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On the pursuit of academic research across all the disciplines

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Introduction

This article is about similarities and differences in the pursuit of academic research all across the disciplines. It forms part of my efforts in the field of big history to find a common language and understanding for all its practitioners, both big historians and specialists from all the academic fields that contribute knowledge to big history.

Over the course of time, all these different academic disciplines have come to employ their own specialized methods and languages, which sometimes overlap, while they do not at other times. Furthermore, some of the terms used, such as 'system,' 'energy,' and 'meaning,' may look the same, yet they have acquired different meanings in different academic arenas.

All of that does not matter too much as long as these disciplines stay apart while their practitioners understand each other well within their own fields. However, the effort of big history to bring all these disciplines together into one single coherent account has inevitably led to a need to confront these issues in an effort to shape one single common language. This discussion has barely begun, not least because the problem may not yet have been sufficiently recognized. As a result, currently many such misunderstandings seem to abound.

In this contribution I do not seek to confront all these issues. That can only be done in a long and intensive discussion involving a great many scholars. Here I will focus on only one aspect, academic research, while, of course, I do not expect to present any final views about this topic. Yet it is my hope that this article will stimulate a discussion with the aim to achieve a clearer understanding of what all academic researchers have in common.

A major source of misunderstandings may be the incorrect perceptions that appear to exist within the field of the natural sciences of how the humanities and social sciences pursue research, or ought to do so, and vice versa. I experienced that firsthand while first engaging in the natural sciences, and later also in the social sciences. Most notably this seems to be the perception that natural scientists are doing hard, serious science, while the rest is not, and that the rest could only turn into a serious science by using the methods of the natural sciences (which are often misunderstood within the field of the humanities).

In practice I have found, however, that although the research subjects and, as a result, the methods used to pursue investigations, are different, the underlying approach of how to engage in academic research is, in principle, exactly the same. Clearly, the academic study of far more complex subjects, such as life and human societies, entails specific problems that natural scientists never have to face. This has deeply influenced the development of the humanities, including the social sciences. But even though the research subjects and the methods employed may be very different, the underlying general approach of how to do research seems to be identical. This will be discussed below in more detail.

This article is an elaboration of an essay dating back to the 1980s. The first draft was written in 1984 while I was studying cultural anthropology at the Free University Amsterdam (Spier 1984). It dealt with empirical academic research all across the sciences; on the interests served by doing empirical research; and on ethical questions that might arise as a result. At that time I was preparing my cultural anthropological and historical field research on religion and politics in Peru, which would hopefully involve living in an

1 I am indebted to an anonymous reviewer for excellent suggestions on how to improve this article.

Andean village while studying its present and past. I felt it was important to reflect on these things beforehand, because I wanted to understand as well as possible what I was setting out to do.

Before studying cultural anthropology, I had received a M.Sc. in biochemistry 1978 at Leiden University. While introducing me to the natural sciences ranging from physics and quantum mechanics all the way to microbiology, this study had included a considerable amount of research into various fields, most notably synthesizing and isolating a number of chemical compounds. As a result, the intensive years between 1970 and 1977 that I spent in the Leiden laboratories had allowed me to gain a first-hand experience of how the empirical scientific method worked in practice, not least thanks to the many open and sometimes revealing discussions that I had been part of within the safe confines of those laboratories. My knowledge was further shaped by taking part in a discussion group that explored the most recent ideas about the history and philosophy of science.

While studying cultural anthropology at the Free University Amsterdam in the 1980s, I took a few philosophy courses, which further improved my views. All of that, including my growing knowledge of how the social sciences worked in practice, provided the intellectual background for my essay of 1984. I then felt that even though the objects of investigation and, as a result, also the methods of investigation, were very different, the general approach across the sciences seemed to be identical.

After having returned from Peru in 1986, I confronted my 1984 ideas with my experience of pursuing cultural-anthropological and historical studies in a Peruvian Andean village (Spier 1986). To my delight, I did not have to change a single idea. But I did find that the essay could be enriched with my experiences. So I wrote an improved version, which became one of the required assignments for obtaining my M.A. in cultural anthropology (Spier 1987). Even though now almost thirty years have passed since writing that essay, I still stand by the general view that was penned down at that time.

The 1984 reflections were written on a typewriter, because the computer desktop revolution had not yet reached our university in the Netherlands. After my return from Peru in 1986, however, during which I had shared life with Andean farmers who lived in circumstances that were in many ways similar to those of peasants in the European Middle Ages, while in the cold sacristy of the village church I had transcribed ancient documents that had partially been consumed by rodents, the computer revolution had taken place.

One of the first things that needed to be done was, therefore, mastering this brand new technology: learning to use desktop computers with two large floppy disk drives, one drive for the MS-DOS program and the text-processing program WordPerfect, and the other drive for saving the files. Of course there was no Internet yet. On this exciting but challenging technology my Andean data were recorded and elaborated using the WordPerfect program, with the aid of which also my improved essay on the scientific method was written. This made it possible today, using a much more recent version of WordPerfect –still my preferred word processor application–, to effortlessly use those (now) ancient computer files.

The essays were written in Dutch. As a result of discussions with big historians, especially regarding the seeming lack of understanding among natural scientists and academics from the humanities about what they share while engaging in doing academic research, I began to wonder whether it would be helpful to translate those ideas into English. This article is the result of that effort. A few insights have been added that seemed so obvious to me at the time that they did not need to be mentioned. Yet they may not always be

obvious to practitioners of the humanities or to readers who are not academically trained. A few other insights were added to illustrate or further elaborate certain aspects.

The essay below is, therefore, a reworked version of the first section of my 1987 essay, which reflected on the empirical academic research. The second section, which was a reflection on the interests served by doing empirical research as well as on ethical questions that might arise as a result, is not included here.

What does the pursuit of academic research consist of?

Before discussing the pursuit of academic research in more detail, it seems important to make a fundamental distinction between two different ways in which humans face the world. The first way can be called 'direct experience.' Everybody experiences the world in a direct way and reacts to it. Such experiences, as well as the resulting feelings can be expressed, among other things, in daily conversation and with the aid of gestures of many kinds, but also in literature, music, dance, decorations, etc. Direct experience and the resulting reactions are the ways in which most humans live their lives most of the time. The US philosopher Robert Pirsig (1928-) called this the 'romantic mode of understanding' (1976, p.66 ff.), while in his book *Involvement and Detachment* (1987) the German sociologist Norbert Elias (1897-1990) used the word 'involvement' to characterize this attitude.

A second way of dealing the world is thinking in terms of underlying forms. While doing so, one takes distance from one's direct daily experiences and starts looking for underlying patterns that may describe and explain the observed events and regularities. In this approach, the leading questions are: "how do certain things work, and how and why are these things happening the way they do?" Norbert Elias called this attitude 'detachment,' while Robert Pirsig uses the term 'classic mode of understanding' (1976, p.67 ff.). In short: these are forms of rational thinking. These underlying forms include a wide range: daily reasoning, all kinds of formalized regimes of thought such as writing and musical notation; religious, normative, ethical and philosophical ways of understanding the world, all the way down to the ways academics pursue research.

This essay addresses the question: what distinguishes empirical academic research from all the rest of human experience and thought, and how is it connected to it?[2] Before focusing on this question I would like to emphasize that in my personal opinion, both direct experience and thinking in terms of underlying forms are essential in our human existence, while I see them as equally valuable.

However different these two ways of approaching the world may be, in practice they are always connected. Surely, there is often, if not always, some interplay between direct experience and thinking in terms of underlying forms, including research performed by academics. When I am trying to play the guitar, for instance, I need some knowledge of underlying form about how the notes and chords are structured on this musical instrument. Yet that is not enough. Playing music first of all involves trying to express and convey certain feelings, in other words: direct experience. Without it, playing the guitar would yield only a technically-executed sequence of notes without any emotional value. We will find a similar interplay when academics are pursuing research, but with a different emphasis on what

2 An anonymous reviewer pointed out that "Daniel Kahneman's distinction between 'fast' and 'slow' thinking may be similar, if not the same, including how natural selection may have generated methods for rapid reactions and also for slower, more careful thinking (rationality?)" (Kahneman 2011).

the end result should look like. The first question that needs to be pursued is, therefore: what distinguishes academic research from all other forms of thinking in terms of underlying forms, and how did it emerge?

How did academic research emergence in Europe?

Over the course of many centuries, constellations of people have emerged in Western Europe and elsewhere who have given special emphasis to a rather strict approach of thinking in terms of underlying forms in relation to empirical observations, while practicing and developing it in specific ways. This approach was first institutionalized in specific houses of learning, in Europe most notably universities and royal academies. Over the course of time, also other institutions emerged where empirical science (in an increasingly broad sense) is practiced, such as today in a great many research institutions.

To be sure, the European universities were preceded by similar scholars and institutions in many parts of the world. Yet the European model, especially the 'Humboldtian' model of open-minded research and teaching first institutionalized by Wilhem von Humboldt (1757-1835) and his colleagues at Berlin University in the early nineteenth century, has become very influential, while it has been copied all around the world. Because of its relentless emphasis on rational thinking, Robert Pirsig called the university system the 'Church of Reason' (1976, p.140). This specific approach of thinking in terms of underlying forms about empirically observed reality is the essence of the academic pursuit of understanding reality.

Before continuing this discussion, it may be useful to reflect a little on the meanings of the word 'science.' In Latin, 'sciencia is derived from the verb 'scio,' which means both 'to know' and 'to understand.' These words do not point to any types of feelings. In the formal academic approach it is all about thinking in terms of underlying forms: regularities and generalizations of direct empirical observations. In the Anglo-Saxon world the term 'science' has come to mean the 'natural sciences,' while the social sciences and the humanities are seen as rather different academic pursuits. Yet in language areas such as German and Dutch such a distinction is not made. To avoid any confusion about this here, the term 'science' is therefore used very sparingly, while the preferred term is 'academic research.' This is all part of the attempt of seeking to find an unambiguous common vocabulary that may be helpful to improve communication across all the academic disciplines.

As mentioned before, most forms of thinking in terms of underlying forms are not regarded as science. In all societies, for instance, there are more or less formalized prescriptive rules concerning how to behave. In most, if not all states such rules have been written down in the form of law, which today also includes huge amounts of jurisprudence. These regimes of thinking in terms of underlying forms are not meant to explain why people behave the way they do. They simply define and prescribe the dominant rules of behavior, including what should happen if people do not follow these rules. This is only one example from the large field of regimes of underlying thought that are not part of the process of empirical academic research. Such regimes can, of course, become a subject of academic research.

What are the fundamental requirements of academic research?

So what can be regarded as empirical academic research? To deserve that qualification, a regime of thinking in terms of underlying forms must fulfil at least four strict requirements.

The first requirement is logical consistency. The formulated regularities should follow the

strict rules of logic, most notably that they never contradict each other. Most, if not all, academics are constantly testing both their own thought structures and those of others on internal and external logical consistency. In other words: a hypothesis or theory must follow these rules and not contradict itself, while it must not contradict other established theories either, unless the purpose is to undermine such a theory.

To my knowledge, the oldest extant standardized forms of logic in Europe were formulated by Aristotle (384-322 BCE). These rules appear to have universal application. While most academics may lack formal logical training, most, if not all, of them appear to be able to apply these rules more or less intuitively with great success. It may well be that this form of thinking is genetically ingrained to some extent in our bodies as part of the long evolutionary history of our species, and perhaps in many earlier life forms as well, as a result of their attempts to map the world and act sufficiently successfully while using those maps to survive the onslaught of the process of biological evolution first outlined by Charles Darwin (1809-1882) and Alfred Russel Wallace (1823-1913).

It is important to see that this requirement of consistency, including no contradictions, is an assumption imposed on the academic method. Although deemed unlikely, it cannot completely be ruled out that nature may be contradictory from time to time. So we may want to keep our eyes open for such possible events. If we find them, it would fundamentally alter the academic method.

The second requirement is that a theory or hypothesis must be able to explain, or at least provide some structure to, empirical observations. This means that empirically-observed situations can be seen as part of more general structures. This is the ultimate goal of empirical academic research.

Because empirical observations are part of the domain of direct experience, this is what links that domain with the domain of thinking in terms of underlying form. But not all observations can be called empirical observations. Claims, for instance, to have witnessed certain events such as the appearance of gods may be called personal observations. But such observations cannot be called academic empirical observations as long as they cannot, in principle, be observed by other trained academics (or could have been witnessed by them, if they had been present when that happened). In other words, empirical observations must be social events within the world of academia. To be sure, the interpretations of empirical observations and even their descriptions may – and often do – differ.

For instance, the claim to have seen a stationary very bright star in a very particular place in the sky about 2000 years ago during an event deemed unusually important is not, by itself, an empirical observation, unless it is confirmed by other observers. In consequence, astronomers may search old records for such possible events that were observed and recorded by others. Alternatively, researchers may seek to reconstruct unusual events in the sky during that particular period of time based on empirical data as well as on their knowledge of celestial processes, such as rare conjunctions of planets and stars, or perhaps exploding stars called supernovae.

If academics find such events, this may turn the other observation into a possible empirical observation, even though the interpretations may be very different. However, the claim that a bright star stood still in the sky for a longer period in time is likely to be contested by academics, because no empirically observed stars have ever done that, with the exception of Polaris, the pole star, which is currently situated virtually right above the North Pole and, as a result, appears (almost) stationary in the sky as

seen by earthbound observers. Yet around 2000 years ago that was not the case, while there was, to our knowledge, no other star at that time that occupied Polaris's current position above the North Pole.

This raises the question of how to decide when one particular theory should be considered 'better' than another one. The great majority of academics think that the criterion is simplicity: the preferred theory contains the fewest general rules while structuring and explaining the largest number of empirical observations. This principle, called parsimony, is also known as 'Occam's razor,' named after the English cleric, William of Ockham (c.1287–1347), who formulated this principle very clearly.

Furthermore, this raises the question of what 'explaining' means. It turned out not to be very easy to answer that question unequivocally. A great many scholars have sought to clarify this, including Scottish philosopher David Hume (1711-1776), followed by Prussian philosopher Immanuel Kant (1724-1804), the "Wiener Kreis" (Vienna Circle) philosophers, Austrian-British philosopher Karl Popper (1902-1994), and US scientist and philosopher Thomas Kuhn (1922-1996). I do not want to pursue this question here in any detail. But clearly, for achieving a good understanding of academic research it is very important to carefully explore what the nature, possibilities, and limitations of explanations are.

This also raises the question of how much empirical certainty theories would provide. Sir Karl Popper pointed out that because we will never be able to know all empirical data, we will never be able to formulate with absolute certainty any general rules that will cover all empirical data. In consequence, structuring and explaining empirical data will always remain uncertain to some extent.

This implies that the results of science are always uncertain to some extent, although in many cases the uncertainty may be limited. For instance, the great improvement over the past decades of scientific insights and their practical applications concerning the construction and operation of airplanes has led to a remarkable decrease in the number of serious accidents per passenger and per distance covered, which are now far lower than they have ever been. Apparently, these insights are ever more reality-congruent, in Elias's terms, in the sense that they contribute to building and operating airplanes that do not randomly fall out of the sky. And as soon as a serious accident happens, huge efforts are made using academic research to find out what went wrong in order to prevent this from happening again. That is how academic research works, and this has led to this remarkable success. A great many similar examples could be given, all indicating the enormous advances in uncertainty reduction that has taken place within those fields. However, not all academic pursuits have led to similar successes. More about that below.

The third major requirement is that empirical data that are seen as sufficiently solid should not contradict our hypotheses or theories. In this context, Karl Popper used the word 'falsification,' with which he meant that when one single empirical observation considered sufficiently solid is found to be in contradiction with a theory, this should lead to the rejection of that particular thought structure.

This means that all hypotheses and theories are in principle 'open-thought regimes,' because all theories can in principle be tested with the aid of empirical observations. These open-thought regimes are in stark contrast with 'closed-thought regimes,' within which any event can be explained without ever casting any doubt on the underlying thought structures. Examples of closed-thought

regimes can, most notably, but certainly not exclusively, be found in religions.

In other words: an academic theory must in principle be open to rejection if it does not conform to established empirical observations. In practice, however, as Kuhn observed in his ground-breaking book *The Structure of Scientific Revolutions* (1970), theories tend to exist in the midst of unexplained observations that are temporarily cast aside. By slightly changing or refining theories, many recalcitrant observations may be accommodated, at least for a certain period of time. For instance, the epicycles used by Ptolemy to explain the planetary movements in his model of the solar system with Earth at its center offer a famous example of such a refinement. In fact, such attempts at refining theories happen most of the time when discrepancies between theory and empirical observations arise. Most academics are understandably wary of quickly abandoning a theory that has worked well in the past. Such a tendency may well have emerged as a result of biological and social evolutionary pressures that our species has experienced during its history.

Yet, as Thomas Kuhn further explained, some observations turn out to be so difficult to reject or accommodate within a theory that they may lead to a state of chaos within the discipline. To overcome this unpleasant situation, new theories are advanced for explaining these observations in different, often more all-encompassing ways. Over the course of time, this will lead to the replacement of the old theories by the new ones. The discovery of the photo-electric effect and Albert Einstein's explanation of it, which earned him a Nobel prize in 1921, leading to the emergence of quantum mechanics (which Einstein did not like), offers a clear example of such a change. Also the emergence of big bang cosmology and plate tectonics can be understood in such ways. Interestingly, these novel academic insights were gaining ground while Kuhn was writing his book, yet he did not mention them then in his writings. One may wonder whether these academic developments may have stimulated Kuhn to undertake his research, even though he may not have been sufficiently aware of them at that time. Furthermore, as Kuhn explained, empirical observations never yield absolute facts. All observations are always interpreted before becoming academic observations. This inevitably introduces elements of uncertainty. As a result, the description and interpretation of empirical observations may change over time. A practical example from physics may clarify this:

> It is possible to determine the concentration of compounds in a solution by measuring the amount of light that is absorbed by that compound. The more light 'disappears,' the more 'stuff 'is present in that particular solution. But the scientist who does such an experiment should never assume that by just reading the meter (empirical observation) the concentration of that compound is known with absolute certainty, because possibly: 1. The meter is not calibrated correctly; 2 the glass vial or the lens is dirty; 3. there are other compounds in the solution that also absorb light; 4. The relationship between absorbed light and concentration is different from expected, and so on.

This list of uncertainties concerning the interpretation of empirical observations is endless. If one reflects long enough on these things, new aspects may be found that may influence one's observations or one's interpretations of them.

This is the case for all empirical observations. No single fact can be established beyond any doubt, simply because all empirical observations entail forms of interpretation (cf. also Kant 1976). Just think of what a piece of rock might look like to a

lay person without any knowledge of chemistry or geology; what it would have looked like to a geologist a century ago; and what it would look like to today's geologists trained to observe them with the latest chemical and geological concepts and methods. And, following Kuhn's argument, this situation becomes even more serious when empirical observations are described and interpreted with the aid of competing theories. As a result, academics from different fields may not be able to communicate effectively with each other any more. In consequence, as Kuhn argued, the transition from an older to a newer theory may also involve a redefinition of empirical observations.

This uncertainty has worried academics, because they feel that their fields ought to be built on more secure foundations. In trying to tackle that issue, before the Second World War scientists united in the 'Wiener Kreis' sought to establish a theory of science based on pure experience. Their huge efforts and spectacular failure have emphasized that pure empirical observations without any form of theoretical interpretation do not exist. In other words, theories and empirical observations are always linked to some extent, which greatly complicates the testing of competing theories.

In other words: within empirical science it is impossible to make an absolute distinction between 'objects' and 'subjects,' simply because the results of academic research always consist of images of the world produced by humans. They may represent the best available academic descriptions today. But they never represent the only possible interpretation of the world itself, and should never be seen as such. In consequence, these images always inform us something about both the observer and the observed, even though often great efforts are made to push the balance in such a way that the information is mostly about the observed world and as little as possible about the observers.

Yet both influences are always present in academic reports. As a result, such accounts can always be read as informing us both about the result of academic research and about the backgrounds of the particular researchers. In fact, I stimulate my students to engage in both types of reading. Even though the results of academic research may not require accounts of the researchers' personal experiences, any understanding of the process of academic research does require such knowledge. That is why I myself am always very interested in such personal stories.

How does such often hard-gained knowledge become academic knowledge? To be recognized as such, this knowledge needs to be shared and discussed among colleagues. In other words, the fourth fundamental requirement of academic knowledge is that it is socially shared. Inventing theories and making empirical observations may well be a private affair. But they turn into academic knowledge only after having become discussed and accepted among academics, at least to some extent. A famous anecdote may illustrate this:

> In the 17th century, a competition among scientists was going on in London concerning the question of who could explain why Earth's orbit around the sun was an ellipse. One day in 1684, the famous physicist Edmond Halley (1656-1742) visited Isaac Newton (1642-1726), who was then living a rather secluded life in Cambridge. Halley posed Newton the question: "what is the shape of Earth's orbit around the sun?" based on Robert Hooke's suggestion that the attraction between the sun and the Earth followed the law of inverse square of the distance. "An ellipse," replied Newton. "How do you know that?" was the next question. "I calculated it," replied Newton. He claimed to have solved that problem 18 years earlier,

> but was unable to find his notes. Halley persuaded him to elaborate this idea mathematically, which led to one of the most famous books ever written in science *Philosophiae Naturalis Principia Mathematica (Mathematical Principles of Natural Philosophy)* published in 1687. At that moment, Newton's private efforts became more generally known, and thus turned into academic knowledge.

After having formulated these four fundamental requirements, it has become possible to offer a definition of empirical academic research, as follows:

> "Empirical academic research consists of a very specific form of thought in terms of underlying forms aimed at formulating general principles that can structure and explain empirical observations. These underlying principles must be logically consistent, while they must be obtained in a continuous process of confrontation with both the available empirical evidence and the established theories. The results of these efforts must be shared with other academics."

Non-rational influences in academic research?
Before exploring the definition of academic research just mentioned, it seems important to pay some attention to the fact that also non-rational aspects are extremely important while engaging in such investigations.

First of all, every empirical researcher investigates something. But what? Why was that particular research topic selected, and not another one, or nothing at all? Such choices can never be determined in totally rational ways. The investigation is done because of the importance attached to it. It is a choice determined by the researcher and/or by the persons facilitating the research. As a result, making such choices goes way beyond the formal approach of academic research and its results, even though a good knowledge of academic research may well have contributed to arriving at such decisions.

In other words, these choices depend on the feelings, value judgments, their knowledge, and the social possibilities and limitations of all the people involved in such activities. All of this belongs to the domain of direct experience as well as perhaps to other fields of thinking in terms of underlying forms. As a result, these choices can never entirely be legitimized only by referring to the principles of formal academic research. While academic research is a tool for achieving specific goals in specific ways, it is not driving the process of investigation.

As soon as the choice for a certain object of investigation has been made, this determines to some extent the academic insights and methods that are going to be used. It is not a good idea, for instance, to do chemical experiments entirely based on cultural anthropological insights. And it is equally unproductive to investigate social behavior by exclusively using theories of the chemical bond. It would not make a great deal of sense to take Peruvian farmers apart and separate their molecules in an ultracentrifuge if one wanted to know their social situation, or seek to isolate DNA molecules by observing certain organisms.

Yet comparisons of theories and methods from different fields may lead to new and interesting insights. In his book *What is Sociology?*, for instance, Norbert Elias fruitfully compared molecular bonds with human bonds while advancing his theory of 'human figurations' (1978a, p.72). He could do so thanks to the fact that he had studied medicine before becoming a sociologist.

Also, while engaging in academic empirical research, non-rational aspects abound. Why, for instance, would a researcher follow certain

research strategies and not others? Why would that person observe certain particular aspects and not others? How would all of that be done in ways that work well? And why would certain hypotheses be formulated and not others, out of the thousands of possible hypotheses? And where would all these ideas come from?

The answer 'logical thinking' is not fully satisfactory. Everyone who has seriously engaged in empirical academic research knows that such insights tend to appear quite suddenly, much like the proverbial light that goes on. At such moments, the researcher intuitively feels that this is the direction that should be taken, or what the solution for that particular problem might look like. The next step is to check very carefully and argue whether these ideas are indeed academically correct. This is done by painstakingly confronting them with all the available empirical evidence and existing theories using rational thinking, and quite often, also by making new observations that may, or may not, confirm those ideas.

This long and often painful process of careful rational, logical reconstruction takes place after the new idea has emerged as a result of intuition. It is absolutely essential to turn such ideas into science. During the process, most ideas are rejected, while the ones that survive may guide the researcher into further pursuing her or his investigations, including further observations and experiments. But the ideas for doing such things come first of all as a result of intuition. In other words: academic research consists of a great many rigorous logical reconstructions of a path that is found by following one's intuition.

Surely, having such intuition is only possible when the researcher has become well versed in her or his field, often after having spent a great deal of time mulling over these problems. That is a most important precondition. But even though knowledge of the field is essential, it may also be helpful to enter the field as a complete outsider. In such a situation, the investigator's thoughts and reflexes have not yet been shaped to the extent that many questions are considered too obvious to ask, while new solutions may be hard to see. This is a major reason why, I think, that, for instance, in physics young researchers are usually the ones who produce the most refreshing insights. As Norbert Elias explained in his memoirs, growing up as Jew in Germany helped him to analyze societies more at a distance. The world historian, William H. McNeill, once wrote me that starting out as Canadian citizen but living in the United States helped him to see things that many of his American colleagues seemed to miss. And in my Peru research it was very helpful, in retrospect, that I knew very little about that country before starting my investigation. It allowed me to look at what I witnessed with a fresh, often confused, look, taking far less for granted as a result, while asking a great many dumb questions that no one else would have posed who was more familiar with those situations. Of course, I worked very hard to improve my knowledge, which went hand-in-hand with almost continuous observations and discussions with everybody who was willing to enter into a conversation with me. All of that was driven by intuition and personal motivation. The following personal experience shows such a process in action.

> At a certain point in time I began to wonder what the social process had been that had led to the construction of the great many ancient Inca agricultural terraces on steep mountain slopes in places such as Pisac and Machu Picchu that seem to defy gravity. I had visited those places myself, and wondered why these terraces had been built in such seemingly impossible places, and why not in places that were lower down the valley where they were easier to construct and quite likely to be more

productive (which was the case near the Andean village of Zurite were I lived). No one seemed to have posed those questions, to my knowledge. I was not aware of any references to such questions in any of the Spanish chronicles either.

In Zurite I had often witnessed the way in which many public projects were executed, namely with the aid of competing workgroups within a regime called *faina*. I had also noticed that competition among these groups for the prestige of having done the best job very much contributed to driving the process to its successful completion. Not least because all armies known to me appeared to have fought more effectively when subdivided into competing groups, I began to wonder whether the Incas would have employed a similar process of competition to construct these 'high prestige' terraces. In those places, all very strategically located, military garrisons would have been stationed. Also in an effort to prevent idleness from kicking in and leading to all kinds of mischief, these Inca soldiers might have been obliged to compete in constructing those terraces and grow food on them after their completion, so that they could provide their own sustenance. When I floated this hypothesis to farmers from Zurite during informal conversations, their faces lighted up. They immediate agreed that this could have been how it had been done. This does not provide any proof, of course, that it was done in such a way, but it does add some plausibility to this hypothesis (cf. Spier 1994, p.64, note 3).

One may wonder how the seemingly magical appearance of such ideas works. Apparently, as part of our biological and social evolution humans have become equipped with a solution-finding mechanism in our brains and bodies that helps us to come up with, and select for, 'sufficiently good' solutions out of the great many possibilities that may exist, while combining knowledge from different domains. I am not familiar with any studies that elucidate how this mechanism might work, or how it may have emerged. But that probably only reflects my own ignorance of these fields. But surely this mechanism exists, and there must be a neurological and molecular basis for it.

In sum: although a sound knowledge of the academic method is indispensable for achieving success in empirical research, the practice of doing it is determined by choices that are not entirely based on the formal method. Taking one's intuition seriously offers a major link between the world of direct experience and that of thinking in terms of underlying form. How to do this well is the central theme of Pirsig's brilliant book.

To what extent can similarities in academic research be found all across academia?

By defining academic research in the way explained earlier, nothing has yet been said about its contents, and nothing either about the choices for a certain type of investigation. All theories can be called 'academic' as long as they conform to the general requirements mentioned. In other words, the definition seems valid for all branches of empirical academic research, from the natural sciences all the way to the humanities.

Both in the field of the natural sciences and in the field of the humanities and social sciences it is often thought that the natural sciences constitute 'harder,' 'more rigorous' forms of empirical academic research. From the point of view advocated here, in terms of the general nature of academic research this is a misunderstanding. Yet in terms of content this may often be a correct assessment, depending

on what one would call 'hard' and 'rigorous,' of course, such as the existence of generally-accepted theories and reproducible data. This situation is at least partially the result of the fact that natural scientists have a much easier job than their colleagues in the humanities, because they investigate far less complex aspects of reality, while scholars of human culture deal with the most complex aspects of reality known to us.

Yet that situation should not necessarily cause the humanities, including the social sciences, to be less rigorous in terms of empirical academic research. Thomas Kuhn argued –and I fully agree with him– that this difference is mainly caused by the fact that within the social sciences it has not yet been possible to establish generally-accepted theories, or paradigms, as Kuhn called them. While the main cause for this situation may be that the humanities deal with aspects of reality that are so much more complex than those examined by natural scientists, which makes it much more difficult to establish general paradigms, this is not necessarily the end situation. This will be elaborated below.

Yet it is, in my view, a misunderstanding to think that the natural sciences are essentially different from the humanities which would make the establishment of general theories in the humanities impossible. Surely, the objects of investigation are of another nature, and this requires different research methods. As mentioned earlier, it is not a good idea to research atoms and molecules the same way as living nature or human societies. Furthermore, because atoms and molecules, stars and planets, rocks and oceans, do not show emotions and do not communicate with each other or with the researcher, while they do not have intentions or assign meanings to the rest of nature, it is much easier to investigate them academically from a greater distance. And the huge numbers of atoms and molecules involved, which are often very similar, allow researchers in the natural sciences to use statistical methods with greater success.

More in general, the lower levels of complexity involved in the natural sciences have allowed their scientists to formulate more precise natural theoretical principles with greater success than elsewhere within academia, and use them successfully to predict the outcomes of controlled experiments or the nature of relatively simple aspects of reality. All of that has led to an ever-increasing human control over the rest of nature. Yet, as Norbert Elias argued in *Involvement and Detachment*, doing all these things with greater detachment has taken a long time. Only about 500 years ago, many astronomers, for instance, were also astrologers, while chemists, including Sir Isaac Newton, were often involved in what is now called 'alchemy' (which in Arabic simply means "the chemistry"). Many of them were seeking to transmutate chemical elements, most notably making gold out of cheaper and more abundant metals. Little did they know that they were manipulating the wrong force, namely the electromagnetic force, instead of the strong force. Both forces were still unknown at that time. It has taken centuries of huge efforts and increasing detachment to reach today's knowledge and control over the rest of nature. The following example may clarify this.

> I experienced such a transition of organic chemistry from basically a cookbook science into a fully-fledged science first hand during my second year while studying chemistry in 1972. One of my lab assignments in organic chemistry was to synthesize a series of compounds called glutarimides. These are fairly basic compounds with little or no commercial value or any other social importance. My supervisor wanted to take measurements of certain chemical bonds of these compounds using the most recent technology (nuclear magnetic resonance spectroscopy).

> Because they were of little interest, no recent recipes appeared to exist for synthesizing them. So I went to the library and explored the chemical literature, which also at that time was well organized. My search took me back to the 1880s and even earlier than that. And sure enough, I came across recipes for synthesizing some of these compounds by well-known chemists. At that time, organic chemistry was not yet well understood theoretically. In doing so, I realized that in the meantime, the discipline of organic chemistry had undergone a transition from what looked like a trial-and-error cookbook approach without a clear theoretical underpinning into a well-established theoretical discipline.

Howe could we achieve greater detachment in the humanities while engaging in academic research? Surely, all academics are human beings, and doing science always involves dealing with one's emotions. Yet over the course of time, natural scientists have learned to take distance from their emotions, which usually do not end up in their research reports. Their accounts focus on the results of investigations as well as on the methods used to obtain them. Their feelings may show up in their personal accounts and histories, if they are willing to write them. A famous example of such personal memories is the book *The Double Helix* (1981) written by US scientist James Watson (1928-), which tells his personal account of how the structure of DNA was discovered in 1952-53 together with Francis Crick (1916-2004) and others. But in their scientific articles, and also in Watson's textbook *Molecular Biology of the Gene* (1970), which was part of my first-year chemistry education at Leiden University, these stories and emotions were entirely absent.

The large separation between detached academic reporting and dealing with one's emotions has become such standard behavior in the natural sciences that it is followed without much, if any, reflection even by newcomers in the field such as me. The following example may illustrate this.

> As part of my attempts to synthesize glutarimides mentioned earlier, I tried to make N-Cl glutarimide. This compound seemed never to have been described; at least I could not find any data about it. When I tried to synthesize it using an approach analogous to N-Cl succinimide, this led to an explosion. Fortunately, I had carried out the experiment in a safe and protected environment. After watching strange bubbles and fumes coming out of the glass reaction vessel through a protective glass window, I warned my supervisor. At the moment that he appeared, the explosion took place. I still remember us running away, me jumping after this huge guy like a little rabbit chasing its mother.

None of that appeared in the lab report, however (I still have it). It only offers a detached description of the chemical events that took place. That was considered the important result. Nobody took any notice of my possible emotions either as a result of this explosion – I was completely on my own in dealing with that. Such aspects were simply not part of the pursuit of academic research at that time. It also made me wonder whether such an explosion was ever reported in the chemical literature, and if not, how many researchers might have done similar things. More in general, it raised the question of what is, and is, not reported in the academic literature. I am not going to purse that subject here. But the increasing separation over the past centuries between personal experiences and academic reporting, including not reporting failures, might be an interesting line of research.

A similar distance is also noticeable in academic reports in the humanities. Yet it happens quite regularly, also in big history accounts, that such accounts are spiced up with the author's personal emotions and value judgments, such as how 'amazing' certain developments are, or who the 'good' and 'bad' actors are. In doing so, such accounts combine personal feelings and personal value judgments with academic reporting. Doing that sort of thing is now considered totally unacceptable in the natural sciences, although such statements may appear in popular science books, because that might lead to better sales numbers.

Let's return to the comparison between the natural sciences and the humanities. It does seem correct, as natural scientists often remark, that there is less solidity in the humanities, most notably a paucity of hard data or good theories. Surely, as said before, the humanities are studying the most complex aspects of nature, which do have feelings and opinions, while there are often many layers of interpretations of meanings and intentions, including many types of uncertain information, if not outright deceit. As a result, this inevitably leads to a great many layers of interpretations. Far fewer experiments are possible, while the ones that are performed are much more artificial in character. Furthermore, the numbers of 'objects,' humans, involved are usually much smaller and much more varied and complex. All of this makes statistical approaches in the humanities and social sciences far less reliable and insightful.

In addition, while studying human societies in the past or present these processes will never repeat themselves exactly. This is not unique to the humanities. In Earth science, climatology, geology, biology, and astronomy, in fact in all studies of historical processes very similar situations can be found. Yet many of those fields have developed their paradigmatic theories, while the humanities have not yet done so. And when scholars of human societies are employing certain theories, they are still divided into a great number of competing schools, as Thomas Kuhn observed more than fifty years ago.
To be sure, there have been efforts to develop general theories of human behavior. In my opinion, Norbert Elias's process sociology currently offers the best available option. But even that promising theory is still in its infancy, or so it seems to me, and much work needs to be done to turn it into a general theory of human behavior. And that work will only be done when sufficient numbers of gifted scholars will obtain positions at universities that enable them to do so and accept the results.

It is often said that in the natural sciences the interplay between empirical observations and theories leads to new empirical predictions that can serve to test the theories, while this would not be the case in the humanities. This lack of prediction may indeed exist in some branches of the humanities. Yet by employing Elias's process sociology it turned out to be possible to predict certain aspects of the past that had not yet been investigated, as some of us involved in this approach discovered at the end of the 1980s and early 1990s (for some examples, see: Spier 1994). This was a pleasant and encouraging surprise. It seems to me that by further developing this theory, there is a good chance that its predictive power will be enhanced as well. In the Epilogue to his book *Guns, Germs and Steel* (1997), US scholar Jared Diamond made a very similar point.

Why would there be less detachment in the humanities?

The main reason why there are no general theories yet within the humanities, following Elias's argument, is that there has not yet been enough detachment within many of these disciplines. Why not, one wonders?

First of all, there seems to be a resistance among certain sections of the humanities against using

theories, because their objects of investigation are deemed so complex that theories simply would do them sufficient justice. Such concerns are understandable but may not be warranted. If one does not try, one will never succeed. Surely, such general theories will only emerge as a result of a long process of trial and error, including a great deal of scholarly discussion. And because a considerable number of grand theories, such as Marxism and Social Darwinism, were proposed that did not work very well, this has soured the appetite of many practitioners in the humanities for trying again.

Yet even though historians and others may reject theories or sparsely use them, they must have them somewhere in the back of their minds. If not, how could these scholars decide what is important to mention and what is not? How would they make those choices out of the zillions of potential data? In my view it is better to put one's theoretical cards openly on the table, so to speak, so that they can openly be discussed. Especially among sociologists but also among some social anthropologists and economists, theories are far more accepted. Yet also in these disciplines a great many schools exist, while few attempts, if any, are made to unify them.

The theory-driven approach offers great advantages, not least that it makes clear what is at stake and what its strengths and potential weaknesses are. Furthermore, it can show more clearly which information is lacking (which is usually by far the largest portion). If one relies on documentary studies without keeping such a bigger picture in mind, these insights may easily get lost. In other words, theories are very helpful for focusing on the bigger picture while helping to solve a great many smaller puzzles.

But as Thomas Kuhn emphasized, theories are also constraining, because they define what the legitimate problems are and, in doing so, potentially exclude other problems and questions. Any researcher would do wise, therefore, to keep one's eye open for what can be observed and reflect on it, especially when something seems to be wrong or seems to fall entirely outside of the theory employed. That is not easy. Most scientists may tend to push out such unpleasant feelings and observations, or may not see or feel them at all. Yet as a result, they may never discover something really new.

Currently, many socio-scientific theories still seem to be in their infancy. Furthermore, it may also happen from time to time that social scientists seek to adapt their data to their theories, not least by omitting data that do not fit the picture very well. This is not entirely uncommon in the natural sciences either. But in that field, it is far more common to engage in critical reflections, including self-reflections, concerning what does or does not work. Yet as Kuhn emphasized, it may be very difficult to obtain acceptance of fresh ways of understanding the world, not least because so many academics have invested their whole lives in working with the established theories, while they may be reluctant to abandon them.

In addition, it occasionally happens in the social sciences and the field of history that intuitive ideas are adopted as structuring principles or hypotheses without carefully examining them by testing them against all the available evidence and theories using strict rational thought. In other words, such intuitive ideas emerging out of direct experience are not sufficiently checked with the aid of a careful rational logical reconstruction. This may lead to rhetorically appealing arguments that, on further inspection, are not sufficiently rigorous.

It seems to me that in this respect the natural sciences are much more advanced than the social sciences and the humanities, because such natural-scientific ideas are usually much more rigorously tested before making them public, while they are thoroughly discussed after having reached the public domain. Of course, all of this may entail serious

problems of understanding each other, as Thomas Kuhn emphasized, including the willingness to understand others. But based on my experiences there can be little or no doubt in my mind that this approach is currently far more rigorously pursued in the natural sciences than in the humanities.
It seems, therefore, of utmost importance to rigorously test hypotheses equally in the humanities before launching them into the public domain. This includes: checking them against all the available information; engaging in further investigations to find evidence in support of, or undermining, the hypotheses; and engaging in efforts to predict the past elsewhere. In biology, Charles Darwin's famous book, *On the Origin of Species* (1859) offers an excellent example of such a scrutinizing self-critical attitude. It would be great, or so it seems to me, if this type of approach would also become dominant within the humanities as a whole.

Surely it is not always necessary to explicate the general theories - no chemist or physicist would do that either in their more specific investigations. But it does seem important to have such theories in the back of one's academic mind, so to speak, and to share them with others as soon as someone asks for them. That is what natural scientists do. Having a theory in the back of one's mind and regularly consulting it while doing the research may also help to improve the quality of the analysis by structuring it, and by using it as a heuristic device.

It has also struck me that in the natural-scientific literature the reporting of fresh results always starts with a summary of previous knowledge. In doing so, it provides an outline of what is new and what is already known. Although the mentioning of previous knowledge is certainly not absent in the social sciences and the humanities, it seems to me that especially articles in journals often lack this aspect (including this article), while books may sometimes not sufficiently do so either.

It is surely easier to do so in the natural sciences than in the humanities, because they are so much more structured and well organized. Yet from time to time claims of originality in the humanities appear not to be not sufficiently backed up by overviews of previous knowledge that clearly was available. I cannot be sure about why this is happening. Surely, the materials are more spread out over disciplines and sources and as a result much more difficult to trace and access. But we now live in a time in which a great deal of such knowledge is almost instantly available. This offers excellent chances within the humanities to set the record straight. Doing so would greatly improve the chances of authors building upon each other's knowledge, and thus help to achieve a more systematic accumulation of academic knowledge and, in consequence, more progress in the improvement of our academic insights.

Furthermore, it happens from time to time that social scientists employ theories in which the personal political and/or social preferences seem to be percolating. Although it will obviously be impossible to take full distance from one's own socio-cultural and personal background, it seems desirable to me to try to avoid such a bias as much as possible. This requires a great deal of self-reflection, detachment, and discussion, and also a willingness to allow certain insights that may not correspond with one's own personal preferences. This can be a painful process, as I have experienced myself. But by allowing this to happen, the analysis may become better, that is: more reality congruent, as Norbert Elias called it. To be sure, all reconstructions of reality and its past will always remain representations of reality. In consequence, they will always be open to discussion and further improvement.

As mentioned before, I think that Norbert Elias's process sociology, explained in his book, *What is Sociology?* (1978), offers a good example of a general theory that seems to be as free as currently possible from personal political or

personal value judgments, other than that it appears to be a good theory for analyzing human behavior. And it seems like that because it seems to work in practice. Elias used it, for instance, to explain changing standards of behavior and state development of what became France (1978b, 1982, 1983). I myself used it to analyze religion and politics in Peru during its entire known history at all levels of society (Spier 1994-95). Interestingly, my analyses were readily accepted and discussed at the University of Cusco (UNSAAC) in 1996 and 1997 when I went there to present them. Apparently in Cusco there was some cross-cultural acceptance of these ideas for understanding their own reality, which is a good sign, or so it seems to me. Other scholars have used Elias's theory for engaging in a great variety of social studies.

Yet a great deal of theoretical work still needs to be done to turn Elias's process sociology into a successful theory of human history. But the potential seems to be there. What is needed now, I think, is to compare the existing studies and discuss what has been achieved and what is still lacking. But if a better theory appears, that would be great as well. Surely, the goal should never be to stick to one particular theory, but instead to look for the best possible theoretical explanations of our common past.

Engaging in academic research

Because practitioners of academic research use this method to achieve what they see as the best possible representations of the world and its past in terms of underlying forms, at least for certain purposes, the scientific enterprise as whole is limited as a result of the first three requirements mentioned above. The world of direct experience, by contrast, including all the feelings and reactions that it engenders, is not limited by these requirements and is, in consequence, much larger in scope. Furthermore, most people on this planet are not scientists, even though they engage in certain types of thinking in terms of underlying forms, while no single scientist is only a scientist all the time and never more than that. In other words, as a result of the requirements mentioned earlier, the results of academic research are by necessity much more limited than the world of direct personal experience.

In socio-scientific and historical accounts this distinction is often not very clearly drawn. Surely, it is much more difficult within the humanities to take distance from emotions, not least because the objects of investigation have them and may show them to you, whether you like them or not. Lifeless nature as studied by astronomers, physicists and chemists do not have feelings. For biochemists and biologists, however, this may turn into a problem as soon as they start to investigate living things that do have feelings. Another personal example may illustrate this.

> As part of my biochemistry study I had to test the brains of rats for certain substances. This was a demonstration experiment. For doing so, these animals needed to be killed. Because of this, and because of possible students' sensitivities, we were given the choice of opting out, which I did, because I could not see any reason for killing animals to do experiments that were only meant to be a demonstration, while they would not yield any new and useful knowledge. I still remember the resentment I felt about this, while I usually felt no such resistance in experimenting with lifeless nature, or with microorganisms and plants. I did feel apprehensive, though, about using dangerous chemicals, including radioactive tracers. Yet in such cases we were not offered an opt out, but were advised instead to use protective measures (which were not always used).

I do not know what my fellow students thought or felt about these things. I do not remember any discussions with any of them concerning this issue. But in retrospect I realized that this was the borderline where such specific ethical problems began, namely as soon as the objects of investigation have feelings. This does not mean to say that studying lifeless nature does not entail ethical problems – it surely does, because of the effects it may have on the world. That is what all sciences have in common.

But it is different when the objects of investigation have feelings, and even more so, when they can talk, and talk back to the investigator. That may be why, I think, biologists and biochemists tend to downplay the abilities that animals have, especially in terms of consciousness and suffering, of feeling pain and anxiety, because recognizing such things might upset their own feelings.

While atoms and molecules, stars, planets, and moons do not have feelings, humans do, and they are able to express them. This inevitably implies that social scientists will have to be more involved, simply because they need to interact with their objects of investigation (which are rarely, if ever, called as such). This interaction, including the need to understand other people, requires a considerable degree of empathy, including with people whom you may find morally problematic. This empathy may be the most important skill needed to engage successfully in social science. For if the academic researcher were not able to understand all people involved in the investigation, how would she or he be able to know what was going on? At the same time, the scientific method requires as much detachment as possible. As a result, social scientists need to learn to combine both involvement and detachment as part of their academic skills.

This combination of involvement and detachment in the humanities is much more demanding than any of the tasks faced by natural scientists. Surely, also these scholars need a great deal of involvement in their work, while they may be dealing with dangerous substances, procedures, or social and ecological circumstances that can all raise important ethical questions. Such researchers may also have to cope with culturally challenging situations. But the additional involvement of dealing with fellow humans as objects of investigation is entirely lacking. As a result, the continuous switching between involvement and detachment for social scientists during their empirical research is much more challenging, while it raises a range of additional ethical questions that will need to be addressed one way or the other, simply because one is investigating fellow human beings who have their own interests.

Especially the challenges of participatory research as a cultural anthropologist are unique, or so I think. While living in one's own research field day and night, the investigator has to consequently try to put oneself into the shoes of a great variety of other people who are culturally different. The aim is to present an analysis of the dynamics produced by how all these people interact with each other. Yet the researcher may not like some of them, while some of them may not like each other either, or they may not like the investigator. Furthermore, many of them may be very poor. (Wealthy people are usually far less accessible to cultural anthropological investigation – for historians, interestingly, the situation is virtually the opposite; it is mostly the wealthy and powerful people who have left written documents or statements). These poor people may well see the researcher as a potentially welcome resource.

As a result, such situations introduce all kinds of tensions and biases which are often not made explicit. Doing all of this in continuous interaction with the people under investigation, including their reacting to and commenting on the researcher and on the investigation in progress, while at the same time seeking to take as much

academic distance as possible, has been by far the most challenging academic enterprise I have ever undertaken.[3]

Furthermore, by entering into social situations that are different from one's own experiences, and by investigating them as thoroughly as possible, social scientists may become acutely aware of many aspects of their own culture that they may have taken for granted until that time. As a result, their personal socio-emotional makeup may undergo considerable change, including finding it hard to operate within their 'own' societies again, because nothing may seem 'natural' anymore. As Tineke Luhrnman, who went through such experiences herself in Ecuador and Peru in 1985-1986, expressed it many years ago: "After one has lived in another people's dollhouse, one becomes acutely aware of the fact that one lives in a dollhouse oneself as well." Natural scientists rarely, if ever, seem to go through such experiences.

As a result, it seems to me that doing social science research, most notably longer-term participatory observations within a cultural setting that is different from one's own, places far greater demands on a wide variety of skills than any other form of science. The rewards have been equally great, though, in terms of what can be learned about that particular society; about oneself; one's 'own' society; and about life more in general. All of this presents major reasons for why it has been more difficult in social science research to take sufficient academic distance, and as a result why there is not yet a general theoretical paradigm of human history.

Final words

If we consider feelings as belonging to the domain of direct experience and academic thinking as belonging to the domain of underlying form, our daily emotional expressions fall outside of the strict academic research method, other than that they can become an object of investigation. Yet at the same time emotions thoroughly guide all these investigations. The same is the case for norms and values, including ethical judgments such as 'good' and 'bad,' 'pretty' and 'ugly,' none of which can be clearly defined from an academic point of view. To be sure, personally I find all these aspects of great importance. But they are simply not part of the academic toolbox of terms that can be used to analyze situations in academic ways, even though they play a major role in the academic enterprise as a whole.

Many people find this difficult and, in consequence, may seek to include an emotional vocabulary or social actions into the academic toolbox. But by doing so, the notion of academic research and the resulting knowledge as we know it today would be destroyed, simply because in that case virtually everything people do could be called as such. And as soon as that were to happen, these analytic terms would lose their specific meaning.

In my opinion, all students of the humanities, in fact all academics, will have to live with the fact that the academic research method will always yield a limited view of reality. Whether one finds the resulting representations of reality valuable or not is based on personal value judgements. But I hope to have shown that the general principles governing academic research are in principle the same all across academia.

I personally hope that there will be more room in the future within the academic world for researchers from all disciplines, not only to write down their academic analyses but also to reflect on their personal experiences that have led to these results, because that would enrich our insights of how the process of academic research works. Combining those two aspects in an equal

3 A report of and some reflections on my Peruvian fieldwork experiences can be found in: Spier 1986 (in Dutch) and in: Spier 1995, p. xiii-xviii.

way was perhaps the major thrust of the work of Alexander von Humboldt (1769-1859). One can argue about how successful von Humboldt and his followers have been. But it seems to me that doing so in whatever ways that appear to work would considerably enhance our views of how academic research works in practice. And that may help us to improve our understanding of the results of academic research, even though these results themselves must be based on nothing else but empirical evidence and underlying academic thinking.

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