

Chapter 3: Methodology and Methods

3.1 METHODOLOGY

A quick reference to the constructivist position of Lincoln and Guba (1985), also known as naturalistic research, could explain my methodological assumptions and thus spare hasty readers who are familiar with and affirmative of this methodology from the first few pages of this section. I do, however, recommend the long route, where I explain the main tenets of naturalistic inquiry for both skeptics and adherents of this methodological tradition. For the former, because I hope a detailed account might make them appreciative of the value of this methodology for the specific purpose of this dissertation, and for both groups, because the long route contains sustainability-related examples, which render my understanding of sustainable development more transparent.

The beginning of my doctoral research is typical for the beginning of any naturalistic study in that I lacked a clear hypothesis. Consequently, there was no foreseeable path determined by standard methodical requirements that would eventually lead me to a pre-structured answer. I did not even utilize the term co-evolution at the outset of my study, let alone the sophisticated thoughts presented in the preceding chapters. These developed over the course of this study, as my own contemplations, interpretations of relevant literature, and encounters with empirical data informed each other. In other words, the real procedure of my research was not structured as linearly as the written product might suggest. It was rather a spiral-like hermeneutic process whose starting point was my frustration with the discrepancy between the high formal output and the low substantial outcome of the sustainability movement.

I thus was intrigued when, a few years ago, I learned about some communities that had achieved remarkable progress toward sustainability. I became eager to better understand what had happened there and why these cases were successful. I did not at that point know the common traits of these cases and decided that the best next step in my research was to learn more about them. The resulting information and a review of relevant literature helped me to tactically select Hasselt and Fürstenfeldbruck as cases for in-depth study (see section 3.2 for a detailed account of the case selection procedure). My

empirical encounter with these cases reshaped my initial ideas to empirically informed conjectures, which in turn enabled me to analyze the case study sites in a more focused fashion. After many cycles on this conceptual roundabout, I noticed that the increase of insight per cycle decreased over time, which I took as an indication that I was closing in on a meaningful interpretation of the two cases under study.

This spiral movement of mutually informing theory and practice did not just happen by itself. It was an intentional process guided by methodological assumptions that correspond to the tenets of hermeneutics. This school of thought has gained acceptance among numerous social scientists as a solid foundation to guide exploratory research, which my study clearly is. After all, I did not attempt to furnish support or falsification for a preconceived, context-controlled, and sharply demarcated claim within a certain interval of confidence. As an adherent of hermeneutics, I acknowledge that it might have been useful to reveal this background at the outset of this dissertation, as it might have influenced the reader's interpretation of the preceding chapters. But considering that everything serves as a context for everything else, I simply had to make a decision regarding where to start.³⁶ Immediately preceding the descriptive accounts of the two case sites, however, the presentation of the underlying methodology and corresponding methods of data gathering and analysis must not be adjourned any further.

Ontological and epistemological premises

My personal ontological premise is that there is a world "out there," not created by men. But I side with Rorty (1989), who suggests that there is a "distinction between the claim that the world is out there and the claim that truth is out there" (p. 4). Hypothetically speaking, if there was something like Truth (note the capital *T*), it would have to be a 100 % accurate description of reality; but any description is language-bound and any language is a human construct. Therefore, any attempt to gauge reality that involves language—and I do not see any alternative—cannot produce a perfectly accurate representation of reality unadulterated by human influence. The concept of a "truth out there" would imply that reality disaggregates on its own account along linguistically defined hyphenation edges into chunks that are congruent with the meaning of human-

made words. If this were the case, then these pieces of reality would fit into holes on a linguistic template like wooden shapes into a child's sorting block. I do not think that reality does us this favor, and correspondingly, I am skeptical about attempts to unequivocally gauge pieces of reality and to re-present them as Truth. Nevertheless, human beings have always tried to get a handle on the seamless whole of reality by pointing to things and labeling them "wood" or "spark" or "iron." This technique, which I suggest calling the "production of facts," has proved very effective because it has enabled human beings not only to communicate but also to build houses, cars, and nuclear reactors. This technique, in Henry James' words, simply "works" (according to Menand, 2001, p. 355).

This approach can only lead to facts about expected manifestations of reality, however. It fails to detect unexpected manifestations, which is noteworthy insofar as human beings are not aware of all manifestations of reality. For example, radioactivity has always been real even before anyone has measured and labeled it. What is even more important is that the Cartesian procedure ends with the production of facts, while their "being" only begins afterwards through human interpretation. Greenfield (1991) remarks in this context that "facts, whatever they are, are less important to us than our judgment of them [because] facts decide nothing; it is people who decide about the facts" (p.103). A mahogany tree, for example, "is" something completely different for a logger than it "is" for a Yanomami hunter. Trees, like all facts, do not have only one "proper" intrinsic meaning for human beings. A knife can be used to cut bread or to kill someone, and neither of these purposes is its *arete*, as Plato would have put it. There is no *arete* of a knife, nor one true application of nuclear fission. Beck (1986/1992) illustrates this point in his explanation that the question of whether or not we should utilize nuclear fission to generate power is contingent, among other factors, on our willingness to bear the risks associated with this technology. The question concerning the "nature" of sustainability thus cannot be answered with reference to absolute Truth either, nor can the question of whether we have to improve technology or change our behavior to get there.

³⁶ Those unhappy about this decision were guided by a footnote on page 1 to this methodology section.

In short, every fact has interpretive slack.³⁷ One of the many possible interpretations of a spark put to work is an engine, which can be used to make a car or a bus work. What this comes down to is that something works because people want *it*, and not something else, to work. Therefore, our challenge as researchers is not fully met when we have produced facts, that is, when we have labeled pieces of reality that interest us at a certain point in time. An indispensable complementary step is to understand what workable interpretation, that is, what application people construct out of these facts and why they do so. Why did the city of Hasselt opt to utilize the orbital space around its city core for more bus lanes and public space instead of car lanes?

Such questions cannot be approached by measuring how often people assign certain pre-cut chunks of meaning to their world—this would be just another linguistic template.³⁸ Instead of such an approach from a seeming Archimedean point, I suggest developing a hermeneutic dialogue between one's interpretation of facts, others' interpretations of the same facts, one's interpretation of others' interpretations and others' interpretations of one's own interpretation (member checks) and so forth. I do not, however, take all statements in this dialogue at their face value because I am aware of their intentionality. This is why I call my approach with Kincheloe "critical constructivism" (1993). This position seeks to identify hidden interests, not in order to filter them out, but to treat them as identifiable voices in the dialogue.

To sum up, the methodology of my dissertation is based on my conviction that reality is "out there" but that we cannot accurately re-present it through language. However, what we can do—and what we cannot help but do—is to label pieces of reality and interpret them. These interpretations are what really matter because they motivate actions. Given this understanding, I conclude that if I want to understand why people do or do not certain things, I have to understand what they declare as facts, how they interpret such facts, what they see as a problem, what they categorize as too risky, and what they see as aesthetically pleasing. The most effective way to reach these insights is to employ the methods described below.

³⁷ This is, by the way, remarkably close to Brentano and Husserl's proposition that perception is always intentional.

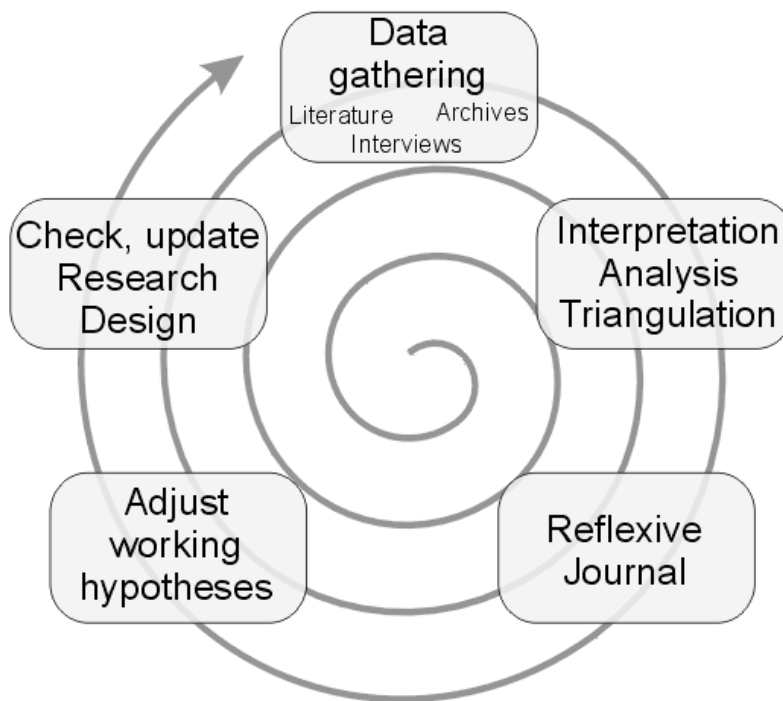


Figure 3.1, The hermeneutic circle

Fusion of horizons

The ideal method of gathering relevant data, then, is to look at the manifest world at my case sites through the eyes of my respondents while wearing critical glasses. This ideal raises a complicated issue, because it is impossible to sneak behind people's eyes without being noticed, as in Sartre's *The Look* (1956, section IV). My mere presence as an enquirer is likely to alter the way people perceive their world.³⁹ In fact, some of my questions to interviewees triggered responses like, "That's a good way to put it" or "That's interesting, I have never thought about that." So the best realistic result, then, is to discursively close in on an interpretation that makes sense to me *and* the actors on site; something Gadamer (1975) poignantly describes as "fusion of horizons" (273). In this

³⁸ This is not to say that quantitative surveys are never useful. On the contrary, they can produce another sort of facts, which can be put to work if they are interpreted meaningfully.

³⁹ Werner Heisenberg has shown that physicists face the same irresolvable dilemma, the so-called Uncertainty Principle.

respect, the distinction between ontology and epistemology becomes fuzzy because "what is known is inextricably intertwined with the interaction between a particular investigator and a particular object or group" (Lincoln & Guba, 1985, p. 110)

Triangulation between archival records, interviews, and literature

It would seem that archival data provides a loophole out of this version of the uncertainty principle because they cannot be retrospectively varnished. But archival records also have been produced and selected for filing with a certain intention—and be it only a special caution due to an awareness of the durability of these records. In addition, archival data might make some sense to the researcher, but how can the researcher know whether his or her interpretations match what once motivated the author to produce an archived artifact? There is no way to mitigate these effects but through dialogue with people who remember. This requires a deliberate confrontation of archival data with other types of information. A mature view of the three types of data I collected (literature, archival records, and interviews) therefore has to accrue from a deliberately propelled dynamic comparison between them. The declared goal of this laborious process is to find the center of the interview-archive-literature triangle. This technique has come to be known as *triangulation* and is intended to lend credibility and trustworthiness to qualitative research (Erlandson et al., 1993, p. 132).

Constant comparison

This confrontation is itself part of a larger dynamic comparison, as shown in Figure 3.1. It depicts the full hermeneutic cycle of my study, through which the research focus, the sources and types of data, my understanding and the research design all constantly challenge and adjust each other. Glaser and Strauss call this the method of "constant comparison" (cited in Erlandson et al., 1993, p. 112), which is a "means for deriving (grounded) theor[ies] ... that is, theories that follow from data rather than preceding them" (Erlandson et al., 1993, p. 112). This is the only method that allows unexpected data to be perceived and processed. In other words, it is the only way to minimize the imposition of pre-conceived perception spaces upon the research process.

3.2 METHODS

If the methodology of a research project represents its strategy, then the methods employed correspond to its tactical devices. The tactics I chose for the study at hand therefore had to resonate with the aforementioned axiomatic assumptions. The remainder of this chapter is dedicated to a detailed description of these methods.

Selection of the two cases

Before I sallied forth into the empirical world, I had to decide where to go. This decision was due at a time when I did not yet know exactly what it was that was unique about the cases in which I was particularly interested. To increase the likeliness of finding patterns among these cases, I first expanded my horizon through systematic searches in best practice databases (esp. www.globalideasbank.org & www.bestpractices.org), through the study of relevant literature, and through personal communications with peers and experts in sustainable community development.⁴⁰ This search produced the following list of potential candidates for in-depth case studies: Helsinki (Finland), Huddersfield (UK), Soldier's Grove (USA), Hasselt (Belgium), Curitiba (Brazil), Fürstfeldbruck (Germany), Chattanooga (USA), IBA Emscher Park (Germany), and the Project Row Houses in Houston (USA). Each of these cases achieved significant change through what I called back then "radical innovations." To further narrow down this list, I established a set of criteria related to five categories: Factors suggested by the literature, my interest in strengthening my expertise in sustainability, relevance for the planning profession, pragmatic deliberations, and methodical requirements.

The literature I was familiar with at that time motivated me to seek cases that were counterintuitive to the conventional notion of progress. On this basis, I did not reject any of the potential nine cases, but I grew sympathetic with Hasselt due to its spectacular departure from the prevailing discourse. My personal academic interest called for cases that had achieved significant steps toward sustainability, that is, that had achieved positive ecological, social and economic effects. Based on this criterion, I rejected Helsinki (see Landry, 2000, pp. 87-90, 119-120, 226-227) and Huddersfield

(Huddersfield Pride, Ltd., 2002) because their sustainability gains were clearly smaller than that of the others. The cases to be selected should also permit valuable lessons for the planning profession, which is why I looked for cases that included many planning-related issues, such as transportation, urban renewal, economic development, the creation of jobs, landscape planning, and environmental protection. This criterion led to the rejection of the Project Row Houses, a "public art project involving artists in issues of neighborhood revitalization, historic preservation, community service, and youth education" (Project Row Houses, n.d.). The call for professionally relevant lessons also made me look for cases that responded to problems of common interest. Based on this requirement, I rejected Soldier's Grove, a community that "relocated its entire business district and a dozen homes to higher ground" (Penn, 1993, ¶ 12) in order to escape periodical flooding. The remaining cases were filtered according to pragmatic criteria such as my language capabilities, which made me disinclined to Curitiba (*Curitiba and its*, n.d.), and accessibility, which recommended Fürstfeldbruck. I already had well-established contacts at the latter site because I had written my diploma thesis about a different aspect of this project. For methodical reasons, I also wanted to find a set of cases that were comparable in terms of time frame and scale. Because I was already leaning toward Hasselt I looked for a suitable match out of the remaining three candidates. IBA Emscher Park (Landry, 2000, p. 91-98) appeared less than ideal because of its scale, covering a population of 2 million inhabitants, compared to the 68,000 citizens in Hasselt. This process of exclusion left Chattanooga (Sustainable Chattanooga, 1995) and Fürstfeldbruck as equally interesting candidates for the second case study. Eventually, Fürstfeldbruck was chosen because of its superior accessibility.

It is certainly worthwhile to muse how I might have come to different conclusions if I had chosen different settings for my in-depth studies. Quite possibly I would have come up with different linguistic tools and with correspondingly different recommendations, which might prove very helpful to a number of communities. This is very much in line with my conviction that there are indeed several paths to sustainability. Hasselt and Fürstfeldbruck helped me find one of them; other studies might have

⁴⁰ I am especially grateful to Charles Landry for his input with respect to this process.

revealed others. The path of co-evolution, however, might be one of the broader paths. *How* broad, may be investigated in future studies.

Data gathering

Literature: The study of relevant literature, combined with my personal experience, can be seen as the entry point into the hermeneutic spiral. I explored the existing body of knowledge in a variety of academic fields such as sustainable communities, individual creativity, organizational innovation, radical innovation, social innovation, creative cities, eco-efficiency, environmental ethics, alternative dispute resolution, participation techniques, pragmatism, the history and theory of urban planning, science and technology studies (STS), and the philosophy of technology. During the preparation of my research proposal, I intensified the reading process, which enabled me to define an initial research question and a working hypothesis and to select the case studies. The review of literature was an ongoing activity, while the selection and interpretation of it became increasingly empirically informed. The bibliography indicates both the depth and breadth of this process. I excerpted most articles and books I read and isolated all arguments on separate index cards (see the description of this unitization process below).

Archival Records: In addition to the benefit of archival records as expressed above, I used them to investigate the local social, economic and environmental conditions of the case cities prior to the introduction of innovations. Two archives provided the majority of historical data for the Hasselt case: First, the electronic archive of the provincial newspaper *Het Belang van Limburg*, from which I retrieved 93 articles in Flemish. Second, the personal archive of "Guido Moerkerk,"⁴¹ member of the engineering staff of the Flemish Department for Transportation, who was personally involved in all stages of the process in Hasselt. In addition, I found relevant information in a number of books in the Limburg provincial library and in numerous publications of the city of Hasselt, the Flemish government, political parties, and citizen groups.

⁴¹ Every time I introduce respondents, whose identity is protected, I present their full pseudonym name in quotation marks.

In Fürstfeldbruck, I studied materials from the archive of the newspaper *Fürstfeldbrucker Tagblatt* at three different occasions for eleven hours in total. The partners of BRUCKER LAND chose not to grant me full access to their archive because it contained proprietary and financial data. The managing director of BRUCKER LAND, Elsbeth Seiltz, however proved extremely helpful in releasing pertinent information. I also revisited my diploma thesis (Brand, 1997) for factual information about BRUCKER LAND. All digitally available items were saved, all others were skimmed on site and photocopied if they appeared remotely relevant. The subsequent analysis process is described further below.

Interviews: I had existing contacts in Fürstfeldbruck from my diploma thesis research and in Hasselt from a visit to the city in the summer of 2000. I approached and interviewed these contact persons first and solicited information about other potential interviewees from them in order to start what is known as "snowball sampling." Each tier of people interviewed generated more names for suggested interlocutors, which led to two effects. First, some names were mentioned repeatedly, which I took as an indication of consensus about that person's importance and second, the total number of names climbed asymptotically to roughly 30 people in each case, which gave me the liberty to pursue a more and more purposive sampling method. (Erlandson et al., 1993, pp. 147-148) The purposive selection of respondents was guided by considerations that appeared relevant to the emerging theory at that specific stage and/or by the reported importance or expertise of a person. I also strove to achieve a balance between the major stakeholders involved in each project.

In the case of Hasselt, I could easily identify the former mayor as a key respondent because he is mentioned with praise in all project descriptions I read before I began my field research. These documents also suggested the local administration, representatives of all political parties in the city parliament, and the public transport company as the main actor groups in Hasselt. Some of these initial interviewees and archival records pointed to further key players, namely representatives of the municipal and provincial administration and of consulting companies. I also made sure to hear the opinion of academics, representatives of the media, and critics of the project—their

inclusion was suggested by my a priori intention to implement the adjective in "critical constructivism" in my research.

The major stakeholders in Fürstenfeldbruck were easy to identify as the self-defined "columns," the five social groups that are represented as equal partners with equal voting rights in the statutes of BRUCKER LAND: Farmers, food craftsmen, environmentalists, consumers, and members of the local Catholic and Protestant churches. As in Hasselt, I also actively looked for respondents who were critical of the project, for academics knowledgeable of the project, and for local journalists who had observed the project over the years.

Some of my interviewees wore more than one of these hats, which allowed me to keep the total number of respondents to a manageable number of 15 in Hasselt and 11 in Fürstenfeldbruck. Besides these official interviews, I also conducted numerous informal conversations with citizens, bus drivers, shopkeepers, etc. The criterion of when to stop the "process of interviewing, analyzing and identifying new respondents" (Erlandson et al., 1993, p. 124) followed Erlandson et al.'s recommendation to "continu[e] until information becomes redundant" (p. 124).

Each interview began with a brief introduction of myself, followed by a quick presentation of my study and the signing of an "informed consent" sheet (see below). The latter included the consent to record the conversation on a micro-tape, which I explicitly re-enquired orally and which all interviewees confirmed affirmatively.⁴² The content of each interview was guided by the current working hypothesis. The issues contained therein were cast into a protocol of 10 to 20 questions for each interview. Some of these questions were included in every interview protocol, whereas others were eventually omitted and new ones emerged in light of the evolving theory. I used this protocol to keep each interview focused, but I preserved the flexibility to let my respondents drift to other issues in order to allow non-expected information to surface. I often double-checked my understanding and, when necessary, asked for elaboration. The duration of the interviews ranged from 35 minutes to 3 ½ hours (over three meetings), with

⁴² In some cases, I felt it would be helpful to record the early stages of an interview with field notes rather than audio recording in order to decrease respondent inhibitions. At some point I later asked for permission to use the tape recorder.

interviews averaging 52 minutes. I digitized each interview as soon as possible and converted them into mp3 audio files, thus easing access, storage, and transcription.

The informed consent agreement has been approved by the Departmental Review Board; it included a guarantee to interviewees that I will keep all information that can be identified with their names strictly confidential. For this reason, I use pseudonyms to refer to specific people throughout this book and to identify the corresponding tapes, files, and other records of these interviews. The pseudonym keys are stored in a 128-bit encrypted file. This protection of anonymity was not granted to public figures such as the former mayor of Hasselt, Steve Stevaert, or the managing director of BRUCKER LAND, Elsbeth Seiltz, however.

Analysis and interpretation

The constant comparison method prescribes that all pieces of data are constantly and systematically assessed. This analysis was performed during the fieldwork phase through the writing of a reflexive journal (see below). After the first round of data collection and at the very end of the data-collection phase, several very thorough and systematic analysis procedures were conducted. For this purpose, all relevant data collected at this point were transcribed. The transcription of all interviews resulted in roughly 104,000 words of written text, not to mention the excerpts of literature and the transcription of archival data.

This material was then "unitized" into "the smallest pieces of information that may stand alone" (Erlandson et al., 1993, p. 117), which ranged from 10 to 200 words. Each "unit" was printed on a 3 x 5-inch index card, with the color of the card indicating the type of source (archive, interview, or literature). The text itself was also printed in different colors. I chose black for original text; my own comments were printed in green, and red was chosen to highlight certain words to help me quickly ascertain main arguments during the subsequent sorting process. Some units were printed both in a stand-alone version and as context for other units; for the former function the print was black, for the latter, gray. The total number of index cards as of March 2003 is roughly 2700 and is still growing from new literature excerpts.

The content analysis was performed as described by Hodson (cited in Erlandson et al., 1993, p. 119) through several rounds of sorting and re-sorting of all index cards. The purpose of this process was not to seek corroboration for a preconceived scheme but to search for "consistencies, discrepancies, anomalies and negative cases" (Hodson cited in Erlandson et al., 1993, p. 112) in the data. In other words, I did not try to check whether the data fit into a preconceived pattern but to find out what pattern the data themselves suggested. I used the first round to explore the possibilities of categorization. For this purpose, I assigned about 500 randomly selected index cards to groups that were derived from the current working hypothesis. Afterwards, I shuffled the cards back into one pile and assigned another set of 500 cards to emerging groups. The first full round of sorting of all of the cards helped me to discern tentative categories and especially to get acquainted with the range and complexity of the data. The results of this process also helped me to identify gaps, which were subsequently filled through a second round of interviews conducted via telephone, mail or email. Two more full rounds of sorting followed at intervals of one month, with each round resulting in a scheme of categories that increased my confidence about its usefulness. I speak of usefulness to emphasize that this scheme does not reflect any higher Truth; it is merely a tool to structure the data I gathered and thus to structure this dissertation. It is a device to describe existing cases of co-evolution, a compilation of "memes" (see chapter 6) that might prove useful to devise future cases.

Reflexive journal

I am aware that early working hypotheses tend to create self-momentum toward a constant provisional arrangement. In order to obviate this trend, I kept a reflexive journal (Erlandson et al., 1993, pp. 143-145)—with 18 entries in total—to force myself to make every step of my evolving theory as transparent as possible. This served as a major element in the audit trail, thus furnishing credibility to my study. It also helped me to trace the path of my deliberations, to detect detours, shortcuts, gaps and unexplored territories. The reflexive journal also proved useful for "parking" certain ideas in order to revisit them later in light of new evidence. This effect was possible because I re-read the journal entries from one, four and ten weeks prior each time I started a new one. The

journal contains explicit statements about evidence that did and did not fit into the current working hypothesis. Thus the re-reading routine ensured that no thought or evidence got lost, which was important because doubtful evidence may evolve over time into something more meaningful, and vice versa. Each entry in the reflexive journal also contained a list of activities completed since the last entry and a list of tasks to complete in the following days. The average length per entry was 497 words; the average interval between entries was 11 days during the fieldwork phase.

Adjustments to the working hypothesis

In my dissertation proposal, I developed a working hypothesis as an entry point into the hermeneutic circle. Insights gained from subsequent cycles of the hermeneutic spiral made it possible and necessary to adjust this working hypothesis. This iterative process increased my understanding and allowed the working hypothesis to mature from a tentative construct to a firm and empirically supported answer to my research question. This incremental growth toward a meaningful interpretation is one of the main traits and strengths of the constructivist research paradigm. It encourages researchers to learn as they go, thus enabling them to constantly adjust their direction in light of new evidence. In the reflexive journal, I explicated every such adjustment to my working hypothesis. Major adjustments resulted from the first round of sorting. The subsequent second phase of data gathering did not reveal radically different, new data. I therefore stopped gathering data, except for missing descriptive data such as statistical details, and focused on increasingly fine-grained sorting processes. These over time resulted in less and less necessary adjustments to the working hypothesis. I took this as an indication that I was closing in on a meaningful final thesis.

Emergent design

Adjustments to the working hypotheses usually necessitated a slightly different focus of attention, the need to interview a specific person, or a change in the data collection procedure (Erlandson et al., 1993, p. 114). This constant adjustment of the research design is known among constructivist researchers as *emergent design* because it is not statically defined at the outset of the project but emerges as the research project

evolves. I accounted for any such adjustment in the reflexive journal in order to avoid the appearance of arbitrariness. In other words, I remained strategically targeted throughout my research project but took the liberty to react tactically flexible to new evidence.

3.3 VALIDITY

In a Bayesian-like move, I admit my bias toward sustainability and my desire to ascertain quicker steps toward its implementation. The admission of this bias may facilitate a corrective perception of my findings for the reader. My awareness of it helped me to discipline and to critically assess my own work. Given this bias, it is even more important to ensure the highest possible level of *trustworthiness*, which is the constructivist—or naturalistic—equivalent of validity in the traditional research paradigm. (Erlandson et al., 1993, p. 132) The ideal of trustworthiness requires constructivist researchers to meet four sub-criteria, all of which have their counterparts in conventional quality criteria as demonstrated in Table 3.1.

One of these criteria is *credibility*, which corresponds to the positivist ideal of *internal validity*.⁴³ A credible constructivist study has to have *truth-value*, which can be furnished by demonstrating the "compatibility of the constructed realities that exist in the minds of the inquiry's respondents with those that are attributed to them" (Erlandson et al., 1993, p. 30). Techniques I applied to meet this criterion included triangulation, peer debriefing—especially with members of my dissertation committee—the reflexive journal, and member checks. Two of my respondents from Hasselt and three from Fürstfeldbruck performed the latter test by sharing their comments on several drafts of this dissertation with me. Their responses have been incorporated into the final version of this dissertation if they were not contradicted by strong counterevidence. In case my and my "member's" interpretation would have been irreconcilable, I could have taken this as indication that I have to bolster my respective argument; however, such cases did not occur.

⁴³ I endorse the concept of credibility but I hesitate to draw the parallel to "internal validity" as Erlandson et al. do (1993, p. 29). Trochim (2000) states that "internal validity is only relevant in studies that try to establish a causal relationship. It is not relevant in most observational or descriptive studies," which my study is. In contrast, he refers to construct validity as "the degree to which inferences can legitimately be

A second criterion corresponds to *external validity* in the prevailing research paradigm, which is concerned with the generalizability of research findings to the whole "population." Constructivist research deals with this concern by demanding *transferability*, which is ensured if research is applicable to other settings. Because, according to constructivist tenets, every construction depends on its specific context, blind transferability should never be claimed. However, I maintain that the propositions in the concluding chapter have relevance for other contexts—not as sufficient conditions to devise other cases of co-evolution, but as inspiration for other communities to form their own working hypothesis. In other words, the final burden to demonstrate applicability rests with the receiving setting (Erlandson et al., 1993, p. 33), but my research attempts to facilitate this process through a rich description of the projects in Hasselt and Fürstenfeldbruck, thus serving as a quarry for others. My truth claim is therefore quite limited. It does not contain a prescription of a universally valid formula, but rather a simple call for attentiveness to potentials for co-evolution and a suggestion of what these might look like.

My main strategy to ensure *dependability*—the third quality criterion and the pendant to "reliability" in conventional terms—was to make every move of my research transparent in the reflexive journal. Maintained this way, it proves the consistency of my re-constructions and "supports not only the credibility but also the transferability, dependability and confirmability of the study" (Erlandson et al., 1993, p. 143). All parts of this journal that are not subject to ethical concerns are available for other scholars who wish to examine the neutrality, or objectivity, of my dissertation. The same holds for "interview guides, notes, documents, notecards" (Erlandson et al., 1993, p. 148), and, most importantly, interview transcripts. All of these documents are part of the "audit trail" (see Erlandson et al., 1993, p. 143), which ensures *confirmability*—compliance with the fourth quality criterion—of my study.

made from the operationalizations in your study to the theoretical constructs on which those operationalizations were based" (Ibid.) Credibility might therefore correspond better to construct validity.

Table 3.1 provides an overview of these and other techniques that I employed in the research process.

Table 3.1. Methodical techniques			
Criterion	Conventional term	Naturalistic term	Naturalistic techniques
Truth value	Internal validity	Credibility	Triangulation Peer debriefing (esp. with members of my dissertation committee) Member checks Reflexive journal
Applicability	External validity	Transferability	Rich description Purposive sampling Reflexive journal
Consistency	Reliability	Dependability	Audit trail Reflexive journal
Neutrality	Objectivity	Confirmability	Audit trail Reflexive journal
Table adjusted to this dissertation project from Erlandson et al., 1993, p. 133.			