Domestic PC Production in the Soviet Baltic States 1977-1992

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Abstract

The thesis argues for the necessity and value of a two-way interaction between high-level abstractions and rich historical narratives mediated by middle-range theories. The basic assumptions of critical realism are used to derive a socio-technical metatheory which, in turn, structures the synthesis of specific substantive theories. The conceptual tools provided by the Multi-Level Perspective, Analytical Sociology and (Technological) Systems of Innovation frameworks guide the study of the cases. The empirical core of the thesis consists of detailed histories of the birth, development and decay of ten different personal computer production attempts in the Soviet Baltic states roughly between 1977 and 1992. In order to generalize from the historical narratives a novel analytical technique is developed and employed. The resulting middle-range theorization locates the mechanisms and patterns of the evolution of these cases on three different levels of aggregation: intra-case, inter-case and system-level. Finally, the study makes analytical contributions to the socio-technical metatheory and provides philosophical justifications based on actual research practice for retaining the realist position.

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Introduction

In the following pages the reader will find a history and analysis of various attempts to design and/or produce personal computers (PCs) in the three Soviet Baltic states—Estonia, Latvia and Lithuania—roughly between 1977 and 1992. I will explain how and why each project came to be, how they evolved, experienced various setbacks and accelerations, and finally, how and why they were stopped, some more abruptly than others. In so doing I will cast light on the little-researched empirical domain of Soviet (personal) computing while also aiming to make methodological and theoretical contributions.

To understand the choice of topic and focus of the research I have to dwell a little on my personal background. From 2005 to 2007 I, at the time a new master's student in media and communication studies at the University of Tartu, Estonia, was engaged in research on the theories of information society. While wading through the literature I also obtained some knowledge about the history of computing, at first largely as an unintended consequence. However, at one point I realized that the experience of the Soviet Bloc was largely neglected in these accounts, whether historical or sociological. Therefore, to continue my studies I decided to focus more on empirical research and to study the history of computing in my native country, Estonia, thinking that I could cover developments from the 1950s to the early 1990s. In line with my training I aimed not only to write a history but also to derive some more general statements from the narrative. In brief: to theorize it.

Unfortunately, searching the literature in the domains of mainstream sociology and media and communication studies revealed something frustrating: although technologies were mentioned quite often in various writings there were only a few frameworks which actually tried to make them part and parcel of the theory. What seemed to be largely missing was a theorization of technology. Nevertheless various traces and references in these accounts soon led me to the field of Science and Technology Studies (STS). What started as one year in London to learn about the field in more depth ended with my becoming a PhD student in STS in Edinburgh.

By that time I had already assembled some material about the school computer called Juku which was designed and produced in Soviet Estonia. During the course of the research I found that there had been other attempts to build computers in Estonia. At this point many specific questions emerged. Why did a small country with a population of about 1.5 million people and without a computer industry decide to take up such projects when there were huge industries in the USSR devoted to computer production? What part, if any, did the school computerization initiative play in other Estonian attempts? Moreover, was the Estonian experience somewhat exceptional and if so, to what extent? Therefore I decided to re-focus my study on the comparison of various attempts at PC-building in all three Baltic countries, supposing that because of the shared historical experience of being incorporated into the Soviet Union the similarities between the three far outweighed the differences.

These are the data-driven aspects of the story. But my increasing familiarity with STS also shaped the project in important ways. Specifically, I began to observe some common traits seemingly shared by a large number of works in the field. An almost unequivocal denial of technological determinism was one of them—a case well-made and difficult to disagree with. Heavy reliance on case studies was another such trait. Rich, complex and interesting case descriptions could often be found. So far, so good. But there were six other traits which in my opinion are more problematic: arguably three would characterize the 'constructivist' camp of STS (often represented in journals such as *Science, Technology, & Human Values* or *Social Studies of Science*), one would belong to the more pragmatic innovation and management-oriented part of the community (e.g. *Research Policy*, *Technological Forecasting & Social Change*) while two would seem to be shared by both. Below, I briefly describe each of these traits.

'Data first, theory second.' This idea highlights an excessive focus on the varieties and nuances of the empirical parts of case studies. Works like this created an impression that most of the intellectual energy had been spent on story-telling and there was little left for framing the research with theoretical categories or provision of middle-range theoretical results. At times it even seemed that the theory was whatever happened to be sticking out once the story had been told. As a result good theoretical tools proved more difficult to find than I had initially presumed.

'Truth is in the detail.' Although not necessarily a consequence of the first trait, data-oriented works quite often tended to be characterized by micro-level focus (small unit of analysis), narrow temporal range or both. Sometimes these choices relied on sophisticated justifications about the supremacy of 'flat ontology' and an accompanying focus on the 'fluidity', 'contingency' and 'complexity' of various interactions. However, analytical moves like these would make it very difficult to even raise some questions, e.g. about the long-range dynamics of highly aggregated socio-technical constellations. Where information society theories seemed to have the courage to operate on macro-level but without a nuanced vocabulary for technology the situation seemed to be reversed for (constructivist) STS.

'Outstanding equals provocative equals obscure research.' No doubt that the first two qualities are definitely present in the best works. But not all that is provocative is necessarily outstanding. Moreover, as a trained journalist it often struck me that many authors seemed to be grossly violating the principle of writing as simply as possible (but no simpler, of course). Instead, many researchers seemed to enjoy tremendously being cleverly confused about the exact meaning of their propositions. However, on closer look much of what was being presented and described as 'interesting' research seemed to consist of loosely connected vague metaphors which —when translated into more mundane language—turned out to be little more than relabelled concepts from existing domains of knowledge characterized by substantial logical gaps. Of course, in the first instance there was the troubling issue of choosing an interpretation because the fuzziness of such works enabled them to be read in multiple conflicting ways.

'The more recent the better.' The innovation and management-oriented part of the community often seemed to presume that analysis of the present was most valuable by default. The scarcity of historical studies focusing on periods other than recent decades seemed to indicate the adoption of a presentist attitude. That is, a belief that ongoing events are so unique that the analysis of the more distant past is unlikely to yield enough relevant theoretical knowledge. The explicit justification for why one should reject such studies outright proved difficult to find however.

'What happens between story-telling and theoretical models is magic.' I also noticed the general lack of discussion when it came to describing how the link between narratives and eventual general statements was forged. This was even characteristic of many truly outstanding works presenting interesting models and hypotheses. That is to say that although this exclusion might not necessarily imply a poor end product, it tends to leave an impression as if theorization was a completely mystical craft not subject to any kind of formalization beyond 'read-and-interpret'.

'One case study, one contribution.' Admittedly this trait might be attributed to the current system of academic knowledge production, including strict and relatively short word limits plus incentives to publish as many articles as possible. Nevertheless I was frequently disappointed by the chasm between the richness of case descriptions and the scarcity of theoretical contributions. Sometimes there were only a few concepts, sometimes a few middle-range observations, sometimes the middle range was skipped altogether and the discussion proceeded straight to the towering heights of abstraction. In the extreme cases, the alleged theoretical contribution made me wonder whether the whole research journey could not have been substituted with a rigorous hour-long armchair theorizing session instead.

A cautionary note should be made here. These claims are not based on a systematic and rigorous literature review as the content analysis of the whole STS literature was beyond the scope of this thesis. Rather they reflect my impressions of recurring themes in various journals that tend to surface time and again in different disguises.

For the sake of brevity I have also omitted references to specific works here. However, each theme is addressed in one way or another in different parts of the thesis in more detail: claims 1 and 3, having more to do with the grounds for omitting certain frameworks, belong to the chapter on theoretical critique (not included in this thesis, see below). Claims 2, 4 and 5 are addressed in different sections of chapter 4. This chapter, along with chapter 5, illustrate the sixth point. Of course, there is a danger that my reading of STS has been selective and has created a misleading picture of the domain: should this really turn out to be the case, at the very least these beliefs have provided some sensitizing inspirations. In what ways?

First, I do attempt to take seriously the need to rely on middle-range conceptual tools and produce middle-range findings. However, I also acknowledge that all such theories rely on higher-level presumptions which in turn rely on even higher-level ones. These issues can be and often are ignored in practice but it does not mean they can be escaped from. I would like to avoid a situation where a synthesis of middle-range theories falls apart on closer inspection because it turns out to be based on different and incompatible ontological and/or epistemological presumptions.

Second, although detailed studies and micro-theories are indeed important, I do not think that STS's (implicit) drive to 'micro-everything' would be a useful *a priori* stance. Instead I hold that analysis operating on multiple levels of aggregation can offer different and complementary results.

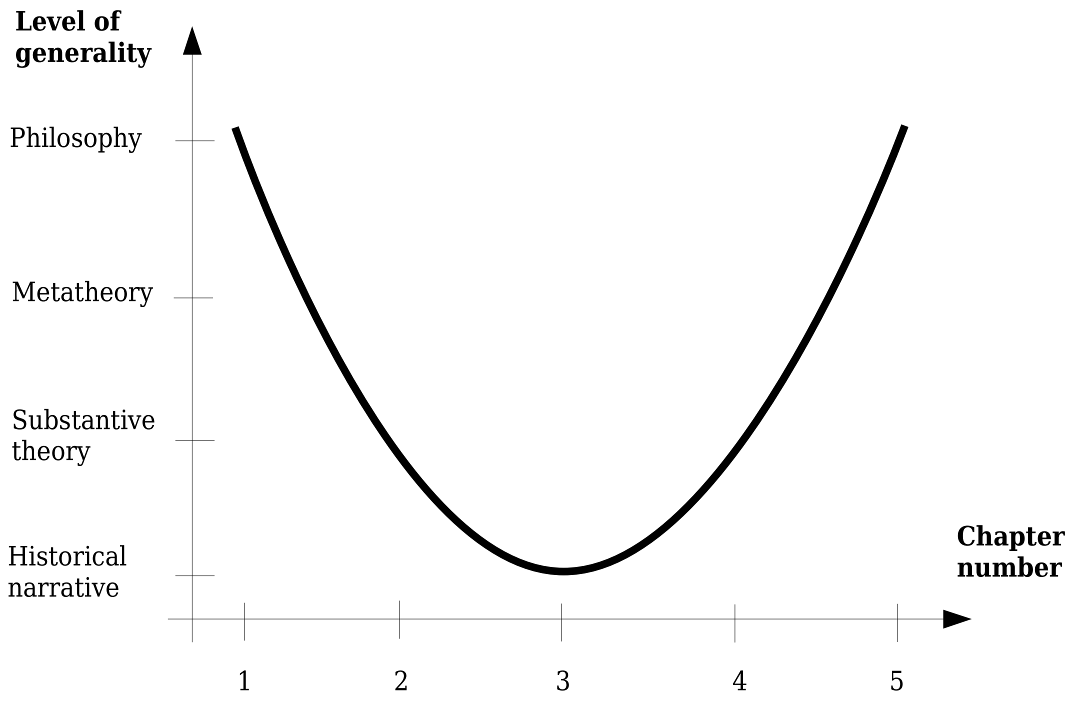
Third, I am willing to accept being 'boring' if that means a preference for borrowing from specialized domains of knowledge and synthesizing rather than reinventing more metaphors. Prior experience has shown me that the social sciences offer an array of solutions. The trick is to recognize them as such in relation to a specific problem. Therefore not only is the creation a form of art—so is drawing connections and making translations between different theories, so is synthesis. It often turns out in the process that such synthesis is far from straightforward and therefore many little theoretical and methodological études must be made. In doing so, I will try to substitute as clear writing as possible for 'interesting' obscurity although the word limit of the thesis means that occasionally the text will be quite dense.

Fourth, it is true that the events covered are relatively recent. However, they took place in a system economically, politically and culturally quite different from that of the West. This might raise the question about the significance and applicability of the findings. I would argue in turn that actually many theories derived from a capitalist empirical basis at least partly operate at the level of generality in which the distinction between the two systems disappears. Thus it is not only possible to tailor the latest theoretical vocabulary to the analysis of historical events but also to enhance that very vocabulary as an end result. That is to say that I deem the ideas derived from middle-range analysis of socialist countries to be applicable to Western ones, although this thesis does not aim to test this claim directly.

Fifth, I believe that the analysis of historical narratives can be made more rigorous. This does not guarantee a remarkable result, but the same is true for quantitative approaches which have nevertheless developed very strict methodological guidelines. By outlining the progression of the analysis hopefully the choices made and not made, the good and the questionable ones become more apparent.

Sixth, and perhaps most importantly, I do not wish to take the safe path of a single empirically-minded focus. Once again it is experience telling me that the research is akin to a journey in which bits of knowledge from various sources operating on multiple levels of abstraction intermingle. One learns considerably more on such journey than the focus on immediate close-to-data results would enable one to show. The justification of the relevance of the findings becomes more difficult but if the amount of actual substance thereby increases it is a trade-off that I am willing to make.

So how do I intend to put all this into practice? In my mind, the structure of the thesis is like a U-shaped curve in which the first half proceeds from more general to more specific and the second half the other way round:



The first chapter focuses on frameworks operating on different levels of abstraction and the relations between them. I will argue that it is possible to see specific substantive theories as nested in socio-technical metatheory, which in turn is nested in philosophy. Beginning from critical realism as a philosophical foundation I will present seven metatheoretical theses that in my opinion could represent the historically crystallized lessons of STS. I will then employ the conceptual tools of Multi-Level Perspective on socio-technical transitions (Geels 2005a, Geels & Schot 2010), the Desires-Beliefs-Opportunities framework from analytical sociology (Hedström 2005, Hedström & Bearman 2009a) and to a lesser extent (Technological) Systems of Innovation (Carlsson & Stankiewicz 1995) to apply them to Soviet Baltic PC construction efforts. To my knowledge, this is the first time that analytical sociology has been applied to technological change.

The substantive theories themselves are used to offer explanations on three different levels of aggregation. Therefore, the specific research questions are as follows:

1) What explains the success or failure of each PC project? What are the patterns of case development? What are the respective intra-case mechanisms?
2) How were the dominant lines of PCs established? What are the patterns of interaction of cases in each country? What are the respective inter-case mechanisms?
3) How did the Technological Systems of Innovation evolve in each country? What are the patterns of system-level development?

The second chapter discusses the issues of proceeding from conceptual tools to historical narratives. Various fields such as management studies, political science, history and mainstream sociology have addressed the question in a manner the STS community might find useful. More specifically, I will elaborate on the methodological criteria of critical realist study, the nature of process theory, mechanismic explanation, case study and the issues pertaining to balancing and evaluating different types of evidence.

Chapter 3 provides empirical flesh for the theoretical skeleton. Honouring STS's strong traditions of in-depth narratives I will provide the histories of domestic PC design/production for each country. Since most of these were related to the Soviet school computerization initiative in mid-1980s these developments will also be covered to some extent. As a result I will offer novel historical knowledge on the topic, which has been little-studied to date.

After a long descent the fourth chapter begins to climb up the ladder of abstraction. Starting from various analytical strategies involved in generalization from the narratives and the identification of different outcomes of such a process I will then offer an analytical technique for reaching those outcomes. I will argue that this technique enables the avoidance of 'data asphyxiation' (Pettigrew 1990), a hazard for many 'bottom-up' analytical strategies. Most of the chapter illustrates the technique in practice. Content-wise, I will proceed from intra-case analysis to inter-case analysis to system-level dynamics.

Chapter 5 illustrates the point that—provided enough attention is paid to the matter—it is perfectly possible to achieve a more sophisticated metatheory and an understanding of one's philosophical groundings by the end of the research journey. It does not imply that such generalizations need to be derived only from the historical narratives: these ideas can emerge from various sources and develop in parallel to the middle-range analysis, only to mature by the very end. It means that these developments did not (and logically could not) play their part in shaping the current empirical analysis. Their function is different: to increase the potential clarity of future works. I will reflect on the basic components of the metatheory, the distinction between different types of rules and their diffusion/evolution. I will also show how the analysis can be used to raise a number of critical questions about the meaningfulness of retaining one's realist position. This issue will be addressed at the end of the chapter.

The reader well-versed in STS will no doubt notice the general lack of two 'native' theories, namely Social Construction of Technology and Actor-Network Theory. The reason is quite simple—I just think that the wholesale adoption of these frameworks creates more problems than it solves. The approach outlined in chapter 1 allows for more theoretical nuances, while also being able to take into account the sensitizing qualities of SCOT and ANT. The detailed analysis that led to these conclusions is found in the 'lost chapter' which I have omitted because of the word limit of the thesis. This chapter is currently available online as a working paper (Kanger 2012).

The concluding chapter points out the greatest shortcomings of the work, discusses the significance of the findings, relates them to existing theories and indicates possible future research avenues.

A final note of caution: the scope of the thesis means that it could offer interest to specialists from many fields. Ideally the following pages should not only speak to the

STS community but also to mainstream sociologists and historians of technology. This means that different readers are likely to focus their critiques on different aspects depending on their disciplinary background. Of course, such specialized critique can be and often is most valuable. However, I would also encourage the reader to try to assess the endeavour as an integrated interdisciplinary whole carrying the message that it is possible, desirable and useful to think big even when researching small. In the long term there is a great deal to be gained from unleashing the full potential of one's cases.

1. Theoretical framework

This chapter outlines the theoretical framework for making sense of the empirical data. I aim to present a systematic, rigorous and clear path from the most general principles to substantive (middle-range) theories devised for a specific task. I will argue that more general principles act as vessels for lower-level claims, limiting their scope to some degree. However, in those vessels much flexibility remains for the researcher to pursue various ideas and explanations.

A brief discussion of the relations between philosophy, metatheory and substantive theories opens the chapter. This includes a justification of the necessity of such an agenda in the first place. Then some principles of critical realism will be presented, followed by an outline of a socio-technical metatheory. Finally, the conceptual tools of Multi-level Perspective (on socio-technical transitions), Desires-Beliefs-Opportunities framework and (Technological) Systems of Innovation are argued to provide a good starting point for conceptualizing historical narratives.

1.1 Three levels of abstraction

The starting point of the following discussion is Giovanni Sartori's 'ladder of abstraction'. The basic idea is simple enough—taking an example, a red apple can be classified as a member of a set of red apples, a set of apples, or a set of fruits. To put it more formally: *"We make a concept more abstract and more general by lessening its properties or attributes. Conversely, a concept is specified by the addition (or unfolding) of qualifications, i.e., by augmenting its attributes or properties"* (1970: 1041). In other words, if one wants to extend one's classification to more objects, one needs to reduce the properties that count (e.g. one needs to give up the colour specification in order to classify something as an apple).

I would argue that this idea can be applied to the social sciences in general. Consider the differences between the following claims: 1) security gates in a retail store help to reduce the number of thefts; 2) technology affects human action; 3) entities with differing causal powers exhibit influence on each other. Statements 2 and 3 can be characterized as more abstract versions of the first one. 'Technology' is a general term including but not limited to security gates; entities, in turn, include but are not limited to technologies. Abstractions like this enable us to spot some basic commonalities between what would otherwise seem widely different phenomena. And although these commonalities might be far too abstract to have a direct application they nevertheless provide a structuring frame for more specific claims, thus potentially leading to a more coherent and explicit overall framework.

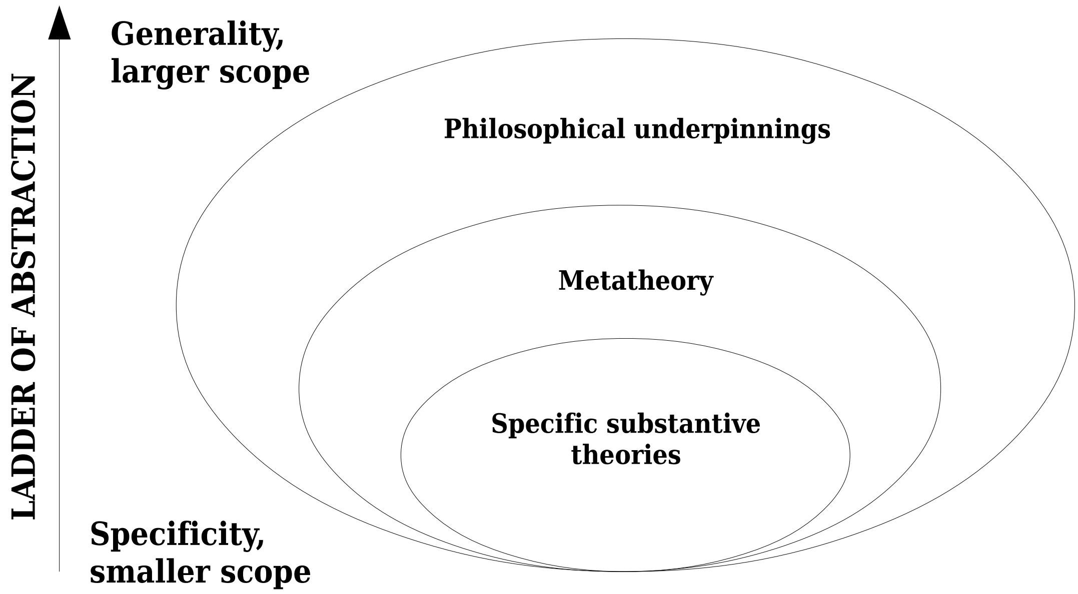
In principle the number of these levels of abstraction can be infinite. In standard (sociological) practice, however, the usage of terms like philosophy, metatheory and middle-range theory seems to indicate that there are at least three domains taken to be sufficiently different from each other. I understand philosophy as a discipline dealing with the fundamental categories of thought, establishing structured frameworks of Being and Knowing on the highest level of generalization (e.g. Bhaskar's critical realism (1975)). Metatheory is understood as a general theory aiming to establish the common vocabulary for a certain knowledge domain (e.g. Luhmann's theory of social systems (1995) as a special case of systems theory, but applicable to a range of widely differing social subsystems at the same time). I call the third specific substantive theories, defining them as sets of interrelated concepts aiming to describe, explain and/or predict some natural and/or social phenomena (e.g. Geels's Multi-level Perspective on socio-technical transitions (2005a)). The relations between the three are visualized in figure 1.1.

The nested circles serve to illustrate that while the domain of applicability decreases as one moves from philosophy to metatheory to specific substantive theory, the number of specifications and distinctions made increases at the same time.[1] Philosophical propositions can be applied to the widest range of different circumstances, yet their abstract nature also means that when it comes to analysing

1 This implies that the questions of the level of abstraction and the unit of analysis (micro to macro) should be kept separate. It is possible to conceive highly general and highly specific micro or macro theories (Brey 2003).

specific empirical situations they remain far too general, losing much of the information that could be usefully accounted for. While metatheory is more specific in some aspects, by setting the frame of reference for quite a wide domain it too suffers from losing too much information when directly applied to empirical phenomena. The specific substantive theories are the ones devised for analysing certain types of empirical phenomena and are therefore closest to the data.

Figure 1.1. Three levels of abstraction



The current study will be explicitly guided by all of these levels. However, one could question the meaningfulness of doing so. Specifically, one could ask what is to be gained from such an effort? Is it not overkill considering the specificity of the empirical problem? As a response I would stress three advantages of the approach: coherency check, increased sensitivity and transparency.

The first function served by higher-level abstractions is that they allow one to reflect on whether the synthesis of lower-level claims is logically consistent. My experience tells me that with the help of more general theories the commonalities and differences between specific theories can often be understood better. Moreover, these tools help one to understand whether the compatibility of differences is logically necessary or not. Thus, the simultaneous application of Actor–Network Theory with its inscribed 'flat ontology' (e.g. Latour 2005) and the notion of multi-level social reality would quickly raise doubts about the fundamental compatibility between the two approaches. One the other hand, the difference between theories like Large Technical Systems (Hughes 1983) and Multi-level Perspective (Geels 2005a) seems to be mostly about research focuses and levels of aggregation (system-internal vs. niche-regime-landscape interplay, see below), having no built-in contradiction.

The second function concerns the informative value of higher-level abstractions. That is, frameworks like this can sensitize the researcher to the aspects that his or her research does and does not but could or should cover. What I have in mind here are very basic issues. For example, the study about the impact of a certain technology on human practices excludes many analytical questions like the role of humans in creating, maintaining and diffusing the technology or the co-evolution of social norms and technological artefacts. A sufficiently nuanced metatheory can illuminate for us the aspects such impact studies might have missed and at what cost. Simply put: seeing the big picture helps us to contextualize local theories.

And third, laying cards on the table early on enables the readers to better assess whether the stated principles are in fact consistent and whether they differ from actual research practice. Previous work with various theories has taught me that it can be dauntingly difficult to trace claims back to their premises. By positioning my research as thoroughly and explicitly as possible, I am trying to decrease the amount of required effort on the part of the reader.

At this point some important qualifications should be made. First, I do not want to claim that being explicit about one's philosophy and metatheory is a necessary precondition of progress in STS (or in fact in any domain of knowledge). Excellent, interesting, intriguing and substantial results can be and often are achieved while remaining wholly at a middle-range level. However, there is a certain risk: if one is unable to specify one's presumptions one risks becoming enslaved by them. That might not only mean the lack of awareness of alternatives, but also the existence of unacknowledged logical contradictions. That this hazard has materialized quite often is illustrated by Wyatt and Balmer's criticism about the scarcity of middle-range theories in STS: *"How can the author possibly think it reasonable to use concepts from completely different normative and epistemological* [and ontological, I would add] *traditions in the same case study?"* (2007: 620). The advantages of the three-level approach—coherency check and increased sensitivity—simply enable one to reduce this threat.

The same consideration is in play when responding to possible fears that the framework will be too rigid and exclusive, favouring one viewpoint and not letting the data speak for itself. And while it is indeed true that every choice manifests some preferences, this is equally true for every kind of research. A 'grounded' approach does not guarantee success: it can equally well lead to being blinded by one's cognitive blinkers. Hence I prefer to adopt the stance of knowing and of risk being over-guided by existing knowledge. The chosen three-level approach, however, leaves much room for difference and disagreement. Moreover, even my preliminary middle-range theoretical synthesis found in section 1.4 only acts as a sensitizing device that will be used to make more specific theoretical statements over the course of data analysis (chapter 4).

Third, although the space created by high-level abstractions is vast, it is not infinite. Therefore, from time to time, unexpected findings can create conditions in which the basic assumptions of higher-level frameworks become questionable. Thus, in a sense we are indeed free to choose our basic assumptions, but this does not mean that 1) the explanatory power of all foundational assumptions would be the same (hence the reason for choosing some and not others); 2) we should not revise our basic assumptions on the basis of our increased understanding of the world. It is not incidental that some of the ideas to be discussed below emerged from backwards reasoning: Bhaskar (1975) analysed scientific experimentation in order to deduce the nature of reality so that such an activity would make sense in the first place; Kroes (2010) used engineers' descriptions of artefacts to theorize the dual nature of the latter. True, the very generality of high-level frameworks makes them relatively immune to the results of substantive theories. But if such a situation nevertheless occurs, it is the philosophy that needs to be revised. In sum: instead of granting them immutability, I advocate the mutual informing of philosophy, metatheory and substantive theory, while acknowledging the flexibility of high-level abstractions. Hopefully the potential and actual tensions can provide a fruitful impetus for an ever-developing, ever-nuanced and ever-cumulative account of the varieties of socio-technical interaction.

Fourth, the scope of the endeavour means a lot of eclecticism: borrowing from many domains, making choices about what to include and to exclude, not exploring certain nuances to full extent and so on. Here I concur with Turner in that *"eclecticism is far preferable to the current scholasticism in metatheorizing that, ironically, becomes highly parochial as scholars dare not tread outside the vocabulary or boundaries of a particular theory or intellectual tradition"* (1990: 44–45). And while Turner wrote this more than 20 years ago, the challenge is still the same: going beyond single approaches, uniting their strengths and discarding their weaknesses.

I have always imagined the proposed framework as an ironclad, water-resistant and rustproof Swiss cheese. From my favourable point of view, it is designed to be a logically consistent, seamless, massive integrated whole. Practically, however, it is bound to contain countless holes that specialists from different domains can criticize. But what must not be missed in the process is the value of the edifice as a whole, which provides an intellectual arena, a structured analytical toolkit that, by drawing connections between various levels of abstraction, demonstrates that one can be close to empirical data while not losing sight of grand ambitions and issues. That being said, the question of the ingredients now needs to be taken up.

1.2 Level one: critical realist philosophy

The first three principles are borrowed from the early version of critical realism[2] (Bhaskar 1975, 1979) and have been formulated by Thomas Brante as follows:

1) There is a reality existing independently of our representations or awareness of it (ontological postulate); …
2) It is possible to achieve knowledge about this reality (epistemological postulate);
3) All knowledge is fallible—and correctable (methodological postulate) (2001: 172).

These propositions enable one to specify the position of the researcher and establish the meaningfulness of scientific enquiry, while being aware of the dangers it entails. First and foremost, they enable one to make a distinction about reality (or being) and claims about reality (statements about being). Thus it immediately becomes possible to ascribe causality to entities independent of anyone's perception (including that of the observer) and thereby to conceptualize some properties of the entities as non-negotiable (that is not voluntaristically produced by the actors/observers). To take a morbid example from Mahner and Bunge (2001): if Jones took too much arsenic he would eventually die, independently of whether we are there to observe it or whether he himself is aware of the fact of his taking the poison. Nevertheless, when the act is observed we can ascribe the causal power to kill Jones to the arsenic and not our ideas about it. (Of course, by stating it one is indeed making a knowledge claim about what happened, but in doing so one has not produced the lethal capabilities of the poison itself.)

The third proposition serves to remind one that there is no necessary, simple and non-negotiable correspondence between being and our statements about it: we can always under-estimate or over-estimate the properties of reality in our knowledge claims, and hence the latter are in the need of constant revision (e.g. if it turns out that Jones

2 These principles are adopted as axioms and therefore will not be justified themselves to avoid infinite regress. However, I note the possibility that some other philosophical approaches might also agree with these postulates.

actually took aspirin instead of arsenic, our estimation about the cause of death will have been wrong).

Considering these assumptions, one can distinguish between three domains from the viewpoint of the observer: 1) empirical—events and entities that are observed; 2) actual—events and entities that can be observed in principle but are not; 3) real—mechanisms which give rise to events and causal powers of entities, which exist but can not necessarily be observed. For example, if someone changes his or her desire to study in the university after a failed attempt to get accepted, I would have a reason to suspect a 'sour grapes' mechanism at work. Alas, for obvious reasons it would be very difficult for me to observe it directly. Similarly, a biochemical mechanism would explain the sequence of processes mediating the intake of arsenic and Jones's subsequent death. However, not every observer (say, a 12^{th} century medic) would be able to detect and formulate it. Nevertheless, it does exist and exerts causal influence.

These distinctions have further implications: 1) when observing empirical phenomena, usually we do not encounter a single mechanism but an interaction of several ones (a classic example is the falling of a leaf which is affected by gravity, winds, air friction etc., so it is not easy to infer the law of gravity from that particular occurrence), meaning that; 2) a number of mechanisms can exert influence on the eventual outcome, although we might (initially) only have indirect means of inferring their existence (e.g. theories, thought hypotheses etc.); 3) a single mechanism might not necessarily manifest itself in every situation because it might be neutralized by a number of others (e.g. a rational behaviour of an individual might be abandoned under group pressure) or because it might not be realized at all, thus remaining a potentiality (just because I am not speaking aloud at the moment does not mean that I do not have the capability of doing so); 4) the same outcome may be realized by the interaction of different types of mechanisms (e.g. market equilibrium can be achieved through individual actors maximizing their preferences or it can be imposed by the government).

Overall the position adopted here is one of a cautious optimist—a distinction is to be made between entities and ideas about these entities, although it is also being admitted that establishing the correspondence might turn out to be highly misleading. This stance enables one to avoid the ontic and epistemic fallacies—beliefs that there is either a one-way train from reality to our knowledge claims, or that the latter are completely arbitrary (Groff 2004: 19). Furthermore it sensitizes one to the complex relation between manifest events and underlying mechanisms.

However, apart from general analytical distinctions, these principles tell us little about the kinds of entities and properties to be observed, their interrelation and interaction. They apply equally well to all scientific domains, excluding the more specific features of socio-technical (or more generally, socio-material) processes. The specification of these processes is already a metatheoretical task.

1.3 Level two: socio-technical metatheory[3]

This level should be seen as an application of critical realist principles to socio-technical processes on the one hand, and as a set of principles common to any socio-technical interaction on the other. As such they provide a structuring frame for the synthesis of specific substantive theories constituting the third level.

In brief, the metatheoretical theses are formulated as such:

4) The three basic causal forces implicated in any socio-technical process are actors, technologies and rules (causal force postulate).
5) These causal forces shape each other mutually (causal force relations postulate).
6) When characterized by a certain structure, characteristic mechanisms, boundaries and emergent properties, some sets of these causal forces can be conceptualized as systems or networks.[4] The boundaries separate the system

3 This section is a continuation to and extension of my previous work on socio-technical metatheory (Kanger 2009).

4 I sidestep the question of differences between systems and networks. For current purposes they are treated as synonyms. In further discussion I will use 'system' and 'network' to denote different levels of aggregation (see below and chapter 4). See also Joerges (1999a) for the similarities

from its environment (systemicity postulate).

7) The systems/networks differ in their relative sizes (levels of aggregation) and can constitute nested hierarchies (systems of sub-systems of sub-sub-systems) in which each new level shows novel emergent properties (micro–macro postulate).
8) These different systems/networks can interact. In cases of nested hierarchy (systems not sharing the same level of aggregation), the interaction is vertical. In cases of parallel systems/networks (sharing the same level of aggregation), the interaction is horizontal (system–system interaction postulate).
9) In any given moment of time the processes taking place in the system/network are enabled/constrained by its conditions of action (i.e. socio-technical structure), which is itself an outcome of a multitude of previous interactions (structure postulate).
10) In the course of a system/network–environment interaction, the actors draw on existing structure, transforming or reproducing it through their actions. As a result, a co-evolution of all entities occurs (basic interaction mechanism postulate).

I will now explain each of these propositions in more depth, beginning with definitions. First, an actor is understood as anyone to whom agency, that is a capability to act, can be ascribed. In other words an actor *"is an entity that in principle has the means of formulating, taking and acting upon decisions"* (Sibeon 2004: 4). This definition also allows that actors can be either individual or collective (e.g. organization, state). Technology is generally understood as a *"configuration that works"* (Rip & Kemp 1998: 330). This is to say that technologies have dual nature *"because they are, on the one hand, physical structures that realise, on the other hand, functions, which refer to human intentionality* (Kroes 2010: 55). So in order to be characterized as a technology, the physical properties of an entity are not enough—it also needs to be complemented by functional properties (whether these are ascribed by actors or observers). Moreover, there is no one-way relationship

between the use of these terms in Large Technical Systems literature.

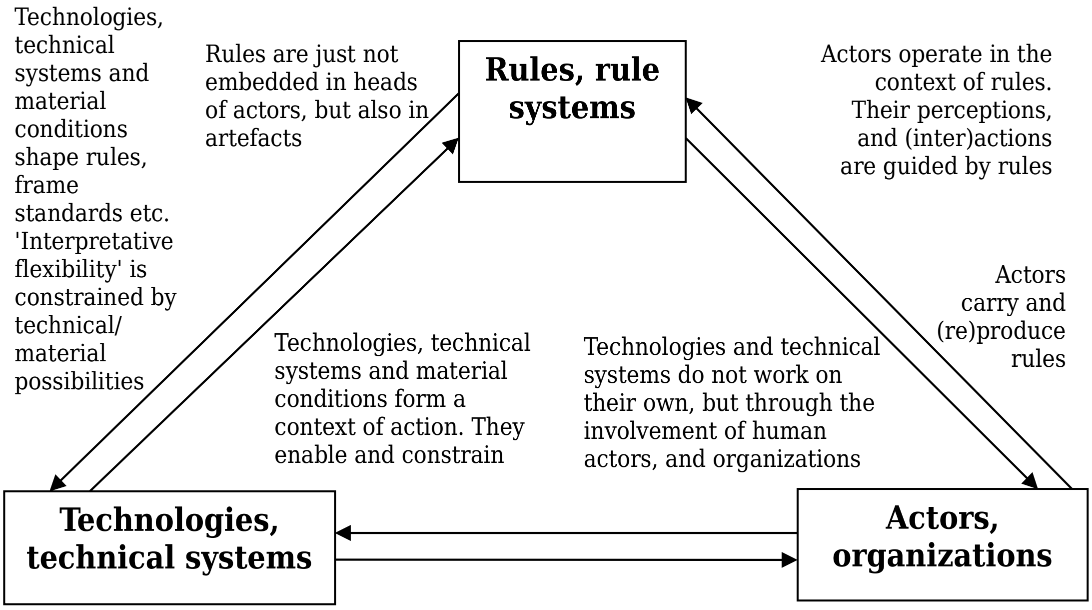
between the two: the same function can be realized in a number of ways and the physical structure of an entity shapes but does not determine what it can be used for, i.e. it has 'interpretative flexibility' (Pinch & Bijker 1984). Finally, a rule can be defined as a tacit or explicit prescription which guides the enactment/ reproduction of social life and is manifested in patterns of practice (partly from Giddens 1984: 21).[5] A rule essentially simplifies complex human experience: instead of making the actor take into account all relevant aspects of every situation and decide on the appropriate action on each turn, it provides a cognitive short-cut instead (especially in conditions of increased uncertainty). As an actor is capable of decision-making without being conscious of it then it can be said that a rule can be tacit or internalized, although at any time a shift to an externalized state (and back) is possible in principle.

The above classification implies that the components of socio-technical interaction can be divided into two types—interactive and indifferent (Hacking 1999: 103–107)—in which the first can be influenced by the descriptions about them and do the same to others (e.g. actors' practices can inform a theory which in turn can alter their subsequent behaviour) and the second cannot (a lamp does not start to glow brighter when you compliment it). Whereas the former are capable of 'formulating, taking and acting upon decisions', the latter are not. This is not to say that technologies or rules cannot affect our behaviour; it is to say, however, that they lack agency, a capacity to choose to act otherwise. Drawing on the synthesis of Frank Geels, the general ways in which these components interact are outlined in figure 1.2.

Two quick qualifications should be quickly made. First, the category of technologies and technical systems also includes material resources, as the postulated effect of the resources is similar to that of the technologies. And second, by including actors and organizations, artefacts and technical systems, rules and rule systems, this figure already hints at the micro–macro distinction. That is, in principle these interactions can take place on different levels of aggregation, from single individuals and artefacts to worldwide socio-technical networks.

5 The reasons why I have excluded the finitist take on rules are discussed in chapter 5.

Figure 1.2. The mutual shaping of actors, technologies and rules (adapted from Geels 2004: 903)



If technologies and rules are not able to exert agency, it follows that their causal significance must somehow be mediated by the actor. Even when the exact mechanism is left unspecified, one can point out situations in which technologies do shape human behaviour, including inspiring of novel possibilities, stimulating of new desires, blocking the achievement of certain goals, and so on. The same goes for rules, which provide the repertoire of action in certain contexts. In the last instance, however, it is the actors who create, diffuse, use and modify technologies, and follow and transform the rules. Since agency also means the possibility of a choice, then specifying actors' material conditions and social norms is not enough to predict their behaviour. The capability of choice lies with the actor. Whether and for what reasons this choice is not always exercised is another question.

This leaves the relationship between rules and technologies. One of the major insights of STS has been that preferred patterns of practice can also be 'encoded' (Mackay & Gillespie 1992) or 'inscribed' (Akrich 1992, Latour 1992) into technologies. For example, unless one wants to wreck one's car, the speed bump enforces the rule that one should drive at low speed in residential areas. On the other hand, new technologies can disrupt existing conventions—for example, information technologies have facilitated the free flow of information to the extent that making people pay for various (digital) products has become difficult. Alternatively, various rules can be built around the possibilities of new technologies or existing material conditions: we may well see the alleviation of copyright laws should widespread piracy continue to be unstoppable, or impose lower speed limits in mountainous areas to prevent accidents.

Here a brief detour must be made. Namely, if actors and technologies both have a physical manifestation that makes it relatively easy to ascribe causal significance (but following the above definition, not necessarily agency!) to both of them, then the question about the ontological basis of rules is more problematic. Elder-Vass (2010a, ch. 6) has argued that rules and norms should not be seen as independent entities, but as causal powers of norm circles enforcing them. In other words, we follow rules and norms because of expected or actual sanctioning from a certain group should we fail to do so.

Wishing to avoid extended debate on the matter, I will briefly point out three counter-examples that problematize this argument. First, one could imagine individual-specific rules (e.g. always tie your shoelaces with one hand) in which case, of course, the bearer of this property could not be a group. Second, Viskovatoff's suggestion that *"rules can be and often are followed without reflection, either out of habit—simply because doing so has worked in the past—or out of simple time pressure"* (1999: 499) indicates a possibility that certain rules become internalized to the extent that they continue to be followed even when no sanctioning group is or even could be nearby (e.g. provided the necessary equipment, some people would continue to hold a fork in the left and a knife in the right hand while eating even when stranded alone on an island). And third, we may adopt a rule simply because we feel it is beneficial, not out of fear of sanctioning (e.g. an agreement between parties on a shared industry standard). That being said, however, I do agree that rules must have bearers: they cannot be conceptualized as independent entities but are always anchored in individuals or (usually) groups.

For current purposes, I find the main significance of the category of rules to lie in their difference-making abilities. That is, the presence or absence of a certain rule can make a difference to the action of the unit in focus. Yes, rules require a bearer. But is the combination of a potential bearer (actor) with an actual causal power (rule) that can make a difference to the outcome[6]. And these properties are not fixed—their creation, diffusion and abandonment takes place over time. For these reasons I find the inclusion of rules as an intermediary category that increases the overall detail of the metatheory justified.

The third thesis adds another specification: namely, it might happen that some actors, rules and technologies become aligned to each other to constitute an interactive and interdependent whole in which a change in one component will influence others (e.g. introducing new legislation and enhanced surveillance techniques might affect the behaviour of the downloaders of illegal content and redress the balance of power between them and the producers). These wholes can be conceptualized as socio-technical systems or networks. Most simply put, *"a system is a complex object whose parts or components are held together by bonds of some kind"* (Bunge 2004: 188), either material or social (conceptual). That is, a system consists of components and relations between them.

But what distinguishes a system from non-system is the fact that by joining together some entities it shows some novel qualities. These are called emergent properties, *"properties of wholes that would not be possessed by the parts, individually or collectively, if they were not organized into this sort of whole"* (Elder-Vass 2007a:

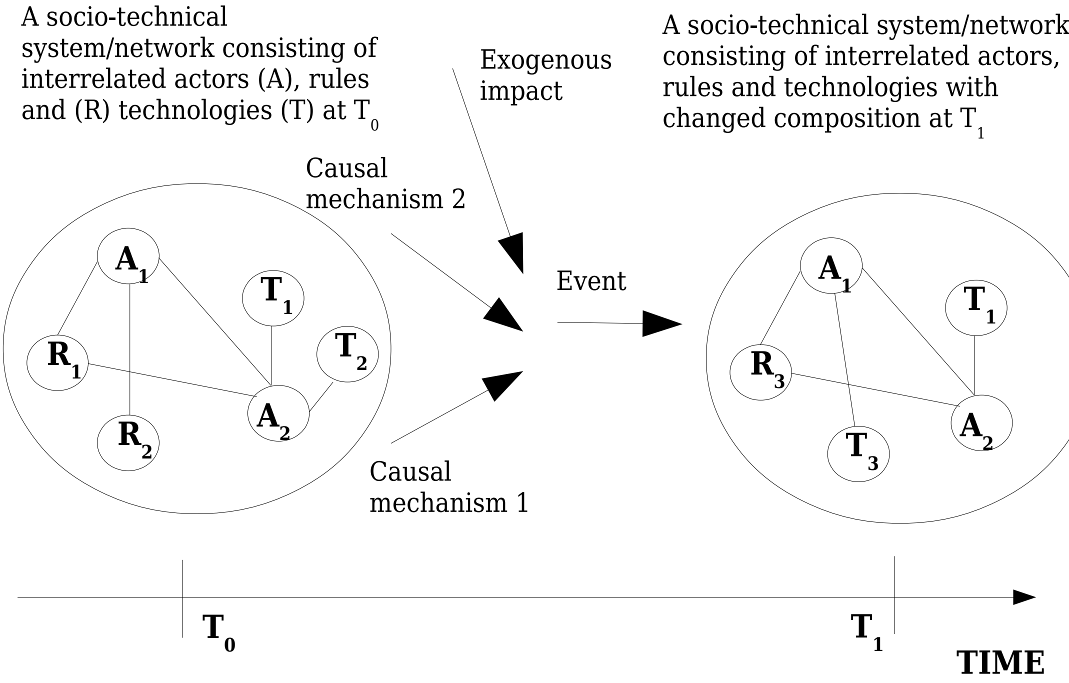
6 Strictly speaking even that might not be the case when someone mistakenly believes that a certain group follows certain rules and proactively adjusts their behaviour. More importantly, one could argue that since actors must always be *"present, irrespective of the outcome"* (Mahoney 2008: 431), their causal significance is trivial when conceptualizing the impact of rules because it is the latter that make a substantial difference to the outcome.

415). Organization is a simple example: its members behave differently as they would individually, the tasks of the worker and the manager are aligned to each other and the actions of the members represent the organization as a whole. In this case it is the individual members plus the characteristic relations between them (e.g. division of labour) that define the entity with emergent properties.

But even when taking into account components, relations and emergent properties, something is still missing from the picture—the arrow of time, the processes. This is where the notion of causal mechanism reappears. *"Causal mechanisms are processes that depend on interactions between the parts, interactions that only occur when those parts are organized in the particular way that constitutes them into wholes that possess this emergent property"* (Elder-Vass 2007a: 415). Hence a causal mechanism is a characteristic process of an entity by which the latter manifests some of its causal powers. In other words, the mechanism is the mediator of a statement "If A, then B", stating *"how, by what intermediate steps, a certain outcome follows from initial conditions"* (Mayntz 2004: 241). But here one must also keep in mind that actual events might be (and, except for scientific experiments, usually are) the results of the interactions of a number of different causal mechanisms. Therefore it is useful to distinguish between an overall event sequence (everything that happens) and mechanisms (a number of which interact and make up the event sequence).

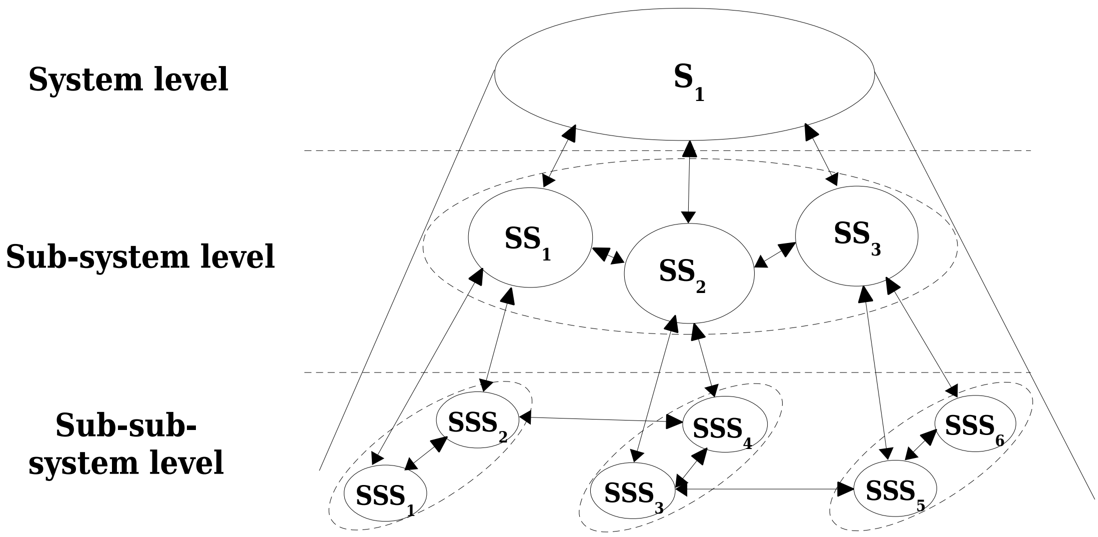
Finally, this system must have something distinctive that makes it possible to conceptualize it as a system in the first place, i.e. it must have boundaries separating it from its environment. Thus taken together, characteristic components, structure, processes, boundaries and emergent properties provide a minimal definition for a socio-technical system or a network (figure 1.3). Everything that lies outside the system is its environment (which, of course, can include other systems). This definition implies that systems or networks are ubiquitous: there are various ways to determine system boundaries: for example, by certain types of activities (e.g. industrial sectors), geographical borders (e.g. states) or combinations of those (e.g. industrial sector in a state). But this delineation itself is not decided at a metatheoretical level, but is left for specific theories.

Figure 1.3. A socio-technical system/network



It follows quite naturally that these systems can be of varying sizes and they can form nested hierarchies. For example, a town can be seen as part of the region which can be seen as part of the state which can be seen as a part of the international system. On the other hand, the wind electricity sector can be seen as part of green energy sector which can be seen as part of an electricity sector. In other words, what can be seen as a system on one level can be seen as a sub-system on another. It is also reasonable to presume that although there is a two-way interaction between the two, the relation is nevertheless asymmetrical (e.g. the state as a collective actor can usually shape the action of a single individual to a greater extent than the other way round). On the other hand, on a certain level of aggregation there might be systems that interact and/or overlap with each other (e.g. inter-firm competition or wars between states). The difference between the sizes of the units of analysis is commonly referred to as the micro–macro distinction and is depicted in figure 1.4.

Figure 1.4. Horizontally and vertically interacting nested and parallel systems



How exactly one should stratify society is left open on a metatheoretical level. A number of different solutions have been offered. Commonly a distinction is made between micro, meso and macro levels. Brante (2001) has suggested five different levels (individual, interindividual, institutional, interinstitutional, international), while Geels and Schot (2007: 402) mention six (individual, organizational subsystem, organizational population, organizational field, society, world system). Nevertheless, although the number of levels can differ, the idea of nested or parallel systems remains relevant in each case.

A system cannot shape its subsystem while being simultaneously shaped by it unless there is some temporal sequence of interactions. The concept of structure is helpful here. It is defined as *"conditions-of-action"* (Sibeon 2004: 54), that is a totality of entities and their prior interrelations that the system in focus is confronted with at a given moment of time. Following the second postulate, this embraces existing: 1) available material resources, technologies and technical systems (e.g. power grids); 2) actors and the relationships between them (e.g. a newcomer must take into account the power of prevailing market incumbents); 3) rules to be considered (e.g. criminal laws). Thus the structure is by definition thoroughly socio-material and can be taken to include 'rules and resources' (Giddens 1984: xxxi), provided that material, not only symbolic, resources also count (Sewell 1992).

In my view, the notion of structure does not only apply to the environment of the system, but also to the very constitution of the system itself (e.g. an organizational structure inherited from the past might become an obstacle for reorganizing the company, the biological limits of humans' abilities for information processing affect the speed of innovation). Structure is what precedes action and shapes (enables and constrains) it, but does not determine the outcome: actors always have room for limited improvisation in the conditions in which they find themselves and it is only through their actions that structure can be reproduced or transformed. Thus in diachronic terms one can speak of 'structure → agency → structure' interplay. Note, however, that as a term signifying the social totality structure can be also used in a synchronic sense in which 'system + environment = structure'.[7]

To put it all together: by drawing on the structure, actors transform and reproduce it through their activities. As various processes and causal mechanisms interact, one is constantly dealing with outcomes arising from three sources (Sibeon 1999): 1) agency causation—a result of actors' intentional and purposeful activities; 2) structure causation—causal influence of the components of structure; 3) chance causation—an unforeseen and unintended consequences of action and various causally unrelated event conjunctions that contribute to the eventual outcome (that is, outcomes that cannot be attributed to neither the structural properties nor the actors' goals). As a result both the environment and the network/system can experience some change. Depending on the impact on the system, one can distinguish between morphogenetic and morphostatic processes (Buckley 1967: 58–59): the first changes

7 Note that my use of 'structure' differs somewhat from that of Elder-Vass. He distinguishes between four different notions of (social) structure: 1) structure-as-empirical-regularity; 2) structure-as-properties; 3) structure-as-relations; 4) structure-as-wholes (2010: 80-86). He advocates the last definition and chooses two specific types of social structures – normative institutions and organizations – to illustrate the viability of this approach. My use of the term is a bit more general in that 1) the notion explicitly includes both social and natural entities, and; 2) includes all kinds of social, material and socio-material entities operating on various levels of ontological hierarchy which can systematically bias the focal entity (the object of analysis) towards certain courses of action, while; 3) not trying to specify all these entities and impacts before the analysis itself.

the system or even calls it into being, and the second sustains it. Both of these processes, however, may be either internal or external to the system; that is, one can speak of either endogenous or exogenous processes that contribute to the stability/change of the system in question.

Additionally, I would propose that the basic interaction mechanism able to capture what is going on between the start and end points of socio-technical development is co-evolution. The use of various similar terms like 'seamless web' (e.g. Hughes 1986, Bijker 1995), 'mutual shaping' (e.g. Williams 1997, Faulkner 2001), 'co-construction' (e.g. Oudshoorn & Pinch 2003) or 'co-production' (Jasanoff 2004, Bijker 2010) indicates that this view is at least implicitly shared by many STS scholars (although what is seen interacting might differ from case to case, e.g. gender and technology, or users and technology). Generally, *"we speak of co-evolution if the interaction between different systems influences the dynamics of the individual systems, leading to irreversible patterns of change within each of the systems"* (Rotmans & Loorbach 2010: 118). Thus the term refers to a continuous interaction between various causal forces, various systems and various levels, in which the change in one challenges the other to react and respond. Note that this does not specify the course of development —evolution does not have any pre-determined trajectory. A technology can break through and become pervasive in society, or it might fail and disappear; in this sense the interaction mechanism is non-propositional. In all cases, however, mutual shaping of constantly changing elements takes place and where one ends up is not where one started. The combination of structure, agency and chance means that there is path dependency, to be sure, but also creative action leading to intended and unintended consequences, which in turn interact in various ways to become conditions of action themselves for further developments.

Having devoted some space to discussing what the socio-technical metatheory is, some attention should be turned to what it is not. While aiming to rewrite sociological metatheory Roger Sibeon (1999, 2004) has outlined four 'cardinal sins' of sociology: 1) reductionism—reducing all explanation to a single principle (e.g.

rational choice, patriarchy); 2) essentialism—making *a priori* presumptions about the *"necessary unitariness or homogeneity of social phenomena"* (1999: 318) (e.g. working class, black people); 3) reification—inappropriate attribution of agency to non-agentic entities (e.g. structure); 4) functional teleology—explaining social causes in terms of their outcomes (e.g. a fulfilment of a general social system need for reproduction as a cause for marriage). A successful metatheory should avoid these pitfalls for they lead to well-known dead-ends of sociology. Therefore the above theorization should pass Sibeon's checks. Is this the case?

Things are quite easy with respect to functional teleology and reification. As I have not included a common goal as an integral part of the system/network definition, it follows that actors can indeed have varying motives for becoming interlinked with others and hence there is no assumption that they are necessarily thinking and acting for the good of the system as a whole. I have also argued that only actors can have agency—it suffices to point out here that this does not mean that any group of individual actors can be called a collective actor. Some of them might simply be statistical aggregates (e.g. all left-handed Slovenian women). Where agency can be ascribed and where it cannot is a question of empirical enquiry. With this qualification I have also dealt with the question of essentialism.

The problem of reductionism is probably most significant because I have above indeed specified only one basic interaction mechanism. My grounds for this have sprung from analytic reasons—co-evolution seems to demand stating little beyond mutual interaction whereby the participating entities experience change in at least some parts of the constitution and the environment in which they find themselves. It is indeed only a little more than Heraclitus's 'you cannot step twice into the same river', but also hinting at the twin enabling/constraining nature of this interaction. In other words, it operates on so high a level of abstraction that it allows for countless specifications about the entities undergoing co-evolution, the conditions in which they do so and different paths this development might take. However, maintaining some caution I would still hypothesize that since there might indeed be some situations in which some other hypothetical mechanisms that cannot be conceptualized as a special case of co-evolution (contrary to some, e.g. rational choice, which can be seen simply as an interplay of actors in the conditions of relatively stabilized rules and technologies) might do much better explanatory work, the last assumption should be taken with a grain of salt and hence some extra attention should be turned to its possible theoretical and/or empirical sources of revision. Unless such a candidate is found, however, I would retain co-evolution as a basic interaction mechanism.

Therefore this metatheory has gone further than the above critical realist principles by specifying the basic ontology that all socio-technical processes could hypothetically share. However, it still falls victim to Malerba's remark about co-evolutionary approaches: *"The challenge for research here is to go to a much finer analysis at both empirical and theoretical levels, and to move from the statement that everything is coevolving with everything else to the identification of what is coevolving with what, how intense is this process and whether there is a bi-direction of causality"* (2006: 18). To make sense of the historical narratives, more analytical tools of greater precision need to be found. In other words, it is time to explore the layer of specific substantive theories.

1.4 Level three: specific substantive theories

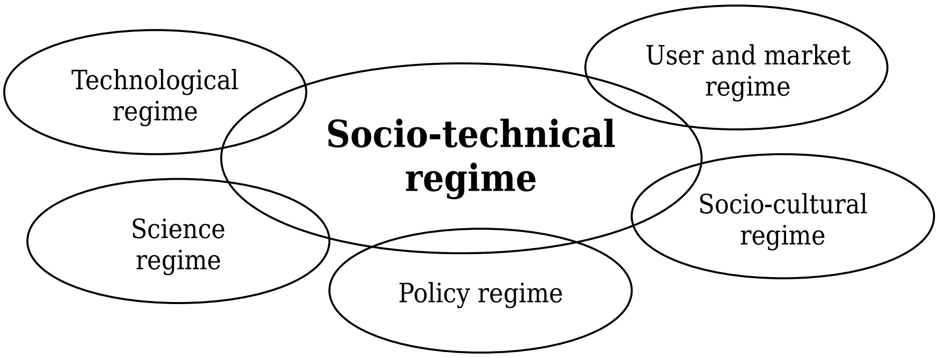
While the previous layers were so general that the empirical focus hardly mattered—it could have been the industrialization of the West or the implementation of computer software in two French companies between 1996 and 1998—things change when specific substantive theories begin to be explored. The reason is simple: since the third-layer theories are designed for specific goals, they might not be exactly suitable for every research effort and hence need to be rejected altogether or customized accordingly. Therefore three types of justifications are in order: assessing the compatibility of employed theories 1) with metatheoretical assumptions—are these theories fundamentally compatible and if not, (how) can the situation be remedied? 2) between themselves—how do different theories promise to complement each other? 3) with the current research goal—what to adopt, what to discard and what to modify? With these questions in mind I will first describe the Multi-Level Perspective on socio-technical transitions, Desires-Beliefs-Opportunities framework and some conceptual tools of the (Technological) Systems of Innovation approach. For various reasons (among which are the word limit and the goal to retain the clarity and focus of the text) I do not aim to provide an exhaustive overview of each approach—rather, only the aspects of each framework perceived as having direct relevance for the current research have been selected.

1.4.1 Multi-level Perspective (MLP)

Building on the general MLP (Rip & Kemp 1998) Frank Geels (2002, 2004, 2005a), has developed a novel way to analyse socio-technical transitions, that is large-scale shifts from one socio-technical system to another. In his initial formulation, MLP focused on explaining how such transitions occur and identifying the patterns of transitions and the mechanisms underlying them (2005a: 6). Later, various extensions have been made such as the typology of transition pathways (Geels & Schot 2007) or an outline of the inner dynamics of a part of the initial framework (e.g. niche-internal dynamics as discussed in Raven & Geels (2010)).

The central concepts of MLP are regime, niche and landscape. Different social groups share different regimes (e.g. policy, science) but these can become partially aligned to each other in a single socio-technical-regime (see figure 1.5) defined as *"the rule-set or grammar embedded in a complex of engineering practices, production process technologies, product characteristics, skills and procedures, ways of handling relevant artifacts and persons, ways of defining problems—all of them embedded in institutions and infrastructures"* (Rip & Kemp 1998: 338). For example, in a transport regime, government regulations, car producers, users' habits, symbolic meanings of cars in a modern society etc. are adapted to each other. Still, suppliers are never in the full service of the government or the users, whereas the latter are never fully tailored to the interests of car manufacturers: there is a combination of interdependence and partial autonomy making the regime 'semi-coherent' (Geels 2005a: 13). This example already hints at the criticism made by Markard and Truffer (2008: 605) that the use of the term in Geels's own writings and in MLP literature as a whole has been inconsistent: sometimes only rules are claimed to define the regimes (hence the distinction between regimes and systems, e.g. Geels 2004), while sometimes actors and technologies have also been included. I will follow the second definition, seeing socio-technical regimes as interrelated sets of actors, technologies and rules.

Figure 1.5. Coordination between groups in a socio-technical regime (Geels 2004: 905)



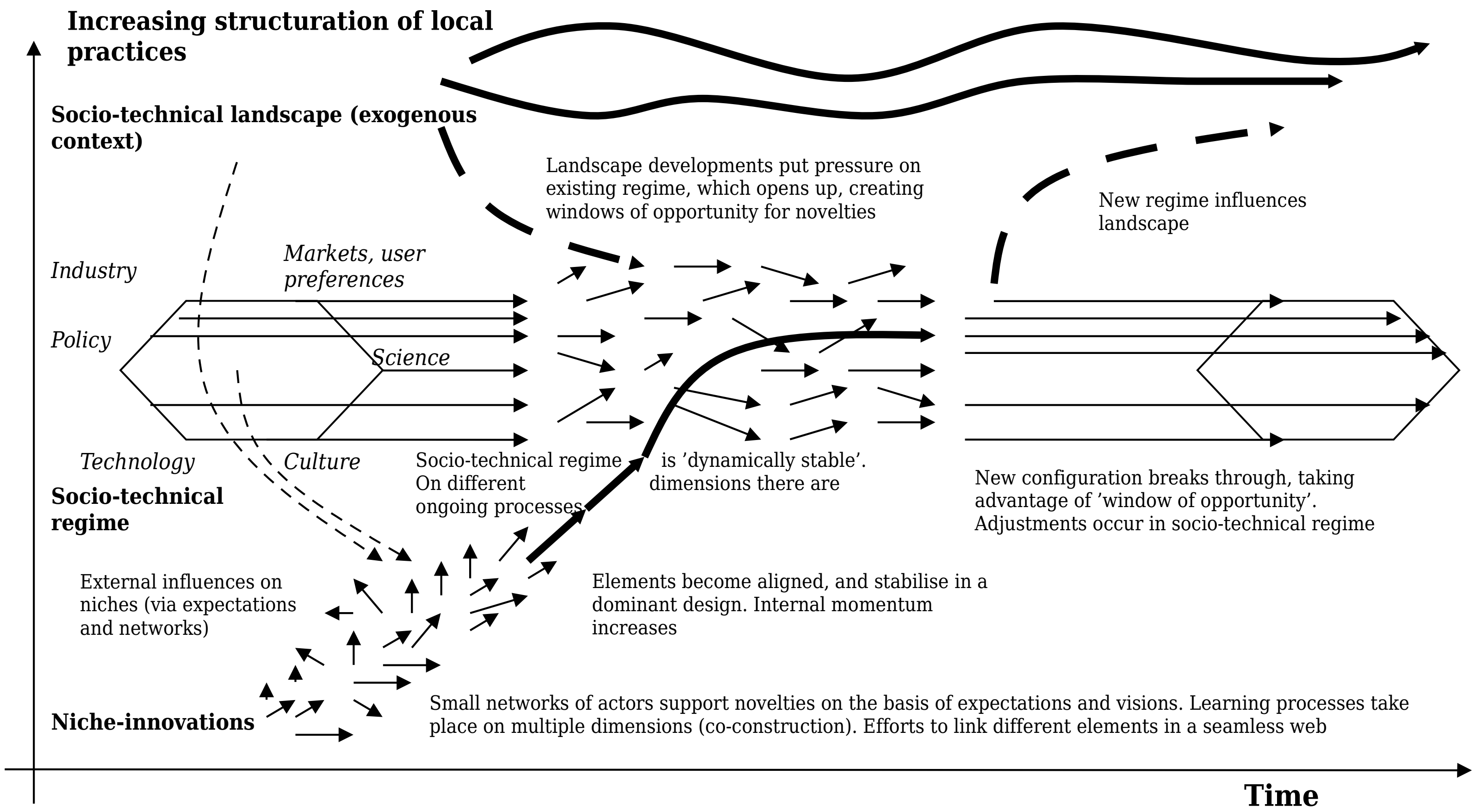
Although the actors in such regimes are structured by pre-existing expectations, technologies, 'rules of the game' etc. their activities can lead to gradual and cumulative changes in technologies, rules, or the behaviours of other actors. In other words, socio-technical transition is a co-evolutionary process in which mutual adaptation in multiple dimensions such as artefacts, cultural meanings, industry structures, policies etc. continuously takes place.

This change is incremental in existing socio-technical regimes in which rules (e.g. search heuristics, lifestyles) are well-established, technologies 'mature' (e.g. a fully developed road infrastructure, cars, petrol stations) and actors are embedded in networks of mutual expectations, giving rise to overall stability, lock-in and path dependence. With a lack of major internal/external tensions, the regime remains 'dynamically stable' and the innovation proceeds predictably along the lines of a certain technological trajectory (e.g. faster computers with more memory).

While the regime constitutes the meso-level of MLP, novel solutions emerge on the micro-level, in the niches. Compared with regimes, actors in niches are few, the performance of technologies low, and rules in constant flux. The 'landscape' (macro-level) on the other hand forms a context for both niche and regime actors, which are not able to influence the landscape itself (at least in the short term). The landscape includes various exogenous components (pre-existing technological infrastructures, wars, liberalization etc.) that can shape niche/regime activities. Together, landscape, regime and niche form a nested hierarchy in which the activities are usually increasingly stable and structured as one moves from micro-level to macro-level.

The socio-technical transition only occurs when processes on all three levels 'link up' (figure 1.6). For example, a landscape pressure such as climate change might create tensions in an existing transport regime (the sustainability of petrol-based cars becomes questioned), opening up a 'window of opportunity' for the wider diffusion of niche inventions (e.g. electric cars) which may or may not have matured yet (e.g. there might still be some uncertainty regarding the dominant design). However the breakthrough of a niche innovation can lead to further changes in the existing regime and subsequently in the landscape (e.g. electric car as a symbol of green modernity).

Figure 1.6. Technological substitution pathway (Geels & Schot 2007: 401)



Depending on the nature of landscape pressure (see table 1.1 for a typology), the states of niches/regimes and the timing of their interactions, transitions can take multiple paths. In the example depicted in figure 1.6, the niches have become sufficiently matured when landscape pressure occurs, allowing for a relatively quick technological substitution of one socio-technical regime for another, e.g. a transition from sailing ships to steam ships (Geels 2002). Alternatively, if niche inventions have not matured, landscape pressure is followed by competition between various technologies until the emergence of a dominant design, e.g. a competition between bicycles, steam trams, electric trams, steamers, electric cars, petrol-based cars etc. as substitutes for a horse-drawn carriage regime (Geels 2005b). If the landscape pressure is less intense, regime-internal actors have more time to adapt and so the transformation is more gradual, e.g. the transition from cesspools to integrated sewer systems (Geels 2006). And with a lack of major landscape pressures the regime changes only incrementally, making it unlikely for the niches to break through at all.

Table 1.1. Typology of environmental disturbances (Suarez & Oliva 2005: 1022)

Frequency	Amplitude	Speed	Scope	Type of environmental change
Low	Low	Low	Low	Regular
High	Low	High	Low	Hyperturbulence
Low	High	High	Low	Specific shock
Low	High	Low	Low	Disruptive
Low	High	High	High	Avalanche

Finally, during the overall process various little mechanisms occur, partially contributing to the transition. Geels (2005a: 267–272) named sixteen such mechanisms, later adding another eleven (2006: 1079–1080). Examples of these include: 1) the important role played by the government in creating and sustaining niches; 2) the role of visions and values which help to legitimize the technologies to ensure their wider diffusion; 3) the role of specialized social groups who advocate new technology and through such lobbying help to legitimize it; 4) strategic games between various market actors that may lead to speeding up or slowing down of the process of innovation (e.g. adopting a collective wait-and-see attitude); 5) the same goes for various social struggles between enterprises, governments and users (e.g. technology is ready for mass production but it is delayed for various reasons). As can be seen, these 'mechanisms' are rather loosely worded, often indicating only one activity and, by contrast with the analysis of the overall dynamics of transitions, have not been extensively developed.

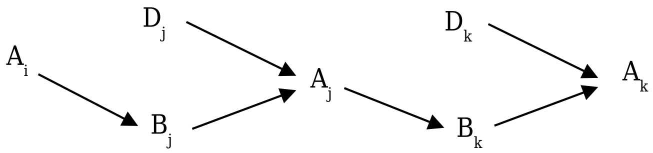
1.4.2 Desires-Beliefs-Opportunities framework (DBO)

Led by Peter Hedström (Hedström & Swedberg 1996, 1998, Hedström 2005, Hedström & Bearman 2009a), analytical sociology is a relatively recent movement characterized by four features (Hedström 2005: 1–6): 1) focus on explanation instead of description by specifying various causal mechanisms by which various social phenomena (e.g. network structures, divorce patterns, residential segregation) are brought about (see Hedström & Bearman 2009a for various examples); 2) dissection and abstraction, that is a decomposition of complex totality into constitutive elements and an accompanying focus on those that are deemed to be most essential to the explanation, leaving other components aside; 3) aim to offer as clear, precise and fine-grained analytical distinctions as possible; 4) focus on actions and corresponding theories that enable us to understand the results of the interplay of various actors. Here I will only focus on the last part, leaving other issues for the following chapters.

The main action theory of analytical sociology is the DBO framework. It explains the actions of individuals as combinations of desires, beliefs and opportunities, influencing each other. Hedström uses a simple example. The action of Mr Smith going out with an umbrella might be explained by his belief that it would rain today, his desire not to get wet, and an opportunity to take an umbrella to prevent this from happening. If any of these three had a contrary value, the action would not occur: Mr Smith might have an erroneous belief that it would not rain, for some reason he would be happy to get wet or he would not have an opportunity to take an umbrella (2005: 39–40).

The aim of the DBO framework is not to explain the behaviour of a single individual, however. Instead it focuses on how the interactions of the beliefs, desires, opportunities and actions of individual actors lead to certain collective outcomes. For example, a bank run can be explained as a result of an underlying mechanism of self-fulfilling prophecy in which the withdrawal of one actor leads another actor to believe that the organization might indeed be on the verge of bankruptcy. Combined with the second actor's desire to avoid financial losses, this mechanism leads to another withdrawal, which in turn influences the beliefs of other actors. Figure 1.7 depicts this example in DBO terms, where A stands for actions, D for desires and B for beliefs, while the subscripts denote different actors.[8]

Figure 1.7. Self-fulfilling prophecy (Hedström 2008: 327, following Merton 1968)



Provided that the characteristics relevant to explaining the phenomena can be measured precisely enough, even very small differences in the composition of actors' desires, beliefs or opportunities can lead to very different outcomes. Hedström and Bearman (2009b: 12–13) use Schelling's (1978) stylized example on residential segregation as an example. This model consists of a lattice in which two groups, Whites and Grays, search for a living place. Each of the groups wants to live near at least some of their kind. Initial random distribution often leaves too few Whites and Grays together, prompting them to move elsewhere. But the migration of Whites into certain areas might prompt Grays to move elsewhere and so on. As a result residential segregation can emerge as an unintended consequence. For example, if 25% of Whites and Grays want to live near their own kind, the moving process

8 See Hedström (2008: 327) and Falleti and Lynch (2009: 1150) for additional examples.

culminates in a neighbourhood where the proportion of the representatives of either groups living nearby is actually 55%. But with a slight change in preferences—from 25% to 26%—the homogeneity of the neighbourhood rises to 73%. The lesson is that even seemingly very different collective outcomes can be caused by fairly similar underlying mechanisms and starting conditions.

1.4.3 Systems of Innovation (SI)

So far little has been said about the boundaries of the systems or networks in focus. Here it is useful to draw briefly upon the vocabulary of the Systems of Innovation (SI) approach. Most generally such a system is defined as *"the determinants of innovation processes, i.e. all important economic, social, political, organizational, institutional, and other factors that influence the development, diffusion and use of innovations"* (Edquist 2005: 182), where innovations are either novel products (product innovations) or novel ways of producing products (process innovations).

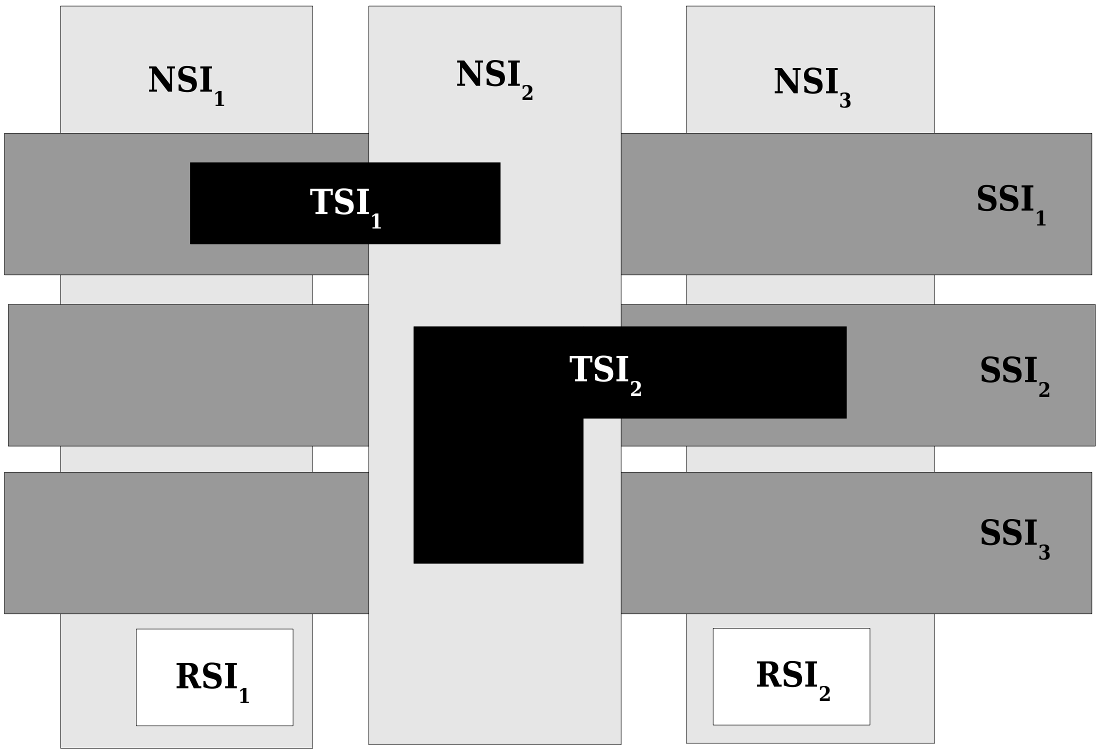
SI understands the success of innovative activities as a result of interdependent evolution of organizations, institutions and technologies in a certain domain and/or locality (Markard & Truffer 2008, Suurs & Hekkert 2009). The boundaries of such a system are determined by whether the interaction between the components is two-way or one-way or, softening this criterion a little, at least 'relatively independent' from the environment (Markard & Truffer 2008: 601).

As such, one can define a System of Innovation in various ways: 1) on a geographical basis as a national or regional SI (NSI or RSI), e.g. *"the network of institutions*[9] *in the public and private sectors whose activities and interactions initiate, import, modify and diffuse new technologies"* (Freeman 1987: 1); 2) on the basis of an industrial sector (e.g. biotechnology, telecommunications) as a sectoral SI (SSI), e.g. *"a set of new and established products for specific uses and the set of agents carrying out market and non-market interactions for the creation, production and sale of those products"* (Malerba 2002: 250); 3) by a specific technological area

9 Freeman's use of 'institutions' conflates actors and rules.

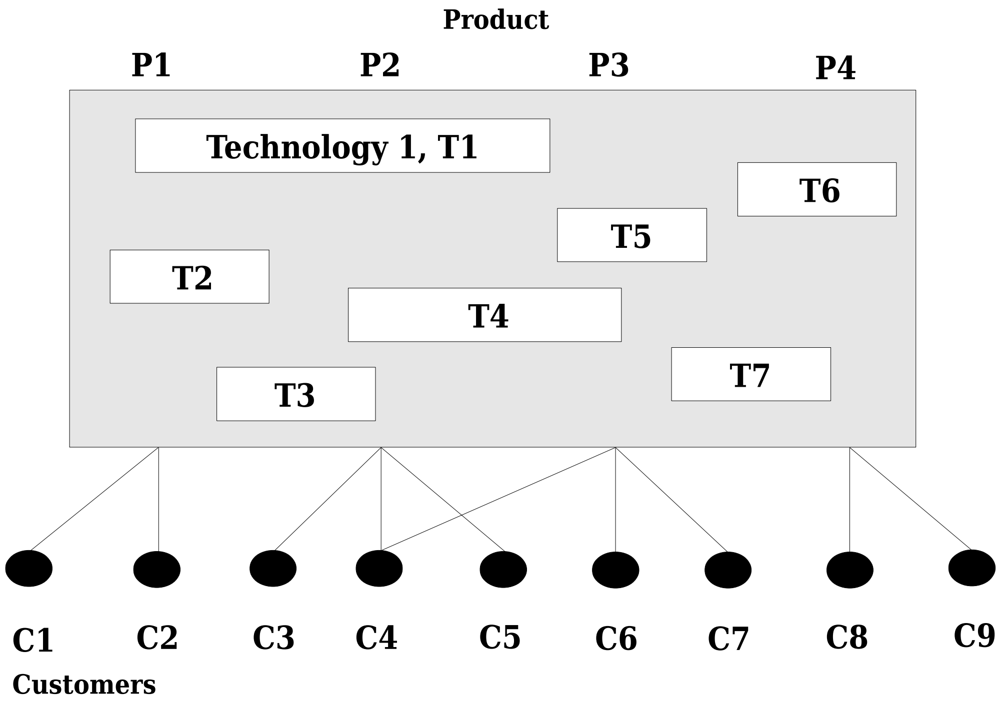
(e.g. microcomputing, wind energy) as a technological SI (TSI), e.g. *"network of agents operating in a specific economic/industrial area under a particular institutional infrastructure or a set of infrastructures and involved in the generation, diffusion and utilization of technology"* (Carlsson & Stankiewicz 1995: 49). Depending on the research focus, various combinations are possible, e.g. a technological SI in a certain country (TSI in NSI) or a comparison of different industrial sectors worldwide (cross-national SSIs). Given the nature of the cases studied, for practical purposes this research uses 'systems of innovation' and 'socio-technical regimes' interchangeably.

Figure 1.8. National, regional, sectoral and technological innovation systems (adapted from Markard & Truffer 2008: 600)



Carlsson *et al.* (2002) have made further distinctions between three takes on TSI research (see figure 1.9): 1) technology as a knowledge field (T); 2) technology as a product or artefact (P); 3) set of products or artefacts fulfilling a particular function for customers (C). In each case the system is delineated in a different manner and so the relevant aspects to be studied also differ: for example, if technology T4 as a knowledge field (e.g. mainframe computing) is taken as a starting point, the research would include studying P2 and P3 as the particular products into which it is crystallized (e.g. ENIAC and Colossus) and the customer base would expand from C3 to C7, who use it for various purposes (e.g. codebreaking, calculating artillery firing tables etc.). However, the other means by which the customers seek to fulfil these goals (e.g. electromechanical calculators) would not be explored. Alternatively, one could focus on certain customers and the types of products they use for particular purposes, cutting across a variety of knowledge fields but not embracing any of them fully. The point is not to claim that one or another focus would be superior *per se*: it only serves to highlight how different analyses are likely to reveal different aspects of the systemic interaction.

Figure 1.9. Illustration of the three research focuses (Carlsson *et al.* 2002: 238)



1.4.4 Towards research-specific application: assessing the three types of compatibility

Having presented a quick outline of each approach, I am now in a position to assess their compatibility with metatheoretical theses, their mutual complementarity and their importance to explaining the cases. I will begin with the assessment of foundational compatibility.[10]

The correspondence of MLP to each metatheoretical thesis is as follows:

1) Causal force—has been borrowed from MLP itself.
2) Causal force interaction—same as point 1.
3) Systemicity—MLP's empirical focus is on shifts from one socio-technical system (or regime) to another.
4) Micro–macro—MLP conceptualizes niche, regime and landscape as nested hierarchies in which the influence of the latter on the former is asymmetrical. Also, the language of micro, meso and macro levels is used. However, when responding to the criticism that what is seen as a regime on one level might be viewed as a niche on another (e.g. wind power as a separate regime or a niche in the context of electricity production in general) (Berkhout *et al.* 2004, Smith *et al.* 2005), Geels and Schot (2007: 402) make a distinction between empirical and analytical levels. Namely, while they admit that levels are indeed empirically nested, they argue that one should first pick a level of interest (whether it is transport, bus transport, or long-distance bus transport) and only then apply analytical levels to it. MLP is said to operate on the level of organizational fields (*ibid.*), defining its lowest empirical boundary. So in effect MLP employs a dual micro–macro distinction: a relational analytic micro–macro definition nested in an absolute empirical one. Keeping that in mind, however, it is clear at the same time that MLP does acknowledge the overall micro–macro distinction.

10 Since I am only borrowing a few tools from the (Technological) Systems of Innovation (TSI) approach that help to delimit the system, I will only assess MLP and DBO here. The initial version of the chapter did include an analysis of the functions of TSI (Hekkert *et al.* 2007, Bergek *et al.* 2008). I concluded that, with some analytical clarification, the 'functional' vocabulary of TSI can be made compatible with the language of MLP.

5) System–system interaction—as shown in figure 1.6, MLP is about the role of the interactions of different systems (and exogenous events) in explanations of transitions. In the initial version mainly vertical interactions were analysed. Later works (e.g. Geels 2007a, Raven & Verbong 2007) have also turned attention to multi-regime interactions.
6) Structure—MLP is explicit about the enabling and constraining nature of the socio-technical structure that the actors draw upon (e.g. Geels & Schot 2010: 30).
7) Basic interaction mechanism—Geels (2010) describes MLP as a crossover between evolutionism and interpretivism in which actors' choices, struggles, sense-making activities etc. are combined with evolutionary theory. Such focus has been stressed virtually from the beginning: the subtitle of Geels's book on transitions (2005a) states that its analysis is co-evolutionary and socio-technical.

The correspondence with Sibeon's checks is as follows:

8) Reductionism—as substantive theories are issue-specific, following a single explanatory principle might be sometimes a perfectly valid strategy (whether it is appropriate or inappropriate in all cases is another matter, to be decided separately in each instance). As an overarching argument, however, this critique is only relevant for metatheory.
9) Essentialism—MLP does not presume the absolute unity of niches and regimes, describing the latter as 'semi-coherent'. However, it is the point of choosing such categories in the first place that the coherence within such units is higher than that between them.
10) Reification—MLP does not ascribe agency to non-agentic entities as defined above, although it allows for mutual shaping.
11) Functional teleology—although MLP makes occasional references to the functions of socio-technical systems it *“does not assume that all actors work towards shared system goals, has no teleology, and no bias towards stability”* (Geels 2010: 56).

How does DBO measure up to these criteria?

1) Causal force—two components of DBO (desires and beliefs) focus on actor-internal causation. The category of opportunities can include various other actors, rules and technologies, although DBO and analytical sociology have generally focused on the interactions of individual actors.
2) Causal force interaction—taking into account the above qualifications, DBO is in principle able to embrace such interactions.
3) Systemicity—despite Hedström's occasional criticism about critical realism and emergence (e.g. Hedström 2005: 70–74, Hedström & Bearman 2009b: 13), Hedström (personal communication, 14.12.2010) has acknowledged that a rigorous explanation in terms of higher-level entities might be considered a temporarily satisfactory strategy. Moreover, at least one of the earlier works in this tradition (Stinchcombe 1998) explicitly discusses a mechanism common to the operation of universities, corporations and states. I would thus argue that DBO theory is capable of pragmatically accepting the interactions between higher-level or emergent entities such as organizations or socio-technical networks.
4) Micro–macro—DBO aims to explain collective outcomes as direct results of individual actions; therefore it acknowledges at least a crude micro–macro distinction. Its somewhat ambiguous attitude about ontologically stratified reality is noted above.
5) System–system interaction—considering the above qualifications, DBO can be applicable to these instances.
6) Structure—DBO appreciates the role of relations and relational structures in shaping individual preferences (e.g. Hedström & Bearman 2009b: 8).
7) Basic interaction mechanism—although the early version of analytical sociology (Hedström & Swedberg 1998) was inspired by rational choice theory, the proponents later become somewhat disappointed with the limitations of this framework (e.g. Hedström 2005: 60–66, Hedström & Bearman 2009b: 8). One could argue that as all elements of the framework

can lead to changes in other elements, DBO is able to embrace a co-evolutionary approach in the wide sense, as defined above.

8) Reductionism—the argument made for MLP also applies to DBO.
9) Essentialism—given its general focus on individual actors, DBO cannot favour the ascription of essential qualities to the types of entities discussed by Sibeon (e.g. class). DBO also allows for frequent changes in actors' preferences and behaviours.
10) Reification—see the previous point.
11) Functional teleology—DBO theory serves to highlight how social structures manifest on an individual level, how action is brought about and how various interactions lead to collective outcomes. In other words, it explicitly theorizes how macro-level conditions are related to micro-level behaviour, instead of simply deriving the latter from the observation of the former.

The comparison is briefly summarized in table 1.2.

I conclude that although so far the empirical focuses of both approaches have been quite different, and therefore they have not explicitly addressed each issue deemed relevant by the above metatheoretical assumptions, there is no necessary contradiction between them. In principle, MLP can be employed alongside DBO. The additional questions of how they complement each other and how they help to make sense of the empirical cases need to be answered next.

At least three strengths of MLP can be singled out. First, although MLP as traditionally applied to socio-technical transitions operates on a relatively high level of aggregation, I think that its theoretical shell remains a powerful analytical tool even when detached from its empirical focus. The notion of dynamic models in which the outcome emerges from the interactions of various levels is an idea that can be extended to various instances, including those operating on lower levels of aggregation. Second, MLP has been fine-tuned to focus on events from the start, making it highly suitable for making sense of historical developments. And third, it offers a theoretical vocabulary to distinguish between the patterns of occurrence of similar events (e.g. transitions).

Table 1.2. MLP and DBO in metatheoretical context

Postulate	MLP	DBO
Entity	Compatible by definition	Although not directly conceptualized, the category of opportunities can include groups, technologies and rules
Entity–entity relations	Compatible by definition	Compatible in principle
Systemicity	Yes	Sceptical in theory, able to accept pragmatically in practice
Micro–macro	Yes, but also specifies the lower boundary of empirical applicability for theorizing transitions	Yes, although generally sceptical about ontologically stratified reality
System–system interaction	Yes	Yes, pragmatically acceptable
Structure	Yes	Yes
Basic interaction mechanism	Yes	Not directly addressed but hypothetically not exclusive
Reductionism	Reductionist by definition, domain of applicability should be justified on a case-by-case basis	Reductionist by definition, domain of applicability should be justified on a case-by-case basis
Essentialism	No (regimes conceptualized as semi-coherent)	No (actors' preferences are allowed to change)
Reification	No	No
Functional teleology	No	No

MLP is weaker when it comes to the conceptualization of underlying mechanisms: *"While patterns are outcomes, mechanisms produce outcomes. ... Furthermore, patterns typically stretch over the entire process of system innovation, while mechanisms take place over shorter time periods"* (Geels 2005a: 6). Making the distinction in such a manner does not allow us to see different transition patterns as variations of a single overarching mechanism (e.g. niche breakthrough). Instead MLP offers a list of shorter event sequences observed over the course of the transition, but does little to integrate them to the rest of the theory. Simply put, in MLP patterns and mechanisms remain separate.[11]

In the DBO framework, mechanisms and collective outcomes are much more intimately related. Small differences in individual preferences can result in quite different collective outcomes, even when the underlying mechanisms are identical. As such, analytical sociology is attentive to working from observed patterns to driving mechanisms. Second, similar to MLP, it is focused on events and event sequences. And third, whereas MLP excludes niche-internal and regime-internal activities and focuses on their outcomes instead to explain the overall transition, DBO enables us to take into account some aspects of actor-internal causation, that is, the effect of interaction on actors' desires and beliefs.

On the other hand, DBO framework as generally employed in analytical sociology tends to force itself into the straitjacket of structural individualism (see chapter 2), making its adherents hesitant about the theorization of larger units of analysis. Additionally, it does not have a sophisticated vocabulary for conceptualizing material resources and technologies. Finally, it seems that, contrary to MLP, DBO has little to say about the process characteristics of the mechanisms, e.g. the speed or the perceived intensity of events. That is to say, the same mechanism can not only yield different outcomes, but can also realize these in different temporal patterns.

Systems of Innovation (SI) has traditionally relied on 'snapshot' analyses. Therefore a lot of attention has been paid to different ways of delimiting system boundaries and specifying research focuses, with accompanying implications. But only recently have SI scholars started to turn more attention to the dynamics of (technological) innovation systems. For example Hekkert *et al.* (2007) and Bergek *et al.* (2008) have proposed typologies of functions (or activities) of TSIs. Later works (see Hekkert &

11 Of course, over the years MLP has been criticized for several other shortcomings including functionalism, teleology, structuralism, technological determinism, descriptiveness and harmful policy implications (e.g. Berkhout *et al.* 2004, Smith *et* al. 2005, Genus & Coles 2008, Shove & Walker 2010). However, most of these criticisms are (at best) imprecise, have been addressed in more recent versions of the theory or are simply irrelevant for the current study (see Geels & Schot 2007, Geels 2010, 2011, for various responses). I will return to the question of description and explanation in chapter 4.

Negro 2009 for an overview) have identified some recurrent sequences of functions, but the overall system dynamics are found to be complex, lacking common patterns (*ibid.*: 591). Therefore at present, Technological SI (TSI) does not offer any theoretical propositions about the evolution of the system over a longer period (making it different from MLP, in which the overall patterns of socio-technical transitions are present). Based on a discussion in chapter 4, I will suggest in the concluding chapter that, paradoxically, a too nuanced typology might be the main culprit. For this reason I will exclude (T)SI's analytical tools for conceptualizing the dynamics of innovation systems from my own analysis. However, it will be demonstrated in chapter 4 that the rest of the conceptual framework of (T)SI (see section 1.4.3) can be successfully integrated with MLP's analytical tools in order to derive a multi-level model of the transformation of (certain types of) innovation systems.

So how to make the best of the strengths of each approach in the empirical analysis? Here I will briefly remind the reader of the research questions:

1) What explains the success or failure of each PC project? What are the patterns of case development? What are the respective intra-case mechanisms?
2) How were the dominant lines of PCs established? What are the patterns of interaction of cases in each country? What are the respective inter-case mechanisms?
3) How did the Technological Systems of Innovation evolve in each country? What are the patterns of system-level development?

The synthetic application of MLP and DBO to answer these questions means that the frameworks are to be extended in five different ways:

1) To my knowledge, the application of MLP and DBO to the Soviet context is novel. It is assumed that the conceptual vocabulary of both is applicable to these cases.
2) MLP's notion of multiple levels is detached from its empirical focus on

transitions.

3) DBO on the other hand is extended to embrace socio-technical developments.
4) In order to answer research questions 1 and 2, MLP is extended to the explanation of the dynamics within and between socio-technical networks. In other words, it is applied on a lower level of aggregation than has been the case so far.
5) The reverse is true for DBO. It is assumed that the notion of goals (desires) and beliefs applies to entities other than individuals (e.g. organizations). Therefore its application is extended to higher-level phenomena.

The wording of the above research questions indicates that the middle-range analysis actually operates on three different levels of aggregation (case-internal, between-case and system-level). The reason is my assumption that each angle would provide a different and complementary picture of the historical developments. The benefits of the above theoretical synthesis common to each level are that it enables one to 1) derive dynamic network–environment models (MLP); 2) analyse the effect of landscape events on local actors (DBO); 3) outline underlying mechanisms and patterns of their realization (MLP and DBO).

On the lowest level of aggregation, the dynamics of each socio-technical network will be analysed separately. With the help of MLP and DBO I will focus on the interplay of network-internal processes (formation, expansion, contraction, disintegration) and landscape movements, covering the development of each PC project in all three countries, roughly from 1977 to 1992. Owing to the chosen time-frame it will be possible to observe network-internal processes occurring in vastly different conditions, ranging from the 'normal' functioning of socialism to the role of possibilities created by Soviet economic and political reforms in mid-1980s to rapid, full-scale social transformation at the beginning of the 1990s. One would presume that in such conditions the preferences of various local actors involved in different networks changed substantially. I will attempt to detect whether these changes were indeed present, what shape they took and whether they had any commonalities. The same roughly holds for the analysis on the next level of aggregation, for which the same tools will be used to focus on the patterns of interaction of different socio-technical networks in conjunction with exogenous events.

But the cases did not interact all the time—many developments took place in parallel. Therefore the sole focus on the interactions between cases would still yield an incomplete picture of the overall dynamics. These could be better grasped if all cases in each Baltic country were conceptualized as regional technological innovation systems (or socio-technical regimes) nested in a national one (the Soviet Union). Such a move aims to capture the process of system/regime-internal transformation when landscape movements gradually gained strength and each regional system gradually became decoupled from the Soviet Union, re-establishing links with the West. The focus is thus on system/regime–landscape dynamics, since existing hardware and software was replaced with newer technologies, but no fundamentally different technological niche challenging the existing regime was present. Analytically speaking then, I do not aim to theorize a shift from one system to another, but a transformation within a system.

As my research takes a product-specific focus it therefore does not address the whole field of (micro)computing in these three countries (e.g. various controllers for specialized uses). Neither does it focus on alternative technologies by which the actors' goals could have been fulfilled. The focus is only on those aspects that are directly concerned with the creation, design, production, diffusion and use of personal/micro/general-purpose computers—i.e. the convergence of numerous technical, economical, political and cultural factors that shaped these processes. Thus it provides a product-centred view of the inner dynamics of technological innovation systems/socio-technical regimes in three neighbouring territories.

Likely the reader has noticed that I have largely abstained from making very precise theoretical propositions about the expected dynamics of PC development in the Soviet Baltic states. Instead I have united the selected components of MLP, DBO and

(T)SI simply to structure further enquiry. That is, so far the conceptual vocabulary has been mainly employed as a set of sensitizing tools for making sense of the historical narratives. It will not remain so. By the end of the theory construction exercise outlined in chapter 4, much more specific propositions will be derived. This, however, requires discussing some methodological questions and, of course, the narratives themselves.

2. Approaching the data

This chapter will tackle various methodological issues related to obtaining (historical) data. The overall aim is to reflect on the aspects often neglected in STS studies to date—for example, process theory or mechanismic explanation—thus providing STS's common but largely implicit practices with firm analytical foundations. In so doing I will draw together a number of separate discussions from different domains of knowledge—critical realist philosophy, social theory, management studies, political science, history—illustrating how they can sensitize one to various nuances of one's project. It also means that the stress of this chapter is more on methodology than on methods. The former is understood as a collection of general principles and techniques that guide the collection and/or analysis of data, whereas the latter refers to a single technique (e.g. quantitative methodology vs. factor analysis).

I will start the discussion with Elder-Vass's 'method for social ontology' (2007b). The conclusions of this discussion imply that some points made in the previous chapter should be elaborated further. Thus I will clarify my position on the type of theorizing involved in the project and the nature of mechanismic explanation. The discussion of the merits and disadvantages of the case study approach follows. Finally, specific techniques of data collection will be outlined, for which the issue of triangulation and the possibility of assembling valid historical knowledge warrant closer inspection.

2.1 Elder-Vass's seven requirements

Once the theoretical framework has been laid out, how should one proceed from there? The problem, as always, is linking abstract concepts to data. If a critical realist philosophy is employed, one would expect this approach to have some ramifications for actual research. Elder-Vass has indeed suggested several criteria a critical realist study should follow. He argues that the researcher should identify:

1) *"the particular types of entities that constitute the objects of the discipline;*
2) *the parts of each type of entity, and the sets of relations between them that are*

required to constitute them into this type of entity;

3) *the emergent properties of each type of entity;*
4) *the mechanisms through which their parts, and the characteristic relations between them, produce the emergent properties of the wholes;*
5) *the morphogenetic causes that bring each type of entity into existence;*
6) *the morphostatic causes that sustain their existence;*
7) *and the ways that these sorts of entities, with these properties, interact to cause the events we seek to explain in the discipline"* (Elder-Vass 2007b: 232).

On the lowest level of aggregation, the fundamental entity of interest would be a socio-technical network. This is constituted by various actors (e.g. for a school computer this would include designers, producers, lobbyists, schools, decision-makers from the education sector, the local communist party and planning committee, universities responsible for training the teachers and so on) using certain technologies (e.g. production infrastructure, available components) and following certain rules (e.g. central laws, user manuals). In practice this would mean focusing on the interactions of organizations that are fundamentally socio-technical—that is, the reasons for establishing inter-organizational linkages do not only include involving more people but also machines, infrastructure, skills, know-how, access to better components and so on. Material and social causes both are implicated in the establishment of a socio-technical network (1).

The relations of these elements can be various, e.g. the linkage of technical components and devices into a PC, supplier–user relations, organizations involved, division of labour etc., with the precise configuration varying from case to case. There is no single way to constitute a socio-technical network (2).

The emergent property of the network is to produce, diffuse and use the PCs. Since the ability can also be attributed to single members of the network it raises a question whether one is really dealing with an emergent property here. The simple answer lies in the linkages: without (potential) users the producers would have hardly any incentive to build the prototype in the first place; likewise the users would have nothing to consume without the producers. The roles of designers, producers and users are taken in relation to each other. Therefore the interdependence between the entities in bringing about the outcome does matter, and so the ability of the socio-technical network as a whole can indeed be considered an emergent property, even if only a fleeting one (Elder-Vass 2005: 334) that is likely to disappear when environmental conditions change (3).

Various mechanisms then refer to different network-internal processes by which the network manages to create, diffuse and use the PCs (4). Morphogenetic and morphostatic causes refer to both network-internal and network-external causes, which contribute to giving rise to or sustaining such a network (5 and 6). Finally, the interaction of different actors, technologies and rules inside the network in conjunction with environmental dynamics helps to explain the emergence, development and disintegration of these networks (7).

Things are quite similar with the second level of aggregation. Here the unit of analysis is changed to the networks of socio-technical networks. The inner dynamics of different socio-technical networks would be black-boxed, whereas any factors beyond the particular networks of networks would be considered to belong to the environment. The goal becomes to explain the collective outcomes resulting from the interactions of separate socio-technical networks while also taking into account the environmental dynamics.

The third level embraces all episodes of the evolution of every socio-technical network in a given region collectively. This does not only include explicitly observed interactions (e.g. competition), but also tacit influences (e.g. knowledge exchange) and parallel developments (e.g. networks developing in relative isolation in different functional niches). But the specific dynamics between socio-technical networks are excluded (similar to the previous level, which excluded network-internal processes) in order to gain a clearer view about the pressures common to all cases in a certain locality and the general direction of the transformation. This level seems to differ from others in that the explanation only includes changes in one unit (regional technological innovation system/socio-technical regime).

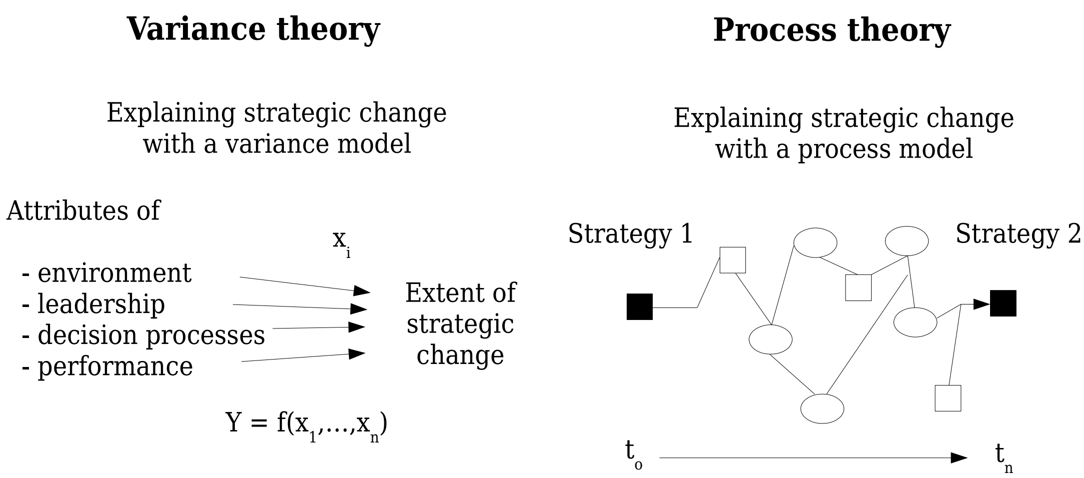
At this point the discussion requires some clarifications. First, the above criteria imply a considerable focus on processes. Therefore it might be asked what kind of theorizing is involved in such an approach, how it differs from the alternatives and what its strengths (and weaknesses) are. And second, it still remains somewhat unclear whether and to what extent one could speak of mechanismic explanation in relation to each of the above level of aggregation. This also necessitates deciding on what is to be considered a mechanism proper. The two following sections will offer some answers.

2.2 Process theory

What type of work are many STS scholars implicitly doing when they engage in a 'thick description' to uncover dense, detailed and variegated historical narratives? Why are they doing it? What advantages does this research strategy entail? Mohr's often repeated distinction between 'variance' and 'process' theory (1982) provides a good starting point.

The main difference between these two types of theory can be captured in the form of following questions: 1) What are the antecedents or consequences of X? 2) How does X unfold over time? (Van de Ven & Engleman 2004: 355). To clarify further: *“Whereas variance theories provide explanations for phenomena in terms of relationships among dependent and independent variables (e.g., more of X and more of Y produce more of Z), process theories provide explanations in terms of the sequence of events leading to an outcome (e.g., do A and then B to get C)”* (Langley 1999: 692). These approaches are contrasted in figure 2.1 using an example of strategic change in an organization.

Figure 2.1. Variance and process approaches (Langley 1999: 693)



It can be seen that variance theory attempts to use the attributes of certain entities to predict a change in another entity. In so doing it hypothesizes the process linking inputs to outputs. The path can be specified by adding more intervening variables, and sometimes the link between inputs and outputs can be intuitive enough for one to be certain of its existence without further probing, but in any case the processes themselves are not directly observed. They remain black-boxed. Process theory (Poole *et al.* 2000, Poole 2004), on the other hand, takes events as its basic units. It attempts to find recurrent patterns of events between a certain starting point and an eventual outcome. In this sense process theory is richer: its data can be simplified and 'variabilized', but not the other way round. This richness comes at a price, however: usually fewer cases can be studied at once because the detection of event sequences takes much time and places a heavy interpretative burden on the researcher.

But there is more to the advantages of process theories. Drawing on various accounts (Langley 1999, Poole *et al.* 2000, Poole 2004, Van de Ven & Engleman 2004, Van de Ven & Poole 2005) at least six different benefits can be highlighted:

1) Process theory is able to take into account the mutation of entities over time. That is, entities can merge (e.g. uniting the efforts of two separate socio-technical networks) or dissolve (e.g. the decision to abandon the project). As

a result, the entity one ends up with might not be the same one started with.

2) Time-ordering of certain events can make a difference to the outcome (e.g. if one seeks government funding before research and development activities and fails to obtain it then one might abandon the project altogether. Conversely, a working prototype might enhance the chances of getting the funding and thus contribute to the continuation of the project).
3) The duration of events might shape the outcome (e.g. persistent lobbying might finally change the minds of the funding bodies).
4) The co-occurrence or conjunction of certain events is important for explanation (e.g. if the efforts of certain socio-technical networks to get their PCs into mass production happen to coincide with the decision of central authorities to give more autonomy for union republics, the projects might have more chance in succeeding).
5) Process theories are able to consider the parallel running of events (e.g. different parts of the network might engage in different activities, say knowledge production and lobbying, simultaneously).
6) Finally, process-based approaches can embrace the increasing and decreasing importance of causes over time (e.g. the conditions of the market economy might become very important during the course of the development of a socio-technical network because of the decline of the USSR, but can be negligible in the beginning).

Therefore, the process-centred approach is better-tuned to the interactive nature of socio-technical processes whereby the outcomes of certain events become the conditions of the next ones and so on. It can thereby also explain why equifinality (different starting points, same outcome) and multifinality (similar starting point, different outcomes) occur.

Several authors (Emirbayer 1997, Cederman 2005, Latour 2005, Abbott 2007) have linked event-based approach to relationalist or non-essentialist position as a seemingly logical consequence. Writes Andrew Abbott: "[The relational approach] *problematizes the very notion of an entity capable of action (the notion of agent),* *viewing entities as constant by-products of repeated action"* and it *"seeks an explicitly processual understanding in which outcomes, actors, and relations are all endogenous"* (2007: 10, 19). The idea is that one should avoid the notion of 'essences' or 'substances' that define the agent in a rigid manner at all costs. Instead the ideas, beliefs, preferences and identities of all units are allowed to fluctuate, making the task of the researcher to locate patterns of similar events.

In my view this link is dubious for many reasons. At best it is a convenient methodological simplification to gain novel insights into data by focusing on events in their own right. A slightly worse option would be a methodological reversal (focusing on events determining entities/properties vs. entities/properties determining events) because it remains unclear what exactly is to be achieved by this move. However, in my opinion the worst choice would be to elevate this position into the status of an ontological creed. I will offer three arguments against this move.

First, although relationalists claim to do away with essentialism, it tends to creep in by the back door. Consider the claim that the goal of process theory is to find general patterns assessed by the criterion of versatility, i.e. "*the degree to which it can encompass a broad domain of developmental patterns without modification of its essential character"* (Poole *et al.* 2000: 43). In other words, process theory attempts to group different narratives together on the grounds of certain similarities they share (note the word 'essential'). How this differs from talking about entities with certain essential characteristics save for the referent (events) is difficult to say.

Second, every researcher is confronted with the fact that the historical narrative simply has to start somewhere. And as soon as the description of the context begins one has to introduce entities with certain properties, enabling or encouraging certain types of actions and constraining or discouraging others. It is likely that one would find some qualities of these entities to extend beyond particular observations (that is, independent of the meanings the particular observed actors ascribed to them) and to have decisive implications for the ways in which they can be related to other entities.

In other words, there is a logical leap between allowing 'outcomes, actors and relations' to be endogenous to the analysis and equating this methodological move with the ontological statement that the observed processes would be fully responsible for all changes in entities, whereas the qualities of the latter would have no part to play in the shaping of the former. I would challenge the holder of this position to locate an instance in which the interactions between only the atoms of iron would produce cheese as a relational outcome.

And third, pinning down the defining characteristics of entities is indeed an extremely difficult task—for example, after decades of research STS still lacks consensus about what constitutes its object of research, i.e. what is to be considered a technology. Moreover, when the properties of the entity keep changing over time, it is especially daunting to fix the qualities that distinguish it from others. On the other hand, for every time-frame of observation the entities are bound to have some qualities that do not change (e.g. I can be quite sure about the immutability of basic biological characteristics of human beings between 1977 and 1992, which would not be true if my research operated on an evolutionary time-scale). In my view, it is exactly the ability to find enough of these enduring properties (so that the definition would not remain too wide) and to leave aside the volatile ones (so that the definition would not become too narrow) within the observed time-frame that makes such defining a true analytical craft, mastered by few.

The problem with abandoning this type of thinking and simply seeing everything as constituted by relations, having no fixed essential or substantial qualities, is undermined by its general applicability. That is, there is no reason why we could not extend the lack of 'essential' or 'substantial' characteristics to the very terms we use to frame the research—that is actors, events, relations and outcomes, to follow Abbott's quote. This quickly leads to an infinite regress of under-conceptualization, wherein the meaningfulness of using any terms could be equally contested. Studying anything at all would become impossible. The mere fact that in our research practice we do choose some framing terms, draw limits to our research observations and justify them indicates that we are using some kind of definitions and classifications, if only implicitly. For a thorough relationalist this would constitute a logical contradiction. For a less thorough relationalist (and observers less allured by the position), this just encourages bad research practice expressed as indifference towards analytical clarification. After all, if everything keeps changing then why bother with determining what that everything is?

My own position is more modest, assuming that 1) entities/properties can only manifest themselves through at least some minimal progression in time; 2) events depend on entities capable of exerting certain causal powers. All properties do not necessarily manifest themselves in events, but events do not solely determine the properties of entities (to avoid the above pitfalls). There is no necessary link between process-based research and ontological relationalism.

This hints at the potential of combining variance theory with process theory: 1) properties are useful for establishing the starting point of the historical narrative, helping to narrow down the arena of choices; 2) events show which properties were actually manifested in the process and by whom, and how all this influenced the outcome for the unit of analysis the researcher is concerned with. In the case of this research, it helps to explain how the domestic PC projects came to be, what kind of requirements had to be fulfilled so that they could emerge in the first place, how the socio-technical networks sought to ensure their success and how their success or failure depended at least partly on other similar networks and contextual processes. This position allows constituent elements, relations and interactions to be included in the explanation without logical contradictions, while also paying attention to potential and actual changes in many properties of the socio-technical networks (e.g. identities or preferences). In fact, this research is mostly about a change of identity: about the birth, growth, maturation, and decline of various socio-technical networks, networks of these networks and technological innovation systems.

2.3 Mechanisms revisited

Mechanisms were frequently mentioned in the first chapter; mechanismic explanation, on the other hand, was not. Therefore one may well wonder what constitutes a satisfactory mechanism-based explanation, especially when the different frameworks of the theoretical synthesis maintain somewhat different ideas about the notion of the mechanism in the first place. For example, I quoted Elder-Vass's definition of causal mechanism, *"processes that depend on interactions between the parts, interactions that only occur when those parts are organized in the particular way that constitutes them into wholes that possess this emergent property"* (Elder-Vass 2007a: 415). At the same time I also made a brief reference to Mayntz, who finds that *"if a cause produces an effect without intermediate steps, no mechanism is involved, and the stated relationship even runs the danger of being a tautology (Kitschelt 2003). The term "mechanism" should therefore be reserved for processes involving linked activities of several units or elements and not applied to "unit acts""* (2004: 242). So when speaking about MLP's mechanisms, I referred to the role of visions and values in legitimizing new technologies or the role of the government in creating niches. Yet I was also drawing on an author making a point that a mechanism should not consist of a single act. How can this contradiction be overcome?

In order to resolve this problem the term mechanism itself needs more reflection. Alas, even a preliminary glance at the literature reveals a swarm of definitions. For example, Hedström and Ylikoski (2010) outline nine different versions, whereas Gerring (2010) comes up with ten. Table 2.1 presents a selection of these.

As can be seen, different definitions entail different restrictions: some of them require mechanisms to be unobservable, intentional, system-internal or micro-level. One definition, on the other hand, is very wide, requiring only the specification of a certain effect and a pathway or a process—according to that definition, in principle, any event sequence could classify as a mechanism. Therefore let me pose the question in this way: what is mechanismic explanation supposed to achieve?

Table 2.1. Selected definitions of mechanism (Stinchcombe 1991: 267, Mahoney 2001: 580, Mayntz 2004: 241, Gerring 2010: 1500-1501, Hedström & Ylikoski 2010: 51)

Author	Definition	Source
Bunge	A mechanism is a process in a concrete system that is capable of bringing about or preventing some change in the system	Bunge 1997, 2004
Elster	A mechanism explains by opening up the black box and showing the cogs and wheels of the internal machinery. A mechanism provides a continuous and contiguous chain of causal or intentional links between the *explanans* and the *explanandum*	Elster 1989
Gerring I	A micro-level (microfoundational) explanation for a causal phenomenon	Gerring 2008, 2010
Gerring II	The pathway or process by which an effect is produced	Gerring 2008, 2010
Hedström	Mechanisms consist of entities (with their properties) and the activities that these entities engage in, either by themselves or in concert with other entities. These activities bring about change, and the type of change brought about depends on the properties of the entities and how the entities are organized spatially and temporally	Hedström 2005
Mahoney	A causal mechanism is an unobserved entity that—when activated—generates an outcome of interest	Mahoney 2001
Mayntz	Causal generalizations about recurrent processes	Mayntz 2004
Stinchcombe	Bits of 'sometimes true theory' or 'model' that represent a causal process, that have some actual or possible empirical support separate from the larger theory in which it is a mechanism, and that generate increased precision, power, or elegance in the large-scale theories	Stinchcombe 1991

I would argue that from this point of view there are three features essential to the notion: a mechanism 1) shortens the time-span between initial conditions A and outcome B by specifying a recurrent and characteristic sequence of processes between them (e.g. the steps by which a self-fulfilling prophecy can become true); 2) decomposes a collective outcome into an interaction between its composite entities (e.g. number of individual rational choices leading to market equilibrium); 3) requires the stability of certain background conditions (otherwise it might not occur in the first place, meaning that we would not be able to pick it up). *"A complete explanation of a social event would look like this:* e = f(U, I, O)*"* (Brante 2001: 184), where I is the level of the event, U is the underlying level in terms of which the mechanismic explanation is offered and O refers to overlaying levels that frame the levels in explanatory focus.

Note that this wording does not require mechanismic explanation to entail the above restrictions. The focus is on the explanation of a higher-level outcome in terms of lower-level interactions. This also leads to the conclusion that it is appropriate to talk about 'unit acts' in mechanismic terms, but only when such an act is itself being explained by a certain mechanism. That is, a unit act 1) can be an outcome (event) of a mechanismic explanation in terms of the interaction of lower-level entities; 2) can constitute a part of the causal chain in a higher-level mechanismic explanation, but; 3) cannot be considered a mechanism in its own right.

This implies a possible hierarchy of mechanisms which corresponds to critical realism's notion of ontologically stratified reality. Instances where explained events or outcomes act as building blocks for further explanations are not difficult to find. For example, Elder-Vass (2007c) synthesizes the views of Bourdieu and Archer on human agency to explain its emergence. On the other hand, social sciences offer virtually countless analyses of situations in which different individuals exert their agency, bringing about a consequence of some sort (sub-optimal solution to the prisoner's dilemma, market equilibrium, self-fulfilling prophecy, formation of an enterprise etc.). Another example comes from MLP: whereas Raven and Geels (2010) focus on niche-internal processes to explain the emergence of a niche in terms of a cycle of variation, selection and retention, the theory of socio-technical transitions excludes niche-internal processes and focuses on the interactions between the outcomes of niche-internal and regime-internal processes instead (figure 1.6). Thus the above contradiction between different takes on mechanisms can be reconceptualized as mere differences between explanatory focuses.

But often the question is not about what is to be explained, but how far one should go with such an explanation. In other words, what level mechanisms should one employ in order to arrive at a satisfactory explanation? The position advocated by analytical sociology is that of structural individualism, *"a methodological doctrine according to which all social facts, their structure and change, are in principle explicable in terms of individuals, their properties, actions and relations to one another. It differs from traditional notions of methodological individualism ... by emphasizing the explanatory importance of relations and relational structures"* (Hedström & Bearman 2009b: 8). Thus the importance of supra-individual structures is acknowledged, yet the ultimate aim is to offer an individual-level explanation.

Although commendable in its ambition, the pragmatic necessity of this doctrine in most cases remains questionable. Why? One reason has been suggested by Arthur Stinchcombe: *"The theory of the mechanism in higher-level theory is often radically shorn of the complexity it has in the discipline that specializes in the level the mechanism comes from, especially eliminating small but theoretically interesting effects, effects that are controlled by compensating mechanisms, or effects that are not systematic at the higher level"* (1991: 384). Stinchcombe brings an example of the ability to compute internal transfer prices for interdivisional supplies transfers. While a majority of the population does not have enough mathematical training to perform these kinds of calculations, one can assume corporations usually tend to hire people who do have such training (presuming that such talent is sufficiently available for every enterprise). Yet another example is brought by Mayntz (2004), who refers to the analysis of bargaining processes between organizations. She finds that *"as long as it is possible to attribute actor quality to larger social units"* (2004: 248) explanation in terms of individuals is simply unnecessary. Thus in these cases the differences on an individual level are offset on an organizational one. Therefore by going down to the level of the individual one might end up in a world of fascinating and intricate mechanisms which, however, have a negligible impact (if any) on higher-level dynamics. For pragmatic reasons then, the explanation in terms of individual actions should often be avoided.

Mayntz (2004: 246–252) has also pointed out that the adoption of structural individualism has led to a certain empirical bias: analytical sociology's greatest success seems to have been achieved when interdependent and uncoordinated individual actions lead directly to an emergent macro-effect (e.g. spatial segregation). At the same time, the mechanisms of other types of entities (e.g. states) have not been much explored. One can draw an analogy with STS, in which the prevalence of micro-analysis has likely led to the dominance of certain types of results and the relative neglect of others (e.g. the relative inability to theorize large-scale socio-technical entities and structures). Therefore I would welcome the 'in principle' part of structural individualism, but empirically turn attention to higher-level entities instead. I will return to this theme in chapter 4.

Finally, there are two criticisms made about mechanismic approaches that need to be addressed. One comes from George and Bennett (2005: 7–8), who contrast mechanismic theories with middle-range theories, whereby the first is taken to focus on a single mechanism while the second deals with recurrent conjunctions of mechanisms. George and Bennett prefer the latter to the former because middle-range theories are said to be less laboratory-like and better able to account for the context of the processes in focus. However, theoretically speaking a lot of 'laboratory work' might be desirable to isolate the mechanism from the flux of change in the first place, so as to be able to see how it might manifest itself in different environments. Moreover, George and Bennett seem to have missed the opportunity that configurations of causal mechanisms might constitute meta-mechanisms for higher-level outcomes that actualize when certain parameters are kept constant. In fact, theoretically it might be always possible to come up with some stable background variables which provide context to whatever change we have in mind. The main difficulties lie in determining that relevant context (what is stable?), detecting unique, contributing but non-essential causes (since many events might be 'overdetermined', see below) and deciding the operating level of mechanisms. For these reasons, arriving at stylized mechanisms is a formidable task and the actual research process likely involves alternating between retroduction and retrodiction (Lawson 1997, Elder-Vass 2010): reasoning from observed patterns to underlying mechanisms, and vice versa.

Gerring (2010) accuses mechanismic approach of lacking in substantial novelty: after all, social sciences have been detecting the causal paths of various outcomes for a long time. However, he also acknowledges that mechanismic approach enables one to be more aware of the importance of causal pathways (*ibid.*: 1503). In my opinion it is precisely this point that justifies the endeavour: the terminology of mechanisms provides a kind of meta-language capable of uniting different process theories in a single framework. By following this logic it might be possible to re-read existing literature, recognize certain models or event sequences as formulations of mechanisms and compile a taxonomy of them, thereby arriving at a larger degree of systemicity. Once again, it is the drawing of connections that potentially emerges as a valuable contribution.

With these questions out of the way it is time to turn to the research design itself. The following section is devoted to case study and its role in the current research.

2.4 Case study

Similar to a large number of STS works, this thesis adopts a case-study approach, which is defined as a *"detailed examination of an aspect of a historical episode to develop or test historical explanations that may be generalizable to other events"*, wherein case refers to *"an instance of a class of events"* (George & Bennett 2005: 8, 17). In other words, the approach aims at a close inspection of a small number of cases, taking into account the complexity of real-life interactions. Owing to its time-consuming nature, it has to make a trade-off between the number of cases/statistical comparability and explanatory richness. *"Case study researchers are more interested in finding the conditions under which specified outcomes occur, and the mechanisms through which they occur, rather than uncovering the frequency with which those conditions and their outcomes arise"* (*ibid.*: 31). On the other hand, closeness to the data means that the events connecting initial conditions with outcomes can be established with much greater certainty (see the above discussion about the differences between variance and process theories).

George and Bennett (2005: 75–76) distinguish between six different types of case studies: 1) atheoretical/configurative idiographic; 2) disciplined configurative; 3) heuristic; 4) theory testing; 5) plausibility probes; 6) 'building block'. Since the stress of this study is on developing more specific theoretical propositions using analytical tools presented in the first chapter, types one and three—*"good descriptions that might be used in subsequent studies for theory building, but by themselves, do not cumulate or contribute directly to the theory"* and attempts to *"inductively identify new variables, hypotheses, causal mechanisms and causal paths"* (*ibid.*: 75)—are the most relevant here.

The main reason for outlining the importance of these two types of case studies is that in my view heuristic case studies actually require atheoretical/configurative idiographic ones to be conducted first. It seems quite impossible to arrive at valid conclusions without having established a solid factual basis first (see the next section): to ensure that the version of a historical narrative as written down by the researcher would be more likely than (at least some) other alternative explanations. The overdetermination of effects—the fact that the same outcome might have been achieved with fewer causes than was actually the case—justifies this stance. As noted by Gerring: *"Indeed, it is often* difficult to tell *which of the many features of a given unit are typical of a larger set of units (and hence fodder for generalizable inferences) and which are particular to the unit under study. The appropriate response to such ambiguity is for the writer to report all facts and hypotheses that might be relevant – in short, to overreport"* (2004: 346). This strategy also provides opportunities for secondary analysis: the same historical narrative can be read with different theoretical ideas in mind. And finally, one should not exclude aesthetic considerations: a well-written narrative is simply an interesting and engaging read on its own.

In the previous chapter I presented three research questions that the current study aims to answer. Noting how it entails operating on three different levels of aggregation, the beginning of this chapter discussed their correspondence to Elder-Vass's seven general criteria. Following the advice of George and Bennett (2005, ch. 4) I will now specify further what constitutes a case, justify the case selection while noting possible biases and highlight the changes in outcomes to be explained.

The basic unit underlying the definition of all three levels of aggregation (intra-case, inter-case, system-level) is a socio-technical network formed around 'domestic PC production attempts' in Soviet Estonia, Latvia and Lithuania. By 'domestic production attempt' I mean that the countries in question must have had some involvement in at least the hardware production phase, and that the goal of each project was to produce machines beyond the particular prototype (this excludes the programming of novel software for devices produced elsewhere and one-off, customized hobbyist designs). I equate the terms 'personal computer' and 'microcomputer', by which I understand small-scale, general-purpose computers directly operated by users. This would exclude older-generation devices (mainframes, mini-computers) as well as machines built for special purposes (e.g. programmable calculators, various controllers). The number of cases corresponding to these criteria (for which sufficient information could be found) was thus narrowed down to ten (three in Estonia, two in Latvia, five in Lithuania).

Corresponding to the distinction between atheoretical/configurative idiographic and heuristic case studies, the choice of cases can be justified in two ways—historically and theoretically. From the historical point of view it suffices to note that fairly little is known about Soviet computing to date and, to my knowledge, no systematic overview has been written. In that sense the cases serve well in contributing to the pool of historical knowledge about the history of Soviet personal computing.

Theoretically, the case studies serve to extend dynamic multi-level perspective theorizing to domains other than socio-technical transitions. The choice of the frameworks presented in the previous chapter was based on an assumption that the simultaneous attention to dynamics within and outside socio-technical networks, networks of such networks and innovation systems would result in more inclusive theoretical analyses and models. In addition, it was assumed that some results of the synthesis can also feed back to each framework separately (see the conclusion).

On different levels the selected cases offer distinct theoretical possibilities: 1) as the observed events took place from the 1970s to the 1990s (note: not for every case separately), it is possible to analyse the evolution of the projects in very different environmental conditions. (These include the 'normal' functioning of the socialist system, specific reforms undertaken from the mid-1980s, mounting pressure, gradual loosening and eventual disintegration of the Soviet system, and a resulting tumultuous change from socialism/totalitarianism to capitalism/democracy); 2) on the level of networks of socio-technical networks it is possible to observe differences in local interactions after the occurrence of a specific landscape stimulus (central reform of school computerization); 3) finally, at the system-level it is possible to compare the experience of three (seemingly) similar countries and to see whether the differences between them outweigh the similarities when it comes to conceptualizing the overall transformation process.

It has to be noted that the above selection suffers from two kinds of biases. First, it more or less excludes designs which were realized by hobbyists, usually self-assembled, not serially produced, and unofficially sold. For example, all over the Soviet Union, including the Baltic republics, various clones of Sinclair computers were built, some of which became more popular than others and hence diffused more widely than just a few machines. But even more fundamental is the bias towards more-or-less realized projects. Why this is so is easy to understand from a pragmatic point of view: it is very difficult to find projects which were only briefly contemplated and then abandoned due to a severe gap between wants and resources. The issue with hobbyist computers is similar: the information about different designs is hard to come by, the original designer is often unknown, and the circle of hobbyists loose and diffuse, making it very complicated to track down and map the exact structure and extent of the network. Therefore it must be kept in mind that the importance of various factors (or the factors themselves) outlined in chapter 4 may be somewhat different for excluded cases.

Finally, the studied outcomes for socio-technical networks cover a range from planning to prototype to trial batch to mass production. The explanatory focus of the middle level is on the emergence of a local dominant design (for a particular functional niche, see chapter 4), while the system-level analysis aims to explain the transformation of the system. In the latter cases the outcomes for each country do not vary. A limitation of the study on the intra-case level must also be noted: owing to the time-frame of the research and resulting data insufficiency, only six cases (three from Estonia and three from Lithuania), were used in formulating theoretical propositions. All cases were included in the analysis of higher-level dynamics, however.

2.5 Selection, assemblage and triangulation: on the nature of historical sociology

The choice of the case study approach does not imply a single method of data collection: on the contrary, usually many different sources are required to constitute a sufficiently thorough understanding of the cases. Therefore the final section of this chapter will address various types of evidence, ways of obtaining and combining them and the means by which the most likely historical narratives might be constructed.

The recency of the events offers a chance to draw on a wider variety of sources than would often be the case for a historical study. I would group the data sources into three categories, presented in descending order of importance: the first group comprises semi-structured interviews with people involved in the projects; the second group includes various written materials (documents, archival materials, popular and scientific articles, histories with different analytical focuses etc.); the third group is constituted by the physical artefacts themselves. Respective strengths and weaknesses of these data sources are summarized in table 2.2.

Table 2.2. Advantages and disadvantages of different sources of evidence (adapted from Yin 2009: 102, expanded by the author on the basis of Bryant 1994, 2000, and his own research experience)

Source of evidence	Strengths	Weaknesses
Interviews	• Targeted—focus directly on case study topics • Insightful—provides perceived causal inferences and explanations	• Bias due to poorly articulated questions • Response bias • Inaccuracies due to poor recall • Reflexivity—interviewee says what interviewer wants to hear
Documentation	• Stable—can be reviewed repeatedly • Unobtrusive—not created as a result of the case study • Exact—contains exact names, dates, references, and details of an event • Broad coverage—long span of time, many events, and many settings	• Retrievability—can be difficult to find • Biased selectivity, if collection is incomplete • Reporting bias—reflects (unknown) bias of author • Access—may be deliberately withheld
Archival records	• [Same as for the documentation] • Precise and usually quantitative	• [Same as for the documentation] • Accessibility due to privacy reasons
Existing scientific works	• Exact—gives critically assessed evidence about the events that the researcher is unable to cover in depth, including background information	• Selectivity—the presented events have been pre-selected by the author • Bias—a danger to confuse the presentation of facts with author's interpretation of them
Physical artefacts	• Provide insight into technical conditions and operations of the time • Can prompt new interview questions, can act as a memory aid for the interviewee	• Selectivity • Availability

Semi-structured interviews provide a good combination of focus and flexibility. They enable the researcher to get answers directly to the questions s/he is looking for, although the actual wording of the interview questions usually differs from researcher's analytical ones (being more specific, worded without the theoretical jargon etc.). At the same time it leaves enough room of improvisation—the order of questions can be switched, some questions dropped if they do not seem to lead anywhere (e.g. the respondent explicitly saying that he or she does not know anything about a particular event), new probing questions invented on the spot etc. Interviews are especially suitable for teasing out the motives of participants, informal relations between them, reasons behind certain choices and developments—all the aspects not present in written materials or there only indirectly and thus requiring some inferences to be made.

It has to be noted here that although every case should be studied with the same research questions in mind—what George and Bennett call 'structured focused comparison' (2005, ch. 3)—this should not be taken to hold for each interview. The structure of each interview varies somewhat from interviewee to interviewee: for example, it would be pointless to ask the end user about the reasons behind design decisions made by the developers. In fact, only a few questions do make sense for every interviewee. However, even the specific interview questions are likely to be recurrent across cases. Therefore in the design phase a list of general questions was constructed for the cases as a whole, so that appropriate questions could be chosen and adapted for each interview.

In addition to the aforementioned advantages, interviewees can often guide the researcher to other important sources of evidence (including other actors, documents and artefacts). Combined with the fact that most of the key actors were still alive at the time of research, interviewing was chosen as a primary method.

At the same time, however, interviews are not without disadvantages: deficient research questions can often lead to one-sided answers, especially when the interviewees themselves are not observant enough to notice them and correct the interviewer. A reverse can also happen: an interviewee can tailor his or her responses according to a personal agenda or the perceived interests of the researcher. And finally, especially when it comes to specific dates and numbers, people's memories are often far from perfect and any assessments are likely to differ considerably between actors. A telling example comes from the work on Juku, in which interviewees' estimates of the number of computers provided for schools ranged from few dozens to hundreds to thousands. But according to a source written a few years after these events (Jürisson 1995) the actual amount was 2,500, which no interviewee was able to recall.

Examination of various documents and archival records can overcome some of the problems with interview data. Such sources can be consulted a number of times, can give exact details about certain events and, in the best case, can lead to the reformulation of interview questions or recasting of the narrative in different terms. However, the relevant documents might be difficult to locate, or some of them might be destroyed or held back. It should not be assumed, however, that *"all kinds of documents ... contain the unmitigated truth. In fact, important in reviewing any document is to understand that it was written for some specific purpose and for some specific audience* other than *those of the case study being done"* (Yin 2009: 105). Therefore many actual considerations of the actors might not be manifest in documents.

Nevertheless it seems that historians tend to assume that documents still somehow provide a more 'authentic' picture of past events. For example, in an otherwise excellent guide to constructing international history, Trachtenberg (2006) reserves only a few pages for interviewing. After warning the researcher against the fallible memories of the interviewees and differing levels of honesty he comes to a conclusion that *"as a general rule you cannot quite take what people tell you at face value, and what you learn in this way is not quite as solid as what you learn from the documents"* (2006: 154). He then goes on to discuss in detail the techniques of avoiding different biases in documentary material arising from selected availability.

I may be overgeneralizing here, but it seems to me that more than anything else this characterization reflects convenience derived from tradition: as historians are used to working with documents they are better aware of the advantages and disadvantages of these. The weaknesses of other, not so familiar, sources are amplified and hence discarded more easily. However, I would argue that the inferences work quite similarly in both cases: for documents, one has to infer the possible motives of the participants on the basis of accessible documentary material. During the interview the question can be asked directly and the honesty of the answer judged. In both cases, it is the work with other sources (documents or interviewees) that helps the researcher to make the decision about whether the particular piece of information is to be considered trustworthy or not. In my view assessments like Trachtenberg's lead to reinforcing the perceived history–sociology divide and encourage the researcher to stick to the sources and techniques with which they are already most familiar.

In the current study documents were ascribed secondary importance mainly because of three factors: 1) many documents and even archives as a whole were destroyed when the Soviet Union collapsed; 2) for many documents it is unknown whether they exist at all, and if so then where (quite often important documents were received from interviewees, who had kept personal copies); 3) the totalitarian regime meant a prevalence of 'double speak', i.e. in most cases (some) actual motives were not (could not be) present in the documents, and so the declared actions and their actual reasons needed double-checking with primary sources, whenever possible.

Other valuable sources include prior works on related subjects. Here I am referring to articles and books which include historical background or direct information about the issues at hand: for example, some brief descriptive writings on computing in the Soviet Baltics (Telksnys & Žilinskas 1999, Tõugu 2009), analysis of the transition of the Soviet Estonian telecommunications sector (Högselius 2005), national histories or histories of the Baltics (e.g. Zetterberg 2009, Kasekamp 2010), analysis of Soviet political economy (Kornai 1992), review of Soviet educational reforms and information technologies (Kerr 1991) and so on. The obvious advantage of these sources is that they provide information about contextual factors important to understanding the case and formulating the appropriate research questions, but which exceed the immediate scope of the research. Moreover, most of them have been peer-reviewed, which raises their reliability.

The drawback is having no control over authors' choices regarding the selection, presentation and interpretation of facts. In the best case the writings might only cover some aspects of interest. In the worst case, however, there is a danger of taking an author's questionable inferences as matters of fact. Following Bryant's distinction between reportage and interpretation, wherein the first *“consists of information that pertains to basic questions of what, where, when, who, how many, etc.”* while the second *“involves establishing the meaning and the significance of these historical 'facts', i.e., the materials that constitute reportage”* (1994: 13) then overreliance on secondary sources might lead to ascribing the quality of reportage to interpretation. Moreover, some interpretations might be heavily contested by specialists. So ideally the use of secondary sources should be coupled with some knowledge about recent progress made by historians.

Finally, there is also a chance to inspect the artefacts themselves. The actual experience of using, touching or examining the computer can prompt new questions about design decisions, components etc., potentially bringing forth novel insights. Not every device may be available for such purposes, especially not those that never progressed to or beyond the prototype phase. In this study such devices or their components were sometimes used as memory aids for the interviewees (e.g. one used a printed circuit board from Tartu computer to explain the weakest spot in the design) and as such they were of tertiary importance.

The most arduous task of historical research is to combine these multiple sources of evidence obtained by the use of various techniques so that they form an integrated and convincing whole. In the best case *"the events or facts of the case study have been supported by more than a single source of evidence"* (Yin 2009: 116); that is to say, they have been triangulated. In this case multiple sources of evidence confirm and support each other (inter-triangulation). I would also say that there is a possibility of intra-triangulation: this happens when conducted interviews or gathered documents are compared with each other and common assessments and perceptions are detected.

The work of a historical sociologist can be compared with that of a detective (George & Bennett 2005: 218): when faced with several suspects and clues one must decide on the basis of evidence which causal explanation would be the most likely. Sometimes one type of hypothetical explanation can lead the researcher to gather more evidence to test that hypothesis, giving further support to it or, failing to find anything (or finding something completely contrary to expectations), disproving it. *"Historiographic composition is thus ultimately disciplined by the empirical and analytical constraints that are placed on interpretations by the available source materials"* (Bryant 2000: 501). In other words, although the evidence is usually incomplete, it is possible to arrive at more or less valid (though potentially fallible) interpretations. Bryant himself demonstrates how the connection between the Greek hoplite revolution and the rise of democracy is supported by a number of different interweaving elements, including the findings of war equipment, inferences about them, demographic data, historical texts etc. Alternative accounts, on the other hand, have failed to embrace the totality of evidence or have drawn dubious comparisons, interpretations or inferences (i*bid.:* 500–501). Trachtenberg has summarized the essence of historical critical analysis as such: *"You first identify the author's general thesis. You then try to understand the structure of the argument that supports the thesis. In particular, you try to see how general conclusions rest on more specific claims. You then evaluate those specific claims in terms of the evidence that the author gives to support them. It is all very straightforward. Along the way, you are taking your measure of the intellectual quality of the work as a whole, and when you find someone twisting the evidence, your opinion of the work plummets"* (2006: 73).

Although he is speaking about the analysis of existing works, I think that this quote, although worded in reverse, entails the basic mechanism of the process of constructing historical narratives: interpretations have to be grounded in evidence, the latter itself to be viewed and evaluated critically, with remaining gaps being acknowledged honestly and self-reflexively, and efforts made to fill them.

How is this critical evaluation achieved? Bryant offers two answers: source criticism and what he calls sociology of knowledge. The first directs attention to the fact that sources of evidence always represent reality in partial ways or even deliberately misrepresent it, according to the interests, ideas, values and ideologies of the author (of a document or a spoken word). It means that the historian must not only turn attention to what is manifest, but also keep an eye open for hidden implications or gaps in the record. The practical ways to achieve this are many: for example, looking for and comparing claims about the same events in different places, collecting different evidence from various angles, detecting the biases of the sources and assessing the information in this light (Trachtenberg 2006: 147–162). This strategy helps to ensure that evidence is sufficient and interpretations are valid. Sociology of knowledge, advised as a second check, helps to situate the historical sociologist in the site of knowledge production. By turning attention to how the production of knowledge is always partly shaped by the social environment of the analyst, it potentially helps to reveal his or her 'blind eye' and prevent hasty overgeneralization of findings. In sum: *"In detecting the biases inherent in created records and monuments, source criticism exposes their manifest and latent ideological intentions and limits, thereby allowing for counteractive reconstructions that discern or apprehend the larger realities that were screened or amended for contemporaneous and possibly posterior indoctrination. In detecting intellectually paradigmatic as well as socially partisan forms of perspectival bias in contending interpretive accounts, the sociology of knowledge correspondingly exposes and so neutralizes their effects, thereby allowing for both informed arbitration and objectively defensible selection-decisions"* (Bryant 2000: 510–511).

Although I am somewhat sceptical about the extent to which the researcher could entirely avoid his or her socialized biases—after all, what makes them so effective is exactly their implicitness, their being hidden from the observer's gaze—there are at least two personal aspects of my own background which merit brief consideration.

The first is my background in social sciences, and that I have received no formal technical education. At the same time it is the very core of STS that in order to explain certain phenomena, causes both technical and social need to be accounted for. This sets various potential barriers, e.g. temptations to avoid important but complicated technical explanatory factors, limited understanding of various technical details or an inability to make independent decisions about the advantages and disadvantages of different technical alternatives. I have tried to decrease these hazards by 1) obtaining more knowledge about computing; 2) asking different interviewees about the technical choices made, including possible alternatives—occasionally some aspects were clarified later on (by second interview or e-mail exchange) ; 3) consulting independent experts (e.g. curators of computer museums).

The second concerns the fact that I am a native Estonian speaker but do not speak Latvian or Lithuanian. This means that some interviews were conducted in a foreign language (English or Russian). As such, some of the richness of the oral data may have been lost. Also, the language barrier influences my ability to seek out and work with written materials. To overcome this issue I have been aided by various people, including but not limited to interviewees, for locating and collecting various written sources. Native speakers also helped me to translate various documents and newspaper articles.

In this chapter I have discussed a wide variety of methodological principles, ranging from the general to the specific. In the next chapter I will put all of these principles into practice and present the historical narratives of the development of PCs in the Soviet Baltic countries.

3. Historical narratives

This chapter presents the stories of the evolution of domestic PC construction attempts in the three Soviet Baltic countries. The material was compiled on the basis of interviews, documentary evidence and existing publications.

The interviewees were chosen to reflect the multifarious nature of the projects: software programmers, project managers, chief engineers, members of committees, teachers and so on, each highlighting different aspects of the story. Overall, interviews with 58 individuals were conducted, 28 in Estonia (14 of them previously interviewed for my Master's dissertation (2009), a thoroughly revised and updated version of which constitutes section 3.1.1 of this thesis), eight in Latvia and 22 in Lithuania. Interviews were conducted in Estonian, Russian and English,[12] and lasted from 30 to 150 minutes. Generally the interviews were conducted face-to-face, apart from two interviews on Skype and one by e-mail. Furthermore, as I could not establish a direct contact, three interviews with one interviewee were conducted by Andrejs Skuja. Including this individual, nine people were interviewed more than once. Some interviewees were later contacted by e-mail for additional clarifications. The full list of interviewees is provided in appendix A. In addition, other people were consulted regarding various minor aspects (e.g. finding the key people, locating the written sources, obtaining preliminary information about the artefacts, specifying the names of the organizations etc.).

To complement the information obtained from interviews, written materials were also collected where possible. This includes journal and newspaper articles, book chapters, technical documentation, photos, academic publications etc.

The information obtained from various sources was compared and assembled in such a way as to present the most plausible course of events. In the course of writing up

12 Note that I have occasionally made slight corrections to the interviewees' English quotes (word order, grammar) to make the intended meaning clearer, as none were native speakers.

the preliminary draft some interviewees were allowed to read parts of the overall narrative and assess its plausibility—however, the final decision as to whether these assessments were well-motivated and thus whether to accept them in full, partially or reject them altogether, was made by the author, who thereby takes full responsibility for possible omissions, false information and biases.

In the following sections the development of ten cases—five in Lithuania, three in Estonia and two in Latvia—will be described in more detail (see table 3.1 for their technical characteristics and comparison with contemporary Western PCs). The 'missing' cases which were detected but on which no substantial detail could be found are also briefly described when relevant. The account begins from the Estonian cases, continues with those from Lithuania and ends with the Latvian projects.[13] As such the section on Estonia is a bit longer than others, since the first-time description involves a fuller explanation of many recurring factors that need only be mentioned later. Such background is required to show not only the actors' choices, but also their contextual reasoning, so that the possibility of alternative options (or the lack of them) could be assessed. But it is only fair to admit that the amount of detail available also partly derives from the fact that empirical fieldwork in Estonia had started somewhat earlier.

The extensive use of oral sources also raises a problem for data presentation: after all, the claims made in the narratives often rely on the (potentially fallible) memories of the interviewees. But referencing each and every factual statement would unnecessarily clutter the text, disrupt its flow and seriously undermine its readability. Therefore I have decided to exclude references to what I have decided to be

13 Thus the focus is strictly on local developments. Useful analysis of wider trends and movements (directly or indirectly contributing to the evolution of current cases, but not being mainly about them) can be found in many other works. Of those, I have found especially useful Kornai's analysis of the political economy of communism (1992), Åslund's analysis of post-Soviet economic and political transition (2002), Kasekamp's history of the Baltic states (2010), Ceruzzi's general history of computing (2003), Gerovitch's (2002) and Malinovsky's (2010) early histories of cybernetics and computers in the Soviet Union respectively. Excellent contemporary surveys in English about the state of Soviet computing can be found in Goodman *et al.* (1988) and Judy and Clough (1989, 1990), while Kerr (1991) provides a fine overview of Soviet computer literacy reforms.

relatively non-controversial claims. This usually means that statements that are supported by more than one source (e.g. the participants of the projects) or claims the veracity of which I had no reason to doubt (e.g. the interviewee's occupation). As a general rule, for every aspect of the development of the particular case I have tried to rely most on the accounts of the people most intimately connected to them (e.g. when speaking about user experience the accounts of the teachers or the members of the education sector were preferred to those of hardware constructors).

However, there are still a number of occasions when references to the interviews will be made. I have reserved these for the following situations: 1) a direct quote; 2) a particularly detailed statement, especially when no reference to a document could be found (e.g. dates, quantities); 3) a particularly controversial or conflicting claim; 4) speculations about the motives and/or actions of other players about which definitive information remains unknown. By using the words 'likely' or 'probably' I also try to point out the situations in which I am presenting my own interpretation or an educated guess on the basis of indirect evidence.

Table 3.1. Selected characteristics of Soviet Baltic PCs and some Western contemporaries*

	Year**	Processor	ROM	RAM	Display	Tape recorder	Floppy	Hard disk	Operating system
Estonia									
Entel	1983	КР580ВМ80А, 8-bit, 2.0 MHz (Soviet Intel 8080A analogue)	16K	64K	B&W or colour TV (8), 90x32 symbols, 180x96 pixels	Yes	No***	No	CP/M
Tartu	1984	КР580ВМ80А, 8-bit, 2.0 MHz	20K/16K	64K	B&W or colour TV (16, Kursk)/ B&W TV (Palivere), 64x25 symbols, 384x256/768x256 pixels (Kursk), 384x256 pixels (Palivere)	Yes	No (Kursk)**/ Yes (Palivere)	No	CP/M
Juku	1985	КР580ИК80, 8-bit, 2.0 MHz (Soviet Intel 8080 analogue)	16K	64K	B&W TV, 40x24/64x20 symbols, 320x240/384x200 pixels	Yes	Yes (2)	No	CP/M
Latvia									
VEFormika	1977	КР580ИК80, 8-bit, 2.0 MHz	0.25-2K	56K	Black-and-green Videoton V24 display, 80x32 symbols	No	No	Yes (2x 2,4 MB)	ДОС-Ф (DOS-F)
VEF Mikro 1021	1981	КР580ИК80, 8-bit, 2.0 MHz	4K	16-32K	32x16/64x16 symbols	Yes	No	Yes	ОСРВ-ВЕФ (OSRV-VEF)
VEF Mikro 1022	1981	КР580ИК80, 8-bit, 2.0 MHz	2-4K	62K	Black-and-green Videoton V24 display, 80x32 symbols	No	Yes	-	ISIS-II, CP/M, RMX-80

VEF Mikro 1024	1983	КР580ИК80А, 8-bit, 2.0 MHz	4-16K	60K	80x24 symbols	Yes	Yes	-	ISIS-II, CP/M, RMX-80
VEF Mikro 1025	1983	КР580ИК80А, 8-bit, 2.0 MHz	2-4K	62K	80x25 symbols	Yes	Yes	-	ISIS-II, CP/M, RMX-80
Lithuania******									
BK-0010Š	1986	K1801BM1, 16-bit, 3.0 MHz	32K	32K	B&W or colour TV (4), 32x25/64x25 symbols, 256x256 (colour)/512x256 pixels	Yes	No**	No	Initially only some monitoring software
Santaka	1986	UA880D, 8-bit, 3.5 MHz (East German Z80 analogue)	16K	48K	Colour TV (8), 32x24 symbols, 256x192 pixels	Yes	No	No	Sinclair ZX Spectrum compatible (Sinclair BASIC)
Poisk	1988	KP1810BM88, 16-bit, 5.0 MHz (Soviet Intel 8088 analogue)	16K	128K	TV (B&W, colour) or CGA colour monitor (16), 40x25/80x25 symbols, 320x200 (4/16)/ 640x200 (2/16) pixels	Yes	No**	No**	MS-DOS
Sigma 8800	1990	KP1810BM88, 16-bit, 4.77 MHz	16K	64K	B&W or colour monitor (16), 80x25 symbols, 720x348 (B&W)/640x200 (4/16) pixels	No	Yes (360K)	Yes (20MB)	MS-DOS
Western contemporaries									
IBM PC/XT	1983	Intel 8088, 16-bit, 4.77 MHz	64K	64K-640K	CGA colour monitor (16), 40x24/80x24 symbols, 320x200/6430x200 pixels	No	Yes (360K)	Yes (10-20MB)	MS-DOS

IBM PC/AT	1984	Intel 80286, 16-bit, 6.0 MHz	64K	512K	EGA colour monitor (16), 80x24 symbols, 640x350 pixels	No	Yes (1,2MB)	Yes (from 20MB)	MS-DOS
Apple Macintosh II	1987	Motorola MC 68020, 16-bit, 15.66 MHz	256K	1MB	Colour monitor (16/256), 640x480 pixels	No	Yes (1,2MB, 1 or 2)	Yes (from 20 MB)	Macintosh System 4.0, Finder 5.4

* These characteristics attempt to refer to the first mass produced or 'standard' configuration, not to the possible-in-principle or configuration-in-development. For display, the number of colours are given in parentheses.
** The approximate year of the working prototype.
*** Respective peripherals could be bought and connected. Custom solutions have been excluded.
**** The characteristics of Lema's PC/XT (see section 3.2.4) cannot be given because of insufficient information and quite likely the lack of a stable configuration. The specifications of IBM's original computer have been provided instead.

Sources: VEF (1983), Krivchenkov (1986), Elektronika BK-0010 user manual (ca. 1986), Malsub (1986), EKTA (1987) Videnieks *et al.* (1987), Märtin (1988), Santaka user manual (ca. 1988), Talanov (1988), Tartu user manual (1989), Basmanov *et al.* (1990), STIMTI (1990.15.02), Boyko (1991), Old-Computers.com, CPUShack, CPU World, various interviews.

3.1 Estonia

3.1.1 Juku

In April 1984, Soviet central authorities initiated an educational reform to start teaching informatics in secondary and vocational schools. A resolution followed in March 1985, stating that 120,000 school computers for at least 8,000 computer classes all over the Soviet Union (USSR) would be centrally produced between 1986 and 1990 (cited in a resolution from 1985.27.05).[14] The reform plan was likely influenced by the growing use of PCs in Western countries. The importance of personal computing had started to pervade the minds of Soviet authorities: the catchphrase 'second literacy', coined by esteemed Soviet computer scientist Andrey Ershov (1981.27.07, 1985a, 1985b), was widely used with an implicit or explicit expectation that in the future programming skills would be essential for virtually any social activity.

By that time two PCs—Entel (see section 3.1.3) and Tartu (3.1.2)—were already being developed in Estonia. The Tartu working group had started talking about their design as potentially suitable for school needs. This promotion caught the attention of people associated with the Institute of Cybernetics (IoC) in Tallinn, who decided that the idea of a domestically produced school computer was a good one in principle, only that the IoC should be the one realizing it instead (Eller interview). This could have served both ends: to do the 'Estonian thing' while gaining prestige for the IoC. The idea sparked the interest of the rector of the Tallinn Polytechnical Institute, Boris Tamm, also a previous vice director of the IoC, who quickly became the most vocal proponent of the endeavour.

On May 12th, 1985, a meeting between the representatives of various ministries (communication, education, finance), the local Planning Committee (responsible for the allocation of resources on union republic level), the Estonian Communist Party, the education sector (different education committees, representatives from

14 For documents, newspaper articles and other similar written sources the dates are specified as exactly as possible. This degree of precision will be maintained in the bibliography section.

institutions of secondary and higher education etc.), the IoC and a possible producer (RET plant) was held. The IoC presented five criteria for the school computer: 1) reliability; 2) low price; 3) simplicity; 4) expandability; 5) connectivity to other computers.[15] It then presented specifications for its prototype: Soviet Intel 8080 analogue microprocessor, 16 KB ROM, 64 KB RAM, black-and-white TV display, tape recorder for external memory, programming languages (BASIC, assembler), text editing software etc. Future expansions included local networking, printer interface and a floppy disk drive. In the IoC's vision this was not supposed to be a high-end product, but 'good enough' so that it could be designed and put into production as quickly as possible (Tõnspoeg interview). At the same time the too-narrow view of its uses was to be avoided: *"Computer is not a calculator with a TV but an information processing device to be used not only to teach programming but in teaching process* [in general]*"* (Jaaksoo's statement in the IoC meeting protocol, 1985.12.05).

The IoC's proposal was approved and it promised to deliver a working prototype in a few months. Meanwhile, letters from the local Council of Ministers and the Academy of Sciences would be sent to the authorities of the Ministry of Communications Industry in Moscow so that the latter would approve mass production in its RET factory in Tallinn. However, since there was no computer industry in Soviet Estonia, no large factories churning out large numbers of PCs and thus no real experience of such mass production, one could well ask why do it in the first place? In order to understand why regional production was advocated a contextual detour is needed.

In the planned economy the production of enterprises was managed by central authorities who allocated a certain fund for each union republic, which in turn dealt with further allocation at the local level. Alternatively, some factories[16] (such as

15 Computing in school was commonly imagined as a network, where the teacher could monitor the progress of students and students in turn could use the teacher's floppy disk for saving data (since the latter were in short supply).

16 Officially RET was called a 'production union' because its facilities extended to numerous locations in Estonia. The factory in Tallinn would be considered a part of the production union as a whole.

RET) belonged to the military–industrial complex, in which case they were under direct central control. Either way the process was generally cumbersome: one had to plan how many components would be needed for how many years, request them and then wait for a central decision as to whether the components would be allocated from existing reserves, whether the request would have to wait until next year or whether the application would be rejected altogether. The needs could be negotiated with the centre and personal connections used to pull favours, but in the end the relation was profoundly unsymmetrical: the power of final decision was firmly in the hands of central authorities.

Add to this what János Kornai has aptly called 'economics of shortage' (1980): constant scarcity of resources of every kind. Money was often secondary, the approval to buy the resources primary. If resources are scarce (this being especially so for something as novel as a computer), but everyone must get something according to the central plan, then everyone will be dissatisfied in the end, unless the production increases dramatically to, say, 120,000 additional computers. The everyday experience of Soviet reality had made people very wary of official promises of near-future abundance of more-or-less anything.[17] *"*[The] *Soviet Union* [was] *a country of dreams"* is how one of the interviewees described the situation (Credit Ališauskas interview).

What about ordering the computers from abroad? Again the flow and allocation of foreign currency was strictly controlled. Soviet roubles were normally non-convertible—a private individual was generally forbidden to own foreign currency, and organizations needed a special account for foreign transactions. The permit to use currency and respective allocation had to be centrally approved. Although at the time the USSR was contemplating a large-scale import of Western computers

17 To take the most general example, Vahtre (2007: 168–169) describes a situation in 1980 when it suddenly turned out to be impossible to obtain the programme of the 22nd congress of the Soviet Union Communist Party from 1961, the type of material usually widely available in bookshops and libraries. The reason was that in 1961 it was stated that the transition from socialism to communism would take place by 1981. 20 years later, however, this was nowhere near to happening. Vahtre acerbically notes that the subsequent 'new edition', published in 1985, found in hindsight that the party's statements from 1961 had 'in principle' turned out to be correct.

(eventually buying Yamaha models, see below), it would not have been enough to equip all schools. And spending valuable currency to cover the computer needs of only one small union republic of the Soviet Union was definitely not a top-priority endeavour. The hierarchy of supply was well-known by the local actors: space, military and industry first, civil uses later; Moscow first, peripheral regions later.

So it is safe to say that the possibility of a quick foreign acquisition was never seriously considered: yes, in principle Estonia could ask central authorities for thousands of school computers but the chance of actually obtaining them in a reasonable time-span was virtually zero. The IoC (1985.12.05) argued that Estonian schools would need 4,000–8,000 personal computers and it would be unrealistic to get them in 5 years time. But the availability of computers needed to coincide with the start of teaching, i.e. autumn 1986. As computing was deemed important by regional-level actors, the latter decided not to rely on the promises of central authorities and to take initiative instead.

There was yet another concern, that of national identity: to counter Sovietization, which aimed at erasing cultural differences in theory, but enforced linguistic and demographic Russification in practice (Kasekamp 2010: 158). Teaching and using Russian was increasingly supported by official doctrines, with Russian being proclaimed as Estonians' second mother tongue since the early 1970s (Zetterberg 2009: 549). Over the years there had also been a continuous influx of Russian-speaking workers, resulting in the percentage of ethnic Estonians dropping from 94% in 1945 to 62% in 1989 (table 3.2). The extrapolation of these trends created a justified fear among ethnic Estonians of becoming a minority in the country, which was also reflected in the reasoning of people in the education sector: *"We feared Russification, it was like a little allergy to Estonians. And I think Juku was made in order not to go – you see, if Russian computers come here too, it is over, then we'll only speak Russian"* (Jürisson interview).

Just in March 1985, Gorbachev had come to power and announced a need for reforms. This meant a gradual loosening of constraints: more initiative could be taken without fear of repression. But it also meant more room for colouring seemingly innocent and practical initiatives with identity concerns: *"I think in some sense Juku was used for political goals. Let's say, I don't know, nationalism and... These were such times when we had to show our level or being better or whatever, do something differently"* (Märtin interview). It was not to be simply a school computer for children—it was also to be a symbol of positive national differentiation. But in the USSR, where nationalism was a swear word in official rhetoric, considerations like this had to remain largely unspoken.

Table 3.2. Titular ethnic groups as a percentage of the population and the population in 1989 (Kasekamp 2010: 155, Eesti Statistikaamet, Latvijas Statistika, Lietuvos Statistikas Departmentas)

Soviet republic	1945	1959	1970	1989	Population in 1989 (in thousands)
Estonia	94	75	68	62	1,565.6
Latvia	80	62	57	52	2,666.6
Lithuania	78	79	79	80	3,647.8

In many ways the IoC was extremely well-positioned for the task: 1) compared with other groups it already had a few years of experience with developing microprocessor-based networked control systems for science and industry; 2) its PC was to be designed specifically for the task; 3) it was a large organization[18] with large numbers of staff—more than 600 people in the second half of the 1980s (Kutser 2000). Although the school PC was not to be a primary task for the IoC, it could nevertheless put much more manpower into the project than other organizations; 4) its good connections from prior contracts (including with the military) meant that the IoC was better informed about available components, had better access to them and could use more specialized elements in the design; 5) it had better resources for designing the PC at its disposal (e.g. a photoplotter used in printed circuit board

18 In the Soviet system the institutes were usually responsible for R&D and small-scale experimental production, while mass production was carried out by plants.

(PCB) design, means for diagnostics and set-up etc.); 6) the organization had dedicated lobbyists and visionaries (with good connections in Estonia and Moscow) who could tirelessly promote the project on many levels; 7) its prestige strongly contributed to positive expectations about its capability to sustain and develop the project further. In fact, its position was so greatly superior compared to the Tartu State University (Tartu) and the Computing Centre of the Ministry of Communications (Entel) that making the school computer seemed to the IoC like a perfectly natural course.

The IoC, or to be exact, its subdivision, the Special Construction Bureau of Computing Technology (SCBCT), produced a working prototype in 6 months solely through its own means (IoC 1985.14.11, 1985.21.11, see also photo 3.1). Its name, Juku, was derived from an Estonian proverb, 'what Juku will not learn, Juhan will not know',[19] with clear educational connotations. The IoC's vision meant a very down-to-earth attitude regarding the construction. If the project was to rely on official supply channels then one could not *"put very special stuff into this computer because we wouldn't have been able to produce it then. It would have been hard to guarantee* [that] *these* [components] *would be available for production"* (Tõnspoeg interview). So the problem looked a bit like a Matryoshka doll: 1) on the outside was a set of technical possibilities in the era as a whole; 2) a subset of which comprised technologies actually available for the Soviet Union; 3) a subset of which were available for the IoC; 4) a subset of which consisted of technologies that could presumably be acquired for mass production; 5) finally, inside of which was yet another subset of what would actually be allowed to be done with these technologies or what could be achieved within a given time-frame.

Alas, compared with Western countries the initial choice was not much to begin with. The historical reasons for this are lucidly summarized by Judy and Clough: *"The Soviet policy of copying Western hardware design, combined with international isolation and an industrial structure that retards domestic development, production,*

19 Juku is a nickname for Juhan used for small children.

and support, effectively doomed Soviet computerdom to an expanding lag behind the West during the 1980s" (1989: 321). The situation had become so poor (see table 3.3 for examples) that it was joked that a 32-bit microprocessor would arrive in the Soviet Union on a rocket.

Figure 3.1. Juku's prototype (Arvo Eller's private collection)

Not only was the technology outdated, it was also often of shoddy quality. The umbrella term 'technological culture' covers a wide variety of all the little things that could and did go wrong in the production process, resulting in wastefully and inefficiently produced, unstable and unreliable final products. Five ministries were producing computers and 23 more were producing materials and components for them (Goodman *et al.* 1988: 198) and any weakness in any link of the chain (e.g. impure production environment, impure materials, bad soldering) affected the outcome. The quality problems were tremendous: a chief engineer of the Lithuanian Sigma production union (see section 3.2.3), recalls that only about 10% of the enterprise's PCBs assembled with chips had no problems at all. Since discarding all the rest was out of the question, the factory needed a special unit of workers tasked with checking and repairing already assembled PCBs. The same engineer visited Western factories in the 1980s and noted the absence of such units, since the percentage of high-quality products exceeded 90% (Drąsutis interview). And Sigma was not even among the plants notorious in the USSR for their low quality products.

Table 3.3. Technological backwardness of Soviet computing (selected from Goodman *et al.* 1988, Adirim 1991; cross-checked from CPUShack, CPU World, Museum of Electronic Rarities)

Delay in microprocessor production (Goodman *et al.* 1988)			
Western chip		**Soviet/East German equivalent**	
Name	***Approximate year of appearance***	***Name***	***Approximate year of appearance***
Intel 8080	1973–1974	K580	1978–1979
Zilog Z80	1976–1977	U800 (GDR)	1980
Intel 8086/88	1978–1979	K1810	1983–1984
Intel 80286	1982–1983	No equivalent produced*	
Intel 80386	1985–1986	No equivalent produced	
Soviets' own estimations (Adirim 1991)			
Domain	***Year of statement***	***Level of declared backwardness***	
Microcircuits with logical circuit and external memory	1987	Backwardness of two generations (E. Velikhov)	
Mass production and use of computers	1989	12 years behind the Western nations (A. Aganbegyan)	
Service, guarantee and support of computer technology	1989	'Where the West was with the introduction of IBM/360' (roughly 25 years earlier) (A. Aganbegyan)	
Infrastructure of computer technology (production equipment, measuring and control machinery, special clean material)	1987	Backwardness of the order of 10 times (E. Velikhov)	

* According to the Museum of Electronic Rarities prototypes exist but mass production cannot be confirmed.

However, from the point of view of a school computer project the situation was even worse. Because from this pool of components it was the military that got the best ones that had passed extensive testing and were likely to work for an extended duration. And there was no hope that a school computer could somehow qualify as a super-important, high-end project eligible for components of assured quality.

So the components that could be used imposed various technical limitations on Juku's design. Microprocessors were unstable and often ceased to work. Tape recorders were used as external memory devices but they were slower than floppy disks and had poor mechanics, resulting in many errors when reading from or writing to the tape. TVs were used instead of computer monitors but they were less convenient to watch because of insufficient resolution and the fact that the sharpness of the display area was uneven. To ease up mass production the material of the case had to be switched from metal to plastics, which affected the cooling conditions of the power supply unit and the processor, which were quite susceptible to changes in temperature. The use of tapes instead of floppy disks meant that the functions of the operating system had to be somewhat reduced. Small memory capacity limited the scope of possible applications (e.g. some of them simply could not fit into the memory). And so on and so on. But what could be seen as a nuisance for the future user provided a creative challenge for developers: *"The bridles were hideous, but there was more playfulness to it, since you had to squeeze the maximum out of these resources"* (Haavel interview). For example, one programmer insisted that it was exactly the limited memory capacity that forced the workers to plan better and come up with more elegant software solutions (Paluoja interview).

Software-wise a decision to adopt CP/M operating system—a standard for 8-bit computers at the time—was made. This decision saved time and resources which would have otherwise had to be spent on programming the operating system and user applications from scratch. Instead, the project could take advantage of 'borrowing' already existing (mostly Western) software. The selection included various programming languages (assembler, BASIC, Pascal, Forth, C), word processing

(WordStar), database management (dBase II), spreadsheet calculation (Multiplan) etc. Some software, however, was created by the IoC itself, e.g. testing and diagnostics programs, graphics editor (GTR), games etc. (EKTA 1987, various interviews).

In parallel with prototype design, the IoC started negotiations with two potential producers, the radio engineering factory RET and Estron, a subsidiary electronics production enterprise of the Kuusalu kolkhoz (collective farm).[20] It was agreed that the three organizations would cooperate in preparing the necessary documentation for mass production by June 1986. As a large plant RET had valuable experience here that others lacked. Estron was also to be aided with appropriate technological preparations so that it would be able to produce a total of 500 PCs in 1986, including an experimental batch of 100 computers. Upon its ministry's central approval, RET would receive necessary components by 1987 and take over the production. At the same time the SCBCT would develop a new design of Juku to be produced by Estron (meeting protocols 1985.04.06, 1985.11.06, 1986.28.02, 1987.12.03).

Meanwhile the education sector was preparing for informatics teaching. The pace was frantic because the whole school computerization process resembled a campaign with characteristic Soviet traits: *"Soviet central education planners decided on an addition to the curriculum (computing literacy); they mandated it for the entire country with little advance discussion; they produced a single text and a single teacher-training program; and they required teachers to shift their teaching assignments on short notice"* (Kerr 1991: 227). In Estonia the task was to be implemented jointly by the Ministry of Education and the Republican Supplementary Training Institute of Teachers. The academics from universities acted as pedagogical

20 Since the profits from agricultural production were often quite low, kolkhozes tried to gain extra from subsidiary production, often remotely or not at all related to agriculture. Subsidiary production enterprises somewhat resembled private entrepreneurship, since they had freedom to choose their own projects, could potentially operate in a Soviet-wide market (which often had a very low competition because state enterprises were slow to respond to user demand) and keep the profits after appropriate tax payments to the state and the kolkhoz. In cases in which the enterprise also had a resourceful leader—and the man behind Estron's success, Vladimir Makarov, was celebrated for his organizational skills—the combination of creative freedom and high salaries attracted many talented engineers.

advisers and visionaries, while the IoC's representatives provided mainly technical consultation.

Teaching needed to start in 1986, but, hardware/software issues aside, there were other immediate problems: 400 teachers had to be trained and study materials prepared. This in turn required quick sub-solutions: searching for people able to train the teachers, organizing the courses, finding suitable candidates for informatics teaching (teachers of mathematics and/or physics were generally preferred for they were presumed to be more capable of the task), translating the study materials, creating additional material and so on.

Contemporary Soviet informatics education was strongly oriented towards programming and algorithms. In general, attention to other domains only started to emerge at the end of 1980s (Kerr 1991: 233–234). In Estonia the advice from academics resonated with the IoC's vision by stressing the need for user applications from the beginning. Therefore it was decided to deviate somewhat from the general thrust of informatics education (Jürisson interview). But this could only be achieved if computers were available on time since, by contrast with reading and writing algorithms, user applications were strictly a hands-on matter.

There was the additional problem of language: avoiding Russian computers, programming languages and materials as much as possible meant that a substitute of some kind needed to be found. Considering the scope of local resources and rapid development of computing it was quickly realized that translating all computer vocabulary into Estonian would be too demanding. The only remaining choice was to embrace the English language, to see it as an opportunity for communicating between different cultures, not as a threat to identity. *“We decided to let the operating system be, let it be in English: if we can create software in our own language and focus on that, it will do”* (Jürisson interview). But the programming languages also used English commands and a glance at Juku's case revealed mysterious words like 'power' and 'reset'.

The first group of teachers was indeed ready to start in 1986. More were trained over the following years. In parallel, a pilot group of teachers was formed who had early access to experimental Jukus and were tasked with disseminating knowledge on computing in schools further down the line (e.g. technical advice, local training sessions). So despite the hurry, by autumn 1986 every aspect of the grand plan seemed to be in place: informatics in schools with well-equipped computer classes was soon to be widely available. From the supply side there was a prototype, consent of two producers, a dedicated banner bearer and support from Bruno Saul, chairman of the Council of Ministers of Soviet Estonia (in essence a prime minister). The approval from central authorities and so the allocation of necessary components was still missing, however.

The issue was tackled on a broad front. Newspaper articles on Juku appeared. Its documentation was sent to factories in Leningrad, Kishinev (Moldavia), Zaporozhye, Riga (Latvia) and Narva (Estonia) to see if any of them would be interested in producing the computer (IoC's resolution from 1987.18.03). The issue was formally raised on Gorbachev's visit to Estonia (resolution from 1987). Since many young developers had been participating in the project, the computer was presented for and gained an award from the Estonian Leninist Communist Youth Union. It was also demonstrated at an all-union exhibition of National Economy Achievements (VDNH) in Moscow, where it was awarded a bronze medal. Exhibitions like this had multiple functions: informing others about available products, finding potential business partners, and receiving awards which increased the prestige of the project, making it harder to ignore and giving grounds to pay wage premiums to developers (important in the context of fixed wages). According to one interviewee from the Entel group, premium-paying considerations were the reason why the IoC influenced the chairman of the Estonian Popov society (a union of radio, electronics and communications specialists) to organize a school computer contest in May 1986 (Malsub interview). Tartu and Entel seized this chance to demonstrate their own computers too, but this intrusion did not affect Juku's first place.

Centrally the case for Juku proved to be difficult to make, however. At the time the Ministry of Radio Industry (Minradioprom) was already producing Agat (Агат) and preparing the production of Korvet (Корвет), whereas the Ministry of Electronics Industry (Minelektronprom) was doing the same with BK-0010 (БК-0010) and UKNTs (УКНЦ) respectively. All four were branded school computers and none of them were software-compatible with each other. At least on paper the competition seemed formidable. Agat was an Apple II clone with colour graphics, whereas BK-0010 had a new generation 16-bit processor. Korvet, while still an 8-bit computer, had 24 KB ROM while UKNTs could boast with two enhanced 16-bit processors, both working at higher clock speed than the one in BK-0010, and 192 KB RAM (BK-0010 user manual, Pavlov 1986.21.11, comparison of Juku and Korvet 1987.12.05, Frolov *et al.* 1988). Off-paper features of existing models were far less impressive, however: Agat's compatibility with Apple was actually quite limited and its colour monitor so bad that it was eventually declared hazardous by the Ministry of Health (Jürisson 1995). The expression 'fifth Agat' denoted its catastrophic breakdown rate—it was used to suggest that for every four Agats one needed the fifth one for spare parts (Krivtsov 1988, quoted in Goodman *et al.* 1988: 159). BK-0010 on the other hand had little RAM, no operating system and scarcely any software at all (see section 3.2.1).[21]

So Juku needed justification. Based on available information—because the computers themselves could not be obtained, of course—the project manager Rein Haavel compiled a comparison of Juku and Korvet, the only CP/M machine of the four mentioned above (1987.12.05). Although seemingly strictly focused on objective technical characteristics, the overall aim was to 'prove' Juku's superiority. This could be achieved in the following way: 1) choosing a sufficient number of categories to be able to point out as many single elements in favour of Juku as

21 As a monopoly producer of key components Minelektronprom had an upper hand in this competition, since it could hold back resources until its own products had matured. In fact, the ministry was suspected of doing exactly this in the case of both Korvet (Judy & Clough 1989: 277–278) and Agat (Eglājs interview).

possible; 2) interpreting technical characteristics creatively (for example, while both computers used the same processor, Korvet's clock speed was 2.5 MHz while Juku's was 2.0 MHz, but by adding a comment about how processors working at top speed reduces reliability—remember, no actual performance comparison was conducted—Juku could be argued to excel Korvet in that particular category); 3) choosing suitable overarching domains of comparison (central system hardware, construction, external devices, flexibility, efficiency, diagnostics and 'functional possibilities'); 4) calculating coefficients (no exact information is given but presumably for each domain the number of criteria in which Juku was shown to surpass Korvet was divided by the number in which the reverse was true). As a result it could now be shown that at best Korvet was equal or close to Juku in some domains (central system hardware, construction), but up to five times less capable in other ones (diagnostics, functionalities). This could be presented to authorities as proof of Korvet's inefficiency and limited capabilities.

Another strategy was to request components to produce school computers, but—in order to deal with the possible rejection—to also include a plea to consider the production of 'intellectual terminals'. (Jaaksoo interview). The actual difference between the two products was zero, of course. The State Committee for Computing and Informatics, an organization formed in 1986 to oversee the development of computing in the USSR (Goodman *et al.* 1988: 195–197), was not that easily convinced, however. Having examined Juku's production request twice it advised against it on the grounds that it did not correspond to technical requirements set for school computers (resolution from 1987.30.06). 'Intellectual terminals' were not found to surpass the ones already in production either.

The Committee's advice was to stick to officially approved Korvet and UKNTs computers. The trouble was that such recommendations tended to refer to a parallel reality. Because the basic assumption of Estonians in 1985 about the continuing shortage of school computers despite official promises had proved to be correct: the supply was nowhere near the desired quantities. Take the comment of Gennady

Iagodin, chair of the State Committee on National Education, about the overall situation in the USSR in 1988: *"We were supposed to receive more than 30,000 UK-NTs machines. We received 2,500. We should have received 34,000 'Korvets', but we actually received 3,000"* (cited in Kerr 1991: 238).

Meanwhile, the IoC continued to make minor improvements. Local networking, floppy disk drive and printer interfaces were developed. A mouse, which allegedly no other Soviet PC had at the time (Hanson 1987.22.04), was constructed. Additional software was adapted or created. An industrial version, Juss, was also designed which had a built-in floppy drive and colour TV display. It was used in several automatic control systems.

As 1986 turned into 1987 and 1987 into 1988, linkages between the IoC, Estron and RET started to weaken. Estron, initially motivated by technical interest and a touch of patriotism, produced an experimental batch of at least 100 Jukus (Tüksammel interview). The enterprise discovered then that the design was 'raw': contrary to expectations computers needed constant tinkering and set-up by engineers. Juku's design seemed too complicated and too demanding to Estron. Matters were not helped by somewhat tense relations between the IoC and Estron, since they had been competing for some contracts in the past. Some employees of Estron had previously worked for the IoC and left bearing a grudge. Also the project had a distinctive IoC flavour and Estron did not like to play a secondary role. To prepare the production in planned amounts additional investments would have been needed. But Estron was already doing quite well with other projects including work for the high-profile Space Research Institute of the USSR Academy of Sciences (Kala interview). So the enterprise decided to quit.

RET had been convinced to participate in the project by the regional Central Committee of the Party.[22] After analysing Juku's technical specifications and

22 The details of events concerning RET and Boris Tamm come from an interview with the manager of RET's Special Construction Bureau Toom Pungas. By the time I started the research Boris Tamm had died, so some of the information could not be verified from the primary source. I have

considering the time needed for implementation and production, it remained quite sceptical. RET people had found out, however, that at Moscow State University a laboratory led by Evgeny Velikhov, a renowned scientist and vice president of the Soviet Academy of Sciences, had worked out an experimental design called K-101 (a 16-bit computer with colour display). RET deemed this design promising and proposed to the IoC and Boris Tamm that Juku be redesigned on the basis of K-101, while implementing the production in parallel. The enterprise believed that it was capable of fulfilling its part in 1 year. Despite two visits to Velikhov's lab, Tamm did not like the idea and continued to support Juku as in its original form, allegedly hoping for quick success and an accompanying enhancement of the IoC's reputation. However, from the IoC's point of view there was a real danger that Juku would fall into a cycle of endless redesign while still failing to get produced (Jelle interview). Facing this trade-off it went for a short-term option. But RET was already manufacturing radio receivers as consumer goods and when its proposition was rejected it felt that it lacked a proper incentive to produce a computer that it perceived as outdated anyway. The production union dropped out.[23]

So the situation that had started out highly promising suddenly looked very frail indeed. Tamm, forced to change gear, contacted Baltijets, a factory in Narva. Similarly to RET, Baltijets was a large centrally controlled enterprise with over 5,000 workers producing various electronic devices (e.g. dosimeters). It belonged to the Ministry of Medium Machine Building, which oversaw the nuclear industry and was thereby also part of the military–industrial complex. As part of an 'elite' ministry it had also very good production facilities compared with RET (Jelle interview). Its interest in Juku production can be explained by the conjunction of various factors.

First, to alleviate scarcity, factories were required to manufacture some consumer

also failed to find any documentary evidence related to the episode.

23 However, Pungas went to prepare a business plan for the production of 20,000 16-bit computers a year in cooperation with Taiwan. Having received preliminary approval from authorities, he also held negotiations with a potential partner from the Taiwan side. However, he was eventually forced to resign and with the disintegration of the Soviet Union the project failed to be realized (Pungas interview).

goods in addition to their main output. A plant could show initiative in this area so long as it somehow corresponded to general central guidelines. However, such a production was of secondary importance to the factories. Therefore, following the principle of least effort, often the most convenient way to meet this requirement was to combine the plant's current stock and production infrastructure in a manner that would not require any major preparations. This often led to curious results: for example, the Töögelmann factory in Tallinn was mainly producing semiconductor devices. Its consumer goods, however, included decorative belts, metal chains for toilet flush tanks, generators for electric fences, battery wire kits etc. (Jõgi 2003: 43–44). At that time Baltijets was interested in the production of consumer goods of some kind, although computers were not a simple and convenient product. However, the potential use as an 'intellectual terminal' to aid the factory's overall production processes might have been a decisive factor in offsetting these considerations (Haavel, Tõnspoeg interviews).

Third, the overall political situation must be taken into account: beginning from environmental protests in 1986 in Latvia (Kasekamp 2010: 161), the opposition to Soviet authorities had gradually become more vocal in all Baltic states, while the central authorities were less and less willing to intervene militarily. A telling sign is Gorbachev's replacement of Karl Vaino, the first secretary of the Estonian Communist Party, who had requested that tanks be brought onto the streets to suppress the demonstrations (*ibid.*: 163). Over a few years then there was a gradual move from demands for increased autonomy towards independence. In these conditions, military orders from Moscow started to diminish—no new orders were placed and existing ones were gradually curtailed (Pungas interview)—which in turn might explain the increased willingness of large factories to undertake new projects.

A high-level meeting between Tamm, Bruno Saul and the minister of Medium Machine Building followed. Relabelled as 'intellectual terminals for real-time system E5104', the production of these machines (with a possible use for school computing) was agreed upon: 200 in 1988 and 1,000 in 1989 (meeting protocol from 1987). This agreement was followed by Saul's letter to the deputy chairman of the USSR Council of Ministers (1988.06). The letter stressed the general scarcity of computers: whereas Estonia's need for microcomputers was claimed to be 5,000, the number actually allocated in 1988 was 200.

This time the project was approved. It even turned out to be possible to equip Jukus with Bulgarian floppy disk drives, ten floppy disks and Epson printers (Jürisson interview, Levi 1990.09.01). Being mainly research-oriented, the IoC had little knowledge of how to prepare technical documentation for mass production. Thus it hired a person from RET who started working on this task in August 1988. In effect, this meant adapting the prototype both to all-union standards (GOST) and to Baltijets's manufacturing equipment. The drawings produced were then used by the factory to set-up the production to be able to adhere to the details specified in the documentation with required precision. If certain technical requirements could not be fulfilled, additional minor modifications needed to be made in the original documentation. SCBCT also agreed to prepare automated set-up and testing systems (Jelle, Haavel interviews). All this took time, so it was 1989 by the time Baltijets was eventually ready. 4 years had passed since the prototype design and by this point Juku was outdated even by Soviet standards, not to mention Western ones.

But the education sector had waited long enough. To some extent the lack of computers had been alleviated by computer classes at some secondary schools, learning centres and universities. These were serving many schools at once and allowed pupils to get at least a glimpse of hands-on computing, albeit on very different models (depending on what one or another organization had managed to acquire). Some pilot group schools had also received experimental Jukus. But most informatics teaching was theoretical: students wrote programs on paper and these were then assessed by teachers. Despite the initial wide vision, the actual teaching practice concentrated heavily on programming (Kivimäe, Ruut, Tõnso interviews) and the shift in focus to user applications was only gradual. It is sensible to assume that both the general lack of computers and diversity of those that were available had crucial roles here. So if there was a choice between getting by with a handful of computers in the hope that many better ones would be available at some point in the future (and the issue with such promises has to be remembered here) and using large numbers of outdated machines in the short term, the latter option was preferred.

2,500 Jukus were produced for schools (the total quantity produced is unknown), of which 2,000 were produced between 1989 and 1991 (Jürisson 1995). Schools requested computers from the Ministry of Education, who then made selections from among the applicants. Usually a set of ten computers was allocated to the successful applicant. Not all computers made it to the schools, however, since the supply was filtered by regional education departments. Hence some computers could be officially listed as having been sent to schools when they were in fact kept by local officials (Ruut interview). However, there is no exact information about the extent of this practice.[24]

There were serious hardware issues. 300 Jukus stopped working within the first year and could not be repaired owing to the lack of spare parts. At least 50% of the computers needed repair every year (Jürisson 1995). Bulgarian floppy disk drives often broke down and disks themselves were faulty. The printers on the other hand lasted for years and were even sold to other organizations after the Jukus themselves ceased to be used (Jürisson interview). Despite the initial reliability requirement the actual user experience of Juku was riddled with difficulties.

The problem was further accentuated by the repair process. Although formally Jukus had a warranty and enterprises other than Baltijets offered repair services, in practice the computers often had to be sent to another town. A long wait, possibly lasting up to several months, then ensued. At least partly this situation was caused by the lack of spare parts. To sidestep this problem, self-repair was a frequent solution.

24 In general theft and fraud were common in Soviet system, however. An interviewee from Tallinn Pedagogical Institute brought an anecdotal example of his own. When two shipments were mixed up—a university received a thermal printer meant for another organization, whereas the other organization received a floppy disk drive meant for the university—the other organization refused to switch them back and had to be bribed to be persuaded to do so (Tõnso interview).

Sometimes various parts of different non-working machines could be combined into one properly functioning PC. Alas, not every problem could be solved in such a manner and not every school had tinkerers with enough skill, in which case delays were unavoidable.

It is difficult to say from where the problem with software originated. Whether it had something to do with Juku being largely a self-financed side-project for SCBCT, Baltijets's lack of experience with computer production, communication problems between the two (the SCBCT group was mostly Estonian while Baltijets's workers were Russian) (Haavel, Jelle interviews), or user inexperience—software issues were numerous. Operating system could be read into the memory only from drive A. If this drive failed (and as noted above, the Bulgarian drives often did), drive B was also useless. The original WordStar software package included other programs (e.g. MailMerge) but only WordStar itself was adapted for Juku. Therefore some commands which also needed other, non-adapted programs, crashed the computer. So did using arrow keys in WordStar.[25] Since the @ key was replaced with one of the vowels from Estonian alphabet, users could not insert any commands in dBase II beginning with @. Character code tables were badly synchronized: occasionally a keystroke, displayed symbol and print-out might have differed from each other. There were two versions of BASIC language, one in ROM and one on floppy disk: the first had commands for graphics but the result could not be saved while reverse was the case for the other version (e-mail discussion between Tõnso & Toom 2000, Tõnso interview). In yet another twist of irony it appeared then that Juku was best suited not for user applications but for programming: the limitations of hardware and software, so troubling for lay users, created challenging obstacles for software writers.

The Republican Supplementary Training Institute of Teachers received feedback about such difficulties, but never sent it on to Baltijets. It would simply not have had

25 There was a somewhat cruel joke on the matter: Juku does not have real WordStar, only a WordStar emulator. But that is okay, because Juku itself is not a real school computer, only an emulator of one.

any effect. The factory had secured certain resources for a certain period of time. Making any substantial changes would have meant running through the bureaucracy gauntlet again—and again with no guaranteed success. Likely there was also a lack of incentive to do so because the users did not have much choice in the first place. Also, the overall relations between the central authorities and Estonia only grew worse, to the extent that finally the supply chain was completely cut off, severely limiting any possibility for modifications even if the plant had wished to make them. The net result was a total absence of influence of user experience on production. After initial negotiations and choices had been made, the move from mass production to use was unidirectional.

In the meantime, political struggles within Estonia and between Estonia and the Soviet Union had culminated with Estonia's declaration of independence on the August 20th, 1991. A rapid shift from a planned economy to a market economy and from a totalitarian regime to democracy followed, involving major changes in virtually every aspect of life. Market liberalization considerably diminished limits to the flow of goods. With the currency reform in 1992 it now became possible to buy as many Western computers as desired for steadily decreasing prices—but for some time these machines were far more expensive than Soviet electronics so no immediate, large-scale replacement could be undertaken. Nevertheless the attitude of the specialists in the education sector started turning against Juku. Compared with Western computers Jukus had many features perceived as obvious disadvantages: 1) low reliability; 2) low speed—users could not dream of using complex graphics or multimedia packages; 3) lack of compatibility—MS-DOS and then Windows had become new standards for 16-bit computers. Similar to CP/M compatibility for 8-bit computers, IBM compatibility now provided access to a vast collection of ready-to-use (educational) software; 4) copyright issues—prior Soviet 'adaptation' of software had actually been a breach of copyright in Western terms. The continued use of such programs in schools would have created legal problems in the future (Jürisson interview, Jürisson 1995).

But halting production was not easy. Although in 1992 a group of experts consulting the Ministry of Education proposed to buy IBM PC-compatible computers (Jürisson 1995), 500 more Jukus were ordered from Baltijets instead. There was speculation (Tõnso interview) that the main reason for this neglect was related to national security. With the sudden disappearance of Eastern orders, large factories were struggling: stocks were plenty but contracts few. Producing Jukus would have kept Baltijets busy at least for a little while, and so delayed the discontent of thousands of employees. At a time when relations with Russia were very tense and Russian military forces had still not withdrawn from Estonia, avoiding conflict in an area adjacent to Russia and populated mostly by ethnic Russians would have been crucial. This explanation is supported by the fact that movements called International Fronts, who had been opposing reforms and the move towards independence in all Baltic states, had been strongly supported by members of the military–industrial complex. It is also true that the director of Baltijets was one of the key figures in a 1993 crisis in which a group of high-ranked Russians attempted to initiate a referendum for establishing the national-territorial autonomy of Narva (Elling 2001). However, this explanation has been disputed on the grounds that, for such a large factory, the production of 500 computers would not have taken much time (Kala interview).

SCBCT realized that the delay had been too long and that IBM-compatible computers had become a new standard. That is why it lost interest in Baltijets as soon as it had fulfilled its part of the deal. But new opportunities had opened up in the midst of reforms. In 1989 SCBCT was allowed to establish a joint venture, EKTACO, with Finnish partners. One of the first tasks was to provide Finnish schools with computers based on Taiwanese components, but assembled and tested in Estonia. This inspired SCBCT to develop a PC-version of Juku based on the Intel 80286 processor. Using connections from the school contract, the components would have been imported from Taiwan while mechanical works, assembly, testing and marketing was to be arranged in the USSR. The prototype was built in 1990 (Jelle interview) but the disintegration of the USSR disrupted the supply chain of local factories, and potential working relations with Russian factories, to the extent that cooperation and mass production became impossible. The project was soon discontinued.

Despite all the delays, the controversies over the meaningfulness of the project and the problems with the end product, it did create a general availability of computers in schools at a time when PCs were considered a luxury item. The extent of the effort is best understood by comparison. Based on the available data, table 3.4 presents a comparison of school computerization in Estonia in 1992 and Lithuania in 1994.

Table 3.4. School computerization in Estonia and Lithuania (Ališauskas 1995, Jürisson 1995, Eesti Statistikaamet)

	Estonia (1992)	**Lithuania (1994)**
Jukus	2,500	
Commodores		~500
UKNTs/BK-0010Š	504	1,300
IBM-compatibles	~60	>1,000
Various other computers	262	533
Total	**3,326**	**3,333**
Number of students in schools	47,200 (1995)	60,113
Students per computer	**14.2**	**18.0**
Students per computer (excluding data for vocational schools)	**13.0**	**18.0**

It is notable that the Lithuanian data only includes schools where informatics was mandatory and excludes vocational schools, whereas Estonian data is an aggregate for all schools (of which 36,800 were in upper-secondary and 10,400 were in vocational schools). It can be seen that, even so, the ratio of students to computers was lower in Estonian schools. If the students in vocational schools are excluded along with data for UKNTs and BK-0010Š computers (as these were mostly used in vocational or Russian-speaking schools) the ratio becomes even smaller. Moreover, the table does not show that IBM-compatible PCs started to be supported to schools in greater numbers from about 1991–1992 in Lithuania (Ališauskas interview). By

1995 Estonian schools had also obtained approximately 900 IBM-compatible computers (Jürisson 1995), roughly equalling the number in Lithuania.

Therefore it can be claimed that, although late, Jukus did eventually enable Estonia to gain a head start in mass school computerization, provided early access and a more standardized study environment (the table shows that in 1992 Jukus made up roughly 75% of computers in schools). The number of students who had got their first computing experience with Juku was in the tens of thousands—much more, much earlier and more frequently than would have been possible otherwise. Even with the influx of Western computers, Jukus could be shifted to secondary and primary school level and then gradually phased out, a process largely completed by the second half of the 1990s. But it was an individual who was mostly critical of the endeavour who perhaps managed to capture best the additional dimension of Juku project: *"If the goal was not so much that children could compute but to show that Estonians can get something done, then it was* [a] *right* [move]*"* (Kala interview).

3.1.2 Tartu

This story starts with the envy of Anne Villems, working in a programming department of the Faculty of Mathematics of the Tartu State University. She envied her husband, a molecular biologist, who had just managed to obtain foreign currency to buy lab equipment. Villems decided that her department needed good Western computers and sought to use the approaching 350th anniversary of the university in 1982 as a pretext. Although the actual chances of getting the currency were slim, university authorities generally did not block such initiatives and were willing to sign the documents—provided that they had already been prepared. Villems chose to go for Apple II, a legendary PC introduced in USA in 1977, which united user-friendliness with flexibility of use (Ceruzzi 2003: 266). There were now two important questions: will the Motorola 6502 processor be embargoed? Will the application be approved?

The processor was not embargoed and, to much dismay, the application was approved indeed. Thus the university received four brand new Apple II computers to enable a completely novel approach to programming. Instead of writing the program on paper, submitting it to the university's computing centre, waiting for the output for a week and starting all over again if the code had been buggy, here was a small, powerful and elegant device allowing direct interaction. *"It was a real cultural shock for a Soviet citizen to see a machine like this"* was how one of the members of Tartu working group described the feeling (Toom interview).

At roughly the same time, an engineer Leo-Henn Humal had advised the university's vice rector for science to establish a research unit related to microprocessor technologies. Although no immediate action followed, a decisive push in that direction came from an all-union directive issued around 1981. This document demanded that universities should start developing microprocessor technologies. The conjunction of central command and local interest resulted in the establishment of a microprocessor sector as part of the Laboratory of Electroluminescence and Semiconductors (LES) in spring 1982.

At first it was not too clear what was to be developed. Gradually the idea of constructing a PC emerged (Humal interview). This project seemed both technically interesting and potentially useful—Humal imagined that it could be used for teaching in the university (mainly for programming) and for automating scientific experiments (Vajakas 1985.15.10). Not everyone shared this vision, however: Humal refers to 'authoritative figures', including one from the IoC, who claimed that such an effort would be unnecessary because industrially produced PCs would be available in great numbers soon. Seeing the current difficulties with obtaining Soviet (not to mention foreign) PCs, Humal himself was less optimistic.

Newly arrived Apples provided an immediate inspiration.[26] The trouble was that the USSR was not producing MOS 6502 microprocessor copies. In fact, the only

26 A telling indication comes from the person involved with the production of the Tartu PC in Palivere (see below), who noted during the interview that Tartu resembled an Apple computer without knowing at that time that it had actually been inspired by one (Vilgats interview).

reasonably up-to-date processor one could hope to acquire was an Intel 8080 copy. Since Humal wanted to avoid getting tied up with official (read: slow, rigid and uncertain) supply channels, he aimed to go for a cheap, simple but robust design: an expandable one-board-computer made from components that with a bit of luck even a hobbyist could buy from a radio electronics shop.

These goals were not completely compatible, however: in order to be made from accessible components, the design of the PCB had to become more complicated. It also meant that the basic design could not include much memory and could have no printer, floppy disk drive or monitor (Vajakas 1985.15.10). In fact, the first version used eight 0.5 KB ROM chips. A tape recorder was used as the external memory device. In the hope that respective components would be available in the future, floppy drive and printer interfaces were developed when the head of LES, Arved-Aleksandr Tammik, managed to get some Soviet electronic typewriters. All but German (Robotron) floppy drives and printers were discarded. Black-and-white TVs were used as displays. An experimental interface for colour TV was also built, but the picture quality turned out to be so low that no further attempts were made (Toom interview).

The basis of the design had become clear by the beginning of 1984. The prototype was working by autumn that year (LES's summary from 1984, Tenner 1985.26.12). Whereas Apple computers had been used for designing the prototype, the latter in turn could now be used to design a second, slightly enhanced version. The use of better, 2 KB EPROM chips allowed integration of the BASIC language into the ROM. When the team managed to get a copy of CP/M from one of the employees of the IoC (Toom interview) and make it run on Tartu, a whole world of software applications opened up. Self-developed applications included a program for PCB tracing (used for designing the second version and controllers for peripherals), text editor TE for entering and editing programs, CP/M's adaptation to using the hard drives of EC mainframes (7/29 MB versions seemed like a vast universe compared with 64 KB RAM), local area network software so that all the students could share one hard drive in a computer class, and some games. By the end of 1985, eight computers were in operation (Tenner 1985.26.12).

In parallel with development, Tammik had used his connections to initiate discussions about possible serial production. In 1982 or 1983 Estron expressed interest in cooperation with Tartu. Estron showed Humal some Western examples (likely Sinclair ZX Spectrums) and claimed that such simple computers would sell well. Tartu seemed interesting enough for Estron to design a power supply unit and a preliminary version of the case. The project was eventually abandoned, possibly because Estron might have found the case too difficult to produce after all (Toom interview). A military factory in Tartu known for its production of black boxes for airplanes (Högselius 2005: 98) was also contacted, but it turned out that the plant was unable to fit the project into their production plan (Humal interview). Finally, there were some preliminary talks with Baltijets, but the enterprise, referring to very busy schedule of its construction department, was only willing to consider production if it was provided with full construction documentation. The irony is that Baltijets had an actual department for preparing such drawings (around 100 people according to Humal's estimate), whereas fewer than ten people in total were working on Tartu. Of those only one had any knowledge about preparing technical documentation. Thus LES simply could not satisfy Baltijets's requirements and therefore no cooperation followed.

The campaign for informatics teaching in schools led Humal to think about expanding Tartu's domain of application. A newspaper article from 1985 mentions that the university had turned to 'respective authorities' to arrange a meeting between all organizations that had been developing microcomputers (Vajakas 1985.15.10). As noted above, by that time the IoC had already taken up the idea itself and started intense work on the prototype of Juku. Humal and Malsub (leader of the Entel group) both witnessed Juku's demonstration in November. Although Tartu was still presented as a school computer it had become clear that the IoC had taken a decisive lead. That is why participation in a school computer contest (Tartu gained the 2nd place) was seen as a possible opportunity for a demonstration rather than as a serious competition between equal participants. In fact, Humal went as far as to state that *"if Juku had been developed earlier we would have gladly used it. We would not have gone through all this trouble."*

But now the computer existed. And despite being locked out of the local school computer competition, other possible uses could still be found. Again it was likely through Tammik's connections that contact with Schetmash (Счётмаш) factory in Kursk, Russia, was established either in 1985 or 1986 (Humal interview). The factory had been producing the Iskra (Искра) line of computers and was searching for a suitable prototype to be manufactured as a consumer good. Tartu PC seemed promising. The plant agreed to construct the power supply unit and case by itself, prepare a full technical documentation (LES could only offer some drawings of the electronics). The Tartu group was to help with the preparation of production. In return the university would get 200 Tartus which it had otherwise planned to produce itself.

Familiar time-consuming activities followed—preparing the documentation, preparing and testing the machinery, making necessary corrections, requesting and waiting for components, learning to test the products properly etc. Several setbacks occurred: in one episode Humal and another member of the working group had to travel to Kursk in 1987 to find out why none of the computers that had been produced so far worked. Soon it appeared that the factory had simply not tested all the functions of some chips (which tended to happen occasionally when new chips became available). Alas, Tartu's design happened to employ one of these functions. Although in this case the problem could be solved by simple replacement, the accumulation of factors like this contributed to the delay of mass production. Production finally started in 1989 and lasted at least until 1991. The name of the model was Iskra 1080 'Tartu' (Искра 1080 'Тарту'). On one hand it is ironic that the number of Iskra 1080s produced very likely exceeded the number of Jukus. On the other hand, these computers were centrally allocated meaning that only a fraction of the total would be actually sold in Soviet Estonia.

Similar to the story of Juku, the university's hopes for quick provision of the necessary amount of computing equipment had been in vain. It had managed to acquire a classroom set of Yamaha computers in 1986, but only on the condition of having to provide time for school informatics lessons (Villems interview). Jukus, a possible alternative choice, were not coming either. Likely for these reasons, Humal started looking for alternative options. In 1988 he established a contact with a subsidiary production enterprise of the Palivere Factory of Construction Materials.

The factory itself was part of a cooperative called Estonian Kolkhoz Construction. Although the cooperative was only partly tied to official supply channels and related obligations, subsidiary electronics production promised a number of already familiar advantages—higher profits,[27] higher salaries, a greater degree of creative freedom and so on. There were more prosaic reasons too: the wives of men working for the construction materials factory were generally not in paid work. They could be used as a cheap labour force for electronics assembly (fully automated production would have been far too expensive) (Enok interview). Coincidentally one of the buildings on the factory premises had just been vacated, and another subsidiary electronics producer for Lääne Kalur kolkhoz was doing so well at the time that it was willing to outsource some of the work.

Under the leadership of Leo Enok, Palivere's electronics production started in 1977. Enok had good connections, especially with the Academy of Sciences, enabling him to initiate contracts with many research institutes (in Moscow, Leningrad and also the IoC in Tallinn) that were generally searching for someone who would produce their prototypes. There were also some contracts with factories, of which RET became gradually more and more prominent. Palivere's quality was good enough for its

27 According to estimates by the head of Palivere's electronics production, towards the end of Soviet times 350 people in the construction materials factory had a turnover of 2,000,000 roubles, while in electronics production there were 72 people and a turnover of 3,000,000 roubles (Enok interview). Although the prices were fixed, this illustrates the value created from subsidiary activities.

contractors and therefore it expanded quickly. In so doing it needed to attract additional workers, and was helped in large part by the fact that it could immediately offer an apartment to new recruits.[28]

Despite having good working relations with the IoC, Juku was never offered to Palivere: it was just too small for mass production, whether in terms of obtaining supplies or the capabilities of manufacturing equipment. The offer from Tartu had less grandeur: production of 200 computers seemed an ambitious yet achievable goal. In addition to economic considerations, patriotic ones—pride in producing an Estonian-designed school computer—were in play too. Still it must be mentioned that the 'school computer' label could be (and was) used to gain leverage in resource acquisition and also that eventually only one computer class was actually set up (Tingas interview).

It is notable but characteristic of the time that Palivere's consent was preceded by no market research: it was believed that the product could be sold in any case. Consumers in the Soviet Union were (mostly correctly) expected not to know anything about computers at the time and therefore not to have any particular expectations. If any did exist, the buyer was simply expected to adjust them. The lay consumer was mostly facing a zero–one choice—get the available computer or get none at all. Choice between competing products was usually out of the question, especially for novelty items like PCs.

The downside of the flexibility of subsidiary production was exclusion from official supply channels. Therefore the success of the enterprise depended a great deal on the ingenuity of its suppliers and its network of connections. Palivere employed the latter skilfully, conducting a series of barter deals with other members of the construction cooperative and customers of its electronics production. Some of the following deals were typical. First a certain quantity of the factory's off-plan produce was exchanged for a few truckloads of particle boards manufactured by another member of the

28 Something very mundane but very important at the same time—usually one had to wait for years to get one.

cooperative. These materials were sent to Zelenograd, where a previously agreed upon number of processors and memory chips were sent back in return. Similar deals helped Palivere to acquire TV displays elsewhere. RET promised to supply 10 kg of copper wire for transformers in exchange for an agreement according to which Palivere would work extra shifts on one weekend to supply RET with some required products so the plant could fulfil its plan on time. Informal negotiations through personal contacts resulted in 200 tape recorders—half of that year's planned retail sales in Soviet Estonia—being redirected to Palivere (officially justified on the grounds of the shortage of school computers). Materials for PCBs came from Leningrad, but boards themselves were made in Lääne Kalur; polyvinyl chloride for keyboards came from RET, metal for the case from Teras factory (Enok interview) and so on.

As always, there were various factors affecting the reliability of the computer. PCB quality was a general problem, itself dependent on production technology and the materials used. Three main problems affected PCBs: 1) conductive tracks were severed (in particular, the insufficient metallization of PCB holes created a lot of problems which could be only temporarily solved by manual re-soldering); 2) conductive tracks were inappropriately connected to each other, leading to short circuits; and 3) tracks came loose from the board (Rätsep, Vilgats interviews). Additionally, production technology limited the possible size of PCBs, making it difficult to manufacture large ones of decent quality. Mistakes could also happen in the process of manual hole drilling. A Palivere technician recalls that in this case the quality issues of Lääne Kalur's PCBs were especially serious, leading to the instability of the computer (Vilgats interview).

Manual assembly could create further problems. Differences in workers' skills and degrees of sloppiness were reflected in the final product. Recurring problems were either poor soldering or too high a temperature of the soldering torch, which damaged microcircuitry. After assembly each computer was visually inspected by magnifying glass. An experienced inspector could spot possible faults by the reflection of the area around soldering. But such workers did not have any substantial knowledge about electronics. The final check was conducted by a qualified technician, whose task was to find the faults missed in visual control, fix them manually, run additional tests and ensure that the final product would work properly (Enok interview).

With Humal's help, preparations for production started in 1988. Some difficulties with supplies delayed the start of production until 1989 (annual report of Palivere factory, 1989). Although the first versions were produced with tape recorders, later machines were equipped with Bulgarian floppy disk drives. If so desired by the buyer, the set would also include a printer which was capable of printing special characters of Estonian alphabet.

However, what had seemed like a commercially safe bet did not turn out to be so safe after all. To begin with, such small-scale production proved to be relatively expensive (each computer cost the equivalent several months' wages of an average worker). Much more serious macro-problems were also emerging at the time—falling output, rising shortages, wage inflation, high overall inflation, a large fiscal deficit and excessive foreign debt—the disintegration of the USSR had it all (Åslund 2002: 50). This was happening hard, fast and for most of the people, both suppliers and consumers, unexpectedly. A telling tale comes from Anne Villems who, not being sure whether there would be any food in stores in the following year, learned how to grow potatoes in 1989—later finding out that every other member of the programming department had been doing the same (Villems interview). So it is reasonable to assume that most potential individual consumers did not have much time to worry about whether investing in a PC now or a few years later would be the more rational choice. Those who could afford to do so, however, were already getting themselves Western computers—if possible from abroad where the selling prices were lower than in the USSR. A warranty check for a computer number 47 exists, and it is likely that fewer than 100 were made (Tingas interview). The production lasted until 1991 at latest and most were eventually bought by various organizations rather than going to schools (Vilgats interview).

Figure 3.2. Heido Vilgats setting up the Tartu computer produced in Palivere (Heido Vilgats's private collection)

It would not be wrong to conclude that Palivere's attempt was simply caught between the cogs and wheels of the overall societal transition. The Eastern market disappeared virtually overnight. RET, who had bought about 80% of Palivere's output, got into considerable difficulties and collapsed in 1993. After that it was all about survival: browsing through existing inventory and trying to come up with products—like doorbells—that someone would be willing to buy. But the story of Palivere's survival through the establishment of early contacts with Finnish and Swedish enterprises, while interesting in itself, is not related to the story of the Tartu.

3.1.3 Entel

The story of Entel (an abbreviation of 'Estonian Intel') goes back to 1971, when the republican Ministry of Communications established a Computing Centre (CCMC) in Tallinn. The opportunity to work with computers attracted Jüri Malsub, who organized his transfer to the CCMC and began assembling a team of specialists. The main task was to perform required computing tasks for the ministry, but in order to earn a 40% wage premium additional contracts were sought.

The CCMC's ability to seek external contracts was hampered, however, by two factors. The first concerned Malsub's family tree: his close relatives had been fighting for Germany in World War II and later as 'forest brothers' against Soviet rule. This made him unreliable in the eyes of Soviet authorities, denying him advancement to top positions on the career ladder, foreign travel and access to military institutes and plants. The second was related to the nature of the planned economy, wherein the division of labour and prioritization regarding the allocation of resources was determined from above. The CCMC was not supposed to be a centre of innovation—as its workers well knew—and thus it was unreasonable to hope that the central plan would cover the desired 'secondary' activities. In the context of scarcity, why should a small, unknown organization with an explicitly defined purpose of merely providing computing services be allocated extensive resources when large research institutes and factories were waiting in line?

But small-scale projects could still be both professionally challenging and profitable. The supply issue was alleviated when the CCMC established contacts with an Estonian chief engineer working for the well-connected Yerevan Scientific Research Institute of Mathematical Machines in Armenia. The CCMC developed a solution for setting up a new version of a computer called Nairi that the institute had just designed but had not managed to finalise and polish. Thereby the CCMC became the partner of the institute and started to implement the Nairi project all over the USSR. In so doing the CCMC gained additional connections, technical know-how and experience of obtaining various resources.

This gradually built-up network became useful around 1980 when the CCMC's interest turned to microcomputers. The first of the reasons was professional: *"Now a large part of a computer had been put onto a microchip and there was simply an interest to study and experiment with it"* (Rätsep interview). The second reason was more practical: problems with the telephone network in Tallinn, where some lines were overloaded but others were used well under full capacity. Microcomputers could be used to gather statistics about the situation.

The story of how the CCMC acquired the first Soviet Intel 8080 analogues perfectly illustrates the continuous struggles with everyday Soviet realities.[29] For a relatively insignificant organization like the CCMC the troubles started with finding out what was out there in the first place: information about products, some of them classified, was (more readily) available to well-connected, high-level organizations (e.g. the Institute of Cybernetics). Especially when it came to newer (often classified) products, low-level organizations had to rely on their own informal networks. The CCMC came upon the rumour that the production of 8-bit microprocessors had started in Kiev, Ukraine. Knowing was not enough, however: authority to act was also needed. The CCMC had enough experience to know that request letters sent to large factories to sell their products that were not backed up by informal authoritative support usually went unanswered. This is why Malsub contacted his schoolmate from the Pöögelmann factory, who spoke to the plant's production manager. The production manager got the phone number of the director of Kiev factory. It is important to note that, by contrast with the CCMC, the Pöögelmann factory and the one in Kiev both belonged to Minelektronprom. Thus it was hoped that the informal intra-ministerial contact along with the request letter authorized by the director of the CCMC would prove sufficient.

Next the CCMC sent a representative to Kiev. However, it turned out that because the factory was classified, the public information bureau refused to reveal either its

29 The entire story comes from Ülo Rätsep, who acted as the CCMC's representative on this occasion.

location or its general phone number. The real trouble was that the director's phone number was one for the internal phone. The representative solved the problem by talking to a nearby taxi driver who started driving around the city asking other taxi drivers wherever they were encountered. Finally one who knew the location was found.

Once in the factory lobby the representative was glad to discover a set of phone booths. The bad part was that none of them seemed to work. Not knowing what to do, the representative then observed others who stepped in the booths and started talking. Closer inspection revealed a message stating that the phones only worked when the booth was fully closed (to avoid eavesdropping). Having slammed the door shut, the representative managed to contact the director, improvised greetings from Pöögelmann's production manager whom he had never met in person, and finally received a signature for his letter. The secretary—accompanying the representative everywhere because it was forbidden to move alone in such factories without a special permit—took him to a warehouse where he was promptly given all currently available microprocessors. This amounted to four pieces. The trip was considered highly successful. Save for the fortunate outcome, trips and arrangements like this were entirely ordinary.

To build a prototype, similar problems needed to be solved at every step. Initially only trial batches of 8080 microprocessors were produced in the Soviet Union which had no clock generators or system controllers to go with them. Substitutes for those had to be designed from other elements. There was no information about how to program the microprocessor and it was forbidden to bring in foreign literature on the matter. But unofficial Soviet typewriter copy of the Intel 8080 manual was full of errors and so very unreliable. Through a person who had studied in Budapest an original manual was obtained from Hungary. Memory chips were difficult to get, had a small capacity and were slow which, in turn, limited other possibilities, e.g. the number of symbols that could be shown on the monitor. Monitors themselves—large, expensive, hard to obtain and unreliable—had to be substituted with TVs, with their respective shortcomings. There was the familiar lack of floppy disk drives and disks.[30] PCBs from Lääne Kalur were unstable. Acquiring keys for making keyboards was also an endemic problem which the CCMC could not overcome. The organization bypassed this issue by devising a sensor keyboard: symbols were depicted on one side of the PCB. Each 'key' had a metal contact in the centre leading to a conductive track on the other side. When finger was placed on the contact, change in electrical impedance signalled a keystroke (Rätsep interview).

Technical considerations like this surely affected the design of the computer. But also important was the purpose to which Entel was to be put—solving various technological problems. It was envisioned that the hardware could be reconfigured depending on the specific problem to be addressed. Hence the developers decided to avoid a standard one-board-computer solution (one motherboard with all basic elements of a computer and connections for peripheral devices). Instead a module-based approach was taken, wherein the power supply unit and processor module were based in a frame with 10–15 vacant slots. These could be connected to various other modules of choice, e.g. memory, monitor and keyboard, but also to a hard drive of an EC mainframe, punched tape reader, video recorder or a photoplotter, to mention more exotic examples. This solution was also convenient for the division of labour, since every group member could always be working on something.

Although this design was flexible it had its fair share of downsides. First of all, each module had 44 contacts. With the addition of modules the chances that one of the contacts would be faulty increased. Compared with one-board computers, it was more expensive and labour-intensive to build. One-board computers were also better exposed to air, whereas Entel's components were placed tightly next to each other (fans were not used at the time). Owing to the high degree of sensitivity to

30 Quality floppy disks were a good bartering item: when Malsub managed to visit Japan in 1987—legal restrictions had already been relaxed—he used all the money he was allowed to convert (the amount was officially fixed) to buy 5.25-inch floppy disks. In order to avoid them being confiscated at Soviet customs he had to hand them out to other members of the tourist group. These disks helped the CCMC to obtain a pirated copy of P-Cad from Moscow, a piece of software used to aid PCB design.

fluctuations in temperature of Soviet components, the overall reliability of the PC was adversely affected (Rätsep interview).

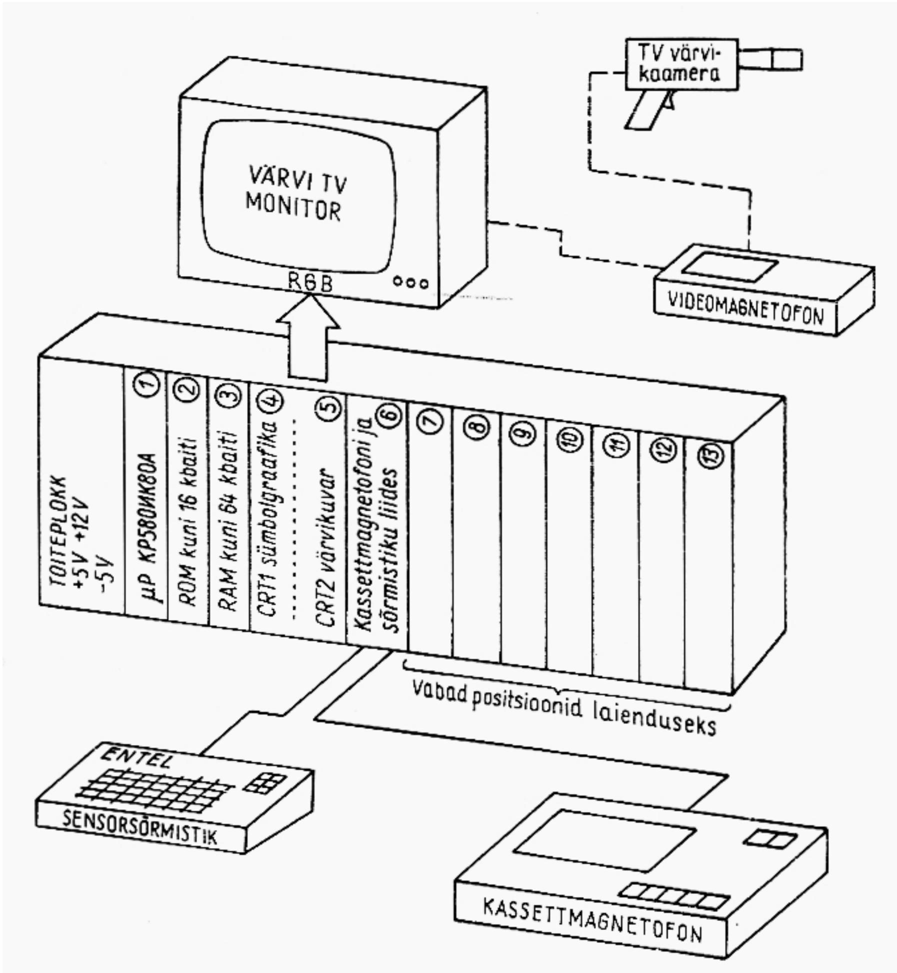
Work on the processor module started around 1981, with memory solutions and graphics (including colour) to follow soon after. With the acquisition of a floppy drive and disks the prototype was working in 1983 (Malsub interview). As with the hardware, obtaining software was not easy and personal contacts had to be used. For example, a version of CP/M was received from a worker at the IoC despite explicit prohibitions for these workers from distributing the operating system. This fact is even more ironic considering that the software itself was pirated from the West.[31]

Figure 3.3 shows one possible set-up. From left to right the central block consists of a power supply unit, processor (1), ROM up to 16 KB (2), RAM up to 64 KB (3), CRT1 symbol graphics (4), CRT2 colour monitor (5), tape recorder and keyboard interface (6) and vacant slots. Also shown are the keyboard and tape recorder themselves, colour TV display, video recorder and video camera.

In parallel with the development of additional modules, first applications started from 1983 onwards. Monitoring the telephone network indeed yielded useful statistics, and one system was developed for the Lithuanian police to scan fingerprints. The CCMC employees themselves were interested in receiving teletext from Finland—as Finnish TV could be seen on the northern coast of Estonia it provided a window to the free world, making TV schedules themselves highly sought after items. A video computer system was developed for Tallinn Pedagogical Institute which used Entel to track and analyse the movements of skiers and swimmers. It was because of the recommendation of an employee of this institute that Uno Pilvre contacted Malsub.

31 One IoC employee recalled people in Moscow scanning the original code and programmers' comments to replace phrases like 'copyrighted' with 'made in USSR' (Paluoja interview). It is also interesting to note that nowhere in the article outlining the basic features of Entel (Malsub 1986) is CP/M compatibility explicitly mentioned.

Figure 3.3. The set-up of Entel as pictured by its developers (Malsub 1986)



The Estonian education sector at that time was coordinated by three different administrative domains, with vocational education coordinated separately from general secondary education. Pilvre was a computer enthusiast working for the Committee of Vocational Education. He convinced Malsub to think about adapting Entel for school needs. While Juku was becoming a computer for secondary schools, Pilvre argued that Entel could find its niche in vocational education: as a computer designed for managing technological processes it would be suitable for industrial arts classes. Pilvre also envisaged its mass production.

Malsub was more interested in new technical challenges, however, and so his commitment to the whole endeavour was half-hearted. Only some novel features were developed. A curious one includes a wooden case which was designed because an employee of the CCMC was married to a department manager in a furniture factory. For promotional purposes Entel also participated in a local school computer contest. The developers did not do too much to prepare specifically for the occasion, however: in 3 days some introductory programs were written and BASIC language integrated into the ROM (Malsub interview). Its limited colour capabilities were enough to attract attention and raise some controversy, but hardly anything else. Its third place was therefore no surprise. Entel fared better in the VDNH exhibition, where it received a silver medal.

Some additional demo programs were developed (e.g. chess, filing instructions for industrial arts classes) and demonstrations to teachers carried out. As a result at least two computer classes for vocational schools were created . A director of the Pöögelmann factory, Taivo Uffert, participated in one those and was sufficiently impressed to suggest the production of Entel as the plant's consumer good. However, being centrally controlled, the mere wishes of the director were not enough. Soon it turned out that it would be impossible to allocate a sufficient quantity of memory chips even for a Minelektronprom factory, the exclusive producer of computer components (Malsub interview).[32] Shortly afterwards Uffert was fired for political reasons and replaced by a Russian director.

The connection between Entel and Pöögelmann was thus severed, and the CCMC lacked incentives to pursue the matter further. Getting Entel into mass production would have required good connections and dedicated lobbying. But the CCMC workers were earning premium pay from various contracts already. The pressure from the management was notably absent—the director of the CCMC only aimed to create an environment *"to let the boys play"* (Tajur interview). If anything useful turned out, good, but wider diffusion of these applications was never seriously

32 As an illustration of the failings of the Soviet supply chain, a teacher from one of the vocational schools that had obtained Entels, having learned of the CCMC's troubles, introduced Malsub to a black market dealer in Moscow who could sell him 200 required memory chips immediately.

considered. In fact, no detailed technical documentation that could be sent to large factories was ever prepared.

One might also say that it was the variety of constantly shifting interests that limited the CCMC's commitment to PCs. For example, by 1985 one employee of the CCMC had designed his own version of a school computer based on the Zilog processor. But Malsub found Zilog technologically outdated, discarded the project and no full prototype was ever built. The combination of technically interesting and profitable challenges meant that the CCMC's interest in computing disappeared as new avenues opened up. When reforms allowed extension of private initiative to many economic activities hitherto state-controlled, the CCMC workers established a cooperative called Viko in 1987 and started producing satellite receivers. There was a brief attempt to cooperate with an enterprise in Leningrad to design a PC based on the 8086 microprocessor, but it was abandoned. Entels themselves continued to be made by (decreasing) demand roughly until the end of the 1980s, with the total quantity produced being around 50 (Malsub interview). New Soviet and Western computers were already coming and the CCMC itself had moved on.

3.2 Lithuania

3.2.1 Developments in the education sector and BK-0010Š

As in Estonia, the history of personal computers in Lithuania intersects with developments in the education sector. What is notably different, however, is the presence of a computer industry: Sigma production union (various factories and later a research institute) which belonged to the Ministry of Instrument Making, Automation Equipment, and Control Systems (Minpribor, see section 3.2.3), Venta institute and Nuklonas plant in Šiauliai which belonged to Minelektronprom. Nuklonas in particular became involved in the serial production of school computers. To understand why and how, the situation at the local level needs to be discussed.

Informatics teaching in Lithuania generally began in autumn 1986. Many problems facing the education sector were similar to those in Estonia: lack of informatics teachers, lack of courses for them, lack of materials, lack of instructors, conservative attitudes and even scepticism among some who perceived it as yet another fad soon to be forgotten. And of course, only a handful of computers with virtually no educational software (Dagienė 2006: 15, 19). The first teachers only passed a 2-week summer course (Dagys 1995), so the actual teaching practice must have involved a great deal of on-the-job learning.

At that time one could speak of three main academic centres involved with informatics education. Each had a slightly different outlook on school computerization. Probably the most prominent in terms of tradition and lasting legacy was the Institute of Mathematics and Cybernetics of Vilnius State University (IoMC). Led by Gintautas Grigas, the institute had established a School of Young Programmers as early as 1981. The call to start school computerization and an accompanying stress on algorithms and programming landed on fertile soil. The IoMC's vision was to develop 'precise thinking', to enhance the skills of problem-solving by dividing overall problems into various sub-problems, to read and comprehend algorithms, and realize them in actual programs (Grigas interview). Computer access was scarce at the beginning of the 1980s, but a lot of these exercises could be performed without a computer anyway. When it came to school computerization, the IoMC favoured supplying schools with Western PCs if possible—at first Yamaha computers which the USSR was negotiating to buy at the time. When this failed, the IoMC oriented itself towards IBM-compatibles, which had started to gain popularity in the West. The combination of these two visions about teaching and 'computers of the future' accounts for the IoMC's overall preference to have relatively few computers of higher quality rather than the other way round. The IoMC was also notable for its dedication to the Lithuanization of computing (software, keyboard standard and vocabulary).

The second centre, the Computing Centre of Vilnius State University (CCVSU) had struck up an agreement with the Exciton (Экситон) plant in Pavlovsky Posad in

1984. The plant was the first producer of BK-0010 computers, Minelektronprom's proposed models of school/home computers (see below). The CCVSU developed a version of BASIC[33] which was embedded into BK-0010's ROM and by so doing gained an early and thorough knowledge of the computer and its capabilities. This made the CCVSU perhaps a bit more sympathetic towards the computer than would otherwise have been the case (	Ališauskas interview). While the IoMC and the CCVSU were both more software-oriented, the third organization, Kaunas Polytechnical Institute (KPI), was more interested in hardware (see section 3.2.2).

At the time computers were a novelty to most people, including members of the Communist Party apparatus. Hence it is very likely that officials might initially not have had any particular vision about school computing, but were rather receptive to different proposals (Zlatkus interview). For example, in 1985 the Lithuanian Planning Committee approved a computer classroom equipped with 10–15 Vilnelė computers[34] (1985.19.06), using TVs produced in Šiauliai and Vilma tape recorders, as a temporary solution until school computers started to be produced. This decision seems to have disappeared without ever being actualized. It can be interpreted as an illustration of the willingness of Lithuanian authorities to accept local initiatives in conditions of little knowledge about the topic and a lack of machines.

Things changed considerably when Minelektronprom stepped onto the stage. It is unknown whether the initiative came from the local level (Venta institute/Nuklonas plant) or whether it was centrally decided by Minelektronprom authorities in Moscow, but a resolution of the Lithuanian Central Committee and the Council of Ministers (1986.30.06) approved the production of microcomputer BK-0010Š (BK stands for 'home computer', Бытовой Компьютер, and Š for 'school', школьный) in Nuklonas from 1986 to 1990, throughout the 12th five-year plan. *"It was a*

33 Initially offered to the Laboratory of the Problems of School Informatics in Riga, Latvia, but rejected by them (Eglājs interview, see also section 3.3.2).

34 Developed in the Semiconductor Physics Institute of the Lithuanian Academy of Sciences for industrial process control. Used Soviet Intel 8080 clone. The team was small, approximately five to six people. A software programmer, Raimundas Malaiška, recalls that maybe about ten computers were built in total, one of which was used in Panevežys TV factory. Little else is known about Vilnelė.

possibility for Lithuania too—to produce computers for the whole USSR. This had an influence [on] *the later decisions on delivering BK to Lithuanian schools more than opinions of experts"* (Ališauskas interview). That is to say, neither the aforementioned centres of informatics education nor the schools themselves had any formal influence on decision-making (Ališauskas, Grigas interviews). The game was now being played at the higher level—but of course such participation could also have meant that more computers would eventually be available for Lithuanian schools.

It is uncertain whether the mobilization of local actors against this decision could have made any difference at this point. What is notable instead is the lack of such action before and after the fact. Regarding schools this can be largely attributed to insufficient knowledge. When asked about whether teachers had any opinions about the BK-0010Š before it came to schools, one answered: *"In the beginning there was only information that there will be some kind of computer"* (Dinda interview). When the actual supply started later on, information was obtained by seminars held by the CCVSU. So it is sensible to presume that at this point schools themselves had no particularly specific preferences except for favouring the presence of a computer over the absence of one.

But the better-informed educational centres were also divided in opinion. KPI was advocating Santaka, its own Sinclair ZX Spectrum clone (see section 3.2.2). But for the CCVSU *"to buy Santaka or something similar but cheap was almost the same as to buy one old-fashioned computer instead of another old-fashioned computer. It was already clear that the future was IBM-compatible"* (Ališauskas interview). Likely this view was also shared by the IoMC, at least to some extent. But as long as IBM-compatibles could not be obtained (no Soviet clones for civil uses and no possibility for large-scale import) while the CCVSU already had practical experience with BK computers, then accepting the central decision could have been perceived as a satisfactory short-term pragmatic solution to the scarcity problem. So the relevant questions came to be about what could be achieved with the resources at hand, how much effort should be put into such a project and for how long.

One may also wonder why the issues of cultural identity were not so pertinent here as in Estonia. An interesting suggestion was made by an interviewee that one reason why local school computerization did not become mobilized around a single Lithuanian PC project, why therefore different visions prevailed and why Russian-designed computers were not seen as threatening might be found in Lithuania's demographic situation (Ališauskas interview)—by contrast with Estonia and Latvia, the proportion of ethnic Lithuanians had remained at around 80% in Soviet time (see table 3.2).[35]

With respect to the computer itself, BK-0010 was a Soviet design running on a 16-bit K1801BM1 processor, an original Soviet single-chip processor developed on the basis of DEC LSI-11 (single-board PDP-11) (Ceruzzi 2003: 244, Malashevich 2008: 97). The PC had 32 KB ROM and 32 KB RAM, half of which was devoted to video. Black and white TV was used as a default display, although with a little bit of soldering a colour TV could be connected (Gurevičius 1988). A tape recorder acted as an external memory device. Initially BK-0010 had a membrane keyboard (figure 3.4) which was later replaced with a traditional one. Its cost was 600 roubles, or 650 including the integrated BASIC (BK-0010Š user manual, Ališauskas interview), making it cheaper than other personal computers (most of which appeared later). Characteristically for Soviet computers, the basic set included neither the display nor the external memory device, which had to be sought out by the user. Twelve BK-0010Šs connected to a teacher's computer DVK-2MŠ (ДВК-2МШ) made up a school set called KUVT-86 (КУВТ-86): in this case the teacher's computer did have a floppy disk drive and (when possible) a printer.

Save for adding Lithuanian characters to the keyboard, the rest of the design was left

35 Lithuania's homogeneous demographic situation, which resulted in a lesser degree of existential threat and less of a potential impact by minority groups, has been used somewhat similarly to explain why Lithuanians were the last of the Baltic republics to start movement towards independence but were the first to declare it (Taagepera 2000, cited in Kasekamp 2010: 168).

unchanged.[36] Plastic details were prepared by the Plasta factory in Vilnius, while a plant in Kaunas was supposed to take care of repairs. Sigma was required to provide a specified quantity of PCBs from 1987 (resolution of the Lithuanian Central Committee and the Council of Ministers from 1986.06.30). However, a warranty coupon from the time shows that at least some computers had already been produced in November 1986.

Figure 3.4. BK-0010 with display, power supply and software tapes (photo taken by the author)

36 For this reason I would characterize both BK-0010Š and Poisk (see section 3.2.3) as quasi-cases: although production took place or was meant to take place in Lithuania, the local input to hardware design was minimal. A good historical overview of BK-0010's development from the designer side is provided by Malashevich (2008), although the value of factual information surpasses that of his somewhat enthusiastic assessments of the viability of the endeavour.

By contrast with decisions about hardware production, academics were heavily involved in software development. An expert group for school computerization was formed by Juozas Zalatorius (meeting protocol of the Lithuanian State Planning Committee 1986.09.04). In cooperation with the CCVSU and KPI a plan for school software preparation for 1986–1990 was formed. The centres agreed to develop various solutions beginning from system software (e.g. networking), and programming languages (LOGO, Pascal), and continuing with educational programs (e.g. systems for studying algorithms or creating exercises for students) and ending with user applications (e.g. text and graphics editors). The plan was quite detailed for 1986–1987, specifying various developments by quarters of the year, but a large bulk of software (e.g. packages of user applications, database software, games etc.) was simply left to be programmed at some point between 1988 and 1990. Eventually it was only the first half of the plan that was mostly realized (Ališauskas interview).

The problem was that BK-0010Š had several shortcomings. The tape recorder was as inconvenient and unreliable a memory device as always and the TV screen was still uncomfortable to watch. Flat keys were difficult to press, and it was difficult to focus on the screen and type without errors at the same time. A later, more traditional keyboard was highly sensitive, which resulted in many letters being created with only one push; occasionally the keys themselves got stuck too. If the whole display was used all software had to fit into 16 KB of RAM, which could not be expanded. This in turn set limits to possible software solutions. Initially 8 KB of ROM was reserved for system software and 8 KB for the Focal programming language, with 16 KB remaining unused (later replaced by the CCVSU's Vilnius BASIC, occupying 24 KB of ROM). But Focal was perceived as an extremely poor language, suitable for calculations but difficult to programme for more complex tasks. The accompanying system software itself was so rudimentary that it was difficult to speak of BK-0010Š as having an operating system. Although the instruction set of the microprocessor was also being used by other Soviet computers (e.g. Elektronika 60, SM-3), adapting the programs was not straightforward. Neither was the computer compatible with Western models. As a result, a lot of extra effort was required on the part of programmers to create or adapt software. Additionally, troubles in schools arose from insufficient knowledge about electronics among teachers and students, leading to errors, crashes and improperly repaired machines (Ališauskas, Bernotas, Dinda, Grigas, Kaklauskas, Sasnauskas interviews, Markevičius 1987). Owing to these factors neither the schools nor the academics liked the BK-0010Š very much. But what was said about the Latvian situation—*"it is better to have BK than it is to have nothing"* (Eglājs interview)—was also believed in Lithuania.

At least for a while the central mindset about the importance of programming and algorithms was enforced by computers with little or no user applications. Unsurprisingly then, this accorded best with the IoMC's stress on algorithms. This was clearly illustrated by a Lithuanian informatics curriculum from 1987, the lion's share of which was devoted to algorithms, computer architecture, operating principles and programming (Ališauskas & Čėsnienė 1992). The ever-present scarcity of computers has already been mentioned numerous times. The solutions to overcome this included looking for the patronage of sponsors (e.g. factories, film studios etc.) or heavy lobbying to obtain rare Yamaha models (Dinda interview, Oginskas 1987.30.04)—or really computers of any kind. This gradually contributed to the diversity of PCs in use: so much so that by the end of the 1980s the situation was described as a 'zoo' (Dagienė 2006: 31). Furthermore, there was a general lack of experience with computing in schools making it difficult to form any prior preferences and expectations at all.

Thus it is sensible to presume that at the time most of the teachers did not deviate much from the algorithm-based and programming-centred direction, and the main skills developed during teaching practice and communicated to others by various means (e.g. conferences, journal articles) were also mainly of this type. In my view the shift towards user applications would have required widely diffused computers (user applications are meaningless to teach without the PCs themselves), a certain amount of standardization (making it easy to diffuse both created and adapted software) and, of course, software itself (for it is unrealistic to think that every user would be a talented programmer). It is no wonder then that perceptions started to change only at the end of the 1980s and the beginning of 1990s. However, before moving on to this era another contender for Lithuanian school/home computerization should first be examined.

3.2.2 Santaka

The origins of Santaka can be traced back to Eimutis Karčiauskas from KPI, who managed to visit Austria in the mid-1980s. He brought back a Sinclair ZX Spectrum, a popular British 8-bit computer released in 1982. Allured by its simple architecture, low cost, colour graphics and wealth of software (especially games) Gintautas Žintelis (also from KPI) and Karčiauskas devised a plan in 1985 to create a Lithuanian Sinclair clone for young people.[37]

The situation KPI was trying to tackle should be familiar by now: a dearth of personal computers in schools and especially homes. And even where computers themselves were available there was not much software to speak of. On the other hand, 8-bit computers were not exactly cutting-edge technology any more. *"We understood quite clearly that Santaka is for a short period of time. It was not the case that it would be possible to use Santaka in ten years"* (Žintelis interview). Therefore the aim was to act quickly while producing as many computers as possible. Considering the difficulties with getting sophisticated technology into mass production in the USSR, the goals were somewhat contradictory—spend too much time on bureaucracy and the project might become obsolete, or act now but without full official support.

Initially the idea was simply discussed between people at KPI themselves and with other personal computing enthusiasts. Notable individuals include Vidmantas

37 They were not alone in this reasoning. Sinclair-type computers became very popular in the USSR. For example, the website Planet Sinclair lists over 50 such clones that were produced in the USSR and Eastern Europe; more machines can be found on a Russian ZX Spectrum website, SpeccyWiki. A few examples can also be found in Estonia, where Estron was contemplating the idea of producing Sinclair-compatibles and proposed the idea to Tartu, Palivere electronics producers were assembling their own ZX Spectrums as a hobby, and a member of Entel group had devised a school computer design based on a Germand Z80 microprocessor analogue.

Balčytis from Vilnius State University and Henrikas Matulionis from the Kaunas Radio Measurement Equipment Scientific Research Institute (KRMESRI). While the first went on to design his own Sinclair[38] and later a PC/XTclone (see section 3.2.4), the second expressed serious interest in KPI's idea.

The KRMESRI, belonging to the Ministry of Communications, was mainly involved in military tasks. These included the construction of various measurement devices (e.g. for checking the parameters of microchips), preparation of necessary technical documentation and small-scale production. As such it had finances, know-how of compiling the technical documentation, some production base and professional employees accustomed to working according to military standards. Although the KRMESRI was already providing some output for civil purposes (e.g. medical measurement equipment) this project could better express the wish of (at least some of) its workers *"to be useful for Lithuania"* (Matulionis interview). In addition, a Sinclair clone was a technically interesting project. Furthermore, the KRMESRI's geographical proximity to KPI eased communication and facilitated the maintenance of close contacts. So in many ways the KRMESRI was an excellent partner for the university. In fact, a working group of six members was soon established in the institute. Compared with the KRMESRI's total number of workers—around 2,700 (Matulionis interview)—it was not much, but nevertheless a considerable and welcome addition to the project.

Work on the prototype at KPI began in 1986 and lasted about 6 months (Matulionis, Žintelis interviews). A feature characteristic of Soviet computer production in general was especially salient in an attempt to make a direct copy a foreign analogue—whereas in the West clone-makers aimed to use fewer chips than the original machines had used, often the newer chips or even ones that were used in the initial

38 According to Balčytis this was serially produced by an unknown cooperative at the end of 1980s. Balčytis's idea was not to duplicate Sinclair hardware as closely as possible, but simply to achieve the same functionality. The result was a simpler device which used far fewer components than Santaka and reportedly enjoyed much popularity among hobbyists (Matelionis interview). However, its full compatibility with the ZX Spectrum has been disputed by one member of the Kaunas group (Prekerienė interview).

model were not being produced in the USSR (yet). So when Western clones could be smaller, more reliable and easier to produce (see Cringely 1996, ch. 9 for a particularly clear illustration) it was the other way round for Soviet versions. But more components meant that more could go wrong. A higher degree of complexity might create additional problems not present in the original design (e.g. the need to deal with increased power consumption). In this particular case, the uncommitted logic array chip used in the ZX Spectrum was not being produced in the USSR and thus needed to be built from other types of chips. Also, available RAM and ROM chips had smaller capacities than those used in the original design and thus their number needed to be increased to achieve the same memory size. Additional issues arose from the ever-present quality problems: components not working, components stopping to work after a while, components being sensitive to changes in temperature, having unpredictable output voltages and so on (Matelionis interview).

But (similar to other projects like Juku or Tartu in Estonia) it was important to use simple enough components so that they could potentially be acquired. Here the main problem was that Z80 processor analogues were at that time only produced in the German Democratic Republic (GDR, East Germany). The usual obstacles applied here: getting approval to buy them officially could have resulted in spending 2–3 years on bureaucratic procedures with no guaranteed success. That would have made the goal to speed up the production process void. Emulating Z80 was considered as an alternative, but this would have reduced the speed of the processor to the point at which some programs using graphics would not have run (Žintelis interview). This worked against the idea of full software compatibility. Paradoxically, by contrast with organizations it was relatively easy for private individuals to buy the processors from East German shops in small quantities—provided that they were permitted to visit the country of course—and bring them back to the USSR. Hence it was possible to buy them on the local black market for use in prototypes. The issue of obtaining larger quantities of microprocessors was bracketed by the development team at this stage of the project.

In parallel, people in the KRMESRI were already preparing necessary technical documentation so that the computer could be manufactured. They had started looking for display and external memory solutions—Šilelis TVs and Vilma tape recorders, both produced in Lithuania, were eventually chosen (see figure 3.5). The group also designed the case and the keyboard. Since it was supposed to be used locally some special characters from the Lithuanian alphabet needed to be added, raising the number of keys and increasing the overall dimensions of the case (Matulionis interview).

Figure 3.5. Santaka set including the main unit, TV, tape recorder, power supply and some software tapes (Saulius Matelionis's private collection)

Žintelis was on good terms with both industry people and politicians in Lithuania. He therefore attempted to advocate the computer as potentially suitable for schools. He and the director of Venta institute, Kazimieras Juozas Klimašauskas, participated in top-level meetings devoted to the issue. The proposed computer could actually claim several technical advantages over BK-0010Š: despite having an 8-bit processor its clock speed was higher (3.5 MHz vs. 3.0 MHz); it had more colours than BK (eight vs. four) and did not need any additional tinkering to display them; it had more RAM; finally and most importantly, it could use any existing piece of ZX Spectrum software. But the main arguments used against the proposal were BK-0010Š's new generation processor and Minelektronprom's promises to produce them in massive numbers—hundreds of thousands if need be.[39] These arguments proved decisive and it quickly became apparent to Žintelis that BK-0010Š would remain the main choice for schools.

By 1987 the working prototype was completed (Rimkus 1987.31.01). People at KPI and the KRMESRI decided to call the computer Santaka—'confluence'—to express the merging of science and manufacturing. Žintelis approached Sigma next to obtain the microchips and produce Santakas, but was turned down. The recollections of people from both sides (Drąsutis, Židonis, Žintelis interviews) are vague, confusing and contradictory regarding this particular episode, but some possible reasons can be teased out. First, Sigma's main task was to produce DEC analogues for Soviet industry and demand for those far exceeded the factory's capability to supply them. So it was quite preoccupied with its primary production. Second, for people accustomed to minicomputer building and well-acquainted with Western technology, Santaka seemed an unreliable 'hobbyist computer' (Židonis interview) with no technically interesting solutions. Third, the production of Santaka as Sigma's consumer good on mass scale would probably have delayed the process, which the developers wanted to avoid. But otherwise small-scale production could have simply used too little of Sigma's capacity while being a nuisance to prepare at the same time. It just did not seem to be worth the effort. Having good mechanical equipment, Sigma did however agree to produce the cases for Santaka.

In the same year (1987) the prototype was also presented to Lithuanian state leaders along with the plan of raising the computing skills of young people. Algirdas Brazauskas, a future key figure in Lithuania's movement towards independence, was

39 This did happen eventually, but only by the beginning of the 1990s. Malashevich claims that the total number of BK-0010s and BK-0011s produced was 162,000, of which nearly 125,000 were produced in the Eksiton plant (2008: 100). Production figures for Nuklonas are unknown.

also in an influential position in the Party, being a secretary responsible for industry matters. He liked the idea and asked the institute to produce 200 Santakas. But the support was informal—Santaka was not part of the local, Soviet Lithuanian plan and it was up to the developers themselves to find the necessary components and production infrastructure. As a Minelektronprom representative, Klimašauskas had opposed Santaka as an official school computer; however, he did not oppose the project altogether as an acquaintance of Žintelis. Thus he agreed to visit the GDR, buy 200 microprocessors and smuggle them to Lithuania (Žintelis interview, Matulionis 2011)—the easiest way to solve the availability problem for small series production.

Initially KPI's design was quite unstable and therefore unsuitable for serial production: there were simply too many small, unpredictable errors differing from machine to machine which had to be corrected manually. It was the task of the group in the KRMESRI to eke out such faults so that the computer could be more easily produced (Prekerienė interview). As always the production equipment itself needed to be set up properly. As a result, the serial production could only begin in 1988. Nevertheless the whole process from prototype to production was considered exceptionally quick for the Soviet context (Matulionis interview).

KPI's idea was to expand the circle of young people with hands-on experience with personal computers. Therefore as wide diffusion as possible was encouraged. Of the 200 Santakas that were produced by the KRMESRI, some were kept by the project participants but most were given to secondary schools for free (Matulionis 2011, Žintelis interview). The informal diffusion of the design and the emergence of homemade copies was also considered a success. Numerous modifications appeared, e.g. a floppy interface and CP/M adaptation. Matulionis (2011) has suggested that the number of self-made Santakas might have exceeded the KRMESRI's production by about ten times. However, because of the lack of statistics and the popularity of ZX Spectrum in the USSR it is impossible to substantiate this estimation. Hobbyists did not need to bother themselves with mass availability of resources and hence the solutions could differ according to which components could be acquired. So they did not necessarily need to resort to Santaka's outline—indeed, Balčytis's previously mentioned design was one of the popular alternatives that diffused beyond a single prototype.

One could argue that from KPI's viewpoint the project of temporary production had been fulfilled. Therefore at some point it dropped out of the endeavour. The KRMESRI itself, however, took the project further. In 1988 Santaka was presented in the Ministry of Communications in Moscow as an example of the institute's success. Various plant managers were also present. Allegedly the minister was a huge fan of basketball and thus most impressed seeing a basketball game being run on Santaka (Matulionis 2011). Subsequently many different plants took a serious interest in the project because Santaka seemed a promising and relatively cheap way to fill a still largely empty home computer niche. For the institute selling the design was a good way to make money, although fixed salaries meant that developers themselves could not gain much from the transactions.

Owing to the impact of the presentation, the minister let the institute itself choose a factory that would start the production. One in Minsk, Belarus, was chosen because of its good reputation and proximity to Lithuania, making it easy to travel and provide assistance in case of potential problems. The specialists in Minsk were able to prepare serial production in less than a year without initially even having full technical documentation. The latter was prepared in parallel by the joint cooperation of the KRMESRI and the factory specialists. The computer went into mass production in the beginning of the 1990s under the name Santaka 002 (Сантака 002) and thousands of machines were likely built over many years.[40]

Santaka was also very successful in the VDNH exhibition, where its various design solutions gained one gold and four silver medals. Subsequently it was supposed to be

40 Matulionis (2011) suggests 5,000 computers a year over 3–4 years, but once again the actual figures are unknown. A picture of Santaka 002 number 4492 from November 1991 can be found online (Frolov 2009).

entered into exhibitions in Prague and Berlin as an illustration of Soviet technological achievements. However, it was only presented in Prague before wider changes in political landscape kicked in. Lithuania was by far the earliest of Baltic republics to declare independence on the 11th of March, 1990, over a year and a half before the official dissolution of the Soviet Union. Relations between Moscow and Lithuania quickly worsened, resulting in an economic blockade then military intervention that resulted in the death of civilians, followed by Soviet special forces murdering Lithuanian border guards to provoke a violent response and so on (Kasekamp 2010: 165–171). The same was true of relations between Lithuania and the other union republics that were still loyal to Moscow. The KRMESRI workers were denied the trip to Berlin, did not get half the money from the Minsk factory and got into severe financial difficulties as orders were abruptly cut.

As one of the engineers was leaving the KRMESRI to become a director of a plant in Krasnodar, Russia, Santaka's documentation was also sent there. For unclear reasons (but quite likely once again related to worsened relations) the institute failed to receive any payment, despite the fact that production was initiated on two models, called Impulse and Impulse-M (Импульс and Импульс-M; SpeccyWiki 2009). With the disintegration of the Soviet Union the project initiated in Lithuania had migrated out of the hands of its original developers for good. Left with no military applications, the KRMESRI's primary but at the same time relatively narrow expertise in advanced high-frequency devices could not find enough output in Lithuania's internal market, nor was it able to reorient its production quickly to Western markets. The institute dissolved in the early years of the 1990s.

3.2.3 Sigma and the education sector: Poisk and Sigma 8800

So far only passing references have been made to production union Sigma, a genuine computer hardware, computer systems and software production industry. All computers described so far were devised and/or produced elsewhere. At best their encounters with Sigma were episodic. It is therefore relevant to ask why it was that the production union devoted to building computers was not very active in the PC domain. This is the subject of the present section. As before, the answer is related to developments in the education sector and wider social context.

Being a producer on the Soviet scale, Sigma was a true industrial giant in local terms —by the end of the 1980s it comprised of seven plants in various locations across Lithuania (Vilnius, Kaunas, Tauragė, Panevėžys, Telšiai and Pabradė) and a research institute that united four design bureaus. About 18,000 people were involved in production and management, while approximately 2,000 people conducted R&D. Established in 1957, the production union had moved from cash registers to minicomputer production, mainly for Soviet industry. At the beginning of the 1980s it was centrally ordered to reorient itself to producing DEC clones (PDP and VAX), and in 1986 it started the production of SM 1700, a VAX 730 clone (STIMTI 1989, Telksnys & Žilinskas 1999: 33–35, Drąsutis 2000.15.11, Drąsutis 2000). Other notable products included multi-layer PCBs and hard disk drives (up to 80 MB) which were highly valued in the Soviet Union for their relatively high performance and reliability (Drąsutis, Židonis interviews). Being a strategically important part of Soviet industry it was directly controlled by Minpribor, meaning in effect that Lithuanian government had no formal say in determining its main activities.

In the West, DEC itself had not picked up on personal computing. In the USSR, Sigma's DEC clones were in excess demand. Building them was a laborious and demanding task. PCs were already being produced by other factories in the Soviet Union, including Minpribor factories (e.g. Schetmash in Kursk, which also produced Tartus, see section 3.1.2). All this being the case, Moscow had no reason to change its technical policy and encourage the plant to divert from its main line of production. From the central point of view this could have seemed simply as an unnecessary duplication of effort. Sigma was to do what it was told to do and leave decision-making to those who knew better.

The utmost priority of Sigma was to fulfil the plan. Upon success new resources and investments could be gained. Failure on the other hand brought the risk of sanction.

Thus the top management of the plant had little reason to encourage initiatives related to PCs, which seemed like toys compared with big, expensive and powerful minicomputers. The directives from central authorities aligned with the execution by top management help to explain why the first wave of PC production passed Sigma by in mid-1980s. It might be argued that this created conditions in which Sigma engineers, well-aware of Western solutions, perhaps found it easier to deem already existing Soviet personal computers, using unreliable tape recorders and inconvenient TV screens, as 'unprofessional' and 'technically uninteresting'. The lack of supporting features of various prototypes (such as proper production documentation), problems with ensuring that the supply would be timely and in required quantities, or questionable correspondence to all-union standards made it all the more easier to ignore novel efforts in this area (Židonis interview). However, it is difficult to tell whether it was genuinely a case of sour grapes: whether some employees could potentially have been interested in personal computing in other circumstances.

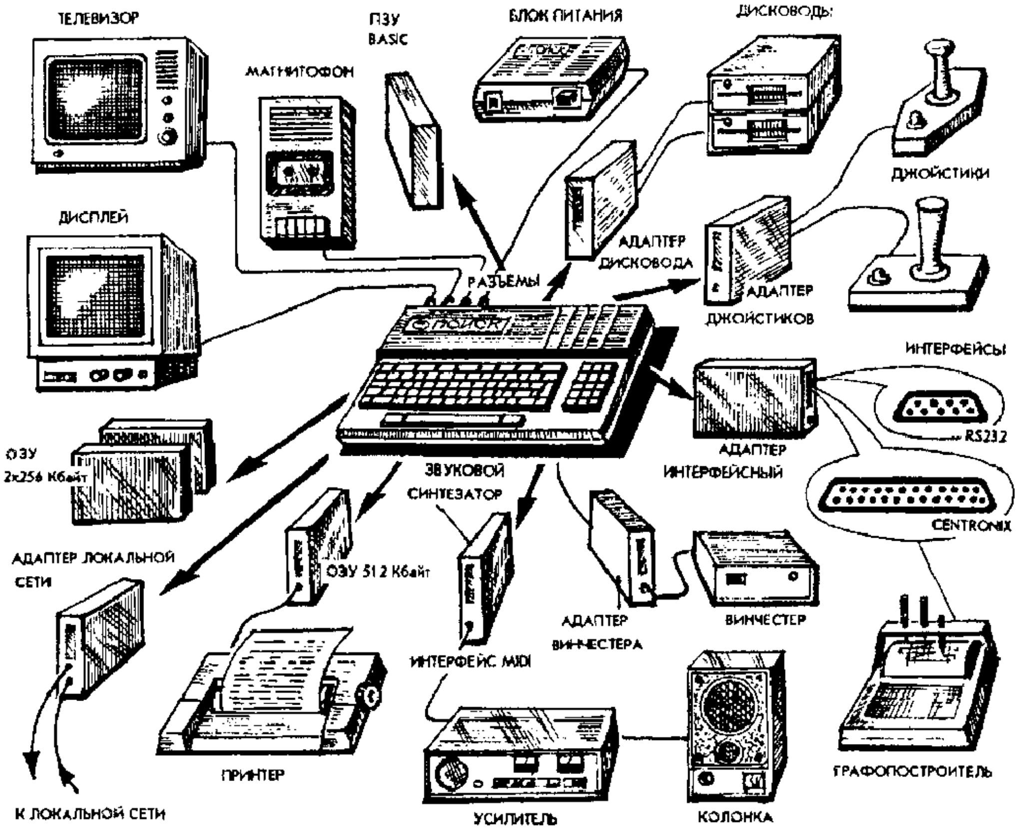
Of course, there was a formal requirement that every plant should also produce consumer commodities. But as explained above, factories' choices were usually based on their existing capabilities, which did not need extensive reorganization. In principle it was possible to show initiative or accept the initiative of others, but that would have meant organizing the production on mass scale—thousands of computers over many years—so that the plant could make use of its capacity. There was little incentive to do so provided that consumer goods were of secondary importance anyway, and easier means were available to satisfy that particular central demand. But it was exactly thus, taken as a whole, that a curious situation emerged in Lithuania: BK-0010Š was mass produced for schools, but the schools were not too fond of it; Santaka was at least potentially more attractive for its compatibility with Western software, but never went into mass production in Lithuania; and Sigma with its ample capacity, infrastructure, resources, know-how and skills virtually stayed out of this process altogether.

The reforms of perestroika attempted to make state-controlled enterprises more responsive to different initiatives. With the introduction of the Law on State Enterprise in July 1987, producers gained more freedom: whereas crucial state orders still had to be fulfilled, the rest of the output could be set by the enterprises themselves. The procurement of resources depended on contracts with other organizations and the prices of those could vary to some extent. In principle it made sense insofar as the strengths of different enterprises could be united in a single project. It was also announced that the plants would have to become self-financing and would no longer be bailed out by the state (Desai 1989: 32–34). The possibility of choice and the promise of making more profit made Sigma look out for various undertakings, PCs among them. Enterprise-internally, a proposal was made to organize the PC production line. Within Lithuania, the possible production of a PC/XT clone designed in a newly established cooperative called Lema was briefly considered, but was quickly abandoned because of lack of interest from both sides (see also section 3.2.4) (Židonis interview).

Sigma's attempt to cooperate with the Scientific-Production Union Elektronmash (Электронмаш) in Kiev, Ukraine, progressed somewhat further. In 1987 Elektronmash had started preparations for the production of a fully IBM-compatible computer called Poisk (Поиск).[41] Soviet IBM PC/XT clones had been attempted before, but they had severe shortcomings: for example, Mindradioprom's EC-1840 was very expensive and not fully IBM-compatible (Judy & Clough 1989: 276–277). Elektronmash's idea was to target schools and lay consumers, meaning that the construction had to become considerably cheaper. This meant, however, that one-to-one copies of graphics controllers and keyboard controllers could not be made. Hence some of these functions had to be handled by the CPU. That in turn made the computer considerably slower, and even increased clock speed (5.0 MHz vs. 4.77 MHz for the original IBM PC/XT) could not fully compensate for it (Smagin 2008).

41 Information on Poisk comes mainly from Boyko (1991).

Figure 3.6. Full depiction of Poisk set-up including all peripherals (Boyko 1991: 85)



Technical specifications of the realized model (Boyko 1991) included a 16-bit KP1810BM88 processor (Intel 8088 analogue), 128 KB RAM, CGA graphics with two modes and the possibility to connect the computer to a colour monitor or a TV. At least on paper expansions were abundant: extra ROM (8–64 KB) and RAM (256 KB and 512 KB blocks), one or two 5.25-inch (720 KB) floppy disk drives, a hard drive connection (20 MB or 40 MB), sound synthesizer, mouse, two joysticks, local network adapter etc. An inkjet printer, plotter and 3.5-inch floppy drive interfaces were also being developed. School sets included 8 or 16 student computers (384 KB RAM) connected to a teacher's computer (640 KB RAM, colour monitor, two floppy disk drives, printer, plotter). However, the initial set-up had only 8 KB ROM, a tape recorder as an external memory device and only three programs (BASIC and testing software), i.e. not even the operating system was included. Every expansion had to be bought separately, significantly adding to the price of the main module (which alone was 1,000 roubles). Various weaknesses aside, the Poisk still looked superior compared with other contemporary home/school computers.

Elektronmash was willing to trade its documentation, which would have allowed it to obtain Sigma's sought-after multi-layer PCBs and hard disk drives (Židonis interview). Mutual visits and consultations followed and Sigma's interest became genuine. A statement from October 1989 (cited in LCS 1989.08.12) made explicit its plans to start producing Poisk as a school computer (among other possible uses).

It seems that this proposal could have resonated well with intended consumers. Despite the production in Nuklonas, by 1989 only 130 schools out 800 had BK-0010Š classroom sets (Petrauskas 1989), and the computer itself had many shortcomings. None of the academic centres saw BK-0010Š as a viable long-term solution and efforts to develop (educational) software became increasingly half-hearted. To the IoMC, Poisk could have seemed a relatively cheap IBM-compatible PC, making their strategic vision finally practical. The CCVSU, perhaps always a bit more flexible, could have seen it as a chance to advance further computerization in schools whatever the means. And KPI had only seen Santaka as a temporary solution anyway.

On the other hand, a lot had happened in the previous few years. In addition to the emergence of cooperatives, joint ventures were also being allowed to a limited extent. One of those was Baltic Amadeus, a Soviet Lithuanian–Austrian enterprise, established in September 1988 (Dagys 1989). Baltic Amadeus imported IBM PC/AT components from Taiwan through Austria, with assembly and testing being done in Lithuania. Although superior in performance and reliability, the prices were still far too high for any of the schools and so the computers were mainly bought by industrial enterprises all over the Soviet Union. Between 1989 and 1992, around 2,000 computers were sold (Zalatorius interview). The examples of organizations that had established foreign contacts signalled increasing possibilities for importation of Western technology in the future. But they also pointed to what became the prevalent strategy in the 1990s—the move from large-scale domestic hardware design and production to local assembly of Asian-produced components.

At the same time, the gradual loosening of the constraints of the regime also meant more freedom to organize and express opinion. In September 1989 the Lithuanian Computer Society (LCS) was established. This was an expert group which aimed to give professional advice to decision-makers and shape the future computerization of Lithuania (statute of LCS 1990, Lupeikienė 1990). LCS took quite a strong stance against equipping schools with Soviet technology. For example, it advised the Ministry of Education and the local Planning Committee against buying UKNTs computers (specifically citing poor reliability and the lack of system and educational software). Buying IBM-compatibles was favoured instead (LCS 1989.29.11). Considering that the core of LCS consisted of key people from all three academic centres, its firm 'no' to Poisk stated in a letter to Sigma and the Lithuanian Planning Committee (1989.08.12) might seem quite surprising—especially given that the initial opinions of local experts asked to assess Poisk by Sigma were reportedly supportive (Židonis interview).

"We tried to find some balance," is the key to understanding this decision (Ališauskas interview). The debates on school computerization involved trade-offs between quantity and quality, diversity and standardization, low and high price, immediate availability and waiting, which determined the acceptance or rejection of new proposals. For example, at the beginning of the 1990s Lithuanian schools acquired a few hundred Commodore computers for free. Quantity, zero price and immediate availability became more important than a strategic orientation towards standardization and IBM-compatibility. Another example concerns a talk between Juozas Kazimieras Klimašauskas, the director of Venta, and Gintautas Žintelis from KPI at the end of the 1980s—Klimašauskas discussed the possibility of KPI helping Venta to design a Soviet IBM-compatible computer. He was turned down on the grounds that the proposal would have been meaningful a couple of years previously,

but was by that point too late (Žintelis interview). Similar considerations were in play with Poisk. Compared with Western computers Poisk was already outdated and would be even more so when it eventually became available; it would also be more unreliable and likely in short supply.[42] Its only advantages would be low cost and superiority over existing Soviet school computers.

In this light, LCS's decision seems less like an issue of possible hidden economic interests in preventing Sigma from entering the PC domain or of blind idealism (Drąsutis, Židonis interviews), but rather like a reasoned choice to abstain from immediate action and wait for better alternatives to emerge. LCS pointed out that whereas Sigma had proposed Poisk as a school computer the schools themselves had not been consulted about their needs and preferences. Instead of haphazard decisions, more consultation, testing and thinking about alternatives was advised. Even then, however, Sigma might have pursued its course—after all, LCS was an expert group with no formal power—but then Kiev decided to raise its demands and ask for more money for the documentation (Židonis interview). As the terms of exchange became unfavourable to Sigma the idea of cooperation was subsequently dropped. From the viewpoint of the education sector it is curious to note that even though BK-0010Š was not especially desired, this outcome actually strengthened its position as the dominant Lithuanian school computer, at least for the near future. It would take 10 more years until BKs would be completely phased out of schools (Dinda interview).

If all participants in these events in all countries agree on something it is probably the largely unforeseen speed of events at the end of the 1980s and the beginning of the 1990s. In many ways the transition was especially hard on Sigma, since making changes in a large-scale organization demanded more resources. And the problems were many. Probably the most acute was the virtually overnight loss of Eastern contracts. While relations between individual people could be friendly, relations

42 This expectation was confirmed by Boyko, who cites Kiev's production figures as being a 'few hundred' in 1989 and 10,000 in 1990. However, he also admits that Poisks are difficult to find in shops (1991: 85). In the light of the above discussion, the problem was likely much greater for peripherals.

between countries were certainly not: in response to Lithuania's proclamation of independence in March 1990, the USSR imposed a 3-month economic blockade. Lack of orders, money and new supplies resulted, while 20,000 workers still needed to be paid somehow. By contrast with Baltic Amadeus, Sigma had not established any joint ventures with Western enterprises and new business partners could not be found overnight, especially in a country still in the middle of political turmoil. Moreover, there was a profound lack of marketing skills—whereas in the Soviet system much ingenuity and effort was put into obtaining resources, now potential buyers needed to be convinced. Having until recently relied on extensive central support with secured contracts all over the USSR, Sigma found itself suddenly in a highly vulnerable position.

Equally important was the state of material support. The problem was that Sigma's productivity was lower than that of Western enterprises. For example, Sigma's chief engineer recalls the production union spending eight times more than American manufacturers to produce the same Winchester hard disk (Drąsutis interview). But there was also an upper limit starting from which a certain technology became too complex to be produced at all. In the case of hard disks, the capacity limit for Sigma was 80 MB. In comparison: Apple Macintosh IIfx, introduced in March 1990, already had a 160 MB drive option (EveryMac.com).

In this situation Sigma needed to take a look at its production equipment, finances and existing stock to quickly come up with some products which would satisfy three conditions: profitability, demand and ability to produce. Many ideas were put forth and tried out. A discussion with Leningrad involved the production of the first laptop in the Soviet Union; Polish Mera-Błonie was contacted to produce printers; Armenians were negotiated with regarding the production of floppy disk drives (Židonis interview). But none of these proposals progressed further than the prototype stage at best, because of recurring issues: lack of hard currency, lack of finances, economic recession and political hostility.

One of the later projects was Sigma 8800, an IBM-compatible 16-bit computer which was planned with a colour monitor, a printer and a 20-MB hard disk. Essentially the idea resembled that of Poisk: an IBM-compatible with a price advantage. An initial outline of technical characteristics was prepared in February 1990, with a plan to start production in April 1991 (STIMTI 1990.15.02). In search of potential customers, Sigma once again approached the education sector.

Yet again the social context had changed. In the now independent Lithuania the Centre of Informatics and Prognosis had been established in October 1990 to guide school computerization. A few key people had moved from the CCVSU to work at the new centre. For the first time the education sector had full autonomy to decide whether to accept or reject any proposal. On the other hand, Sigma's need to secure contracts had increased. But in many ways the dilemma was the same: in whom and on what grounds should trust be invested?

Initially the Centre of Informatics and Prognostics showed interest in the proposal. But for reasons unknown the project was delayed—in December 1991 the computer was still to be tested by Sigma (State Institute of Information Technology 1991.21.12). By that time, however, schools had started receiving Western IBMs with 80286 processors, by various means (charity, purchases by municipalities etc.). Sigma's PC/XTs, while domestically produced, were mostly based on Soviet technology, were expensive (small series, obsolescent production equipment) and would have been outdated even more by the time they would have become available in mass quantities. The education sector was willing to approve Sigma's offer on the condition that the computer would use 80286 processors (Zlatkus interview), for which there were no widely available Soviet analogues. Sigma could not meet these requirements. The enterprise also tried to negotiate with Riga, most likely the VEF plant, for production of Sigma 8800s as control devices for the plant's private branch exchanges. The negotiations fell through, however, and eventually about 100–200 PCs were made (Desiukevič, Drąsutis, Židonis interviews). These were used to aid Sigma's own production.

All in all, Poisk and Sigma 8800 were just small episodes for Sigma—reasons for its collapse ran much deeper.[43] To continue computer production, the organization would have needed to escape the trap of existing capabilities failing to meet changed expectations. Upgrading production capabilities would have required a lot of investments, both in terms of equipment and for retraining the workers. Although there had been discussions between DEC and the USSR, and some of Sigma's engineers even received some training in DEC's facilities, DEC's growing financial troubles meant that no investments followed. Other promises of foreign financing also failed to realize (Židonis interview). The tumultuous political and economic environment probably did not help to gain the trust of potential investors, who preferred to continue investing in low-cost mass production in Asia. There was also the question of whether after such large-scale technological renewal Sigma would have been competitive. Too big for the domestic market, it would have needed to enter into global competition and produce computers cheaper than its competitors. As an independent producer, it could not have bought components cheaper than could large corporations. Also to be taken into account is the amount of mental adaptation needed to operate in a market economy. In every Baltic state the beginning of the 1990s was about (re-)learning *"what is capitalism and how to eat it"*, as summarized by one Estonian engineer (Jelle interview).

In this regard perceptions had strongly turned against Sigma. The organization evoked an image of an eastwards-facing, sinking Titanic that had relied too much on its Eastern contacts for too long and was too passive, hoping for state initiatives rather than taking its own. Lithuanian politicians did not have much faith in the viability of Sigma either, and there was insufficient political will and a lack of support to keep the enterprise going (Židonis interview)—but it has to be reminded that taking into account the overall situation, doing so would have been a most

43 Thus I do not want to create the impression that the reasons for the downfall of Sigma (or others such as RET or VEF, see below) were necessarily related to their inability to produce personal computers. However, my explanatory focus is on the domestic PC production attempts, not on the collapse of the industry, which would require a separate account.

difficult task. Additionally, other Lithuanian enterprises that had emerged outside Sigma had no interest in handing their contacts over to Sigma so that the organization could prosper.

Finally, the internal cohesion of Sigma was also compromised—this time by its own employees, who sensed that they could be better off by establishing their own small businesses with a competitive edge in some specialized niche and started to leave the organization. In the end, Sigma was simply unable to withstand all of these pressures. But further discussion of its collapse and its descendants would take us too far from the topic at hand. Therefore it is sensible to stop here and take a look at the case which managed, for a while at least, to thrive on the downfall of Soviet industry.

3.2.4 Lema and its PC/XT

Lema's story started, somewhat by accident, around 1985–1986. At that time there was a foreign PC/XT-clone in Vilnius State University. One academic with acquaintances at Sigma had heard that the organization had obtained another clone, known as Apricot. He wanted some data from Sigma's computer, but the floppy disk drives of the two machines supported different formats. Thus the computers had to be connected by serial cable. An unfortunate electrical failure occurred during which some components burned out. As foreign-made machines were highly valued, every possible means needed to be sought to repair them. Vidmantas Balčytis, an employee of the IoMC, started looking for solutions. Consulting the documentation that came with the university's computer, he soon found that most of the original components could be replaced with Soviet analogues, making the repair much cheaper than initially supposed. This discovery prompted the idea to start making PC/XTs using mainly Soviet components.

However, because some Western components would still be needed, foreign currency was also required. A suitable opportunity presented itself in spring or summer 1987, when the university managed to establish some contacts with a military institute in

Moscow.[44] The institute was looking for someone to duplicate a WANG computer using as few Western components as possible and agreed to provide foreign currency for parts that could not be replaced. Balčytis and Rimantas Kazlauskas were then allowed to travel to Moscow to examine the original WANG. Upon closer inspection they realized that its architecture was very similar to that of an IBM PC/XT. With IBM slowly establishing itself as a standard in the West, it was professionally more interesting to attempt a PC/XT clone instead. So while they formally started to work on a WANG design, the work was actually done with PC/XT in mind.

Once again, the exact copy was impossible to make. For one thing, the Soviet and Western standard distance between pins of the chips differed (2.50 mm vs. 2.54 mm). Also, the dimensions of some available chips (RAM and EPROM) were simply larger than those used in the original. In some cases, functional equivalents of certain chips not available in the USSR had to be devised. Differences in hardware led to minor changes in software (BIOS, testing procedures). The original components that could not be replaced or were difficult to replace included the Intel 8088 processor, DMA controller, timer controller and interrupt controller (Balčytis interview). In some cases even if Soviet analogues existed they were so hard to find that when the opportunity to use foreign currency was available it was easier to buy the components abroad.

The preliminary design was completed in 1988 (Balčytis interview). However, by that time, both the university and the military institute had lost interest in the project. The reason was quite simple: in the intervening period joint ventures between Soviet and Western companies had been allowed and some workers from the university had seized the chance to establish Baltic Amadeus.

The developers of the design, however, did not want to give the project up, since they already had a working model and diagnostic tools. They therefore started to explore ways to profit from the computer. Taking advantage of the loosening of the Soviet

44 Its official name could not be recalled by the interviewees.

regime in general and the law of cooperatives in particular, they acquired an approval from the Central Committee of Lithuanian Communist Union of Youth and established a cooperative called Lema at the beginning of 1989 (Kazlauskas interview). Being a small enterprise with around ten employees, Lema was mainly oriented to designing hardware, building prototypes and small-scale customized production. The advantages and disadvantages of cooperatives in the late USSR are well known: having to pay higher prices than state enterprises for some resources and relying on one's own supply channels vs. more freedom to choose and modify production goals, the possibility of paying higher wages and permission to convert foreign currency.

One of the ways to make profit from computer sales was to seek out people who had visited Western countries and brought back a PC. These were in high demand among various organizations. For legal reasons, however, it was difficult for state organizations to buy technology from private individuals. It was easier to buy from cooperatives like Lema. These could also provide a warranty and service to add value. The problem was that although the price of a Western PC was high—equivalent to that of a three-bedroom flat in Vilnius (Balčytis interview)—the profit from reselling one was not.

For Lema then, it would be more profitable to produce and sell its PC/XT clone. Since Lema had good relations with the Tauragė plant (part of the Sigma production union), PCBs were prepared there. Chips, bought from various sources, were manually soldered to the PCBs. The computers were then checked with various diagnostic tools Lema itself had prepared. In one day a single person could solder three boards and make one computer work (Balčytis interview). As possibilities to acquire new resources became available, the design was gradually improved (e.g. addition of a floppy disk drive controller, expanded use of Programmable Array Logic chips to imitate the functionality of some Western chips).

Despite Lema having working relations with Sigma, the latter never produced the computer. Sigma briefly evaluated the design (Židonis interview), but it was not deemed suitable for manufacturing. Many factors might have played its role here, e.g. lack of proper documentation, lack of consideration for all-union standards which mass-produced goods had to adhere to, use of foreign components etc. But Lema itself did not attempt to push its design into mass production too hard either—people at the enterprise perceived themselves mainly as designers, not manufacturers.

By contrast with the other cases, it is interesting to note that in Lema's case the quality of components was not seen as the most crucial issue: *"I insist on my opinion, that the main problem was design"* (Balčytis interview). It was indeed joked that the low quality of Soviet technology stemmed from engineers being unable to resist the temptation to improve the original design. On one hand this is consistent with Åslund's more general remark about the increase of value detraction down the Soviet production chain: *"Soviet raw materials were excellent, Soviet intermediary goods (such as metals and chemical) were shoddy, while consumer goods and processed foods were substandard"* (2002: 125). It is indeed likely that each new level of production allowed for additional errors and difficulties, which cumulated with an unreliable end product. It is also likely that a good product design could alleviate at least some problems presented by poor lower-level components.

On the other hand, the implication that most Soviet engineers were sub-par is very far-fetched, whether it refers to Sigma or the Institute of Cybernetics. At least in some cases making an exact functional copy with available Soviet technologies was an explicit demand of the authorities, leaving little room for improvisation (e.g. when RET was commanded to copy Sharp's HiFi set (Jelle interview)). In other cases, needed components were simply not available in the USSR (or were very scarce or very expensive): as a result the functionality could not be fully replicated (e.g. Poisk did not use any Western components and suffered from performance loss). It is also notable that quality problems were mentioned less often among small-scale producers. One could argue that in small-scale production a more customized approach to each unit was possible—if problems occurred, computers could be tailored individually until they worked properly. Mass production and standardization left less time to tinker with individual machines.

Finally, there is also the question how many components could affordably be discarded. Here Lema profited from change in the political climate. When conflict with Moscow worsened, large factories did not have orders to fill. Suddenly finding themselves overstocked with supplies, they were willing to sell to whoever could afford them. This enabled access to many items heretofore hardly or not at all available, including components intended for military purposes. Fixed prices in combination with increasing inflation meant that the components effectively got cheaper and cheaper as time passed—provided that one had foreign currency. Therefore Lema had increasingly easier and cheaper access to Soviet components. It could thus pick out the best ones and discard the rest: a degree of freedom of choice that would not have been possible even a few years earlier. The lion's share of the design's expenses was from the Western components (Balčytis interview).

Therefore Lema continued on the path of small-scale, customized production. According to the interviewees' estimates about 100 computers were bought mainly by factories and newspaper or broadcasting companies (Balčytis, Kazlauskas interviews). Later some of them also ended up as text buffering devices for teletype systems which were used to contact Russian enterprises. Somewhat similarly to Entel and the CCMC in Estonia (see 3.1.3), Lema was small, flexible and not strongly committed to its PC project. Therefore it could quietly phase it out in the first half of the 1990s when new and superior computers (e.g. IBM PC/AT) started to be diffused more widely and prices of foreign PC/XTs started approaching Lema's own. Lema simply moved on to more profitable projects.

3.3 Latvia[45]

3.3.1 VEFormika and VEF Mikro series

By contrast with Lithuania, where major industrial enterprises were created after the Second World War, Latvia was more similar to Estonia in that it had substantial pre-war production experience. Its VEF plant, started in 1919, was specialized for the production of various communications equipment such as telephones and radio receivers. Its profile was retained after Soviet occupation, when VEF became a renowned producer of various telegraph and telephone exchanges for civil and military purposes all over the USSR. The enterprise was expanded into a production union in 1979, centred in Riga but another plant in Stučka and additional production units elsewhere (Alūksne, Malta, Skrunda). Its prominence was reinforced in 1984 with the establishment of a research institute devoted to the design and development of the latest communications technology for the Soviet Union. In 1990 VEF had about 20,000 workers (Jērāns 1988: 721–722, Jubels 2009: 727–728, 775). Belonging to the Ministry of Communications Industry, it was a centrally controlled enterprise. Similarly to Sigma, its scale of operation and thinking, its connections, and its supply channels and resources were vastly superior to those of the lesser players whose struggles have been described above. Drawing an analogy with Western enterprises, VEF was to Tartu group what IBM was to Apple in the 1970s.

By the beginning of the 1970s VEF had started developing quasi-electronic automatic telephone exchanges (systems for connecting telephone calls). In that regard engineers, especially younger members of the profession, soon took an interest in the advances of integrated circuit technologies. This was not coincidental: having less prestige to lose and no particular direction of technological development to defend, younger workers were more willing to experiment (Ļenskis interview). In this respect the motive force was Mikhail Tovba, working in VEF's Special Construction Bureau, a sub-division employing about 1,500 people.

45 Owing to difficulties with finding interviewees, this section relies less on oral and more on various written sources. In this regard the help from Andrejs Skuja in locating various materials and providing translation has been truly invaluable.

Here VEF's good connections came into play. Possibly as early as the end of 1976 or the beginning of 1977, one of VEF's suppliers visited Kiev and brought back a microprocessor from a K580 series computer, a Soviet Intel 8080 analogue. Seeing the architecture of the processor, Tovba realized that it could be employed in the design of telephone exchanges. Work on the prototype started around February 1977 and was completed in a few months (Tovba interview), meaning that it may well be one of the earliest microcomputers in the Soviet Union (the first according to Tovba). The computer was named VEFormika. Tovba then approached Kiev with an idea to emulate the processor on a mainframe computer so that 60 programmers could start to develop and debug software for the system.

But upon his return from Kiev in May the project experienced an immediate setback. Tovba recalls the chief engineer of the Special Construction Bureau telling him that at best microprocessors could be used in refrigerators, washing machines and... billycans. The reason for this peculiar statement might be that the chief engineer himself was involved as an author in the patent of another control system. In any case the net outcome was that all chiefs of the divisions of the construction bureau were explicitly forbidden to use microprocessors. At this point Tovba resigned his position.

He did not leave VEF, however, but moved on to the Computing Centre of the factory, where the centre's chief enabled him to develop the computer further. Along with a few people who had left the construction bureau with him, Tovba hired students from Riga Polytechnical Institute to aid with further design efforts. Photos taken at the time indicate that the number of people involved was fewer than 15. The salary was minimal (just enough to get by), the room where the work was being done had no windows—nevertheless, in a paradoxical manner so characteristic of the Soviet Union, the marginal(ized) status of the project did not preclude its presentation in an exhibition in November 1977 celebrating the 60th anniversary of the USSR and devoted to the achievements of the Soviet electronics industry, visited by the General Secretary of the party Leonid Brezhnev himself. In yet another twist of irony Tovba himself had to wait in the hotel and was not allowed to visit the exhibition for security reasons. When he took a vacation to visit Kiev in July 1978 things did not look too promising though: despite the one-off exhibition the PC-building had met severe internal resistance in the factory and the production was nowhere near in sight.

His holiday was cut short, however, and he was instructed to return to Riga immediately. The deputy minister of Communications Industry had heard that VEF had built the first personal computer in the USSR, and so was paying a visit to the plant. The result was a true reversal of fortune: Tovba received a bonus, was given his own division with about 60 people and was tasked to develop an automated computer-aided design system (to be called Ekrāns) based on VEFormika. The work started in the autumn of 1978 (Tovba interview).

This was a prestigious task prioritized by the powers in Moscow. As such, the designers were granted extremely good access to various resources that could be put into the system. Let us compare: whereas most of the PCs described in table 3.1 did not have a hard disk even by the end of the 1980s, Ekrāns, built 10 years earlier, included two Bulgarian-produced hard drives, each with a capacity of 2.4 MB. Videoton displays came from Hungary, another sign of privileged supply (for comparison, recall the difficulties of the Santaka group in obtaining microprocessors from the GDR (see section 3.2.2). The early access granted to Kiev-produced microprocessors (initially probably the experimental versions) has already been mentioned.

With a special production shop for microcomputers being organized in February 1980 (Kolektīvais līgums—mūsu dzīves likums 1980.19.02) the Ekrāns system itself and the technical documentation were ready by autumn. According to the plant's newspaper it was the first microcomputer-based 'automated workplace' in the Soviet Union (Livšics 1984.31.08). In September the production of VEFormikas started

(Suslovs 1980.30.09) and by 1982 more than 500 had been produced (Kļimanova 1982.15.06). Tovba estimates that the total number produced might have been more than 1,000.

After VEF had produced the computers they were shipped to Vyshgorod, Ukraine, where the system as a whole was assembled and then allocated to enterprises all over the Soviet Union. Indeed there were news reports of VEFormika being used in over 40 cities in the USSR in 1982 (Kļimanova 1982.09.07) and of Ekrāns being installed in approximately 100 enterprises and institutes in 1984 (Livšics 1984.31.08). Yet in the very same year, complaints arose that Ekrāns was yet to be employed in VEF itself (Tehniskās pārkārtošanas galvenais uzdevums 1984.17.02). The production ran until roughly 1985 (Tovba interview). In 1987 around 600 enterprises were using the system (Mantojums 1987.10.03).

All in all, VEFormika was designed for industrial use and remained in such use, although the possibilities of using it for agricultural and home needs were briefly mentioned early on (Korneliuss 1980.01.12). Its main functionality was claimed to be data collection and processing, control of technological processes, automation of design processes and local networking. They were also seen as workstations (or 'automated workplaces') and 'intelligent terminals' ("Sakari-81" 1981.01.09, VEF 1983). The price—20,000 roubles (VEF 1983)—made it a very expensive device indeed (recall the 600-rouble BK-0010Š in the mid-1980s for comparison).

Although the factory newspaper mentions that VEFormika was modernized to some extent to expand the capabilities of Ekrāns (Suvorovs 1984.10.07), the basic architecture of the computer remained the same: some chips were replaced with newer versions, more memory added and additional software written. The reasons are familiar by now: *"Nobody wanted to deal with production, it was like a punishment"* (Tovba interview). It was relatively easy for people working at VEF to acquire new technologies for experimental purposes. But then one needed to test the components, write a request to the ministry, await approval, and await response from the enterprises (often saying that the components would not be currently available or available only in limited quantities). All in all, it was extremely difficult—or next to impossible—to guarantee that new components—and moreover, the variety of new components from different sources—would be available on time. And even if they did arrive there was no guarantee that the description would correspond to the content, e.g. the number of actual chips could be fewer than the quantity written on the box in which it came (Tovba interview). Thus the problem was not the lack of money, but the lack of ability to exchange money for actual products. At the same time the approval of allocation of resources still meant an obligation to produce a given amount, and the failure to do so could have brought serious sanctions for the factory. To this one should add the time required to compile the necessary documentation with appropriate parameters that needed to correspond to the central standards (GOST), to do the checks to ensure that correspondence to these standards was indeed achieved and to prepare for production. Therefore from the managerial point of view it was often safer to avoid difficult projects altogether (Ļenskis interview). From the engineering point of view it was easier to proceed incrementally. This would explain why VEFormika continued to be slightly modified until the mid-1980s and became gradually outdated with the emergence of the 16-bit processor standard.

Tovba recalls that the information about VEFormika was given to the plant's Technical Research Department (TRD), a sub-division of roughly 400 people responsible for optimizing and developing VEF's internal production processes. However, TRD had already independently started to become interested in integrated circuits a few years earlier. In 1969, VEF constructed a relay-based scoreboard for a newly built sports arena in Riga. In 1974 the same construction was used in a Universiade in Moscow, where it caught the interest of representatives from Longines and Omega (Červinskis interview).[46] The fact that these enterprises liked the appearance of the scoreboard very much, but vainly searched for integrated circuits inside it, suggested the constructors that ICs could be a promising

46 All three interviews with Jurijs Červinskis were conducted by Andrejs Skuja.

technological domain worth looking into.

The exact construction year of TRD's first microcomputer is unknown. In 1978 the head of TRD, Pēteris Videnieks, commented about the first microcomputer being built at VEF (Preses konference rūpnīcā 1978.17.02), but the specific model was not mentioned (hence it could have been VEFormika). Nor does VEF's book on microprocessor-based control systems (Videnieks 1981) mention the year of TRD's first microcomputer which, however, already includes references to different types of computers. The first explicit mention of VEF Mikro 1021 and 1022 comes from 1982, when they are claimed to have already been in use (Ciesalnieks 1982.10.08). However, some experimental batches of microcomputers with other names (VEF-Sports, VEF-Vita, up to 20 machines) had already been built by the end of the 1970s and the name VEF Mikro was allegedly in use in 1981 (Červinskis interview).

Similarly to VEFormika, VEF Mikro was first and foremost intended for industrial use—more specifically, to manage a plant's internal production processes. Its uses included monitoring the workers' output, quality control of products, automatic testing of the parameters of radio receivers, checking the circuits of quasi-electronic telephone exchanges (with two versions of systems called Kontests), controlling devices for assembling electronic circuits, controlling drilling machines (for PCBs) and creation of hardware and software (using systems MKS-802 and MKS-803, respectively) (Videnieks 1981, VEF 1983, Ventiņš & Skorinko 1988.17.05, Ventiņš 1988). More exotic uses include the real-time sports information system Gimnasts-2, built for managing the gymnastics competition in the Moscow Olympics in 1980 (actually based on the predecessors of VEF Mikro (Červinskis interview)) and the plant's own automated canteen service (Pudāns 1983.30.12). Depending on the application different peripherals could be connected, e.g. perforator, printer, floppy disk drive, display, devices for programming read-only memory etc. (VEF 1983, Videnieks *et al.* 1987).

Figure 3.7. VEF Mikro 1021 (Pēteris Videnieks's private collection)

Figure 3.8. VEF Mikro 1025 (photo taken by author)

The initial versions, VEF Mikro 1021 and 1022, were actually developed by two different teams in TRD, led by Jurijs Červinskis and Jānis Ventiņš, respectively. So were the follow-up models, 1024 and 1025, built some time around 1983 ("Automatizācija-83" 1983.01.07, VEF 1983, Ventiņš & Skorinko 1988.17.05, Ventiņš 1988, Červinskis interview). However, the comparison of their basic characteristics (see table 3.1) seems to indicate little differences between them: the processors, RAM, ROM, display and basic software seem to be roughly the same (e.g. Intel Soviet 8080 copy in 1021/1022 and 8080A in 1024/1025). The only notable difference is that at least one version of VEF Mikro 1025 was said to use three microprocessors: one as a central unit, one for the video terminal and one for external memory (Ventiņš & Skorinko 1988.17.05, Ventiņš 1988).

The reasons why the models were many but the changes few are unknown. However, the structure of the Soviet system might be assumed to account for this. On one hand, as mentioned above, upgrading the design radically was a time-consuming and highly uncertain process, perceived as increasingly pointless as the Soviet technological lag kept increasing and Western equivalents could be acquired more and more easily. Therefore the design process was likely to be incremental. On the other hand, inventive efforts were still rewarded by the central authorities. Therefore it might have simply been a good rhetorical move to create different labels for essentially similar products and production upgrades.

Although in principle all VEF Mikros could be used as standalone devices, in practice they were always integrated into various systems (Videnieks interview). Save for a few exceptions, such as giving about 20–25 computers to a scientific institute in Chelyabinsk (Červinskis interview), most of them were used for plant-internal purposes only. The reasons are once again familiar: that way it was easier to avoid bureaucracy, to avoid becoming entangled with the state plan, with its regulations, standards, documentation, obligations and often failing promises. The price for speed, however, was to remain local and unofficial.

The VEF Mikro 1025 at least was still being produced in 1986 (Bramņika 1986.10.10), and it is likely that the actual production lasted even longer, until the late 1980s (Krivchenkov, Videnieks interviews). Approximately 200 1021s, 100 1022s and around 600–1,000 1024s and 1025s (taken together) were eventually built (Krivchenkov's estimate). Owing to the aforementioned factors, the computers were used until needed and only slightly upgraded—whenever possible, they were increasingly substituted with foreign machines: first Robotrons from East Germany and later PC/XTs or ATs from Taiwan (Ļenskis interview). Similar to the other cases described, (foreign) substitution (where possible) was preferred to a new round of the product cycle. If anything, VEF's position enabled this process to start even earlier than it did elsewhere.

At this point two questions emerge. First, if VEF was really one of the earliest in the Soviet Union to get into microcomputing, then why did it not become a major producer alongside Minradioprom and Minelektronmprom? Second, how did VEF's experience with microcomputing manifest itself in relation to the (local) school computerization initiative? The focus of this dissertation precludes me from discussing the first question. One can assume that the answer is to be found in the nature of central control, the rigidity of changing the production focuses of various plants plus fierce inter-ministerial rivalry between Minelektronprom and Minradioprom about computer production. The second, however, requires closer consideration of the developments in the Latvian education sector.

3.3.2 Latvian response to the school computerization initiative

Compared with other Baltic states, Latvian PC production started very early indeed. To a lesser extent this was also true for school computerization. The take-up of the latter initiative can be traced back to Ilmārs Vītols, the founder of the Institute of Solid State Physics in the Latvian State University (LSU) and an enthusiastic supporter of computer-related activities. Around 1983–1984, he wrote a letter to the central powers stressing the need for school computerization and educational software, and insisting that a corresponding organization governing these processes should be established in Riga (Eglājs interview). Matters might have been helped further by the fact that some Latvian scientists had close ties to Andrey Ershov, a key figure in Soviet school computerization (Vītiņš interview).

Vītols's letter seems to have coincided with the central decision to start preparing for the teaching of informatics in schools. As a result, the establishment of a Laboratory of the Problems of School Informatics (LPSI) at the Computing Centre of Latvian State University was approved. LPSI was to be responsible for preparing for the teaching of informatics in schools and for developing corresponding materials for the whole Soviet Union. It was one of only six such laboratories in the USSR (LPSI 1986), with a planned staff of 25 people. In 1985 it could command 120,000 roubles for technical devices and another 50,000 for salaries (order from LSU's rector 1984.07.12).

Modris Eglājs was appointed head of the laboratory. Having previously done research in the field of nuclear physics, he was somewhat surprised to hear that the university had decided he would be the best man for the job and that the Central Committee had already approved his suitability for the task. Nevertheless it was a prestigious opportunity which left much room for improvisation, as it was initially far from clear what exactly such a centre would be expected to do (Eglājs interview). Therefore the first tasks were very practical: find the space, find the people and find the technology. Luckily a military complex had just been vacated in the centre of Riga—on the other hand, they had left it in a dreadful condition. Eglājs insisted on full renovation including new furniture and parquet flooring, a luxury item at the time. When he was initially denied the request by the university he threatened to quit the job. But as he had already been approved by the Central Committee this would have compromised the university, potentially resulting in sanctions. Therefore using the central support as leverage it was possible to equip the laboratory according to the highest standards of the time. This, of course, also went for people and technology. When it came to the former it was necessary to avoid giving in to the informal pressure to employ the children of high-ranked officials instead of actual specialists. When it came to the latter, such recommendations as *"*[in the] *first year you can work with calculators"* needed to be overcome (Eglājs interview).

Stressing that the proper technical equipment was lacking, that the task was important, that there was a need to hurry and consequently that there was a need to adapt Western software, the laboratory soon started to search for opportunities to obtain various computers. Symptomatic of the level of scarcity is the request of the Latvian Planning Committee to the respective central organization and Minradioprom for the allocation of just two Agat computers to the laboratory (Latvian State Planning Committee 1984.28.08), or the request of the local Ministry of Higher and Vocational Education to the Latvian Planning Committee to receive 15,000 dollars to buy an original Apple III computer (1984.17.09). In other words, these struggles for acquisition were experienced by the very same laboratory which was supposed to develop solutions for school computerization in the first place.

The Sistematronika-84 exhibition in November turned out to be a true eye-opener. It was here that people from LPSI first saw the Acorn BBC be presented (Ministry of Higher and Vocational Education 1984.04.12). This British design, running on Apple's MOS 6502 1.8 MHz processor could boast a variety of attractive features: colour graphics, games, excellent sound, lots of educational software and a working local area network which students could use to share a common drive: an unseen feature in the Soviet Union at the time. Moreover, the British representatives were willing to sell. After a series of negotiations about the exact terms between the local and the central authorities (e.g. Ministry of Higher and Vocational Education 1984.04.12, letter from the deputy chairman of Latvian State Planning Committee 1984.05.12), a contract between 3SL Overseas Ltd and Elektronorgtehnika (Электроноргтехника, official buyer) was reached by the beginning of 1985 (contract from 1985.31.01). The sum allocated for hardware and software was close to £40,000 (GBP), a huge amount of money at the time. As a result LPSI received a classroom set with one teacher's computer and 15 working places for students, a local area network solution, a wide variety of software, courses for teaching and training from the company—the full list extends to 11 pages. By June 1985 the deal was concluded (protocol from 1985.17.06).

For a while this computer classroom set became a kind of a trophy, presented to various visitors from Latvia and elsewhere. LPSI itself started to advocate the wider use and possible mass purchase of Acorn BBCs for the USSR and was initially successful, as laboratories from Moscow and Kiev also took interest. However, at some point, allegedly due to more successful lobbying (Eglājs interview), Yamaha computers gained more support instead. And when the latter were finally bought they were far too few to cover the needs of the whole Soviet Union: for example, in 1986 Estonia received 6 Yamaha classroom sets, each with 15 working places (Jürisson 1995).

What about domestic production? Like Estonia and Lithuania, Latvia is a small country, but contrary to the others most key activities were concentrated in only one city. Therefore LPSI was well aware of the developments in the industrial sector. For example, the need to look into the possibilities of using VEF's production for school use had been stressed by the local Planning Committee as well as the laboratory itself (Latvian State Planning Committee 1984.28.08, LPSI 1984). Later mention is made of cooperating with VEF, Komutators and Radiotehnika plants, wherein the designs of the first two are deemed most promising (LPSI 1986). Thus it seems that for some time LPSI was also pursuing multiple options at the local level. Yet no domestic design eventually prevailed: instead it was the familiar BK-0010 that diffused most widely. Why?

At least part of the answer is to be found in LPSI's own preferences and choices. The laboratory's pragmatic approach to school informatics was well expressed by its former employee: *"To proceed in such a manner that we could likely outdo others and where we would be noticed"* (Vītiņš interview). It seems that the formal role of the laboratory matched quite well with the interests of the workers and thus LPSI was first and foremost oriented to contributing to developments on the level of the

USSR as a whole. In so doing it aimed to find a suitable niche where it would have the edge. For example, it abstained from writing textbooks because the competition was high and it would have been unlikely that a Latvian book would be preferred to ones written by Ershov and other leading figures of Soviet school computerization. Compiling various exercises and other methodical materials for existing textbooks was a niche much less occupied (Vītiņš interview).

The programming and reprogramming (adaptation) of software (educational programs plus some more basic software, e.g. local network for BK-0010s) was another such niche. *"At this time it was* [the] *general belief that hardware is everything—the software is creating itself somehow and it is not a problem"* (Eglājs interview). Software and supplementary teaching materials thus became the main directions of LPSI, with the aim to acquire as much knowledge about the practical use of PCs elsewhere and apply it in Latvia using any computers that could be obtained, whatever the source (Eglājs interview). *"I think that neither us in the laboratory nor the ones in the ministry were afraid that the technology will not come"* (Vītiņš interview).

This would explain why LPSI was indeed probing different possibilities, including some hobbyist proposals, but not pursuing any of them very intensively. Bad or lacking graphics were one of the reasons for rejecting some Intel-based clones (Eglājs interview). The stress on software therefore meant having to rely on the scarce central provision of different types of PCs. The problem of scarcity was overcome by taking the attitude that even one computer per school was better than nothing. The problem of adaptation simply meant more work for programmers, a task LPSI thought itself capable of handling.

Of course, one should not place the sole responsibility on LPSI. From the other side there were well-known issues with the Soviet system: large local industrial enterprises were centrally controlled and there was virtually nothing except persuasion local authorities (not to mention the universities or the LPSI) could do to shape their attitudes. The fact that the factories were quite occupied with their main, 'serious' production also meant they had little incentive to engage in local matters. As a result *“nobody in Latvia was pushing very hard for local school computers”* (Eglājs interview). Lacking a dedicated local leader, the support for a single domestic project also failed to emerge.

3.3.3 VEF and others: experiments from the mid-1980s and onwards

Having discussed VEFormika, VEF Mikro series and the choices of the education sector I will now turn my attention to other cases of which little is known at the moment. As such, the outline in this section will have to be somewhat sketchier than in the previous sections.

Although the VEF Mikro series was not introduced to Latvian schools, it may well be that the school informatics initiative, possibly owing to some communication between the local education sector and members of the factory, did have some impact on VEF's plans after all. Otherwise it would be hard to explain why, in 1984, VEF's Computing Centre suddenly started developing a device called the 'educational microprocessor kit', EMK. It essentially consisted of a small set of modules that could be arranged in various ways, e.g. to assemble a working microprocessor. It had limited programming capabilities, a small keyboard for input and a six-digit display (figure 3.9). It was planned to be enhanced with additional modules so that it could be connected to another kit, used as a voltmeter or a music player. It was envisioned that eventually it could have the full capabilities of a personal computer (Suvorovs 1984.10.07, Bramņika 1987.11.08, Maigeļdinova 1987.27.10).

In a way this is a prime example of a hardware-oriented approach, because the production of such a kit relied heavily on the assumption that in order to use PCs effectively one would need a detailed understanding of the working principles of the machine. For VEF, however, this made good sense because it could draw on its expertise in microcomputing while avoiding the troublesome question of acquiring peripherals (Ļenskis interview). In fact, at least for a while EMK seems to have been advocated quite vehemently, not only for schools but also for decreasing the training times for various technical specialists without having to buy more expensive devices (Vitze 1985.18.01).

Figure 3.9. Educational microprocessor kit (photo taken by the author)

Initially the endeavour proved successful. Production began in December 1986, two years after the work on the design itself had started—allegedly VEF's quickest project. It was also said to be the cheapest of its kind, with a price of 700 roubles (Lifšica 1987.10.07, Bramņika 1987.11.08). And the orders were coming in: 600 a month were produced in 1987, but the lack of a sufficient number of workers and requested components (again) were cited as major problems (Lifšica 1987.10.07). One of the workers from the Computing Centre recalled visiting Moscow and receiving an order for around 10,000 EMKs (Ļenskis interview). The planned production for 1987 was 10,000 kits, but the alleged demand for 1987–1988 was 35,000. It is notable that even talks with the Indian government were started regarding export of the kits (Lifšics 1987.15.12, Rūmniece 1988.16.02).

However, just as the rise of the EMK was meteoritic, so was its downfall. A warning sign came from a computer exhibition in Riga in 1988 when the visitors complimented the kit, demanded quicker production(!), but also noted that the device had arrived on the scene a few years too late (Zaicevs 1988.25.03). A year later the project was already called a costly mistake in the plant's own newspaper and production was stopped (Maigeļdinova 1989.03.10).

The newspaper article cites the sharp drop in demand as the main reason, following on from the fact that many other plants had started to produce similar devices for cheaper prices. However, one must also be reminded that some of the actual PCs—albeit without peripherals—were also in the EMK's price range. In that context it is quite likely that the ready-to-use sales pitch of EMK did not prove very convincing: its miniature keyboard and display, along with the promises of future expansions, could not make up for the fact that it was not a fully functional PC, by contrast with the likes of the BK-0010 (once the TV and the tape recorder had been connected of course). And although slowly, computers from the USSR and abroad were making their way into various domains. As the EMK could only be considered a temporarily satisfactory replacement for personal computers, it is safe to assume that the market for such devices was drying up rapidly anyway. Perhaps the fact that the project was undertaken at all once again illustrates the unforeseen trajectory and rapidity of the events unfolding in the second half of the 1980s.

In previous sections I have described the convergence of a set of issues—the rigid inter-ministerial division of labour, the central control of VEF and its formal detachment from the influence of local authorities, the difficulties with starting mass production and renewing designs, and the lack of a local leader lobbying actively on multiple levels—that contributed to VEF's computer production remaining largely factory-internal and advancing incrementally. The circle was broken by the economic reforms, after which the requirement of self-financing of factories provided an incentive to develop desirable and profitable consumer items—with VEF-Prīma emerging as a result (Prohorova 1988.23.12).

Developed in cooperation between VEF's Computing Centre and TRD, little is known about the PC. Its features included 160 KB of RAM, colour graphics capability, two joysticks, more than 50 accompanying programs, and the potential to connect a tape recorder, a floppy disk drive and a printer (Prohorova 1988.23.12). Tellingly the 800-rouble machine was claimed to surpass 'a number of personal computers' produced in the USSR (Prohorova 1988.03.01). Developed in a year, production was supposed to start in July 1989. However, allegedly only a prototype was built (Červinskis interview). According to Andrejs Skuja's personal recollections the public demonstration was announced, but never took place. The reason? Jurijs Červinskis, the designer came into conflict with his superiors, became badly offended and subsequently cancelled the project (Červinskis interview). He later went on to cooperate briefly with the plant of hydrometeorological devices, which attempted to produce a Sinclair Z80 clone designed in Leningrad. The experimental batch of around 400 computers was built around 1989. Because of faulty chips, however, the project was soon halted.

Even less is known about the Komutators factory and its production. Allegedly it was heavily involved in producing specialized electronic devices for military needs that had such strategic importance that by the time the Soviet Union collapsed most of the technologies had already been removed (information provided by Andrejs Skuja). Komutators was also one of the plants to which the technical documentation of Juku was sent (Kashin 1987.30.05, see also section 3.1.1). The reasons why the plant was not interested in producing Juku cannot be stated for certain, but LPSI's report from 1986 allows for an educated guess. Namely, while mentioning the designs of VEF and Komutators as the most promising, mention is also made about the ease with which the software of Yamaha computers could therefore be adopted. As VEF's computers were Intel-based, this suggests that Komutators might have been attempting to clone Yamahas. Having their own design already would also explain why Komutators was not interested in Juku.

Yet another attempt can be traced back to Latvian scientific institutes, namely the Institute of Solid State Physics and the Institute of Polymer Mechanics. Here the prior experience with microprocessor technologies, the lack of computers and the release of the 8-bit Soviet computer Irisha (Ириша) provided a sufficient incentive for two friends to start developing a design called Skudra (meaning 'ant' in Latvian). The initial goal was to to build a computer for personal needs while surpassing Irisha's design in elegance. Constructed after working hours, the prototype, with 16 KB ROM, 128 KB RAM and a tape recorder as an external memory device, was ready by 1985. CP/M was used as an operating system, but the software needed some adaptation to be run on Skudra (Žuks interview).

The news soon reached the ears of Ilmārs Vītols, the previously mentioned founder of the Institute of Solid State Physics and the person who had played a crucial role in the establishment of LPSI. Seeing the simplicity of the design and realizing that it was constructed from relatively accessible components (Špungins 1988.20.02)—likely out of necessity rather than choice from the point of view of the constructors—Vītols thought it would be suitable for mass production. As he knew the director of the local Radiotehnika plant he offered the design for the production union.

Radiotehnika, an enterprise mainly focused on the production of radios and other audio equipment, was initially interested in the proposal. Therefore the Skudra team proceeded to prepare three prototypes. An experimental batch of ten computers with floppy disk drives was built by 1987–1988 (Žuks interview). Skudra was also present at the computer exhibition in Riga in 1988, with mass production being planned for 1989 (Spila 1988.22.01). By that time the situation had changed, however. The growing opportunities to buy computers elsewhere, with high-end users already able to afford foreign-made computers (IBM PCs) and the hobbyist community enthusiastically adopting Sinclair clones with their ubiquitous software, little room was left for Skudra. Hence both the designer team and Radiotehnika itself lost interest in the project. In broad strokes the story is then quite similar to many others—a good-enough idea at the time simply taking too much time to realize, largely because of the tardiness of the Soviet production system and unforeseen rapid developments in the environment.

In parallel, Radiotehnika was also briefly pursuing the possibility of producing BK-0010 computers. In 1986 a Special Constructor Bureau dedicated to the development and production of 'semiconductor microcircuits' was established (Unisonā ar laiku 1986.12.08). Some trial batches were produced in 1987 (Ļisicina 1988.01.02, Sadzīves kompjūtera BK-0010 1988.15.03), but little else is known. It is very likely that the causes for abandonment were similar to Skudra's: BK-0010 was becoming more and more outdated, it was a side-project anyway and at that stage it was simply easier to discontinue the project.

In the end it seems that the fate of local PC production was very similar to that in Estonia and Lithuania. Existing attempts were gradually halted in light of increasing possibilities of substitution, starting from high-end users in roughly the second half of the 1980s and proceeding to lay users by the beginning of the 1990s. Moves towards independence, growing political animosity, then the loss of existing supply chains and a lack of Western contacts raised a number of existential questions—for example, the debates in VEF's newspaper *Vefietis* seemed to move from the questions of how to produce to what to produce to whether to produce at all as the 1990s approached. At the beginning of the 1990s, large industrial enterprises went through major reorganization, were privatized part by part or were closed down altogether. As none of them were specialized for computer production anyway, this was probably not considered a serious option in independent Latvia. As in the other Soviet countries, Latvian domestic PC production dissipated in the face of Western computers.

4. Middle-range analysis

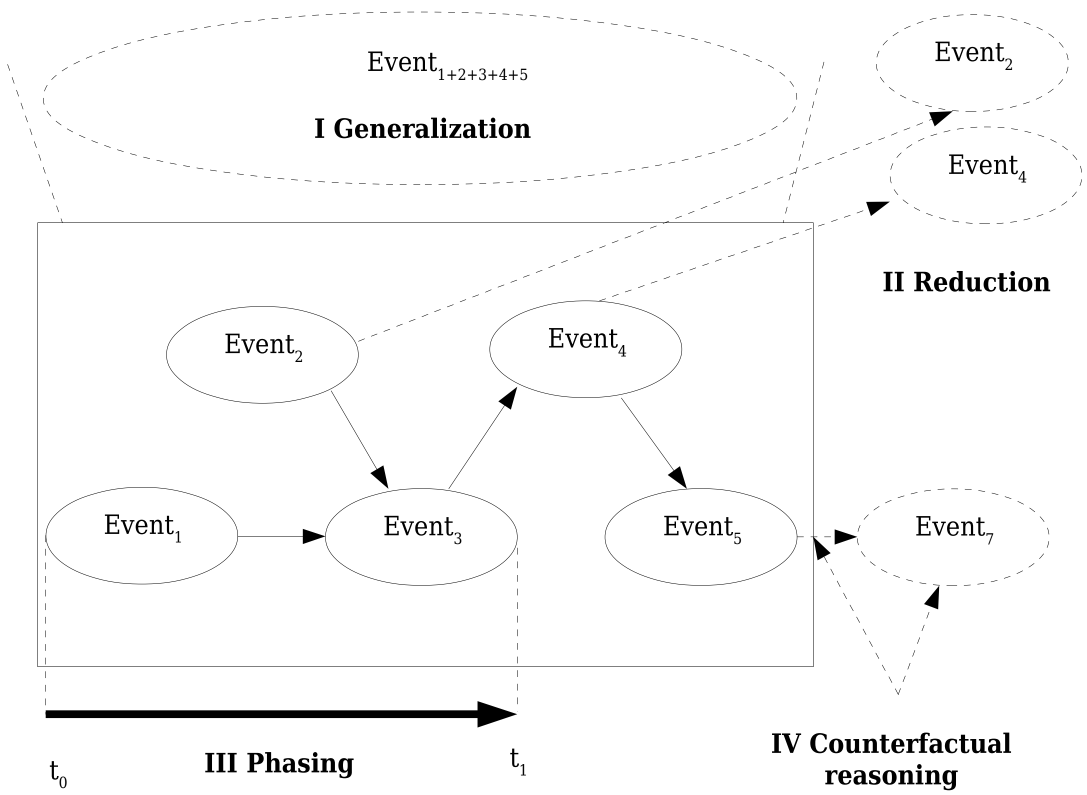
In the introduction the thesis was depicted as a U-shape, proceeding from the highest levels of abstraction to specific narratives and back. The lowest point of the curve, and so the highest amount of detail was reached in the previous chapter. In this chapter the direction is reversed. Based on the histories presented in chapter 3 various reductions, simplifications and generalizations will be made.

It seems that compared with various techniques of data collection and analysis, the process of theorizing from data has received relatively scant attention. Therefore I will first make a little detour into the nature of theorizing and try to find answers to two questions: what actions are performed when one is said to 'theorize' from historical processes? What are the differences between the potential outcomes of these actions and correspondingly what different types of 'theory' could there be? I will then draw briefly on what I believe are the strengths and weaknesses of STS and grounded theory to offer a simple technique for approaching the analysis of historical narratives while avoiding complexity overload at the same time. For illustrative purposes this technique will then be applied to intra-case analysis in detail. Inter-case and system-level analyses end the chapter.

4.1 From narrative to theory: what is involved and how to do it?

Once the initial materials have been worked through and narratives distilled from them, how should one proceed? Relying on the assumption that further theorizing on the basis of historical narratives is a desirable goal, how does one begin generalizing from such a wealth of data? How does one pick out patterns from the flow of continuous interaction of various entities and causes operating on various levels? I propose that there are four basic ways of doing so. These commonly—although I would hazard a guess that usually intuitively—used strategies are: 1) generalization; 2) reduction; 3) phasing; and 4) counterfactual reasoning.

Figure 4.1. Four ways of simplifying the complexity



In the theory chapter the term 'ladder of abstraction' was used to point out that in order to group certain entities or events one needs to reduce the number of properties that count. In other words, as soon as we start to classify some aspects of the narrative we are already performing generalizations. Taking the standardization of a technological product as an example, let’s assume that a researcher has compared two cases. In so doing she or he has found that in one instance the standard emerged in a producer–consumer feedback loop whereby some commercially most successful products gradually became standards. In another case the government wanted to speed up the process, intervened and imposed its own standards, which the enterprises subsequently had to adhere to. On one level of generality we seem to have two completely contrasting explanations: bottom-up vs. top-down pathways of standardization. However, if the research was to adopt the SCOT framework (e.g. Pinch & Bijker 1984, 1987) a commonality can be noted: namely, in both cases there were certain groups attributing meanings to technological products, by which process closure (standardization) finally occurred. In other words, on this level of generality the explanations would not differ at all. By generalizing, the cases were rendered equal in content.

In theory it might be possible to generalize all elements of the narrative without excluding anything. However, usually a selection between essential and non-essential elements is being made. That is to say, the explanation of the case always entails a mix of particular unique causes and more recurrent ones. Once again, to bring a hypothetical example: say, an organization obtains a premium from a government for a successfully implemented project. A day later the happy project manager slips on the ice and breaks her leg. Being forced to stay at the hospital for some time she unexpectedly has some free time on her hands. Reading the newspaper she notices an article about computers being required for educational needs and suddenly comes up with an idea of using the available organizational funds to develop a cheap but efficient school PC. She then convinces her partners and thus the prototype is built. Now, when government funding, expressed social need and a working prototype might be considered general conditions of success, slipping on ice and reading a newspaper are not. Moreover, it is quite likely that the idea could have been born anyway (e.g. someone else might have also read the paper and proposed it in a meeting). Although all of these causes did contribute to the outcome it is probably a sensible strategy to discard the final two and build a model around the potentially more general ones. Eliminating causes deemed superfluous, random and one-off defines a reductive strategy.

A particular combination of reduction and generalization constitutes the third strategy concerning the timeframe of the narrative. Here one first selects the events of interest (reduction), leaving some aside, and then groups them by a common denominator (thus, following Sartori's (1970) definition (see section 1.1), performing a generalization). For example, one might divide the development of a PC into a number of product cycles, each characterizing one model of the computer. The length of these cycles may vary but the class of events they belong to are exactly the same and therefore comparable.

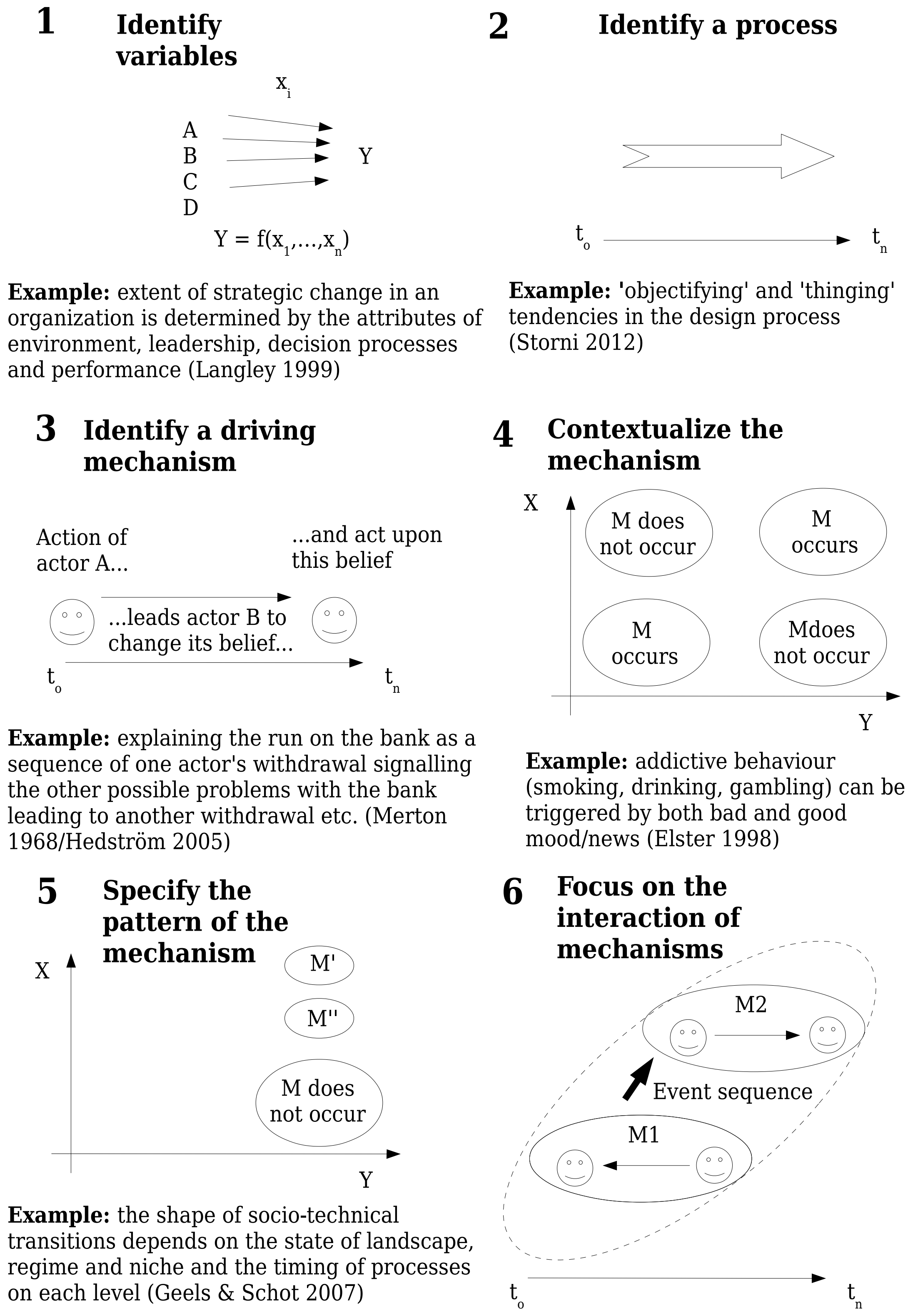
And finally, there is always an option not only to focus on the immediately observable, but also to ask: *"How* should *it be on logical grounds?"* (Taagepera 2008: 5). In other words, relying on prior research experience, reading and theoretical imagination one can ask whether something is missing from the picture, whether some elements are confounding the underlying mechanism or whether the presence of such factors means that in the observed cases the mechanism has been realized only partially, but could have manifested itself fully under different conditions.

Of course, these strategies are closely interrelated in practice. That is, arriving at stylized models often involves removing causes, classifying them, drawing generalizations, identifying certain episodes, comparing the observed to the expected and shifting between them all. For example, one might research the involvement of the education sector in PC development and find that in all observed cases it demanded a domestic computer. However, in one case it also provided funds for the development of the prototype. Once again, this particular occurrence can be deemed a unique cause to be dropped from the general explanation. However, it can also be subsumed under the category 'initiative from the education sector', by which process it re-enters the explanation. Moreover, in the actual research process the distinction between unique and general causes is far from clear-cut. The understanding might emerge during the research or, worse, when the results are compared with the wider population of cases (of which there might or might not be a good selection depending on the state of the field). This makes theorizing from process data a complex craft demanding that attention is paid to various aspects simultaneously, as is well summarized by Poole and colleagues: *"Typologies of sequences should enable the researcher to recognize resemblance among patterns that differ in length, exhibit different degrees of overlap among contiguous events, and exhibit "noise" in the form of nonessential events which complicate the sequence"* (2000: 44).

But what is the significance of all these strategies? To my mind the above discussion serves to sensitize researchers to the ways in which their analytical choices define the theoretical population that a case represents. This of course turns the usual demand that a case should be selected according to its theoretical relevance on its head. I am arguing instead that in the presence of sufficiently rich historical data—which case data usually is—the theoretical relevance often depends on the way in which the narratives are analysed. Data can lead to theory selection, not always and necessarily the other way round. And this does not depend on whether one aims to construct or test a theory. Therefore, for data-driven research, defining the class of events *a priori* in terms of belonging to 'Soviet innovation in the field of microcomputing', 'Soviet innovation' or 'innovation in microcomputing' simply might not get one very far. The same goes for the classification of cases as 'typical', 'divergent', 'critical', 'most similar', 'most different' etc. As this interpretative flexibility is a property of most qualitative data, I think that the accusation that a piece of research is (initially) data-driven is actually less of an issue in qualitative research than it might seem.

However, this is not to deny that once the analytical choices have been made the cases do start to represent a certain part of the overall population: in other words, they act as empirical instances of particular theoretical claims. Nor do I intend to claim that the results of each analysis would automatically constitute a relevant theoretical contribution—because of poor selection decisions the outcome may well end up duplicating already existing knowledge. After all, it is not the various practices of theorizing that are being assessed, but the outcomes themselves. And in that respect one can distinguish between six possibilities, each of which could be considered a 'theory' of some kind (see figure 4.2). In the following section I will briefly describe each of these possible outcomes.

Figure 4.2. Possible outcomes of theorizing from narratives (part 1 adapted from Langley 1999)



The first possible strategy would be to detect some variables that make a difference to a certain specified outcome. It is a powerful strategy, especially if the relevance of these variables is tested on a large sample. However, as the sole outcome of a single case study it is usually unsatisfactory because it quickly sacrifices all the internal dynamics of the case. An example would be Bijker's (1995: 123) notion of technological frame, which essentially unites widely differing elements (e.g. key problems, testing procedures, tacit knowledge) that influence the attribution of meanings to an artefact in the process of technological development.

The second outcome abstracts a general direction, process or tendency from the narrative. In a good case a few parallel processes or phases can be identified. An example is provided by Storni (2012), who highlights two alternating movements in the design process in which the emergence of a certain design trajectory can be understood as orderly practices coming to dominate over unexpected and surprising movements (objectifying vs. 'thinging' tendencies).

In the third case one could enquire deeper and offer a generative mechanism responsible for the observed outcome (e.g. an event like a bank run). The difference between this and the previous outcome is that the identification of a process is essentially a description of the outcome, while the addition of a generative mechanism enables an explanation of *"why we observe what we observe"* (Hedström & Bearman 2009b: 9). For example, the observation of the sequence of certain phases constituting the lifecycle of large technical systems (Hughes 1987) does not in itself explain what sustains these phases and what enables shifts from one phase to another, whereas the conceptualization of a bank run in terms of 1) the actions of one individual; 2) leading to changes in the beliefs of another; which in turn leads to 3) subsequent actions, could be considered explanatory.

The fourth outcome takes this reasoning one step further by uniting a proposed mechanism with contextual factors (in principle, outcomes 3 and 1). Here an attempt is made to specify the conditions in which some mechanisms occur, those in which they do not occur and where the tipping points are. Elster (1998) uses an example of a study conducted by Tversky and Shafir (1992): in a series of experiments it was found that people tend to accept gambles when they know whether they have won or lost the previous one, but usually reject them when the previous outcome is unknown. One could interpret the amount of information available to the gambler as a contextual factor affecting whether the mechanism of finding excuses for what one wants to do anyway actualizes or not.

Implicit in the previous two outcomes is the assumption that the realization of the mechanism remains largely the same in each case. The fifth outcome challenges this belief by turning attention not only to the differences between the properties of the context and the actors responsible for realizing the mechanism, but also the timing and duration of local and contextual processes. For example, Geels and Schot (2007) have shown that when a relatively rapid landscape pressure opens up the regime, quick substitution only follows when a niche has sufficiently matured. If not, competition between various niches takes place until one of them emerges as dominant. Conversely, if the landscape pressure does not unfold that quickly, the regime has enough time for adaptation and the transformation is more gradual. The constituents of the analysis (niche, regime, landscape), the event to be explained (socio-technical transition) and the basic process (niche–regime dynamics) remain the same, but the pattern is different in each case.

The sixth outcome introduces even more complexity: here one could focus on the internal dynamics of the constituents of the original mechanism. For example, whereas the theory of socio-technical transitions focuses on the interaction of the outcomes of niche-internal and regime-internal processes, it is also possible to disaggregate both into their respective subcomponents and the interactions between them (e.g. as Raven and Geels (2010) have done in the case of niche formation). Hypothetically, the overall transition could now be explained in terms of the interactions of smaller units of analysis, although in practice the complexity of the analysis increases considerably (which likely explains why I have failed to find such a work). Alternatively, if one has identified multiple mechanisms one could focus on how their interaction in time makes up the aggregate event sequence to be explained (see Gambetta (1998) for three examples). If such an interaction itself can be deemed a meta-mechanism the circle is complete: one has essentially identified a higher-level mechanism (outcome 3) and another round of theoretical specification can follow.

Note that figure 4.2 was not meant to imply that some of those outcomes would be inherently better or worse than others: as Edmondson and McManus (2007) have argued, it depends heavily on the state of prior knowledge. For example, in the early stages of theoretical development, finding concepts, variables or general tendencies can indeed be very stimulating, whereas in the mature phase rigorous quantitative testing can often yield better results. However, when it comes to many case studies in STS I frequently get the feeling that the data would have enabled many more insights beyond the identification of a few concepts and relations between them. Outcomes 3–6 are rarely found, meaning that the cases remain undertheorized. This, in turn, hampers the theoretical cumulativity of STS. I believe that usually this does not need to be the case, however, and with a sufficiently rigorous approach the narratives offer ample possibilities for more nuanced theorizing.

But in that regard there is yet another issue: namely, when reading STS case studies one often notices that the journey from data to theory remains opaque. This goes for both 'classic' studies, such as Pinch and Bijker's on bicycles (1987) or Callon's on scallops (1986), and for contemporary studies, including those published in high-ranked journals. For example, of the articles published in *Science, Technology, & Human Values* during the past 3 years that claimed to use the case study approach, some speak about data collection (e.g. interviews, documents, trade publications) while not mentioning or scarcely mentioning any analysis at all (e.g. Davis & Abraham 2010, van Egmond & Bal 2011, Storni 2012). Other works mention specific techniques such as (iterative) coding, but only in passing (e.g. Glenna 2010, Morrison & Cornips 2012). The same is true for articles that claim to have used grounded theory, an approach characterized by a clearly specified set of guidelines—

it is amusing to note that all such articles devote exactly one sentence to the issue (Felt *et al.* 2010, Frickel *et al.* 2010, Timmermans 2011).[47]

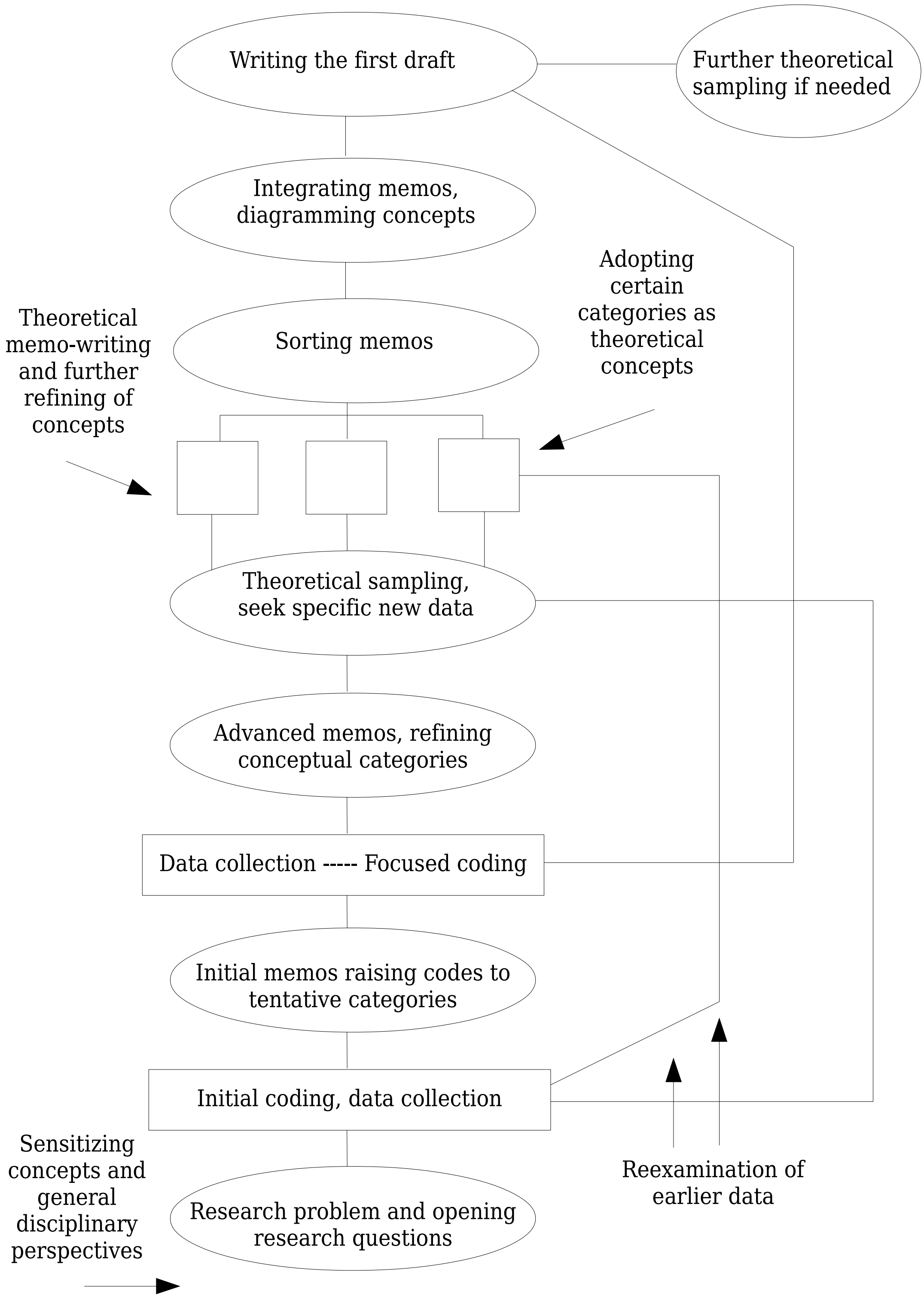
There are at least two explanations for why this might be the case. A sizeable part of the STS community might think that 1) the link between data and generalization is usually intuitive and self-evident—people reading the same data with the same theoretical expectations can easily arrive at the same conclusions and/or; 2) what happens between data and theoretical models is a craft that cannot be captured or formalized—some are simply able to theorize better than others and no amount of description of techniques can substitute for that.

There is much to agree with in this account. First, some generalizations, especially higher-level ones, can often be quite intuitive. For example, it does not demand much effort to make a mental connection between any STS case description and a claim that 'the interactions are complex and mutual shaping of actors and technologies takes place'. I also agree that the capabilities of theorizing differ: good theorizers are few and far between.

On the other hand, the way in which such models were derived should be made visible to others so that the researcher's choices could be assessed better. Langley (1999) discusses seven different strategies: narrative, quantification, alternate templates, grounded theory, visual mapping, temporal bracketing and synthetic strategy. Space considerations do not allow me to cover each of these in depth. Therefore I will discuss the shortcomings of many such techniques on the basis of grounded theory, likely the most popular qualitative approach by far.

47 If the reader remains unconvinced at this point let me propose a hypothetical situation in which one aims to publish an article containing quantitative analysis in a high-ranked journal. The discipline—economics, sociology, psychology, management studies, political science—does not matter. The results are accompanied with only a following methodological note: “The data was analysed according to the principles of linear regression analysis.” What would be the chances of passing peer review?

Figure 4.3. The grounded theory process (Charmaz 2008: 11)



The process of grounded theory, as understood by Charmaz (2008),[48] consists of the following elements: 1) formulating the research problem and initial questions; 2) performing initial data collection and coding; 3) writing preliminary memos to create categories out of codes; 4) more focused coding; 5) the creation of advanced memos and refined categories; 6) theoretical sampling in which the emerging theory guides further data collection to test the propositions in-the-making; 7) more refining, resulting in the emergence of some theoretical concepts; 8) sorting and integrating memos; 9) writing the first draft, possibly followed by more theoretical sampling. Note that the process is iterative: in many phases emerging categories prompt new examinations of existing data or require collecting more data according to new ideas. The end product should be a theory strongly 'grounded' in data (that is, the links between theoretical propositions and data can be easily and clearly established).

There are two reasons, however, why the parallel process of data collection and analysis is unlikely to work very well for this thesis. The first is related to the nature of historical sociology. One can argue that in the process of establishing the historical course of events something resembling the above set of procedures is at work: one collects the data, assesses it, formulates a preliminary idea about how events unfolded, proceeds to collect new information while addressing the existing gaps and testing alternative explanations, re-assesses the information acquired etc. until one explanation can be deemed more plausible than the others. But in this thesis (and in other historical sociological works) the narrative is not an end point in itself, but an intermediary step providing grounds for further theorizing. This means that as long as there are multiple equally plausible historical explanations there are multiple generalizations to be derived from each of them. However, as some of the explanations become more unlikely over the course of research the space for theoretical variety decreases and some previously possible generalizations lose connection with the evidence. Since the ways in which one can theorize complex

48 I prefer Charmaz's treatment to the orthodox version (Glaser & Strauss 1967) and to the more narrow recent interpretations (e.g. Suddaby 2006). The reason is that Charmaz retains the essence of procedural guidelines for moving from data to theory while abstaining from making what I deem unnecessary restrictions. For example, Charmaz allows prior theoretical literature to influence the problem formulation, and she acknowledges the possibility that grounded theory can be 'objectivist' or 'constructivist', descriptive or explanatory etc.

narrative data are abundant anyway, it seems sensible to avoid overburdening oneself with both first-order (narrative) and second-order (theory) generalizations during the process of narrative assembly. In my opinion, the complexity of qualitative research is managed better by settling down to a certain interpretation of events first and then theorizing further on that basis.[49]

The second observation, derived from my personal research experience, is also related to managing complexity. When one looks at the grounded theory process one notices that despite the iterations the general direction of the process is still bottom-up, moving from a variety of initial codes and memos to more general and interrelated constructs.[50] The trouble with this kind of approach—to speak in metaphors—is that it forces one to juggle with all the balls from the beginning, even before one has learned how to do so with two or three. But when confronting the complexity of data in its entirety it is very easy to get lost and miss the wood behind the trees. Indeed, grounded theory has been accused of failing to turn attention to more general and large-scale patterns (Langley 1999: 700). Moreover, considering the limited time resources of each researcher, the effort is quite failure-prone in that the whole data has to be worked through in minute detail and all strings pulled together before the theory could be said to have emerged.

Personally, I have noticed that it is often preferable to start from the other end, to try and capture the process as a whole—to ask: "What is going on here?" After the answer to this question has been found, one can specify: "Yes, but what exactly is going on here?" By gradually adding new elements, the theorization moves from abstract and schematic towards middle-range and nuanced. In addition to feeling more intuitive the advantage is that the process can be stopped by the analyst at any

49 Of course, one cannot escape the irony that future historical research can undermine the initial narrative and thereby put the derived generalizations in doubt. Should this happen one could attempt to save the theory by arguing that the explanatory power of the generalization is not automatically lost—it only loses connection with one particular instance, but may still apply to a range of others. But theorizing the extent to and conditions in which this happens already belongs to the domain of the sociology of scientific knowledge.

50 The same is true for at least some strategies outlined by Langley (1999), especially quantification and visual mapping.

time, yet at any point one also has a theory of some kind (whether it is too simple or too complex is an entirely different matter).

In fact, many STS case studies I have read leave an impression as if the authors had actually followed this strategy and (at least implicitly) had tried to capture the essence of their story in such a manner. For example, the core of Bijker's extended study on bicycles (1995: 19–100) seems to be: 'Social groups attribute different meanings to an artefact by which consensus is achieved and the artefact stabilizes in a dominant design'. Callon's study of scallops (1986) can be summarized as a sequence of translation proceeding from problematization to 'interessement' to enrolment to mobilization. The main trouble with case studies like this is, however, that such high-level theorizations are often the endpoint of the analysis, meaning that there remains a *"gap between relatively simple, sensitizing conceptual schemes and detailed, complex case descriptions with some empirical generalizations"* (Geels 2007b: 633). So sympathizing with the (apparent) starting point of STS and the rigour of grounded theory, the following intra-case analysis will simply attempt to make the best out of their combination.

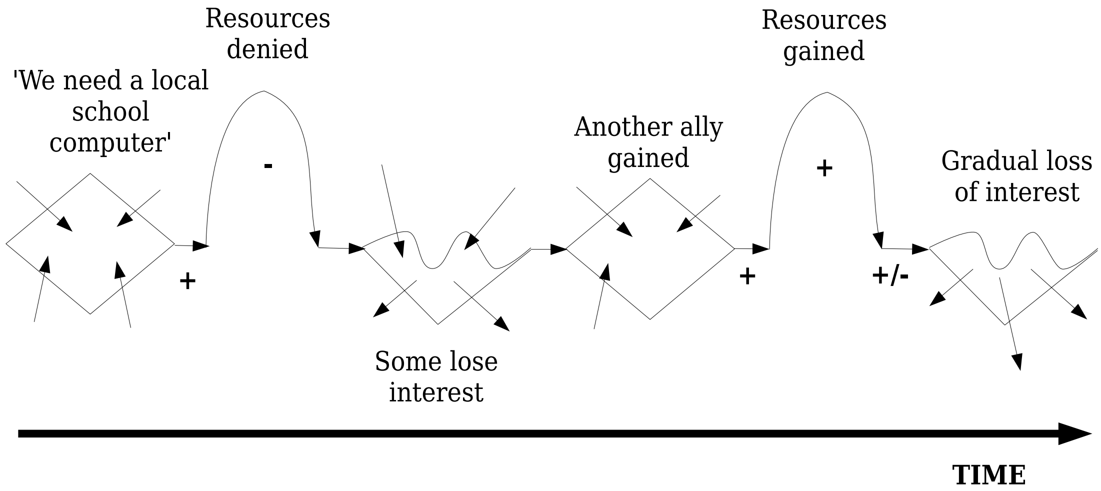
4.2 Intra-case analysis

4.2.1 Step one: detecting the key nodes

To illustrate the technique (and the gradual emergence of substantive results) the following outline will be quite extensive. A few clarifications: first, the level of analysis sets certain demands on the detail of data required. Therefore, owing to the combined reasons of space and data insufficiency the intra-case analysis will mainly focus on three cases in Estonia and three in Lithuania. Developments external to these localities (i.e. production in other factories in the USSR) will be excluded. Latvian events will be included in inter-case and system-level analyses. Finally, space considerations mean that the treatment of the cases is necessarily short. The reader is referred back to the previous chapter for more detail.

The very first step was to start from the case that I knew best—Juku. I attempted to summarize the basic course of its development in a few sentences: "Strong network around the Juku project formed fast, but it proved to be difficult to get it into production. Various participants dropped out of the project and substitutes had to be found, delaying the process and leading eventually to an outdated product. Difficulties with upgrading and shifting user perceptions put an end to the project." Figure 4.4 shows a visualization made on the basis of this summary.

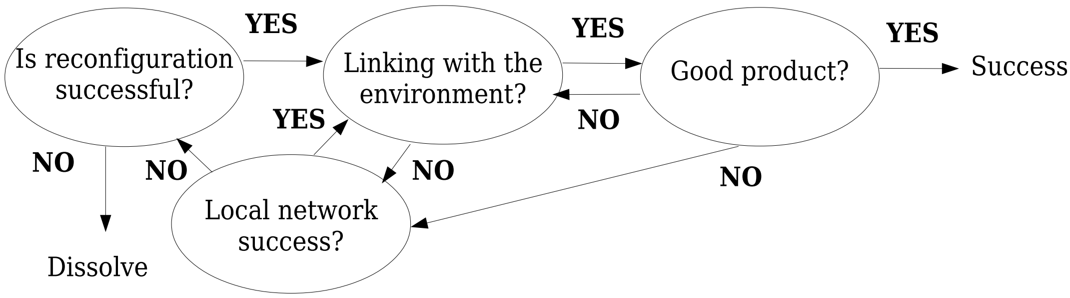
Figure 4.4. A rough visualization of Juku's development



It can be seen that in general the network of Juku went through two phases of expansion and contraction similar to each other regardless the differences in interactions with its environment. It could also be expressed in terms of its properties undergoing some changes (e.g. losing RET and Estron with their production infrastructure and know-how of compiling technical documentation resulted in an inability to perform some activities and the need to look for new partners).

This formulation led to a question: what kind of properties? Are there any key nodes of development that would capture the story in more detail? A trial-and-error process followed in which I attempted to select and link the optimal number of nodes (enough to capture the whole development process, but not so many that would result in duplication or over-complicated depiction). Figure 4.5 shows one such early sketch. Note how this visualization obscures the temporal progression in favour of providing more information about case-internal developments.

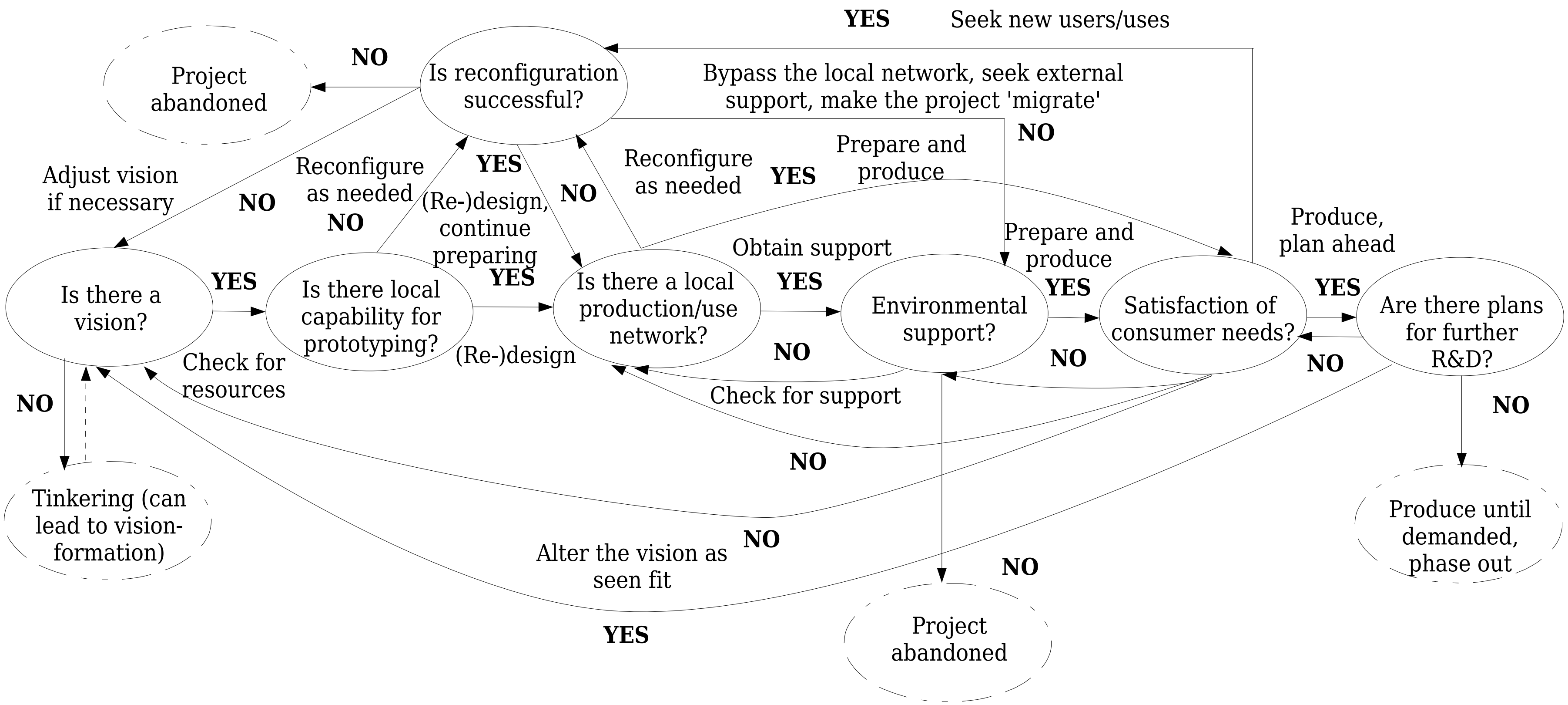
Figure 4.5. An initial sketch of Juku's key nodes of development



Many problems occurred when comparing this sketch with the narrative. For example, there was no part for the vision that often preceded actual developments, the criteria for defining a good product were vague, the perceived quality of the product changed over time (e.g. even the most committed participants admitted that Juku went into production far too late, but even then helped to reduce the shortage of computers considerably) etc. Iterations and re-drawings followed, involving the addition, deletion or merging nodes and interactions until existing and emerging questions like the ones found above could be more or less solved and approximate representation of moving from one property state to another could be attained.

I then moved on to other cases. By comparing the brief summaries of each case with the model, I quickly discovered many elements that did not fit. For example, initially the Entel group was not planning mass production and thus did not seek support from the environment, instead proceeding straight to production. To account for the possibility of reformulating the vision during the development of the case, a respective feedback link had to be devised. The Poisk project sensitized me to the possibility of a project being abandoned outright if environmental support fails. And so on and so on. The iteration was stopped when I deemed the result to be applicable to all cases.

Figure 4.6. Changing properties of socio-technical networks



Note: Although the figure has been inspired by flowchart diagrams it is strictly speaking not a flowchart because it allows for non-unique connectors to exit from some nodes. The reason is my preference for ontological accuracy over methodological requirements: different networks simply chose different ways to proceed from the same property state.

Explanation of the key nodes is as follows:

1) 'Vision' refers to more or less fleshed out ideas about the reasons for development of the computer, including but not limited to technical characteristics, price, domain of application, potential users, plan for mass production etc. (Note that it does not need to be an eventual or an explicit vision—characteristic users and uses might only be imagined by the designer).
2) 'Capability for prototyping' refers to necessary resources (including actors with necessary skills, knowledge and technologies) for building a prototype. At minimum it could be a single sufficiently equipped enthusiast.
3) 'Local production/use network' refers to the preliminary constellation of actors, technologies and rules involved in the project. Domain-wise it includes engineering, economics, politics, culture etc. Functionally it includes potential producers, distributors and users (note that this does not mean that the network is fixed, e.g. that all application domains are known, all potential users have been included etc. These may well turn out considerably larger or smaller than initially expected).
4) 'Reconfiguration' refers to changes in the socio-technical network to achieve a desired goal or adapt to changes. These changes are multiple but all involve some alterations to the constellation of the network (e.g. the prototype might need improvements, existing users might lead to new ones).
5) 'Environmental support' refers to the wider context external to the local socio-technical network in focus that asymmetrically affects the chance of the latter to realize its vision (e.g. in planned economy this might refer to approval from the central authorities, in market economy it can mean the wider market of consumers not included in the design process whose behaviour can lead to adjustments in the local network).
6) 'Satisfaction' refers to the question whether the product is able to satisfy at least some of consumer needs (e.g. in planned economy products might be of relatively low quality but nevertheless be desired due to overall shortage).
7) 'R&D' refers to plans regarding further development of the product, i.e. the

beginning of another product cycle.

Explanation of possible interactions is as follows:

1) An idea, even a raw one about the potential uses of the computer, might lead to mobilization of necessary resources to realize it. Failure to conceive of one can lead to aimless tinkering (although in principle it is possible that the vision gradually emerges from such activity).
2) If the necessary resources and capabilities for prototyping exist, a prototype will be created. If not, then additional reconfiguration is needed (e.g. more components, people with relevant skills, more information).
3) If the product is seen to have any use, the need to produce it in larger quantities emerges. Here direct contacts with some potential producers, distributors and users are often established, if only through preliminary negotiations. If the mobilization is successful, production could start immediately or support sought from the environment first (note that this is one of the main differences between a market economy and a planned economy—in the former mass production can start immediately, but the success of the product still depends on its resonance with the environment; in the latter only small-scale customized production is possible without central support, otherwise approval from the authorities must be obtained first). In case the mobilization of the extended local network fails for some reason, reconfiguration of the network is needed.
4) If reconfiguration is successful, a local network comes into being. If not, the original vision can be reformulated, leading to possible changes in other elements (e.g. new prototype, new participants). Alternatively, the project can be abandoned altogether. Yet another option is to attempt bypassing the local network (e.g. gaining the support from the central authorities for local production or making the project 'migrate' by linking up with producers or users outside of the local network).
5) If preliminary environmental support has been secured, the question of to what extent the product is able to satisfy consumer needs emerges. If

environmental support is absent this might lead to problems with the stability of the local network and the project might be even abandoned.

6) If the product is found satisfactory in at least some respect, the socio-technical network in question might be reconfigured (e.g. expanding the production with all required preparations and adjustments, adapting the product to new uses). Continued production can also give rise to a question about the next product cycle. On the other hand, if the product turns out to be lacking in some aspects, support from global network and local extended network needs to be re-checked. If these still hold production can continue unchanged at least for some time. Alternatively, the original vision might need adjusting.
7) If the new product cycle is to be started, further research and development activities are required, with the possibility that both the original vision and the local network need to be rethought and reconfigured accordingly. Alternatively, the production of the existing product can continue without any or with only minor improvements (e.g. the addition of better memory chips, but no alterations to the basic design) until demand exists. In that case the project will be gradually phased out of production and use.

This model can be applied to each of the cases:

Juku: this case was characterized by fast vision-formation, including the plan of mass production (1), creation of a local prototype by an influential organization (2) and mobilization of a local supplier–user network (3). The disapproval from the central authorities (5) led to delays, after which two producers dropped out because they had changed their vision about feasible production (3). Reconfiguration activities followed (4), resulting in the agreement with another producer (3). The outdated product was perceived as better than nothing in the short term (6), but not in the long term, leading to R&D to create the second generation school computer (7). The vision (1) and prototyping capabilities (2) remained largely the same, but the end of the USSR, disruption of the supply chain and other difficulties resulting from wider social macro-processes meant unsuccessful network mobilization (3) and reconfiguration activities (4), which is why the project was abandoned.

Tartu: after the LES lab was created the idea of building a PC emerged gradually. The initial vision of Tartu (1) was about small-scale, customized production for the university's own needs. After creation of the prototype (2), discussion of production with various parties followed (3, 4), but did not result in actual cooperation. School computerization programme led to reformulation of the vision (1) around roughly the same computer (2), but the local support network formed mainly around Juku (3). Reconfiguration activities (4) led to a contact with a factory in Kursk (5). As the start of production was delayed, Palivere was sought as a partner at the local level (3). When Palivere's production finally started, Tartu was quite outdated and only had a steadily decreasing price advantage (6). Competition with Western products was deemed unthinkable and thus no R&D activities followed (7).

Entel: the initial vision emerged (1) from technical interest and practical problems of the Ministry of Communications, affecting the design of the prototype (2). Contracts for various projects followed (3). References from satisfied customers (6) led to various new contacts (4), in turn leading to further changes in the local network (3). One of the new potential users approached with a proposal to adapt Entel for schools, leading to changes in vision (1), minor additions to design (2) and links with the vocational education sector (3). Contact with the Pöögelmann factory meant another reconfiguration (4) and another potential adjustment in vision (1), but support from the central authorities could not be secured (5) while the commitment of the Entel group itself was rather half-hearted. The computers continued to be used for a while (6) and the project was quietly phased out (7) as the interests of the group changed.

Santaka: the vision of a cheap, simple, software-compatible and quickly produced computer for schools and young people (1) led to the cloning the Sinclair Spectrum (2). Although contacts with the KRMESRI were established (3), mass production did not get an official approval and a satisfactory agreement with Sigma regarding the production could not be established (4). The vision had to be slightly adjusted with respect to the quantities to be produced (1), but otherwise the prototype was made (2) and small-scale production prepared with an approval from local politicians (3). Produced computers (6) were given to schools and copied by hobbyists (4), leading to a larger local network (3). Due to the initial vision seeing Santaka as a temporary solution and the increasing (expectations of the) availability of newer computers no R&D followed (7), and the project was terminated on the local level. In parallel with local production, however, the KRMESRI established successful links with the central authorities (5), leading to the migration of the project and subsequent production in two Soviet factories (6).

Sigma 8800: with the increased need to find products that could be produced and marketed Sigma came up with a PC/XT project (1) and a prototype (2) which it offered to schools. The education sector initially agreed (3). Sigma proceeded to prepare the production and produced some computers (6). In the meantime the education sector increased its demands with respect to the desired characteristics of the computer (3). Sigma was unable to meet these requirements (4) and the project was soon abandoned (7). The already produced computers were eventually used in Sigma's own production processes.

Lema's PC/XT: tinkering with an IBM PC/XT clone to fix it led to a vision to copy the IBM design with as many Soviet components as possible (1). The capability of prototyping (2) was hampered by the lack of foreign currency, which could only be solved by establishing a contract with a partner from Moscow (4). The development resulted in a working prototype (3), but owing to new possibilities existing partners lost interest in the project (4), leading to the establishment of Lema (3). Small-scale customized production followed, with new contracts gradually coming in (6, 4, 3). In parallel, the half-hearted contact with Sigma did not lead to mass production (4). Otherwise no major alterations to the design were made (7) and with new possibilities and more profitable projects the PC was gradually phased out.

BK-0010Š: the vision of Venta/Nuklonas saw the production of BK-0010Š as a school computer (1) in which the design (2) could be acquired from the environment almost without any changes. The local support was limited because some parties were excluded from the decision-making process and divided in opinion, while others thought that agreeing to the production proposal might result in a more favourable attitude of the central authorities in the future (3). The central powers provided official support (5), leading to production (6). With the increasing availability of newer computers and the accompanying shift in user perceptions no R&D followed (7) and the production was eventually stopped.

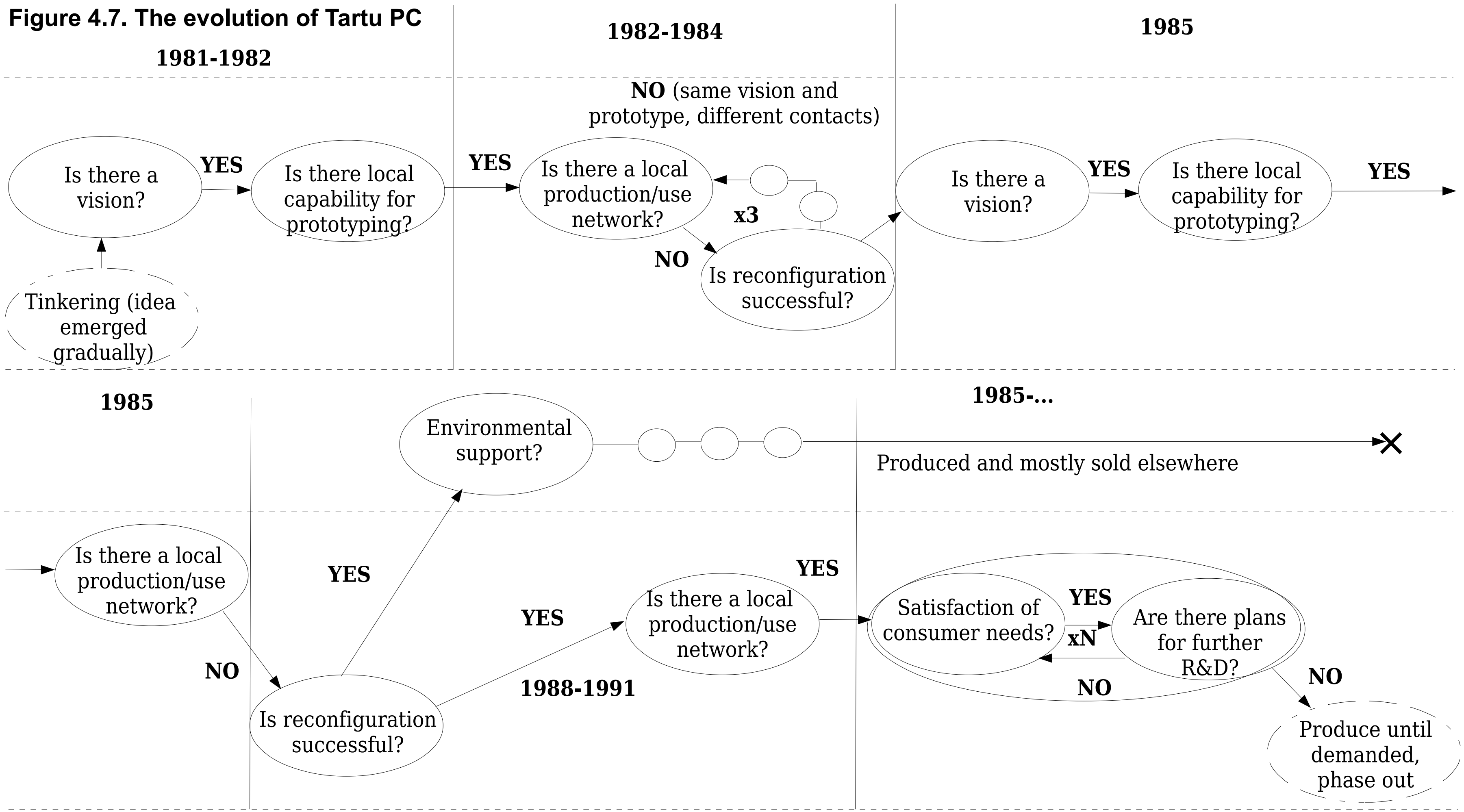
Poisk: the possibility of engaging in cooperative activities between state enterprises and earning profits led Sigma to the idea to start producing a Soviet IBM-compatible (1), the design which had already been developed elsewhere (2). Although there was notable resistance from the local level, albeit without formal authority (3), production might have still started (4, 3) if the potential partner had not increased its demands (5). Lacking the support from the environment, the project was abandoned and Sigma started seeking other, more promising projects.

4.2.2 Step two: from key nodes to key node sequences

Figure 4.6 presented a model of the key nodes of all cases and the interactions that led from one to another. Although seemingly quite complex it actually excludes many important details about the actual development of each case. Most importantly, it hides the unfolding of each case in time. Thus one cannot distinguish between different phases of development, consider parallel developments or detect the repeated occurrence of some event sequences.

Thus in the second step I mapped the development of each case separately, turning explicit attention to the shortcomings outlined above. An example of such a map for the case of Tartu PC is provided in figure 4.7 (all others are found in appendix B).

Figure 4.7. The evolution of Tartu PC

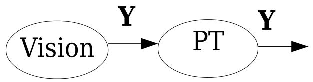
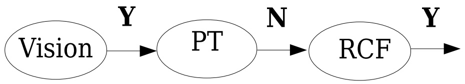
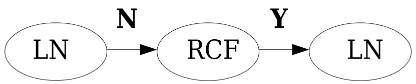
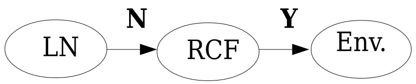
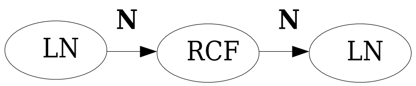
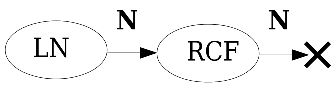


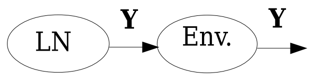
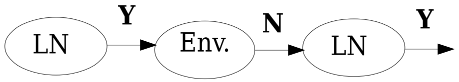
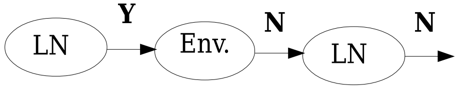
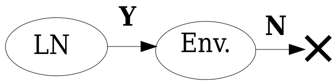
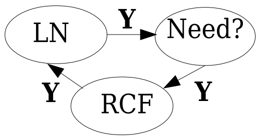
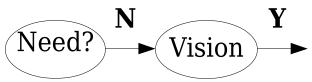
The dashed line in the figure separates local developments from the environment, whereas the vertical ones divide the evolution into phases. The small empty circles refer either to the parts of the local event sequence that remained roughly the same or to unspecified developments in the environment. Finally, X marks the end of the project in the environment.

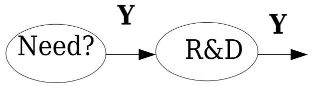
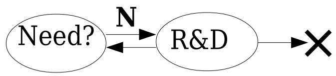
It can be seen that figure 4.7 unites the temporal progression of figure 4.4 with the amount of detail of figure 4.6. The use of vertical separators allows attention to be turned to the evolution of the cases as series of key node sequences with certain outcomes. In Tartu's case one can distinguish between a preparatory phase in which tinkering with microprocessor technologies gradually led to the idea of the computer, followed by a search for possible producers. The third phase, triggered by school computerization in 1985, involved changes in vision but ended without any success. Yet another round of search in the following phase led first to the migration of the project and then to the establishment of local contacts. In the fifth phase the project started losing ground, first gradually and then more rapidly, until the production was terminated.

Applying this logic to each case, I detected a set of such key node sequences, each concerning a particular aspect of the evolution of the network. I then grouped the sequences according to which aspect of the development they seemed to be about. The results are presented in table 4.1, along with a short explanation of each. Additionally, the outcome of each sequence has been indicated—that is, whether the network expanded (+), remained the same (0), decreased (–) or collapsed (×).

Table 4.1. Key node sequences of socio-technical network evolution

No		Sequence	Description	Example	Outcome
1	a		Preparations: shared vision along with (capability to build) a prototype	IoC envisioned Juku as a simple, cheap and quickly produced school computer and had the capability to design one	+
	b		Preparations: shared vision exists but some elements for prototyping are missing, leading to attempts at reconfiguration	Lema's PC/XT: not all components could be replaced with Soviet ones, therefore foreign currency was needed before the project could continue; partners in Moscow were thus found	+
2	a		Local search: unsuccessful initial attempts to secure local support, followed by successful reconfiguration	Many organizations were initially interested in the Tartu project, but only Palivere started local production	+
	b		Local search: unsuccessful initial attempts followed by unsuccessful reconfiguration, leading to the attempts to 'migrate' the project	Tartu people established connections with the Kursk plant so the latter would mass produce their design	+
	c		Local search: unsuccessful initial attempts followed by unsuccessful reconfiguration, leading to no change in the initial network	Santaka was advocated for mass production but was eventually produced on a small scale by the initial KPI-KRMESRI network (note: the example is not perfect because the vision also needed to be adjusted accordingly)	0
	d		Local search: unsuccessful initial attempts followed by unsuccessful reconfiguration, leading to the collapse of the network	When the education sector changed its preferences, no new potential market was found for Sigma 8800 and the project was soon abandoned	×

	a		Adapting to the environment: searching for the support of the environment is successful	The proposal from Venta/Nuklonas and the approval from the central authorities enabled mass production of BK-0010Š to begin	+
3	b		Adapting to the environment: searching for support from the environment fails, but local network remains committed to the project	No exact empirical match, but similar to 2c—the closest match: Entel continuing on the path of small-scale, customized production after mass production failed to be realized (but as the contacts with the Püügelmann plant were disrupted the shape of the network was altered too)	0
	c		Adapting to the environment: searching for support from the environment fails, followed by the instability of a local network	After the mass production of Juku was refused approval by the central authorities two participants quit the project	-
	d		Adapting to the environment: searching for support from the environment fails, leading to the collapse of the network	When Sigma's potential partner in Kiev raised its demands the former abandoned the Poisk project	×
4	a		Normal diffusion: use of products leads to new customers with similar preferences and application of the product to new functional domains for which extensive re-design is not necessary	Santaka design diffused into hobbyist circles where it was copied and modified to some extent	+
	B		Normal diffusion: addition of new users with different preferences, followed by (slight) adjustments in vision	An enthusiast from the education sector influenced the Entel group to adapt the computer for school needs along with the idea of mass production	+

5	A		Decline/renewal: while existing user needs are still fulfilled, the new product cycle is already being planned (proactive renewal)	While existing Jukus were alleviating the scarcity of school computers, IoC people started thinking about a potential upgrade	+
	B		Decline/renewal: as no renewal is planned, users with changed preferences gradually drop out until the network decays	No next-generation computer was planned in Lema. The existing design was produced until demand dropped off and then was completely phased out	×

Vision = presence/absence of the vision, PT = prototype, RCF = reconfiguration, LN = local network, Env' = support from the environment, Need? = satisfaction of user needs, R&D = plans for research and development activities, X = project terminated.

As noted, such a mapping opens up the compressed model presented in figure 4.6 and shows that each case involves moving through multiple loops to capture the whole sequence. But another issue also manifests itself: namely, that in certain situations some steps were actually redundant. In Juku's case, for example, the vision and the prototype remained roughly the same for years while partners for production were searched for. This directs attention to the cumulative nature of socio-technical network evolution. At least three different states of the network have been implicit in the discussion: 1) vision; 2) prototype; and 3) set-up for production. But (how) is this distinction relevant for the next step of analysis?

4.2.3 Step three: inserting the multi-level interaction

One could criticize the results described above from many angles. For example: 1) the results are too undifferentiated, downplaying or failing to make a clear distinction between network-internal and network-external events; 2) this inadequate distinction leads to a lack of understanding of the interaction between network-internal and network-external events; 3) the groups and the activities enacted by these groups that underlie the key node sequences remain obscure; 4) the sequence of certain key nodes does not necessarily imply that they are causally connected; 5) the sequences might be misleading or simply empirically inaccurate—for example, in Juku's case the attempts at network-building preceded the development of the prototype.

There are several ways that these problems can be addressed. What I chose to do was to zoom in on each phase of development. As seen from figure 4.7, however, some of these phases contained a number of interactions or episodes (e.g. repeated unsuccessful searches for different partners). I decided to focus on explaining the outcome of each such episode, turning attention to the temporal unfolding of the interaction between different local socio-technical actors/networks. I also sensitized myself to the properties of these actors/networks and to the role of network-external events. The overall purpose of this step is thus to explain changes in the properties of the socio-technical network as a result of network-internal and network-external events. In this section I will present these episodes to show how the understanding of these processes gradually improved during the analysis process. Once again I will take Juku as my starting point.[51]

Case 1, episodes 1–2: explaining the formation of the local network

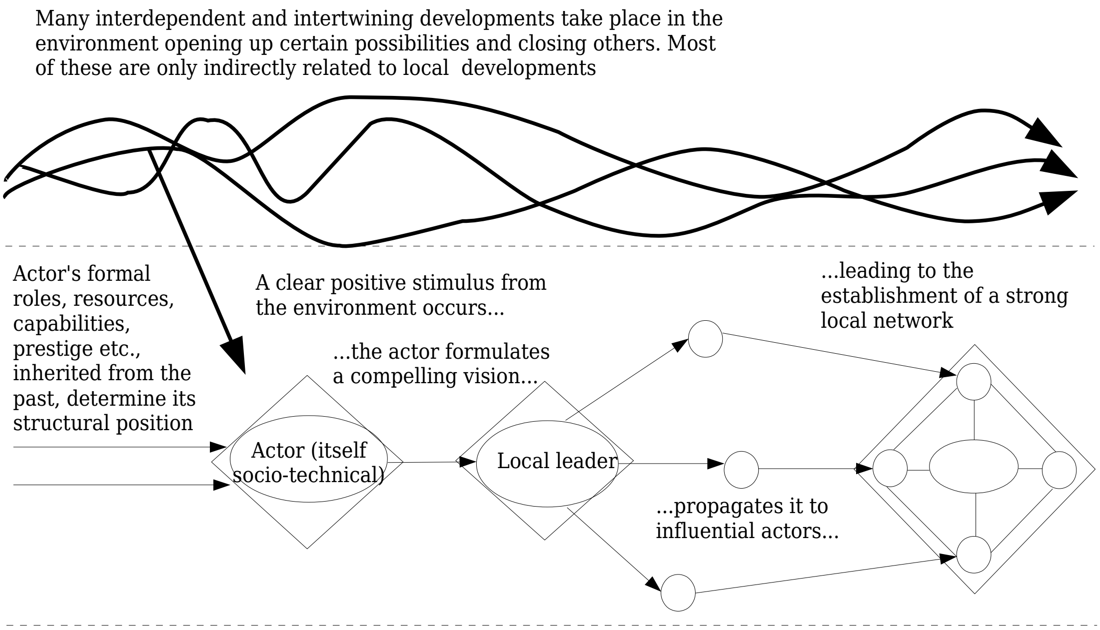
IF there has been a clear positive environmental stimulus

AND a strong leader

that presents a compelling vision

THEN formation of a strong local network follows

Figure 4.8. Network formation (Juku)



Correspondence to the narrative is as follows: by the time the events started, the IoC was a well-established, capable organization with ample resources and connections(2). School computerization provided a direction for adapting the IoC's experience in microcomputing (2). The IoC came up with an idea of a locally designed and mass produced school computer (3). Meetings and negotiations with

51 It must be remembered that (similarly to the preceding analysis) the following theorization gives up some of the conditionality of the narrative, favouring one explanation of events over others (e.g. the change in RET's preferences is taken at face value, excluding problems with potentially biased presentation of the events by an interviewee). On one hand, this is regrettable; on the other hand, it is rather unavoidable if one is to move forward from deconstruction to reconstruction.

local decision-makers, producers (RET, Estron) and future users (4) resulted in the formation of a broad support network dedicated to the realization of the idea (5).

The first problem with such a generalization is the increasing imprecision of the terms used. One can well contest the adjectives used: what do 'clear', 'strong' and 'compelling' actually refer to? Some tentative criteria can be presented in response:

1) Clarity of the environmental stimulus is mainly defined by its proximity and connectedness to local events. For example, the idea of a local school computer production can be easily traced back to the influence of central initiatives for school computerization.
2) Strength of the network reflects mainly the aggregate attributes of the network and thereby its potential capability to effect, resist or adapt to changes. It is expressed in many attributes: number of actors, production infrastructure, relevant know-how and skills, resources for producing the PCs etc. As such, all actors and networks of actors are thoroughly socio-technical.[52] In this case the IoC managed to establish a broad network of decision-makers, potential producers, designers and users with sufficient production capabilities and potential influence on central authorities—the machines as well as the people mattered.
3) Whereas the strength refers mainly to the aggregate attributes of the socio-technical network, the compelling quality of the vision denotes mainly the degree of commitment of its components to act as a whole. The criteria by which the quality of the vision might be judged are many. These include (but not limited to): a) clarity; b) perceived timeliness; c) perceived realizability; d) scope; e) match with pre-existing organizational interests and commitments; f) expected pay-off from new commitments; g) extent of new commitments and obligations; h) power to decide over the terms of participation (including entry and exit). As such, different participants might ascribe different values and different levels of importance to these criteria,

52 Hughes seems to have a similar idea in mind when speaking about the mass of a large technical system (1987: 76).

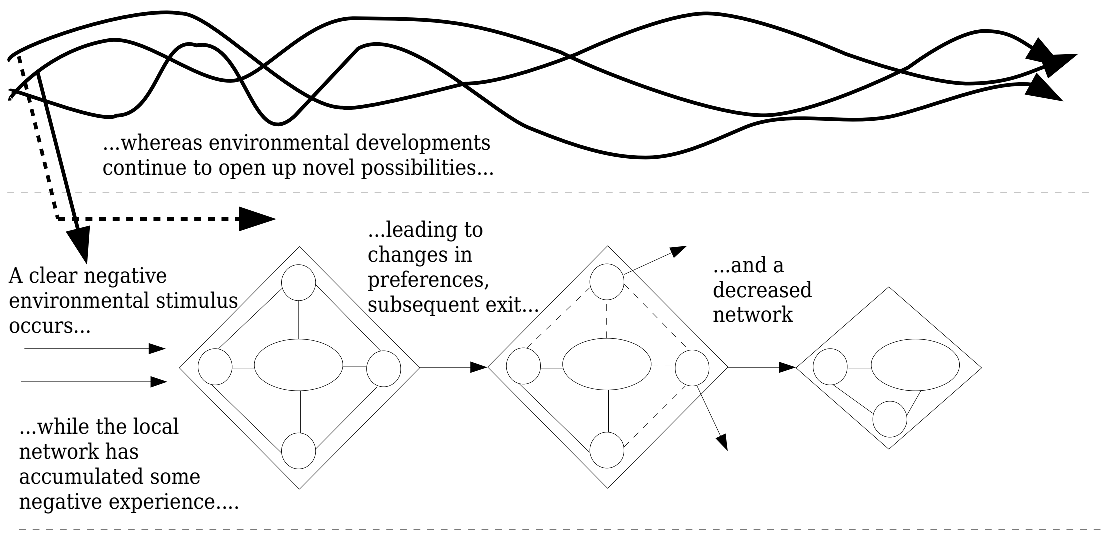
influencing their joining/staying/leaving the network. In this case the vision of a locally designed and mass produced computer was articulated clearly, it was deemed sufficiently up to date (provided the production would start soon), it seemed possible to realize this vision, and the production might have brought prestige to all and profit to at least some of the participants.

Although the use of these adjectives has been clarified, it must be noted that these variables are not strictly measurable: it is unclear how some of them can be operationalized more rigorously and if so, whether the data on current cases is sufficient to do so. The characterization is based on my knowledge of the cases and their contexts, and is relative to the entities involved. The degree of arbitrariness and subjectivity in analytical decision-making is therefore notable. As a first approximation, however, I deem the approach satisfactory because compared with step two it allows for attention to be paid to more nuances.

Case 1, episodes 3–4: explaining the contraction of the local network

IF there has been a clear negative environmental stimulus
along with changed structural possibilities
AND the performance of the local network has yielded negative results
there are irreconcilable differences regarding the renewal of the network
and there is a prototype but the vision keeps weakening
THEN contraction of the network follows

Figure 4.9. Contraction of the network (Juku)



Correspondence to the narrative is as follows: the central authorities refused to approve the project and did not allocate resources (1). New prototypes were being worked out elsewhere in the USSR (2). Estron had experienced unexpected difficulties with the production and quit (3). RET had come to see the project as outdated when finally implemented, had found a more promising prototype and, unable to convince the IoC, also quit the project (4). As a result, two potential producers along with the necessary production infrastructure and know-how of preparing full technical documentation disappeared and the overall strength of the network decreased (5).

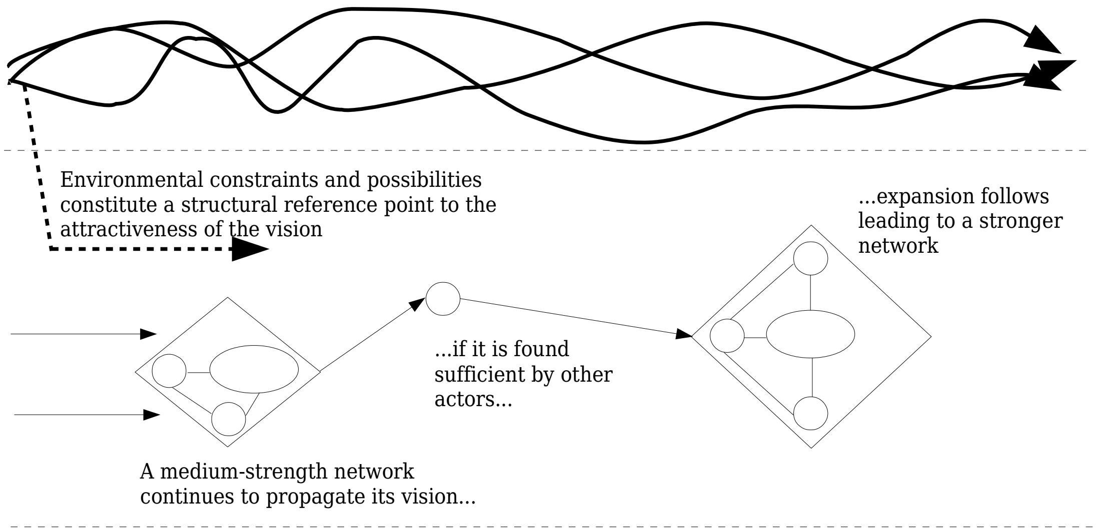
Case 1, episode 5: explaining the expansion of the local network

IF the strength of the local network is moderate

BUT there is a prototype and the vision is sufficient

THEN expansion of the network follows

Figure 4.10. Expansion of the network (Juku)



Correspondence to the narrative is as follows: changes in environmental conditions also changed the frame of reference and accompanying expectations (in Juku's case the actual content of the vision was more or less the same, but (global and Soviet) developments in the domain of computing provided a different measuring stick for judging the merits of the project—thus a 'reasonably up-to-date' computer in 1985 became 'outdated' by 1989) (1). The network still had local political support, a clearly defined user group and the design of the prototype, but it had lost the producers along with the relevant infrastructure and know-how (2). Although the project had become somewhat outdated, it matched with Baltijets's need to use computers for their own production processes,[53] hence making it 'sufficient' (3). Baltijets was part of an elite ministry with good production infrastructure and thus was a strong ally (4).

In principle the description of this episode is similar to the first one: a smaller entity formulates a vision that exceeds its capabilities and therefore, requiring additional entities, diffuses it through various means (organizing meetings, negotiations, presentations) in an attempt to raise the interest of other actors. In both cases the

53 It seems reasonable to assume that the realizability of the vision actually increased over time as the components used in Juku became more widely available, whereas newer components with higher performance remained difficult to obtain.

outcome is an increase in the overall strength of the network. What is different is the initial size of the network—single actor vs. existing medium-strength network with some experience and expectations—and the attractiveness of the vision.

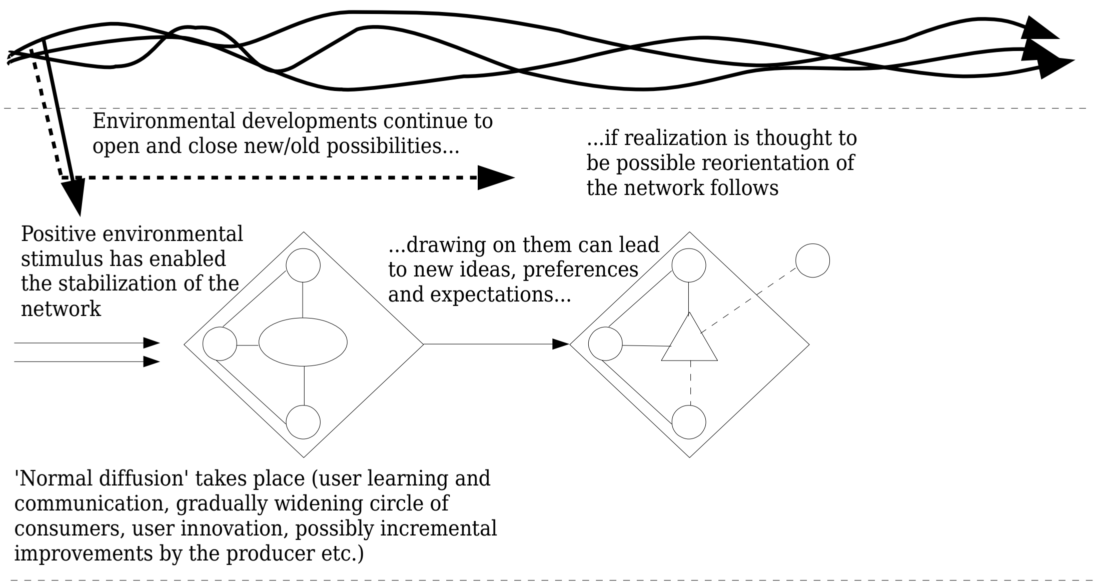
Case 1, episode 6: explaining the renewal of the local network

IF there has been a clear positive environmental stimulus while environmental developments have opened up new possibilities

AND the local network has stabilized on the path of 'normal diffusion' while it is considered possible that it can to cope with future change in consumer preferences (i.e. the expected rapidity and scope of environmental dynamics do not exceed the network's adaptive capability)

THEN reorientation of the network follows

Figure 4.11. Renewal of the network (Juku)



Correspondence to the narrative is as follows: the central authorities approved the production and allocated necessary resources (1). Production was prepared and started in Baltijets (floppy drives and printers were added, schools gradually obtained the computers, new applications appeared, the number of people having hands-on experience with Juku gradually increased etc.) (2). In the meantime various reforms had resulted in decreasing economic and political constraints and increasing possibilities (3). One of such possibility—to start joint ventures—was taken up by the IoC (4). The experience from working with Finnish and Taiwanese partners led to the idea to design a new version of Juku. At the same time contacts with Baltijets virtually stopped after the plant had started production. The involvement of new actors was considered instead (5).

The outcome of this episode brings us back to the beginning of another product cycle. Here the vision can be adjusted, a new prototype designed, existing components excluded from the network and new ones included until a sufficiently strong and stable configuration is achieved. Although new partners were contacted and a prototype designed, the project did not proceed further.

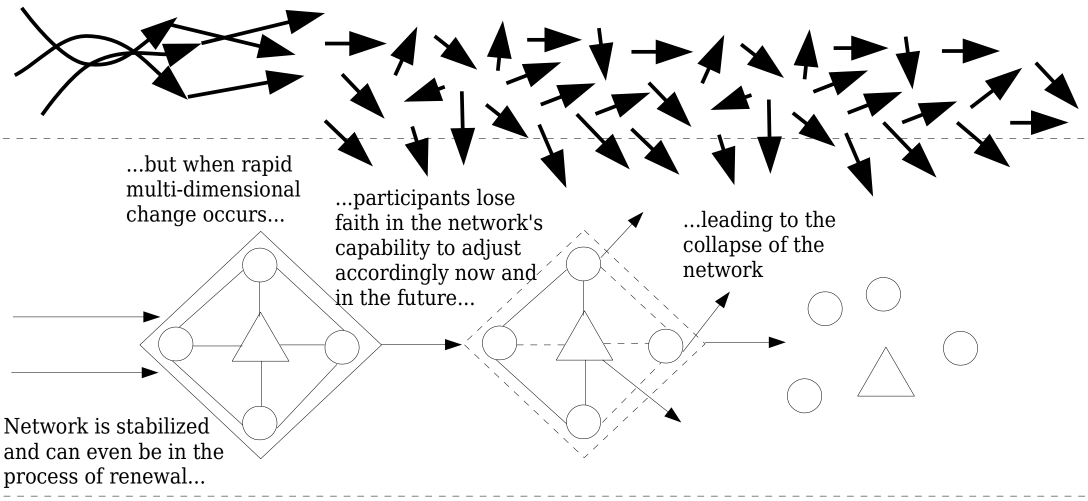
Case 1, episode 7: explaining the collapse of the local network

IF the local stabilized network (with a vision and a prototype) encounters rapid and wide-ranging environmental changes

AND changing demands and preferences exceed the network's adaptive capability and the participants expect this to continue (so coping with future change in consumer preferences is deemed impossible)

THEN the network collapses

Figure 4.12. Collapse of the network (Juku)



Correspondence to the narrative is as follows: the old version of Juku was being produced and the IoC had already designed a prototype of the newer version (1). Independence meant transitions in economic, political and cultural domains involving many radical disruptions, e.g. the loss of Eastern contacts along with the lack of Western ones (2). Disappearance of the closed market meant opening up to global competition, leading the participants to change their preferences regarding the feasibility of the project and future course of action—catching up with the achievements of foreign computer production was deemed unrealistic (3). Baltijets stopped production, the IoC moved on to other activities and school computerization was reoriented towards Western PCs (4).

Note that the term 'lose faith' does not necessarily refer to irrational behaviour: on the contrary, the participants had rational and reasonable expectations about their ability to compete with Western PCs considering the outdated production infrastructure, lack of funds, superiority of Western computers and rapid advances in computing technology. The fact that even the strongest local networks did not survive the wider societal transition gives an indication of the strength of this pressure.

The first round of the analysis is thus complete. It is time to reflect on some of the findings. I will focus on environmental developments here as a sufficient variety of them have been observed already.

First, theoretically one should distinguish between environmental stimuli and structural opportunities or constraints. The first is defined by its proximity to the local event, where the logical connection between the environmental stimulus and the local response can be easily established (e.g. the connection between the central initiative of school computerization and a vision of a local school computer). Structural opportunities and constraints on the other hand can be characterized as crystallized properties of past environmental dynamics which have preceded local events in focus but have had no direct connection to them. At any point they can be drawn upon by the local network, however. An example of this is an economic reform allowing joint ventures between Soviet and Western companies. Initially, when this opportunity was seized by the IoC, it had nothing to do with local school computerization. Experience, however, led to the idea of establishing a new network and designing another version of Juku. Thus the environmental change had an impact on the outcome, but was temporally and initially thematically disconnected from the local response. Another example of this is provided by RET, which took advantage of changes in the domain of computing and altered its preferences when the Juku network failed to achieve early central support. Once again such developments had occurred in parallel, and until that point had no direct connection with the local network.

The problem is that the number of potentially relevant structural constraints and opportunities is overwhelming. In any theorization only some of these, usually most visible, can be picked up, while others are relegated to the background. But that does not meant that they are unimportant altogether—for example, the overall quality of Soviet technology or problems with shortage and accessibility influenced each and every case (albeit to different degrees). But listing all of these background factors would quickly result in massive and mostly tedious lists, going all the way back to the fundamental laws of physics.

The problem with such selection is, of course, that in different contexts these 'silent' factors might suddenly become visible as changes in their values better enable the detection of their impact on the outcome. This in turn would instantly refute the IF–THEN statements presented above, with their seemingly deterministic flavour. For example, the characterization of episode 5 rests implicitly on the assumption that other suitable local actors are present and known to the local leader. Otherwise the network could collapse or the product could be made to 'migrate' despite the qualities of the network and the vision remaining exactly the same. Without further comparison it is quite difficult to tell in which context one or another structural factor can become visible and what their impacts might be. However, it should be noted that the 'determinism' of IF–THEN statements is actually conditional on the values of a huge number of 'unseen' background variables.

However, the distinction between structural opportunities and constraints and environmental stimuli is not enough, as the latter are far from uniform. As shown in chapter 1, such stimuli can be defined as combinations of varying degrees of four attributes: frequency, amplitude, speed and scope (Suarez & Oliva 2005, Geels & Schot 2007). In the above episodes there were basically three environmental stimuli: 1) transition-related; 2) educational reform-related; and 3) central authorities-related. Only the first seems to correspond well with one of the types outlined by Suarez and Oliva (2005): high amplitude, speed and scope of changes but with low frequency would make such a societal transition an 'avalanche' change.

In the second case the reform spanned the education sector, so its scope might be described as medium. It was also supposed to be implemented relatively quickly (high speed). However, I would characterize the amplitude as rather low—Soviet reforms were often more about rhetorical slogans than actual change itself. Quite likely this was the case here—despite slogans like 'programming is the second literacy' and the aim of changing the thinking of children, initially few computers were provided for schools, their integration to other subjects was low and the teaching process itself was not fundamentally changed. In fact, based on previous experience of Soviet reforms, some teachers thought computers to be a passing fad (Dagienė 2006). The frequency of such reforms can be described at least as medium. Therefore I would generalize and call it a domain-related reform.

In the third case it must be remembered that decisions about the viability of different projects were frequently made by central authorities and the result could range from enthusiastic approval to outright ban. Although the Soviet bureaucracy was perceived as notoriously slow, the speed of the decision itself (while often negative) could be regarded as relatively quick. Finally, as the decision was network-specific, the scope did not extend beyond a particular case. Therefore, I would call this type of impact a network-specific shock.

Table 4.2. Types of observed environmental stimuli

Frequency	Amplitude	Speed	Scope	Type of environmental change
High	High	High	Very low	Network-specific shock
Medium	Low	High	Medium	Domain-related reform
Low	High	High	High	Avalanche

The analysis will now proceed to the case of Tartu.

Case 2, episode 1: explaining the formation of the local network

IF there has been a clear positive environmental stimulus

AND a weak leader

that presents a weak vision

THEN formation of a weak local network follows

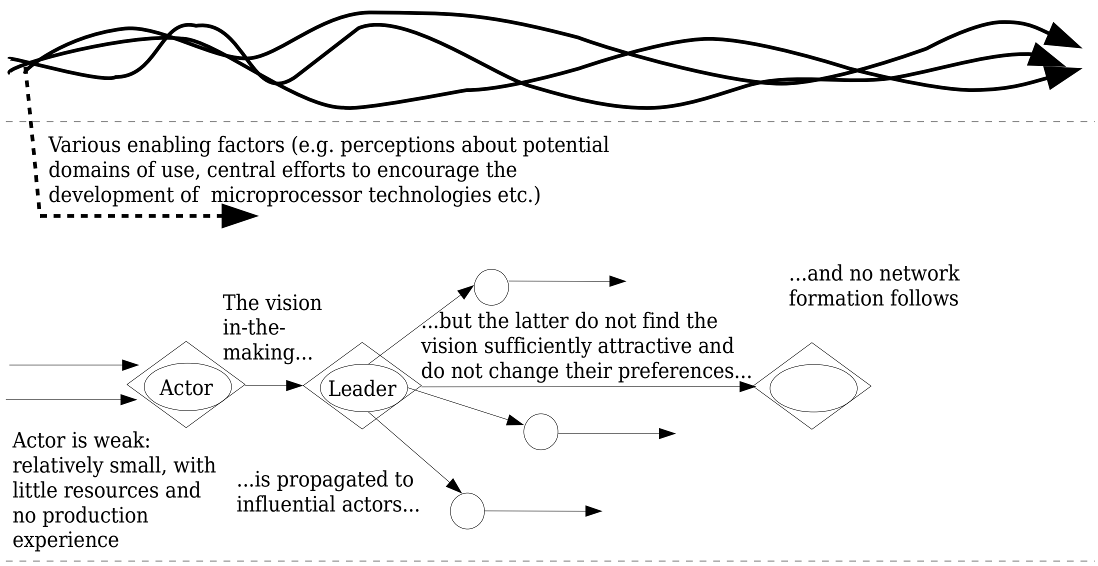
Correspondence to the narrative is as follows: the all-union directive demanded that universities develop microprocessor technologies (1). This coincided with an expressed interest of an enthusiast from Tartu State University to establish a working group with this aim (2). In the beginning there was no specific idea about the direction in which actual developments should start—the idea to build a PC emerged gradually (3). A microprocessor sector was formed as a part of the LES lab (4).

It can be argued that in the cases of Juku and Tartu the environmental stimulus was similar—a domain-related reform opening up multiple opportunities, but not prescribing any specific solutions. On the other hand, the working group in Tartu was much weaker and had no clear vision. As a result the network remained internal to the university for some time.

Case 2, episodes 2–4: explaining the failure to form the local network

IF a weak leader
presents a moderate vision
THEN the formation of the local network fails

Figure 4.13. Failure to form the network (Tartu)



Correspondence to the narrative is as follows: developments in the environment had enabled the team in Tartu State University to start developing microprocessor technologies (1). The team was small, it did not have many resources, prior practical experience with computer design or production preparation etc. (2). As the computer had been mainly developed for the university's own purposes, the idea of serial production was not coupled with a broad vision (in fact the movement towards a PC was gradual and likely influenced by discussions with potential partners) (3). Although sympathetic in theory, Baltijets required full technical documentation for production and another possible partner, the plant in Tartu, thought it impossible to fit the computer into the production plan (4). Despite some consultation no actual production followed (5), and hence no network formed around the Tartu project (6).

The case of Estron offers another entry point for theoretical refinement. If one accepts that the reasons why Estron did not participate were technical in nature for Juku and Tartu both (difficulties with preparing the production of the case, difficulties with the production as a whole) and attempts to explain these events in terms of the vocabulary already developed, one encounters at least two problems. First, it might be that the overall quality of the vision can suffer not because of the ambition and timeliness of the plans, but because of its ill match with participating organizations' interests, since both criteria are included in the definition of vision. One can make a case for Estron that this is an example of preferences changing after negative experience. Theoretically, it could lead to a somewhat counter-intuitive situation in which a clearly presented, ground-breaking vision classifies as a low-quality one only because it does not match the wants of other actors.

At the same time, the aim of the 'vision' category is the same: to point to the degree to which the socio-technical network is ready to act as a single whole. Therefore, it would be best to divide the defining criteria of the vision into two sub-categories: 1) strength—clarity, timeliness, realizability, scope; and 2) match—match with pre-existing commitments, expected pay-off, extent of new commitments, power to decide over terms of participation. Note that the difference between the two is not about objective/subjective or absolute/relative dichotomy, as it may at first seem: 1) the degree to which the vision can be deemed clear, timely or realizable depends on the judgement of the analyst; 2) 'strength' corresponds to the content judged from the perspective of the (changing) socio-technical context of the time, whereas 'match' is concerned with the viewpoint of the particular actor. Thus the vision can be strong context-wise but organizationally ill-matched, affecting the unity of the network.

Also, Estron was a notable organization but still small compared with local R&D leaders like the IoC or big plants like Baltijets, whether it came to available resources, established connections or a formal role. The fact that it encountered difficulties in computer production that led to moving on to other projects also means that the 'strength' of the partner should also be considered an important factor. The content of this attribute is similar to that of the socio-technical network as a whole. One can therefore clarify the above statements for Baltijets and the plant in Tartu as follows:

IF a weak leader
presents a moderately strong but ill-matching vision
to a strong partner
THEN formation of the local network fails

Whereas the episode with Estron could be described as:

IF a weak leader
presents a moderately strong and moderately matching vision
to a moderately strong partner
THEN formation of the local network fails

This would take into account the differences in the capabilities of actors plus the match of the vision with their preferences, while still explaining the overall outcome. Compared with the likes of RET and Baltijets, Estron can be deemed a moderately strong organization at the local level. Producing a personal computer in the first half of the 1980s was also quite a novel, challenging and interesting endeavour, although the vision was not very clear or wide in scope. And while Estron was strong enough to undertake the challenge (twice), it could be said that the lack of organizational strength affected its ability to participate. The ability to engage in other equally profitable deals that required less effort also meant that the organizational match was not high and thus Estron's commitment was easy to change, especially when the project had not progressed far.

Case 2, episode 5: explaining the failure to form the local network

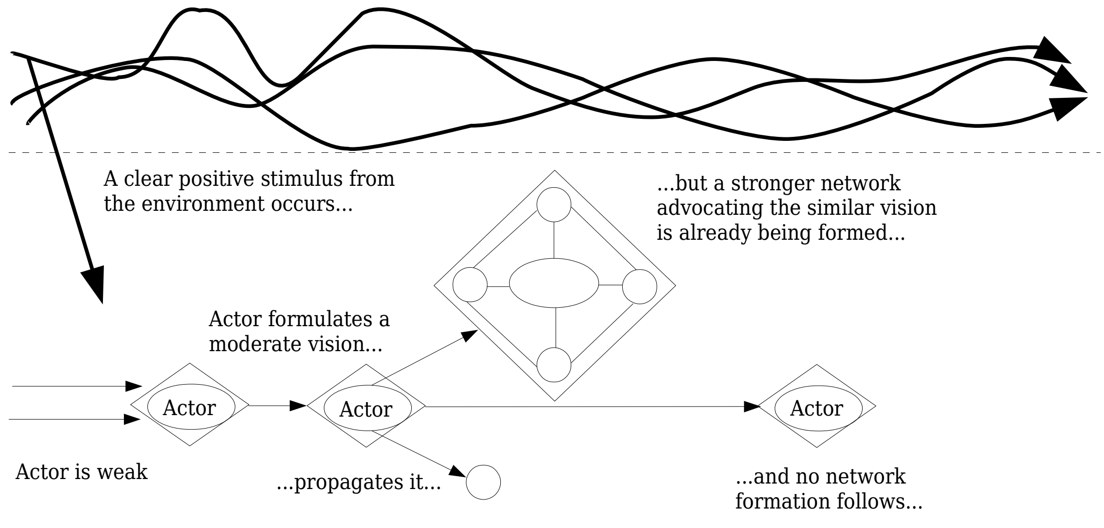
IF there has been a clear positive structural stimulus

AND a weak leader (with a prototype) that presents a strong vision

BUT stronger leaders are also propagating the same vision

THEN formation of the local network around the weak leader fails

Figure 4.14. Failure to form the network (Tartu)



Correspondence to the narrative is as follows: Tartu group had failed to find a producer for their prototype (1). School computerization provided an incentive (2) to 'update' the vision (3) and start to propagate Tartu as a potential school computer (4). However, the very same initiative was also taken up by the IoC who, as a stronger actor, managed to move much more decisively and quickly established a strong network itself (5). As a result the network failed to form around the Tartu project (6).

Another minor point of theoretical refinement: the prior analysis has worked on an assumption that the vision is specifically targeted. In Tartu's case there is no evidence about actual negotiations having taken place during this particular episode. Instead the idea was mentioned in a newspaper interview. This turns attention to the possibility that the vision can be general, uncoupled from a particular referent. The propagation of such a general vision can act as a signal to attract possible partners. Presumably it was the failure to achieve this that led the university to extend its search outside Estonia, resulting in a (temporary) migration of the project.

Case 2, episode 6: explaining the formation of the local network

IF a weak leader
presents a weak but well-matching vision
to a weak partner
THEN formation of a weak local network follows

Correspondence to the narrative is as follows: around 1988 Tartu group, having experienced delays in implementing production in Kursk, contacted Palivere (1). The idea was to produce 200 computers for schools—by that time an 8-bit computer had become considerably less of a technical challenge than it had been 5 years previously and it was probably also easier to obtain the components (producing a school computer while making a good profit also made it a good match with Palivere's interests) (2). Palivere could be characterized as a small player even among subsidiary electronics production enterprises (compared with Estron or Lääne Kalur), not to mention the large state-controlled factories (hence its characterization as a weak actor) (3). Cooperation between Tartu and Palivere followed (4).

The above process is very similar to that already shown in figure 4.8: an episode of successful vision-propagation from one actor to another, resulting in network-formation. The differences are in the strengths of both actors and the challenge presented by the project compared with the overall socio-technical context.

Case 2, episode 7: explaining the collapse of the local network

IF the local stabilized network in production phase encounters rapid and wide-ranging environmental changes

AND rapidly changing demands and preferences exceed the network's adaptive capability and the participants expect this to continue (so coping with future change in consumer preferences is deemed impossible)

THEN the network collapses

Correspondence to the narrative is as follows: Palivere started to produce the Tartu PC (1) but technology-wise the end of the Soviet Union meant an influx of newer and faster Western computers with decreasing prices while the existing supply chain was abruptly cut (2). The Tartu PC itself was not comparable with Western PCs (3) and upgrading the production facilities was deemed unrealistic (4). Hence Palivere dropped the project and moved on to search for a suitable market niche that matched its existing capabilities (5). The process is similar to one depicted in figure 4.12, but in this case the computer was already being produced and the network was much weaker.

The clarified categories can now be used to briefly revisit the case of Juku:

1) One could claim that the IoC's vision was compelling content-wise, but only moderate in match—both partners remained somewhat sceptical about the project—whereas the partners were strong (RET) and moderately strong (Estron).
2) The attractiveness of the strength of the vision decreased over time because of local developments and changes in socio-technical context. When production was delayed, the match decreased until the participants exited.
3) Whereas technically the project continued to be less and less challenging, it was still ambitious in its scope, which is why it can be still called moderately strong at the time when Baltijets was contacted. The match with the factory's interest was also moderate in the sense that the plant needed some computers

for their own production and something to produce as a consumer good, but was probably little or not at all motivated by the concerns of local identity.

4) The renewal of the network was at least partly about updating the technical content of the vision, but also keeping an eye out for new participants.
5) The rapid opening up to global competition meant that the capabilities of the network in relation to the socio-technical context quickly turned out to be insufficient to cope with the standards of Western computers. The vision lost its appeal both content-wise and match-wise as the preferences of the participants changed.

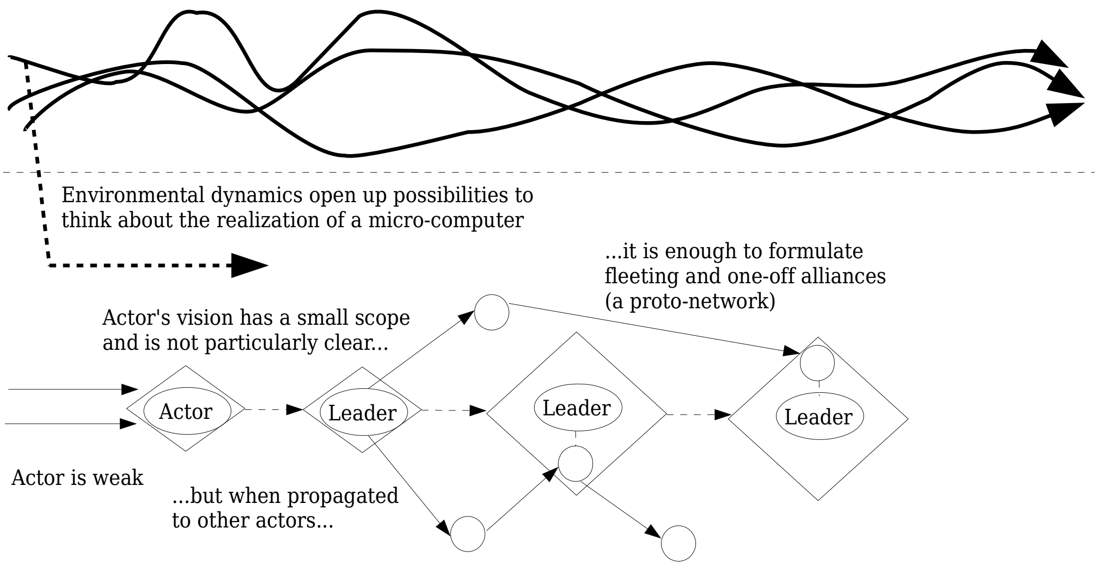
Case 3, episode 1: explaining the formation of the local network

IF environmental developments have opened up favourable opportunities

AND a weak leader (with a prototype)
presents a weak and moderately matching vision

THEN formation of a weak, fleeting producer–user network follows

Figure 4.15. Network-formation (Entel)



Correspondence to the narrative is as follows: 8-bit microprocessors started to be produced in the USSR, making access to them somewhat easier, while increasing importance was ascribed to microcomputing etc. (1). Entel group was small, lacked formal authority and had to rely on informal networks to acquire resources (2). The plan was only partly inspired by practical problems of the ministry, and professional interest was also a major motivating force (although the developments started quite early the PC was not imagined to have a wide circle of users) (3). Using its informal networks potentially interested customers were found (4). As a result a small-scale network preparing customized, one-off solutions for different clients came to be. In parallel, relations with partners assisting with certain elements of production were gradually established (5).

Why characterize the vision as weak in this case? On one hand, one could claim that as an early starter the content of the vision was quite strong in the technical sense. Also the fact that there were clients buying different one-off applications implies that there must have been a match with their interests. But then again, the scope of the vision was quite limited. For example, no mass production was envisioned and no preferred functional domain was specified. In fact, taking into account that the PC was adapted for numerous uses, with existing clients leading to new ones, it is even hard to speak of the group as having a vision at all or if so, then not a clearly specified one. On the other hand, the clients were not interested in establishing long-term relationships and thus the extent and durability of new obligations and commitments was low. Therefore, the interest was sufficient to participate, but not enough to establish more durable ties—hence the characterization of the network as both 'weak' and 'fleeting'.

Case 3, episode 2: explaining the failure to expand the local network

IF there has been a clear positive environmental stimulus
AND a weak network (with a prototype)
presents a strong vision
BUT stronger local leaders are also propagating the same vision
THEN expansion of the weak network fails

The case here is very similar to that of Tartu (case 2, episode 5). Entel participated in the school computerization project, but was far too weak compared with the network formed around the IoC. Juku remained the dominant line for a local school computer.

Case 3, episode 3: explaining the expansion of the local network

IF there has been a clear positive environmental stimulus

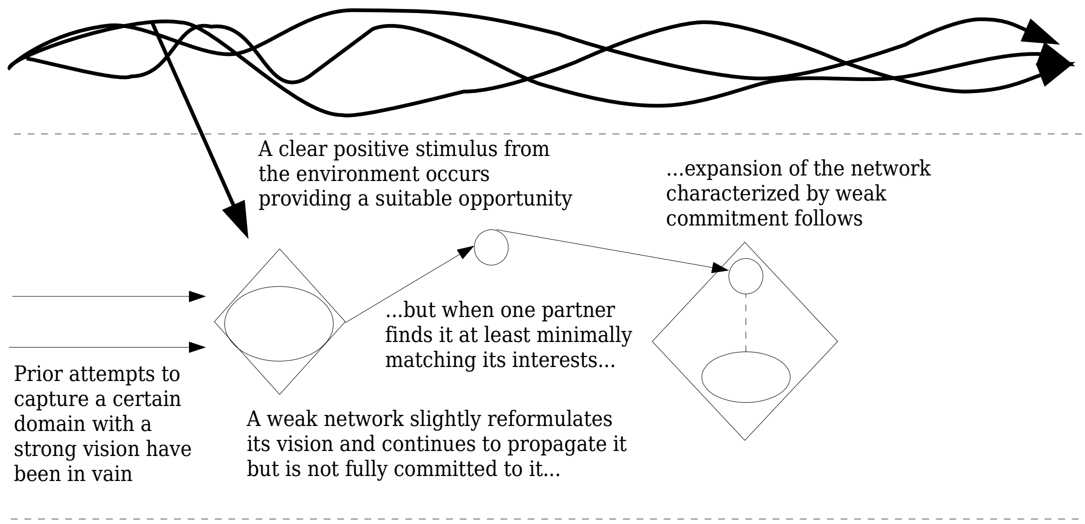
AND a weak network (with a prototype)

redirects a strong vision into a slightly different functional domain

and presents it to a strong partner

THEN expansion of the local network follows

Figure 4.16. Expansion of the network (Entel)



Correspondence to the narrative is as follows: by that time it was apparent to other players that Juku would become the main local school computerization design (1). The stimulus provided by the central authorities was still topical (2). The proposal to adapt Entel for vocational schools came from an enthusiast from the education sector (the fact that the education sector was divided between different administrative domains provided an alternative niche in which Entel could potentially prosper as the Juku project was mainly focused on the computerization of general secondary schools) (3). Demonstrations to teachers led to contact with the director of the Pöögelmann factory (4) who expressed interest in producing Entel as the plant's consumer good (5).

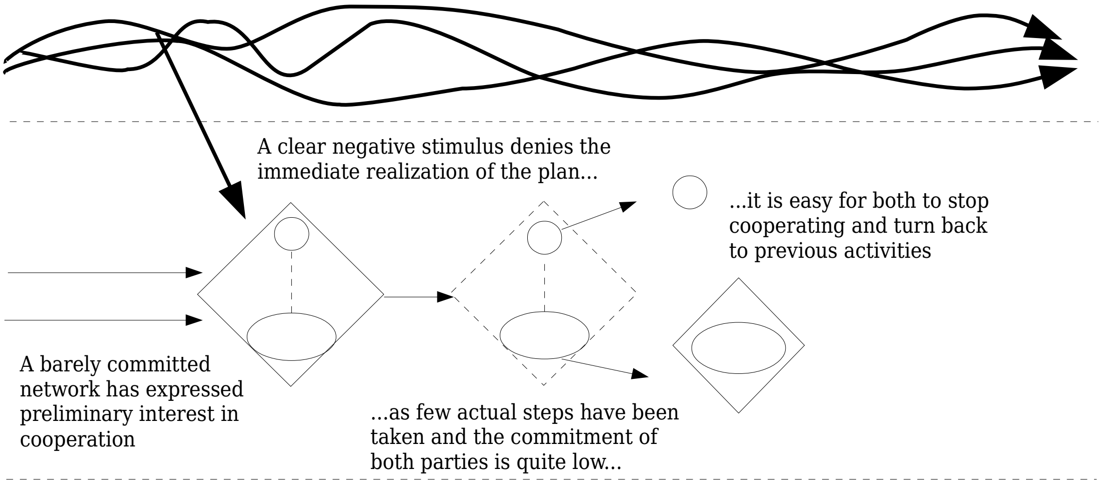
This is an interesting variation on the theme of the reaction to the failure to expand the local network. Whereas the Tartu people extended the search outside Estonia, the Entel group found a way to benefit from the existing structural conditions, retain the basic content of the vision (computers for schools) and continue the search locally. As a result contact with a strong actor was established. But this also directs attention to the issue of overall commitment. So far the use of strength and match as the criteria of the vision has been used to imply that the overall commitment of the network was at least moderate. In Entel's case the situation is different: first, the members of the Entel group themselves were not too enthusiastic about providing PCs for vocational schools, despite the enthusiasm of the member of the education sector. Second, the Pöögelmann plant was centrally controlled, therefore the interest from the director did not automatically guarantee actual production. The plant was also producing its own consumer commodities, so it was probably not hard-pressed to pursue the project. Therefore, one could make a case that although the network did expand, the overall commitment of the participants was actually quite low.

Case 3, episode 4: explaining the contraction of the local network

IF a moderately strong but barely committed local network (with a prototype) encounters a clear negative environmental stimulus

THEN the vision is weakened to a point at which the contraction of the network follows

Figure 4.17. Contraction of the network (Entel)



Correspondence to the narrative is as follows: the network linked the creators of the prototype to the possible mass producer and part of the education sector—the coalition was not as strong as that of Juku since it lacked wider support from the education sector and local politicians (moreover, the degree of commitment was also quite low) (1). The factory proved unable to acquire necessary components and the director was soon replaced (2). In these conditions at least the Pöögelmann plant and the Entel group did not want to pursue the possible mass production further, as realizing the project would have demanded much effort (e.g. dedicated lobbying) (3). Pöögelmann continued its existing production while the Entel group turned back to small-scale customized production and other activities of interest.

In many ways this description is quite similar to the contraction of the Juku network (figure 4.9). In both cases the stability of the initially formed network was compromised by network-specific negative signals from the environment which, denying the network an immediate realization of its goals, led to or accelerated the change of preferences of the participating actors. At a certain point the critical threshold was reached and some actors left the network. Differences can be found in the relative strengths of the networks and their degrees of commitment.

Case 3, episode 5: explaining the decay of the local network

IF environmental developments have opened up new possibilities

AND a weak local network with a relatively low degree of commitment does not see an easy way to cope with changes in future preferences

THEN decay of the network follows

Correspondence to the narrative is as follows: owing to the economic reforms it became possible to establish cooperatives, while new computers both Western and Soviet increasingly became available (1). The interests of the Entel group shifted to satellite receivers (2). As the production of the latter seemed more profitable and Entel was gradually becoming obsolete, no serious effort was put into upgrading the design of the PC (3). As a result Entels continued to be made on (decreasing) demand until the project was completely phased out by the end of the 1980s (4).

Compared with the other cases analysed so far, Entel offers an example of a phasing out of the network rather than a quick collapse. The reason can probably be traced back to the early start and the opportunistic nature of the leader. On the one hand, no sudden new opportunities to get the computer into mass production opened up. On the other, the changing interests of the leader meant that these were not actively sought for either. When other new more profitable opportunities for small-scale production appeared, a shift to other activities took place before the great social disruption. Therefore, the decay of the network was more gradual in nature.

Case 4, episode 1: explaining the failure to form/expand the local network[54]

IF there has been a clear positive environmental stimulus

AND a weak leader presents a strong vision

BUT a stronger local leader is already propagating the same vision

THEN no expansion of the network follows

54 I am treating KPI's two attempts to expand the network as parallel events as they occurred in a relatively short time-span. I am assuming that the order of these two events is unlikely to affect the overall explanation. Hence the ambiguity about whether the network is being formed or expanded.

Correspondence to the narrative is as follows: school computerization provided a stimulus to start thinking about the provision of personal computers for Lithuanian young people (1). Although KPI had good connections with various members of the industry and local politicians, it was essentially focused on scientific activities, had little experience with actual production and could devote only a small group of enthusiasts to develop the prototype (2). The idea to provide many young people with as many computers as quickly as possible in order to advance their computing skills could be considered a strong one in that moment, as the scope of the project was potentially wide (3). Local Minelektronprom authorities were proposing the production of BK-0010Š in mass quantities instead (4) hence Santaka did not gain full support from local politicians, remaining an unofficial parallel choice (5).

One can note similarities with the cases of Tartu and Entel, in that the functional domain was captured by a stronger actor. In this case, however, the stronger actor simply 'domesticated' a pre-existing design.

Case 4, episode 2: explaining the formation of the local network

IF there has been a clear positive environmental stimulus
AND a weak leader
presents a strong and well-matching vision
to a strong partner
not engaged in other networks with a similar vision
THEN formation of the local network follows

Correspondence to the narrative is as follows: school computerization initiative (1) was taken up by the KPI (2). The vision matched the KRMESRI's interest in technical challenges and the desire to contribute to local well-being—apart from that the extent of new obligations was not great (the scale of production was not clearly specified), meaning that the KRMESRI could devote only a fraction of its labour force to the task (3). The institute had experience with production according to military standards, a skilled labour force, finances, knowledge of compiling technical documentation etc. (4). On the other hand, the institute had no engagements with other local networks and was therefore available (5). Hence cooperation between KPI and the KRMESRI followed (6).

In some ways the episode is similar to Entel's attempt to cooperate with the Pöögelmann plant. In both cases there was a weak leader presenting a topical and attractive vision to a strong partner following a positive environmental stimulus. Nor were the partners engaged with similar projects at the time. The main difference is in the degree of commitment, which was very low in Entel's case but high in the case of Santaka. Thus the Santaka network might be called vision-directed compared with the more opportunistic nature of Entel, persisting after having experienced negative signals from the environment.

Case 4, episode 3: explaining the renewal of the local network

IF some environmental dynamics continue to unfold in an expected direction
BUT have not opened up any particular windows opportunity
this results in the gradual decrease of the strength of the local vision
while the local network remains otherwise committed to the task
THEN reorientation of the vision follows

Although direct evidence of this particular episode is hard to find, it is a logical implication of the first episode of Santaka's failed network formation/expansion. It is plausible to assume that as the initial and more ambitious probing for production possibilities failed and Lithuanian school computerization centred on BK-0010Š, the initial goal had to be somewhat toned down. In other words, if the goal was to produce as many computers as possible as quickly as possible, the production quantities had to be adapted to the capabilities of an already existing KPI-KRMESRI network. Otherwise the search for new partners and/or the official acquisition process would have delayed the project even further, while newer and faster computers would become available, decreasing the strength of the vision—theoretically up to the point at which the users would have no need to acquire the product at all.

Case 4, episode 4: explaining the failure to expand/the limited expansion of the local network

IF a moderately strong network (with a prototype)
presents a moderately strong but ill-matching vision
to a strong partner (not engaged in other networks with similar vision)
THEN no expansion of the network follows
OR expansion is limited

Correspondence to the narrative is as follows: the network of KPI and KMESRI (1) approached Sigma (2) with the idea that it would assist the network in producing Santaka. However, Sigma had little motivation to participate as it was occupied with its primary production while also producing its own consumer goods. KPI itself preferred to avoid possible delays with the production which would have followed had Sigma started to produce the computer as a consumer good in mass quantities. The match between the preferences of both was far from ideal (3). As a result, Sigma's role in the network was limited to the production of the case (4). One can note similarities with the IoC's attempt to cooperate with Baltijets (figure 4.10), but in this case the match between KPI–KRMESRI and Sigma was worse, leading to a more fleeting commitment and a more limited role.

Case 4, episode 5: explaining the decay of the local network

IF some environmental developments have continued in an expected direction
while others have not opened up fortunate windows of opportunity
AND the local network has stabilized on the path of normal diffusion
while the strength of the vision is gradually decreasing
THEN at one point the network decomposes/stops acting as a whole

Correspondence to the narrative is as follows: developments in the domain of computing meant that 8-bit computers slowly but steadily started giving way to 16-

bit computers (1). Environmental developments had not resulted in favourable opportunities to expand the network locally (2). About 200 computers were produced, allocated to schools and also copied among hobbyists. This resulted in various modifications and an expanding circle of people who obtained hands-on experience with the PC (3). After some search for other partners and opportunities at a very early stage, Santaka's vision had stabilized as a quick, temporary solution. The content of the vision did not entail the development of the new version (4). Therefore at one point KPI and KRMESRI had no reason to continue the cooperation. The former turned to other activities while the latter established successful contacts with Moscow on its own (5).

Case 5, episode 1: explaining the formation of the local network

IF rapid and wide-ranging environmental changes are taking place in the environment

AND a strong leader
presents a moderate and ill-matching vision
to a strong partner

THEN formation of a strong but weakly committed network follows

Correspondence to the narrative is as follows: amidst extremely rapid economic, political and cultural shifts, Sigma found itself in a situation in which the Eastern connections had been cut, but new ones had not yet been established. In order to sustain itself the organization needed to come up with marketable products fast (1). The organization was still a local industrial giant even, if its production infrastructure was outdated compared with Western companies (2). Sigma's suggestion to produce PC/XTs for schools was not very appealing as the possibility of obtaining newer and better Western computers was slowly emerging. In addition, Sigma was perceived as a remnant from the past, both technically and mentally. On the other hand, Sigma itself was only looking for a marketable product and was thus not necessarily attached to the PC project (3). As both partners were influential and there was a potential producer–user connection, the network might be called strong. However, as both were considering various other options in parallel, the interest in cooperation was lukewarm at best (4).

Case 5, episode 2: explaining the collapse of the network

IF rapid and wide-ranging environmental changes are taking place in the environment
opening up windows of opportunity
AND the the strength of the vision keeps weakening
while there are irreconcilable differences regarding the renewal of the network (with a prototype)
THEN the network collapses

Correspondence to the narrative is as follows: the rapid large-scale social transition was still ongoing (1). New chances to acquire Western IBM-compatibles opened up for the education sector (2). Sigma's quite expensive PCs using many Soviet components seemed less and less an appealing choice (3). The education sector increased its demands, requiring the computer to be 286 rather than XT. Apparently Sigma was unable or unwilling to fulfil this requirement (4). Sigma moved on to focus on other products, while the education sector shifted decisively to acquiring foreign computers (5).

Case 6, episode 1: explaining the failure to form the local network

IF environmental opportunities are scarce
AND a weak leader
creates a moderate vision
THEN the local network does not form

Correspondence to the narrative is as follows: although personal computing was establishing itself, foreign currency and foreign components were very difficult to obtain (1). An enthusiast from the Vilnius State University can be considered a weak leader in terms of available resources, finances, production capabilities etc. (2). The vision was technically demanding—building a PC/XT using as many Soviet components as possible. However, this made it difficult to realize and thus the vision was not specifically targeted. The vision was probably mainly motivated by technical and professional interest, lacking scope and clarity. For this reason I would deem the overall strength of the vision moderate (3). For some time the project was on hiatus—it had been demonstrated that the endeavour was possible in principle, but there were no resources to realize the vision owing to the lack of a suitable window of opportunity (4).

This case is intriguing because in all other episodes the content of the vision matched—or rather was from the beginning tailored to—the available resources. In the case of Lema, however, the exceptional circumstance of having to repair a Western PC led to the idea to create a mostly-Soviet PC/XT. Compared with the other cases, the acquisition of resources proved to be a much bigger problem because of the framing of the problem in a certain manner. Therefore this is a good example of a metatheoretical statement made in the first chapter—while the overall structural repertoire remains the same at a given point of time, the relevant problems might differ due to the selection of the actor. This explains why the environmental opportunities can be said to be scarce in this case but stimulating for other cases. In all other episodes the other weak actors were also struggling, but as they stuck to only Soviet components the scarcity of foreign currency did not make itself instantly visible. By choosing to frame the problem in terms of locally available and cheaper components they could choose a functional domain (school computing) where the opportunities had opened up.

Case 6, episode 2: explaining the formation of the local network

IF there has been a clear positive stimulus

AND a weak leader

has a moderate vision

THEN formation of a weak local network follows

Correspondence to the narrative is as follows: the university had managed to establish contacts with military representatives from Moscow who were willing to provide currency to build a WANG clone (1).[55] The leader and the vision remained the same, meaning that it would have been very difficult to consider the production of such a computer in mass quantities (2) but since necessary resources could now be acquired, a working group inside Vilnius State University was formed (3).

One can note that save for differences in the nature of environmental pressure and the strength of the vision, this episode is pretty similar to the formation of the Tartu network (case 2, episode 1). Paradoxically, in this case it was probably the strength, not the clarity of the vision (a in Tartu's situation) that inhibited the formation of a stronger network, as the needed resources were difficult to obtain and potential producers would not have even considered the project seriously. In both cases, however, the end result was the same: a weak, university-internal local network with the university itself acting as a cover organization (providing conditions for the project to continue).

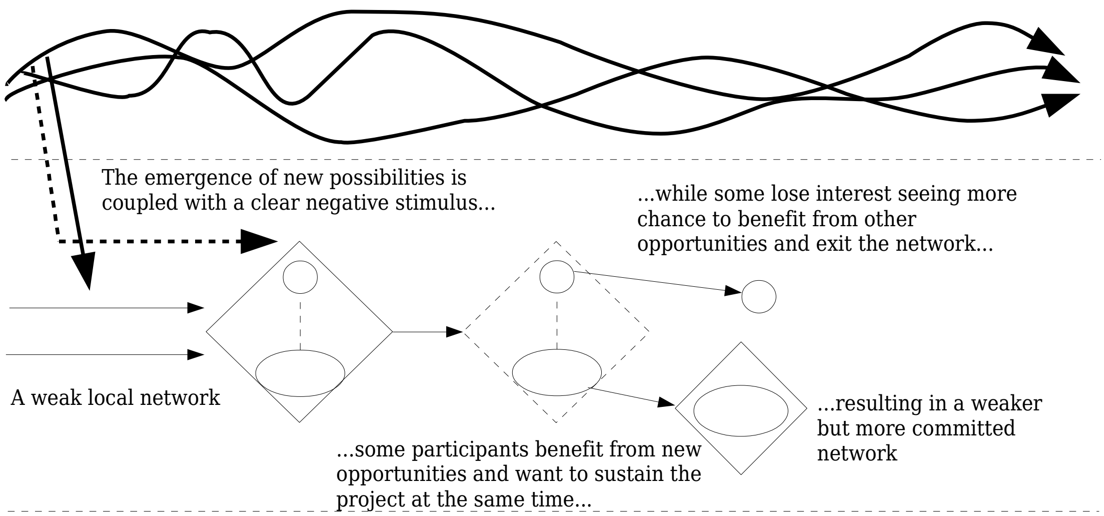
Case 6, episode 3: explaining the reconfiguration of the local network

IF the weak local network (with a prototype)
encounters a clear negative environmental stimulus
while environmental developments have opened up new possibilities for all participants

THEN the match of the vision decreases to a point
at which the existing network contracts a new one comes to being

55 Note that since the analysis was geographically delimited in a certain manner the relation between Vilnius State University and the partner from Moscow is being treated as one between the network and its environment.

Figure 4.18. Reconfiguration of the network (Lema's PC/XT)



Correspondence to the narrative is as follows: the university people working on the project were few (1). The partner from Moscow lost interest in the project (2). The working group had already prepared the prototype and diagnostic tools. The possibility to establish cooperatives gave a chance to generate profit from this product (3). Vilnius State University itself had lost interest in providing institutional support (4). The group that designed the PC/XT formed Lema cooperative to start the production (5). As the overall strength of the network decreased, one could claim that the commitment increased, because on average the remaining members were motivated to put in more effort into the realization of the project.

Case 6, episode 4: explaining the expansion of the local network

IF rapid and wide-ranging environmental changes are taking place in the environment

AND a local weak leader
with a moderate and (at least) moderately matching vision
expects the changing demands and preferences not to exceed the network's adaptive capability (in the short term)

THEN formation of a weak, fleeting producer–user network follows

Correspondence to the narrative is as follows: overall social transition once again provides the context, opening up a number of opportunities to make a quick profit—a right to make transactions in foreign currency and thus to obtain components from abroad, overstocked factories selling their components for decreasing prices because of the combination of fixed prices and inflation (1). Lema was a small cooperative mainly focused on design and prototype-building of various customized applications, its PC/XT among them (2). While the technical appeal of the project had decreased, Lema still thought it possible to find interested buyers. The enterprises which could not afford to buy a 'real' Western computer were still many at the same time, even if they were not interested in long-term cooperation (beyond warranty service) (3). Therefore it was expected that the PC could still be marketed for a certain period of time (4). The formation of a small fleeting producer–user network resulted, probably quite similar to that of Entel's (case 3, episode 1) (5).

Case 6, episode 5: explaining the decay of the local network

IF some environmental dynamics continue to unfold in an expected direction
BUT have not opened up any particular windows of opportunity
AND a weak local network has stabilized on the path of normal diffusion
while the strength of the vision is gradually decreasing
and there is no easy way to cope with change in preferences
THEN decay of the network follows

Correspondence to the narrative is as follows: transition to a market economy made it easier to pursue various projects with foreign partners. These new deals were more interesting professionally and also more profitable. The developments in the domain of computing resulted in better Western computers (1). Favourable conditions of instability were disappearing, along with the closing down of factories and re-profiling of the industry (2). Lema computers were still being produced on demand for various partners (3) with the prices of Western computers decreasing and newer ones arriving, fewer customers were interested in the product and thus the project became less relevant (4). Therefore it was easier to profit from new projects rather than attempt to upgrade the old one (5). As a result the production was gradually phased out (6).

Above I have noted the exceptional nature of this case compared with others, in that initially the requirements of the project tended to exceed the opportunities available for the leader. This illustrates a point made in chapter 2: a selection bias of the cases towards more or less realized projects. Therefore, it must be kept in mind that in cases in which the network-building did not take place or was abandoned early on, the importance of various factors or the factors themselves may be different from those highlighted here. Further work on vision-formation could help to specify the conditions under which the opportunity–vision balance is such that the project is deemed viable.

4.2.4 Making sense of the findings

At this point I will stop adding more detail to the analysis, because the complexity of the last step raises enough questions about the significance of the findings on its own. Additionally, I think that the principle and viability of top-down analysis has been sufficiently illustrated. Therefore, I will now attempt to synthesize the results and make some theoretical propositions regarding the evolution of socio-technical networks. In so doing I will also draw connections with different outcomes of theorizing from the narratives, as depicted in figure 4.2.

First, why focus on episodes? More specifically, what defines an episode? Generally, each describes a particular interaction. But can these interactions be grouped in any way? Closer inspection reveals that in all episodes related to the change in the size and shape of the network, the basic underlying processes can be described as variations on the same theme:

1) The formation/expansion of the network:
    a) A presents a set of preferences to partner B;
    b) B changes its preferences;

c) the subsequent actions of A and B are based on at least some shared preferences (i.e. aligned to each other).

2) Unsuccessful formation/expansion:

   a) A presents a set of preferences to partner B;

   b) B does not change its preferences;

   c) the subsequent actions of A and B are not based on shared preferences (i.e. not aligned to each other).

3) Contraction/decay/collapse of the network:

   a) the actions of A and B are based on at least some shared preferences (i.e. aligned to each other);

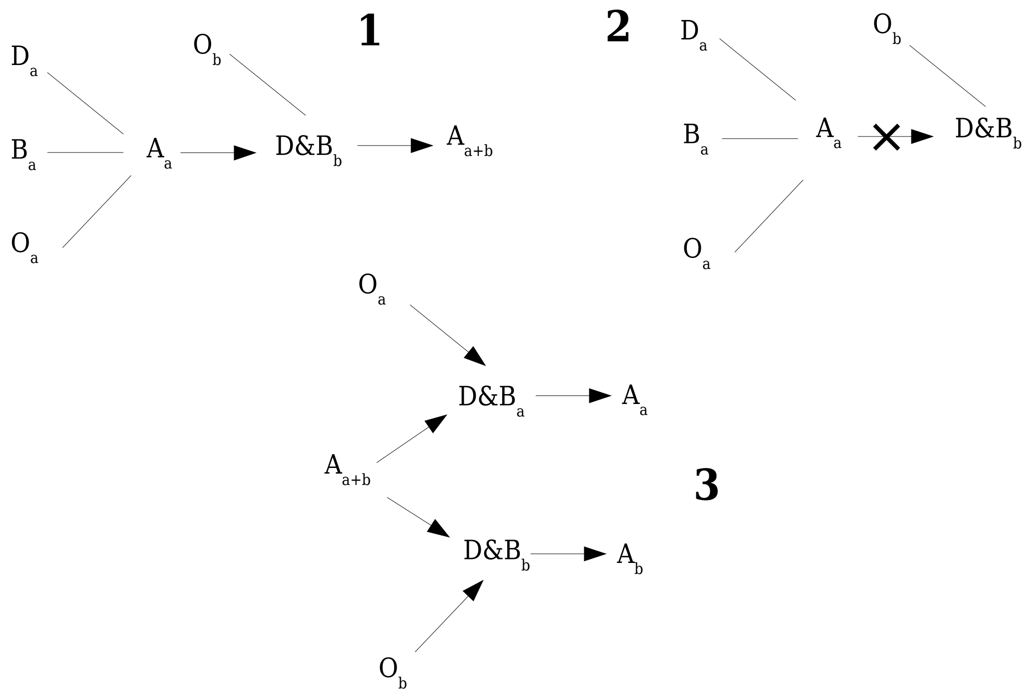
   b) A and/or B change their preferences;

   c) the actions of A and B are not aligned to each other anymore.

Figure 4.19 depicts all three situations. D stands for the desire to engage in a PC project, B denotes the beliefs about the way to proceed, and O signifies the existing opportunities to do so. Subscripts designate different actors, straight lines indicate the characteristic desires, beliefs and opportunities of these actors and arrows show how the influence is transmitted from one actor to another.

Note that the translation of these statements into DBO vocabulary reveals that no distinction is being made as to whether the actions of actor A affect the desires or the beliefs of actor B (in other words, whether the presentation of the way to proceed evokes a desire to do so or whether the desire to proceed leads to the specification of relevant activities). Also, prior experience and new opportunities are both allowed to shape the preferences of the members of the network (that is, the cooperation experience within an existing network might not necessarily be negative, but the opening up of novel possibilities can nevertheless lead to its contraction). However, it must also be noted that these formulations go beyond detecting the changes in the size and shape of the network over its course of development and identify explanations as to why they occur. As such they can be considered generative mechanisms of network evolution (outcome three on figure 4.2).

Figure 4.19. Three basic processes underlying the dynamics of socio-technical networks



This, of course, does not indicate the conditions in which these mechanisms are realized. In other words, the underlying processes are not contextualized. Over the course of the analysis a number of such factors were identified. These can be characterized as attributes of certain variables:

1) Type of environmental change (structural opportunities, network-specific shock, domain-related reform, avalanche).
2) Phase of development of the project (no clear vision, vision, prototype, readiness for production).

Additional properties of the leader/network were as follows:

3) Strength of the leader/network (weak, moderate, strong).
4) Strength of the vision (weak, moderate, strong).
5) Match of the vision (incompatible, ill-matching, moderately matching, well-

matching).

6) Strength of the partner (no clear partner, weak, moderate, strong).

In some episodes the importance of additional factors became visible:

7) The extent to which the niche had been occupied by other players/networks pursuing similar vision.
8) The extent to which capable actors were still available for negotiation in the locality.
9) Expectations about the possibilities of continuing the project (short-term, long-term).
10) Prior experience of the performance of the network.

Such a characterization in which the realization of the mechanism is dependent on the values of background variables corresponds to outcome four on figure 4.2. However, in order to test the relevance of each and their possible clustering on the probability space, a statistically representative sample of episodes is required. The current selection of cases is not suitable for this purpose: it was observed that network-formation, (non-)expansion and contraction took place in different phases and under the influence of different types of environmental change. In other words, the variation was too large, the variables too many and the cases too few to make any statistical generalizations.

Does this mean that the synthesis has to stop here? Not at all. Because so far no attention has been paid to the possible differences in the realization of the underlying mechanisms dependent on the timing of interactions between the environment and the network, i.e. outcome five on figure 4.2. In that regard nine propositions on the patterns of intra-case evolution can be presented:

1) The lack of environmental pressure means that there is no particularly suitable window of opportunity for any socio-technical networks to emerge. The development is mainly motivated by technical interest, solutions customized and/or local (for solving specific problems). Since no particular

functional domain has opened up it is difficult to raise the interest of partners, even if this is sought. The process of network-formation is likely to be drawn-out, with the vision emerging gradually and the ties between the participants weak and fleeting. Examples: Tartu, Entel, Lema's PC/XT.

2) When there is no environmental pressure and the network has already entered the phase of production, gradual phase-out of the project is likely. Lacking suitable windows of opportunity, the producers are likely to move on to other projects rather than attempt to start another product cycle. The product can still be produced on demand, but the number of new users decreases while the number of users switching to other products increases. Examples: Entel, Santaka, Lema's PC/XT.
3) Domain-related reform opens up a space of opportunities. As such it guides the attention of local players to a certain functional domain. At the same time the effect of the stimulus is limited, leaving other social domains largely intact (at least in the short-term). Shared stimulus and general stability create the conditions for the emergence of strong networks. Strong players with a clear vision enter. Network-formation and initial development is quick. Examples: Juku, Santaka.
4) If domain-related reform occurs while the leader/network already has a prototype, a re-domaining strategy can follow. This involves the leader/network quickly changing its vision about the domain of use, along with possible modifications to the prototype to try to take advantage of changed environmental conditions and its (newly emerged) early lead. If the niche is already occupied, another re-domaining attempt (different functional domain or geographical location) can be made. Examples: Tartu, Entel.
5) By contrast with domain-related reform, avalanche change means major shifts in various opportunity structures. Multiple spaces of opportunities open up and disappear simultaneously. In these conditions there is a need to act quickly, but there is also much uncertainty about the best course of action. The process of network-formation is rapid, but the ties between participants remain weak as the actors keep scanning the environment for more

favourable opportunities. Example: Sigma 8800.

6) If avalanche change coincides with the (still up-to-date) production phase of the network, 'riding the wave' can be attempted. In other words, temporary advantages provided by the context of rapid change are mobilized to earn quick profit from various applications. The process is quick, the network fleeting, and production is likely to be small-scale and applications customized to specific users. Example: Lema's PC/XT.
7) More often than not, however, avalanche change has a destructive impact on the network regardless its phase of development. Uncertainty and the urgent need to act lead the participants to frequently changing preferences about the ways in which to achieve their goal (computerization) or the desirability of the goal itself (computer production). Abrupt disintegration follows and each participant continues on separate paths of opportunistic survival. Examples: Juku, Tartu, Sigma 8800.

Whereas in all of the above cases the environmental conditions affected a group of players simultaneously, the network-specific shock is different by definition. However, it can still have an indirect impact by signalling other players as to whether there is room to be occupied in the functional niche or not and/or what kinds of strategies are likely to bear more fruit:

8) The role of a positive network-specific shock is similar regardless of the exact phase of (early) development (vision or prototype). In the first case it enables the creation of a local niche in which a technically more demanding product can be developed. In the second case it enables the stabilization of the network on the path of 'normal diffusion' (gradually expanding producer–user network). In both cases it enables access to more resources, increases the certainty about the pay-off of the chosen direction and presumably leads to an increase in the commitment of the leader/network. In sum, it enables the network to pass from one phase of development to another, working as a catalyst. Examples: Lema's PC/XT, Juku.
9) The role of a negative network-specific shock has the reverse effect: by

inducing the delay it enables the network-internal discrepancies to (fully) develop. It denies access to resources, increases the uncertainty about the pay-off of the chosen direction and presumably decreases the commitment of the leader/network. Weakly committed participants leave early, whereas more committed ones stay longer until the technical appeal of the vision has decreased to a point at which questions about the meaningfulness of the project start to emerge. If the vision remains unchanged, contraction or dissolution of the network follows. Examples: Juku, Entel, Lema's PC/XT.

As seen from the above analysis, each of the cases experienced some twists and turns over its course of development. Therefore, it was analytically meaningful to decompose the evolution of each PC into a number of episodes and formulate propositions about these episodes instead. However, this obscures the possibility that the overall development of the cases might at least partly result from the differing characteristic traits of the networks. Pursuing this logic further, one could distinguish between two major types of socio-technical network:

1) Opportunistic network—the preferences of the participants and the overall vision are changing frequently and the network tries to make the best use of changes in environmental opportunities. On one hand, this means a certain flexibility is available and quite possibly multiple successful attempts to find different niches of application (re-domaining) can be made. On the other hand, the commitment of the network is unlikely to remain strong for an extended duration of time and will be easily affected by negative environmental stimuli. The users are willing to change their ideas about the best course of computerization, and the producers are ready to abandon the project and move on to others. I would characterize Tartu, Entel, Lema's PC/XT and Sigma 8800 as opportunistic networks.
2) Vision-directed network—here the preferences of the participants are more durable as the project is inspired by a single environmental stimulus. The problem is clearly defined in relation to a specific functional domain and the scope of the project is ambitious, requiring the formation of a broad

consensus and a strong network in order to be fully realized. On one hand, it gives the network a capability to move decisively towards a certain goal: as the participants are more committed, the network is more resistant to environmental stimuli. On the other hand, the network can become characterized by mission blindness: some of the participants can be unwilling to re-think not only the goal, but even the ways in which this goal can be achieved. I would characterize Juku and Santaka as vision-directed networks.

It follows that these two types should differ somewhat in their responses to environmental stimuli:

1) Opportunistic networks are likely to react to minor chances of diffusing their product and to adopt narrower niches. Therefore they are likely to emerge even when there is no particular environmental pressure. By contrast, vision-directed networks are less likely to appear from scratch.
2) Vision-directed networks emerge after the occurrence of domain-related reforms and are more likely to prevail over an extended period of time, whereas opportunistic networks are more prone to follow a re-domaining strategy (jumping on the bandwagon, staying if immediately possible, exiting early if not).
3) Avalanche change has a negative effect on both types, triggering an 'every-man-for-himself' strategy. However, opportunistic networks should be able to adapt better to changing conditions and their survival should be more likely. New networks that emerge during avalanche change are likely to be opportunistic, whether suited for pursuing this strategy or not.
4) Positive network-specific shock has a similar effect on both types. If such a shock is negative, opportunistic networks are more likely to collapse earlier.

Although these strategies clearly depend on actors' choices, I would also like to point out that the distribution of opportunistic and vision-directed networks is most unlikely to be random. Instead it is sensible to presume that structural conditions affect the choice for one or another. For example, if the leader's initial structural position is unfavourable, it is easier to choose an opportunistic strategy because otherwise much more effort would have to be put into vision-directed network-formation (e.g. programming a lot of software, providing full technical documentation, convincing the big local players to join in, lobbying activities directed at central authorities etc.). The implications of the differing capabilities of different players for their actions were well-recognized by the interviewees themselves. The most explicit example comes from the leader of the Tartu team, commenting the efforts of the Juku group to get the computer into mass production: *"The fact that they were struggling shows exactly that we did not have this kind of competitive power by far"* (Humal interview). The exact specification of the background conditions in which different kinds of networks come to be would once again require statistical analysis, however.

At this point some readers may feel that the enquiry has not gone far enough, remaining too descriptive: more depth, more detail, and more explanation would be required. The theorization has excluded and simplified various aspects and much of the complexity has remained untapped—there are so many fascinating black boxes still to be opened. I would agree with everything in the previous sentence. However, instead of initiating yet another round of analysis, I would like to address the accusation of descriptiveness in more detail. In fact, I want to put forth the following, seemingly paradoxical argument: a demand to decompose every phenomenon into the most detailed interactions between smallest units of analysis possible can easily lead to an impoverished overall theoretical understanding because it sets very high (if not completely unrealistic) expectations for the researcher.

The critique of descriptiveness is one of the recurring themes in social science: instead of explaining why the phenomena occur, social scientists allegedly too often stick to describing how they do so, thus conveniently avoiding real causal explanation. Alas, what is how and what is why depend on how the question is presented. The progression of the above analysis can be taken as a convenient example:

1) To begin with, one can focus on observing how different cases evolve and note some regularities, e.g. the alternation between expansion and contraction in case of Juku.
2) This observation can be criticized on the grounds that it does not explain why these alternations occur.
3) By probing further, finding key nodes and recurring key node sequences, one could claim in response that 'a contraction of the network was a result of a negative environmental impact followed by actors exiting the local network'. One can probably agree that this constitutes a sort of an answer to the why-question—an overall change is explained in terms of smaller sub-changes.
4) However, one can also quite correctly point out that the sequence itself does not necessarily imply causal connection. Thus we should be asking why the actors decided to exit the network in the first place.
5) Now one can offer an answer in terms of an environmental stimulus inducing a delay, during which the structural properties of the environment changed to the extent that some actors decisively changed their preferences and exited the network.
6) Alas, this observation can again be criticized on the grounds that 'changed preferences' is an umbrella term for an array of processes and should therefore be explained itself.
7) In response one can identify the following event sequence: a) an actor with a set of desires regarding the goal and beliefs about the ways in which to achieve the goal; b) searched for potential alternatives; c) changed its beliefs about the best way to achieve the goal; d) attempted to convince other actors of the network about the supremacy of its alternative; e) having failed to do so, changed its desire about the goal and; f) exited the network altogether.

I will stop here as the problem with such reasoning has become abundantly clear already. Yes, each how–why cycle leads one closer to the data and increases the empirical accuracy, but at the same time the overall sequence continues to be cut into shorter and shorter bits and the unit of analysis decreases: in this example from organizations contributing to network dynamics to organization-internal dynamics contributing to a certain organizational outcome. In fact, every outcome can always be decomposed into a conjunction of various lower-level events, each of which in turn is a result of yet another event sequence. In practical terms, the descriptions set out in point 7 require considerably more work and historical detail than the ones in point 5, while the overall outcome to be explained (stated in point 1) remains exactly the same. To illustrate it even more bluntly: if the analyst decides to limit the lowest level of explanation to the change in preferences, the mechanisms of preference-formation are simply omitted from the analysis. The latter, although theoretically useful, are excluded for the practical purpose of managing the research.

In my view, there are at least three reasons why the *a priori* demand to explain in as much detail as possible is unsatisfactory and at times even counterproductive:

1) Methodological and practical limits to the availability of data and duration of data collection—for example, an ethnographic observation, no matter how detailed, does not and cannot reveal neurological causes of exerting agency, nor does it need to do so to give a useful account of some social phenomenon.
2) The logical expectation that as the distance between the phenomenon to be explained and the elements making up the explanation increases, i.e. more and more units of analysis and interactions between them are taken into account, the amount of information becomes increasingly difficult for the researcher to handle. And when every episode suddenly seems conditional on the myriad of factors, there is a grave temptation to claim that higher-level patterns are illusory, to abstain from explaining them altogether and/or resort to somewhat bland statements to account for the situation as a whole like 'the interactions are complex and unpredictable', 'mutual shaping and learning continuously takes place' etc.
3) Finally, one could claim that historically STS itself has suffered from this attitude. Its traditional focus on the local, the complex, the contingent, the fluid and the uncertain has largely been unsuccessful in leading to meso- or macro-theories with a strong micro-grounding. The original call of Pinch and

Bijker (1984) to 'open the black box' of technology was soon accompanied by 'closing it behind you' (Williams and Russell 1987) or 'finding it empty' (Winner 1993) to hint at these difficulties. Subsequently the micro-bias of STS has been a recurrent theme in an array of reviews and criticisms, e.g. Russell (1986), Williams & Edge (1996), Rammert (1997), Klein & Kleinman (2002), Russell & Williams (2002), Brey (2003), Bruun & Hukkinen (2003). To date STS has continued to struggle with the issue so that only recently Wyatt and Balmer were still complaining that *"even the best writing in the field ... could provoke such uncharitable thoughts as: What is this a case study of? What does it add to our understanding of different concepts? Interesting as the story itself may be, how does it contribute to discussions of anything beyond itself?"* (2007: 619–620).

To recap: 1) in my opinion the large gap between complex narratives and very general theories often found in STS case studies is exactly a result of a (possibly implicit) *a priori* preference for minute detail; 2) insisting on going into as much depth as possible and starting to solve the problem 'bottom-up' threatens to maintain that gap because in the mess of information generalizations of a very high level are the easiest to make. On the other hand, 3) increasing accuracy is indeed preferable in the long term because it enables the establishment of a closer correspondence between the events of the narrative and theoretical categories.

Above I have made the point that mechanism-based explanation is a thoroughly epistemological affair: the way in which mechanisms are defined and the amount of detail involved depend heavily on the choices of the researcher. I also warned against going in depth too early, as to do so might obscure larger patterns. Instead I offered a way of gradually increasing the degree of detail of the analysis. However, one may wonder whether I have zoomed in too close myself. Perhaps the amount of detail brought in has already made it difficult to notice some interesting higher-level regularities? In other words, would we see something different if we focused on the interaction of socio-technical networks instead?

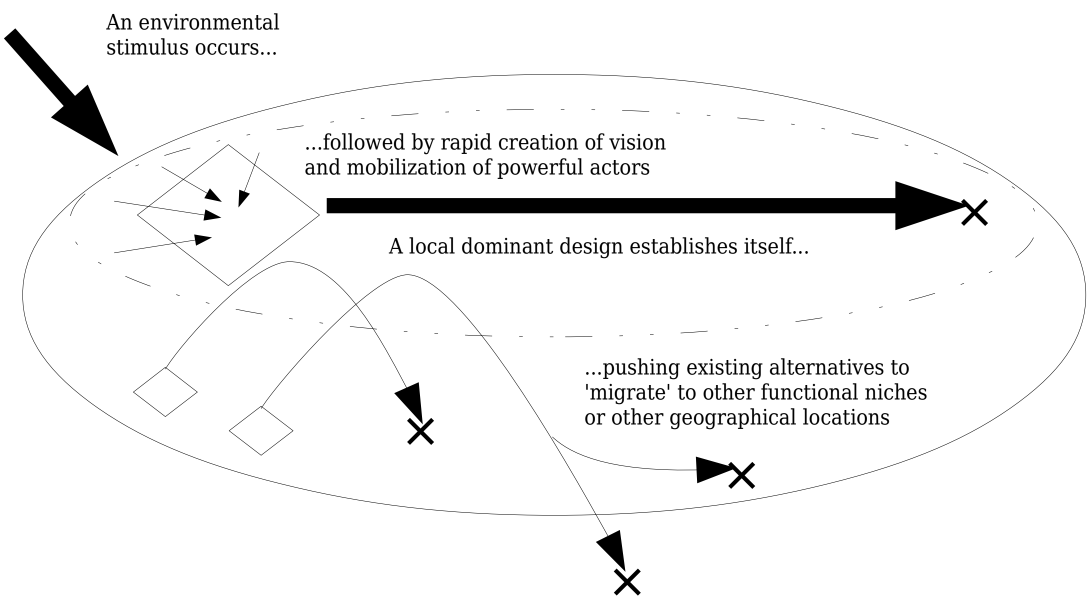
4.3 Inter-case analysis

In this section I will start by asking how Estonia, Latvia and Lithuania responded to the school computerization initiative. Following this logic, three different patterns can be identified, one for each country. It has to be stressed here, however, that 'inter-case' is not meant to signify the comparison of different cases (which, to some extent, was already done in the previous section). Instead this term is used as a shorthand for 'interactions between cases resulting in particular collective outcomes' which in this context entails focusing on the emergence of local dominant designs for school computers.

Estonia: local dominant design pathway

IF an environmental stimulus
is followed by the rapid creation of local vision and alignment of actors (including powerful ones)
THEN a local dominant design emerges
pushing alternatives from the functional domain

Figure 4.20. Local dominant design pathway

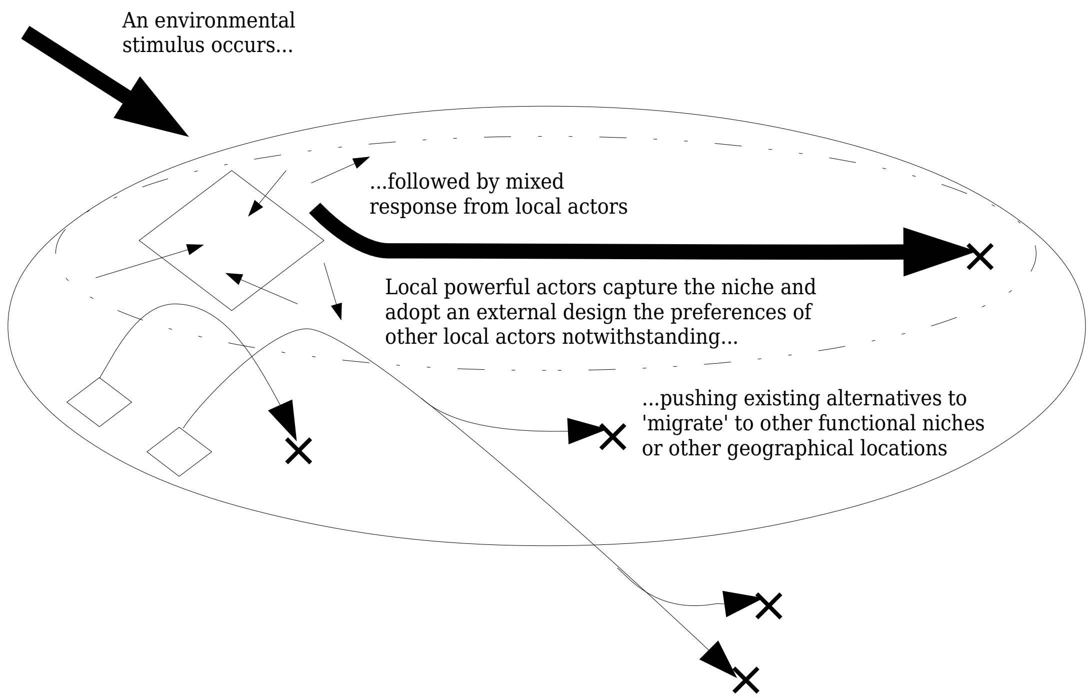


Correspondence to the narrative is as follows: the command to start teaching informatics was quickly taken up by a potential designer (the IoC), producers (RET, Estron), and was supported by academics, the education sector and local party officials, owing to intensive lobbying. The emergence of Juku as the main school computer led Tartu and Entel to search for other niches of application (e.g. Entel for vocational schools) or to search for a similar niche elsewhere (Tartu in Kursk), even though both were conceived and prototyped before Juku.

Lithuania: external dominant design pathway

IF an environmental stimulus
is followed by a mixed response from interested but less powerful local actors
THEN the niche can be captured by interested and more powerful actors with their own existing design, not requiring the consent of others
pushing alternatives from the functional domain

Figure 4.21. External dominant design pathway

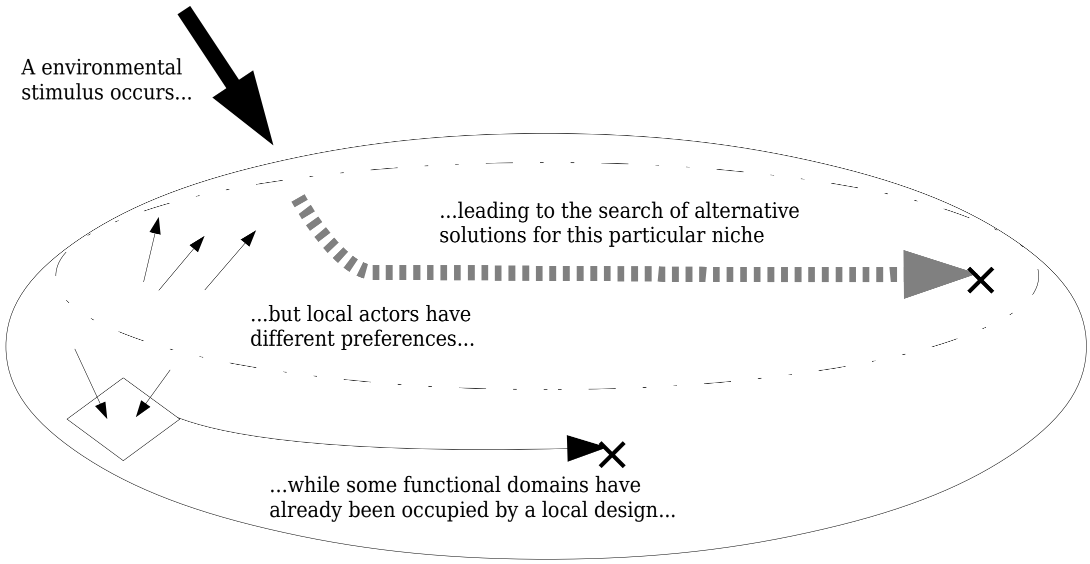


Correspondence to the narrative is as follows: each of the three academic centres responsible for implementing informatics in schools had somewhat different ideas about the best ways to do so. As a result, no broad coalition supporting a single vision emerged. The centres were also excluded from formal decision-making. The local authorities remained indecisive at first, but were then convinced by Minelektronprom's authoritative proposal to adapt the existing design of BK-0010Š and start mass production in the Nuklonas plant. Santaka remained an unofficial parallel in Lithuania, later migrating to a home computer niche in Minsk and Krasnodar.

Latvia: non-intersecting pathways

IF an environmental stimulus occurs
and there are already local existing designs in some functional domains
BUT actors from either domain are not motivated to engage in network-building
THEN parallel development continues, with different possible solutions and outcomes in both domains

Figure 4.22. Non-intersecting pathways



Correspondence to the narrative is as follows: the VEF plant had been developing its microcomputers for industrial applications since the 1970s. The education sector also had an early start, but the Laboratory of the Problems of School Informatics decided to acquire as many computers as possible wherever they were produced and to focus on software production. Despite some initial contacts, none of the parties were pursuing the issue of local PC production very actively. As a result, local computer production efforts remained largely separate from the school computerization process.

Although highlighting important differences between the countries, the patterns are not very revealing in some aspects. Because if they are patterns of something, then what exactly? In other words, is there an underlying mechanism, or even several? In order to tackle this problem, I will begin by noting the commonalities and differences between the three countries. The question of which of these factors were actually important and why gets us closer to the driving processes. Table 4.3 lists eight such factors implicit in figures 4.20–4.22.

Table 4.3. Differences and commonalities of school computerization in the Soviet Baltic countries

	Estonia	Latvia	Lithuania
Presence of a central stimulus for informatics teaching	√	√	√
Perceived need for school computers	√	√	√
Existence of local visions/prototypes	√	√	√
Industrial production capability	√	√	√
Prior experience with serial production of PCs		√	
Motivation to engage in local network-building	√		√
Did the dominant network form around the strongest leader?	√		√
Was there a general unity of preferences in the locality?	√		

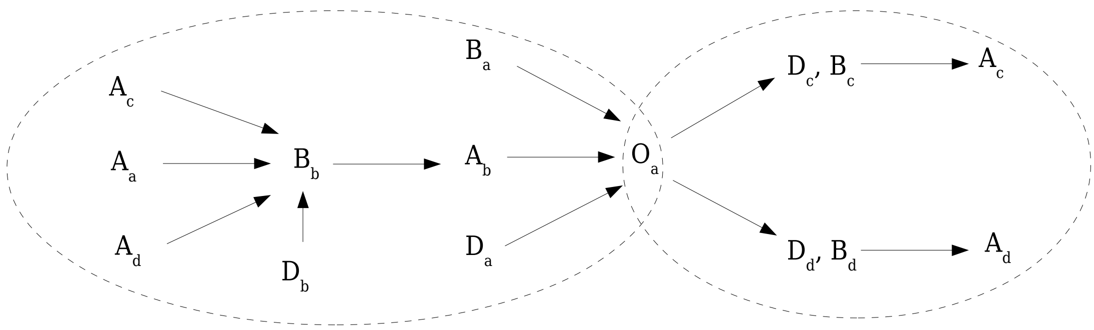
It is interesting to note that if only the first five criteria are considered, the observed patterns run counter to what one might expect. By the mid-1980s Latvia was the only country with years of experience in serial microcomputer production; Lithuania, while not having such experience, did have a computer industry; while Estonia had neither. Based on this situation, one would expect that it would have been easiest for Latvia to create and produce a domestic design, whereas Estonia should have had to rely on acquiring computers produced elsewhere in the USSR. But actually the reverse happened: Estonia went for local design and production, Lithuania adopted an external design but started local production, and Latvia did neither. Therefore, other reasons related to local choices had to be in play to even out the differences in the starting points of each country.

The first distinguishing characteristic is related to the framing of the problem. In Estonia and Lithuania the issue of school computerization was quickly associated with local PC production as one of the ways to overcome the scarcity of computers. In Latvia no existing or proposed solution seemed attractive enough to the key members of the education sector, while the local decision-makers and producers were not showing much initiative either.

This would explain the difference between Latvia and the other two countries. However, in Estonia and Lithuania some local network-building building took place. And in both countries it was the strongest leader that established the main line of school computers: the Institute of Cybernetics in Estonia and Venta institute/Nuklonas plant, as Minelektronprom representatives, in Lithuania. It seems that the main difference can be found in the unity of local actors. In Estonia the IoC people initially managed to establish a broad alliance between designers, producers, users and decision-makers. In Lithuania the preferences of the education sector were more fragmented and remained so. As the strongest participant with central support, Venta/Nuklonas could capitalize on the lack of local consensus and obtain the approval of the decision-makers. Lacking serious resistance from the user side, Nuklonas could take the easiest route by simply adapting the existing design of its ministry, which was already being centrally advocated as a school computer anyway.

Let me now attempt to formalize these considerations using the DBO framework once again. I would start with the actions (A_a, A_c, A_d), of the leaders of different networks, directed towards the decision-makers who desire to computerize schools (D_b), although at that point they may not have any clear beliefs about the best ways to achieve that goal. It is the action of the leader (e.g. lobbying, presentation of the vision) that leads the decision-maker to form new beliefs or to alter the existing ones about school computerization (B_b). Subsequent official or unofficial backing from the decision-makers (A_b) changes the opportunities available for the initial actor (O_a). This in turn triggers further changes in the desires and beliefs of other networks (D_c, B_c, D_d, B_d), leading to corresponding actions. Figure 4.23 summarizes the event sequence, which consists of what I have called the mechanisms of opportunity space capture and coping strategies (outcome six in figure 4.2).

Figure 4.23. The opportunity space capture and resulting coping strategies



This depiction seems to summarize quite well the commonality of processes in Estonia and Lithuania: in both cases the Juku and BK-0010Š networks managed to obtain the support of the local decision-makers. The situation is different for Latvia because one of the components necessary for the initial action to occur—the desire to do so (the motivation to engage in local network-building in table 4.3)—was absent. Hence the opportunity space capture mechanism failed to initiate.

I have pointed out the degree of unity of preferences as the distinguishing factor between Estonia and Lithuania. However, it is one of the core tenets of the DBO framework that preferences are not set in stone. What further distinguishes between Estonia and Lithuania is that in the former the opportunity space capture was directed to both groups—local decision-makers and potential users. That is to say, the unity of preferences was actively sought after and the process inclusive. In the Lithuanian case the education sector was mostly excluded from negotiations between producers and decision-makers, i.e. the process of opportunity space capture was exclusive from the viewpoint of future users. Anticipatory shaping of preferences did not take place, but the users themselves did not resist actively either.

Following from this, one can propose a tentative hypothesis that the mechanism of opportunity space capture:

1) Materializes when it is supported by both decision-makers and users.
2) Materializes when there is support from decision-makers, but the users remain divided or indifferent.
3) Fails to materialize when the decision-makers might be willing to offer their support, but the users actively mobilize against it.
4) Fails to materialize when decision-makers and users resist the attempt.

Unfortunately the narratives do not offer sufficient evidence to test proposals 3 and 4. The closest would be the case of Poisk, in which Sigma's proposal to the local authorities, bypassing the education sector, was actively resisted by the Lithuanian Computer Society. The project was indeed dropped, but it cannot be attributed to the resistance of the user mobilization for certain, since Sigma also experienced a network-specific shock (problems with reaching an agreement with the partner from Kiev). Therefore, it is quite possible that in the absence of this event, Sigma would have continued anyway. This indicates a need to further theorize the power differences between groups active in the opportunity space.

The second part of figure 4.23 draws attention to various reactions of other networks after the opportunity space has been captured by one of them. Logically, there are four possibilities:

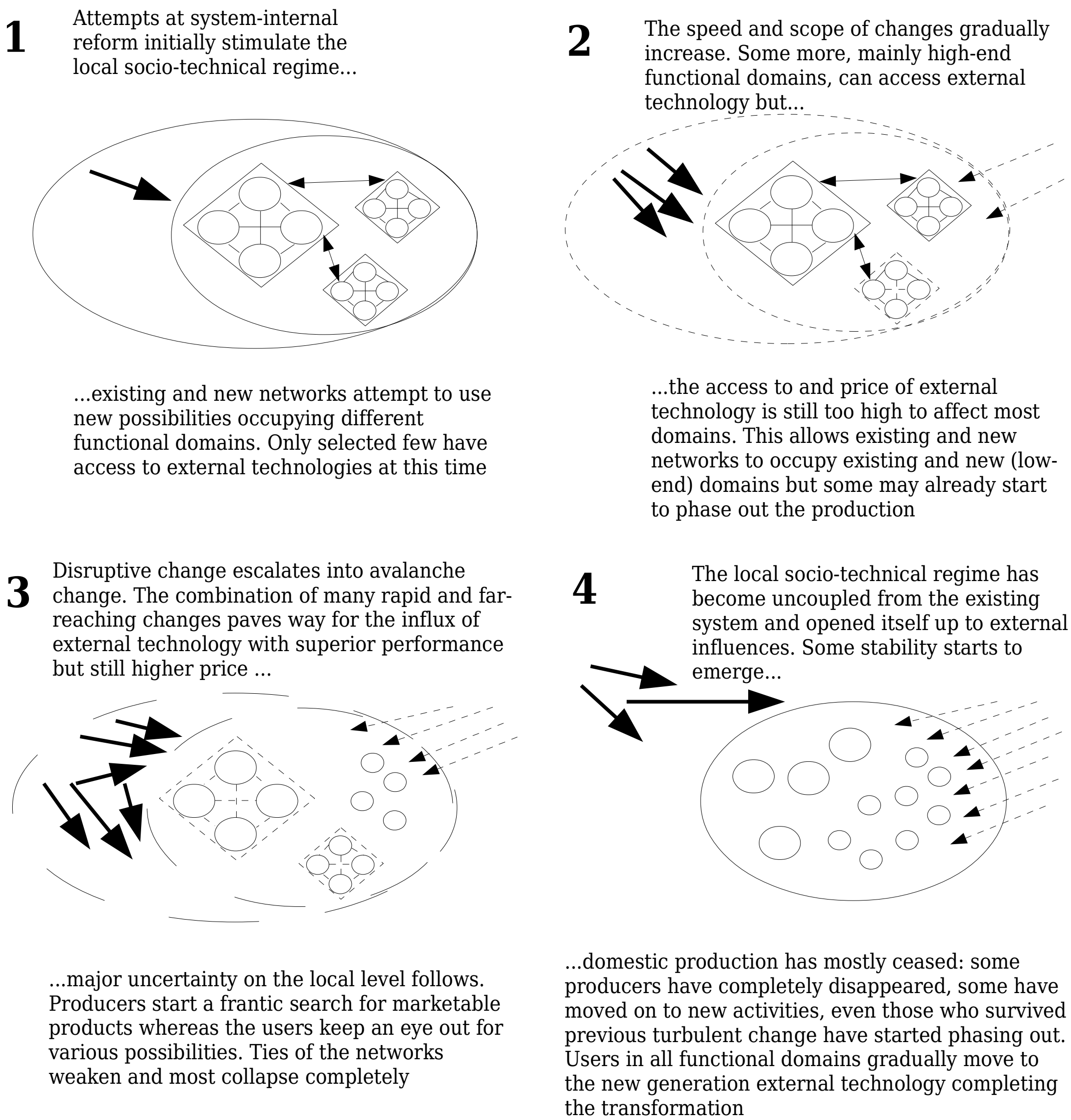
1) Neither the desire of school computerization nor the belief in the best way to do so change, in which case the competition between the networks continues.
2) The desire to participate in school computerization remains, but the belief about the best way to do so changes.
3) The desire to participate in school computerization disappears, but beliefs about the opportunities to do so remain—in this case the leader/network would be convincing itself that it did not desire the goal that much anyway.
4) Both the desires and beliefs change and the network moves on to other functional domains.

For the short-term perspective, I think that the second reaction is the most pertinent here. In Estonia, Entel tried its luck in the vocational education sector and Tartu made a late re-entry with small-scale production towards the end of the 1980s. In Lithuania the Santaka group decided to continue development and small-scale production as an unofficial parallel option. I think that two contextual factors explain why this response was prevalent: first, the opportunity space had been captured by the strongest leader, signalling grave difficulties for other players/networks to engage in direct confrontation. But at the same time all the players were well-aware of the systematic shortage in the USSR and had justified expectations that this would be the case for computers too. This meant a justified belief that the functional domain would only be partly occupied and that there would be still some room left for other networks. This would also explain why the attempts of smaller networks to (re-)enter the functional domain continued over the years.

4.4 System-level analysis

Hopefully I have managed to show that intra-case and inter-case focuses indeed help to tease out different patterns present in the same historical narratives. However, at this stage I also feel that the analysis on these two levels has created a somewhat fragmented picture. Yes, mechanisms and patterns of different kinds have been identified, but do we have an overview of the course of events as a whole? I think not, until we switch the level of aggregation to treat all cases in one locality collectively as technological innovation systems (or socio-technical regimes). Relating changes in the system to changes in the environment and choosing a time-frame of 8–10 years, the pattern of system-internal transformation emerges. I would argue that this model, presented in figure 4.24, captures the basic dynamics of all three countries.

Figure 4.24. System-internal transformation



The transformation can be divided into four distinct phases. At first the attempts to reform the communist system internally increase the freedom of action and direct the attention of producers and users to certain issues. As the overall stability of the system is not in question, these environmental influences can be treated as positive stimuli. In Estonia and Lithuania, the school computerization reform encouraged the creation of new networks (Juku, Santaka) and the re-domaining of existing ones (Tartu, Entel). The stability of the rest of the socialist system meant that the large-scale introduction of foreign computers was not going to happen, at least from a short-term perspective. Western computers were rare, extremely sought after and used only for limited applications by organizations that were highly influential or lucky enough to afford them.

The system-internal developments, however, do not stabilize, but increase in strength, becoming more and more disruptive in nature. In the second phase some visible cracks begin to appear, but most actors do not yet believe that the system itself would be in any serious danger. Many well-known difficulties such as getting the computer into mass production or renewing the prototype for a new product cycle remain the same. Some products of the existing networks may therefore be phased out and the participants of the networks move on to new activities to take advantage of novel opportunities. But overall, the performance of local networks is similar to the previous stage. Gradually increasing opportunities also mean that Western computers continue their inroads into various applications, but their influence still remains limited. At the same time, some of the Soviet components can be more easily obtained than before, lowering the entry barrier for smaller players (Tartu). The ratio of Soviet computers to Western ones can even increase as the former invade low-end functional domains (e.g. home computers) at a higher rate.

In the third phase the disruptive change has turned into an avalanche. The speed, scope and amplitude of changes are breathtaking. Uncertainty abounds. All countries experienced the virtually overnight disappearance of the supply chain. Independence meant a freedom of choice, but also de-isolation from the Western world. Technologically lagging computer production was no longer shielded from external influence. Lack of investors meant that the producers urgently needed to find something that they could produce with the existing infrastructure and which the customers would be willing to buy. From the user side, Western computers were now starting to pour in (e.g. used PCs being donated to Baltic schools), making the formation of local networks even more difficult. In these conditions local computer production was going to be extremely difficult, if not impossible, and many networks quickly collapsed (Juku, Tartu, Sigma 8800). But the case of Lema's PC/XT also shows that the conjunction of certain factors allowed some to continue profitably in the short-term.

In the fourth phase the confusion starts to disappear and stability slowly emerges. The price advantage, maybe the last shield of Soviet computers, also starts to vanish. Catching up with Western production is deemed impossible by the local actors. Large state-controlled factories have mostly collapsed; many small enterprises have emerged instead. Concerning the PCs, there has been a move from local design and/or production to assembly and re-branding, or simply re-selling of Western computers. Other players have shifted to new activities (e.g. focused entirely on software). The networks that might have survived the previous stage also start to phase out their product as new possibilities become more profitable (Lema's PC/XT). The old computers continue to be used in various functional domains for some time, until they can be completely replaced (e.g. Jukus and BK-0010Šs were still used in schools around the mid-1990s). This completes the transformation.

In this chapter I have illustrated a crude analytical technique for generalizing from the historical narratives. I have also presented analyses on three different levels of aggregation, illustrating how each of them enables the capture of different mechanisms and patterns of development. I have argued that one should avoid zooming in on minute detail *a priori* at any cost, because it might too easily lead to an inability to capture wider patterns. However, it could be noted that although I advocated a top-down approach to historical narratives, the analysis itself progressed from the lower to the higher level of aggregation. Is this not a slight contradiction?

A little secret can be revealed now: the order in which these patterns emerged during the research process was actually from the higher to the lower level of aggregation. The first idea of a system-level pattern came after I had conducted only one case study (Juku). The background knowledge obtained during the research enabled me to sketch the basic course of events. When most of the cases had been researched I began to notice different paths of school computerization in each country. Finally, after most of the data had been collected and written into historical narratives, intra-case results started to appear. The lower-level analysis enabled me to specify various nuances of higher-level analyses later on (e.g. outlining in more detail the local responses of system incumbents during various phases), but it did not prompt a fundamental revision. It was the structure and logic of the thesis that forced me to reverse the order of presentation. But it was the actual research experience that encouraged me to put forward the suggestion of a top-down approach, from higher to lower levels of aggregation, from more general to more specific, from less to more. Confirmatory research or intuitive progression? I will leave it for the reader to decide.

Having performed the middle-range analysis, only a few questions remain from the viewpoint of this thesis: can these theories, narratives and analyses be used to re-think some higher-level conceptualizations? Is it possible to use this whole research experience to build a more nuanced metatheory? And if so, then how? These opportunities are explored in the next chapter.

5. Higher-level generalizations

It is well known to practising scholars that the research process is far from linear: rarely if ever does one proceed directly from theory to research design to fieldwork to analysis to results without looking back. Rather more likely, new knowledge gained in any of these phases will feed back to previous parts of the process, prompting partial revisions, re-examinations and repeated analyses from different angles. Charting these movements truthfully would make the research report excruciatingly difficult to follow, justifying the linear structure of the text usually found in books and journal articles.

This point serves to remind the reader that the following ideas pertaining to metatheory and (to some extent) philosophy have been gathered into this chapter largely for presentational reasons. The actual origin of these ideas is more varied. Some bits resulted from working with existing substantive theories, some were inspired directly from the narratives, some had already been in my mind in some vague shape for quite a while but had not been sufficiently thought through, and some indeed emerged after the middle-range analysis when I shifted my attention to the higher-level meaning of these findings. But they all share a level of generality that goes beyond middle-range theories, i.e. they are potentially much more widely applicable.

In the following discussion I first cover the distinction between rules and meanings adding one basic component to the socio-technical metatheory outlined in chapter 1. I then move on to propose a typology of rules and sketch out the possible phases of rule evolution. The discussion of the usefulness of the realist approach along with the possibilities of speaking about 'technical' or 'material' causes concludes the chapter. I occasionally draw on the historical narratives as illustrative examples, but the very act of forcing oneself to think in more general terms reveals that the narratives at hand are often not enough: one constantly needs to exercise theoretical imagination and ask what kinds of categories are not directly observable in the data but should be there logically. Thus the actual discussion covers much more ground and draws on a variety of examples in order to highlight the potential scope of these ideas.

5.1 Basic causal forces

The initial version of the socio-technical metatheory employed in chapter 1 adopted Geels's (2004) distinction between actors, technologies and rules as its basic elements. I argued that technologies and rules manifest their influence through the mediation of actors. However, this did not mean that the explanation could be reduced to actors—the absence of technologies or rules would surely make a difference to the outcome. That is to say, actors can and do behave differently in different socio-technical contexts. The narratives illustrated this general point many times: actors frequently changed their preferences as new technologies (e.g. 16-bit computers) or new laws (e.g. the possibility to establish cooperatives) appeared.

Borrowing from and building on Giddens (1984: 21), a rule was defined as a tacit or explicit prescription guiding the enactment or reproduction of social life and manifested in patterns of practice. The question is whether this definition is applicable to all the instances found in the narratives. Which ones do I have in mind? Take the newspaper articles about the Soviet Baltic PCs like Poisk (Boyko 1991) and Juku (Hanson 1987.22.04): both contain descriptions of new computers, including the possibilities they offered and their potential uses. However, one would be hard-pressed to claim that articles like these are prescriptive—they do not create the impression that these described uses would be necessarily required whereas alternative uses would be somehow sanctioned. In other words: while the rule 'demands' a certain course of action this is not necessarily so for aforementioned descriptions. These descriptions seem rather to have the quality of a resource instead. Various words used in everyday language like statements, classifications, claims, ideas, representations, utterances, propositions, information, knowledge or wisdom seem to suggest this possibility—influence without prescription. This is analogous to many theoretical frameworks in STS and elsewhere, e.g. Technological Systems of Innovation, which refers to knowledge development and knowledge diffusion as key activities (Hekkert *et al.* 2007) vs. creating and changing institutions (Edquist 2005: 191), or desires and beliefs as factors that influence actions (Hedström 2005).

As a first approximation I will therefore use the umbrella term 'meaning' to cover these instances. This term is preferred to 'data', 'information', 'knowledge' and 'wisdom' because all of those words seem to constitute a kind of a hierarchy, referring to the degree to which a certain meaning has been integrated with other meanings. Alistair Duff brings the following example: raw data is obtained from space probes, packaged as a report (information), and set into the context of existing theories (knowledge), after which this new understanding can be put into a 'sensitive and timely' use (wisdom) (2000: 27). Meaning, on the other hand, seems to be more general and thus preferable, referring to any kind of unit regardless its degree of contextualization. So meaning can be a unit of data, information, knowledge or wisdom. Figure 5.1 integrates the category with Geels's actor–technology–rule triad.

The addition of meanings opens up six new relationships:

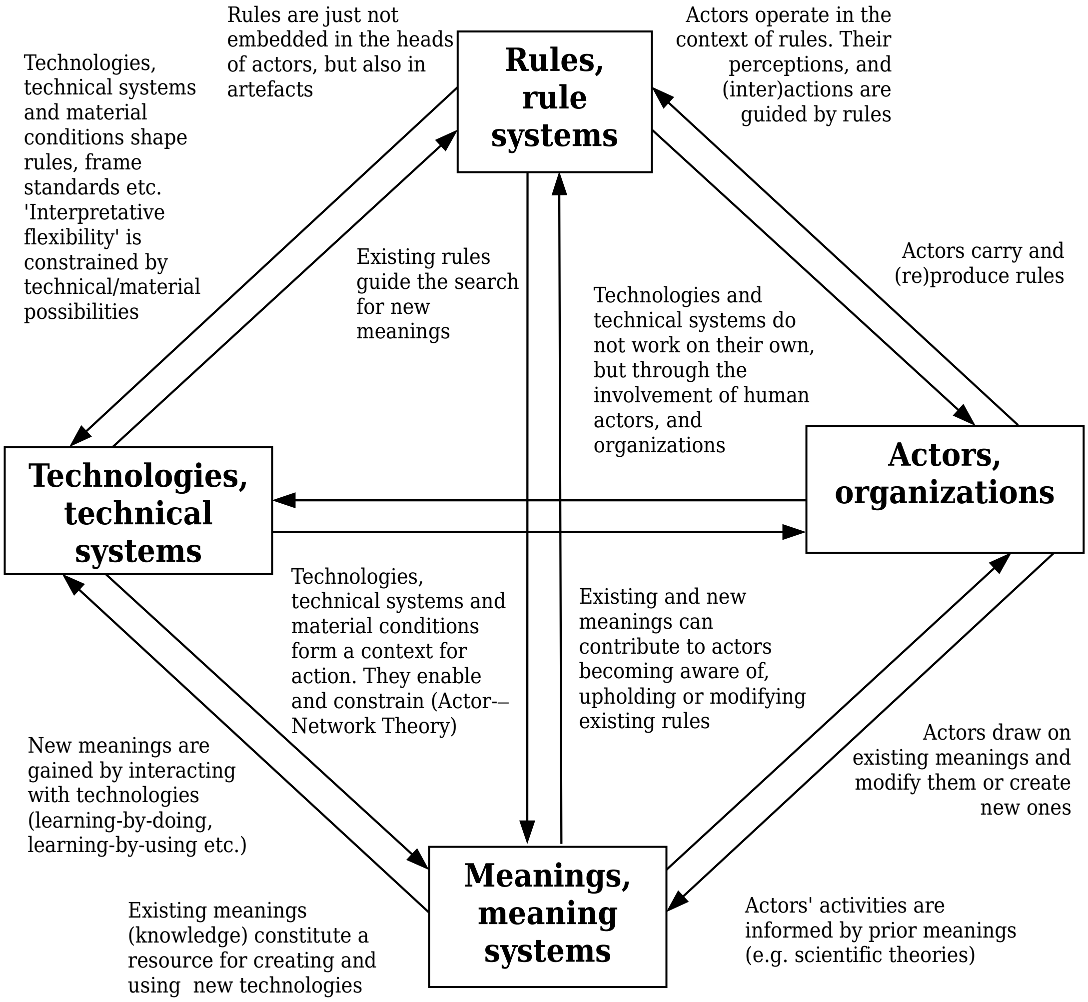
1) Actor → meaning—the actors create, modify and use existing meanings. An example would be the creation of a scientific study about the harmful effects of gasoline-based cars to the environment.
2) Meaning → actor—existing meanings can be seen as both enablement and constraint for the creation new ones. Following the above example, it would be very difficult for policy makers to reach a consensus about the extent of the problem and the activities to be undertaken unless the relation between cars and pollution had been established beyond reasonable doubt.
3) Meaning → rule—stocks of knowledge can lead to the transformation of rules. The study on pollution can become the basis of the introduction of higher taxes on gasoline and provision of subsidies for the development of alternative solutions.
4) Rule → meaning—rules can also guide the search for new meanings. The changed laws and new subsidies act as incentives for considering cars that run on electricity, solar power, biodiesel etc.

5) Meaning → technology—new technologies can be constructed and developed on the basis of existing meanings. Knowledge about the properties of various metal alloys, power sources, user habits and the like provides a basis for the development of novel car designs.
6) Technology → meaning—new meanings can be gained from practical experience and hands-on tinkering, as reflected by various phrases such as 'learning by doing' (Arrow 1962), 'learning by using' (Rosenberg 1982), 'learning by interacting' (Lundvall 1988) and 'learning by trying' (Fleck 1994). In this case the trial-and-error process of car construction reveals new information about the components, their durability, efficiency, expected cost of the product etc.

As it is difficult to imagine a rule being followed without the possibility of mental representation on the actor's part, it follows that rules are best characterized as an (important) sub-set of meanings. To put it simply: whereas rules are for something, meanings just are. In relation to a specific situation meanings do not prescribe a certain course of action; that is to say, they enable more interpretive flexibility than do rules. Otherwise they are quite similar: both make a difference to the outcome (the actor might disobey a given order or simply lack relevant knowledge—the computer would not be built in either case), both can be explicit or internalized and so on.

The category of meanings is quite easy to integrate with the rest of the metatheory. Meanings can be characterized as elements of socio-technical systems (e.g. technical knowledge) or systems in their own right (e.g. mathematics). As such other postulates proposed in chapter 1 also apply: systems of meanings or meanings integrated into socio-technical systems can be hierarchical (e.g. mathematics as one of the languages), they can interact (e.g. the use of mathematics in sociology, the use of sociological vocabulary in describing everyday behaviour) and they are elements of structure (e.g. accumulated expertise on microcomputing in an organization).

Figure 5.1. The mutual shaping of actors, technologies, rules and meanings (adapted from Geels 2004: 903, extended by the author)

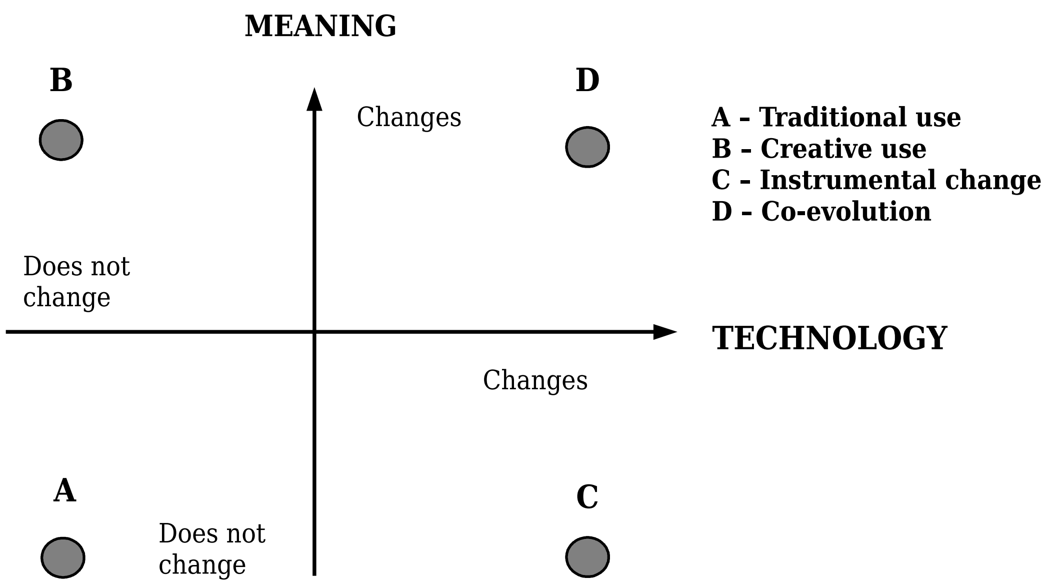


The same is true for co-evolution. Figure 5.2 outlines four possible outcomes of the interaction between technology and its meaning. A very simple example of a hammer illustrates each of them:

1) Traditional use—the hammer is used in the way it was meant to be used according to the pre-existing social conventions, that is for nailing. This is what actors do with their routine surrounding technologies every day; after all, we use countless technologies in one day while taking their physical structure and functions as given.

2) Creative use—the actor discovers that the hammer can also be used for straightening bent nails. In this case technology itself has not been changed: its physical structure remains the same while a new function has been added. Again, this is something creative actors do every day: the inventors usually do not foresee every possible use, which enables one to talk about 'unintended consequences'.
3) Instrumental change—the actor notices that she or he could improve the nailing process by making the hammer more comfortable, e.g. by balancing the handle a bit better. While technology itself is (slightly) renewed then, its purpose has remained just the same.
4) Co-evolution—the actor notices that since it is only one side of the hammerhead that is frequently used, the other end could be modified to remove misplaced nails from wood. As a result, both technology's physical structure and functionality change.

Figure 5.2. Possible outcomes of the interplay of technology and its meaning



5.2 Towards a typology of rules

Having made this tentative analytical distinction between meanings and rules, I will now take a closer look at the category of rules. As I have drawn extensively on critical realist sociology and the Multi-Level Perspective, I will start by probing how Elder-Vass and Geels have tackled the term. By identifying their respective strengths and weaknesses I will provide a synthesis that goes beyond each characterization. But note that in so doing the discussion of both authors will be highly selective and will omit much of the argument that is not directly relevant to the current topic.

In order to give an account of the role of norms and norm circles in the construction of institutional reality, Elder-Vass (2010b, 2012, ch. 4) has drawn on Searle's distinction between constitutive and regulative rules (1995). The first type, defined as 'X counts as Y (in context C)', creates opportunities for certain activities to occur. For example, in order to play chess the properties of the pieces need to be specified (e.g. what counts as a knight). Regulative rules coordinate *"activities that could or would occur whether or not the rule concerned existed"* (2010b: 6). For example, driving cars does not necessarily need a specification of the side of the road to be driven on.

Elder-Vass takes this argument further by distinguishing between norms that are 'indiscriminately indexical' and those that are not. That is to say, whether the norm applies to all actors who are part of the norm circle (e.g. 'hold the fork with your left hand') or only to a certain part of it. In the latter case the regulative norm requires a constitutive one (or an 'indexing norm' as Elder-Vass calls it) to specify the specific part of the norm circle in the first place. For example, in order to talk about the activities a goalkeeper is allowed to undertake in a football game, it needs to be clarified who counts as a goalkeeper in the first place. It is the crux of Elder-Vass's argument that these types of norms are mutually constitutive: the activities to be regulated require the specification of roles, but a mere definition of a role is meaningless without the specification of corresponding actions this role enables. Simply calling one football player a goalkeeper does not make any sense unless some special rules regulating his or her activities follow.

The distinctions used by Geels are borrowed from Scott's institutional theory (1995, especially pages 51–58),[56] which proposes that there are 'three pillars' of institutions: regulative, normative and cultural-cognitive. Geels defines each of them as follows: 1) *"the regulative dimension refers to explicit, formal rules, which constrain behaviour and regulate interactions, e.g. government regulations which structure the economic process. It is about rewards and punishments backed up with sanctions (e.g. police, courts)"*; 2) *"normative rules ... confer values, norms, role expectations, duties, rights, responsibilities"*; 3) *"cognitive rules constitute the nature of reality and the frames through which meaning or sense is made"* (2004: 904).

At first sight the distinctions made by Searle and Scott seem to share many commonalities. Constitutive rules resemble the cultural-cognitive pillar in that both seem to function as building blocks of other rules. Regulative rules and the regulative dimension both seem to be concerned with coordinating certain interactions. It also seems that Searle's notion of regulative rules at least partly covers the normative dimension, as it includes role expectations, duties and the like. So both classifications seem to identify at least two basic types of rules, with Scott going further by articulating Searle's regulative rules into two subsets. However, some problematic aspects remain in relation to the current discussion.

First, it needs to be made completely clear that Elder-Vass's argument of constitutive and regulative rules that strictly necessitate each other only applies to certain types of norms. While the claim is valid for some social institutions that constitute rule systems and do require both the role specification and the corresponding activities, Elder-Vass and Searle seem to hold that for indiscriminately indexical norms the regulative norms do not require constitutive ones at all, because the activities to be regulated could occur independently of the existence of the rule.

56 I have chosen to neglect a major strand of STS literature which explicitly acknowledges institutions as one of the fundamental theoretical building blocks—Systems of Innovation. This choice is based on the comparison of MLP, Sectoral Systems of Innovation and Technological Systems of Innovation conducted by Coenen and Díaz López (2010). They find that MLP's conceptualization of rules is the richest of the three.

However, this claim can be contested once we reflect on our propensity to exhibit recurring patterns of practice in everyday situations. Take the example of keeping a certain distance when queuing to use a cash machine. Now, it seems that none of the rule-followers are being specifically singled out, the rule applies to everyone: stand too close and people would give you a strange look, move away or even ask you to move. On the other hand, acts like this presume that one already has at least some preliminary notions of a queue, a cash machine, some sense of distance and so on. That is to say, every action requires at least minimal recognition of the features of the environment, and at least some of the particular features of the situation are taken to be the representatives of pre-existing categories. So it seems that it would be impossible to do away with constitutive rules. On the contrary, every action, however mundane, seems to rely on countless 'X counts as Y' type rules.

But this reliance is somewhat different from the cases described by Elder-Vass, in which one type of rule would not make sense without the other. In this case the notions of a queue, a cash machine and distance do not become automatically meaningless just because they have not linked together into a rule about keeping distance. Their applicability extends well beyond a single regulative rule. In this way they retain their meaning-like quality: although relying on certain lower-order classifications, these do not determine their inter-linkage and thus act rather like resources for higher-order rules.

Now let me turn to the distinction between cultural-cognitive, regulative and normative dimensions. More specifically, I would like to inspect the defining characteristics of each. I have already pointed out the similarities between the cultural-cognitive dimension and constitutive rules. The problem is that the definitions of regulative and normative rules are not mutually exclusive: both types 'constrain behaviour and regulate interactions', creating mutual expectations that are quite evident considering that the category of normative rules indeed consists of 'role expectations', 'duties', 'responsibilities' and the like; and normative rules can also be backed by sanctions or rewards (e.g. a company's perceived unethical behaviour might lead to a boycott by its clients without necessarily involving the intervention of formal state bodies). Therefore, one should look for some other feature in order to justify the distinction.

In fact, I would argue that this typology actually conflates two different analytical dimensions. Whereas the first is concerned with the rule's function, the second focuses on whether it is implicit or explicit, i.e. the first is about the type of rule and the second about its explicitness. It is quite easy to read the sequence of regulative–normative–cognitive as one of decreasing explicitness, beginning from highly institutionalized laws and ending up with hidden and largely unconscious frames of reference. With the essential difference between constitutive and regulative rules being pointed out above, the word 'values' in the definition of normative rules hints at the possibility of extending the classification to three different types.

These are as follows. First, constitutive rules are the ones defined as 'X counts as Y'. Second, instrumental rules specify the conduct of some activities. I have argued that these activities themselves always rely on constitutive rules. But there is also a sense in which some instrumental rules cannot always be considered to have a quality of being 'just' (e.g. instructions of starting a car) but some can (e.g. a law against stealing). I have chosen to use the adjective 'moral' to highlight this third dimension. I would argue that in order for a rule system to persist for an extended duration, all three types need to be involved: constitutive rules specify certain roles, instrumental rules regulate interactions between them and moral rules give grounds to justify the regulation of interactions. In the case of a community or an enterprise, the often seen expression 'who we are, what we do, why we do it' captures the essence of this constitutive–instrumental–moral system.

For the lack of a better term I have chosen 'implicit' to refer to the hidden dimension of rules—the ones that (seem to) underlie various patterns of practice (see below), but are not explicitly articulated by the actors concerned. In distinction from the implicit rules, informal rules might be linguistically represented but not be crystallized into explicitly codified sets of rules. These characteristics, in turn, define the formal dimension. The resulting typology, presented in table 5.1, brings all of these dimensions together with examples of each. I would suggest that the categories used are mutually exclusive and exhaustive, i.e. there is no overlap in content and all possibilities have been accounted for.

Artificial languages and legal documents provide examples of constitutive and formal rules. Whether one is trying to define an intellectual property or a patent, some distinguishing characteristics have to be outlined so that a certain X at a certain moment could be classified as Y. Programming language is a good example of how rule-following can be enforced by a machine. In this case the interpretive flexibility has been greatly diminished because the use of an incorrect command simply results in an error. Thus in order to program the user must follow the constitutive rules quite strictly. Another example would be the set of Soviet all-union standards—*GOST*—specifying various characteristics for a wide range of products.

Table 5.1. Typology of rules

	Constitutive	Instrumental	Moral
Implicit	Grammar, meaning of words	Formal greetings, queueing distance	Taboos, universally shared values
Informal	Fools and jesters, identity of a community	Organizational routines, self-regulation	Offering elderly people a seat, exchanging gifts
Formal	Mathematics, legal definitions	Traffic laws, government funding regulations	Codes of ethics, religious texts

In the case of the informal dimension, the role of a jester points to the social role of acceptable deviance, a 'necessary' outcast: while not taken seriously by the others, the ascribed role of jesters also enables them to point out the shortcomings of the community. In this case one can argue for the historical existence of such a role and possibly the acknowledgement of its function, yet the role itself is informal. The history of the Soviet Union often gives a splendid chance to reflect on informal roles because of the large discrepancy between official rhetoric and everyday experience. To take the example of the Special Construction Bureau of Computing Technology of the Institute of Cybernetics in Soviet Estonia—the director of the organization was enacting at least two roles. The formal role was quite similar to any director in any organization, whereas the informal role was to provide as much freedom for talented employees as possible (Leppik interview). This included not only financial and technical freedom, but also the management of relations with local and central authorities to avoid disrupting the flow of work by ideologically-motivated central decisions, often perceived as ignorant and arbitrary. As colourfully summarized by one of the interviewees—regretfully off the record—the informal task was to 'act as an umbrella against the shitstorm from above'.

Finally, leaving aside the textbooks, most of our utterances rely on internalized rules of grammar and meanings of words. And—to use Elder-Vass's example (2012, ch. 4)—when playing chess we do not usually ponder on the definition of the pieces. One can probably make the same comment about experienced programmers who follow the syntax instinctively rather than explicitly considering and re-considering the meaning of every command.

Speaking of formal and instrumental rules, various laws are once again prime examples for they explicitly serve to guide and sanction interactions. Whether one is required not to cross the road at a red light or to submit certain documents to apply for funding, some activities are being brought in line. For example, the decree to start teaching informatics in schools played an important role in the emergence and development of many PC projects in the Soviet Baltic countries.

As with informal constitutive rules, the informal instrumental rules in the USSR were often in conflict with the formal ones. For example, although in theory the less significant organizations could write letters to factories and request components, in reality one needed to travel there in person, rely on communication skills and bribes to get them. The director of SCBCT managed to capture the underlying rule covering all informal activities in one sentence: *"Do not confront the prevailing order publicly"* (Leppik interview).

Lastly, there might be cases in which rules have been internalized to the extent that we do not even acknowledge them: the way we greet people sharing the same cultural community or the way we keep social distance are good examples. In the USSR a satirical term *Homo sovieticus* was used to refer to a set of cultural traits acquired and shared by most Soviet citizens that were manifest in various activities and expectations about the activities of others: doublethink, paternalism and isolationism to name a few (The Economist 2011.10.12). It is sensible to presume that these prescriptions for behaviour were not consciously acknowledged by the individuals most of the time, but rather, as (generally) useful guidelines in Soviet social context, automatic responses to various stimuli.

The most explicit manifestations of moral rules would be codes of ethics or religious texts. The Ten Commandments would be a good example of a set of rules conveying not only a code of conduct, but also reflecting the fundamental values of Judaism and Christianity. One could argue that in official rhetoric the development of the Soviet Union was guided by an explicit overarching utopian moral rule that postulated achieving an egalitarian social order as its ultimate aim.

Similarly to constitutive and instrumental rules, the category of moral rules also operates on an informal level—for example, it is commonly expected that two friends might invite each other to their birthdays and exchange gifts. The violation of this principle would probably be perceived as rude and unjust. Concerning the historical narratives: although the motives were usually mixed, many interviewees also expressed a perceived obligation to 'do something for the country' as one of the drivers for PC building (e.g. Enok, Jaaksoo, Matulionis interviews). This could be characterized as an informal moral prescription that complemented the utilitarian reasoning with the feeling that the cause is noble.

The category of implicit moral rules provides the toughest test for the typology. On one hand, this possibility seems to be required by the logic of the categorization. On the other, one may well wonder whether moral evaluation without conscious reasoning is even possible. Would it turn out not to be, the choice of the above categories could be questioned. But if evidence of such phenomenon could be found, one could argue that the typology has indeed managed to build on prior classifications.

And indeed, over the past decade researchers working in the field of moral psychology have proposed that humans do possess sets of innate rules for behaving morally, a universal moral grammar, operating analogously to a linguistic competence. In a review article, Mikhail (2007) presents some initial evidence from psychology, linguistics, anthropology and neuroscience to substantiate this claim: 1) developmental psychologists have found that even little children seem to have a certain moral code, e.g. they distinguish acts with a similar result on the basis of intent or purpose, they distinguish moral violations from the violation of social conventions (Mikhail uses theft vs. wearing pyjamas to school as an example) etc.; 2) every natural language seems to distinguish between obligatory, permissible and forbidden acts; 3) certain prohibitions (rape, murder) and legal distinctions seem to be universally shared among different cultures; 4) some studies have located parts of the brain involved in moral cognition.

The theory of universal moral grammar goes further, however, in that it aims to test the hypothesis directly. In a series of studies (see Mikhail 2007 for more detail), people were asked to solve various dilemmas. One involved a situation in which the respondent sees a train storming towards five people. The respondent is standing next to the switch, which she or he can use to direct the train onto another track. As the driver has fainted, the respondent is the only one capable of influencing the situation. Unfortunately, there is also one man standing on the track to which the train could be diverted. The respondent is asked whether she or he would flick the switch or not. A variation of this dilemma suggests that the train could be sufficiently slowed down so that five men could escape by pushing a large heavy object on its way. However, the only thing nearby that would qualify is a large man standing next to the respondent. The respondent is asked whether she or he would shove the man on the track to save the lives of the others.

It appears that despite the fact that the outcome would be exactly the same in each case—sacrificing one to save five—an overwhelming majority of people (roughly nine people out of ten, see Mikhail 2007) agree to flick the switch in the first case, but would not shove the man on the tracks in the second. In a comparative study, Hauser *et al.* (2007) found that this proportion is little affected by respondents' differences in gender, age, ethnicity, nationality, education and even exposure to moral philosophy. The same study also tried to determine whether the respondents can justify their choices. It turned out that out of 597 subjects, 267 could provide no justification at all, while roughly 70% of the justifications provided were insufficient in that they failed to point out a clear difference between the above scenarios. At least part of these incomplete justifications appealed to a 'gut feeling' (*ibid.:* 13–14). This seems to suggest that moral rules can indeed operate without explicit acknowledgement. Even more—the capability to behave morally might be a biological feature rather than something learned over the course of growing up (which is not to deny the role of the environment in shaping whether and how this tendency is actualized).

Two such universal rules that underlie moral choices have been identified. One of those is the principle of double effect, which holds that *"it may be permissible to harm an individual for the greater good if the harm is not the necessary means to the greater good but, rather, merely a foreseen side effect"* (Hauser *et al.* 2007: 3). In the first of the above scenarios, the death of a man could be perceived as a side effect—he just happened to stand on the track and the respondent had no means of altering his position. In the second scenario the man had to be used by the respondent as the necessary means to an end, thus violating the above principle. Another rule is the prohibition of intentional battery, which *"forbids purposefully or knowingly causing* *harmful or offensive contact with another individual or otherwise invading another individual's physical integrity without his or her consent"* (Mikhail 2007: 145).

Thus I would conclude that the typology is indeed a step in the right direction, increasing the analytical clarity of the notion of rules and as such allowing a better understanding of the components of rule systems or regimes (see the conclusion). However, at this point a number of qualifications should also be made to show what kind of problems this typology does not solve (too well).

First, the typology is tentative. In fact, the foregoing discussion already implies that the chosen categories can be further refined. For example, the dimension of implicitness fails to make a clear distinction between unconsciously enacted but nevertheless learned rules and the ones that are already part of our biological inheritance and bias us towards certain behaviour.

Second, the typology is static. This means that it does not include various mechanisms of internalization, externalization, learning, diffusing and sanctioning. I do agree that rules can be learned from pre-existing texts, by rational imitation (Hedström 1998), by explicit conditioning by others (Jones 2010) and so on—but (currently) this typology is descriptive, not explanatory. It aims to offer a better classification, but does not attempt to hypothesize the mechanisms characteristic of one type or another.

Third, the analytical clarity of the categories can be questioned. The problem is that while the difference between constitutive and instrumental/moral rules is that of a type the difference between instrumental and moral ones could be seen as one of degree since the examples of both are partially about regulating actions (see table 5.1). A preliminary solution might be to view moral rules as values which function to specify the desirable properties underpinning instrumental rules of behaviour. Take Gordon Gekko's famous 'greed is good' motto. On one hand this statement does not attempt to constitute neither the meaning of 'greed' nor 'good'. On the other hand it does not specify any action either. However, (at least implicitly) it relies on some notions of 'greed' and 'good' (thus being underpinned by constitutive rules). Moreover, it can also be taken as a basis for deriving more specific rules for behaviour (e.g. while investing, only seek to maximize your own welfare). At first glance this seems to be a good way to deal with the problem of mutual exclusivity but more work remains to be done in order to see whether this is actually the case.

At this point it should also be noted that the typology largely excludes the problem of the hierarchy of rules and rule systems. That is, hypothetically one can deconstruct any type of rule as being a surface expression of a more fundamental one. For example, instrumental traffic rules might be read as expressing an underlying moral rule that one should attempt to minimize the harm done to others. However, there are probably limits to the sensibility of the argument that every seemingly 'neutral' and 'descriptive' body of statements actually hides deep ideological commitments. For example, if we knew about the existence of a culture only by one hammer, we could hardly learn more about their ideology apart from the assumption that they valued hitting something. Therefore, while acknowledging the importance of the issue I would prefer to leave the question of the hierarchy of rules open to further empirical scrutiny.

Related to this is the problem of instrumental and causal sameness (Turner 1994, in Elder-Vass 2010b), which states that we cannot derive a rule of behaviour from observed patterns of practice because we have insufficient information about the actual internal causes driving the actors. In other words, similar behaviour might result from different motivations. While I do agree with the argument content-wise, I would argue that depending on the purpose and level of aggregation of the research rules can continue to be used as conceptual simplifications. That is, if we observe a certain practice in a certain context and are interested in a higher-order phenomenon of which this pattern is only a part, we can simply assume the equivalence between instrumental and causal sameness. The difference would only become significant once the context is shifted and some actors suddenly change their behaviour, which would give grounds to make additional assumptions about the variety of driving internal causes or the hierarchy of rules.

Returning to the example of chess, in order to observe the interaction of chess pieces on the board I can start from the fact that the actors behave as if they were following certain rules. I can then derive the rules they seem to follow from their practices, assume that they hold more generally and test the assumption elsewhere—this time before the practices themselves even occur (or at least before they have been observed). If the postulated rules of chess still seem to hold, it means that the distinction between instrumental and causal sameness is irrelevant from the point of view of the particular research. Moreover, one could assume that despite the differences in actor-internal causal configurations, in one way or another they are all aggregated into a very general mental representation, a belief that in a certain context something should be classified in a certain manner or a certain action should follow—otherwise the patterns of practices would not follow and the observer would have no means to suspect the existence of any rules in the first place. But at the same time it does not mean that the rules so postulated would remain eternally immutable—they continue to be challenged by new observations—it only means a pragmatic temporary trade-off is made to avoid dealing with hypothetical issues which might not even turn out to be relevant for the research.

For this reason I would also like to avoid extended discussion on extensional semantics and finitism (Barnes 1982). In principle, the distinction is important: one can indeed argue in favour of the stance that meanings are not absolutely fixed before usage, that every act of rule-following requires decisions whether the particular situation qualifies as one in which the rule should be applied, and that rules and meanings are constituted through practice and always revisable. But if that does not mean complete interpretive anarchy then the sources of constraints eventually need to be located somewhere, e.g. in experience, habits and artefacts (MacKenzie 2008: 103). And again, if one is not specifically interested in the theory of actor-internal causation (e.g. how beliefs, attitudes, emotions etc. contribute to a rule-like behaviour) then this distinction can once again be ignored for certain research purposes.

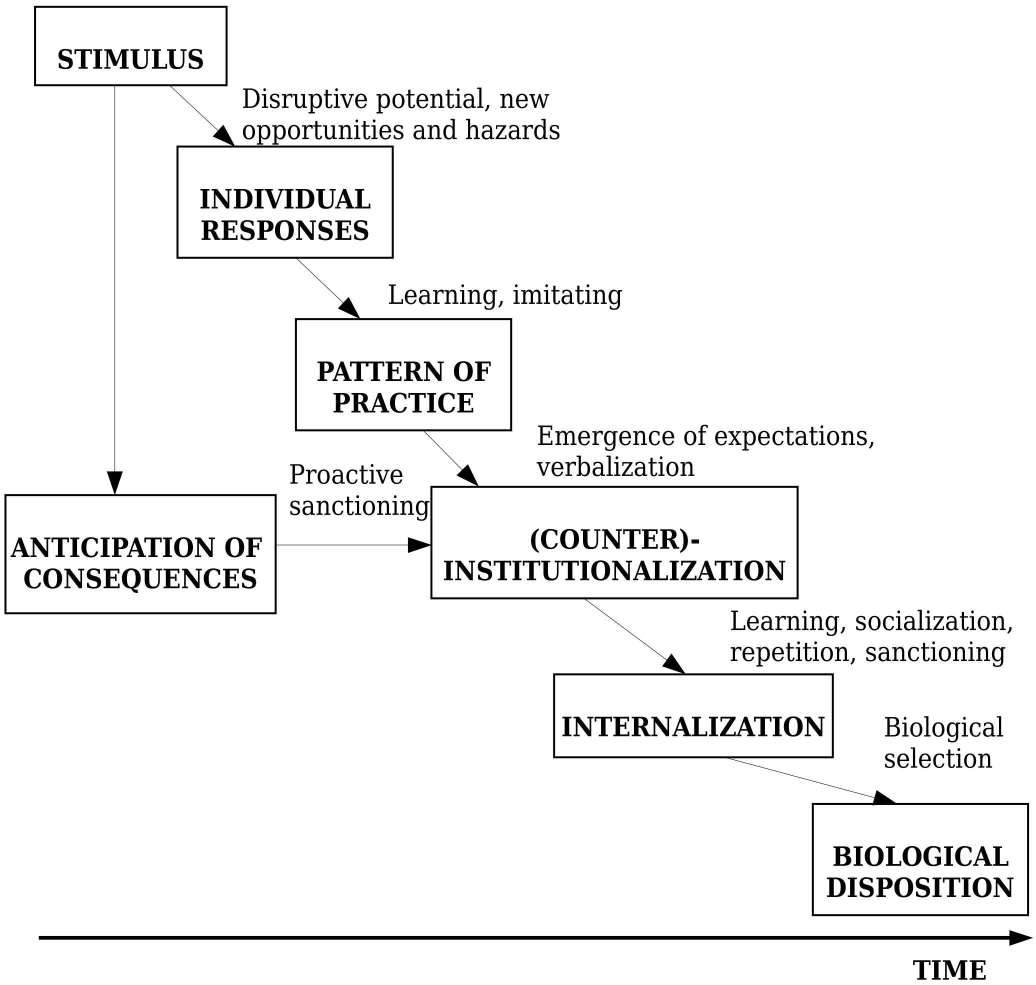
In sum: 1) if there are two competing explanations for a similar outcome and; 2) the researcher is interested in the interaction of outcomes rather than the emergence of the outcome itself and; 3) the implications of different lower-level explanations for the higher level are unclear or (seemingly) irrelevant for the current research; then for pragmatic purposes the first point can be neglected to avoid infinite regress and make the research manageable. Admittedly this is quite a complicated way to say that there are simply limits to what one can learn during one research effort and that at some point one should just acknowledge these limits and get on with it.

5.3 How do rules come to be and how do they diffuse? A tentative sketch

I have argued that the above typology was not meant to solve the underlying mechanisms of rule evolution. As various qualifications testify, the problem is quite complex, even if only for the multiplicity of such mechanisms. However, I do think that various bits and pieces in the above discussion can be pulled together into a tentative descriptive model of different phases of rule creation and diffusion. This is represented in figure 5.3.

The process begins with the occurrence of a stimulus of some kind or the expectations that the current practices of a society will eventually lead to one. This stimulus has the quality of introducing some new possibilities or hazards to the existing social order—were this not the case the actors would have no reason or means to alter their actions at all. Note that this does not distinguish between external and internal stimuli. Also, this is not meant to imply that the stimulus would be instantly and widely recognized by all potentially affected parties. In fact, one could speculate that this process is rife with unintended and unforeseen consequences. Moreover, it can be hypothesized that the longer the duration of unequal perception the more severe the eventual conflict with existing institutions, since new practices have had more time to crystallize. This would explain, for example, why the struggle between the defenders of the free flow of information on the internet and the holders of intellectual property rights only quite recently escalated into a full-scale debate about the extent to which the network could and should be regulated. Historically the internet had developed relatively autonomously from business interests—indeed the interest of the private sector was lukewarm for decades (Winston 1998, ch. 18). This probably enabled the restriction-free technical infrastructure and the culture of information sharing to develop until the problem of conflicting practices became blatantly manifest at the beginning of the 2000s with the introduction of new file-sharing applications like Napster, Kazaa and BitTorrent.

Figure 5.3. The creation and diffusion of rules



Initially, however, various individual responses remain local and varied in nature. Since it is uncertain which practices will prevail, it might not be clear how to react even if the stimulus is recognized early enough. However, some of the responses seem to take greater advantage of the stimulus or deal better with its consequences. As individuals begin to exchange information about their experiences by learning and imitating, eventually some patterns of practice become more prevalent.

At this point many things happen. First, expectations emerge. This signifies the acknowledgement of the actors themselves that the practices have crystallized into relatively predictable patterns manifested in certain situations. From an individual point of view one realizes that in a certain context most of the actors tend to exert certain actions, exhibiting a rule-like behaviour, and adjusts one's own actions accordingly in advance. Second, the question of whether these practices are wanted or unwanted arises. Depending on the power constellation of a given society or community, certain practices might become verbalized into a code of behaviour or even officially legitimated, further consolidating expectations about the future behaviour of others. Alternatively, a counter-institution might be devised to negate the pattern. To turn back to the example of the internet: the ongoing debate about whether to mitigate copyright laws or to enforce even harsher punishments for violations illustrates the tension between accepting and legitimating widespread patterns of practice and attempting to counter them with more intensive sanctioning.

Third, I hypothesize that in this phase something even more fundamental, what I would call rule specification, occurs. In previous phases the mechanism underlying the pattern of practice might have been purely imitative. That is, people might have followed a rather basic rule like 'in the conditions of limited information and great uncertainty, follow the behaviour of others'. With verbalization and institutionalization, the rule becomes much more context-specific, e.g. 'drive on the right-hand side of the road', and so do the corresponding sanctions.

In some cases, however, the stimulus can be or at least seem to be disruptive enough to warrant proactive sanctioning. The regulation of nuclear weapons is an example of how institutionalization preceded the use of these weapons in warfare as standard practice in order to avoid severely detrimental consequences.

The time-scale of all of these phases is roughly from months to decades. The span of the next phase can extend from decades to centuries. Here the members of the society are socialized into 'correct' behaviour early on, so that many rules indeed become automatic prescriptions followed without any reflection. The process still involves learning and sanctioning, but the reasons for the existence of the rule might be obscure for both those doing the socializing and those being socialized. This would explain why rules with quite practical origins continue to be followed long after the initial stimulus itself has disappeared. For example, a popular tale ties the original function of the handshake to ancient times when it was supposedly used to demonstrate that no weapons were being concealed. While most people have stopped carrying swords since then, the handshake itself has remained as a gesture of politeness.

As the above discussion of the universal moral grammar theory indicates, there may be yet another phase, the time-scale of which extends from thousands to millions of years. The underlying mechanisms are once again different.[57] Working over a very long time-span, this biological selection results in individuals with 'in-built' propensities to behave in a certain manner in certain contexts.

I believe this rough-and-ready descriptive model enables the formulation of some testable propositions. However, since the level of abstraction is very high it remains

57 I do not claim to have any substantial expertise in this area, and therefore would like to avoid extensive discussion of evolutionary psychology and sociobiology. I also wish to avoid the debate on the exact mechanisms of selection at work (e.g. individual or group-level). This phase has only been included for the purposes of completeness, implied by the above discussion. I would, however, caution the reader not to discard this phase as entirely irrelevant from the sociological perspective on the grounds that the tempo of current normative change is so fast that the environmental stability cannot be sustained long enough for biological variation/selection mechanism to be realized. It may well be that some very general trends, e.g. the increasing complexity of our world-system, will last long enough to have some (equally general) implications after all.

to be seen whether more detailed narratives of historical institutions can be fitted to this model, and if so then at what cost. These problems remain to be solved in the future. Before concluding this chapter, however, I will briefly turn back to the question of the usefulness of (critical) realism as a general basis for more specific theories.

5.4 Justifying realism

In chapter 1, three critical realist principles—independence of (at least some aspects of) reality from our perceptions, the possibility of establishing correspondence between our claims and this reality, and the potentially fallible nature of this process—were adopted. The choice of such axioms is a right of the researcher and in principle needs no justification: after all, every axiom relies on an array of other unjustified claims. And if one aims to justify them all one simply ends up in a vicious spiral of infinite regress. Alternatively, one may think that this exercise of justification is unnecessary—it may seem that as materiality is an integral part of STS most of the proponents are 'closet realists' anyway. However, at least some influential STS thinkers seem to challenge this view. So when arcane statements like *"the world is not simply epistemologically complex. It is ontologically multiple too"* (Law 2008: 367) or *"constructivist technology studies can be* agnostic *about this idealism-realism question"* (Bijker 2010: 64) continue to be made, it's clear that there is some disagreement about the usefulness of realist approach and that the issue merits at least some justification, if not a full-scale debate.[58] But I hesitate to tackle this question with the (in my opinion unnecessarily) complex language and obscure labels so infuriatingly characteristic of some of the approaches of STS. Instead I will proceed from a simple example related to research practice, gradually teasing out further implications. By focusing on why I would prefer to continue to speak about material or technical causes, I will also provide an answer to the question: why remain realist?

58 Because of space limitations the chapter devoted to an extensive analysis of ANT and SCOT had to be omitted. In the current chapter some problems are only briefly alluded to. See Kanger (2012) for more detail.

Consider building PCs in the Soviet Union. It was claimed in chapter 4 that various limitations affected the design, e.g. faulty and unstable microprocessors, slow tape recorders with poor mechanics resulting in many reading and writing errors, strain on the eyes due to using TVs as monitors and so on. All of these limitations were labelled 'technical'. But the use of this term could be contested in several ways:

1) One could ask 'faulty', 'slow' and 'poor' for whom, and for what purposes? In other words, all of these categories have been ascribed by the interviewees or the researcher. But if the purposes and the suitability of the technologies are firmly human-defined, does it not make the category of 'technical' or 'material' largely redundant?
2) At that time faster and more reliable components were being produced in the West. But restrictions on obtaining them seem to belong to the realm of social institutions, so the issues would seem to be little about the constraining capacities of the material.
3) As we have the benefit of hindsight we know now that many better computers have been built since. Are the constraints then not material, but rather more accurately about the lack of proper knowledge instead? After all, if the engineers had known then what they know now, they would have been able to build better computers decades ago.
4) Try as we might, we can only know the world through our perceptions and linguistic categories. Does it not then make more sense to stop ascribing causality to what cannot be known directly anyway, and instead focus on what we can be sure of—that is, the existence and interplay of various meanings?

The first claim can be answered in a number of ways. To begin with, if one adopts methodological relativism, one is able to acknowledge in theory that the material has an impact on human action—it is only that the whole process of technological development is analysed as if it does not. This is the position advocated by Bijker, who fears that otherwise researchers might too easily lapse into technological determinism (2010: 71). In my opinion this is a rather weak defence because it fails to solve the problem of how the exclusion of certain causes could yield a substantively more accurate, rather than merely more 'interesting', result. It is the weakness of the analyst, not the theory, if one confuses technological determinism with the causal significance of technology or the material.

The focus on the ascription of functions and meanings also excludes the possibility that the actors are simply exhibiting a sour-grapes syndrome, i.e. tailoring their desires to what they believe can be achieved (Hedström 2008: 326–327). Moreover, the possibilities for 'social construction' might become considerably narrowed down once the non-negotiable properties of the material start to 'bite back'. As stated by Vincenti and demonstrated in his research on the technical constraints of Edison's electrical lighting system: *"...once some basic elective decision has been made, possibly (even probably) on social grounds, a kind of technical logic can take over, leaving designers and inventors little or no choice in important aspects of their engineering solution"* (1995: 553–554).

One could, however, make a seemingly stronger defence than Bijker by insisting that although the material aspect of technology is indeed a logically necessary cause, it is simply a trivial one. In other words, *"it is always (or almost always) present, irrespective of the outcome"* (Mahoney 2008: 431). Since the ascriptions of functionality do not derive unambiguously from material properties, only the former make a significant difference in defining the 'essence' of technology for a given social context. As such the exclusion is justified because the material does not add any explanatory power; after all, it can be interpreted in millions of possible ways. Thus claims Sismondo: *"No matter how unmalleable a technology might look, there are always situations, some of them highly hypothetical, in which the technology can take on unusual uses or interpretations"* (2010: 101).

But the careful wording of this statement (note the words 'highly hypothetical') already indicates some difficulties with deeming technology as a necessary but trivial cause. For in principle this requires the researcher to assume that at any point of time, any material resource or a technological artefact is equally amenable to the ascription of any meaning or functionality, with an equal outcome. That is, one should assume a complete detachment of function from structure. Anything would be equally suitable for any purpose. But the fact that not many people—even after being liberated by decades of anti-essentialist research in STS—are willing to devote huge sums to building jet planes from cheese illustrates that at least in practice this position seems to be untenable. There seem to be good reasons to hold on to a belief that there is some affinity between (some) properties of certain artefacts and their uses.

If that is the case then what about the second claim, which argues that the real determinant of the outcome is to be found in social institutions, since these limit access to material resources? In my opinion this type of criticism simply pushes the cause back one step. Yes, it is certainly true that historically all technologies are all constructed by humans for certain purposes. More specifically, it is also true that it was the nature of the relationship between the USSR and Western countries that hindered the influx of new technologies. But for a certain actor with a certain goal in a certain socio-technical context, this problem eventually materializes in the shape of the properties of available resources—either the microprocessors work reliably for an extended duration of time or they do not, for example.

Here one could appeal to the argument that the property of such socio-material entities is relational in each case, *"that elements in a system are significant—and indeed achieve their form and character—only in relation to one* another" (Law 2008: 631). In other words, when we employ notions like 'affordance' (Gibson 1979, Hutchby 2001, Kirchhoff 2009) this already presumes both, material properties in association with a certain purpose. A material entity affords something in relation to its user's desires, and depending on the characteristics of the latter the affordances can be different. A lake can be a living environment for a fish or a source of refreshment for a weary traveller. From that perspective the 'reliability' of the microprocessor can only be understood as a relation between its material properties and its ascribed function.

In some ways this solution is better than the previous one in that it does allow material properties to play some explanatory role. As a temporary solution (when one focuses on the interplay of different socio-technical entities with their relational—which in this use is virtually identical to emergent—properties) it might be tenable. However, from the point of view of critical realism it also seems to encourage disinterest towards the question of analytical decomposition. To put it more simply, it does not attempt to answer the question of whether the influence between the ascription of function and material properties is asymmetrical, and if so then to what extent. How much possibility is there to switch the configurations of material resources so that the criteria of functionality would remain the same? Moreover, so that they would remain the same in largely different contexts? In a society with a 15 km/h speed limit a horse can indeed act as a functional substitute for a car, but what about longer distances or differences between the food or fuel required? Problems like this seem to suggest that there is at least a *"hierarchy of real-world constraints"* (Vincenti 1995: 566). However, the treatment of all configurations as relational simply excludes this issue by implicitly assuming the equality of the relevance of material properties and the ascription of function. Thus it is no wonder that to date little attention has been turned to the matter (Vincenti 1991 providing a welcome exception). A realist take on the matter might provide a way towards the solution, and I fully agree with Vincenti's proposal that *"a taxonomy of real-world constraints might be useful to compile"* (1995: 572). To summarize: I believe that relationality as a simple and temporary solution has actually become quite permanent, thus continuing to maintain the existence of certain analytical blind spots.

What about the claim—supported by retrospective proof!—that the real limitation is insufficient knowledge, not material properties? Like the previous critique, this one also shifts the driving cause. In principle it is claimed that one can never be certain whether the laws of nature restricted the actor's fulfilment of his or her goal or whether the actor simply lacked relevant knowledge. But note that this distinction becomes relevant only in hindsight. Once again, for an actor at a certain point of time in a certain socio-material context, his or her lack of knowledge manifests in the durability of the properties of material resources. For this reason I find it sensible to treat the cause as 'technical' or 'material' even if in retrospect the added knowledge would enable the observer to dispute this claim.

To see why this is so, let us remember that the observer (or researcher) is also largely limited to the pool of knowledge of his or her time. So the researcher could use the benefit of hindsight and demonstrate that at least some limits deemed to be material by the historical actors could be more accurately characterized as limits caused by insufficient knowledge in the light of the information now available. However, not all causes can be reduced in that way: using Vincenti's example (1995: 565), perpetual motion is still deemed impossible by the current laws of physics. And here the nagging question emerges: how can the observer be sure that his or her own attributions really stand the test of time? Because in principle the observer's attributions of causality to the material properties can always turn out to be erroneous in the light of future scientific and technological developments, and thus one could infinitely extend the argument of insufficient knowledge as the real cause. In brief, the analytical distinction between the material and the knowledge-related could never arise.

This brings me to the final point: why not let it all go and extend this argument infinitely? Is it really a problem if one focuses on actors' own perceptions and does not evoke any external criteria? After all, it is only through these perceptions that we are able to obtain any information at all. Why not then analyse any socio-material processes simply as the interplay of 'social' and 'material' meanings? For example, if a microchip was ascribed a meaning that it is 'faulty' then this meaning itself makes a difference to the construction of other meanings and thus helps to explain the eventual result.

To me the main problem lies in the fact that increased scepticism also means fewer nuances. Because for a realist, idealism proposes an inconvenient question: why are certain ideas more durable and resistant than others? An answer to this—adopting a position that there are some entities with causal powers independent of our own perceptions—brings an additional burden because the ascription of external causality needs to be justified somehow. Although critical realism also admits that there is no one-way train from conceptions to reality—our ascriptions may be simply wrong—that itself does not resolve the issue. In fact, coupled with the acknowledgement that offering absolute proof is impossible, the realist position seems quite uncomfortable. But Sokal and Bricmont's argument—that *"the mere fact that an idea is irrefutable does not imply that there is any reason to believe it is true"* (2008: 176)—can be usefully turned around: it is possible have a reasonable belief, say, in a fact that it was a faulty microchip with independent causal powers that caused the computer to break down, not merely our belief. By focusing solely on the meanings we once again risk analysing socio-technical processes as power struggles between social groups in which the properties of the artefacts or material resources themselves have no part to play. This, of course, evokes an array of troubles indicated above.

None of which is to say that critical realism is unproblematic. From the STS point of view much remains to be done, whether it is formulating a sound conception of technological causality, creating a taxonomy of material constraints, exploring the relation between physical, knowledge and normative boundaries or determining the extent to which socio-material reality can be constructed. But I would hold that the very basic tenets of realism, the possibility of attributing (partial but not determinate) causal efficacy to entities beyond our perception, is simply too powerful a weapon to be discarded easily. Ultimately it offers more analytical distinctions than the meaning-centred approach and thus enables attention to be paid to more interactions. So my short answer to the question of why one should remain realist is: "It's an excellent hypothesis."

In this chapter I have covered various metatheoretical and philosophical problems encountered during the course of my research journey. It is now time to provide answers to the initial research questions, draw together the main contributions of the dissertation, discuss their significance, point to the shortcomings of the work and suggest potential opportunities for future research.

Conclusions and discussion

The U-shaped curve depicting the logic of this thesis has been completed. In the first chapter I moved from the critical realist philosophical principles to socio-technical metatheory to the synthesis of specific substantive theories—Multi-Level Perspective, Desires–Beliefs–Opportunities framework and (Technological) Systems of Innovation. I aimed to show how the frameworks of higher levels of abstraction structure the lower ones, enabling the researcher to remain logically coherent and aware of the alternatives not necessarily present or explicit in the data.

The second chapter tackled various methodological issues related to building a rigorous mechanismic process theory. The immediately practical problems pertaining to data collection, sources of evidence and possible biases were also discussed. The third chapter further increased the complexity by providing detailed historical narratives about ten attempts to create, diffuse and use personal computers in the Soviet Baltic states, roughly between 1977 and 1992.

In the fourth chapter I gradually started making more general statements on the basis of the historical narratives. As a result, three different middle-range theories, each operating on a different level of aggregation, were constructed: one concerning the evolution of a single socio-technical network in a changing environment, one focusing on the interactions of these networks in creating and maintaining a local dominant design, and one explaining the transformation of local innovation systems in the face of an increasing openness to Western influence. Finally, chapter 5 reflected on the metatheoretical and philosophical implications of different issues encountered during the whole research journey. The socio-technical metatheory was substantially clarified with regard to the conceptualization of rules and meanings. A justification for retaining critical realism as a useful starting point of enquiry was also provided.

In the following section, I will start by providing answers to the middle-range research questions presented in chapter 1. I will then move on to highlight other contributions of the thesis as they extend beyond the construction of specific substantive theories. The subsequent section will relate the middle-range and metatheoretical results to the existing state of knowledge, thus highlighting the wider significance of the thesis. The final section indicates the parts of the work that could be improved on and points to future research opportunities.

Main contributions of the study

The middle-range part of the thesis raised the following questions:

1) What explains the success or failure of each PC project? What are the patterns of case development? What are the respective intra-case mechanisms?
2) How were the dominant lines of PCs established? What are the patterns of interaction of cases in each country? What are the respective inter-case mechanisms?
3) How did the Technological Systems of Innovation evolve in each country? What are the patterns of system-level development?

The theory-construction exercise in chapter 4 provided the following answers for different levels of aggregation:

1) Intra-case level:

   a) The success or failure of a PC project is dependent on a number of factors, the importance of which change over the course of the internal development of the socio-technical network and its environment. Especially in the conditions of rapidly changing environmental conditions, there is unlikely to be a single model able to capture the whole sequence of network-internal and network-external processes. The process is likely to be better understood as a modular sequence of separately-theorized episodes.

   b) The driving mechanisms of case evolution are the attempts at network creation/expansion or the contraction/disintegration of the network that result from the changing desires, beliefs and opportunities of the participants.

c) The realization of these mechanisms is affected by a number of background factors: type of environmental change, phase of network development, strength of the leader/network, strength of the vision, the extent to which the vision is shared, strength of the partner, the extent to which the functional niche has been occupied by other players/networks, the presence of local actors not engaged in similar projects, expectations about the possibility to continue the project and prior experience of the performance of the network.

d) Depending on the timing of events, the interaction of environmental changes and local socio-technical networks results in different patterns of development. In the absence of particular environmental pressure, network-formation is likely to be gradual and drawn-out; so is the phase-out of an existing network. Domain-related reform provides a strong stimulus and leads to the emergence of clear visions. Strong committed networks are created while existing weaker networks may attempt to change the usage function of their PC (re-domaining). The process is relatively quick and has a clearly directed nature. Avalanche change speeds up the processes of network-formation and contraction even more. However, as uncertainty is high and the preferences and opportunities in constant flux, stable networks are very difficult to maintain. Networks, even if created, are likely to be weakly committed, with abrupt disintegration of existing networks a dominant trend. Finally, positive network-specific shocks accelerate the move from one stage of development to another (e.g. from prototype to mass production). Negative network-specific shocks retard development, leaving the network stuck in a drawn-out phase. Parallel environmental developments can subsequently lead the participants to change their preferences and to abandon the project altogether.

e) It is possible to distinguish between opportunistic and vision-directed networks. Opportunistic networks aim to act quickly, flexibly and often unofficially to make the best of various environmental opportunities. On the other hand, the ties between the participants are often weak, meaning that such networks are more susceptible to disintegration or moving on to other activities as soon as negative environmental signals appear. Vision-directed networks are more strongly committed and persist longer, even when they have experienced some negative environmental stimuli. However, their durability can become a weakness when blindness to alternatives sets in. The choice between an opportunistic or vision-directed strategy is likely significantly influenced by the structural position of the participants, with weaker leaders/players having to resort to an opportunistic strategy more often.

2) Inter-case level:

a) The emergence of local dominant designs can be understood as a sequence of two underlying mechanisms: opportunity space capture and resulting coping strategies. The first involves the focal network changing the beliefs of the decision-maker, whose backing then opens up the opportunity space for the focal network to realize its vision. It is hypothesized that the mechanism comes into play when decision-makers and producers are active, but users remain divided in opinion or indifferent. Mobilized resistance from decision-makers or users, on the other hand, blocks or delays the realization of opportunity space capture.

b) The successful opportunity space capture triggers the coping strategies of other networks. In the observed cases the networks' desire to fulfil the niche remained, but the beliefs about the best ways to do so changed. The strength of the network capturing the opportunity space and the overall shortage characteristic of the Soviet economy likely explain the choice for this strategy —direct competition with the dominant network was out of the question, but the functional niche was insufficiently fulfilled (actually and expectedly) to warrant further attempts.

c) The crucial factor preceding the initiation of the opportunity space capture mechanism is the motivation to engage in network-building: the absence of this motivation explains why no dominant local school computer design emerged in Latvia. The factor that made a difference to the pattern of emergence of the dominant design is the general unity of preferences in the locality. In Estonia the preferences were proactively shaped, leading to a broad consensual alliance in favour of the local design. In Lithuania the users remained divided or insufficiently informed and thus the decision-makers favoured the proposal of the strongest actor, leading to the 'domestication' of an external design.

3) System-level developments of each country could be captured with a single, four-phase model:

   a) Attempts to reform the existing system result in limited environmental stimulation from the viewpoint of local actors. Since the stability of the system is not compromised, network-building increases. The quantity of Western computers remains very low and limited to a few top functional domains.

   b) The intensity of the pressure gradually increases, opening up more and more opportunities. Domestic network-building continues and the quantity of Soviet computers continues to increase. Because of various limitations (e.g. price, accessibility of foreign currency), the proportion of Western computers may decrease, although their absolute numbers continues to climb.

   c) A disruptive pressure escalates into an avalanche change, resulting in a frantic search for marketable products by the local networks. The official limits to the flow of goods start to diminish, but the price of Western computers remains high. Thus the local network-building attempts may continue for some time, although expectations of their viability are low. Most of the new projects are soon abandoned and the existing ones ended as the preferences and the opportunities of the participants change quickly.

   d) The innovation system, decoupled from the Soviet Union and reoriented to the Western world, starts to stabilize. Decreasing prices allow the gradual substitution of Soviet technology for Western PCs in a few years. A shift from domestic hardware design and production to assembly from foreign components and/or import takes place. Phase-out in user domains completes the overall transformation a few years later.

These are the main middle-range theoretical results. However, the study also makes additional, wider contributions:

1) Historical: so far systematic works on Soviet computing have been few and far between. Moreover, to my knowledge there is no extensive treatment of Soviet personal computing. Therefore, this thesis provides substantively novel historical knowledge about a field that has been little studied to date.
2) Methodological: the analytical technique combining the intuition of multiple STS studies and the rigour of grounded theory is, to my knowledge, a novel contribution to generalization from historical narratives. The technique, which starts by making the most basic generalizations then proceeds with step-by-step addition of more theoretical nuances, enables the researcher to gradually add more detail to his or her emerging middle-range theories. The advantage is that the analysis can be stopped at any point, yet the analyst still has a theory of some kind. This overcomes two problems—being overwhelmed by data and remaining content with simplistic generalizations —the traps of grounded theory and STS, respectively.
3) Metatheoretical:

   a) The theoretical framework operating on three levels of abstraction – philosophy, metatheory and specific substantive theory – where the more general levels guide and structure the more specific ones is a novel approach. It helps to ensure the logical coherency of one's theoretical synthesis, increase one's sensitivity to additional factors one's theory could take into account and makes the researcher's assumptions explicit to the reader. As such I would claim that this approach eases the translation of general issues into specific problems and vice versa (e.g. it enhances one's capability to reflect on the metatheoretical or philosophical implications of one's research).

   b) The basic components of the socio-technical metatheory—actors, technologies, rules—were complemented with the fourth category of meanings. It was argued that rules could be conceptualized as a subset of meanings.

   The distinction between different types of rules as offered by Searle (1995) and Scott (1995) was demonstrated to be neither mutually exclusive nor exhaustive. This task was achieved by recategorizing the rules along two different axes: constitutive–instrumental–moral and implicit–informal–formal.

4) Philosophical: four defences derived from the research practice were offered to justify the realist position. Simultaneously an argument was put forth that the meaning-centred approach threatens to lead to an incomplete understanding of socio-technical processes and to maintain certain analytical blind spots.

Significance of the findings

It is the challenge of every theory-construction exercise to link the results to the existing frameworks. Therefore, in this section I will focus on the following question: which domains of literature could benefit from this thesis? I will begin the discussion from specific substantive theories and the system-level analysis.

Although by identifying some mechanisms not found elsewhere in the DBO literature and making some minor refinements to the conceptualization of environmental impacts not present in current writings on MLP, I think that the main contribution on the highest level of aggregation is made to the Systems of Innovation literature. That is, the four-phase model goes beyond noting the importance of the environment for the internal dynamics of the system and actually includes the dynamics of both in a single framework. In that manner it overcomes the internalist tendencies of SI works that, even when embracing a dynamic perspective, still have little to say about the environment of the system (see also Högselius 2005: 297–301).

In this regard I would like to draw attention to the bulk of literature largely excluded from this thesis: activities of innovation systems. Researchers working in this direction have grouped the activities taking place in the system by seven different functions that they can serve (the contribution itself can be positive, neutral or negative, see Hekkert *et al.* 2007, Bergek *et al.* 2008). Examples of such functions are entrepreneurial activities, market formation and knowledge diffusion. Recent work has focused on detecting the 'functional sequences' of innovation systems—for example, innovative activities leading to knowledge development and resource mobilization (Hekkert & Negro 2009: 591). The authors point out that the importance of activities might differ depending on the phase of system evolution (Bergek *et al.* 2008: 419), that a few recurrent sequences can be distinguished and that some functions are the 'key drivers' (Hekkert & Negro 2009: 591–592).

To me, however, these results illustrate perfectly the points made in chapter 4: that the theory can be too sophisticated in some aspects and too impoverished in others. In principle these functional sequences bear a strong resemblance to the key node sequences detected in the intra-case analysis—only that, in addition to operating on different levels of abstraction, these sequences were made up of properties, not activities. As such most of the criticisms made for those key node sequences also apply for functional sequences: there is no distinction between network-internal and network-external events (meaning that this interaction remains underconceptualized), the groups underlying the sequences are obscured and the occurrence of sequences does not imply causal connection. In addition, we learn little about the transformations of the system, i.e. the situations in which the properties of the system change decisively (e.g. from formative to growth phase). On the other hand, the vocabulary that does exist distinguishes between many functions. When this is coupled with an analysis in which multiple parallel events are categorized as belonging to at least one function (Hekkert & Negro 2009) it is no wonder that common and recurrent patterns are difficult if next to impossible to find. This, in turn, makes it hard to theorize the larger segments of system evolution. What follow once again are all-too-familiar statements like *“the dynamics are complex and ... there is not one ideal way of how it can go”* (*ibid.*: 591). Well, yes, but...

My own take mainly focused on the interaction of changing environmental conditions and the preferences of local actors. As a result it was possible to theorize the whole process as an approximately 10-year-long phase of system-internal transformation of technologically underdeveloped countries involving the large-scale substitution of Soviet computers for Western ones, the end of local hardware production (industry) and an accompanying shift in entrepreneurial activities. In MLP terms it highlights the situation in which the avalanche change does not necessarily lead to a transition from one socio-technical system to another, but rather stimulates a transformation within the system (after all, personal computers remained in use). Focusing on major trends in that manner enabled the teasing out of a longer-term pattern that might have been obscured had I solely focused on counting and sequencing the system-internal activities.

This thesis started out by borrowing from MLP and SI. Therefore it was likely at the outset that it would contribute to discussions about the level of aggregation at which both of these frameworks operate. However, as the timeframe of the research allowed me to probe further and further in data analysis, it gradually became apparent that the results of the lower level of aggregation have the potential to speak to other strands of academic literature. This means, however, that the following discussion needs to break the general rule of not including any new material in the conclusion. Brief references to other fields of research have to be made to highlight the potential gaps and connections.

Whereas large-scale and long-term patterns were relatively undertheorized at the system level, the situation is somewhat different for the inter-case level. I am referring, among others, to general models for the emergence of dominant designs or the diffusion of innovations. Tushman and Rosenkopf (1992), for example, have proposed a technology cycle model that consists of four phases: 1) technological discontinuity; 2) era of ferment; 3) dominant design; and 4) incremental change. Rogers (2003), on the other hand, has noted that the adoption of an innovation can be depicted as an S-curve: the users' uptake is slow in the first and the third phase, but rapid in the second. However, from the point of view of this thesis, these models remain too broad as they focus on too a long timeframe and on the aggregate outcomes of the interactions of various networks. Moreover, they are largely internalist—that is, they do little to theorize the dependence of the progression of the cycle on environmental developments. As a result, one does not learn much about how innovations come to be established in different functional domains, how they are adapted for different domains and how the developments taking place in one domain impact the others.

In this sense my own approach resembles the 'biographical' take on the evolution of artefacts, which stresses the need *"to follow* [a technology] *as it evolves, matures or crosses organizational boundaries"* attempting to *"trace the 'accumulated history' of* [a technology] *and show how it continues to influence the structures and practices of later adopters"* (Pollock *et al.* 2003: 320). However, the ambitiousness of this goal—to theorize the whole biography of an artefact, focusing on the multi-site and multi-level long-term development in considerable detail—means that proponents still seem to be devoting much effort to outlining the theoretical and methodological concepts and requirements to which such an approach should adhere (e.g. Williams & Pollock 2012) rather than putting forth a set of middle-range theoretical propositions and hypotheses.

The contribution of this thesis is far more modest than theorizing the whole biography of an artefact. Instead of attempting to (re)theorize the whole technology cycle, the aggregate pattern of diffusion or the biography of PCs, I have shown how a certain line of computers came to be established in a certain functional domain (school computing) and the subsequent implications of this event for other, similar socio-technical networks. It is true that this conceptualization omits quite a lot of complexity and focuses on explicit interactions and only some particular moments in the lifecycles of each PC. On the other hand, this strategy allowed increased precision by explicitly distinguishing between basic underlying mechanisms and some background factors, the combination of which, in turn, explained 1) whether and why the mechanism of opportunity space capture was realized; 2) what pattern this process took; 3) what the subsequent coping mechanisms were; 4) and why they realized. As such I arrived at clear and testable theoretical statements, albeit limited in scope.

It may well be that with such a strategy I have stepped into the very same trap highlighted above—focusing on too much detail and failing to see the big picture. After all, these episodes of network interaction do not cover every aspect of the overall dynamics of each network. What emerges instead is a rather fragmented picture of different episodes in which the interaction was explicit.

At this point, however, the choice to focus simultaneously on multiple levels of aggregation justifies itself once again by allowing one to grasp common patterns behind the specific interactions between networks. Not only does the system-level analysis include a longer timeframe, it also encompasses the moments when the cases evolved in parallel niches. As such it allows attention to be drawn to the common pressures experienced by each PC project. That is to say, the multi-level aggregation strategy allows one to tease out the hierarchy of causes. For example, whereas the inter-case analysis illuminated the significance of the unity of preferences and the motivation to engage in network-building for shaping the realization (pattern) and the underlying mechanism (opportunity capture) of school computerization, the system-level focus illustrated that in the longer term these specific patterns failed to have a lasting impact, since rapid decoupling of the Baltic countries from the Soviet Union and re-coupling to the Western world enabled the actors in all three states to substitute the Soviet PCs, whether locally produced or not, with Western computers. In other words, in the longer term the end result did not depend much on the specific pattern by which the functional domains were initially filled. However, this is not to claim that the conceptualization of these patterns is entirely insignificant—it all depends on the research question. The inter-case focus brought attention to factors omitted or downplayed by the system-level analysis and vice versa. The different perspectives are thus complementary, not competitive.

Finally, there is the intra-case level at which the development of each socio-technical network was individually theorized. Again it seems that my own theory occupies a niche between existing frameworks. For example, Actor–Network Theory has devoted excessive attention to single networks and has even made some crude distinctions between networks that operate at different levels (local and global, see Law & Callon 1992). However, ANT has also continuously maintained a huge gap between the generality of the theoretical vocabulary and the complexity of case descriptions (Geels 2007b: 631–635). Moreover, one of the founding fathers of ANT has made an argument that there is actually no need to go beyond description at all (Latour 1988). Therefore, ANT middle-range theories of individual network evolution are currently absent.

Extensive work, on the other hand, has been conducted in the field of management studies under the rubric of the 'innovation journey'. Longitudinal case studies of different innovations undertaken in the 1980s demonstrated in a familiar manner that *“none of the innovations developed in a simple linear sequence or stages or phases of activities over time. Instead, a much messier and more complex progression of events was observed in the development of each innovation”* (Van de Ven *et al.* 1999: 23). These studies also detected what the authors called 'patterns of commonality' for each period of the journey (initiation, development, implementation/termination). These are essentially recurring events such as the rapid change in the criteria of success and failure of an innovative activity, the frequent occurrence of setbacks or the need to establish relations with other organizations that locks the innovation on a specific path (*ibid.*: 23–24). From the viewpoint of my own study, important shortcomings remain in the approach and results of Van de Ven and others: 1) the analytical focus of the framework is mainly on organization-internal processes, not on the network level; 2) although numerous findings on various specific aspects of the innovation journey have been presented, the picture of the overall development is still one of complexity and uncertainty—the whole journey is basically constituted by a sequence of three phases and some recurrent events in each; and 3) the role of the environmental conditions in impacting the course of the innovation journey remains undertheorized.

My analysis has focused on the formation and contraction of networks of socio-technical actors to explain the success and failure of each PC project. The multi-level perspective sensitized me to the issue that a single overarching model that captured each case is unlikely to be found—unless it is formulated in very general terms and therefore somewhat blandly—since many large-scale environmental disturbances were observed. However, it seems that exactly the same issue was also encountered by Van de Ven and others—and the social contexts of their cases did not experience such major disruptions. In other words, the networks seem to fluctuate even in relatively stable macro-social conditions. I chose to overcome this analytical obstacle by dividing the overall lifecycle of each network into meaningful episodes, including both network-external and network-internal events, and theorizing each of them separately. This strategy enabled me to formulate theoretical proposals about the patterns of network formation and network contraction depending on the state of the network and the environmental conditions. As such it seems to be a novel contribution to the field of STS, as I do not know of any other middle-range, multi-level, co-evolutionary dynamic models aimed at explaining the success or failure of individual socio-technical networks.

Based on this experience of data analysis I would make the following proposal: if the chosen level of empirical specificity does not seem to allow for capturing the whole event sequence with a single middle-range model, yet the analyst wishes to avoid very high-level generalizations (which risk being banal) or giving in to the complexity and abstaining from looking for common patterns altogether, then configurational theory might be a good solution. That is, instead of summarizing the case with one model, certain potentially recurring modules could be identified, with each used to explain part of the sequence. The combination of these modules, in turn, would explain the overall event sequence observed in the particular case. This strategy has two benefits. First, it enables the analyst to make more theoretical use of the rich data available. Second, it enables him or her to detach the modules from the particular case and to test their applicability elsewhere. For example, based on the findings of this thesis it would be possible to locate some instances of major societal disruption and to investigate whether socio-technical network-formation 1) takes place very rapidly (compared with instances where the pressure is more moderate); 2) results mostly in networks with weak ties; and 3) exhibits a high rate of failure.

I will now turn to a brief discussion of methatheoretical contributions, which by definition are much wider in their scope of applicability. As such it is justified to ask: how do these findings help us to conceptualize technological change better?

In chapter 1, raising the awareness of the researcher to possible alternatives and blind spots was identified as one of the advantages of thinking in metatheoretical terms. To take an example from the literature: after a careful discussion about the strengths and weaknesses of Multi-Level Perspective and Systems of Innovation, Markard and Truffer (2008) conclude that actors, institutions (rules) and technologies should all be counted as constituents of an innovation system. On the other hand, the typology of system functions tested on various cases (see Hekkert & Negro 2009) includes knowledge development and diffusion among the key activities (Hekkert *et al.* 2007, Bergek *et al.* 2008). Therefore, the basic assumptions of the SI framework do not embrace the fact that in empirical practice the researchers have been focused on the cumulative feedback of actors, technologies, institutions and knowledge, not only the first three of these. The explicit inclusion of knowledge (meanings) would help to contextualize the middle-range research better by making researchers more reflexive about their own assumptions.

The ways in which this might happen are various, because of the high level of generality of these assumptions. I will bring a brief example from my own experience when, during the course of this research project, I developed a little hypothesis—unconnected to the main focus of this thesis—about the possible self-reinforcing development of innovation systems. It began with an observation of a certain tension in SI literature: although the scholars acknowledged that the actors of the innovation system might not be thinking in systemic terms and claimed to have defined the functions 'analytically' (e.g. Bergek *et al.* 2008: 409), much attention was also paid to the issue of 'enhancing the performance' of SIs (e.g. Bergek *et al.* 2008: 419–422, van Alphen *et al.* 2010: 406–407). The core of this tension became very clear, however, once reformulated in metatheoretical terms: it is a situation of a description becoming a prescription which, in turn, potentially feeds back to a future description of the system. That is, meanings shaping rules shaping meanings. With the problem made explicit, it became possible to search for literature connected to the issue, e.g. the performativity of economics (Callon 1998), distinctions between different types of performativity (MacKenzie 2006: 16–20), critical discussion on the rhetorical use of the SI framework (Miettinen 2002) and debates about whether the approach originated from academic or policy circles (Sharif 2006: 749–752). This in turn allowed me to formulate the hypothesis that the development of an innovation system may be described as a virtuous cycle: 1) scientific analytical description gives grounds for performance assessment including the specification of desirable goals of an innovation system and respective policy advice; 2) this normative advice, when taken up by politicians, becomes implemented in certain policy measures; 3) the creation of fertile conditions and incentives enhances innovative activities and increases cooperation between local players, resulting in 4) a higher degree of actual systemicity when the same locality is analysed in the future. In other words, the initial analytical functionalism may be a causal factor moving the system towards substantive functionalism, through the mediation of normative functionalism. The role of metatheory in enabling the initial abstraction and guiding the following re-specification of the hypothesis is notable.

In a similar manner, the typology of rules provides a structuring map, an analytic umbrella for a number of middle-range propositions. I will briefly point out three possibilities of empirical specification:

1) Moving from descriptive to classificatory typology, e.g. from conceptual definition to assigning cases to types (Elman 2009: 122). For example, it would be possible to re-work Geels's (2004: 906) classification of different types of rules characteristic of different regimes (e.g. science, policy, users, markets and distribution networks).

2) Moving from descriptive to explanatory theory, that is, making *"predictions based on combinations of different values of a theory's variables"* (Elman 2009: 122). Following the discussion in chapter 5, if regulative and normative rules are better characterized as instrumental rules of differing explicitness then one could predict that the corresponding sanctioning mechanisms would be coercion for formal instrumental rules, normative pressure (e.g. shaming) for informal ones and imitation for constitutive ones (Scott 1995: 52, Geels 2004).
3) Exploring the boundary conditions in which the encoded properties of artefacts are perceived as moral or immoral. Here one could re-open Winner's (1999) famous, albeit contested (Joerges 1999b) narrative on the bridges of Long Island and imagine a case in which the low height of the bridges was merely an unintended consequence. Although the buses would still not get through the overpasses, thus preventing poor and black people from entering certain areas, would we still call the design immoral? Would the principle of double effect (Hauser *et al.* 2007) be applicable here, considering that the architect could have chosen differently? Or, if he really did not know any better at the time and could not foresee the full ramifications of such a design, what restrictions would have applied to reversing this choice once the overpasses had already been built? Is there a conflict between moral values? Is it a matter of power relations or one of convenience? Questions abound.

I have highlighted some ways in which the findings of chapter 5 could lead to a better understanding of technological change. However, I have to stress that the debate on this level of generalization should not be restricted to STS, but should also have an impact on mainstream sociology, which still largely continues to omit the material from and developments in STS. When Emirbayer (1997) comes up with a manifesto(!) for a relational sociology that makes only one passing reference to Latour; when Sibeon aims to rewrite the whole sociological metatheory and discusses 'materials' and 'material diffusion' at length, but uses these terms to denote *"discourses, social practices and typifications"* (2004: 167); or when Dant (2006) sets out to sketch a 'sociology of objects' and ends up spending far more space on Braudel and Elias than on the whole field of STS then I would say there is cause for concern. The social systems of the sociologists are not immaterial and do not float in the air independently from their material underpinnings—as the STS community has abundantly demonstrated. But in my opinion, much remains to be done in communicating the implications of this proposition to mainstream sociologists more forcefully.

Shortcomings and opportunities

This thesis has made a number of criticisms of other frameworks. Therefore, at this point it is only fair redirect the critique to my own research. In the following section I will indicate seven weaknesses of the thesis and the means by which they might be overcome:

1) Although the thesis focused on the comparison of the Soviet Baltic states, I managed to interview fewer people in Latvia than in Estonia or Lithuania because of difficulties related to locating relevant sources. This shortcoming was at least partially alleviated by more extensive coverage of written material. However, more oral data would help to add detail to the narratives and strengthen the theoretical inferences.
2) Although I learned about many other cases during the course of my research, not all of them could be covered because of the timeframe of the project and difficulties finding knowledgeable interviewees. This might have some implications on system-level inferences, since not all cases about which the generalizations are made have been covered. However, as far as I know no PC project survived the large-scale societal transition and therefore it is likely that the system-internal transformation model holds.
3) When it comes to the cases studied in depth, the people closely related to hardware and software design could generally be found. However, in some cases the people from the factory that mass produced the PC, especially the top management (notably Baltijets for Juku and Nuklonas for BK-0010Š), could not be contacted. The user side was covered for cases in which the

networks were more extensive. The dispersed and small circle of users made it difficult to find interviewees when dealing with small-scale, customized PC projects, however. More work remains to be done in this area.

4) Since the empirical terrain was mostly uncharted, the most effort was spent on assembling the local micro-narratives. This meant less stress on contextual factors, especially when it came to various macro-level statistics (e.g. product cycle length, product quality etc.). It is unclear at the moment, however, how much this shortcoming can be overcome at all, since a) often the information no longer exists; b) the information might exist, but its location is unknown; and c) even if the information exists it might be grossly inaccurate and misleading (e.g. the inflation rate continues to be very difficult to estimate).
5) The middle-range theoretical concepts were defined rather loosely. The category of vision is potentially problematic—although I attempted to tease out the underlying variables affecting the strength and match of the vision, the conceptualization is still open to the accusation of tautology, i.e. if the network formation fails, one is led to conclude that there must have been something wrong with the vision. I have tried to avoid this threat by indicating as accurately as possible specific features that influenced the underlying processes of network formation and contraction for each episode. However, in case one aims to perform further quantitative analyses to detect the conditions in which these mechanisms are realized and in which they are not, the variables need to be specified more precisely.
6) All the analysed cases were part of a system different from that in the West (e.g. in terms of the availability of resources, the barriers to getting a prototype into mass production or the difficulties with beginning another product cycle). However, the three middle-range theories aspire to be more universal, not merely theories of Soviet innovation.
7) All the cases involve the domain of computing, whereas the three middle-range theories aspire to be more universal, not merely theories of innovation in this domain. Essentially points 6 and 7 are both concerned with a similar problem: are these findings applicable beyond the particular (narrow)

empirical domain? In reply I would repeat the point made in chapter 4—the choices made in the data analysis process define the theoretical population that the cases represent. I believe that in this case the analysis was stopped at the level of generality at which the differences between the Soviet and the Western systems did not play a considerable role. In other words, it is sensible to presume that the same underlying mechanisms of network formation and contraction are quite general in nature, that the patterns of innovative activities generally depend heavily on the intensity of environmental conditions, that the functional domains are frequently captured by appealing to the local key actors, that the innovation systems do experience internal transformations from time to time and so on. Further testing on new cases, of course, would help to substantiate or refute this belief.

In this research project I have tackled various issues on many fronts. I hope that in so doing I have managed to avoid the risk of superficiality. In many respects, however, many promising leads were opened up which I could not pursue at this time. Categorized by different domains, some possible future research opportunities include:

1) Historical: covering all the cases in the Soviet Baltic states to assemble a comprehensive history of personal computing in the region. Another option would be to widen the empirical scope and include the histories of large production unions mainly devoted to PC production elsewhere in the USSR (e.g. Belarus or Kiev). Finally, the macro-history of Soviet personal computing (including the division of labour in the Communist Bloc) and a comparison with Western developments would also be an enticing endeavour. When it comes to the peculiarities of Soviet innovation, however, I would favour an in-depth look into the production of electronic musical instruments, as my hands-on experience with some of them immediately makes me wonder what the producers might have been contemplating to come up with such remarkably low-quality items.

2) Middle-range theoretical: the most obvious way to continue would be to test each of the three theories on more recent cases from different socio-material contexts. On the other hand, to explore the extent of external validity it would also be interesting to take a look at cases from the more distant past instead (provided that they are sufficiently documented). The same goes for domains other than computing. In addition to in-depth case studies that enable the detection of new patterns and underlying mechanisms, large-scale quantitative analysis would help to identify the boundary conditions of mechanism realization for each level.
3) Methodological: the current analysis can be substantially refined. For example, a typology of activities can be included to make the theorization more sensitive to the alternation between characteristic activities and forms (properties) of the networks (or the networks of such networks). More can also be done to embrace the parallel occurrence of events. It would also be interesting to integrate the most crucial factors observed at each level into a computer simulation and to see whether the dynamics between the networks would yield results similar to those actually observed. If not, there might be reasons to suspect that the conceptualization has missed some relevant factors.
4) Metatheoretical: building on the foregoing discussion, one can probe into the question of the hierarchy of rules and meanings. More specifically—if every rule seems to rely on some meanings which, in turn, are the outcomes of more fundamental rule-following (constitutive rules), then what is the exact relation between the two? It would be tempting to develop a general model of the diffusion of rules able to theorize simultaneously short-term interactions, long-term institutionalization and very long-term evolutionary selection. It might also be tempting to sidestep the familiar sociological and philosophical hypothetical pitfalls about the observer's inability to know whether his or her attribution of similar practices to a single underlying rule is actually valid or not, or the claim that rules are constituted anew with each practice, and turn to advances in neuropsychology instead. Findings in this domain may well

lead to a thorough rethinking of the notion of rules.

5) Philosophical: as far as I know, the issue of technological causality still remains to be resolved in a satisfactory manner. On a very general level, the mutual shaping of technology and human activity is now generally accepted in the STS community. On the other hand, if the choices are still made by humans then how exactly does technology affect human action? What is the vocabulary to speak about the impact of the technical? Also, following Vincenti's call (1995) one can attempt to compile the taxonomy and hierarchy of material constraints. Thereby one may also arrive at an answer about the asymmetry between the material properties and the ascription of meanings. This, in turn, would constitute a part of the solution to the conundrum of the degree to which reality can be socially constructed.

This thesis set out to improve on some aspects of STS I found wanting. To avoid placing too much stress on data, the process of middle-range theorizing was taken very seriously. To show the links between theoretical categories and historical narratives, a suitable analytical technique was devised and put into practice. To avoid the micro-trap, middle-range results were offered on three different levels of aggregation. Stylistically, I aimed to make the writing as clear as possible. I also suggested that the historical cases can be used to derive theories applicable to other spatial and temporal contexts. Finally, I argued that not only is it helpful to be aware of one's philosophical and metatheoretical groundings—the experience of the whole research journey also enables one to perfect them and provides a chance to link one's own research to wider sociological debates.

This brings me to the overarching theme of the thesis: it is beneficial, not just possible, to think big, even when researching small. In my opinion, a local and complex empirical focus should never be used as an excuse to avoid either middle-range or foundational issues. When sufficiently rigorous thinking is exercised, one's research may turn out to have various implications across several domains of knowledge, from history to theory, from theory to methodology, from micro to macro, and from specific to general. If this thesis has managed to convince the reader of the feasibility and meaningfulness of this stance then my mission is largely accomplished.

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Meeting protocol about the development and production of Juku between the Institute of Cybernetics, Estron, RET and members of local government. 1986.28.02. In Russian.
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Appendix A. Interviewees and their organizational affiliations

Ališauskas, Ričardas – Computing Centre of Vilnius State University (Vilniaus Valstybinio Universiteto Skaičiavimo Centre), later Centre of Informatics and Prognosis (Informatikos ir Prognozavimo Centre)

Balčytis, Vidmantas – Vilnius State University, later Lema

Bernotas, Marijus – The faculty of Kaunas Polytechnical Institute (Kauno Politechnikos Institutas) in Šiauliai

Červinskis, Jurijs – VEF (Valsts elektrotehniskā fabrika), Technical Research Department

Dagienė, Valentina – Vilnius State University, Institute of Mathematics and Cybernetics (Matematikos ir Kibernetikos Institutas)

Desiukevič, Aleksandr – Sigma

Dinda, Albertas – Švenčionys Zigmas Žemaitis High School (Švenčionių Zigmo Žemaičio vidurinė mokykla)

Drąsutis, Algimantas – Sigma

Eglājs, Modris – Computing Centre of Latvian State University (Latvijas Valsts universitātes Skaitļošanas centrs), Laboratory of the Problems of School Informatics (Skolu informātikas problēmu laboratorija)

Eller, Arvo – Institute of Cybernetics (Küberneetika Instituut), Special Construction Bureau of Computing Technology

Enok, Leo – Palivere Factory of Construction Materials (Palivere Ehitusmaterjalide Tehas), subsidiary electronics production unit

Grigas, Gintautas – Vilnius State University, Institute of Mathematics and Cybernetics

Haavel, Rein* – Institute of Cybernetics, Special Construction Bureau of Computing Technology

Humal, Leo-Henn* – Tartu State University, Laboratory of Electroluminescence and Semiconductors (Elektroluminestsentsi ja Pooljuhtide Laboratoorium)

Jaaksoo, Ülo* – Institute of Cybernetics

Jelle, Kaido – RET (Punane RET), later Institute of Cybernetics, Special Construction Bureau of Computing Technology

Jürisson, Tiina* – Republican Supplementary Training Institute of Teachers (Vabariiklik Õpetajate Täiendusinstituut)

Kaklauskas, Liudvikas – The faculty of Kaunas Polytechnical Institute in Šiauliai

Kala, Ülo* – Kuusalu kolkhoz, subsidiary production enterprise Estron

Karčiauskas, Eimutis – Kaunas Polytechnical Institute

Kazlauskas, Rimantas – Vilnius State University, later Lema

Kivimäe, Aarne – Nõo High School (Nõo Keskkool)

Krivchenkov, Aleksandr – VEF, Technical Research Department

Ļenskis, Igors – VEF, Computing Centre

Leppik, Kalju* – Institute of Cybernetics, Special Construction Bureau of Computing Technology

Malsub, Jüri – Computing Centre of the Ministry of Communications, later Viko

Markevičius, Rolandas – Ministry of Communications and Information Technology

Märtin, Kaarel* – Institute of Cybernetics, Special Construction Bureau of Computing Technology

Matelionis, Saulius – Kaunas Polytechnical Institute

Matulionis, Henrikas – Kaunas Radio Measurement Equipment Scientific Research Institute (Kauno radijo matavimų technikos mokslinių tyrimų institutas)

Paluoja, Rein* – Institute of Cybernetics, Special Construction Bureau of Computing Technology

Paulauskas, Evaldas – Sigma

Prekerienė, Joana – Semiconductor Physics Institute (Puslaidininkių fizikos institutas) of the Lithuanian Academy of Sciences, later Kaunas Radio Measurement Equipment Scientific Research Institute

Pungas, Toom** – RET, Special Construction Bureau

Rätsep, Ülo – Computing Centre of the Ministry of Communications, later Viko

Ruut, Raivo* – 1. Viljandi High School (Viljandi 1. Keskkool)

Sasnauskas, Vitalis – The faculty of Kaunas Polytechnical Institute (Kauno

Politechnikos Institutas) in Šiauliai

Saul, Bruno – Council of Ministers of Soviet Estonia (ENSV Ministrite Nõukogu)

Tajur, Enn – Computing Centre of the Ministry of Communications

Telksnys, Laimutis – Vilnius State University, Institute of Mathematics and Cybernetics

Tingas, Urmas – Palivere Factory of Construction Materials, subsidiary electronics production unit

Toom, Olev** – Tartu State University, Laboratory of Electroluminescence and Semiconductors

Torn, Rain – Computing Centre of the Ministry of Communications, later Viko

Tõnso, Tõnu* – Tallinn Pedagogical Institute (Tallinna Pedagoogiline Instituut)

Tõnspoeg, Tõnu* – Institute of Cybernetics, Special Construction Bureau of Computing Technology

Tõugu, Enn – Institute of Cybernetics

Tovba, Mikhail – VEF, Special Construction Bureau, later Computing Centre

Tüksammel, Tõnu – Kuusalu kolkhoz, subsidiary production enterprise Estron

Videnieks, Pēteris – VEF, Technical Research Department, later VEF Scientific Research Institute (VEF Zinātniskās pētniecības institūts)

Vilgats, Heido – Palivere Factory of Construction Materials, subsidiary electronics production unit

Villems, Anne – Tartu State University, Faculty of Mathematics

Vītiņš, Māris – Computing Centre of Latvian State University, Laboratory of the Problems of School Informatics

Võhandu, Leo* – Tallinn Polytechnical Institute (Tallinna Polütehniline Instituut)

Zalatorius, Juozas – Vilnius State University, later Baltic Amadeus

Židonis, Evaldas – Sigma, Scientific Research Institute of Computing Technology and Informatics (Skaičiavimo technikos ir informatikos mokslinio tyrimo institutas)

Žintelis, Gintautas – Kaunas Polytechnical Institute

Žuks, Jānis – Latvian State University, Institute of Solid State Physics (Cietvielu fizikas Institūts)

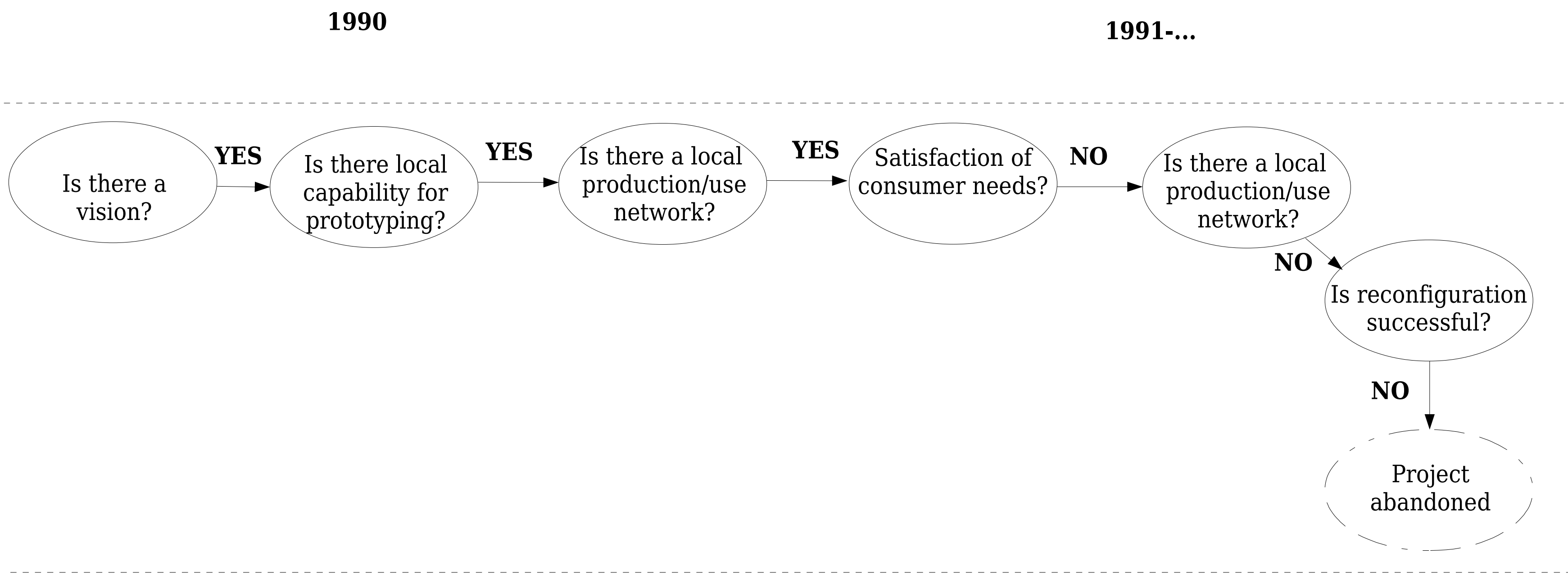
Zlatkus, Giedrius – Computing Centre of Vilnius State University, later Centre of Informatics and Prognosis

* Interviewed for my Master's dissertation (Kanger 2009) and not re-interviewed for this thesis.

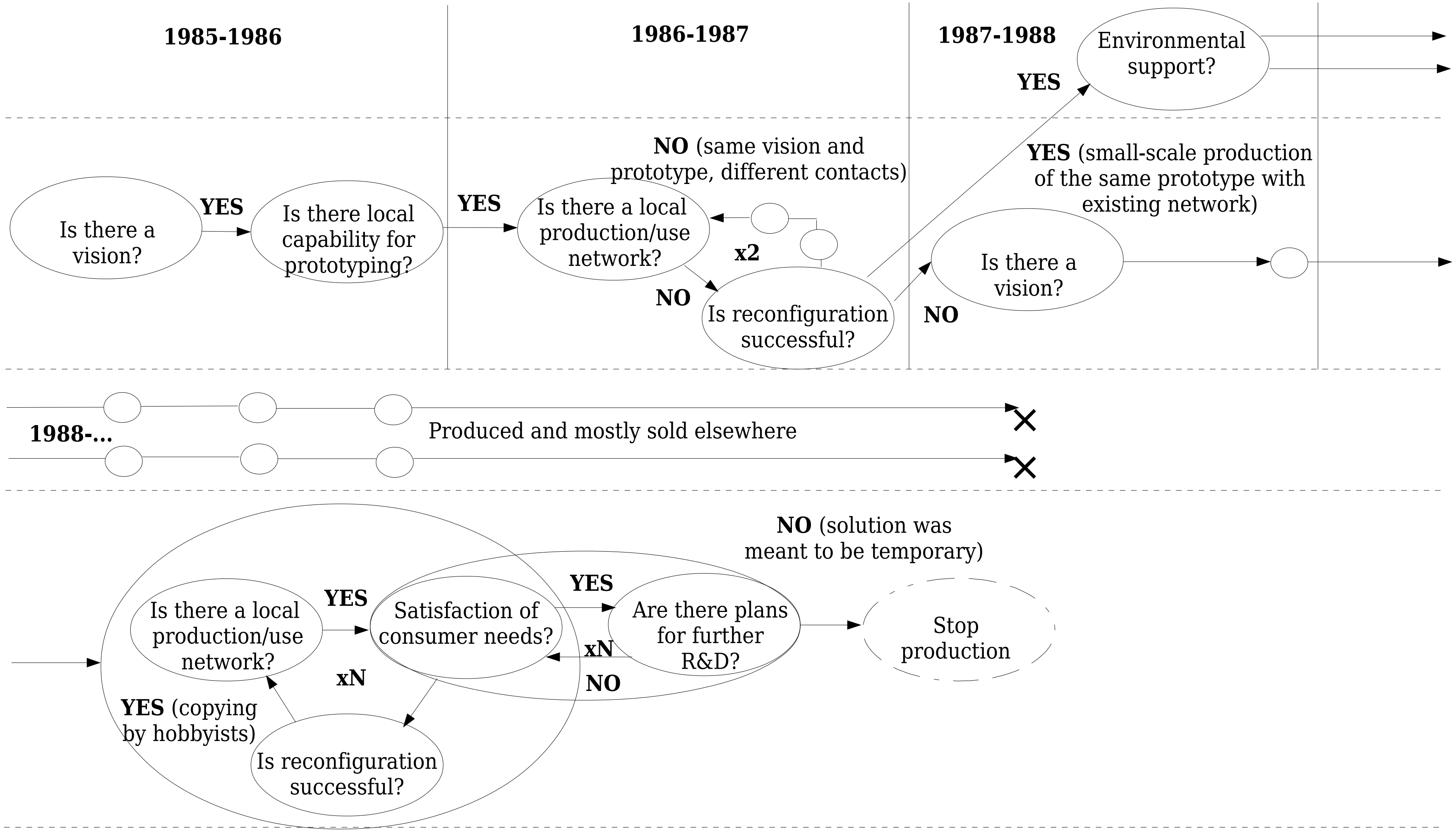
** Interviewed for my Master's dissertation and re-interviewed for this thesis.

Appendix B. Key node sequence maps

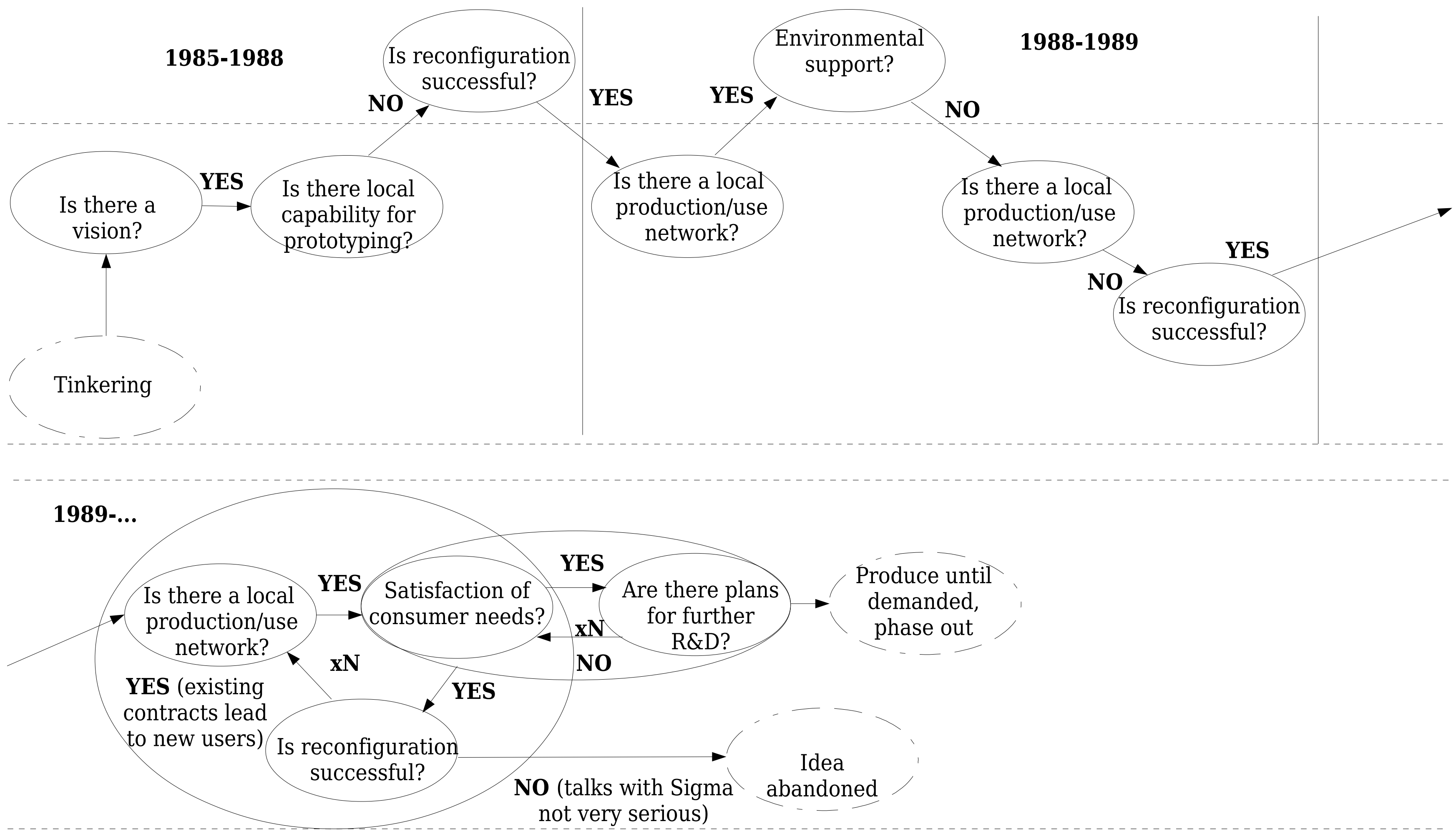
SIGMA 8800



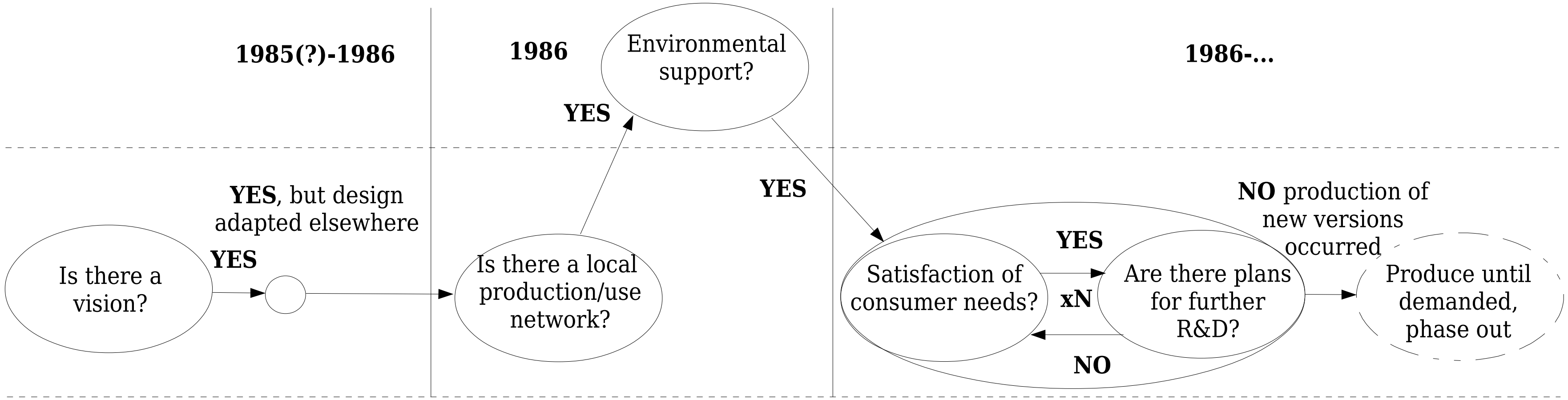
SANTAKA


1985-1986
1986-1987
1987-1988
Environmental support?
YES
Is there a vision?
YES
Is there local capability for prototyping?
YES
Is there a local production/use network?
NO (same vision and prototype, different contacts)
x2
NO
Is reconfiguration successful?
YES (small-scale production of the same prototype with existing network)
Is there a vision?
NO
1988-...
Produced and mostly sold elsewhere
Is there a local production/use network?
YES
Satisfaction of consumer needs?
YES
Are there plans for further R&D?
xN
NO
xN
YES (copying by hobbyists)
Is reconfiguration successful?
NO (solution was meant to be temporary)
Stop production

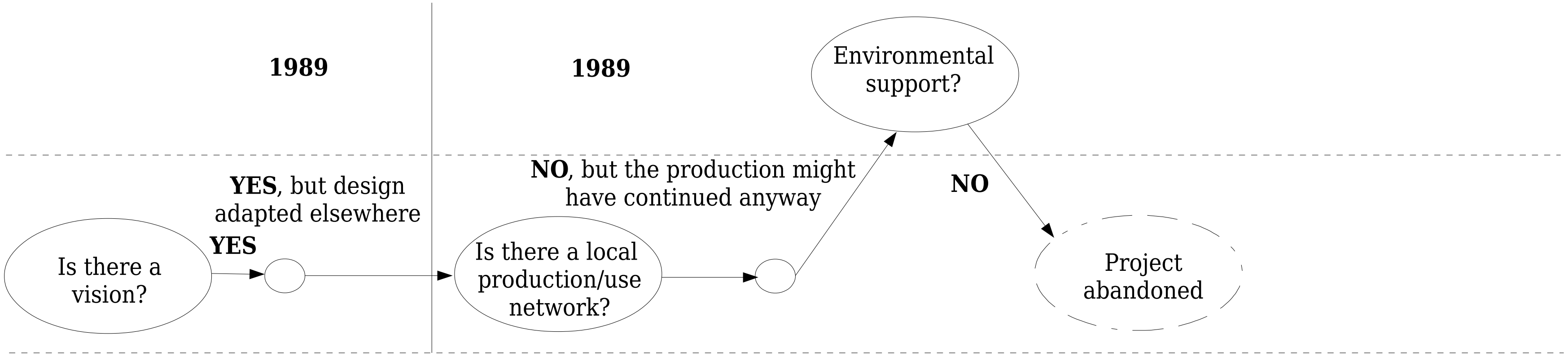
LEMA's PC/XT


1985-1988
Is reconfiguration successful?
NO
YES
Environmental support?
1988-1989
YES
NO
Is there a vision?
YES
Is there local capability for prototyping?
Is there a local production/use network?
Is there a local production/use network?
YES
NO
Is reconfiguration successful?
Tinkering
1989-...
YES
Is there a local production/use network?
YES
Satisfaction of consumer needs?
Are there plans for further R&D?
Produce until demanded, phase out
xN
xN
NO
YES
YES (existing contracts lead to new users)
Is reconfiguration successful?
Idea abandoned
NO (talks with Sigma not very serious)

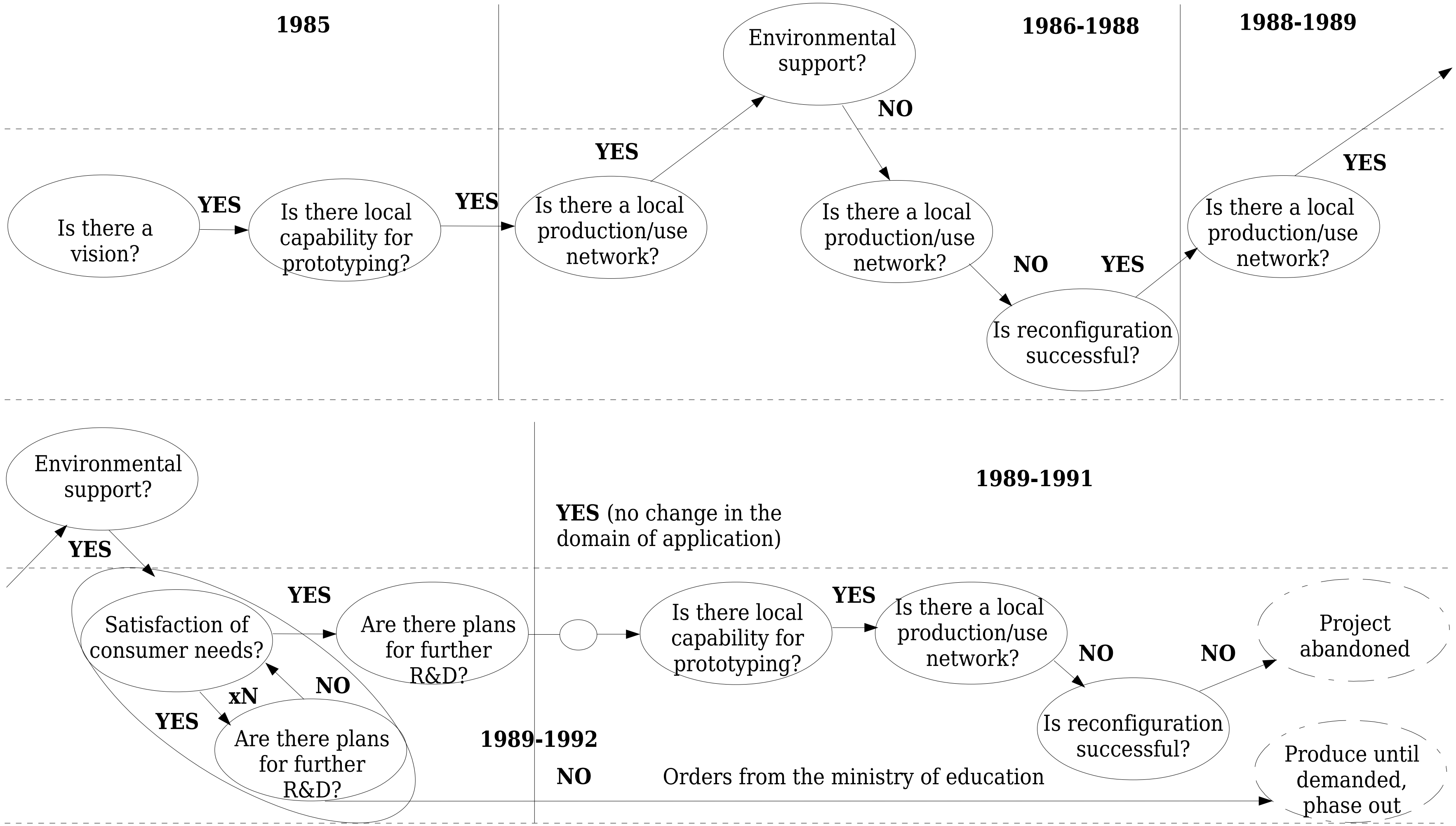
BK-0010Š



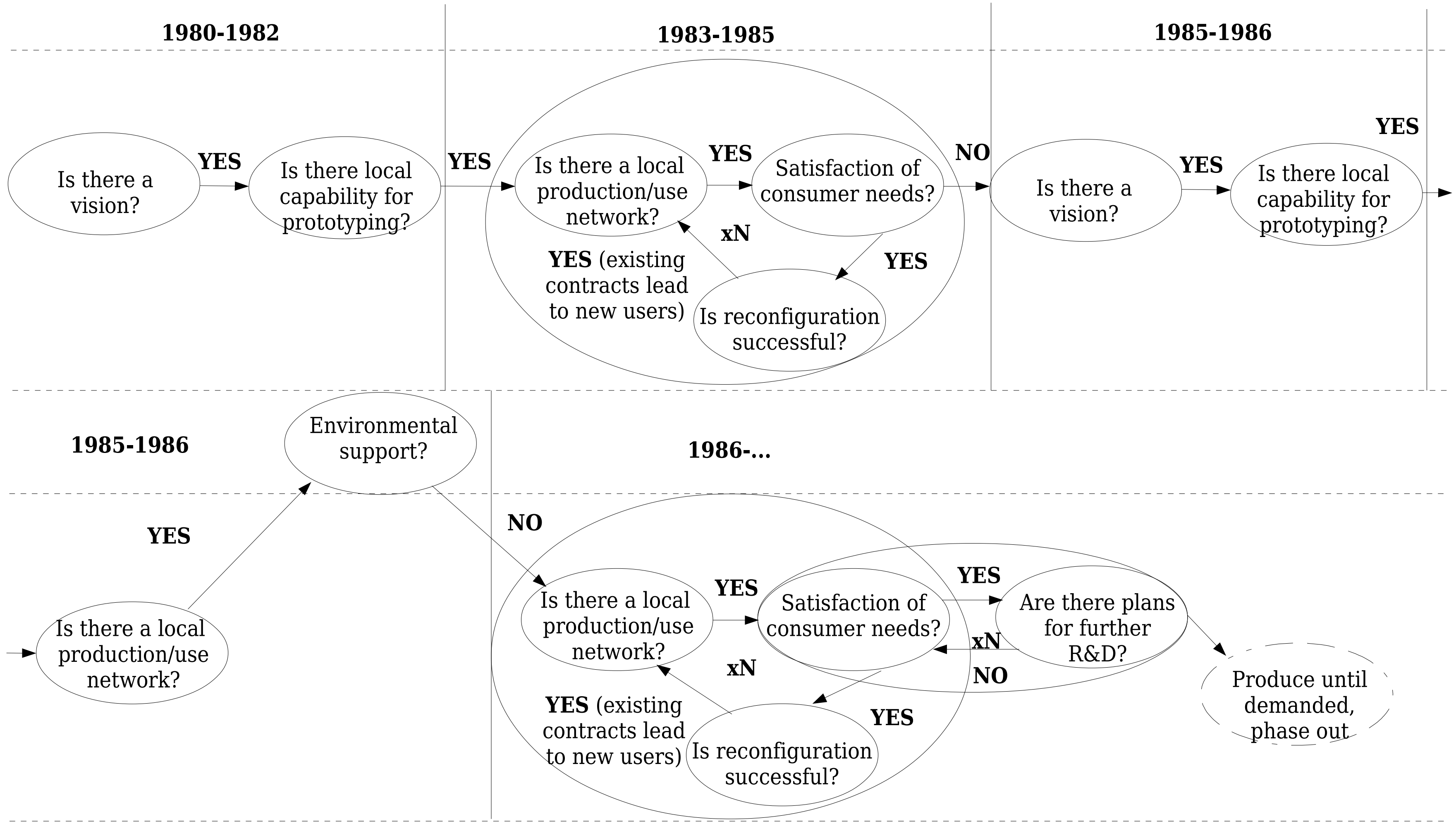
POISK



JUKU


1985
1986-1988
1988-1989
Is there a vision?
YES
Is there a local capability for prototyping?
YES
Is there a local production/use network?
YES
Environmental support?
NO
Is there a local production/use network?
NO
YES
Is reconfiguration successful?
Is there a local production/use network?
YES
Environmental support?
YES
Satisfaction of consumer needs?
YES
Are there plans for further R&D?
xN
NO
YES
Are there plans for further R&D?
1989-1991
YES (no change in the domain of application)
Is there local capability for prototyping?
YES
Is there a local production/use network?
NO
Is reconfiguration successful?
NO
Project abandoned
1989-1992
NO
Orders from the ministry of education
Produce until demanded, phase out

ENTEL


1980-1982
1983-1985
1985-1986
Is there a vision?
YES
Is there local capability for prototyping?
YES
Is there a local production/use network?
YES
Satisfaction of consumer needs?
NO
Is there a vision?
YES
Is there local capability for prototyping?
YES
xN
YES (existing contracts lead to new users)
YES
Is reconfiguration successful?
1985-1986
Environmental support?
1986-...
NO
YES
Is there a local production/use network?
YES
Satisfaction of consumer needs?
YES
Are there plans for further R&D?
xN
NO
xN
YES (existing contracts lead to new users)
YES
Is reconfiguration successful?
Produce until demanded, phase out