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Handbook of Grounded Theory

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Abduction: The Logic of Discovery of Grounded Theory

Jo Reichertz

We decided to write a book about methods in the mid-1960s. We felt that changes were in the air and wanted to write for the new generation—the people over 30 were already too bound by convention. Barney was more positive about the project. I was more sceptical because I was older. The title indicates what was important to us *The Discovery of Grounded Theory* (1967): unlike the usual method books which are concerned with verification, we were more interested in the discovery of theory 'out of the data'. Grounded Theory is not a theory but a methodology to discover theories dormant in the data' (Legewie & Schervier-Legewie, 2004).¹

Grounded theory (GT), which Anselm Strauss refers to here in an interview decades later, is one of the most successful methods ever developed and has added a more qualitative note to social research. This is, however, not a result of the clarity and simplicity of this method established by Barney Glaser and Anselm Strauss but is rather due to the fact that it counteracts the common prejudice, which is to some extent entertained in science, that theories quasi emerge by themselves from the data (without any previous theoretical input). According to this belief, one only has to evoke the theory inherent in the data by means of suitable methods, the theory would then become apparent without the active actions of scientists. The theories are thus believed to emerge slowly in a process of gradual abstraction from the data. Therefore, one of the most famous quotations from *The Discovery of Grounded Theory* is the following: 'Clearly, a grounded theory that is faithful to the everyday realities of the substantive area is one that has been carefully induced from the data' (Glaser & Strauss, 1967: 239). The incorrectness of such an inductive procedure has already been proven by

Popper in general and, with respect to GT, by Kelle (1994, 2005) and by Strübing (2004: Chapter 27) in particular. Many users of GT therefore regard this approach as an inductive method and are of the opinion, 'that the approach signals a return to simple "Baconian" inductivism' (Haig, 1995: 2). Representative for many others, here is an example from *Qualitative Research in Sociology: Grounded Theory* 'is known as an inductive or ground-up approach to data analysis' (Marvasti, 2004: 84). At first the two founders of GT shared this view: 'From its beginnings the methodology of Grounded Theory has suffered from an "inductivist self misunderstanding" entailed by some parts of the Discovery book. Although this inductivism plays a limited role in research work of many Grounded Theory studies (including those of the founding fathers) it has often lead to confusion especially among novices who draw their basic methodological knowledge from text books' (Kelle 2005: Chapter 24).

The fact that original GT has split into two directions (differing in the emphasis on the meaning of prior theoretical knowledge for research) became evident in Strauss's *Qualitative Analysis for Social Scientists* (1987), and certainly was clear in *Basics of Qualitative Research* (Strauss & Corbin, 1990). In both texts it was argued that theoretical pre-knowledge flows into the data's interpretation while Glaser insists that the codes and categories emerge directly from the data. The differences were made public in Glaser (1992; see also Glaser, 2002). The differences have been the object of heated debates (Kelle, 1994; Kendall, 1999; Miller, 1999; Strübing, 2004) in the scientific literature since then.

This later position found in both Strauss and Strauss and Corbin takes into account that observation and the development of theory are necessarily always already theory guided. 'Every type of inquiry rests on the asking of effective questions' (Strauss & Corbin 1990: 73). Furthermore, it also allows for the fact that scientists must be in a position to modify or even reject concepts during and due to observation. With this logic of research, GT falls within the realm of abductive research logic. Neither Strauss nor Strauss and Corbin have systematized this logic of abductive discovery, nor have they linked it to the considerations of Charles S. Peirce, the founder of abduction. To an extent their work can be read in other ways.

This contribution aims to close this gap: The purpose of this chapter is to examine whether the logic of later GT (Strauss from the 1980s onwards) is actually abductive. This might contribute to a better understanding of the nature of empirically GT construction. First, an adequate description of the abductive logic following Peirce is needed and second, an assessment is required whether the GT of the later Strauss (together with Juliet Corbin) can be reformulated with the ideas and notions of Peirce. Thus my thesis is that GT was to a very small extent abductive from the start and became more and more abductive in its later stage; at least in the work of Strauss. Thus the Glaser–Strauss controversy can be characterized, at least in part, as one between induction and abduction.

ABDUCTION: A RULE-GOVERNED WAY TO NEW KNOWLEDGE

Social researchers who take an interest in the fluctuation of their own professional vocabulary have been able, for more than two decades, to witness the flourishing of a concept which is around 400 years old: it concerns the term *abduction*. The boom has been so significant that we sometimes hear talk of an 'abductive turn' (Bonfantini, 1988; Wirth, 1995). First introduced in 1597 by Julius Pacius to translate the Aristotelian concept *apagoge*, abduction remained quite unnoticed for almost three centuries. It was Peirce (1839–1914) who first took it up and used it to denote the only truly *knowledge-extending* means of inferencing (so he claimed) that would be categorically distinct from the normal types of logical conclusion, namely *deduction* and *induction* (1973, 1976, 1986, 1992). Several decades were to pass before Peirce's ideas were systematically received and adopted (Anderson, 1995; Apel, 1967; Fann, 1970; Hanson, 1965; Moore & Robin, 1964; Reichertz, 1991, 2003; Tursman, 1987; Wartenberg, 1971).

Today the term 'abduction' has become something of a byword within social research (but not only there): educationists, linguists, psychologists, psychoanalysts, semioticians, theatre-scientists, theologians, criminologists, researchers in artificial intelligence, and sociologists announce in their research reports that their new discoveries are due to abduction. The great success of abduction, in my opinion, may be traced back to two particular features: first to its indefiniteness and second to the misjudgment of the achievements of abduction that derive from this. Frequently, the use of the idea of abduction has led many of its users to one particular hope, that of a *rule-governed* and *replicable* production of new and *valid* knowledge. This hope is found, above all, in artificial intelligence research and in a number of variants of qualitative social research (e.g. Charniak & McDermott, 1985; Hemker, 1986; Knorr, 1985; Kreppner, 1975).

These approaches have in common that they stress both the *logical* and also the *innovative* character of abduction. For abduction is no longer treated as a traditional, classical means of drawing conclusions, but as a new method that is not yet incorporated into formal logic. However, it is, in every sense, a means of inferencing. It is precisely in this quality of being a 'means-of-inferencing' that we find the secret charm of abduction: it is a *logical* inference (and thereby reasonable and scientific), however it extends into the realm of profound insight (and therefore generates new knowledge). The secret charm of abduction lies straight in this kind of inference-being: abduction is sensible and scientific as a form of inference, however it reaches to the sphere of deep insight and new knowledge. Abduction is intended to help social research, or rather social researchers, to be able to make new discoveries in a logically and methodologically ordered way.

This hope, to be able to make new discoveries in a logically and methodologically ordered way, is directed against Reichenbach (1938) and Popper (1934) who, by separating the logic of discovery from the logic of justification, 'drove' the first into the realm of psychology, and allowed only the second into the realm

of serious science. This separation should be reversed: the unfortunate disjunction of contexts of discovery and justification should be removed by means of abduction. A rethinking of this kind promises a great deal: liberation from the 'chance of a good idea' (Habermas, 1973: 147), and (it is hoped) 'synthetic inferences *a posteriori*.'

Because of this hope many social scientists have treated, and still do treat, abduction as a magic formula: always applicable when the cognitive basis of the process of scientific discovery is being investigated: 'The attempt to characterize the act of the generating of hypothesis and subjective recognizing no longer only than arbitrarily and not further analyzable but comprehend it in form of the abductive conclusion can perhaps show the way in a direction, which is in the humanities well-known as hermeneutic procedure of creating knowledge' (Kreppner 1975: 69). In my opinion, however, this hope is the result of a widespread misunderstanding of Peirce's position with regard to the differences between 'hypothesis' and 'abduction' as forms of inference. From the modern point of view it is beyond question that, up to about 1898, Peirce combined two very different forms of inference under the name of 'hypothesis.' When he became aware of this unclear use of the term 'hypothesis,' he elaborated a clear distinction in his later philosophy between the two procedures, and called the one operation 'qualitative induction' and the other 'abduction' (for more detail see Reichertz, 2003, also Eco, 1981). Many social scientists, with reference to the *achievements* of abduction, rely on Peirce's later work (in my view wrongly), but with reference to its *form* and *validity*, on his work on hypothesis. It is only on the basis of this 'hybrid meaning' that they succeed in designing a logical operation which produces new knowledge in a rule-governed way.

DEDUCTION, QUANTITATIVE AND QUALITATIVE INDUCTION, ABDUCTION

The social order around which humans (often but not always) orient themselves in their actions is constantly changing and is, moreover, 'sub-culturally fragmented.' The different order(s) therefore possess only a localized validity and are continually and, since the advent of the 'modern,' with increasing rapidity being changed by individuals who previously (up to a point) adhered to them (Eisenstadt, 2003; Foucault, 2004). Moreover, both the *form* and the *validity* of this order are bound to the meaning attributions and interpretations of the acting subjects. Social science explanations of actions aim at the (re)construction of the order that is relevant to the acting subjects. Admittedly this kind of order can no longer be derived from proven grand theories, first because these are, as a rule, not sufficiently 'local,' and second because they have frequently already been overtaken by constant social change. Thus, appropriate new views of the structure of social order must constantly be generated. For this reason it is highly sensible to examine as closely as possible the life practice that is to be understood, and (on the

basis of these data) to (re-)construct the *new* orders. It is obvious that the examination must start from older views and so have some link to them.

If we are now to make a serious attempt, in (qualitative and quantitative) research, to evaluate collected data, in other words to typologize them according to particular features and orders of features, the question very soon arises of how we may bring a little order to the chaos of the data. This is only to a very small extent a matter of work organization (sorting of data) and much more a question of how the unmanageable variety of the data may be related to theories: either pre-existing or still to be discovered. In this undertaking (if one pursues the ideas of Peirce) we may, in ideal terms, distinguish *three* procedures and, in what follows, I shall subdivide the second procedure into two sub-groups; not because there are fundamental differences between the two, but rather because in this way the difference we have already spoken of between *abduction* and *hypothesis* or *qualitative induction* can be made clearer (for a fuller discussion of this, see Reichertz, 2003).

Subsumption

One type of data analysis consists of the procedure of *subsumption*. Subsumption proceeds from an already known context of features, that is from a familiar *rule* (e.g. all burglars who steal from a medicine chest are drug addicts), and seeks to find this general context in the data (e.g. the unknown burglar has robbed the medicine chest) in order to obtain knowledge about the individual case (e.g. the unknown burglar is a drug addict). The logical form of this intellectual operation is that of *deduction*: the single case in question is subordinated to an already known rule. Here a tried and trusted order is applied to the new case. New facts (concerning the ordering of the world) are not experienced in this way; we have deduced that the unknown burglar is a drug addict (knowledge that may be quite useful to the police, if the rule is true). Deductions are therefore *tautological*, they tell us nothing new. But deductions are not only tautological but also *truth-conveying*: if the rule offered for application is valid, then the result of the application of the rule is also valid.

Generalizing

A second form of analysis consists of extending, or *generalizing*, into an order or rule the combinations of features that are found in the data material. Proceeding from the observation that 'in the case of burglaries a, b, and c the medicine chest was robbed;' and the case-knowledge that 'Mr. Jones committed burglaries a, b, and c,' the inference is drawn that 'Mr. Jones always robs the medicine chest when he breaks in.' The logical form of this intellectual operation is that of *quantitative induction*. It transfers the quantitative properties of a *sample* to a totality, it 'extends' the single case into a rule. *Quantitative inductions* therefore are equally tautological but not truth-conveying. The results of this form of inferencing are merely *probable*.

One particular variant of the inductive processing of data consists of assembling certain qualitative features of the investigated sample in such a way that this combination of features resembles another (that is already available in the repertoire of knowledge of the interacting community) in essential points. In this case one can use the term that already exists for this combination to characterize one's 'own' form. The logical form of this operation is that of *qualitative induction*. From the existence of certain qualitative features in a *sample* it implies the presence of other features (e.g. At the scene of a crime I see a particular set of clues. In very many respects these agree with the pattern of clues associated with Mr. Jones. Conclusion: Jones is responsible for the clues). The observed case (*token*) is an instance of a known order (*type*). To summarize: if *quantitative induction* makes inferences about a totality from the quantitative properties of a *sample*, qualitative induction (in contrast) supplements the observed features of a sample with others that are not perceived. It is only in this sense that this form of induction transcends the borders of experience, that is, only the experience of the sample in question. This inference only extends knowledge to the extent that it proceeds from a limited selection to a larger totality. *Qualitative induction* is not a valid but only a probable form of inference, although it does have the advantage of being capable of operationalization (albeit with difficulty). Qualitative induction is the basis of all scientific procedures that find, in collected data, only new versions of what is already known.

Abduction

The third type of data processing (apparently similar, but in fact totally different) consists of assembling or discovering, on the basis of an interpretation of collected data, such combinations of features for which there is no appropriate explanation or rule in the store of knowledge that already exists. This causes surprise. Real surprise causes a genuine shock (and not only in Peirce's opinion) and the search for the (new) explanation. Since no suitable 'type' can be found, a new one must be invented or discovered by means of a mental process. One may achieve a discovery of this sort as a result of an intellectual process and, if this happens, it takes place 'like lightning,' and the thought process 'is very little hampered by logical rules' (Peirce, 1931–1935: Vol. V: 117). An order, or a rule, in this procedure must therefore first be discovered or invented, and this has to happen with the aid of intellectual effort. Something unintelligible is discovered in the data and, on the basis of the mental design of a *new* rule, the rule is discovered or invented and, simultaneously, it becomes clear what the case is. The logical form of this operation is that of *abduction*. Here one has decided (with whatever degree of awareness and for whatever reason) no longer to adhere to the conventional view of things. This way of creating a new 'type' (the relationship of a typical new combination of features) is a creative outcome which engenders a new idea. This kind of association is not obligatory, and is indeed rather risky. *Abduction* 'proceeds,' therefore, from a known quantity

(= result) to *two* unknowns (= rule and case). Abduction is therefore a cerebral process, an intellectual act, a mental leap, that brings together things which one had never associated with one another: A cognitive logic of discovery.

TWO STRATEGIES FOR PRODUCING ABDUCTIONS

If one is to take seriously what has been outlined above, one would have to come to the conclusion (pessimistic though it might be for everyday scientific practice) that abductive discovery of new things is dependent either on pure chance, a benevolent God, a favorable evolution, or a particularly well-endowed brain. Science as a *systematic* endeavor would, according to this definition, seem doomed to failure. However, even if one cannot *force* lightning to strike in an algorithmically rule-governed way, could there perhaps be ways of proceeding and precautions that would make it easier for the (intellectual) lightning to strike? Even lightning is not entirely unexpected. To extend the metaphor, it happens only as a consequence of a particular meteorological situation. In a storm one can look for the oak tree or seek out the beeches or even go to the top of the church tower. None of these steps will make it certain that lightning will come and strike; but the likelihood is nonetheless very much greater than with someone who only loves the sunlight, who always takes refuge in a cellar during a storm, and who (if he does happen to find himself in a storm) always tries to find out where the nearest lightning conductor is. In short, if discovery is truly related to accidents, then one can either give accidents a chance or deny or reduce the possibility.

Peirce himself cites two *macro-strategies* that are particularly well-suited to 'enticing' abductive processes or at least to creating a favorable climate for their appearance. One can be derived from the story where Peirce talks retrospectively about his talents as an amateur detective (Peirce, 1929). In this Peirce tells how, during a voyage at sea, his overcoat and a valuable watch were stolen. He was very alarmed, because the watch was not his own property. He therefore decided to recover the watch, by any means and as quickly as possible. He had all the crew called together and asked them to form up in a line. Then he walked along the line and addressed a few apparently inconsequential words to each of them.

When I had gone through the row, I turned and walked from them, though not away, and said to myself: 'Not the least scintilla of light have I got to go upon'. But thereupon my other self (for our own communings are always in dialogues) said to me, 'but you simply *must* put your finger on the man. No matter if you have no reason, you must say whom you think to be the thief'. I made a little loop in my walk, which had not taken a minute, and I turned toward them, all shadow of doubt had vanished (Peirce, 1929: 271).

Peirce named one person as the culprit and subsequently, after a great deal of confusion (see Sebeok & Umiker-Sebeok, 1985 for a full description), it emerged that the man suspected by Peirce was indeed the thief. The stimulus for this individual initiative in matters of 'detection' was therefore provided by *fear*: not the fear of

losing the value of the watch, but the fear of a 'life-long professional disgrace' (Peirce, 1929: 270). When, after his first conversations with the crew, he could not name a suspect, he increased, by an act of will, his pressure to do something. In this partially self-induced emergency situation, the abductive lightning struck.

Of course, abductions cannot be forced by a specific procedural program, but one can induce situations (and this is the moral of this episode) in which abductions fit. According to Peirce, the presence of *genuine doubt* or *uncertainty* or *fear* or *great pressure to act* is a favorable 'weather situation' for abductive lightning to strike. Peirce, however, develops another possible way of creating situations in which new knowledge may more frequently be obtained. For this to work the investigator, as Peirce advises, should let his mind wander with no specific goal. This mental game without rules he calls 'musement,' a game of meditation, or daydreaming. How one achieves the condition of daydreaming may be seen in the following formulation:

Enter your skiff of musement, push off into the lake of thought, and leave the breath of heaven to swell your sail. With your eyes open, awake to what is about or within you, and open conversation with yourself: for such is all meditation! (...) It is, however, not a conversation in words alone, but is illustrated, like a lecture, with diagrams and with experiments (Peirce, 1931–1935: Vol. 6, 315).

To do this requires leisure, that is to say, freedom from an immediate pressure to act is a fundamental condition, without which the skiff will not be able to embark. This apparently contradicts quite vehemently the preconditions for successful abductions which Peirce sets out in his detection example. Admittedly, the contradiction is resolved if one looks for what is typical in the two 'abduction-friendly' settings. In both cases the procedures mean that the *consciously working mind*, relying on logical rules, is outmaneuvered. Peirce-the-detective allows no time for the calculating mind to busy itself with the solution of his problem, and Peirce-the-daydreamer switches off his power of logical judgment by entrusting himself to the 'breath of heaven.'

All measures designed to create favorable conditions for abductions, therefore, always aim at one thing: the achievement of an *attitude* of preparedness to abandon old convictions and to seek new ones. Abductive inferencing is not, therefore, a *mode of reasoning* that delivers new knowledge, and neither is it an *exact* method that assists in the generation of *logically ordered* (and therefore operationalizable) hypotheses or some new theory. Abductive inferencing is, rather, an attitude towards data and towards one's own knowledge: data are to be taken seriously, and the validity of previously developed knowledge is to be queried. It is a state of preparedness for being taken unprepared.

RESEARCH RESULTS: RECONSTRUCTION OR CONSTRUCTION?

Abductive efforts seek some (new) order, but they do not aim at the construction of *any* order, but at the discovery of an order which *fits* the surprising facts;

or, more precisely, which solves the practical problems that arise from these. The justification for this selective attention (which targets a new order) is not the greatest possible closeness to reality or the highest possible rationality. The justification is, above all, the *usefulness* which the 'type' that is developed brings to the question of interest. It can bring order and the means of linguistic representation, however these new 'types' are indispensable tools if one is to be able to make predictions about the future on the basis of a past that is hypothetically understood because it is ordered: they are indispensable when it is a matter of producing answers to the question of 'what to do next?' New orders, therefore, are also always oriented towards future action.

An abductive discovered order, therefore, is not a (pure) reflection of reality, nor does it reduce reality to its most important components. Instead, the orders obtained are *mental constructs* with which one can live comfortably or less comfortably. Abduction is something we all do, when there is a crisis or when we do not know what to do next. For many purposes, particular constructs are of use, and for other purposes, different constructs are helpful. For this reason, the search for order is never definitively complete and is always undertaken provisionally. So long as the new order is helpful in the completion of a task it is allowed to remain in force: if its value is limited, distinctions must be made; if it shows itself to be useless, it is abandoned. In this sense, abductively discovered orders are neither (preferred) constructions nor (valid) reconstructions, but *usable* (re-)constructions.

When faced with surprising facts, abduction leads us to look for meaning-creating rules, for a possibly valid or fitting explanation that eliminates what is surprising about the facts. The end-point of this search is a (verbal) hypothesis. Once this is found, a multi-stage process of checking begins. If the first step in the process of scientific discovery consists of the finding of a hypothesis by means of abduction, then the second step consists of the *derivation of predictions* from the hypothesis, which is deduction, and the third step consists of the *search for facts* that will 'verify' the assumptions, which is induction. If the facts cannot be found the process begins again, and this is repeated as often as necessary until 'fitting' facts are reached. With this definition Peirce designed a three-stage discovery procedure consisting of abduction, deduction, and induction.

Finding and checking are, in Peirce's opinion, *two* distinct parts of a *single* process of discovery or research. If the finding stage is largely a result of a conscious and systematic approach, checking takes place according to operationalizable and rule-governed standards that are controlled by reason. *Certainty* about the validity of abductive inferences, however, cannot be achieved even if one subjects an abductively developed hypothesis to extensive testing; that is to say, deduces it from its consequences, then seeks to determine these inductively, and then repeats these three steps many times. Verification in the strict sense of the word cannot be done in this way. All that one can achieve, using this procedure, is an intersubjectively constructed and shared 'truth.' In Peirce's opinion even this is only reached if *all* members of a society have come to the same *conviction*.

Since, in Peirce's work, 'all' includes even those who were born after us, the process of checking can in principle never be completed. For Peirce, absolute certainty can never be achieved so: 'infallibility in scientific matters seems to me irresistibly comic' (Peirce, 1931–1935: Vol. I, X).

THE STRAUSS CONCEPT OF GT AND PEIRCE'S LOGIC OF RESEARCH

In early GT there were two strands, an inductive one, which worked on the assumption that categories and even theories emerge out of the data if only one looks closely enough, and a theoretical strand, which banked on the fact that prior knowledge about the world and scientific theories (apart from the data) are useful (cf. Kelle, 2005). In the later variant of GT by Strauss and Strauss and Corbin (in my further considerations I will only focus on this variant of GT), one can find both strands, the inductive and the 'theoretical' one, but with reversed emphasis. In the inductive variant by Glaser and Strauss, knowledge concepts or theories were officially and explicitly founded on induction (while the theoretical strand worked in the background), and in the later variant by Strauss and Corbin, theory was officially and explicitly founded on theoretical knowledge (while the inductive strand worked in the background; Strübing, 2004: 50ff). Strauss and Corbin have strongly emphasized this turn of their concept: 'Also, researchers are still claiming to use "grounded theory methods" because their studies are "inductive"' (Strauss & Corbin, 1994: 276). Thoughtful reaction against restrictive prior theories and theoretical models can be salutary, but too rigid a conception of induction can lead to sterile or boring studies. Alas, grounded theory has been used as a justification for such studies. This has occurred as a result of the initial presentation of grounded theory in *The Discovery of Grounded Theory* that had led to a persistent and unfortunate misunderstanding about what was being advocated. Because of the partly rhetorical purpose of that book and the authors' emphasis on the need for grounded theory, Glaser and Strauss overplayed the inductive aspects (Strauss & Corbin, 1994: 277). Later GT claims to be more than a method for the coding of data. Even if coding is an essential part of research for Strauss, GT doesn't exhaust itself in coding (see Strübing 2006). Otherwise GT would run the risk of merely doubling the data on a more abstract level. It is therefore very unfortunate if the later GT is described as a coding paradigm. Strauss and Strauss and Corbin repeatedly pointed out that GT is 'a general methodology, a way of thinking about and conceptualizing data' (Strauss & Corbin, 1994: 275). Thus GT helps scientists to fulfill two tasks: the intellectual task of coding (open, axial, selective), and the intellectual task of developing and redeveloping concepts and theories while repeatedly moving to and fro between the collection of data, coding, and memoing (logic of research).

Of decisive importance for the question whether later GT makes use of abductive thinking² is therefore (a) whether it provides opportunities for the

emergence of abductive conclusions at the level of single thinking acts, i.e. during concrete coding acts, or (b) whether the logic of research as a whole is abductive or not. Thus the central issue is neither whether GT works abductively in all cases and in all fields (this would be nonsense), nor whether one is allowed to revert to knowledge apart from the data (this primarily pertains to qualitative induction), but whether GT systematically counts on the appearance of new codes or hypotheses. 'Abductive' here does not simply mean that the research data is taken seriously and that the findings have to fit the data (this must be accomplished by all serious research) but essentially that the research is laid out in such a way that new hypotheses can and do appear at every level, that the interpretation of the data is not finalized at an early stage but that new codes, categories, and theories can be developed and redeveloped if necessary. If one takes a closer look at the work of Strauss and Strauss and Corbin to see whether there are methodical routines and practices within GT which favor the appearance of new hypotheses, much evidence can be found.

Example (a): One passage is very clear as regards 'induction' as a basis of coding. It here becomes apparent that Strauss doesn't mean the logical conclusion 'induction' at all but rather all the actions and attitudes which lead to a hypothesis, and exactly this is also addressed by Peirce with his considerations: 'Induction refers to the actions that lead to discovery of a hypothesis—that is, having a hunch or an idea, then converting it into an hypothesis and assessing whether it might provisionally work as at least a partial condition for a type of event, act, relationship, strategy, etc.' (Strauss, 1987: 11f).

Example (b): In Strauss's work, one can find repeated references at the level of the research logic to a permanent testing of verdicts once taken. Data elevation, coding, and the making of memos are related to each other in a three-step process: Hypotheses lists deduction of consequences and the testing of these consequences by means of the data and data analysis. This exactly corresponds to the logic of 'abductive' research: '(...) data collection leads quickly to coding, which in turn may lead equally quickly, or at least soon, to memoing. Either will then guide the searches for new data. Or they may lead directly to additional coding or memoing. Or—please note!—they may lead to inspecting and coding of already gathered (and perhaps already analyzed) data. That latter kind of "return to the old data" can occur at any phase of the research, right down to writing the last page of the final report of the theory' (Strauss, 1984: Unit 1, 18).

Furthermore, recurring references to the necessity of not only relying on existing knowledge but of creating new codes, categories, and theories can also be found in the work of Strauss and Strauss and Corbin: 'Creativity is also a vital component of the grounded theory method. Its procedures force the researcher to break through assumptions and to create new order out of the old. Creativity manifests itself in the ability of the researcher to aptly name categories; and also to let the mind wander and make the free associations that are necessary for generating stimulating questions and for coming up with a comparison that leads to discovery' (Strauss & Corbin, 1990: 27). The wording itself already reveals

how close the ideas of Strauss and Corbin and Peirce are. In other text passages which refer to the flash-like discovery of the new, the common ground (including the abstract agreement) becomes still clearer: 'Yet, the most gratifying moments of research for analytically inclined researchers will be those that bear on their discoveries. They may be matters of quick flashes of "intuition," or major breakthroughs in understanding the meaning and patterns of events, or the deeper satisfaction of having solved the research's major puzzles' (Strauss & Corbin, 1990: 28).

In later GT one can find (in addition to the coding and the development of theories of a middle or long range) two intellectual operations: the finding of similarity (coding in known codes) and the development of the new (creating new codes). This kind of scientific work has its parallel in the distinction between qualitative induction and abduction as made by Peirce. The operation, the intellectual jump which 'states' things in common between acquaintance and data and codes in already known concepts is the first step: the qualitative induction (as executed above). This thinking act adds something to the data too. The second step is the intellectual jump which adds something very new to the data, something that they do not contain and that does not already exist as a concept or theory either. This is abduction.

The question whether GT (in the variant of Strauss and Corbin) contains an abductive research logic can therefore be answered with a resounding 'yes.' Fortunately, however, it does not only contain the logic of abductive reasoning but also that of qualitative induction. The logic of later GT thus permits abductive reasoning, counts on it, enables it, grants it place. More is not necessary.

One can, in conclusion, wonder why Strauss did not further develop the ideas of Peirce on abduction. Strauss undoubtedly knew the idea of abduction, because he mentioned it in his work at least once (cf. Strübing, 2004: 51). In connection with the question where our knowledge for induction, deduction, and verification comes from, Strauss stated (and this is indeed an important but not the crucial determinant for the new variant of GT) that this knowledge is nurtured by experience, but can also derive from theoretical pre-knowledge. 'They come from experience with this kind of phenomenon before—whether the experience is personal, or derives more "professionally" from actual exploratory research into the phenomenon of from the previous research program, or from theoretical sensitivity because of the researcher's knowledge of technical literature' (Strauss, 1987: 12).

Strauss adds in a footnote here: 'See the writings of Charles Peirce, the American Pragmatist, whose concept of abduction strongly emphasized the crucial role of experience in the first phase of research operations' (Strauss, 1987: 12). For Peirce, the fact that intellectual operations are nurtured by knowledge of every kind in the phase of discovery is not decisive for defining abduction, however, it is of crucial importance for him that new knowledge can be generated by means of this operation. In 'Study Letters of the FernUniversität' in *Qualitative Analysis in Social Research: Grounded Theory Methodology* (Strauss, 1984),

the notion 'abduction' does not yet appear. In later documents, Strauss does without the explicit idea of abduction. Why did Strauss not use this term before? Did he not know it? As a pragmatist he would have had good reasons (and many opportunities) to see the parallels between his way of coding (particularly the open and selective coding) as well as his type of generating theory by 'coherent perception' and the abductive reasoning of Peirce.

The following deduction might illuminate the sudden but brief appearance of the word 'abduction' in the work of Strauss (which, if correct, is explained by this surprising fact): Anselm Strauss became acquainted with pragmatism and its research logic via Blumer via Dewey.³ Strauss also mentions the influence of Peirce in some passages, but his notes are always very general. There is no evidence that Strauss has systematically studied the writings of Peirce, but Strauss's lack of citation to Peirce did not mean a lack of knowledge: he primarily knew Peirce as an action theorist, a semiotist, and a logician, so that for him as an empirist and 'working sociologist' in the tradition of the Chicago School there was no real need to look for notes in the work of Peirce as far as the logic of discovery is concerned. Strauss knew the concept of abduction from at least 1968 and when he got to know it he saw the parallels to his form of coding and generating theories,³ but he did not further expound on these parallels. If he saw the chance to build a methodological basis (as a general theory) for his GT with the abductive research logic, he did not use it,⁴ possibly because (due to restricted time resources) he focused on convincing other researchers of the fertility of its methodology and methods, rather than on coordinating its procedure with the conceptualities of the dominant methodology and on fastening it against criticism.

NOTES

1 Heiner Legewie and Barbara Schervier-Legewie led the 1994 Interview with Anselm Strauss. Ten years later it was published in *Forum Qualitative Sozialforschung*.

2 *The Discovery of Grounded Theory* contains many considerations that point to an abductive reasoning: 'The root sources of all significant theorizing is the sensitive insights of the observer himself. As everyone knows, these can come in the morning or at night, suddenly or with slow dawning, while at work or at play (even when asleep); furthermore, they can be derived directly from the theory (one's own or someone else's) or occur without a theory; and they can strike the observer while he is watching himself react as well as when he is observing others in action. Also, his insights may appear just as fruitfully near the end of a long inquiry as near the outset' (Glaser & Strauss, 1967: 251). Interestingly, the authors did not mention Peirce in this passage but explicitly referred to the secondary literature 'Nature of Insight' and 'Creative Work' in a footnote.

3 'Contributing to its development were two streams of work and thought: first, the general thrust of American Pragmatism (especially the writings of John Dewey, but also those of George Mead and Charles Peirce) and including its emphases on action and the problematic situation, and the necessity for conceiving a method in the context of problem solving' (Strauss, 1987: 5). For the history of grounded theory see: Kendall, 1999: 743f. In Strauss & Corbin, 1990, Peirce is no longer mentioned when the influence of pragmatism is discussed (see Strauss & Corbin, 1990: 24).

4 An insignificant, but nonetheless interesting, question is why Strauss used the term 'abduction' so many years later. My hypothesis is that the term 'abduction' (for a short time) found its way into

grounded theory methodology via Germany. The following reasons support this hypothesis: a study visit of Anselm Strauss to Germany fell in the time between Study Letters (written in 1982/1983 and published in 1984) and the publication of *Qualitative Analysis for Social Scientists* (1987). During this study visit he exchanged thoughts (by invitation of Hans-Georg Soeffner among others) with the research teams of Richard Grathoff (University of Bielefeld), Fritz Schütze (University of Kassel), and Hans-Georg Soeffner (FernUniversität Hagen). At that time, all of the above-mentioned researchers (Grathoff, Schütze, and Soeffner) dealt with the features of abductive reasoning. Hans-Georg Soeffner still remembers having repeatedly mentioned Peirce's notion of abduction to Strauss in connection with the possibilities of 'coherent perception' when coding. First, discussions about the parallels of abduction and Strauss's theory of research already took place in 1981/1982 when Hans-Georg Soeffner was in San Francisco (Hans-Georg Soeffner, personal communication in 12/2006; Strauss, 1987: XV).

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