lead to difficulties.

According to the first option, science is to be understood in the postmodern/constructivist/etc. sense. The “all discourses are equal” point of view is decisive here. According to this stance, then, the “white male science” has no supremacy in saying and/or defining what is science or what it ought to be. Considering that not only “all discourses are equal” but also, “equally valid”, every activity which is claimed to be scientific ought to be understood as, well, scientific. But this cannot be morally/politically right as claims of being scientific are privileged while other claims would end up being discriminated. Such is not the way of all-encompassing postmodern equality and, thus, every claim/statement/discourse/etc. should be granted the right of university presence and, most importantly, tenures and public financing. Of course, in such relativistic system the word science might be dropped altogether since it would be completely meaningless: a word that refers to everything cannot refer to anything. University would become just a roof under which there would be any kind of activity that can be found anywhere else. Partly this is already going on as traditional subjects are either substituted with or new additions made by more and more meaningless courses, or even full programs. In such a bazaar of fair-attractions, university must eventually lose its justification.

Second option seems initially as less relativistic. This is, to a large extent, the situation of the present. According to it there is a basic dichotomy between human sciences and natural sciences. Although this solution might keep cooking classes out of the university – for the time being that is – such a dichotomy, however, would still be problematic as the word science would refer to two completely different kind of activities.

Furthermore, such a dichotomy cannot demarcate reality and its effects into two non-overlapping categories. As is the case now, human scientists do not and obviously cannot keep their claims and statements from "infringing" the realm of the natural sciences. The claims of, say, literary criticism contain both logical and empirical components which open doors for their analysis – and possible dissatisfaction about them. If natural scientists made statements that contained components of literary criticism, literary critics could justifiably analyse and possibly criticise them. However, the human sciences have continuously and unsuccessfully attacked the matters of fact and logic. Considering that they have been shown wrong time and time again, and that despite all of this, they continue to be established as "sciences", one certainly wonders when will the natural sciences finally say that enough is enough and "resign" the university institution. In a setting where nonsense is academically rewarded, it is difficult and unmotivating to carry out serious research. The second problem concerns the nature of the dichotomy itself, or rather, the approach of the human sciences. Considering that "its way of doing things" lacks any clarity and concreteness and that it basically attacks many of the sensible – and so far the only functional – parts of science, it has nothing with which to prevent a slide to complete relativity. This dichotomy, then, is nothing else than a precursor to the first point. Under this and the previous points media studies (and even the human sciences) would be perfectly fine as sciences. In other words, when science and high-level research are achieved as a matter of simple announcement, even knitting would qualify.

According the third option – which is also the position of this thesis – we take the word science a little bit more seriously, that is, we mean by it what is and has generally been meant the word "science". In this sense, for instance, such philosophies and approaches as positivism, empiricism, etc., would continue to be valid. Not because there is a complete and perfect philosophical proof justifying them, but because practice has shown them to be "more or less" correct. Certainly the merits of scientific results speak in favour of these concepts as opposed to their so-called alternatives. If we, then, want to hold on to what is generally meant by science and demand that universities ought to be places where the highest level of science is being taught and produced, those fields that either will not or can not meet the requirements would sooner or later be forced out of the university. Such an event would not in any way prevent people from practicing and participating in those fields. They simply would be practiced elsewhere, as non-sciences – like astrology, for example. It goes without saying, then, that if we want to hold on to the concept of science, media studies (and human sciences) in its/their current fashion-form are in direct contradiction and as such cannot be accepted as sufficiently meeting scientific criteria. (Again, this is not to say that they cannot be scientific, it's just much of it currently isn't.)

These factors – which can be thought of as external – can be extended by the eternal problem of what is the role of a university: is it supposed to provide with practical skills or scientific knowledge. In this thesis, we have already evaluated media studies (and human sciences) from the science point of view and have concluded that the science part is severely lacking. But what about practical skills? Quite frankly, I cannot think of any skill that could be achieved by devoting years to studying media (or other human sciences) – unless we include, say, statistics and statistical analysis, which has its concrete practical usages but, unfortunately, is not being taught. Maybe the practical part lies in the ability to produce totally obscurantist texts. Whether this is a valuable skill or not, there seems to be a high level of success as each new generation of scholars is able to produce fantastically meaningless studies. However, if we do not count this dubious ability, the human sciences in general rest on extremely weak justification indeed, as they lack both scientific and practical foundations. Even categorisation as art does not really fit as most forms of art require concrete skills: a musician must know how to use an instrument, how to read music notation, grasp of music theory; a painter must, unsurprisingly, know how to actually paint, and so on. From this point of view, then, media studies (and human sciences) are in a sort of limbo; not science and neither practical ability (or both).

But – continuing on the external factors – perhaps justification is to be found completely somewhere else. The many examples mentioned in this thesis showed strong political/ideological leanings. Even if we disregard such over-the-top cases like feminism and postmodernism, there are even more "reputable" names in human sciences that seem to defend this view. For instance, Giddens (1982: 166) wrote that "as critical theory, sociology ... poses the questions: what types of social change are feasible and desirable, and how should we strive to achieve them?" (The same question is fitting even from the media studies point of view as much of it is really just "media oriented" sociology.) Is this the agreeable reason for existence, as a science that is? Or, to ask in a more Weberian manner, do value-judgements belong to science, and not as a mere side-effect but as the main legitimator? If the answer is yes, can the separation between the sciences themselves and/or any other politically motivated activity be maintained? If social (or political) change becomes the driving force then not only every scientific field must, and indeed will, have equal say in such matters but so will everyone else as long as they make an "ought-to-be" claim. Such science, however, must decide between the preference for factuality or values. These are necessarily exclusive as, for instance, there is nothing factual about a desirable social change. Feminism is a perfect example of what happens when facts are sacrificed in favour of value-judgements. This leads, almost invariably, to a situation where it is not facts that are used to give support to value-judgements but, rather, a mixture of other value-judgements, half-truths, and outright lies. In a case of conflict – which is a guaranteed situation as there is no universal view on what is "right" – there are two ways where this could lead: 1) open and violent conflict resolution where it is the "biggest guns" and not any of the competing values themselves which will ultimately win; 2) or, in a more "civilised" situation, people "agree to disagree" which leads us back to square one, namely, to a relativist position. As a last point, value-judgements need to be authoritative to be taken seriously; by what higher right sociology or media studies (human sciences in general) could appropriate such authority for themselves? Needless to say, then, politics cannot legitimise a scientific field, nor to correct any mistakes carried out in that field. Facts and values are two entirely heterogeneous problems: scientists deal with the former while prophets and demagogues with the latter. A field that chooses the latter over the former will necessarily be in trouble.

If analysed internally, it is difficult to arrive at different conclusions than as compared to the external factors. That is, even if we ignore the effects of the university institution, other fields, ideologically motivated science, etc., we realise that exactly the same issues would be found internally. For instance, the questions – raised by the representatives of, in this case, media studies – of what the field ought to be have no other choice but being exactly the same questions as put forward by, say, natural scientists. Possible demands of being more scientific will correspond to what is usually understood by science. Defending relativistic positions would put the field in the same situation as according to the first and second points already mentioned above. No solution there then. The only solution would be to, on the one hand, redefine science, so that the activity has no resemblance with what is presently meant by science and, on the second hand, give it a reasonably clear definition so that only a particular kind of activity would pass the requirements. Of course, such a solution would not necessarily make much sense, as the field would have to meet and solve all the external factors which would, again, return the whole issue back to square one. To avoid this, the field would have to be totally self-contained and independent. Although theoretically possible situation, it is, nonetheless, extremely unlikely. But considering that no such internal solution is in the works, the matters will, for a foreseeable future, remain somewhat chaotic; certainly far from being just perfect.

Considering the undoubtedly strong connection between a field's formation and how it is being taught, some brief remarks should be directed at education, especially the role of a thesis. First of all, the analysed theses are not an exception; not as representatives of media studies, nor human sciences as such. This is not a premature claim considering, for example, in what kind of state the methodology of human sciences presently is. Of course, to get a more precise idea, theses – master's as well as higher level – from different human sciences should be analysed. This is definitely an area where additional research is certainly called for.

Anyway, the defence of the theses – and not only the analysed ones but also any other that are similarly unscientific – could be conceived of as resting on two possibilities: 1) we redefine or obscure science so that it would either mean something completely else or nothing at all. However, as noted above, this solution is untenable as it introduces a whole range of destructive consequences – destructive for the argument(s) that is. We can, then, skip over this one; 2) we proclaim that it is not the function of a master's thesis – or higher education as such – to be scientific but, rather, it should fulfil some, well, other functions. In the following I will consider some of the possibilities – it also should be kept in mind that these possibilities are to a certain extent interconnected.

First of all, it could be said that the purpose of a thesis is to show that its writer is capable of producing a bit longer text. If this would be the function of master's level then at what point are the students to learn counting; during post-graduate level? With such a speedy progression researchers would die of old age before they could even proceed to such advanced concepts as, say, causality. It is certainly possible that lower levels of education have failed in their task and that students who enrol in higher education cannot write (or count) sufficiently. In this case the university has no other option than to take over some or even all responsibilities of the lower level. But this is a result of a broken system. It is hardly the ideal state of affairs. Secondly, it can be claimed that the purpose of a thesis is to show that its writer knows the basic issues of the field. But what about exams then? Isn't it also their purpose to test the exactly same things? If the exams (or an essay, etc.) and the thesis are supposed to do the same thing then one or the other is redundant - and thus an unnecessary expenditure.

The above can lead to certain sub problems. An emphasis on "knowing the field" leads to a repetition and, at best, produces texts where the students show that they have read – or at least successfully copied it from earlier theses – the material mentioned in the list of references. However, shouldn't a thesis epitomise scientific thinking rather than the mere fact that something has been read? While reading is important, it is the thinking part that pushes science forward. It is, therefore, the faculty of the mind that should be cultivated and tested. Furthermore, the requirement of knowing the field would not only produce repetitive texts, it actually would lead to more or less uniform texts. If we take, for instance, physics – which is composed of such branches as mechanics, thermodynamics, electromagnetism, etc. – the result would be that each and every thesis contained exactly the same things; the potential only difference being how the branches were ordered. Such a requirement would make research questions obsolete as these, firstly, narrow down the scope of the field and, secondly, possibly could lead to new ideas, hints, and paths which are located in a "scientific no-man's land", for example, situated between two scientific fields. But such a thing would be impossible if we set up "knowing the field" as the requirement. Such a demand doesn't lead to innovation, it actively stifles it. How is a field supposed to evolve when that evolution is discouraged during the most critical phase and by what logic are we to think that if during the whole master's level period a student does what he is being told, suddenly afterwards will his mental capabilities burst into bloom. In this case higher education would be, in fact, an obstacle, not help.

Lastly, it should be mentioned, it can be claimed that by writing a thesis the student should learn how to write "scientifically", to produce a sort of "scientific prose". This is partly related to the requirement of "being able to write in the first place" but mostly it is a nonsensical requirement: it is wholly subjective and it cannot, in any way, increase the validity and/or reliability of theoretical and empirical results. Science deals with factuality, poetry with style. It is either deceitful or simply a matter of confusion to claim to do science while the emphasis is totally on non-scientific issues.

There is really no way out of this: either we aim at science or at something else. Unfortunately the whole chain of media studies, human sciences, and higher education point to the something else part. This "something else" can be, and it truly is, anything; ranging from full-blown obscurantism, ideological propaganda, to efforts of producing at the "science factories" poets rather than scientists. At the very least one would hope that the representatives of the whole axis were honest enough to admit that they are doing and want to do something else than science. But, alas, the show continues on. So, to reiterate again, if science is the aim then the field of media studies in particular and the human sciences in general are in a mess. If, on the other hand, science is not the aim then everything is potentially fine, as long as the field(s) correspond with that other aim. However, considering that the alternative aim – if there is one in the first place – is so far unclear, there can be no correspondence in which case the result is also a mess. It is safe to say, then, that for the foreseeable future things will keep on going as they have gone so far until the system will grind to a halt and a "new" paradigm is established.

REFERENCES

Alkula, Tapani, Seppo Pöntinen & Pekka Ylöstalo (1994). *Sosiaalitutkimuksen kvantitatiiviset menetelmät*. Helsinki: WSOY.

Andersen, Robin (2006). *A Century of Media, a Century of War*. New York: Peter Lang Publishing.

Ang, Ien (1996). *Living Room Wars: Rethinking media audiences for a postmodern world*. London: Routledge.

Anttila, Anu-Hanna (2005) Yleistettävyyden ongelmat historiallisen sosiologian tutkimuksessa In: *Tutkimus menetelmien pyörteissä* (eds. Pekka Räsänen, Anu-Hanna Anttila & Harri Melin). pp. 201–218. Jyväskylä: PS-Kustannus.

Barrat, David (1986). *Media Sociology*. London: Routledge.

Bell, Allan (1998) The Discourse Structure of News Stories In: *Approaches to Media Discourse* (eds. Allan Bell & Peter Garrett) pp. 64–104. Oxford: Blackwell.

Bell, Allan & Peter Garrett (1998) Media and Discourse: A Critical Overview In: *Approaches to Media Discourse* (eds. Allan Bell & Peter Garrett) pp. 1–20. Oxford: Blackwell.

Beveridge, W. I. B. (1980). *Seeds of Discovery: A sequel to the art of scientific investigation*. London: Heinemann Educational Books.

Bignell, Jonathan (2000). *Postmodern Media Culture*. Edinburgh: Edinburgh University Press.

Bonjour, Laurence (2002). Epistemology. Oxford: Rowman & Littlefield Publishers.

Brubacher, John S. (1977). *On the Philosophy of Higher Education*. San Francisco: Jossey-Bass.

Chalmers, A.F. (1985). *What is this thing called Science?*. Milton Keynes: Open University Press.

Chomsky, Noam (1976). *Tiedon ja vapauden ongelmia*. (*Problems of Knowledge and Freedom, The Russell Lectures*; *American Power and the New Mandarins*; *For Reasons of State*, transl. Paavo Löppönen, Jukka Tuominen, Markku Mäkelä). Helsinki: Otava.

Chomsky, Noam (1991). *Deterring Democracy*. London: Verso.

Chomsky, Noam (2003). *Chomsky on Democracy and Education* (ed. C. P. Otero). London: Routledge.

Corner, John (1998). *Studying Media: Problems of Theory and Method*. Edinburgh: Edinburgh University Press.

Curran, James (2002). *Media and Power*. London: Routledge.

Danesi, Marcel (2002). *Understanding Media Semiotics*. London: Arnold.

Dellve, Lotta, Abrahamsson Henning, Kajsa, Trulsson, Ulrika & Lillemor R-M Hallberg (2002) Grounded theory in public health research. In: *Qualitative Methods in Public Health Research – Theoretical Foundations and Pracitcal Examples* (eds. Lillemor R-M Hallberg) pp. 137–173. Lund: Studentlitteratur.

Dey, Ian (1993). *Qualitative data analysis: a user-friendly guide*. London: Routledge.

Eskola, Antti (1966). *Sosiologian tutkimusmenetelmät II*. Helsinki: WSOY.

Eskola, Antti (1981, 4th ed.). *Sosiologian tutkimusmenetelmät I*. Helsinki: WSOY.

Eskola, Jari (2001) Laadullisen tutkimuksen juhannustaiat. In: *Ikkunoita tutkimusmetodeihin II* (eds. Juhani Aaltola & Raine Valli) pp. 133–157. Jyväskylä: PS-kustannus.

Eskola, Jari & Juha Suoranta (1998). *Johdatus laadulliseen tutkimukseen*. Tampere: Vastapaino.

Fairclough, Norman (1998) Political Discourse in the Media: An Analytical Framework In: *Approaches to Media Discourse* (eds. Allan Bell & Peter Garrett) pp. 142–162. Oxford: Blackwell.

Fearn, Nicholas (2001). *Zeno and the Tortoise*. London: Atlantic Books.

Feyerabend, Paul (1975). *Against Method*. London: Verso.

Feyerabend, Paul (1987). *Farewell to Reason*. London: Verso.

Feyerabend, Paul (1992) Atoms and Consciousness. In: *Common Knowledge* I, no. I, pp. 28–32. Durham: Duke University Press.

Frey, Lawrence R., Carl H. Botan & Gary L. Kreps (2000). *Investigating Communication: An Introduction to Research Methods. Boston*: Allyn and Bacon.

George, Alexander & Andrew Bennett (2005). *Case Studies and Theory Development In the Social Sciences*. Cambridge: MIT Press.

Giddens, Anthony (1982). *Sociology: A brief but critical introduction*. London: Macmillan.

Glaser, Barney G. & Anselm L. Strauss (1967). The Discovery of Grounded Theory: Strategies for Qualitative Research. New York: Aldine de Gryuter.

Gross, Paul R. & Norman Levitt (1994). *Higher Superstition – The Academic Left and Its Quarrels with Science*. Baltimore: The Johns Hopkins University Press.

Habermas, Jürgen (1984). *The Theory of Communicative Action, Volume 1: Reason and the Rationalization of Society*. Boston: Beacon Press.

Habermas, Jürgen (1989). *The Theory of Communicative Action, Volume 2, Lifeworld and Systems: A Critique of Functionalist Reason*. Boston: Beacon Press.

Habermas, Jürgen (2004). *Julkisuuden rakennemuutos: tutkimus yhdestä kansalaisyhteiskunnan kategoriasta*. (*Strukturwandel der Öffentlichkeit*, transl. Veikko Pietilä). Tampere: Vastapaino.

Hansen, Anders (1998) Content Analysis In: *Mass Communication Research Methods*. pp. 91–129. Basingstoke: Palgrave.

Harding, Sandra (1987) Introduction – Is There a Feminist Method? In: *Feminism and Methodololgy* (ed. Sandra Harding) pp. 1–14. Bloomington: Indiana University Press.

Hartsock, Nancy C. M. (1987) The Feminist Standpoint: Developing the Ground for Specifically Feminist Historical Materialism In: *Feminism and Methodololgy* (ed. Sandra Harding) pp. 157–180. Bloomington: Indiana University Press.

Harré, Rom (1972). *The Philosophies of Science – An Introductory Survey*. Oxford: Oxford University Press.

Heikkinen, Hannu L. T. (2001) Todellisuus kertomuksena. In: *Ikkunoita tutkimusmetodeihin II* (eds. Juhani Aaltola & Raine Valli) pp. 116–132. Jyväskylä: PS-kustannus.

Hemánus, Pertti & Ilkka Tervonen (1980). *Objektiivinen joukkotiedotus*. Helsinki: Otava.

Hempel, Carl G. (1965). *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*. New York: Free Press.

Hesse-Biber Nagy, Sharlene & Patricia Leavy (2004) Distinguishing qualitative research In: *Approaches to Qualitative Research: A Reader on Theory and Practice* (eds. Sharlene Nagy Hesse-Biber & Patricia Leavy) pp. 1–15. New York: Oxford University Press.

Hintikka, Jaakko (2002) Looginen empirismi kuusi vuosikymmentä myöhemmin. In: *Wienin piiri* (eds. Ilkka Niiniluoto & Heikki J. Koskinen) pp. 250–260. Helsinki: Gaudeamus.

Hjarvard, Stig (2002) The study of international news In: *A Handbook of Media and Communication Research: Qualitative and Quantitative Methodologies* (ed. Klaus Bruhn Jensen) pp. 91–97. London: Routledge.

Holloway, Immy (1997). *Basic Concepts for Qualitative Research*. Oxford: Blackwell Science.

Hume, David (1955). *Enquiries: concerning human understanding and concerning the principles of morals*. Oxford: Clarendon Press.

Husa, Jaakko (1997). *Lainopin Maailmanviivat: Lainoppi, dualismit ja metodologia*. Julkisoikeuden tutkimuksia ja kurssikirjoja. Joensuu: Joensuun yliopisto.

Jameson, Fredric (1991). *Postmodernism: Or, the Cultural Logic of Late Capitalism*. London: Verso.

Jensen, Klaus Bruhn (2002) The humanities in media and communication research. In: *A Handbook of Media and Communication Research: Qualitative and Quantitative Methodologies* (ed. Klaus Bruhn Jensen) pp. 15–39. London: Routledge.

Kakkuri-Knuuttila, Marja-Liisa & Kaisa Heinlahti (2006). *Mitä on tutkimus? Argumentaatio ja tieteenfilosofia*. Helsinki: Gaudeamus.

Karlberg, Ingvar, Hllberg, Lillemor R-M & Anneli Sarvimäki (2002) Introduction and aims of the book – Health, Public Health and Research on Public Health. In: *Qualitative Methods in Public Health Research – Theoretical Foundations and Pracitcal Examples* (eds. Lillemor R-M Hallberg) pp. 13–38. Lund: Studentlitteratur.

Kiviniemi, Kari (2001) Laadullinen tutkimus prosessina. In: *Ikkunoita tutkimusmetodeihin II* (eds. Juhani Aaltola & Raine Valli) pp. 68-84. Jyväskylä: PS-kustannus.

Knightley, Phillip (2003). The First Casualty: The War Correspondent as Hero and Myth-Maker from the Crimea to Kosovo. London: André Deutsch.

Kuhn, Thomas S. (1996, 3rd ed.). *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Kusch, Martin (1986). *Ymmärtämisen haaste*. Oulu: Pohjoinen.

Lakatos, Imre (1980). *The methodology of scientific programmes*. (eds. John Worrall & Gregory Currie). Cambridge: Cambridge University Press.

Latour, Bruno & Steve Woolgar (1986). *Laboratory Life: The Construction of Scientific Facts*. Princeton: Princeton University Press.

Latour, Bruno (2006). *Emme ole koskaan olleet moderneja*. (*Nous n'avons jamais été-modernes: Essai d'anthropologie symétrique*, transl. Risto Suikkanen). Tampere: Vastapaino.

Lammenranta, Markus (1993). *Tietoteoria*. Helsinki: Gaudeamus.

Lehto, Anna-Maija (1998) Laatua surveytutkimukseen. In: *Faktajuttu: Tilastollisen sosaalitutkimuksen käytännöt* (eds. Seppo Paananen, Anneli Juntto & Hannele Sauli) pp. 207–232. Tampere: Vastapaino.

Levitt, Norman (1999). *Prometheus Bedeviled: Science and the Contradictions of Contemporary Culture*. New Brunswick: Rutgers University Press.

Lippmann, Walter (1965/1922). *Public Opinion*. New York: The Free Press.

Lippmann, Walter (2008/1920). *Liberty and the News*. Princeton: Princeton University Press.

Lyotard, Jean-François (1985). *Tieto postmodernissa yhteiskunnassa* (*La condition postmoderne*, transl. Leevi Lehto). Helsinki: Gummerus.

Madge, John (1953). *The Tools of Social Science*. London: Longmans, Green & Co.

McLuhan, Marshall (1994). *Understanding Media: The Extensions of Man*. Cambridge: MIT Press.

McManus, John H. (1994). *Market Driven Journalism: Let the Citizen Beware?*. Thousand Oaks: Sage Publications.

Medawar, Peter (1984). *The Limits of Science*. Oxford: Oxford University Press.

Mill, John Stuart (1973). *Auguste Comte and Positivism*. Ann Arbor: University of Michigan Press.

Neurath, Otto (1973) Empiricism and Sociology. In: *Otto Neurath: Empiricism and Sociology* (eds. M. Neurath & R. S. Cohen) pp. 319–421. Dordrecht: Reidel.

Neurath, Otto (1997). Protokollalauseet ("Protokollsätze". *Erkenntnis* 3 (1932/33), 204-214, transl. Risto Viikko) In: *Ajattelu, kieli, merkitys: analyyttisen filosofian avainkirjoituksia* (ed. Panu Raatikainen) pp. 95–103. Helsinki: Gaudeamus.

Niemelä, Jussi K. & Osmo Tammisalo (2006). *Keisarinnan uudet (v)aatteet – Naistutkimus luonnontieteen näkökulmasta*. Helsinki: Terra Cognita.

Nordenstreng, Kaarle (1975). *Tiedotusoppi – Johdatus yhteiskunnallisten viestintäprosessien tutkimukseen*. Helsinki: Otava.

Pietilä, Veikko (1982). *Tiedotustutkimus: teitä ja tienviittoja*. Julkaisuja / Tampereen yliopisto. Tiedotusopin laitos. Sarja C. Tampere: Tampereen yliopisto.

Pietilä, Veikko (1996). *TV-uutisista, hyvää iltaa: merkityksen ulottuvuudet televisiouutisjutuissa*. Tampere: Vastapaino.

Pietilä, Veikko (1997). Joukkoviestintätutkimuksen valtateillä. Tampere: Vastapaino.

Popper, Karl R. (1966). *The poverty of Historicism*. London: Routledge.

Popper, Karl R. (1982). *The Open Universe: An Argument for Indeterminism*

Popper, Karl R. (1994) *The Myth of the Framework: In defence of science and rationality*, (ed. M.A. Notturno). London: Routledge.

Raatikainen, Panu (2004). *Ihmistieteet ja filosofia*. Helsinki: Gaudeamus.

Rakitov, Anatoli (1978). *Tieteellisen tiedon rakenne*. (*Anatomija naučnogo znanija*, transl. Robert Kolomainen). Moskova: Kustannusliike edistys.

Riesman, David, Nathan Glazer & Reuel Denney (1961). *Lonely crowd: A Study of the Changing American Character*. New Haven: Yale university Press.

Roberts, Helen (1981) Women and their doctors: power and powerlessness in the research process. In: *Doing Feminist Research* (ed. Helen Roberts) pp. 7–29. London: Routledge.

Robson, Sue & Angela Foster (1989). *Qualitative Research in Action*. London: Edward Arnold.

Rolin, Kristina (2006) Näyttö naistutkimuksen epätieteellisyydestä jäi esittämättä. *Tieteessä tapahtuu*, 8/2006, pp. 57–61. Helsinki: Tieteellisten seurain valtuuskunta.

Ronkainen, Suvi (1998). Kaikuva empiirisyys – surveyn epistemologiset mahdollisuudet. In: *Faktajuttu: Tilastollisen sosaalitutkimuksen käytännöt* (eds. Seppo Paananen, Anneli Juntto & Hannele Sauli) pp. 233–255. Tampere: Vastapaino.

Routila, Lauri (1986). *Miten teen tiedettä taiteesta*. Keuruu: Clarion.

Ruelle, David (1991). *Sattuma ja kaaos*. (*Chance and Chaos*, transl. Kimmo Pietiläinen). Helsinki: Gummerus

Rush, Ramona R. & Autumn Grubb-Swetnam (1996) Feminist Approaches to Communication. In: *An Integrated Approach to Communication Theory and Research* (eds. Michael B. Salwen & Don W. Stacks) pp. 497–518. New Jersey: Lawrence Erlbaum Associates.

Schlick, Moritz (1997) Positivismi ja realismi ("Positivismus und Realismus", *Erkenntnis* 3 (1932/33), 1-31, transl. Risto Viikko) In: *Ajattelu, kieli, merkitys: Analyyttisen filosofian avainkirjoituksia* (ed. Panu Raatikainen) pp. 70–94. Tampere: Vastapaino.

Schrøder, Kim Christian (2002) Discourse of fact In: *A Handbook of Media and Communication Research: Qualitative and Quantitative Methodologies* (ed. Klaus Bruhn Jensen) pp. 98–116. London: Routledge.

The Vienna Circle (1973) The Scientific Conception of the World: The Vienna Circle. In: *Otto Neurath: Empiricism and Sociology* (eds. M. Neurath & R. S. Cohen) pp. 301–318. Dordrecht: Reidel.

Toivonen, Timo (1999). *Empiirinen sosiaalitutkimus: Filosofia ja metodologia*. Helsinki: WSOY.

Turner, Jonathan H. (1989). *Herbert Spencer: A renewed Appreciation*. Newbury Park: Sage.

Töttö, Pertti (1996) Aguste Comte (1798-1857): Positivismin isä. In: *Sosiologian klassikot* (eds. Jukka Gronow, Arto Noro & Pertti Töttö) pp. 61–88. Helsinki: Gaudeamus.

Töttö, Pertti (1997). *Pirullinen positivismi: kysymyksiä laadulliselle tutkimukselle*. Jyväskylä: Kampus kustannus.

Töttö, Pertti (2000). *Pirullisen positivismin paluu: laadullisen ja määrällisen tarkastelua*. Tampere: Vastapaino.

Töttö, Pertti (2005). *Syvällistä ja pinnallista: Teoria, empiria ja kausaalisuus sosiaalitutkimuksessa*. Tampere: Vastapaino.

Weber, Max (1975). *Roscher and Knies: The Logical Problems of Historical Economics*. New York: Free Press.

Wright, Sonja R. (1979). *Quantitative Methods and Statistics: A Guide to Social Research*. Beverly Hills: Sage Publications.

Wynn, Charles M. & Arthur W. Wiggins (2001). *Quantum Leaps in the Wrong Direction*. Washington: Joseph Henry Press.

Attachment 1. The used population of the media studies theses. Sampled theses are marked with bold font.

(2002). Draaman elementit television ajankohtaisohjelmassa: ajankohtaisohjelman katsojien näkemyksiä.

(2002). Myöhäismoderniteetin ulottuvuuksia: näkökulmia sähköisen median yksilöllisyyteen ja yhteisöllisyyteen

(2002). Valokuvan suhde todellisuuteen digitaaliajassa. (nsla)

(2003). Seurakunnan ulkoinen viestintä ja seurakuntakuva: tapaustutkimus Vaasan suomalaisen seurakunnan tiedottamisesta 1995.

(2003). Viestintäteknologia utopiana ja dystopiana. (mcl)

(2004). Dinaa etsimässä kerronnan ja psykoanalyysin keinoin: vertaileva tekstianalyysi Dina-kirjasta ja -elokuvasta.

(2004). Dramatisoitu demokratia: tasapuolisuus ja viihteellisyys television yhteiskunnallis-poliittisissa keskusteluohjelmissa.

(2004). EU:n perustuslakineuv ottelut neljässä suomalaisessa sanomalehdessä syksyllä 2003.

(2004). Identiteetin rakentuminen pohjalaisten lehtien yleisurheilujutuissa.

(2004). Myöhäiskapitalistinen kulttuuriteollisuus: mytologinen analyysi postmodernistisista kulttuuriteollisuustuotteista. (ecl)

(2004). Urheilujournalismi Etelä-Pohjanmaan maakunnan imagon rakentajana: esimerkkeinä sanomalehdet Ilkka ja Pohjalainen. (la)

(2005). Auktoriteettien valtataistelu: julkisuuden pääsyn problematiikka ja lähdekäytäntö Turun Sanomien talousjournalismissa vuosina 2001-2002.

(2005). Dokumenttielokuvan monimuotoisuus televisiossa: katsaus Dokumenttiprojektin tarjontaan.

(2005). Elokuva unten mailla - unen ja elokuvan analogia. (la)

(2005). Kriittisen mediatutkimuksen alkulähteellä: Theodor W. Adorno ja teoria kulttuuriteollisuudesta. (ecl)

(2006). Kohti elokuvakerronnan temporaalista fragmentaarisuutta.

(2006). Kuusitoista elonmerkkiä: vuosien 1993-2004 latvialaisten näytelmäelokuvien kerronnan analyysi. (mcl)

(2006). Narratologia, kirjallisuus ja digitaalisuus: vanha ja uusi kerronta digitaalisella aikakaudella.

(2006). Todellisuuden representaation problematiikka: katsaus dokumenttielokuvan teoriaan.

(2006). Vaihtoehtoinen pienlehti: esimerkkitarkastelussa syväekologinen kulttuurilehti Elonkehä.

(2007). "Puoluepomon SEKSISKANDAALI!": Ilta-Sanomien uutisjutut stigmatisaation välikappaleina.

(2007). Tunteeko sika: sarjakuvahahmon tunteiden analysointia Aristoteleen tunneteorian pohjalta.

(2007). Viina vaatii veronsa: argumentaatio analyysi alkoholiverotusta koskevista pääkirjoituksista kolmessa sanomalehdessä.

(2008). Alueellinen identiteetti ja sen stereotyyppisyys Ilkassa.

(2008). Jean-Baptiste Grenouille - antisankari: vertaileva tutkimus pahuuden representoinnista kirjassa ja elokuvassa

(2008). Kalevan kansan perintö: Savo kansallishengen välittäjänä ja ylläpitäjänä 1880-1889.

(2008). Naistenlehtien kansikuvien välittämä naiskuva: empiirinen analyysi vuosilta 2002 ja 2007.

(2008). Nuoriso, tv, valistus ja viihde: tarkastelussa nuortenohjelmien sisältö ja journalistiset keinot.

(2008). Todellisuus, tieto ja tarina: tarkastelussa dokumentaarin käsitteet ja representaatio.

(2008). "Vastakkainasettelun aika on todellakin nyt": presidentinvaalit 2006 retorisena analyysina Pohjalaisen tekstaten-palstalla.

(2009). "Maakuntien Suomi vaarassa" ja "Vasta nyt kuin juna": kriittinen tutkimus kahden puoluepoliittisesti sitoutuneen sanomalehden EUhun ja Emuun liittyneistä pääkirjoituksista.

Grades in ascending order: approbatur (a), lubenter abrobatur (la), non sine laude approbatur (nsla), cum laude approbatur (cl), magna cum laude approbatur (mcl), eximia cum laude approbatur (ecl), laudatur (l)