THEORY AND PRACTICE - THINKING STYLES IN ENGINEERING AND SCIENCE

Hanspeter Schmid
Bernafon AG,
Morgenstrasse 131,
3018 Bern, Switzerland.
E-mail: h.p.schmid@ieee.org

ABSTRACT

This paper describes knowledge as an element of thinking styles, which are properties of thinking collectives. According to the theory outlined here, the choice of a thinking style to solve a certain problem is relative, but once the thinking has been chosen, realism prevails. This paper also describes the genesis and development of thinking styles and, with them, of facts.
The theoretical concepts are illustrated with two examples of thinking styles: a description of the thinking styles of circuit theorists and circuit designers (theory vs. practice), and a comparison of the thinking styles of two closely related technical societies of the Institute of Electrical and Electronics Engineers (IEEE).
Applications of the theory are also presented in this paper; they include information management, documentation tools, and writing styles, and mainly draw from the author's own experience with these topics.

BACKGROUND

Much has been said about the relationship between theory and practice, and it has been said by people who are both technically more experienced and philosophically better educated than I am. Nevertheless, I regularly thought and debated about the philosophical background of science for the better part of the time I spent at the Signal and Information Processing Laboratory of the Swiss Federal Institute of Technology, mainly for three reasons.

One is that I soon noticed the incredible discrepancy between the ideal science producing facts and objective truths, as it is portrayed at high school and even in university lectures, and the chaotic mess of contradicting and inconsistent views I found at the front of research.

Then it also occurred to me that one often finds seriously flawed papers even in the most renowned of technical journals, so often in fact that I simply cannot believe that the time pressure on the referees is the only reason that such papers get published.

The most important reason for my decision to write this paper was, however, my experience with writing my paper (Schmid 2000). For three years, I discussed several versions of the paper with my doctoral father (this is the Swiss name for the supervisor of a doctoral student). We argued for hours about the paper and its quality, but I never managed to write the paper in a way that he would agree to publish it. It will become apparent in this paper that this was not actually a personal argument, effectively we argued from two very different perspectives, the perspectives of a designer and a theorist. The same text that is clear, valuable, and true from my perspective must have looked unclear and trivial from his perspective. Finally, he stopped our discussions by saying that he could still not agree with me, but that he trusted me with having found something, and he suggested that I publish the paper on my own.

Knowing about the ultimate importance of perspectives, I saw to it that the reviewers would use my perspective, not his. This could easily be done, because the theory-practice divide has been enforced a few years ago in my field when the most important publication of the IEEE Circuits and Systems Society, the ``IEEE Transactions on Circuits and Systems," was split into two independent parts, ``Fundamental Theory and Applications" (TCAS-I) and ``Analog and Digital Signal Processing" (TCAS-II). So all I had to do was to explicitly declare my theoretical ideas as rationalised analog-IC-design experience and submit the paper to TCAS-II, where I knew that the associate editor who would get my paper would be an analog-IC designer as well. And the paper got accepted.

The problems are still not quite over, though. It turned out that the title of the paper, ``Approximating the Universal Active Element," was badly chosen. The term ``Universal Active Element" clearly comes from the theoretical realm, and one of the reviewers told me later that at first he thought ``oh no, I don't have the time to look at this kind of paper!" In his review, he wrote ``I am very enthusiastic about this paper ...." Now he was a reviewer, meaning he really had to read the paper, but many people who would find it interesting will not read it because they already lose interest when they see the title.

Coming back to theory and practice: Maybe I would never have seen the difference between the worlds of what I call theorists and designers, had it not been for a remarkable circumstance. During the years I worked at it, I showed the paper to many colleagues and found it very polarising: Some liked it a lot, some saw not much of a point in it, but there was almost nobody in between these extremes. It was even more remarkable that the ones who liked it were the ones who had experience in IC design, the ones who did not like it were the more theoretically inclined, and this in spite of the fact that the main contribution of the paper is of a theoretical nature. Most remarkably, the designers suggested improvements in the theory, while the theorists asked for design criteria.

This was when I started thinking about the difference between theorists and designers, and since I like writing and technical communication, it was inevitable that my findings would find an application in writing too.

In this paper, I will provide very few citations in the text, because I cannot easily tell where I acquired all these ideas, but I will now mention the authors whose work had a major influence on mine and cite some of their work. I will also very briefly describe the main idea they conveyed to me. These descriptions are caricatures; to get the real picture, one should rather read the cited works.

First to the people whose views I used in this paper. Yehuda Elkana: There is truth in both realism and relativism, but since every person routinely uses both at the same time, it is better to see the two as two sides of the same coin (Elkana 1978, 1981). Ludwik Fleck: Thinking is not done by individuals, but by groups, so intellectual individualism is mainly wishful thinking (Fleck 1977). Paul K. Feyerabend: The only methodology that is common to all scientists is that they do what looks promising to them, so the only conclusion one can reach when one tries to describe science by its methods alone is that *anything goes* (Feyerabend 1981, 1975). Clifford Geertz: Lean descriptions are not suitable to describe cultures, one must give thick descriptions; reasoning rationally is not sufficient, one must also demonstrate things and tell stories (Geertz 1973). Arthur Schopenhauer: Using cunning tricks, one can win a debate no matter whether one is actually right or not (Schopenhauer 1995, Schmid 1999). William Broad, Federico Di Trocchio: It is rather easy to cheat in science without being found out for a very long time (Broad & Wade 1982, Trocchio 1999).

I also studied the ``enemy," mainly through Feyerabend's work, but also directly. Karl Popper: The scientific method is to make bold assumptions and then try to falsify them; verification is logically impossible (Popper 1972). Gunnar Andersson: Science can be understood in terms of formal logic (Andersson 1988).

What I did not know was that ``distributed cognition" is actually a field of study that has a few things in common with the ideas presented in this paper, as one of the reviewers of this paper pointed out. Not knowing that body of literature, I cannot provide good references, but would like to invite the readers to follow that link by themselves.

Last, but not least, I can only recommend the very cynical treatise ``Plan your fame" by Daniel Vischer (1992), in which he shows what you have to do to become famous and maybe respected in a scientific community, and how little it has in common with the public image of science.

FACTS

According to a wide-spread, old view of science, which can be caricatured by `` `a fact is a fact' is a fact," the truth of a theory or a statement does not depend on who looks at it. Facts are there, just waiting to be discovered, and it is mainly a matter of chance who discovers them and when.

This old view is still found in many popular science texts and text books used at university, either explicitly or between the lines. It is, however, a very inhuman view, since it will, in its extreme form, lead to the belief that somebody (often the person looking at the world) is right and everybody else is wrong. The view is also historically absurd, most scientific views that were believed to be true four hundred years ago have been replaced by now, and there is no reason to assume that any of today's theories will persist longer.

The other extreme view, namely that there is no reality at all, that the truth of everything is always relative, is equally absurd. If this were the case, it would be impossible to assert even the most obvious of facts, like who ate the last cookie or whether it was the butler who murdered the duke, let alone anything more complicated, like how large the threshold voltage of a certain MOS transistor is.

Both an extreme realist view and an extreme relativist view lead to absurd conclusions. Nevertheless, both views contain some truth as well. The best solution to this dilemma is to assume that *every single person uses both views.* This mode of thinking was called two-tier thinking in (Elkana 1978), and works as follows: Once a person has selected a certain perspective from which to look at the world, realism prevails. Facts are really facts from one perspective, and as long as the perspective does not change, the facts will not change either. The choice of a perspective, however, can only be made relativistically, since there is no absolute, external-to-all perspective from which all other perspectives can be judged and compared.

Note that a relativistic choice is not the same as an arbitrary choice. Sometimes, a person may have the conscious choice between a very small number of possible perspectives, but in most cases the perspective is given to the person. But given by whom?

THE THINKING COLLECTIVE (DENKKOLLEKTIV)

We are living in a very individualistic time, and people (laymen as well as scientists working in the field of artificial intelligence) generally have adopted the main assumption of individualist psychology, namely that thinking is done by individuals. However, this view has at most bio-mechanical relevance; all thinking is deeply rooted in the social environment of a person, and because these social factors by far outweigh the individual ones, one may just as well say that thinking is done collectively.

This collective thinking is something that most people experience quite often. When close friends chat, they say things and express ideas which they cannot say, or even think, when they are on their own. When they part, this ``magic" is lost, but as soon as they meet again, it is back as if it had never been away. Together, they are more than just the sum of the individuals. They think together, they are a *thinking collective,* and the ``magic" is the *thinking style* they share (Fleck 1977). What distinguishes one thinking style from another are words that have special meanings, ideas that only have a meaning to the people belonging to the same thinking collective, insider jokes, values, ideologies, and so on. And there is also something else: in a thinking style, some things can only be thought in a certain way, and some things are even unthinkable. So while people transcend themselves when they are in a thinking collective, they also become subject to *thinking constraints.*

This becomes clearer when we have a look at larger thinking collectives. First, let us compare two large ones, namely the analog signal processing experts from the IEEE Solid-State Circuit (SSC) Society and from the IEEE Circuits and Systems (CAS) Society. The comparison is oversimplified, mainly because the thinking style of the SSC society leaks into the CAS society through the people who contribute to both societies. But the difference between the two societies is quite clear because their members maintain that difference on purpose.

Most articles appearing in SSC publications and at SSC conferences describe a working microchip. It is very difficult to get a paper published without having a working chip. Exceptions are only tutorial or overview papers written by scientists who have already published several conventional SSC papers. On the other hand, the analog papers in CAS publications that describe a working chip are in the minority. The reason for this difference is that the two societies have differing ideas of what knowledge actually is, they use different *images of knowledge* (Elkana 1981). In both societies, knowledge can come from many different sources, e.g. experience, evidence of the senses, clear and distinct ideas, tradition, authority, novelty, beauty, intuition, analogy, and so on. The difference is that experience with IC design is the most important source of knowledge in the SSC society, but only of minor importance in the CAS society. In CAS publications, even people with no design experience at all can publish new concepts for integrated circuits, provided that these concepts are intellectually pleasing, novel, or derived by analogy from CAS tradition. This again is explicitly discouraged in SSC publications: ``[The Abstract] must be factual [and include] performance data. Claims such as `new,' `advanced,' `high-performance,' and `high-speed' are not acceptable" (IEEE 2000).

The difference between the thinking style of SSC and CAS scientists is a fundamental one, because it makes them see science itself differently. The two thinking styles cannot be reconciled and *must not* be reconciled, since having different views that exist so closely together is the key to scientific progress, as will become apparent later in this paper.

Let us now look at a smaller thinking collective, the group of scientists who deal with high-gain broadband amplifiers and feedback around such amplifiers. Their thinking style has a much less fundamental trait, namely the tendency to express concepts in terms of two-port theory (c.f. Moschytz & Carlosena 1994, Cabeza et al 1994, Cabeza & Carlosena 1997, Payne & Toumazou 1996, Arbel 1996, Choma 1995, Wilson 1999). It became apparent in (Schmid 2000) that this tendency used to be a thinking constraint that actually made it impossible for the collective to comprehensively classify the most wide-spread of operational amplifiers.

Thinking styles are very important because they actually determine what facts are. Something is accepted as a fact by a thinking collective if and only if it fits into its thinking style. This is obvious in the SSC society: no chip--no paper.

So if something becomes a fact only if it is in proper style, why was (Schmid 2000) accepted for publication? One reason is that one person belongs to a vast number of thinking collectives, and the *context* decides which thinking styles are active at the time the person thinks about something. A change of perspective is sometimes easily possible, it can be caused by a perceived context change, or also deliberately or even playfully. In the case at hand, the reader is forced to shift his perspective (i.e., to abandon the two-port thinking style) by a comparison of two perspectives which shows that the one used up to that time is inadequate to tackle the problem. This is possible only because there is another perspective that fully embraces the two compared perspectives.

Being in the proper style is not only necessary if something should become a fact, but also *sufficient.* If a paper is in accord with the thinking styles a reviewer activates when he reads it, he will accept the paper. Several papers published recently in the IEEE Transactions on Circuits and Systems describe new integrated circuits which simply do not work. For some reason, the reviewers did obviously not use an analog-IC designer's thinking style, in which it is

immediately apparent why the circuits do not work. However, the papers in question were in accord with several other thinking styles that are wide-spread in the CAS society, so they were accepted anyway.
Alan D. Sokal played with other people's thinking styles in an impressive way when he submitted what he later called a ``parody" paper to a renowned sociology publication and got it accepted ([23], c.f. [24]). He did this to show that sociology is not really science because it suffers from the fact that one can deliberately (or unwittingly) publish nonsense as long as it is in a proper style. What Sokal completely neglected was that the same is probably possible in any kind of science; it is certainly possible in engineering. It is also an inevitable consequence of the collective nature of all thinking. The mechanism that prevents such things from happening too often is psychological: trust. If an author publishes too many bad papers, his reputation gets damaged, and he is not trusted anymore.
Another possible mechanism is to fix some criteria that a paper must fulfil if it should be accepted for publication at all, but since such criteria should be valid for a wide range of thinking styles, they can only be of a quite general nature, e.g., that papers on integrated circuits should only be accepted if at least AC, DC, and transient simulations are provided, or of didactic nature, e.g., that a DC characteristic showing only a straight line does not contain enough information and should be replaced by a figure showing the deviation from linearity. Note that introducing such criteria may be dangerous; they are effectively thinking constraints, and if they are introduced into an existing thinking style, the facts will change!

THE DYNAMICS OF THINKING STYLES

Thinking styles are maintained by a thinking collective. As such collectives form and develop, the thinking styles form and develop as well, and together with them the facts. Fleck described this extensively in his 1935 book whose title, translated into English, is ``The Genesis and Development of a Scientific Fact" (Fleck 1994, 1977). Four aspects of the dynamics of thinking styles are particularly important in the context of this paper: the acquisition of a thinking style, its tenacity, how thinking styles interact, and how a thinking style develops from the front of research towards popular science.
At the beginning of a new thinking style is chaos. The people involved in the genesis of the thinking style move around in that chaos by trying out many things and thinking many thoughts, without understanding what they are doing. The first non-chaotic thing they find when moving around in this chaos is resistance, they find it easier to move in some directions than in others. Then they make first assumptions about what they perceive as a chaos, they decide what they want to investigate first, or, in other words, they deliberately introduce thinking constraints. Sooner or later they will see patterns in the chaos, patterns which, once perceived, will from then on always be immediate appearances, ``Gestalten," which are then called facts. Since the source of these facts is not only the chaos, but also the thinking constraints that were introduced deliberately, facts are rather made than discovered. Therefore, the normal development of a thinking style is towards more and stronger thinking constraints and towards more intensive ``Gestaltsehen." It is interesting that a person who learns to use a new thinking style or a scientist tackling a new, complex problem normally live through a very similar process, starting with the feeling of facing a big chaos and arriving at seeing many immediate appearances. Thus acquiring knowledge goes hand in hand with both seeing a larger number of immediate appearances (in other words, knowing more facts) and being more constrained in ones thinking (Fleck 1977). All this is very similar to the concepts of information theory, which says in mathematical terms that information is gained by reducing entropy (Shannon 1998).
The reason why somebody introduces thinking constraints in the first place when he faces chaos is that he needs some continuity in thinking. Thus, the further a thinking style develops, the more tenacious it gets. There is always a tendency to adjust everything to fit into the thinking style, but this can be done more passively or more actively.
The most passive kind of tenacity is that contradictions against the thinking style become unthinkable.
Tenacity becomes more active when something that does not fit into a thinking style remains unseen, or is even kept secret. For example, a one-input rail-to-rail input OTA was recently published in TCAS-I, in which one half is only switched on for positive signals and the other for negative signals. A single transient simulation would have shown the authors that their circuit has enormous dynamic zero-crossing distortion, or, in non-engineering terms, that it does not work, but either they did not see it, or they saw it but omitted it. In the latter case, it can be that they thought it was a minor problem or a problem with the circuit simulator software, or it can even be that the omission was deliberate, although this was most probably not the case in this particular example. It was pointed out in (Trocchio 1999) that such ``small frauds" occur more and more frequently in modern economy-driven science, because nowadays many scientists need to present good results in order to get money for further research. In some cases, tweaking results unwittingly or deliberately may become essential for a scientist if he wishes to survive.
An even more active way of dealing with contradictions is to declare them not to be contradicting, sometimes with great effort. One example is the amplifier classification made in (Payne & Toomazou 1996). The widespread current-

feedback operational amplifier (CFB opamp) does not fit in that classification, but it is treated as a special case in (Payne & Toomazou 1996), as a connection of two amplifiers that do appear in the classification. Thus the authors of (Payne & Toomazou 1996) did their best to save the two-port thinking style; if they had been ready to break it, they would easily have arrived at a classification containing the CFB opamp like the one that was later presented in (Schmiod 2000) by the author of this paper.

The most active form of thinking style tenacity is when people see certain facts although they contradict other important thinking styles they use. This has happened in (Cabeza et al 1994), where the authors state that they show how the four high-gain two-port sources can be implemented, but then actually end up with implementations of only one of these sources plus three devices that are not two-port sources, but still useful, properly working amplifiers.

The last two examples must not be taken as reproaches, both groups of authors did fine work. The examples above were about an idea, the idea of amplifier classification, that was looked at from two different thinking styles (classical two-port theory and IC-design-related theory). Time will show whether the new thinking style will replace the old one, which will only happen if some of the main proponents of amplifier-related thinking styles integrate it into their perspectives. This again will only happen if they find the new thinking style valuable, which is the *only* criterion that always plays a role for the integration of new thinking style elements into existing thinking styles.

This brings us to the interaction of thinking styles. One can never say that the same thought is both true for person A and false for person B. Either both A and B have the same thinking style, in which case the thought is either true for both of them or false for both of them. Or the two have different thinking styles, and then the thought *is not actually the same thought* for the two, since it either must be unclear to one of them or they understand it differently. One of the main elements of scientific progress is certainly that thoughts move from one thinking style to another, which can only happen if a *person* gets in close contact with both thinking styles. When a thought moves into a different thinking style, it necessarily changes, becomes a different thought, and this is the main source of scientific progress. This idea can be expressed in a much briefer and more provocative form: Scientific progress only happens when one person *mis*understands the ideas of another person (c.f. Schmid 1999). Therefore one task of knowledge management must be to make sure that people are exposed to a wide variety of thinking styles.

All that has been said up to here makes it sound as if it were easy to distinguish between two related thinking styles, and as if it were easy to tell whether a new thinking style starts to exist or whether a branch of an old one develops radically. But it is not easy, and the reader should resist the urge to use my theory to make divides between thinking styles. Divides that are clear to some degree only ever exist if there are two organisations like the SSC and the CAS who are interested in maintaining such a clear divide.

The ideas about how science works that were discussed up to here are very much different from the image most people (including many scientists) have of science. How could so many people possibly be wrong about their own business? The reason for this is the way in which a well-developed thinking style develops further. When ``Gestaltsehen" develops from the initial chaos, scientists start to send around e-mails, sometimes with attached scanned hand notes, and soon the first conference and journal articles are published. The papers related to the thinking style are, however, very far from forming an integrated whole. There are many contradictions, and every scientist has a different idea of which aspects of reality are more important, and which ones should be treated as parasitic effects, or deferred for later inclusion into the theory.

At some stage, the thinking style becomes important enough for the people involved that a handbook is made, a technical book whose main purpose is to portray the thinking style as an integrated whole. Then a clear divide is made between basic theory and practice, second-order effects, and negligible parasitic effects. When a thinking style becomes handbook science, it becomes more rational, but also moves farther away from the chaotic reality.

A further, more extreme change is the move from handbook to textbook science. In a textbook, everything is made as rational and logical as possible, and as many contradictions as possible are removed. In order to do this, many details have to be omitted. The thinking style is thus fundamentally *reconstructed* for teaching and becomes far less closely related to reality.

One last, but rather important step, is to express a thinking style as popular science. For this, the reconstructed style is further simplified and expressed in popular terms. Almost everything that is really important for the people who have helped to develop the thinking style is by then omitted from the description, but now it is accessible to everybody and becomes known far beyond the group of scientists and students who are seriously interested in it.

This means: the more wide-spread a thinking style becomes, the more rational, integrated, and consistent it gets, but it also becomes far less complex and thus less realistic.

Now it becomes apparent why it is that most people, even scientists, say that science is essentially rational and scientific theories are wholly integrated and consistent. People know a very small part of science, if any at all, from the front of research, a larger part from textbooks (lectures, high school), but the major part from popular science. If they are not aware of the differences between front research, textbook and popular science, they will unwittingly see their

own project as the exception and the distorted textbook and popular views as real. This also has important consequences in how scientific papers are written, as will be shown below.
A final remark: I believe that a lot of what rational reconstructivists said about science is based on their reading mainly handbooks, textbooks, and papers written in handbook and textbook style, i.e., publications that intentionally are rational reconstructions.

ABOUT THEORISTS AND DESIGNERS

So why is (Schmid 2000) so polarising? The reason is that theorists and designers have two fundamentally different thinking styles. For a designer, every new project starts with chaos, he is used to gaining experience until he achieves sufficient ``Gestaltsehen" to solve the problem. For the designer, immediate appearance, sensory evidence, and experience are the most important sources of knowledge, and theories are just tools, like maps to a hill-walker, which give a rough impression of where to go but do not tell when and how one has to jump over a rock. If designers develop theories, they do it by rationally reconstructing their ``Gestaltsehen." This is the work methodology I used when I wrote (Schmid 2000).
Theorists are different. For them, the theories are the most important elements of science, and their most valued sources of knowledge are tradition, analogy, theoretical beauty, and logical reasoning. They are like birds, for whom the map used by the hill-walker is more than detailed enough, and who only dive down to look at details in places that look promising on the map. They often do a systematic search for such places. If theorists discuss design, they start from theories and incorporate more and more non-idealities to approach reality. A similar work methodology has been described in (Moschtyz 1977a,b).
Both the theorist and the designer can write works that have both theoretical and practical relevance, but they are not likely to really appreciate each others work. In the case of (Schmid 2000), I was asked to use my new theory to derive some general rules telling the reader which amplifier he should select for a certain application. From a designer's point of view, this request is just as meaningless as it is for the hill-walker when the bird asks him to grow wings and do some flying in order to better understand how to use ropes, crampons, and ice axes. Vice versa, the hill-walker can ask the bird to walk a few miles over scree in order to practice flight manoeuvres.
This also explains a remarkable circumstance: The main contribution of (Schmid 2000) is theoretical, but the theory is the rationalised description of a designer's ``Gestaltsehen." So it happened that the designers who read the paper found it very interesting and worthwhile, but the theorists did not really see the point. Furthermore, the designers made many valuable suggestions on how to improve the theory such that it is a better rational account of the immediate appearances they see as well, but the theorists asked for more design-related rules, since they did not, even could not see that the theory itself was already a rationalised thinking style of a designer thinking collective.
At present, the divide between theorists and designers is a very real one in the circuits and systems society, since the former mainly serve as reviewers for TCAS-I, while the latter mainly serve for TCAS-II. Naturally, knowing this, I submitted (Schmid 2000) to TCAS-II to place it in a designers' context. It was accepted, but it might well have been rejected if I had submitted it to TCAS-I. This shows how very important cunning reasoning (Elkana 1981) is, the counterpart to epistemic reasoning that is deemed ultimately important by many scientists and philosophers of science.
Again, these explanations are oversimplified. In reality, every scientist is basically capable of thinking like a theorist or like a designer. There are very few scientists who have only learnt to use one of the two thinking styles. From this point of view, calling a scientist a theorist means saying that he is *at present more inclined* towards using the theorist thinking style. This inclination can well change during a scientist's professional life, and some people can even switch back and forth between the two thinking styles.

SCIENCE AS THEATRE

Seeing the colourful complexity and irrationality of science, one starts to wonder why most scientific papers describe the results in such a rational way. All that happened along the long way to the results, all the reasons for decisions made along this way, everything irrational, disappears from the final publication, and with it disappears most of the experience of the scientist. Only a very little part of his experience remains, and *it is not the part that made ``Gestaltsehen" possible for him.*
As a result, one could never become a good scientist by reading papers alone, and this is why conferences are so important: there one can gather the information omitted from the papers by talking to the people who wrote them.

I think however, that one could easily provide some of this essential information in scientific papers, in the form of background information, like the first section of this paper. The image that guided me is that papers are mostly written like Greek dramas, but should be written like epical theatres.
A scientific paper is conventionally written like a Greek drama: everything comes out as it has to come out, destiny rules, and the point of the Greek drama is merely to describe how the inevitable happened. From the perspective of the above discussion, however, it would make much more sense to write scientific papers like Brecht's epical theatres, the point of which is to describe *why it came out as it did, when it could so easily have come out differently*. The main differences between a Greek drama and an epical theatre are the following: In the Greek drama, the actors deceive the spectators, but in the epical theatre, they *demonstrate and tell stories*. The audience identify themselves with the characters in the Greek drama, but in the epical theatre, *the audience are kept at a distance and are forced to think for themselves*. In the Greek drama, the actors remain in their roles, in the epical theatre, *the actors come out of their roles*. The greek drama draws the audience into the plot, but in the epical theatre, the audience is cosciously distanced through *narrative comments by the story teller*, inserted songs, or by other means. Finally, the Greek drama has a clear end, while the epical theatre *only ends in time, not in thought*.
The question ``why did it come out as it did, when it could so easily have come out differently?" is of ultimate importance to the person who wants to adopt the thinking style used by the authors of a paper. The reader of a paper can answer this question better if the paper contains more epical elements. Although the better part of a paper will still have to be written in the conventional, rational form, the authors should come out of their role as describers of the inevitable, by demonstrating how they work and think, and by telling the story behind their paper. This can either be done in special sections, as in this paper, or by short, personal comments by the author that are inserted into the descriptive material of the paper. The latter is done remarkably often in the papers that are considered very good and important by the scientific community.
The stepping out of the role must be made clear to the reader. While it is in fact conceivable to include songs into technical papers (c.f. Mason 1956), which had a major impact on circuit theory and also features a rhyming abstract), it is preferable to use a different writing style and, if the publisher permits, also a special layout for epical elements. Finally, the `Conclusions' section should only mark the end of the paper, but not the end of the thought, and should thus contain open questions and indicate possible ways in which the reader may direct his thoughts.
I think that technical and editorial committees should seriously discuss the possibility of writing scientific papers in an epical style and of explicitly asking for such elements in the author's guidelines of journals and conferences. Until then, it is up to every author to think about how he can introduce epical elements in his papers in a way that the result is not too unconventional to be accepted by the reviewers of a certain publication.

HOW THE AUTHOR APPLIES HIS THEORY

To show the reader how the ideas described in this paper can be applied, I will now describe a few of my own projects.

Conflict resolution

In the past, I solved a few conflicts between my thinking styles and others simply by telling and showing people that I value their view even though I do not share it. And it also helps me that I believe in the value of *all* thinking styles, even if I don't see it yet. So if somebody I talk to says something that sounds stupid, I work until I understand them. The main tool I use for this is to try and determine the other person's image of knowledge and compare it to mine. Of course, anger and laziness sometimes prevent me from doing this, but that is a different topic.

Individuality in organisations

Carol Steiner once asked me an interesting question: why is individual knowledge valued highly in some organisations but not in others? A possible answer follows from the discussion above. What other people call ``individual knowledge" is, in my words, ``knowledge that an individual brings into the organisation's thinking style from another thinking style." If this transfer should succeed, the other people in the organisation must trust the person transferring the knowledge through the time during which that person does the transfer. This can really take quite long, so first above all the person must be trusted. So individual knowledge is valued highly in some organisations but not in others because *people* are trusted highly in some organisations but not in others.

I think that real trust only occurs in an organisation when the *management* of the organisation value people highly and have true respect and understanding for all people. The management are so critical because they determine the company thinking style, so if, and only if, they really trust people, the other people in the organisation will trust too.
I think that if this is not the case in a certain company, only radical measures help. At least somebody who has this true humanity and understanding must come into the management, and *everybody* who cannot tolerate such human views must be fired. In my company, this happened by chance a few years ago: after it was sold by the former owners, who had acquired it as a part of a telecom company they bought up, but who were not really interested in hearing aids, the whole management was replaced, with the exception of the person who now is the managing director. The management we have today meet my two criteria, and both the company thinking style and lots of practical aspects of daily company life are exceptionally human.

Knowledge management

Speaking of management, what about knowledge management? In my view, knowledge is very dynamic and only a product of thinking styles and thinking collectives. It cannot be managed directly, it can only be managed through the people who make it happen. As an amateur who by chance got into the role of a part-time knowledge manager, I do only three things and nothing more: I try to understand how knowledge happens in my company or organisation, I help people see how knowledge happens, and I try to give people a good environment in which they can make knowledge happen.
The latter can mean providing people with a documentation system that lets them organise documents as best fits their thinking style. As the thinking style develops, the system may have to adapt itself. The best I could think of is to set up a web server that works like the internet, and to give it the potential to mimic chaotic and journal science, handbook science, and textbook science. To account for the chaotic and journal science, all engineers can add content to the web site as they please, from scanned hand notes over e-mail messages to papers from journals, and they can interrelate their work with the work of others as they see fit by using hyper-links. Then there is a second layer of documentation, where things that have actually been built are described, or where concept studies are kept for the future. These documents are ordered in a more planned way, in a tree structure, by editors who are responsible for a certain part of the document tree; these documents correspond to the ``handbook science." Finally, there are also official documents used to give to managers, to users of the technical components, and for quality control; they are entered into a rigid, well-defined frame. These documents correspond to ``textbook science."

Structure of single documents

This can also be done with single documents. My documents normally start with a list of changes that enables readers of previous versions to skip what they have already seen. Then there comes a chapter in the style of textbook science, where managers and engineers having very little time get a brief, rational, straightforward explanation of what I have done and what my integrated circuit does. They have to trust me on that if they only read the textbook chapter. If they need more, they can read the handbook science chapter where I describe the assumptions, results, and trade-offs in detail, so that the readers can see why I think what I said in the previous chapter is true. With this chapter, the readers can also use or re-design my circuits.
If they want to check on this too, they can read the journal and chaotic science chapter, which is nothing more than scanned hand-written notes, paper excerpts, and computer plots.

Intuition in documents

Finally, I have also started to use writing style elements that should capture the intuitive part of science. I have not yet used them in my company, mainly because I could not figure out how to do it with the publishing software we use.
In my doctoral thesis (Schmid 2000a), which I wrote in LATEX, I wrote a very brief margin paragraph for every single paragraph of the main text. This margin paragraph tells the reader what the most important aspect of the main paragraph is *for me*. Reading just the margin, a reader can get an intuitive grasp of the structure of my text. An additional benefit is that if I have trouble writing the margin text for a certain paragraph, this indicates that the paragraph does not contain a single, clear thought!
An additional style element is that every chapter has an introductory section called ``background" where I describe how I got the results. This is not a rational reconstruction, but the story of what I actually did and why. Then the subsequent sections can be in traditional scientific style.

The clear mark-up of the intuitive parts, plus an explanation of what it is, in the introduction to my dissertation, allows the reader who only wants to have traditional scientific texts to skip the intuitive material. This may be very important for the acceptance of a publication.

Brechtian refinement

I am presently working on a refinement of these style elements. One major problem of science is that people identify themselves far too much with the ideal view of rational and objective science, I am especially interested in realising Brecht's estrangement effects (the ``coming out of one's role" described above) in technical documentation.
I have not yet found a satisfactory solution that does not disturb the text flow too much. Getting the estrangement effects right is one of the main challenges of a Brecht play, so I don't expect it to be easy to do in technical writing.

CONCLUSION

The theory of thinking presented in this paper is a kind of middle way between two extremes, in three important aspects:
Facts have no inherent existence. People have tried to find objective, absolute facts in the past, but failed. Some concluded from it that there are no facts at all, but this is absurd as it contradicts daily experience. The way out of this dilemma is to state that facts exist only within a thinking style, which a person chooses to apply in a specific situation for various reasons. Facts are therefore both relative *and* real.
Individual thinking has no inherent existence. People have tried to find individual, absolute thinking in the past, but failed. Some concluded that there is no individual self, but this is absurd too. The way out is to accept thinking as a collective process and search for individuality in the way that thinking is performed by certain people in certain situations. Thinking is therefore both individual *and* collective.
Last, but not least, thinking styles and facts are not timeless. People have tried to find timeless knowledge in the past, but failed. Some concluded that there is no such thing as knowledge or facts, but again, this nihilistic view is absurd. The way out is to recognise knowledge as a process rather than a state, and facts as things that change with time. On the other hand, people using a well-developed thinking style strive to prevent changes. The nature of thinking styles is therefore both dynamic *and* static.
Classical technical writing does, however, only emphasize the real and the static. It either neglects the people altogether or emphasizes the individual side of thinking. How can we re-integrate relative, collective, and dynamic aspects of knowledge into technical writing?
I have made a few suggestions of how to do it, derived from personal experience, mainly related to technical writing and to a theory-practice duality I found in a technical society of the IEEE. These are just my first small attempts. Clearly, lots of work still needs to be done before the ideas presented here can have an effect on technical documentation. In this sense, the main purpose of this paper was to encourage the readers making their own attempts to transfer the ideas presented here into the thinking styles they use themselves.

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