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8. The Validity *of* Action Research – Validity *in* Action Research¹

My theme, validity and Action Research raises several questions. What is validity? What is Action Research? Are they unequivocal concepts? Are they relevant for each other, or compatible? An overall objective of this book is to relate and integrate discussions about Action Research better with discussions within more mainstream research traditions, rather than presenting Action Research as a parallel and unmediated alternative, a separate paradigm, or discourse, “out of touch” with mainstream social research². Since the concept of validity in social research was both conceived and born from within mainstream research, it is important to review the mainstream discussions, to assess the relevance of “validity” for Action Research. Hence, I will discuss the concept of validity and its historical entanglement with different methodological approaches and paradigms, followed by a critical review of some concepts of Action Research, in order to sort out aspects of validity relevant for Action Research. But the following can only be selective suggestions and summaries, maybe representative in a holographic way. References serve mostly as positions presenting validity challenges Action Research approaches must solve and transcend.

The strongest justification for Action Research is produced by showing that what most distinctively characterizes Action Research cannot

1 Discussions with Richard Winter, Benedicte Brøgger, and Lars Klemsdal have contributed to improve the quality of this text. So has feedback from the editors.

2 Preventing separation, without conflating distinctions, is an important concern of mine. See Eikeland (1985), (1995), (1998), (2001). Paalshaugen (1992) explicitly defends Action Research as an incommensurable paradigm.

be a) reduced to a mere combination for utilitarian purposes of applied theories and techniques, borrowed from other research approaches (e.g. Spjelkavik; 1995). Instead, b) Action Research represents a core element of those other approaches, not sufficiently articulated (i.e. tacit) within mainstream presentations. Hence, c) Action Research, when developed and justified properly in this way, could contribute importantly to solving some inherent contradictions, impasses, and validity-problems bothering mainstream social research. This situation makes other approaches intuitively approximate Action Research, labelling it a “practical turn”, “turn to practice” (Schatzki and Knorr-Cetina, 2001; Gherardi, 2000; Nicolini et al. 2003), “participative approach”, “practical reflexivity” (Cunliffe and Easterby-Smith, 2004), and similar things. I try to show – *in outline* – a movement from a) to b) and c), both from within mainstream approaches, and from within Action Research since the forties.

Hence, mainstream validity challenges cannot be ignored by Action Research, and relegated to other parallel disciplinary discourses. Instead, they should be used to “mainstream” Action Research itself, within an emerging socially distributed mode of knowledge production in need of new research practices, and a new understanding of different ways of knowing (Cf. Gibbons et al. (1994), Nowotny et al. (2001), Eikeland (1999a), (1999b), Eikeland & Fossetøl (ed.) (1998).

8.1 Approaches, paradigms, and trajectories

I will write about three “ways of accessing data” in mainstream social research, through: (1) observation, (2) asking questions, and (3) experimentation. They are not mutually exclusive, but often presented as distinct. More basically, I describe different “paradigm-relationships” between researchers and the researched, based on ways of knowing extracted from the thoughts of Aristotle as presented in Eikeland (1997a; 1998b; 1999b; 2001; forthcoming): (I) a spectator-astronomer (*spectas*) paradigm, (II) a manipulator-user (*manipus*) paradigm, (III) a stranger-visitor (*stravis*) paradigm, and (IV) a native-performer-community (*napeco*) paradigm. Each paradigm means striving to live up to different ideal standards of research practice. Hence, the same “way of accessing data” may still be different within different “model-relationships”. Finally, I refer to differ-

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ent historical, developmental trajectories of disciplines and approaches.

Observation (1) and questioning (2) have been the main bases for sociology and anthropology, while experimentation (3) has had a stronger base in psychology. Both (1) and (3) treat subjects studied as external (“natural”) objects, whose changes are to be explained, predicted, and sometimes provoked, modified, and controlled, by the researchers. Communication, “normal” interaction, and mutual influence between researchers and researched are minimized and controlled, in order not to invalidate findings. Questioning (2) breaks the separation of researchers and researched, but brings with it a host of potentially invalidating “researcher-effects” and “reactivities” of the encounter, which should be “controlled for”.

Still, these approaches are *all* in the “othering-business” of studying what *they* – the others – do, based on a division of labour, and a distinct separation, between the researcher and the researched. The researchers move in (on the others), and stay temporarily, observing, asking questions, or administering treatment, while taking notes, or recording. Then they move out from “the field”, leave “the others” behind, and report their “findings” – their empirical “experience” – with their interpretations and explanations, to a separate community of researchers, different from the observed, where knowledge about them is supposed to be accumulated. This self-conceptualisation has not changed much over the last 50 years, regardless of all the discussions about how the observers are observed (observational reciprocity, relativity of perspectives), the reactivity-effects of the research processes, the potential arbitrariness of the conceptual framing of observations, interpretations, and explanations, and other difficulties (Cf. Eikeland, 1985b; 1995). Attempts at modelling social research on natural science have tried to accommodate to a *spectas* paradigm (I) (observing and explaining, without intervening), or to a *manipus* paradigm (II) (controlled, experimental intervention). Qualitative social research remains mostly within a *stravis* paradigm (III) (questioning and close observation with minimum intervention). But although they approach things from different starting points, their trajectories seem to converge, bringing them to a similar intellectual “terminus” of “theory-pluralism” among an unlimited series of differently positioned, reciprocal observers, doing research *on* each other (see Chapter 2).

Action Research has a different trajectory. It developed from within the experimental *manipus* paradigm (II), emerging, transcending, and separating from it gradually, and not always very consciously. Quasi-experimentation grew into a parallel trajectory from the same roots, Don Campbell (1916–1996) being a major figure. Still, Campbell (1978) explicitly acknowledges his relationship and affinity with the Action Research tradition. The acknowledged founder of Action Research, Kurt Lewin (1890–1947), was an experimental psychologist, and John Dewey's (1859–1952) philosophy was all about the expansion and diffusion of the experimental method and attitude, from insulated laboratories to everyday activities in communities, schools, and organisations.

In a way, Action Research was born in 1946 by transforming the social field-experiment practically from within, when, one evening, the subjects of research were invited to join in the interpretation of the findings (French; 1985, French and Bell; 1990:25, Benne et al.; 1975:1–6, Lippitt; 1949). This practical “*Aufhebung*” of the controlled and manipulative experiment, changed the relationship between the knower and the known, both principally and practically, breaking the division of labour between the researchers and the researched. Treatment was no longer to be administered by researchers on “blinded” subjects. But still, experimentation (3) should not be abandoned. It should be transformed. Action Research should not remain within the horizon of a *manipus* paradigm (II), however. It should adapt to a *napeco* paradigm (IV), doing research *with*, not *on* others, searching for actual commonalities we relate to as competent, native performers, even when borrowing techniques developed within other paradigms (e.g. 3–IV).

The very separation – through divisions of labour and allocation of tasks – between researcher and researched, the knower and the known, contributes much to the invalidation of knowledge, for either epistemological, ethical, or both reasons. Some current Action Research, however, seem to be stuck in the ruins of a *stravis* paradigm, a *manipus* paradigm, or both, speaking uncritically about “interventions” to create changes in the reality of people they visit. With only vague ideas about any *napeco* paradigm, they keep fighting against an older *spectas* paradigm.

8.2 Validity in general

According to *Webster's Unabridged Dictionary* "valid" derives from Latin "*validus*", meaning "strong" or "powerful". Among several meanings, the one that claims validity as something "sound", i.e. well grounded on principles or evidence, and able to withstand criticism or objection, seems to be the most relevant meaning for research in general. Descriptions, arguments, inferences, interpretations, justifications, insights, explanations, predictions, and conclusions would all seem to need validity in this sense. Why is research supported as a common good by political decision-makers? Why should it be? Probably because they, and most people, still believe that research produces valid results in this sense, which are somehow better than dogmatic, or just any stray, opinion or belief, and better than unexamined habit and tradition. Presumably, research produces unbiased and adequate, or, at least improved, knowledge of some "thing" known.

This prevalent "enlightened" *doxa* is basic to the legitimacy of modern institutions. Arbitrary, uninformed, or dogmatic public bureaucracies, "professionals", or "mob rule", acting only in their private interest, are intolerable. If "science" cannot deliver valid knowledge and competence, most people will feel an urge to find something *better* (more valid). An important reason is this: Why should I listen to, and learn from, whatever anyone tells me? Provided you cannot force, manipulate, or seduce me, probably most of all because your observations, arguments, or inferences are better (more valid) than mine, or your knowledge and competence is better (more valid).

Validity, then, concerns the *status* of research, or, of *any* arguments and points of view, or, of any practice, competence, and skill, as somehow providing *better* insight into, or mastery of, something, than any arbitrary habit or opinion. Hence, validity is not arbitrary or just descriptive, merely registering what happens to count – *de facto* – as current opinion accepted by some people in some context. Validity needs justifications, legitimacy, and competence. It is normative, prescriptive, and argumentative (*discursive*), specifying what should count rightfully as standards of truth, justice, fairness, beauty, and similar normative questions. It says (simplified): trust, believe, or learn this, *for these reasons!* Even rela-

tivist attempts at abandoning or deconstructing truth and objectivity, (do and must) say the same thing *about their own deconstructive claims*. So, validity, in the broad sense, seems unavoidable for any activity or opinion posing as science or research, or claiming to understand, know, or master anything. Since Action Research does all of this, “*we must make sure we are not kidding ourselves*” (and others), as Reason and Rowan (1981:243) have formulated it on behalf of Action Research.

8.3 Mainstream special validities

But “validity” also has a more restricted sense within social research, as a technical term associated with *testing* and *measuring* social phenomena. In this sense, it worked like a Procrustean measuring rod in the middle of the 20th century, dismissing as unscientific, knowledge that did not “measure up” to technical definitions of it. Measuring and testing spring from the tendency in the natural sciences since the 17th century, to quantify and measure all things (Cf. e.g. Porter; 1995 and Crosby; 1997). It also springs from the social practice of *testing* individuals, to find out if they were of a certain desirable or undesirable kind, or possessed similar properties, for example “witches” in early modern Europe, or, “fitted for mandarin-work” in the administration of the Chinese emperors of old (Cf. Wainer & Braun, 1988: xviii). Fused, these traditions surfaced in a modernised, Western scientific guise, in the aftermath of soldier testing in World War 1, as part of a broad, burgeoning “test-industry” or movement, according to Cronbach (1916–2001) (1970:9), for 50 years the most central figure internationally in psychometrics. Tests try to find or invent *observable* measures for *non-observable* entities like intelligence, attitude, “status”, etc. As *indicators*, results immediately raise questions like: “Are we measuring what we think we are measuring?” (Kerlinger (1973:457) or, “is this a *valid* measure”?

8.3.1 Construct validity

According to Cherryholmes (1988:421), “Social research methodology entered adolescence, if not maturity, in July 1955.” Angoff (1988) shows that testers had mostly been concerned with *pragmatic* valida-

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tion, i.e. the mere predictive power of tests, *useful* for certain purposes, like finding “mandarinability”. They had been less concerned with providing consistent theoretical explanations, giving better insight and understanding, and a broader legitimacy for the use of tests. In order to provide tests with scientific validity and legitimacy, and to improve the interpretation and explanation of results, Cronbach and Meehl (1955) argued that “construct validity” was the primary kind, decisive also for other kinds of validity. “Constructs” are conceptual parts of *theories*, not operationally defined, i.e. non-observables such as “witchiness”. Cronbach (1970:142) illustrates: A reading test may *predict* poor “school performance” of a child. But without high “construct validity” in interpreting the results – i.e. a valid explanation – the test *results* will not tell us *why*, whether it is caused by an emotional disturbance, mental ability, organic speech deficiency, cultural or sub-cultural conceptual differences (ethnic, class, gender, etc.), witchcraft, or combinations, thereby also obfuscating adequate action to remedy the situation.

Construct validity is primarily necessary for *practical* purposes, not predictive. But, Cronbach (1970:123 and 142) continues, “Construct validation is more complex than other types of validation”. “*Predictive* validity is examined in a single experiment”, comparing it with other *concurrent* measures of the same, or with real time *criteria*, while “Construct validity is established through a long-continued interplay between observation, reasoning, and imagination”. Mishler (1990:436 & 418–419), summarizing subsequent discussions, sees convergence among “prominent validity theorists on the primacy of *construct* validity”. Cronbach (1970:142) concludes that the “Process of construct validation is *the same* as that by which scientific theories are developed”.

Developments in social theory and methodology since 1970 have shown (Cherryholmes; 1988) that the admission that a) “*All* validation is (ultimately) construct validation”, b) “The end goal of validation is explanation and understanding” (Cronbach (1984) in Mishler (1990:418)), and c) validation processes are identical to theory development, was like opening Pandora’s box (Latour; 1999:23) – i.e. the floodgates of phenomenology, hermeneutics, Marxism, feminism, science studies, post-structuralism, and deconstruction – on the discussions about validity. The flood of critique shifts the emphasis from technical measures to “choice” or the development of an interpretive horizon, i.e.

to what the researchers bring along to the “thing” studied. As recognised by Cronbach (1988 & 1989), it shifts the horizon from a disciplinary psychometric discussion, to the general horizon of philosophy of science, epistemology, and methodology, and even wider, to discussions about power, politics, knowledge forms, and the institutional, cultural, and historical formation and embeddedness of knowing, i.e. to questions about the validity of all the built in preconditions, preconceptions, and presuppositions of certain forms of science and research. It deconstructs, *from within*, the restricted concepts of “predictive”, “concurrent”, and “criteria” validity. But in order to retain a critical sense of direction – not drown in the flood, and let all things pass – an obsession with validity seems necessary (Cf. Lather; 1993). From here, there is no *valid* “turning back” along the road that brought us. Naïveté is over, enter nativeness. For survivors from the flood, and from the “evils” of Pandora’s box, flooded lands are fertile.

8.3.2 *Whose constructs?*

These discussions about validity worked in the border zone between a *spectas* (I) and a *manipus* (II) paradigm. Their “constructs” were made and defined, not by “natives”, but by a segregated group of researchers, either trying to minimize intervention and all reactivity of research methods, or administering controlled treatments, trying to predict and control, through “blindfolding / blind-testing” subjected participants. Behind their somewhat artificial precautions, are several presuppositions, hardly self-evidently valid, but still central for maintaining both *internal* and *external* validity for this kind of research. *Internal* validity is the assumption that the “independent variable”, introduced and varied by experimenting researchers, is what actually causes the registered changes in “dependent variables”, *not* some uncontrolled and unknown intermediate, or complementary, cooperating cause, making results like “x causes y” valid at least for those exactly specified experimental conditions.

External validity is the assumption, granted internal validity, that the introduction of “the same” independent variable as in the experiment, will have the same effect when introduced under similar non-experimental conditions (Mishler (1990:417)). This arrangement of research

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seems to presuppose, *first of all*, that there are “natural states” of communities and organisations, or of any people studied, to be approached by researchers. These states are supposedly their working mode *without* the presence of researchers intervening and studying them, making “unobtrusiveness” and self-obliteration a research ideal. *Secondly*, that what is observed can be trusted to represent adequately the things observed, not only what the observers bring as preconceptions and prejudices from their point of view. This seems to require observation through a perspective from “nowhere-in-particular”, without any selective “filters” or coloured lenses. And *thirdly*, that human events are controlled by efficient *causes-and-effects*, which can be separated, isolated, manipulated, and controlled. *Finally*, along with these, and as a consequence, looms the ethically dubious practice of treating other human beings merely as external, natural objects.

But, *first of all*, people are influenced continuously by the state of their own bodies, by each other, by the institutions framing and directing their activity. Why eliminate only the researcher-effects? What makes the rest more “natural”, and desirable as a subject of study? *Secondly*, all observations must be “filtered”, “framed” and “coloured” by whatever cultural-cognitive “instruments” the observer brings to the observation, making it thoroughly *interpreted*. To perceive is to perceive something *as something*, and always *some* things, never *no* thing, or *every* thing. And, *thirdly*, since people are different, “the same cause” may produce one reaction in one individual, culture, institutional setting, organisation, or community, a different one in another one, depending on how “the cause” is interpreted, or handled, by the ones “affected”, and on whatever other causes are influencing them. Physical, chemical, and biological agents may have effects evading human, cultural filters, but we are mostly *not* merely physical, chemical, or biological to each other. Statistical correlations may, of course, be valid on an aggregated level, but it is, by definition, not possible to deduce from these alone, anything about any new individual case encountered. Statistics is a “spectator sport”, as Jaeger (1990) appropriately points out.

In spite of this complexity, social life as lived experience is not arbitrary, random, and chaotic, and somehow *not* knowing and handling these complicated facts of social life, would imply *not* knowing how to participate competently in everyday activities. Since most of us par-

ticipate, and find our ways around, somehow, most, or at least many of us, must know already. What we perceive, and how we interpret it, is determined by who we are, our background, our position, our interests, etc. There are no simple operations to perform in order to reach an unbiased “view from nowhere”. Also, technical interventions have low validity, if any, in social settings, except, maybe, statistically, in introducing large-scale measures. What *must* be known by performing natives appears too complicated to catch for this kind of science. Hence, learning what competent natives must know already, acting and interacting in their societies, seems a more promising subject of study than externalised “quasi-natural” states of societies, *without* researchers, or with “researcher-effects” eliminated. Maybe “researchers-as-native-apprentices” is a better idea (cf. Coy (ed.), 1989).

8.3.3 *Watch the natives (turning against you)!*

Phenomenologists break the frameworks set by the spectators-*cum*-manipulators’ paradigms, according to Cherryholmes (1988:43off.). Natives have advanced and complex competencies, not merely based on detached observation, technical manipulation, or pragmatic considerations of usefulness. Hence, the phenomenologists want to take seriously the “constructs” of the “natives” by finding out what they are, understanding them, and using them as starting points for developing theory. The prototypical discipline for doing this remains anthropology. Its conventional practice of studying far-away cultures makes it clear, however, that this is a move from a *spectas* and *manipus* paradigm, to a *stravis* paradigm. Although even the discipline and practice of psychometrics could be considered a strange, native culture to be visited and observed by anthropologists, the phenomenologists are still segregated researchers (Cf. e.g. Latour and Woolgar (1979), Latour (1987) and Knorr-Cetina and Mulkay (1983)). To remain or “go native” is considered a threat to validity (opening the floodgates), since we presumably then acquire certain “interests”, and invest “emotions” in the practices of the natives, become moved by limitless, uncontrollable influences, and come to see things *too much* as the natives do. By “going native” we presumably lose interest in, and the ability to do, research, and stop reporting back to the separated research community. We become biased,

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and unable to be disinterested and “objective”. Hence, “participant observation” *is*, quite deliberately and consciously, being a stranger and visitor, not yet a native-apprentice, since true apprentices aim to be masters themselves.

But the emphasis on construct validity does give the floor to anthropologists like Geertz (1973). Like many, Geertz abandons all belief in “data”, i.e. in anything presumably “given” as “uncultured”, or “theory free”. Cultural “findings” are always already designed, selected, and interpreted by the natives, and must next be selected and interpreted by the anthropologist observers, creating Giddens’ (1973) famous “double hermeneutic”. Hence, the primary business of anthropology (Geertz: 1973: 9ff.) is not measurement, but interpretation and explication of meaning. The researchers’ business is to understand “the natives”. Participant observation has, however, conventionally been understood as *observation*, avoiding, as much as possible, intervention and influencing events. Hence, “participation” remains a way of closing in on the observed. Schatzman and Strauss (1973:13f.) still share the presuppositions of the *spectas* paradigm about “unobtrusiveness”, comparing themselves to zoologists and archaeologists, merely “observing events in a natural situation”, instead of the supposedly contrived artificiality of experimental laboratory research.

But what we perceive depends on who and what we are, and reactivity is pervasive. Geertz’ (1973:16) validity-question cannot be avoided: “*How* can you tell a better account from a worse one”? and, “what *basis* do we have for our interpretation”? Observation based interpretations may say more about the observers, transferring their personal and private contexts, than it says about the observed, since what the observed is interpreted *as*, must be something the observers bring along, and presumably understand. We cannot just say; “I saw it”, or, “I heard it”, without *understanding* it.

Is it possible at all for interpretations to be based on close, but non-intervening and unaffected observation through preconceptions brought along from “theory” or “home culture” alone? Currently, there is a tendency to rest content with stranger-visitor researchers providing *their* interpretations, since *both* explanation *and* understanding appear to require more than what seems possible to provide, and reasonable to expect, from participant observation. At least this emphasizes that

whatever is concluded is done *by* the researchers, not by the researched, although the aim of the phenomenological approach was to understand and base their theories on *native* constructs. Instead the *stravis* researchers are thrown back at their own presuppositions and preconceptions, all “threats to validity” produced by the division of labour itself between the observer and the observed.

If even interpretations made by pure, unbiased, and uncontaminated observers say more about the observers, than they do about the observed, then privileged “points of view” seem hard to defend as starting points for gaining access to “the things” studied. What interpretive framework to use no longer depends on what “fits the facts” either. There are strict, logical reasons for concluding that the same observations may be explained and interpreted within an unlimited number of frameworks, since any number of false premises may produce true conclusions. “This whale is warm-blooded” is a true conclusion that is correctly deduced from the false major premise that “all *fishes* (Xs) are warm-blooded”, and from the false minor premise that “this whale is a *fish* (or, an X)”.³

Once more, the gates seem to open up to all kinds of motives or justifications for *choosing* one explanatory framework before another, strategic, political, religious, ethical, pragmatically useful, selfish, emotional, etc. I have my theories, motives, and justifications, while you have yours, and those who happen to share similar preferences, world-views, and causes join the same gangs! But we also seem to be approaching a common terminal station for the trajectories based on the separation of researcher and researched, the ultimate, and generalised “othering-station”. At this terminal, then, all the separated researchers seem to agree in an indifferent, “hen-tropic”, and “caco-phóney” theory-pluralism among an unlimited, and stalemated series of “free”, equal, and reciprocal observers, using theories too as useful instruments, chosen for any preferred purpose.

3 Cf. Eikeland (1985b). To find the right middle term (fish, mammal, or some other X) was Aristotle’s great challenge. If there is no right one, anything goes. This is also why neither *induction* (neither the plebeian, nor aristocratic one of Larry Laudan (1981), nor Peirce’s *abduction* (1868 and 1878) will work in a non-arbitrary way.

8.3.4 *Watch Yourself! Or, Who's a Native?*

In turning the attention away from the observed, towards the observers, at least the researchers regain colour, and become visible. There are no rock bottom, uncultured, and theory-free “data”. But neither can researchers pretend to be colourless, transparent, invisible, and unobtrusive (unbiased, neutral) media for the perspectives of any others, observed or interviewed. Instead, we *all* become colourful, opaque, situated, and prejudiced observers, no matter from what distance we observe each other. All perspectives and aspects are different but equally valid in this absolution of the scientific observer. Anything goes! Such equality in difference invites absolute indifference too, however.

To transcend the terminal state of indifferent theory-pluralism, and let the *napeco* paradigm emerge as interpretive horizon, we have to realize two things. *First*, deep-seated prejudices, basic assumptions, preconceptions, and presuppositions brought along to every observation as “instruments of perception”, are not *chosen*. Modern theories, and other “head-stuff”, are like superficial opinions, *words*, easy to remove, change, or exchange. But prejudices, etc. are not like declarations. They are not merely “espoused theories” (Argyris and Schön; 1978), or theories at all. They are subconscious and tacit, merged with or submerged in our practices and routines, in our acquired experience, and in the gradual, constitutional formation of our knower-subjectivities, stored in deep layers of emotional and habitual reactive-perceptive patterns. Changing *them* is not like changing a theory or opinion. What we see and understand *through*, both as knowers-researchers and as natives, is our acquired *habitus* (Bourdieu, 1972; 1980), inscribed and cast in historical forms, institutional divisions, and practical categories.

Secondly, all observers do *more* than merely observe each other. If we only see them *observing*, the turn “inward”, towards the observers, may come to a superficial halt in the endless row of subjective meanings, partial aspects, and perspectives. But more than observation becomes common if we move beyond the merely observational stance, and ask questions about the formation of our presuppositions, basic assumptions, preconceptions, and prejudices as observers, in line with the *transcendental*, or “Copernican”, turn of Immanuel Kant (1724–1804). We may even ask, in line with the *existential* turn of Martin Heidegger

(1889–1976) and Ludwig Wittgenstein (1889–1951), what the necessary preconditions are for the complex performative and communicative competence implied in our everyday native practices. Then we can use the turn consciously to transcend the stalemated and indifferent cacophony of theory-pluralism. What do we as actors always already have to know and do in order to do what we actually do, in our sophisticated everyday practices? This is what the phenomenological researchers were looking for. But now we change positions. The natives are no longer “the others”. *We* have ourselves become the natives. The basis for our interpretations is no longer merely what we *see* the others do, or our recordings of what the others tell us on the basis of *their* acquired experience. The relevant “empirical” experience transforms from hard-to-get, illusionary “data” *about* the others, accessed at validity’s peril, to our own acquired, personal experience, in a sense always already accessed. The ways and means of doing, or not doing things, accumulated in the personal experience and competence (*habitus*) of any one of us as practitioner-knower-researcher, adjusted to the realities of lived practical experience, becomes the entrance door to more adequate knowledge, and to more adequate concepts of knowledge. We are all natives, and the natives’ competence no longer simply skews, biases, and distorts cognition in ways to be eliminated and controlled for. It is a *sine qua non* of any cognition.

Our witnesses to this transforming move beyond observation will be Devereux (1967), Hammersley and Atkinson (1987), and Hastrup (1995). They work from within a *stravis* paradigm. But in the determination of fieldwork as no longer modelled on unbiased, and unobtrusive observation, they represent transitional figures towards Action Research, from within an interpretive, stranger-visitor approach. Devereux, a psychoanalyst-anthropologist, turns his critical gaze at himself, and at other researchers, as participant observers, using psychoanalytical concepts like “transference” and “counter-transference” in order to understand the relationship between observer and observed (1967:41–42f.). His reflections focus on how technical research methods can be used as psychological defences against anxiety in the researchers, and how untreated psychological traumas and neuroses in the researchers work as projections and counter-transference towards what is observed. As with analysts in Psychoanalysis, the prejudices can be controlled, according

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to Devereux, through a compulsory apprentice-didactic Psychoanalysis. Basically, however, *any* unattended difference between the researcher and the researched, or rather between *any* two people, may be projected or transferred as a distorting interpretive framework (both ways). But apprenticeship is retained as a necessary way of getting through the jungle of projections.

Hammersley and Atkinson (1987), while sticking strictly to the distinction between researchers and researched, have stopped trying to eliminate reactivity, i.e. the effects on the researched of researchers interacting with the field. Instead, they recommend using reactivity and researcher effects as sources of information, aligning, in spite of themselves, the researchers with the researched in their mutual affective relationships, since this interactive reactivity is outside any controlled research method. And what the researchers can do, the natives can do too. Researchers are natives, and *vice versa*. Hastrup (1995) has consciously abandoned trying to assimilate what goes on during an anthropologist's fieldwork to any models of observation, recognizing fully the weaknesses inherent in observation, and purging the *stravis* paradigm of any *spectas* remains. Instead, she uses her own participant *experience*, no longer reducible to observation or technical "data", as a starting point for theorizing. But her participant experience is hardly different in kind from other participants' experience, once more equalizing the researchers and the researched. As native-novice she becomes an apprentice, but under conditions hardly adjusted to learning. Instead of the assumed "culture-adequate" and culturally specific results of observation, the knowledge-generating process as such, based in gradually acquired personal experience and emerging insights, moves to the centre of her attention.

But still, none of these authors write much about *how* their "self-study" proceeds, although it clearly cannot be merely by conventional empirical methods. The methods of their methodological studies are not presented in any detail. Studying themselves, not as imprisoned in private and subjective selves, or in language, but as the way to get to know human life and social realities more object-adequately, adjusted to the nature of the studied object at hand, i.e. "objectively", their primary methods can no longer be observing, interviewing, or experimenting *on* the others. Instead, they have to "theorize *through* practice" (Zalewski;

2000), as methodologists and practitioner researchers do. What they, as natives, or as researching practitioners, have to adjust their practical conduct to, i.e. whatever they must take into consideration in doing what they do without failing or stumbling, and thereby accumulate as normative experience, *is* the social reality sought for. If not, why heed it, why adjust to it, why care about it, as we do? The only trouble is that these adjustments are often done tacitly and subconsciously. So, how can they be grasped and articulated? What does “theorizing-through-practice” mean? How is it done? At least it must mean remembering that we are all natives, and that we have to start wherever we are.

For natives, or native-apprentices, the old, Delphic-Socratic “know thyself” emerges as relevant, and unavoidable. Once we realize that what is to be recollected in the analysis of our selves, are not primarily private fears, inwards-looking, and empty emotional subjectivities, reminiscences of former lives, or a separate world of ideas, Socratic midwifery and recollection work becomes relevant as a modern, empirical research method (cf. Eikeland; 1998b). Recollection concerns conscious and subconscious “memories” and tacit knowing, the collection and integration of which constitutes experience. In recollection work we do not study others in analogy to external objects moving and changing, with hypothesized inner, subjective wills and motives. We study our own *habitus*; i.e. “ways-of-doing-things” – “what-does-it-mean-to-do-x-or-be-y?” – and what is implied in these different ways, through critical examinations of our own native-performer experience and competence, their adequacy or inadequacy. Prejudices and presuppositions are not simply chosen, but formed experientially through interaction and practice. We, the natives, are not atoms that remain self-identical if abstracted from history, circumstances, past personal experiences, and hopes and expectations for the future. We carry them all in our selves, individually and collectively, as our socially formed selves. If we critically inspect our own prejudices, presuppositions, and practices, then, and what these in fact take into consideration and are adjusted to, the outcome will not be merely “subjectively chosen”, private, theoretical frameworks, but historically and culturally – practically – formed conceptions, or even, occasionally, “object-adequate” skills as prejudices – ways-of-doing-things that really work – among expert or virtuoso performers, i.e. what science has always been hunting for.

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What emerges through recollection, then, are things we have in common *qua* certain kinds of practitioners. For detached, non-participants, observing from a distance, language, social skills, norms, traditions, power relations, inner wills and motives, even organisations, and similar “entities” are invisible and intangible, not something necessarily taken into consideration in order to perform their observations. Native practitioners, on the other hand, must heed and handle entities as these practically, in order to manage at all, although not always competently. In order to manage practically, we must know the language, power relations, and traditions, and how to deal with different kinds of situations and people. But “the lens” of mainstream research only lets us “see” the “brute facts” of externally observable events, as Salner (1989:49) formulates it. Anything “inside”, or “between”, things occupying a physical space, is invisible.

Social researchers not immersed as participants may either operate with extraneous mathematical models, or with conjectured subjective meanings and motives, ascribed to observable individuals in order to prevent their theoretical reduction to mechanical, machine-like objects. But language, norms, social skills, traditions, power relations etc. remain “theoretical constructs”, and projecting specific “wills” and “motives” remains a moral obligation, not a practical necessity. For native practitioners, however, they are experiential reality, common, hard-hitting “powers” in their lives, which they have to handle, or fail. Even without knowing them, they *feel* them. To experience their reality, we must participate as native practitioners. Hence, if there is a *privileged* position here, it is the native’s. What we as natives have in common, are habits, standards of competence, language, tradition, norms, and ways-of-doing-things. These are the bases for the “common meanings” of Charles Taylor (1971), unattainable by anyone extraneous to the collective as anything but conjectures and hypotheses.

Research based on the separation and difference between researchers and the researched, recognizes and appeals to certain forms of evidence and “data”. It ignores, and renders suspect the practical experience of native performers, readers, thinkers, speakers, and writers, or, it is unable to attain to native experiences because it doesn’t analyse *its own* nativeness, i.e. the prejudices, etc. of its own *habitus*. But nativeness cannot be eliminated from research. Research must go *through* native experience.

Hence, the validation and use of practical, personal experience must move from periphery to centre of any research process, transforming it in the movement. The “whats” and “hows” of this process *is* Action Research. Hence, a critical review of validity challenges in mainstream research points to a strong Action Research, tested by developing answers to validity challenges of mainstream approaches. Validity *requires* an Action Research where action and research are united in a reflective native approach studying what-it-means-to-be-and-act-as-a-native, what is implied and presupposed in ways-of-doing-things the native-performative way. But can current shades of “really extant” Action Research (REAR) – more often than not performed as unmediated alternatives in segregated and fortified opposition to mainstream research – live up to the standards of an Action Research emerging in and from “enemy country”, developed and tested “in battle”?

8.4 “Really Extant” Action Research – various validities in varieties of REAR

Since the seventies, Action Research has become a diverse, and world-wide community, documented in many publications.⁴ How does it relate to the content of the preceding paragraphs? What Kvale (1989:73) says about qualitative social research could be said about Action Research as well:

It has been an exception rather than the rule, that a qualitative research report includes a discussion of the reliability and validity of the results. If such concepts are mentioned, it may be to dismiss them as positivist reifications.

Validity has not been a central concern in the REAR communities. In a sense, Gustavsen (1986:152f. & 1988:234f.), central in Scandinavian Action Research over the last 35 years, has “dismissed” the discussion by

⁴ See e.g. O’Hanlon (ed.) (1996), Toulmin & Gustavsen (eds.) (1997), Hollingsworth (ed.) (1997), McTaggart (ed.) (1997), Reason & Bradbury (eds.) (2001), Winter & Munn-Giddings (2001), Day et al. (eds.) (2002). These collections mostly represent what I call “second wave AR” below.

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pointing out that Action Research may not have high validity, neither internally nor externally, but mainstream research does not really fare much better, and at least Action Research is relevant for practitioners and develops theory democratically. But even if this were true, leaving it at that would be insufficient. Neither Action Research nor validity can simply be reduced to democratic procedures.

Below, I outline some Action Research-approaches in order to clarify differences concealed behind the label of Action Research. Validity questions are not identical for all kinds. I mention several strands without discussing all. In approximately temporal succession, they are: 1) the re-location and transfer of experimentation, 2) feedback of results, 3) results and research expertise used in political activism, 4) researcher-practitioner collaboration in OD-work, 5) research useful for practitioners, 6) counter-cultures of indigenous knowing, and practitioner-research, and, finally, 7) immanent critique. These are not separate and mutually exclusive approaches, but shades of Action Research, often intermingled. No. 2, 3, and 5 are mentioned without elaboration in section 4.1. In section 4.2, no. 1, 6, and 7 are presented together. As an influential current of Action Research in Scandinavia, no. 4 receives a separate discussion in 4.3.

8.4.1 *Action Research as “applied research”*

In the 1940's and 1950's, survey feedback research was perceived by established social science in the USA as a break with mainstream attempts at keeping research activities segregated from society. It was launched as an early Action Research approach (Cf. French; 1985, French and Bell; 1990). As indicated by the designation, research-results are fed back to the researched for application or discussion. The research processes – data collection and data analysis – remain the conventional ones, however, not necessarily limited to surveys, but still based on a division of labour and separation of researchers and researched. Feedback research is a kind of “applied research”, applying mainstream research techniques for ulterior practical purposes. It doesn't raise specific validity questions beyond those concerning conventional mainstream methods.

From 1965 to 1980, many Scandinavians saw Action Research as putting “politically correct” themes on the research agenda, and as

the use of research results in political activism. Researchers considered themselves a kind of counter-expertise. No research could be neutral in its choice of questions to research. Most contemporary, mainstream research was conceived to be working as “servants of power” (Baritz; 1960). Hence, inversely, the legitimacy of using research as a tool serving underprivileged groups in society could not really be questioned. But this too is a kind of applied research, based on a division between facts and values, and between means and ends inherited from Max Weber (1904). Research techniques, as instruments, are considered neutral “facts” or means beyond value-disagreements⁵. This approach does not question methods any more than feedback research does. Neither does it raise specific validity questions beyond the mainstream approaches reviewed. “Research action”, or “researcher activism”, rather than “Action Research”, might be better designations for this, as well as for feedback research.

A diluted cumulative effect of these “researcher activism” variants is the now prevalent attitude both in Action Research communities and in broader circles, that, since it cannot be neutral, and since the “ivory tower” is crumbling anyway, research should be useful and relevant for practitioners. Research is evaluated in moral and political terms according to how immediately applicable it is, according to how easily it can be understood by practitioners, and according to how appealing it is to people who are primarily concerned with getting things done, i.e. who do not have the time and patience to elaborate too thoroughly. The legitimacy of the practitioners’ interests is usually not questioned. This attitude borrows legitimacy from the also prevalent attitude that “science” has been deconstructed with “positivism”, by the flooding of research validity by apparently insurmountable challenges referred to earlier sections, and that science has lost both its validity and its reliability.

8.4.2 *Action Research as radically different ways of doing research*

The approaches in 4.1 do not really question the validity of the what, how, and why of mainstream research methods. In a way they remain

⁵ Brox (1990) represents such an approach.

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naïve. Their criticism and indignation with established research is moral and political, directed at “wrong” applications of “neutral” instruments. The Action Research-variants below are related in that their opposition and critique towards established, mainstream research is not merely moral and political. It concerns the research instruments themselves. Research methods are not neutral, and Action Research is not merely using the same instruments for different, politically correct purposes.

8.4.2.1 First wave of Action Research

This is continuous with the birth of Action Research in the forties, through the double step of a) moving experimentation from the laboratories into the field, and, more radically, b) inviting the subjects of research to join the community of researchers in the primary interpretation of findings.⁶ Re-locating experimentation and expanding the community of inquiry is different from participant observation, interviewing, and conventional experimentation, although these may be parts. Transferring experimental research *from* insulated laboratories based on a division of labour between researchers and subjects (as objects), *to* experimentation in everyday settings in workplaces, communities, and organisations as communities of inquiry, transforms it.

In the transfer, the division of labour is suspended and principally overcome. This suspension transforms the experiment from technical manipulation and spectator theories, to a development of common and shared knowledge and competence, from using others as “guinea pigs”, to experimenting *together*. It does make experiments “uncontrollable”, but at the same time, it makes the objects known into co-researchers (knowers), departing from continued attempts at retaining control in the “quasi-experimental” tradition. Ultimately, then, this transformation changes the kind of knowledge sought for.

The first wave of Action Research did not break with the self-interpretation of mainstream research, however. It conceived of itself as a continuation and expansion of attitudes and practices of a unified science. It was simply “scientific”. For different reasons, this first wave of Action Research in the forties, and the fifties, petered out or was redirected as

⁶ As representatives of first wave AR, cf. e.g. Collier (1945), Lewin (1946), Chein, Cook and Harding (1948a) & (1948b), Lippitt (1949), Corey (1953), Shumsky (1958), Whyte and Hamilton (1964), and Marrow (1964).

organisational development-work (OD), program evaluation, and sensitivity training during the sixties (Sanford; 1970, Campbell; 1978).

8.4.2.2 Second wave of Action Research

The second wave of Action Research rose in the seventies as part of a politically radicalised and counter-cultural climate, both in industrialised and third world countries. It developed as parallel counter-cultures of “alternative”, “indigenous” knowledge and practitioner research, independently in many different places, and often without any clear continuity with or knowledge about the first wave of Action Research.

“Practitioner research”, “teacher research”, etc. belong in this counter-cultural second wave too, although these terms sometimes mean just practitioners using conventional research techniques. Islands of Action Research were parallel to each other but also to mainstream research, i.e. without really relating to, or interacting with mainstream research. But “counter-cultural” Action Research uses the results of science criticism to justify doing something different, and still call it research. Since mainstream research is “rotten”, we have to rid ourselves of it, and do something completely different.

Instead of working as spectator-astronomers, manipulator-users, or stranger-visitors, counter-cultural Action Research has mainly worked by gathering people in dialogue about their experience as professionals or as native members of different communities. Neither observing others, nor the manipulation of, nor the intervention into, their realities, but these experience-focused dialogues, as such, constitute the Action Research processes. Professional researchers participate fully in, and often facilitate these processes, and other practitioners participate as co-researchers, making the research process open and shared among all practitioner-participants, in line with the first wave break-through in the forties.

Hence, counter-cultural Action Research has preached and practised dialogue, but hardly with mainstream research, giving counter-cultural Action Research a somewhat “sectarian” image in the institutionalised research world. And some babies were undoubtedly thrown out with the dirty mainstream water.

8.4.2.3 Third way of Action Research

Finally, there is Action Research as immanent critique.⁷ This is a way of doing Action Research, but hardly a wave. The approach shares features with both first and second wave Action Research. With the first it shares an obligation to start from within mainstream research practices, and by insisting on a critical dialogue with *anyone* as the way forward, eventually transcending given starting points. It shares the obligation to experiment in real social settings with practitioners as members of peer groups of inquiry and interpretation. But it does not share any obligation to retain and defend the three conventional paradigms of social research (I, II, and III above) with their institutional arrangements.

The first wave had no real understanding of profoundly different approaches in a unified, still expansive, scientific movement. With the second wave the third way shares the obligation to develop “indigenous” knowledge *as* natives, and *with* natives. It also shares the participatory research practices. But it does not share its separatist inclinations opposing mainstream research. Mainstream social research is one of the most interesting native tribes, not because it is right, but because it is wrong in most interesting ways. Ways I have tried to indicate in the first part of this article. The third way does not share any obligation to defend and preserve, separate, substantial cultures uncritically as they are, if they obstruct the pursuit of its final, and strongest obligation towards some very old and traditional, higher ideals of intellectual activity and individual autonomy, searching for truth within open and critical groups of peers, whose community of practice is constituted primarily through this “trans-cultural” or “super-cultural” obligation and inquiry in itself. This highest obligation to “cultivation” is a precondition for diversity.

What makes immanent critique into Action Research is its insistence on thinking through personal practices of both researchers and natives, searching for patterns and inconsistencies within things said and done. It is obsessed with validity (cf. Lather; 1993), and this obsession pulls it ahead in all fields and directions. Immanent critique is not separate or different from any other position or practice. It unfolds and develops

⁷ Cf. Eikeland (1985), (1998a), (2001). See also Antonio (1981) on the concept of immanent critique.

inherent tendencies in any practice, position, or perspective, to the point where they transform themselves, and new patterns emerge and can be articulated. Hence, immanent critique is transforming Action Research, and transforming mainstream research, as well. This chapter tries to emulate this approach, by starting where it does, the way it does.

8.4.3 *Action Research as researchers and practitioners collaborating in OD work*

Still another approach must be presented and examined. It figures as an important “second-wave” current of Action Research, but there are significant differences that should be addressed. It contains the dominant tradition at the Work Research Institute (WRI) in Oslo, which has had a clear influence on ways of doing Action Research in Sweden and Denmark since around 1970. Since the second half of the sixties researchers at the WRI have assisted and written about OD projects in Norwegian work life, private and public.

Since the first half of the seventies, this activity has been called Action Research.⁸ The projects have mostly worked with participatory methods involving whole departments, or small and medium sized enterprises, to establish organisational structures allowing increased participation in daily work. But since I am not writing history, only certain aspects of one dominant WRI approach will be discussed here, concerning the relationship between the researchers and the field. My own work has also been based at the WRI since 1985, since 1987 with developing and building systematic learning capacities – permanently interlaced Action Research-spirals as learning systems – into organisations, using distinctions and alternations between a) “on-stage-performance” in “work organisations”, b) temporary “project organisations”, and c) “back-

⁸ In spite of this, it is one of the few in the second wave with continuity connecting it to the first wave of AR. Although the label AR was not used before the beginning of the seventies, Einar Thorsrud, the founder of the WRI, had personal contacts since the late fifties with Eric Trist, Fred Emery, and Philip Herbst at the Tavistock Institute, and with Chris Argyris, and Don Schön in the USA. Trist, Emery, and Herbst became important figures in Norwegian developments from the first half of the sixties.

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stage-reflections” in “development organisations”.⁹ A distinction with a similar origin between “development organisation” and “work organisation” is more widely diffused through the work of P.H. Engelstad, Ø. Paalshaugen, and B. Gustavsen. The similarity springs from the close collaboration from 1986 to 1990, between the two first mentioned, myself, and B. Bernhardsen, in an Action Research-project among car dealers and repair shops, where the distinction was first circulated (Cf. Eikeland, 1987; Paalshaugen, 1988). Practices appear similar, but interpretations differ. A basic nexus indicated by the concepts of “immanent critique” and “counter-public spheres”, exists between my own Action Research-work, and the strong justification suggested in the first part of this article. This nexus will emerge more clearly at the end.

8.4.3.1 Complementarity?

The dominant WRI-approach is based on a division of labour between researchers and field-participants, in a way recognizing these as different cultures and discourses. The division is construed as “complementary”, each party bringing contributions from different discourses into OD-work in organisations. According to this view, no one is privileged or “above” the other, although the researchers often speak of what they are doing as their “interventions” in the organisations of others in order to create changes. In the theoretical elaborations, however, the researchers remain principally distinct and separated, as complementary parts.¹⁰ But the model itself does not originate with the relations of researchers to a field of research different from themselves, inquiring into “the others”. Its origin lies in the model of a cooperative team in practical project work (Thorsrud; 1976), with a defined division of labour and complementary roles between specialists in different fields. Still, I don’t think it really challenges fundamental presumptions of modern institutionalised research.

⁹ See Eikeland and Berg (1997). The whole approach has clear similarities to the “free space” thinking presented in the article in this book by K. Aa. Nielsen and B. S. Nielsen (2006), mainly due to similar inspirational sources in Negt (1971) and Negt & Kluge (1972).

¹⁰ Gustavsen (1990), Engelstad (1995), Paalshaugen (1991). Greenwood and Levin (1998) emphasize “co-generative” learning between “natives” and external researchers in a democratic process, but do not use the term “complementary” to describe this.

Starting from and accepting a division of labour between “researchers” and “practitioners”, then, this approach frequently raises questions about the role or contribution of research/ers in development work. The loci of the collaborative development work are never the researchers own work places or tasks, however, but the work places of “the others”. The researchers move out into the social realities of the others, not vice versa. The loci are where the field participants are natives, not the researchers. Hence, I will keep writing about “the natives” in this context.

The “role of research” is normally described as bringing in research perspectives to assist the development processes. But this role does not usually or necessarily mean doing tasks specific to any distinct and identifiable research process. Hence, the basis for the “researchers’ perspectives” is unclear, since conventional research is constantly challenged verbally. The research/ers’ role usually means anything individuals with formal positions as researchers might contribute to a project with practical objectives, often tasks hard to separate from what management consultants normally do, as e.g. planning and leading a conference or seminar, project management of some kind, taking notes and writing summaries, teaching, “applying” theoretical or empirical research results, or even far more menial tasks. This appears to be what being “action oriented” means, contributing to getting things done.

But the details of the research processes as such, e.g. of “data-collection”, “data-analysis”, “theory-development”, or of whatever else might identify research as a kind of activity different from other activities, somehow get mysteriously lost, or they merge (are conflated, de-specified) completely with *anything* people designated as “researchers” might do, making them into researchers formally, but not necessarily *de facto* by doing specified research work. Research tends to be reduced to an intended “side-effect” of “interventions” to create changes in the reality of people the researchers visit and collaborate with, and of contributing to project and development work. But “intervening”, “creating changes”, and “getting things done” in a project – without anything in addition – hardly qualifies as research in itself, unless getting things done anywhere and in any way at all does. And the “research addition” cannot be merely systematic observation, questioning, or conventional experi-

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mentation, with pitfalls indicated in the first sections of this article.

Core Action Research competence is *not* equal to “getting things done”, *plus* systematic observation, questioning, and experimentation. Turning the gaze of “close-up” spectators more intensely towards the “practice” of others is *not* equal to an epistemological “turn to practice”. It is important to keep in mind that A) doing whatever is required for the purpose of acquiring knowledge and understanding, is different from B) doing whatever is required for the purpose of changing a social system or individuals, no matter how much we may “have to change it in order to understand it”. A) and B) may overlap. A) may require changing something and somebody, and it certainly requires practical experience, as the previous sections of this article has tried to show. B) may require learning and searching for new knowledge related to intended changes.

But the ultimate objectives of the two are different, making the practical requirements different too in spite of accidental overlaps, since they move at different rhythms and velocities, go through different stages, and search for different ends. Research methods are not automatically methods for making changes, nor are methods of change necessarily research methods. A) changes things *in order to* understand. B) understands as much as necessary *in order to* change something. So, is Action Research an A)-kind of method, or a B)-kind? The WRI tradition usually thinks of it as a B)-kind. Hence, they do things *with* others, but do they do *research with* the others?

In this approach researchers participate in development processes among “the natives”, where researchers “intervene”. The planned change and development processes are not among the researchers. The natives are only to a quite limited degree invited into the research-processes of the researchers. The differences and the separation of cultures and discourses between the natives and the researchers are maintained. The research processes remain mostly closed country (black box), even to other researchers. Exactly such an invitation to the natives to take part in the specific research processes – opening them up – was what launched Action Research in the forties, however. Later on, in the counter-cultural second wave of Action Research, collaboration in conscientization-work, specifically, has been central. What distinguished Action Research at the start, and made it “revolutionary”, was not col-

laboration, or broad participation, around any project objectives whatever, where “researchers” participate as complementary team specialists. Within second-wave Action Research, researchers have acted as facilitators in groups *sharing* research and learning processes, as presumed experts in such processes, where dialogue has become a central way of working.

The WRI-approach has also been promoted as a *dialogical* approach since the middle of the eighties (Gustavsen; 1984 & 1985). But “dialogue” is hardly distinguished from other ways of conversing like e.g. deliberation or negotiation. The main “dialogue” is not between researchers and field participants joining hands in processes with the clear and specific purpose of learning or research. In the WRI-tradition the researchers are mostly organisers, administrators, and instructors of conversational processes, “restructuring” the discourses in organisations on behalf of the native others, processes the others – the natives – go through, not the researchers. In addition, these conversational processes, mostly arranged as so-called “Dialogue Conferences” *presumed* to be dialogical, since the researchers mostly do not participate in the group work, are not primarily research processes of the A)-kind, but mainly preliminary steps in designing practical development projects, securing ownership and support for processes and results. Project planning, designing participation, is also said to be done “in dialogue” with representatives of employers and employees in organisations. But when any kind of conversation for any purpose, performed in any way, becomes “dialogue”, and anything done is “Action Research”, important differences disappear, and without differences, understanding is hard. Indifference rules, and everything is equally valid, or invalid.

In order to understand, it is important to keep in mind, that just like the concept of complementarity, the concept of dialogue promoted by the WRI-approach does not, like many other concepts of dialogue, have its origin primarily in the critical elaboration and development of specific research or learning processes. Most other concepts of dialogue currently in circulation, not merely colloquial, get their inspiration either from ancient dialogical philosophy in Plato (428–347 BC) and Aristotle (384–322 BC), from Martin Buber (1878–1965), from Karl-Otto Apel (1922–) and Jürgen Habermas (1929–), or from David Bohm (1917–1992). But the primary, and practical, origin of the WRI concept of dia-

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logue lies with processes of *negotiation* between employers associations and workers unions (Cf. Engelstad, 1983; Engelstad and Gustavsen, 1983). These parleys, established in Norwegian work life since the thirties, are what many projects in Norway during the seventies tried to expand and develop to include collaboration in organizational development. In 1982 they finally succeeded in getting the expansion included in general agreements between the major work life organisations (LO and NAF/NHO). These expanded negotiations and the resulting compromises about specific steps of action or general arrangements, then, are what were attempted infused with Habermasian concepts of “ideal communication” in the eighties (Gustavsen; 1984, 1985, 1987 & 1990).

During the nineties the division of labour within this current of Action Research has been described as going between process and content in development processes, researchers designing the processes, e.g. of Dialogue Conferences, while the field participants – the real conference participants – provide the content from their work experiences. This relationship is called complementary. But it is hardly an ordinary complementary relationship. What does complementarity mean? As in a jigsaw puzzle or in a machine, each complementary part fills in the gaps, or completes the lacunas of the others by providing what the others lack reciprocally, i.e. producing a kind of whole together (*com-plere* /-plenus = *full, or whole together*). But even if complementary parts were fluid and flexible, continuously adapting to changes in the others, each part would remain what it is as part without merging with the others or gradually becoming identical to them. Merging or identical parts are hardly complementary. Complementary parts retain separate tasks and roles, separate functions. In many ways complementarity freezes the relationships in divisions of labour between producers and consumers, between merchants and customers, between tasks in work organisations, or generally between roles in social systems. Together they make up a whole society, work organisation, or whatever. But what kind of whole or totality is this relationship between researchers and field-practitioners part of?

8.4.3.2 Masters and Apprentices Going the Same Way

The relationship between thinking and acting is not like other technical

or social divisions of labour between farmers, shoemakers, tailors, etc. The relationship between research and practice is hardly complementary any more than thinking and acting is. Thinking without acting is empty. Acting without thinking is blind. To the extent that this division has become part of historically entrenched social divisions of labour, its legitimacy is highly questionable. As indicated, such a division of labour allocating thinking and reflecting to certain individuals or groups, and mere execution to others, contributes to a host of difficulties in the production, transfer, and communication of knowledge, and ultimately it contributes to the invalidation of knowledge. It is *the* main problem within mainstream research, trying to assimilate “the others” as subjects of study, to objects or to instruments. This division is one of the major difficulties with many models of organisations and societies when it is not a temporary and technical part of a system of rotation, but made permanent as part of a social and structural division of labour. The complementarity model reproduces this without challenging it. Hence, a different, and I think more fertile way of construing the relations between research and performance in different social fields, which takes account of the practical revolutions made by Action Research, and the differences in competence, is to compare it to a master-apprentice, rather than to a complementary producer-receiver relationship¹¹. Who, then, are masters, and who the apprentices?

We met an apprenticeship model in the *stravis* paradigm above. Validity considerations “forced” researchers to participate and observe, not as unobtrusive “astronomers”, but as apprentices in cultural practices. Devereux (1967) gave the model prominence as a way of learning to pass through the impasses of mutually transferred projections between spectators. This time, field participants are not external moving objects to be described, predicted, and explained, nor are they material to be formed and changed, or instruments to be used, nor are they strangers to visit. Nor are they specialised partners in a team with partial, complementary roles, executing the “meta-designs” of “researchers”. If theory and practice, thinking and doing, reflecting and performing, are to be united, the same people must participate in both, as in alternat-

¹¹ On apprenticeship see Coy (ed.) (1989), Rogoff (1990), Lave and Wenger (1991), Caldwell and Carter (eds.) (1993), Ainley and Rainbird (eds.) (1999), Nielsen and Kvale (eds.) (1999) & (2003).

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ing – cyclic – phases and levels of the same process. This is what the well-known Action Research-spirals are all about, alternating between reflecting and acting within open, experimenting, collaborative, research processes. Masters and apprentices must go through the same processes over and over, together, bringing them all closer to mastery through practice and a learning, inquiring dialogue.

In contrast to the complementarity model, the master-apprentice relationship is a dynamic learning relationship based on full sharing, because it is designed to make a master of the apprentice. In building capacity for organisational learning in organisations, this is the objective, to transfer and re-locate research and learning skills to the others, but not merely didactically. The apprentices must be fully initiated into the “secret” tricks of the trade, although through several stages and levels. There cannot be a permanent division of labour. An apprentice does not have a partial role. S/he is on the way to mastery, as is the master. A didactic, class-room, teacher-student relationship *is* complementary in a certain sense, since, there, the student is not on his way to become a teacher. A didactic teacher is not primarily a practical role model for the student, unless the student is becoming a teacher. Hence, what these teachers convey is “pure theory” separated from experience, to be received *as* theory by listening students. Didactic teaching is based on listening and remembering, not on imitation, experimentation, and practical guidance. In apprenticeship the master is a practical role model. Although masters might teach systematically, instruct, and “give orders”, most exchanges have to be more dialogical, based on questions and answers explicating what is going on and being done, how and why, here-and-now, *in practice*¹².

Masters and apprentices are not complementary. They share common standards for what they are doing, striving towards the same to attain them. Their performances and skills are at different distances in different directions from realizing them, masters closer, apprentices farther away. Where you are, practically, in relation to standards of performance and to the ability to articulate this, decides whether you are a master or an apprentice, not formal positions, titles, or distinctions. The

¹² As a normative standard for learning relationships, apprenticeship implies more than the “undesigned” legitimate peripheral participation (LPP) and “learning-by-hanging-around” of Lave and Wenger (1991).

master is the servant of the other's learning. If an unequal relationship is petrified as part of a social structure, it becomes conservative. But apprenticeship is not necessarily part of an unchanging, hierarchical, social structure. *Qua* learning relationship it cannot be, since formally locked positions of authority and subordination are detrimental to the "masterly" autonomy to be learned. Hence, the core learning relationship comes more appropriately to its own when liberated from fixations to social structure and status.

Both the dynamism and the commonality within it are emphasized when underscoring that the role of master and apprentice is not formally determined nor permanently allocated between participants. It changes and alternates continuously. It rotates, increasingly as advanced levels are attained. All involved in the relationship are on their way, at different places along the same way, changing, moving, and transforming, approaching the same standards of performance. In such a liberated apprenticeship everybody's prejudices are on trial all the time, through a searching and inquiring dialogue, as Plato's Socrates – a master, if anyone is – pointed out a long time ago.

But this kind of critical dialogue for learning and research is *not* the same as negotiating and compromising on coordinated steps of action, or on general agreements on rules of conduct. Neither does a complementary relationship have this transformative dynamism of mutual criticism between individuals moving forward along, and in, the same way. Complementarity stifles the relationship between the different parts.

In the *stravis* paradigm, the researcher is the apprentice (at best), not merely an extraneous, close-up observer. In the *napeco* paradigm, those who know the most and best in practice, or those who provide the better arguments, are "masters". Authority is not predetermined. But most research practices are still forced to work within given institutional divisions of labour. Could professional researchers be "masters", then, while the natives are apprentices, without reproducing the pitfalls of separate roles traversed above, and of didactic, top down instruction? They could, in the research processes (the dialogical commons), and in all the difficulties of methodology, i.e. in the methods of methodology. These are things all practitioners observing and categorizing need to know in order to act competently. And, both masters and apprentices are members of the same community of inquiry. Since not even masters

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are perfect, the common way of progress constitutes the community, i.e. they all have the learning and inquiring way of relating to their own practices and to each other, in common. An apprentice is a trainee, and we are all permanently apprentices, but alternate as masters. The common way of developing an emerging mastery *is* the real community. Hence, a group of “masters” and “apprentices”, released from being part of a social structure, is a group of peers, and a community of inquiry (Torbert, 1976).

Participants may have complementary fields of activity and expertise, substantially different. In the way of researching and inquiring into these complementary experiences – its specific processes and activities – they are still similar or alike. Experts in processes of inquiry are masters some times, having a greater knowledge and awareness of pitfalls of methodology, while experts in substantial fields of activity are masters at other times, carrying in their own embodied experience and *habitus* the criteria for deciding the appropriateness of emerging conceptualisations. The merging of research processes with practices, and the open sharing among participants in the inquiry, is what constituted Action Research in the forties. The same fusion is needed to overcome the validity challenges of mainstream research. The aim of becoming like the other in certain respects (skills, knowledge, and understanding) is what constitutes apprenticeship. Validity needs a researcher to learn what a native knows by becoming a reflective *native*. It also needs a native to learn what a researcher knows by becoming a *reflective* native. Native researchers are what learning organisations need, and it is also what has to be preserved in order for the core of Action Research to unfold. If anything or anyone in this is complementary, it is a merging complementarity in total flux.

But are dialogical apprenticeship-approaches *interventions*? A municipality read my book, and invited me. I provided supervision-through-dialogue in order for them to understand better, and to develop ways of cultivating “learning by doing” more systematically in their organisations. This was not intervention. It was not administering technical treatment as causes, to get calculated, artificial changes as effects. If what I provide generates improved understanding, this produces development. Principally, this is more similar to a therapeutic non-intervention against subjects, protecting them against interferences and interventions

from obstructing surroundings and extraneous influences. Extraneous obstructions and influences intervene as efficient causes. Bringing my own practices to consciousness, making me *see* things previously invisible, in order to perfect them, does not. Unfolding developments and emerging patterns differ from interventions. They approximate the Aristotelian relationship between *praxis* (doing it), and *eupraxía* (doing it well). A sprouting bud or blooming flower does not intervene into its former way of being. It fulfils it. Stopping the bud is intervention! Masters provide practical forms as elucidations of a model. Apprentices approximate, and train themselves by imitation, experimentation, dialogue, and supervision, into the same form or pattern, *not* identical to a particular master, of course. The form or pattern of a *common* standard – “die Sache”, “saken”, or the “what-it-means-to-do-or-be-something” – is separable in thinking, and, as such, separate from any individual master. It is common to and shared by masters and apprentices. When apprentices apprehend it, they develop into masters autonomously, without interventions.

8.5 Immanent critique and dialogical validity – Exposing threats to validity

Earlier, I claimed a basic nexus between the strong justification for Action Research presented, and a certain Action Research-practice of alternating between “on-stage-*performance*” in “work organisations”, and “back-stage-*reflections*” in “development organisations”, indicated by the concepts of “immanent critique” and “counter-public spheres”. I will try to clarify and conclude.

8.5.1 *Counter-public spheres and immanent critique*

Immanent critique generates development and transformation by exposing inner insufficiencies, tensions, and contradictions – logical and pragmatic – in the practices of positions, paradigms, cultures, discourse formations, etc. in relation to their own proper objectives, also carried internally, tacitly or explicitly. It tries to bring thought, speech, and act

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into mutual accord. As an activity, immanent critique needs, not just a *public* sphere in organisations as indicated by Paalshaugen (2002), but a *counter-public* sphere.¹³

Although there are obvious barriers (psychological, social, cultural etc.), different in size and quality for people in different positions, a democratic public sphere is *in principle* open, and free for all to join the way they like. But all kinds of openness and freedom are not equally conducive when it comes to inquiry into and exposure of internal insufficiencies etc. nor equally conducive to learning and development. Exposure and speech, and even silence, can be confused, oppressive, abusive, abrupt, seductive, concealing, manipulative, strategic, injurious, gossipy, formalistic, etc. A democratic public sphere must allow for this, and rhetoric is its art of mastery. But openness uncovers, and leaves unprotected, vulnerable layers of experience, emotions, and practices.

A counter-public sphere must, of course, be open for all kinds of utterances emerging from and expressing personal experiences. But it must simultaneously somehow protect individuals against “power-talk”. “Power-talk” and talk that obscures are often unavoidable and necessary starting points. But they must be gradually and as far as possible prevented, suspended, and eliminated in order to aid the articulation of experience. So must psychological, social, and other barriers preventing access and participation. Open, critical, and constructive dialogue is necessary. It must gradually suspend, not difference, disagreement, or conflict, but the kinds of talk and oppressive silence just mentioned, and the barriers against participating and contributing as well. The art of doing this must be learned, by doing, i.e. by practising.

Creating a public sphere requires mainly the removal of restrictions. Creating a counter-public sphere requires the acquisition of skills in exposing and deconstructing power-talk, obscure talk, and the barriers preventing individuals from taking part. Hence, a public sphere can be established by decree backed by power, a counter-public sphere cannot. It establishes itself by critically exposing power-talk and obscure talk, and by creating protective back-stage spaces where critical reflection and immanent critique is allowed. When personal and collective learn-

¹³ Paalshaugen (2002) is a re-interpretation of “development organisations”. This re-interpretation really brings the concept back to square one. Cf. the call for permanent, local public spheres in Eikeland (1985a).

ing and inquiry is the objective, power-talk is exposed and suspended. Immanent critique and counter-public spheres are two sides of the same coin, and constitutive for peer groups of inquiry among masters and apprentices.

Above, I have tried to promote critical development and transformation towards valid social knowledge, by exposing insufficiencies, etc. both in mainstream research methods, and in some really extant Action Research approaches. The two trajectories – transformed mainstream research, and a mildly transformed Action Research – converge towards what I think is a conceptually and practically strong – valid – form of “mainstreamed” Action Research. The spectator-astronomer (*spectas*) paradigm, the manipulator-user (*manipus*) paradigm, and the stranger-visitor (*stravis*) paradigm are all insufficient, with serious inherent invalidities. Although we are all spectators, manipulators, users, strangers, or visitors in relation to others some times during our lives, and these ways of knowing are unavoidable, we all carry with us preconditions from elsewhere in doing these things competently, preconditions which – for a number of historical reasons – mainstream social research has been very good at neglecting. This “somewhere else” is our native-ness – the personal “me” and the “I” – which are not merely private or psychological, but historical, social, institutional, transcendental, etc. This “somewhere else” is where Action Research has to start, and the native-performer-community (*napeco*) paradigm emerges as the “always-already-there” of the other paradigms as well, when we start focusing critically on praxis – our own “ways-of-doing-things” – as the basis for understanding.

The Action Research-review ended up with a model of permanent apprenticeship, supporting and supported by the *napeco*-paradigm. We are all always already native apprentices in different national, social, and local tribes, and in professional tribes of psychometricians, anthropologists, Action Researchers, etc. When we start investigating and inquiring into them through immanent critique, they transform. The question is: What do natives need in order to develop their skills as reflective natives, practitioner researchers, and apprentices? Hardly more *spectas*, *manipus*, or *stravis* researchers! We need to organise our common ways of learning and inquiry, the Action Research cycles of reflection and action, the master-apprentice communities of practice and inquiry. The

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alternation between “on-stage-performance” in “work organisations”, and “back-stage-reflections” in “development organisations” is designed to organise – provide space for – such learning activities, and facilitate “learning-to-learn”. On stage, we perform our roles and appointed tasks. Back stage, we discuss and analyse critically experiences from performing on stage, we practice to improve, we switch roles and plays, etc. Thus, the Action Research-cycles shifting between reflecting and acting receive organisational form.

The space back stage must become a counter-public sphere, protecting people from rhetorical forces at work in the public sphere. As indicated, this must be done by consciously giving space to dialogue, where dialogical aspects of language use are distinguished from negotiating practical compromises, from persuasive and seductive rhetoric, from instructive didactics, from formal syllogisms, and from merely soothing and comforting speech.¹⁴ Critical dialogue makes the subconscious conscious, the submerged and merged emerge, the implicit explicit, the enfolded unfold, by bringing context into the explicit “text”, and keeping the dialogue focused on and springing from acquired personal, participant experience, articulating tacit presuppositions, preconceptions, prejudices, ways-of-doing-things, and basic assumptions of participants.

Focusing the dialogue both on the acquired practical experience (*empeiría/Erfahrung*), and on particular experiences (*páthos/Erlebnis*) of the participants, brings their *habitus* and its institutional inscriptions to the centre of attention. In this way we can also clarify the “back-pack” of mutual expectations we, as individuals “trapped” in institutionalised researcher roles, encounter “field practitioners” with. At first encounters, we are *never* primarily “personal” individuals, but group-role individuals representing cultures, institutions, or organisations, confronting each other through our own prejudices transferred and projected. We are not personal “I”s, but socialised and habituated “me”s. The *personal* encounter, where the habitual “me-roles” can be gradually “stepped out of”, made visible, and discussed, is for the “back-stage” space that must be created and secured as “development organisation”. In principle,

¹⁴ I have tried to clarify these distinctions in Eikeland in (1997a), (1998b), (2001a), and (forthcoming).

Action Research invites everyone to dialogue as equals “back stage”. Mainstream social research does not, but remains “off-stage”, as interpreting audience and spectators.

8.5.2 *Action Research, generalisation, and “Kontextaufhebung”*

Spectas, manipus, and stravis research de-contextualises knowledge by taking aspects of a situation out of it (abstracting), importing them into their theories and models. Thus re-contextualisation becomes a problem in the application and adjustment of theories and models in new situations. The practico-dialogical Action Research suggested here proceeds differently. It does not de-contextualise in the first place. It works by – what cannot easily be expressed in English, hence German must be used – *Kontextaufhebung*, i.e. by bringing elements of the context, hitherto not verbalised (tacit), into the explicit “text”, or dialogue, and thereby changing the situation (expanding it), but still keeping it intact (retaining, suspending, and transcending it). Instead of merely speaking and acting *in* the situation, defined by it, we speak *about* the same situation, transcending it and redefining it. This – *Kontextaufhebung* – is what bringing sub-conscious, tacit, implicit habits and skills into explicit, articulated, consciousness is all about. It is not just horizontal problem solving, but vertically raising consciousness without losing foothold on the ground. It’s like growing taller. You see more of the surrounding context than when you are smaller, more submerged, and sunk into wherever you have your feet. Immanent critique and “*Kontextaufhebung*” is Action Research because it is not a) spectator based observation, not b) experimentation *on* others as causally, culturally, or socially, determined objects, not c) interviewing anyone. It works dialogically and critically with personal, practically acquired experience, focusing on *habitus*. Like experimentation and Action Research, “*Kontextaufhebung*” *changes* its subject of study when it submerges itself into it, and makes all the preconceptions, assumptions, and prejudices explicit.

Spectas, manipus, and stravis research also have problems with generalisation, since they study the distribution of properties of the others as natural objects. By starting with our own native competencies and their preconditions – *habitus* – as human beings, as members of specific cultures, as members of professional groups, and representatives of institu-

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tions, etc., *napeco* research starts by searching for what we always already have in common, i.e. commonalities or generalities at work in what we do in particular times and places. In their competent forms and patterns, common “ways-of-doing-things” are simultaneously empirical, normative, and general. This is no paradox or impossibility from within an Aristotelian concept of experience.¹⁵ The point is to bring people to an awareness and consciousness of what generalities or universals are always already at work in their own practice and context as “basic historical concepts” (Koselleck, 2002, 2004).

8.5.3 *Dialogical opening*

As several writers emphasize (Cf. Merriam 1998:199), the most important rule for securing validity is making all practical moves, the component parts of what is done, their connections and presuppositions, and anything disturbing the generation of insight and understanding, visible, exposed and understood, bringing it all *into* the dialogue, *opening* the research process, or any process, up for inspection. Action Research was conceived by doing this in the forties. The *method of methodology*, i.e. the tribe of mainstream social researchers examining their own ways-of-doing-things, also contributes importantly to making the moves, presumptions, and preconditions of research processes visible. Hence, an important provision for securing validity is discussing and exposing all known, possible, and relevant threats to validity, a practice initiated by Francis Bacon’s (1561–1626) discussion of idols, and emphasized by e.g. Campbell (1988, pp. 264–265). Disclosing and revealing these things implies exposing and making conscious, increasingly wider contexts, bringing these into dialogical Action Research-spirals of learning.¹⁶

Some think of validity as *trustworthiness* and credibility of research results, i.e. how others can have confidence in research results (Cf. Merriam (1998:198f.), Guba and Lincoln (1989), and Mishler (1990:417)). But people trust widely different things. Many are too credulous. Therefore, there is a *dialectical* aspect to this, and a *rhetorical* aspect. The differences

15 Aristotle’s concept of experience is the theme in Eikeland (1997a)

16 Cf. Winter (1987), who has an excellent discussion of validity. Winter’s ultimate requirement appears to be that AR must include reflections on its own preconditions to achieve validity.

between persuading (rhetorically), convincing or proving (didactically, or demonstratively), and showing (dialogically) disappear in Mishler's (1990:420) conclusion that "validation is the social discourse through which trustworthiness is established". Validity cannot be reduced to just any such social discourse. Without the distinctions, it opens for a normative reading of Latour's (1987) "science in action", letting anything go, getting support by any means!

The primary purpose of an Action Research process is hardly to find out whether whatever is *said* also gets *done* either, as Paalshaugen (1991) implies, but rather to find out whether what actually gets *done* (or not done) by Action Researchers and others, is also what is *said*, not "covered up" in some way by language. In order to secure validity, research processes must be opened up, not covered up. It may be a problem for project work that people, for a number of reasons, do not always do what they say. But it certainly is a serious problem for research, and for the common understanding of human activity at all, that people more often than not, do not reveal and say what they actually do, but cover it up, or stash it up, in order to make it look nicer, prettier, more rational (rationalization), more politically correct, more innovative etc., than it is.

Hence, Action Research is normative, and requires *one* specific kind of change in social systems above all else: its own preconditions, the realization of the social and psychological pre-requisites and preconditions enabling people – without danger – to *observe, tell, understand, and change* what is *actually done* by *power, neighbours, colleagues, themselves*, or anyone, not only espoused declarations of values, but the details of *actual* practices and events, i.e. a critical counter-public sphere, and the possibility to learn individually and collectively. Building the capacity to systematically alternate between performing "on stage", and reflecting critically "back stage", may challenge organisations stifled by routines and habits, or led by power and rhetoric. But this is what research validity needs. It is what organisational learning needs. It is also what innovation and competitiveness requires. Its potential is enhanced by emerging "new modes of knowledge production" (Gibbons et al. 1994, Nowotny et al. 2001), and by emerging new relations between academic institutions for research and learning, and a knowledge-based work life.

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