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The "Other" Speaks Up. When Social Science (Re)presentations Provoke Reactance from the Field

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Abstract: *»The other talks back. Auslösung von Feldreaktanzen durch sozialwissenschaftliche Re-/Präsentationen«*. This paper addresses science communication problems: How do researchers convey social science representations and findings to the researched? How are the latter described in research reports? How do they react when they read or hear such reports and when they subsequently engage in discourse with researchers? Typically, social science researchers approach a field site with an attitude of curiosity that is unburdened by an immediate pressure to act. The field inhabitants, by contrast, are subject to the practical constraints of these everyday worlds; they identify personally with their milieu and its protagonists, and they are correspondingly sensitive. The present paper describes their defensive reactions, taking as an example the reception of a research project presented at conferences attended by a mixed audience. It highlights the reactions and strategies displayed by the researched in the contexts of discourse and meaning negotiation in response to unwelcome representations. And it offers several interpretations of the interactions between the researchers and the researched. Field members may oppose the revelation of contextual and causal factors construing it as "washing dirty laundry in public". Researchers react to this in their textual representations, and their reactions may take the form of score-settling. The present paper asks how such contradictory, conflict-laden constellations and perspectives in the discourse between the observers and the observed can be productively dealt with.

Keywords: relationship between research and practice; confrontation with participants' perspectives; chair succession; reactions of field members; studying up-studying down; enterprise succession; ethnography of science; science communication.

1. Social Science Research in Reflexive Discourse

Put simply, empirical social science research can be described as a structure comprising objects (a research field inhabited by persons of interest); a subject

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(a researcher who is interested in these objects); and an audience that reads or listens to research reports. The researcher plays, first, the role of (participant) observer and, second, that of the author of a scientific text, be it a lecture, a journal article, or a monograph. With regard to the communicative processes that take place in this context, one can say – cum grano salis – that the main communication partners of the researcher in his or her role as observer are the inhabitants of the field – the "informants" or "field partners". The researcher as author, on the other hand, communicates mainly with and to colleagues, in other words fellow members of the scientific community.

As a rule, researchers produce the (re)presentation of the research object, (contextualised within their categories of perception and assessment) in the absence of "the other". Hence they have a virtual monopoly on scientific description. The appropriateness and acceptability of these texts is not the subject of negotiation with the persons who are the object of description and analysis. Historically, serious doubts as to the "ethnographic authority" of the ethnologist's monopoly on authorship and his or her authoritative-monologic representation of the research object did not arise until the "crisis of representation". This debate was conducted in the 1980s and 1990s in the field of ethnology, which can be considered the leading discipline in this context. Some authors subsequently endeavoured to place ethnographic representation on a dialogical, multiperspectival, or polyphonic footing and to involve the research objects to a greater extent in the realisation of ethnographic accounts; in other words to allow them to participate in the production of textual representations (cf. Clifford & Marcus, 1986; Geertz, 1990; Berg & Fuchs, 1993; see also Breuer, 1999, pp. 236ff.).

The (self-) reflexive examination of the effects on the field caused, or encouraged, by ethnological research and (re)presentation was provoked not least by criticism voiced by the research objects themselves. In the history of their discipline, ethnologists have played a major – and, from a contemporary perspective, ignominious – role in the military and economic subjugation, exploitation and destruction of indigenous social cultures in the colonial context. In recent times, the "savages" or "natives", who are the objects of ethnological research interest, have increasingly changed their status from "informant" to "critic", among other things. They have started to read for themselves the ethnological or ethnographic texts that have been written about them; to comment on them; to question them; and to react to them socially and politically (cf. for example, Brettell, 1996; Sluka, 2007).[1]

[1] Human research objects (popularly known as guinea pigs) are no longer willing to put up with everything either. They, now have a voice: see, for example, <http://www.guineapigzero.com/>; <http://minority-health.pitt.edu/archive/00000252/01/A_Public_Culture_for_Guinea_Pigs_US_Human_Research_Subjects_after_the_Tuskegee_Study.pdf> [accessed 20.2. 2011].

A status imbalance in favour of the researcher in the relationship between the researcher and the research object – what Nader (1972) refers to as a "studying-down" constellation – is characteristic of "classical" ethnological research. As a result of changes in the field, and of revised conceptualisations of the relationship between the knowing subject and the object to be known (the "knowledge relationship"), the relationship between the subject (researcher) and the object (the researched) is increasingly perceived as egalitarian. Moreover, constellations in which the researched are superior to the researchers in terms of the economic, social, and/or cultural capital at their disposal are becoming more common. This is referred to as a "studying-up" (Nader, 1972) or a "researching-up" situation (cf. Warneken & Wittel, 1997 on researching up in an ethnological context). In organisational science research, for example, interviews are frequently conducted with senior managers; researchers sometimes find themselves in the role of supplicant; and interview situations can resemble a papal audience. In such cases, the researched have a greater chance of organising the contact and the interaction with the researcher according to their own preferences – unlike the objects of classical ethnology, whose options with regard to shaping interaction with researchers were – and still are – generally limited to indirect and passive strategies. The inhabitants of a field site sometimes have a wide range of strategic and tactical possibilities to lead researchers by the nose and to use them to promote their own cause (cf., for example, Selvini Palazzoli et al., 1984; Whyte, 1984). In studying-up constellations, researchers are faced with specific problems when it comes to asserting themselves in the field and publishing their reports. In their analyses of studying up in the context of enterprise research, Warneken and Wittel (1997, pp. 10ff.) identify submission, mystification, distancing, and the desire to get even as some of the effects of this constellation on the researcher. These effects can influence the research findings and the scientific publications.

The presentation of scientific interpretations and research findings in discursive contact with members of the researched group can be considered on a number of different levels (see, for example, Terhart, 1995). From an epistemological viewpoint, it can be asked whether the communication between researchers and the researched about the findings achieved (object reconstructions) can contribute in some way to the evaluation of the quality of social science knowledge. In some methodological approaches, contact with and the approval of the field partners are sought in order to bring about an improvement in the validity of research findings (member check, communicative validation, dialogic consensus etc.). From the perspective of an enlightenment-oriented relationship between research and practice, an effort is made to bring the "critical knowledge potential" of research to fruition praxeologically in a process of collaboration and communication with research partners in their field of action. The aim here is to improve the world from the bottom up, as it were (action research, participative research etc., cf. Bergold & Thomas, 2010).

On the level of psychological processing, these conceptualisations are based on the individual reactions of field members to the scientific descriptions of their milieu – to a perspective, a terminology and a presentation format that are foreign to them. Because these individuals identify with the research object in question (for example with the social context qua membership), with its problems, and with the perspectives and ways of doing things that prevail there, they are personally affected by the presentations of the "observer". The latter "does something" to them. They may feel respected and valued; they may consider themselves fairly or unfairly characterised by the alienating descriptions of familiar protagonists, prototypes and processes. In the ensuing dialogical contact, they have an opportunity to articulate their reactions to the researchers who have produced and presented these results (characterisations, data, interpretations, texts). In other words: The "others" speak up. They may be enthusiastic, they may applaud; they may be insulted or annoyed; they may raise objections, express criticism; they may seek, avoid, or terminate contact with the researcher; they may exert influence on the publication of scientific reports. Such effects, and the reactions they provoke, have to do with these persons' close ties and identification with the field; with their own perspectives, interests and partialities; with the problems addressed by the research project; and with the perceived intentions of the researchers etc.

The present paper deals with such communicative processes between researchers, as producers of social science knowledge, and the researched, as recipients of this knowledge, in research situations where the researcher and the researched were on an equal footing or where the researched had a higher status (studying up). The report is based on my experiences as a social science /psychology researcher when communicating my research concept and findings to "affected" field members. The project in question was devoted to the development of a theory of the transfer of the ownership of personally important objects from predecessor to successor.

It will be shown that under certain circumstances the researched in social science projects do indeed have a voice and the authority and agency to defend themselves aggressively against what they consider to be impertinence on the part of scientists who are targeting them. In the course of such heated exchanges, the parties negotiate what really is (or is not) the case; what procedures of scientific description are appropriate; what data are legitimate and permissible; and whether, or in what form, the results may be published.

2. Generation of a Theory of the "Transfer of Personal Objects"

In 2009, I published a book that I had been working on for about ten years. It was devoted to transitions between predecessors and successors in various personal, organisational and institutional contexts (Breuer, 2009). The basic

methodological approach taken was the Grounded Theory concept. Under this method, the researcher approaches an everyday empirical field with a theoretically open mind and develops a generalising theory by induction or abduction through the hermeneutic analysis of representations of individual cases from that field. By comparing models of different fields, it may be possible to raise the level of abstraction of the theory a step higher and by so doing to extend its scope, thereby generating a so-called formal grounded theory. This was the objective of my research approach (cf. Strauss, 1987, pp. 241ff.; Breuer, 2010, p. 108; 2011a).

Initially, I focused on cases where (small) family businesses were passed on to the next generation. This research interest grew out of a collaboration with the economic historian Clemens Wischermann (cf. Kollmer-von Oheimb-Loup & Wischermann, 2008). My main theoretical orientation was the social constructivist theory proposed by Berger and Luckmann (1966). The first case studies related to the way in which the handover of farms in the Münsterland region in north-west Germany was managed (cf. Breuer, 2000). Next, I turned my attention to small enterprises in the hotel and gastronomy sector (hotels, restaurants, pubs) and various types of skilled trades enterprises (cf. Breuer, 2009, pp. 273ff.). I then extended the scope of my research beyond family enterprises, and focused on management- and chair-succession processes in public-sector institutions such as schools, cultural institutes, and universities. Following this, I concentrated on more private, interpersonally intimate constellations such as the passing on of parental roles (father- or mother-role succession, for example in the context of adoptions) and marriage/partnership successions (separations and new partnerships, divorces and remarriage). And finally, I turned my attention to the transfer of the "ownership" of bodily organs (heart transplants, for example). The comparison of such diverse domains yielded a generalised theory of the transfer of personal objects – a name I gave it in the course of the elaboration phase.

The core concept that emerged is called object transfer. It refers to the transfer of the power of disposal over "things" that are fundamental to the identity and the identification of the owner ("personal objects"). The comparison of such a wide variety of empirical fields yielded a trans-disciplinary social science category: the transfer of personal objects. This category can be used to conceptualise the dynamics of interpersonal, social, organisational, and institutional structures, especially with regard to the links and the interplay between material and symbolic components, between the individual and the social, and between the past and the future. Here is a very brief overview of the basic concepts and categorical dimensions of the theory (for more detail, see Breuer, 2009, 2011a):

The basic vocabulary of the theory of predecessor-successor transitions comprises the following components:

- the protagonists of the transfer/succession: predecessor and successor in the possession of power of disposal over the object in question;
- the object of the transfer/succession: a configured personal structure and the underlying relationships;
- the context and its actors: historical, political, and institutional circumstances of the transfer, and the members of these fields;
- behavioural patterns and regulations: social schemas for the transfer; applicable laws, rights and obligations of the predecessor and the successor, traditions, formal and informal rules;
- attitudes and identifications of the protagonists and the other actors: attitudes, motivations – especially in relation to the relevant objects and to characteristics of the transfer/succession process;
- the interests of the protagonists and the other actors by virtue of their respective positions or roles in the transfer/succession context;
- strategies of the protagonists and the other actors for the realisation of the goals related to these interests within the framework of the transfer scenario.

The theoretical conceptualisation and analysis levels of predecessor-successor transitions are:

- the schematic nature (i.e. the patternedness) of the object transfer: there are certain cultural, social and psychological schemas and scripts for the transfer process that guide the subjective orientation and the actions of the protagonists, the way they interpret their own actions and those of others, and their understanding of the processes;
- the openness to interpretation and perspective-dependent nature of the transition processes: depending on the perspective, the relevant observable actions and events can be understood and interpreted differently; there can be conflicts and differences of opinion about the way things should be understood; people's understanding can undergo changes;
- the personal capacity to influence the transfer process and the structurally-determined or -constrained nature of that process: on the one hand, the actors find themselves bound by transfer guidelines and traditions; on the other hand, they have individual, voluntaristic freedom to (dis)regard or modify them;
- the transcendental nature of the object transfer: each transfer of an object refers to something beyond the tenure or lifetime of the predecessor; hence it is the subject of anticipation and negotiations and continues to influence events even after the transfer has been executed;
- the temporal structure of transitions from predecessor to successor: object transfers display characteristic temporal patterns; for example they can be executed in a timely or untimely manner; succession can take place with or without an interim period of vacancy, or with or without a period of overlap (double-fill situation);

- the negotiable nature of the process between the participants: transfers of the power of disposal over an object are shaped by the interpersonal relationships of the protagonists and are explicitly or implicitly, directly or indirectly, negotiated between them (and the other actors involved).

3. Predecessor-Successor Transitions: Perspectival Presentations in Discourse

Interpersonal succession processes in the form of accounts written from a particular perspective are typically encountered in narratives and stories. Accounts of transfers of, or succession to, personal objects that have been idiosyncratically formed and are fundamental to an individual's identity, for example a lifework or masterpiece, tend to trigger subjectively engaged responses and reactions on the part of the audience. This is especially the case when the recipients are personally involved in or familiar with the case; when they belong to the social milieu in question and therefore identify with the system of values and rules prevailing there; and/or when they adopt a personal stance, hold a certain position, or pursue personal interests in that milieu. Succession stories frequently trigger positively or negatively tinted emotions on the part of readers or listeners who are in some way connected to the milieu and who identify with the object in question. It can easily happen that the account touches a nerve with field members.

Succession stories can be presented in various formats. For example, one can typologically differentiate "heroic tales" and "tragic or comic stories". While the former can be used to orchestrate family-legend or -myth formation, the latter are the stuff that journalistic reports, court disputes, and gossip are made of. Researchers with a social scientific interest can actually learn quite a bit from these accounts. Especially in cases where established schemas become problematic – when complications arise or the succession fails –, structures can be uncovered and described that remain hidden, or in the background, when the succession process goes smoothly. Because they are taken for granted as long as they work, insiders are often unable to make these structures explicit. To put it in simple terms: Only when schematic sequences and routinised mechanisms are "cracked open" can their constructedness be revealed and the underlying characteristics, their "artificiality" and modifiability be identified. For a researcher with a social scientific interest who uses a theory-generating grounded theory methodology, such a representation – or "decoding" – is a potentially fruitful heuristic with which to shed light on hidden functional components. The members of the milieu, on the other hand, may consider the representation to be disrespectful; they may regard it as a violation of a taboo; or they may feel that their dirty laundry has been washed in public. It was only when I encountered such reactions when presenting the results of my research to mixed

audiences that I became aware of the explosive nature of such disparate perspectives and the problems they cause.

I presented my theory of predecessor-successor transitions at several conferences devoted to handover processes in specific social fields and institutional settings. When doing so, I endeavoured to flesh out the abstract skeleton of my theoretical model and to render it plausible by using empirical illustrations (case histories and process examples) related to the theme of the conference. Two of the conferences in question were devoted to family business succession (see Section 3.1 below). Family businesses are distal milieus for me in the sense that I am not a member (an actor, or an affected party) of that field. The other conference was devoted to university chair succession processes, among other things (see Section 3.2 below). This is the sub-culture to which I belong qua profession.

3.1 Family Business Succession – Description of a Distal Milieu

The participants in the first conference, which was entitled "Business Succession: Past and Present", comprised scholars and researchers from a variety of disciplines (mainly historians and social scientists) and members of the field in question (prototypically former heads of family businesses, who had handed the reins over to the next generation and had ended up at the conference eagerly intent on doing something useful with their practical knowledge of succession). In addition to describing smooth transitions from one generation to the next, I used interview excerpts to illustrate the various strategies employed during and after the handover by a predecessor who had difficulties letting go of his lifework. In other words, I highlighted psychological difficulties with which the conference participants from family businesses were not unfamiliar. What is more, as potential "lingerers", these participants were in a sense being targeted. Indeed, this constellation and thematic focus were to cause quite an uproar among the audience, which was something that I had not anticipated beforehand.

To show what I mean, let me give one example that I used in my presentation (cf. Breuer, 2008a, pp. 56f.). I focused on a relatively common temporal structure in transfer processes that I refer to as "cohabitation". Although the predecessor has formally handed over control to his successor (prototypically his son or daughter), he hovers in the background and interferes in the running of the business. Our research data[2] includes separate interviews with Mr Härtling, a father who was 77 years old at the time, and Claudia Brenner-Härtling (then 42 years), one of two daughters who had taken over the busi-

[2] Some of the interviews were conducted within the framework of a university seminar that I conducted.

ness, "Härtling's Hairdressers"[3]. The attitudes and actions of the parties, and the characteristics of the relationship during this cohabitation phase, are portrayed quite differently by the two interviewees.

Contractually speaking, the relationship is clear and everything has been settled. The father was relatively young (58 years old) when he transferred the business to his daughters. However, he held on to a key of the back door, literally and metaphorically. His apartment is located in the same house as the hairdressing salon; he is the landlord of the business premises; and there is (and always has been) a connecting door between his apartment and the salon, which affords him access at any time.

> Mr Härtling sen.: The way we handed on the business [...] that is no easy step. You are at the summit, and then you go downhill. But life does still have other qualities. At first I went on doing the books. Until I realised: you're no longer being involved! So you withdraw. [...] That is part of the quality of life. I can go downstairs [to the salon], I can chat with the customers and say good day and hallo.
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> Claudia Brenner-Härtling: Well first we worked here [just] as hairdressers and then after two years we took over the salon. And he really wanted to give it up, but then again he didn't. [...] He kept a close eye on what we were doing [...] and he was always throwing a spanner in the works behind our backs. And that led to ill-feeling now and then [...]
>
> Over the years he has withdrawn more and more from daily operations, but never entirely, not even now. When he comes back from his holidays, the first thing he does is to check how the salon looks and then he goes into the office to see how the books are doing [...] I'm actually the one who says: OK, I shall explain to you why you shouldn't do that. [...] If you don't find your things because he opens letters and files everything away, [things] that maybe haven't been paid yet, bills and stuff, then there is a complete shambles. [...] He wants to hand it over, but he still wants to be in charge, to retain control. [...] He's always like Big Brother, Big Brother is watching you. [...] He is also the landlord you see and the house belongs to our parents. [...] We're not allowed to brick up that door over there [doorway between the parents' apartment and the business premises]. I think that would be just as bad as taking his car away from him (Breuer, 2008a, pp. 56f.; our translation).

In a footnote to a contribution to a book published later, I discreetly mentioned the hostile, and sometimes angry, reactions on the part of some of the members of the audience at that conference:

> During the discussion at the Stuttgart conference this perspective gave rise to [...] critical enquiries from participants as to why psychologists always concentrate (only) on the pathological, the problematic, and the negative. It was felt that 'the positive' was lacking in this way of looking at and presenting things (Breuer, 2008a, p. 46).

[3] All names have been anonymised.

Moreover, in his "moderating" introduction to my text in the book publication of the conference papers, the social and economic historian Toni Pierenkemper (2008, pp. 10f.) made some retrospective comments that reveal doubts and scepticism as to the methodological soundness of my research project.

During the second conference at which I presented my theory of enterprise succession, supporting it, once again, with relevant interview excerpts that juxtaposed the viewpoints of predecessors and successors (cf. Bach 2008a; Breuer, 2008b, 2011b), I experienced quite similar dismayed reactions and rejection on the part of the "insiders" in the audience. They dismissed my examples as extreme negative cases and non-representative exceptions (worst cases) that showed entrepreneurial families, and especially the predecessors/retirees, in a bad light. This was heatedly discussed, and it was striking that the more the individual was personally affected or involved, the more agitated the reaction was (cf. Breuer, 2011b, pp. 93f.).[4] In the conference documentation, this manifest contrariness was smoothed out in an unattributed retrospective editorial comment under the heading "Comments on the Presentations". It read: "Future discussions on transfer strategies should cover both best-practice examples and worst-case scenarios. They should not be played off against each other" (see Bach, 2008b, p. 50; our translation).[5]

At both conferences, the aforementioned defensive responses on the part of field members were intensified by the fact that, in each case, my presentation was preceded by a contrasting paper in which examples of elaborate handover planning and execution were presented as success stories. At the first conference, the presentation in question was delivered by Carola Groppe. The theme was her brilliant social and historical-mentality study of the Colsmans, a family of entrepreneurs (cf. Groppe, 2004). The paper focused especially on the family's handover management over many generations, which has been characterised by open, reflexive, and elaborate intrafamily communication on the subject of the regulation of succession. This has contributed to the financially successful management of the firm over generations.[6] In the second case, the positive contrast was personified by a young entrepreneur from the bakery sector. The family firm, which has been in existence for five generations, had stagnated financially while his father was in charge. Thanks to the innovative business concept (ecological bakery) that he introduced when he took over, he had put the firm back on its feet and got it going again. At the Stuttgart conference, the

[4] Against the background of their own biographical experience with the problems of gaining access to an entrepreneurial family and adapting to its habitus, some contrasting reflexive comments were forthcoming from accompanying wives who had married into the firm.

[5] In an editorial note in the book publication, the editors protested against my "appeasement accusation" (cf. BlanckenburgG & Dienel, 2011, p. 94).

[6] The presentation is not included in the print version of the conference papers (Kollmer-von Oheimb-Loup & Wischermann 2008) for reasons that have nothing to do with my account.

young man delivered an impressive and captivating presentation on the way in which he had revitalised the family business (Baier, 2008).

These best-practice stories went down very well with the audience. Against this background, the heuristically accentuated "worst-case" examples from my repertoire put the audience – or at least those members who took the examples personally – in a bad mood and provoked defensive reactions on their part. In their feedback to my presentation, they made no secret of their distaste.

3.2 University Chair Succession – Description of a Proximal Milieu

The conceptual framework of the other conference context, which related to university chair succession, among other things, was the master-pupil relationship in different cultures and social fields (science, art, and religion). In the light of the fact that the basic idea was to contrast sub-cultures, I expected that I would feel at home at the conference because my grounded theory approach particularly emphasises the heuristic potential of case- and domain comparisons. However, once again, the way in which I handled the theme, and the illustrative examples I used, gave rise to a situation in which my presentation was generally perceived as a mirror held up to the conference participants. Since they were mainly university professors and junior scholars and scientists, they were members of the field in question and, thus, the theme was one that was relevant to their identity. I approached the general theme of the conference by addressing some aspects and scenarios relating to the appointment of successors to university chairs in Germany that involve the succession of "academic teachers" by one of their "pupils". On the one hand, I located the conference theme in the subculture of the participants (to which I, as a researcher and speaker, also belonged). From a structural perspective, on the other hand, I adopted a different focus, regarding succession as a temporal process.

In the academic milieu there is a practice of patronage between teachers and pupils. It continues to play an important role when it comes to filling university chairs and it is part of the usual repertoire of this sub-culture. Erudite pupils of "distinguished" teachers enjoy certain advantages when it comes to moving up the academic career ladder. This is especially the case when their teacher vacates his or her post and a successor is sought and appointed (cf., for example, the empirically-supported account of chair succession in a medical subdiscipline in Kovácz, 2010).

During my research and presentation, I encountered characteristic "sensitivities" among members of this milieu. And, once again, I was naive at first. One point that I addressed in my conference paper was the discursive-rhetorical characteristics of succession processes. Accounts of this nature are encountered quite frequently in the official self-representations of universities and institutes. Richard Muench (2007, pp. 297ff.) uses the term "rhetoric of excellence" in this connection. Such self-portrayals can be found, for example, in chronicles,

anniversary festschrifts, promotional flyers, or on university websites. They often take the form of heroic tales that highlight an inspirational former chair holder and, in so doing, ennoble and applaud the tradition and reputation of the university or institute in question. I refer, bluntly, to this mode of institutional self-representation as the "glossy brochure format". Such accounts are frequently tendentious and their historiographic soundness is often questionable.

I framed my presentation (and the book chapter that subsequently grew out of it) with two such questionable examples of the "heroic tale" text type and the "glossy brochure" presentation mode, one of which I placed at the beginning (1) and the other at the end (2).

1) On the website of the Pathological Institute of the University of Würzburg, the "founder of cellular pathology" Rudolf Virchow (1821-1902) is honoured as a former chair holder. Or rather, the institute adorns itself with his story by means of a "doctored" portrayal, from which the following passage (our translation) is taken:

   Virchow's Pupils and Successors in Würzburg

   Virchow's importance is also evident from the large number of pupils he had in Würzburg, no less than 50 of whom became well-known or even famous. They include:

   - pathologists: Klebs, Eberth, Rindfleisch
   - anatomists: His, Gegenbauer, Czermak, Häckel, Goll, Grohe
   - clinicians: Kussmaul, Friedreich, Ziemsen, Gerhardt, Biermer
   - ethnographers: Bastian, Rohlfs, Nachtigal, Semper

   [...] From 1865 onwards, Dean Franz Rinecker, who had been the driving force behind Virchow's appointment in 1849, appointed three of his [Virchow's] pupils in succession to his former chair. They ensured that their teacher's spirit remained present in the Würzburg Institute of Pathology until the beginning of the 20th century.

   - Friedrich von Recklinghausen, director from 1865 to 1872, studied under Virchow in Würzburg and was subsequently his first assistant in Berlin for six years.
   - Edwin Klebs, director from 1872 to 1873, was another of Virchow's Würzburg students who went on to become his assistant in Berlin.
   - Eduard von Rindfleisch, director from 1874 to 1906 was a doctoral student and assistant of Virchow's in Berlin.[7]

2) In an announcement entitled "Germany's Elite Institute", a department of RWTH Aachen University presents the following chronicle of succession (our translation):

   2004. Having reached the age of 65, Professor Pfeifer has retired from the post of institute head of the tool machine laboratory. He was the first ever holder of

[7] See <http://www.pathologie.uni-wuerzburg.de/geschichte/virchow_in_wuerzburg/rueckkehr_nach_berlin/nachfolger_u_schueler/> [accessed 24.2.2011].

> the chair of Production Measurement Technology and Quality Control in Germany and is considered to be the figurehead of German quality sciences. [...] With effect from 1 September 2004, his pupil Dr.-Ing. Robert Schmitt succeeded him as chair of Production Measurement Technology and Quality Control [...] and as director of the Fraunhofer Institute for Production Technology [...].[8]

To illustrate in detail the relevance of my theory's categorical inventory and the way it functions, I had intended to use an empirical case of chair succession from the material collected within the framework of my research project. In the case in question, a number of complications, conflicts, and rule violations had occurred during the procedure to recruit a successor. However, because of the prescribed time limits on conference presentations, I was unable to present the case history at the conference itself. Instead, I incorporated it as a supplement when I was adapting the paper for a book publication in which I had been invited to participate. The example (slightly abridged) was taken from my book Predecessors and Successors (Breuer, 2009, pp. 261ff.). The passage (our translation) reads as follows:

> At the University of M., the long-serving holder of a chair of humanities retired from office on reaching the legal retirement age. As an 'emeritus', he was then entitled to exercise certain rights at his old university in accordance with the traditional privileges that applied at the time. These rights included the (shared) use of an office, teaching and examination rights, and the right to confer doctorates.
>
> For many years, this professor had developed his own teaching principles and special 'school', which had, however, remained marginal in his discipline. This did not bother our protagonist very much. On the contrary, he cultivated his outsider position when forming his group of local followers, and in the course of time he increasingly detached himself from the standard operations and the discussion context of his discipline. He established his own tradition (teaching and typical scholarly infrastructure – journal, scholarly press, society etc.), which, although attracting a considerable number of – mainly student – followers locally (i.e. at his university), was frequently denigrated or ridiculed beyond the confines of his local milieu.
>
> As far as the organisation and social culture of the chair was concerned, he employed a traditional ordinarial model. The intellectual and institutional structures were tailored solely to his person. Only he possessed the authority to distinguish right ways from wrong in the world view of his community; to select, accept or exclude pupils; to inaugurate, or to excommunicate. The only people in his entourage who could 'come to anything' were those who submitted to his views, who conformed, and who desisted from developing divergent opinions or concepts. Independent minds among his pupils could not develop with any hope of success. He liked personally dependent followers who looked up to him and did not question his views. On the one hand, that spared

[8] See <http://www.institut-wv.de/6691.html> [accessed 24.2. 2011].

him critical or competitive offspring and prospective colleagues who could challenge his authority on his turf. On the other hand, as his career approached its end, there were only 'weak' pupils who were relatively lacking in independence and profile and could hardly be brought into position as serious contenders for the succession to the chair. At the same time, however, our professor had many followers among the students. He had been able to convince them of the worth of his teaching and they had adopted his views as their own. The social form of this system – a configuration comprising a master and his conformist apostles and pupils – was sometimes described by outsiders as a sect-like structure: dedicated supporters comprising students and close staff members, and a manifestly extreme power divide between the 'guru' and his 'community'. Beyond the confines of this structure (among colleagues, fellow scholars, in the local academic community) this 'school' was largely ignored. However, things were allowed to take their course with little outside interference.

When it came to filling the vacant chair, our protagonist was in an awkward position. On the one hand, he desperately wanted the chair to be filled by a representative of his particular professional orientation in order to ensure the continued existence of his school. On the other hand, however, as he had never let any of his followers come to the fore, none of them were now able to fill his chair, literally and metaphorically. There was no sufficiently distinguished pupil for the post. Nonetheless he made every effort to influence the selection of a successor. His aim was to bring one of his protégés into position. However, according to the usual criteria of the disciplinary culture, the individual in question did not have the necessary qualifications. With the help of his devoted followers, our protagonist endeavoured to compensate at other levels for this deficit by exerting more or less massive social and 'political' influence in various areas. When so doing, they blithely ignored formal demarcation lines and even more so the rules of collegiality and good taste.

To ensure that their tradition would be carried on by the successor, our emeritus and his disciples tried to ensure the selection of a conformist candidate and the fortification of their sphere of influence against an environment perceived as hostile. The following are examples of the strategies and measures employed:

- The emeritus regularly participated in the meetings of the appointment commission that selects a successor from among the candidates, or rather plays a major role in paving the way for the appointment decision. According to the code of appropriate collegial behaviour, such interference on the part of a predecessor is distasteful to say the least. However, it was grudgingly tolerated by his colleagues at the university.
- He got his supporters, students and assistants to strongly oppose and agitate against unwelcome and potentially promising, i.e. dangerous, candidates for the succession. He supplied them with background information and incited them to carry out actions that also involved the use of forms of psychological terror. For example, they initiated petitions against competitors; they circulated slander in the competent federal state ministry (which had the final decision when it came to filling the post); they launched campaigns in the local press to defame competitors for the job; and they filed

official complaints [Dienstaufsichtsbeschwerde] against members of the appointment commission to whom they were not well disposed. The predecessor remained hidden in the background during these activities. While he let his militant supporters off the leash, as it were, he was able to feign innocence, thereby keeping his hands clean in public.

- During our protagonist's term of office, deserving and conformist pupils who held posts as research associates or assistants had received lifetime contracts with his institute. Shortly before he retired, he used a clever ruse to procure a permanent post there for one of his eager pupils. By institutionally securing the academic 'second row' (i.e. the non-professorial teaching staff) in this way, he did his best to ensure that his school would continue to exist. Moreover, the possibility that a successor with a divergent disciplinary approach and following would be able to bring in assistants of his or her own choice was thereby prevented because the posts assigned to the holder of the chair were already occupied. The eventual successor would have to deal with the personal legacy – the scholarly offspring – of his predecessor for a long time to come. Indeed, in view of their age, they will outlive the successor's tenure. Here, too, the measures taken by our protagonist were contrary to the principles of morality in the academic context. When implementing them, the predecessor sometimes tactically outwitted the university administration.

In the end, the protagonist's unscrupulous strategy outlined above essentially failed. For outsiders, neutral observers and participants the difference in qualifications between the candidate proposed by our former chair and the person who was eventually appointed was too obvious. Moreover, the exposure of the questionable interference on the part of the predecessor caused reactance effects on the part of some participants in the process who had originally been impartial. One major outcome of the predecessor's tactics was the exceptional delay in filling the post and the prolongation of the period during which it was vacant. This afforded him greater opportunities to exert influence during the interim period.

The defeat of the emeritus led him to abandon all previously assumed obligations (supervision of theses, for example) immediately after the appointment of the new chair, presumably because he felt that he could cause his unwelcome successor more problems in this way.

After my contribution had undergone anonymous peer review, the editors of the book publication wrote to me as follows:

> [...] we would request you [...] to delete the chair succession example. In our view, the structural-sociological description of the master-pupil relationship is very vivid and instructive and the provocative and polemic example would divert the focus away from the questions of importance to the project and the volume. (e-mail of 15.06.2010 from N.N., a member of the editorial team; our translation.)

When revising my contribution, I removed the example, not least because the text had to be shortened anyway. In the modified text, I indicated that the case history had been dropped due to space limitations and could be looked up in

Breuer (2009). Observing the space limits, I devoted an excursus to the discursive relevance of the episode in question, pointing out that it furnished a concrete illustration of the importance of one aspect of the proposed categorical system, namely the fact that social science accounts of succession processes are always rhetorical (re)presentations from a particular perspective. I argued that, in view of the fact that these representations affect the identity of the participating field members and trigger emotional engagement on their part, communicative complications are naturally to be expected when master-pupil succession negotiations in the academic world are discussed publicly. I quoted Pierre BOURDIEU (1988, pp.31ff.), who highlights problems of this subculture that stem from the fact that the social world being discussed is one in which its members are personally involved. He focuses especially on "the question of exemplification, [i.e.] illustration by use of examples". In Bourdieu's view, no matter what precautionary measures authors take, they can hardly dispel the suspicion that they are guilty of denunciation.

I submitted my revised text in August 2010, confident that I had fulfilled the conditions imposed by the editors of the book. After quite a while, I received the following e-mail:

> [...] as part of the preparation of the manuscripts for publication, I hereby enclose the refereed version of your essay and would be grateful if you could give the go-ahead by 27 January 2011. (e-mail of 20.01.2011 from N.N.; our translation.)

In the "refereed" version all illustrative examples and the excursus on the discursive characteristics of the succession theme had been eliminated without any explicit comment or communication. I sent a somewhat irate reply to the effect that I considered the "refereed" version to be "mutilated" or "censored". This yielded the following "explanation" from the leading editor of the volume:

> Dear Colleague, [... N.N.] from the review/copy editing group forwarded your e-mail to me today. I regret to note that the communication has not been to your satisfaction. I am sorry about that. I had assumed that in the previous exchange of e-mails you had been informed of the amendments and abridgements that were jointly decided upon in the [XY] project. Should transparency have been lacking, I, as one of the main persons responsible [for the project], apologise for this. Hopefully I can contribute as follows to clarifying the matter. Originally, your contribution was one of those that were reviewed negatively. However, after one reviewer came out strongly in support of your paper, and rightly so, a consensus was arrived at that your contribution could be accepted under certain conditions. These conditions included [the requirement] that all forms of polemics and value judgment must be avoided [...] as value-neutral, objective presentation is of paramount importance in our disciplines [italics added; F.B.]. We would be happy to publish your contribution under these conditions. (e-mail of 21.01.2011 from the editors; our translation.)

Besides fundamental doubts as to the suitability of my contribution, this communication contains a reclassification of "data" used for (re)presentation pur-

poses as "polemics". In view of the fact that the maxim of "value-neutral, objective presentation" in the scholarly discourse of "our disciplines" rules out the use of polemics, my type of data is not approved for use in a scholarly publication. Thus, according to these regulations, perspectival representations of social processes in an everyday field (in this case, the academic milieu) are not permitted and may not be discussed. Within the framework of the focus and perspective that I adopted, this argument appears to be an example of involved field members imposing a taboo on and resisting certain descriptions, and of a "rationalising" reaction to the negative response this triggers. Characterisations intentionally presented as "data" are reclassified as "polemics" – as washing dirty laundry in public – and thereby excluded.

In addition to giving my permission for the "abridged" text to be published, I wrote the following reply:

> I would like to comment on the line of reasoning as follows. I consider your understanding of 'polemics' in this context to be completely inappropriate. To my mind, the succession examples which I originally used and which I gave in the revised essay of [last] August when referring to discourse phenomena are empirical facts. In other words they are empirical discursive phenomena with the help of which I clarify and illustrate something that I previously postulated theoretically or categorically, namely that there is front-stage and back-stage communication in the master-pupil and predecessor-successor relationship, i.e. formal and informal discourse that may be contradictory. If you wish to exclude such phenomena from the stage of discourse by pronouncing a verdict of polemics, then, in my opinion, you are doing no favours to the argument that presentations in our disciplines should be 'value-neutral and objective'. (My e-mail of 24.01.2011 to the editors; our translation.)

In my interpretation, the negotiations with the editors outlined above reveal that both sides felt personally affected and reacted accordingly. The reactions displayed by the editors can be regarded as a response to "insults" to their own milieu and as resistance against an unwelcome description by reclassifying it as a violation of professional standards ("polemics") and by exercising publishing control. My reaction, as author, was ambivalent. On the one hand, I held on to my ambition to publish; on the other hand, I was offended by the authoritative way in which the other side enforced its view with the help of what I felt to be censorship, and I protested against this measure.

4. Frameworks for the Interpretation of Communication Disparities

The framework within which scientific research findings and illustrative examples are interpreted may be quite different for field members, researchers as authors, and researchers as members of the field in question. From the researcher's perspective, an example of an unsuccessful effort to solve an everyday problem (here: succession management) is an "interesting case" which,

under certain circumstances, can be used to reveal and theoretically unlock the connections between social and psychological mechanisms that go unnoticed or remain hidden to the observer when things proceed smoothly. Depending on their methodological-explorative orientation, researchers may be less interested in how often such a case occurs or whether it is representative of the field in question because individual cases can be also useful when it comes to illuminating the structural background of the phenomenon under investigation. From the field member's perspective, on the other hand, the exposure of such an example of failure may constitute a disparagement of the persons involved or of the milieu as a whole and may therefore be considered an indiscretion, a denunciation, an embarrassment etc. It may awake personal memories and fears of failure or touch a "sore spot".

The social relationship between the researchers and the researched in the everyday life fields discussed here provides a framework for strategies on the part of the "research objects" with which they can respond to and influence the way the researcher characterises them. This is due to their competence and autonomy and to the distribution of power within the relationship. In the cases/areas discussed in the present paper, the affected field members reacted with defensive responses and measures to what, for them, was an unpleasant confrontation with experiences of alienation.

I encountered the following defensive measures in the conference interactions described earlier:

- Like their mythological counterparts, the bearers of bad news are punished ("shoot the messenger") – albeit not by death but merely by denigration. For example, the perspective of "the psychologists" is discredited or ridiculed: they are accused of making a problem out of everything, of being interested only in negative aspects; the scientific soundness of their research methodology is called into question.
- The semantic content of the message is questioned, declared to be false or an exception to the rule. The case study presented is classified as "extreme" or "not representative". Moreover, "it was not conducted in a methodologically sound way".
- If the researched are in a position to control the means of communication, the dissemination of the message is prevented or the content is censored; scientific publication is not permitted, or is allowed only under certain conditions; the permission to publish is withheld.

The disparities between the perceptions of the protagonists juxtaposed here can be viewed or "understood" in different interpretational frameworks. These frameworks can refer to a structurally determined constellation (see (1) below) or be based on a concept of development (see (2) below):

1) The ethnologist Volker Gottowik attributes the fundamental disparity between the perceptions of researchers and the researched to the fact that the ethnological (and the social science, F.B.) viewpoint and discourse have

their own language and performance. These are characterised by "exaggeration, caricature, or variation of an apparently familiar motif or theme" (1997, p. 325, our translation). The social science "representation of the strange in the categories of the familiar" (loc. cit.) inevitably causes "the other" to experience an oral or written representation of his or her own subcultural life formulated in a manner foreign to that milieu as an alienation.

Pierre Bourdieu also characterises the said perception disparities in structural terms. He attributes them to the different positions occupied by the observed field member and the observing social researcher. Bourdieu illustrates the constellation of perspectives with the help of an analogy: A "sociologist who studies the school system has an 'approach' to school that has nothing in common with that of a father who is looking for a good school for his son" (1993, pp. 370f., our translation). Here he highlights the contrast between the researcher's "contemplative eye", or perception as "drama", and the exigencies and urgencies of "real life" to which the father is subject. Because they occupy different social positions, their room for manoeuvre and the constraints to which they are subject also differ.

According to this interpretation of the communication constellation, there is little reason to expect an epistemologically and praxeologically productive exchange between the two standpoints and perspectives. There appears to be neither a prospect for validation or optimisation of validity, nor for the improvement of practice with the help of "critical enlightenment".

2) Within the framework of a model of (knowledge) development the disparate perspectives can also be linked to the difference, or the dialectic transition, between various modes of reflection, namely the originally centred, the decentred and the recentred stance. Arne Raeithel (1983, 1996) elaborated these modes in his proposal for a model of general epistemological development. This model is based mainly on ideas formulated by Hegel, Piaget, and Holzkamp. I once briefly summarised the basic idea behind Raeithel's categories (in Breuer, 2003, paras. 28-30, our translation) as follows:

   > An originally-centred stance means that during his/her activity the subject regards the structure of the object but does not reflect on his/her activity in relation to the object. The subject acts in an unmediated way, as it were, following a pattern of which he/she is unaware.
   >
   > A decentred stance refers to the process of stepping back and gaining distance from one's own patterns of activity; focussing on the pattern; assuming an observer- or meta-perspective vis-à-vis one's own original perspective; developing a reflexive awareness of the originally-centred subjective concepts.
   >
   > A recentred stance represents a level of reflexive activity at which the observing subject 'can reflect on, alter or re-invent the parts of the social system' in dialogue with him- or herself and with others.

The originally centred stance is typically adopted in situations where the actor is obliged to take urgent action and when he or she follows habitual or tradi-

tional patterns. Decentering, on the other hand, means that the actor reflexively observes his or her own actions and their context from a distance.[9] In the context of the present study, recentering could take the form of (re)organising a handover/succession process in collaboration between the field members and researchers in the role of participants or advisers. An ideal scenario in this development model is that "informants" would eventually become the cooperation- and discussion partners of the researchers – possibly after passing through an intermediary stage in which they play the role of critics.

The feeling of alienation triggered by the description of a phenomenon from an outsider's perspective can constitute dencentering and thus serve to transcend original pattern-bound stances. However, this calls for "favourable circumstances" such as the absence of immediate pressure to act; willingness to engage in communicative reflection on the determinants of one's own actions and reactions; participation of all relevant parties in the field (protagonists and other actors); openness on the part of the participants towards alternative and novel viewpoints; a "relaxed" interaction situation or an agreed interaction framework. In my view, it would be feasible to organise "mixed" conferences of researchers and members of the everyday field in question as prototypes of such scenarios. However, there is no guarantee that such a (self-) reflexive exchange of views will function automatically and without a hitch.

One hindrance observed in the case of the transfer of personal objects from a predecessor to a successor in family business- and university milieus (and presumably present in other fields as well) is the fact that, for a variety of reasons, the tendency and willingness to engage in open communication about the transfer of "ownership" is not particularly pronounced: "We don't talk about these things!" (see Breuer, 2009, pp. 287ff.). In principle, participants should be in a position to perceive, understand, and communicate about the phenomena dealt with here and at the said conferences. However, certain participant constellations, attitudes, identifications, interests, and temporal processes are such that some protagonists and actors have every reason to keep their own ideas, intentions and strategies to themselves; to communicate selectively or in a whisper; or to utter only doctored statements. The fate of the "personal objects" that were dealt with in the present paper turns out to be quite an intimate matter, and it is just as difficult to discuss it in public as it is to talk about certain other intimate topics.

[9] In his deliberations on philosophical anthropology, Helmut Plessner (1975 [1928]) elaborates the concept of "positionality" to describe the relationship between organisms and their environment. He arrives at an analogous distinction between "centric" and "eccentric positionality", the latter being based on self-reflexivity. However, he relates this distinction to the fundamental distinction between animals and human beings.

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