Theory and History in the Human and Social Sciences

Martin Dege

Action Research and Critical Psychology

An Investigation of Subjectivity and Participation

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Theory and History in the Human and Social Sciences

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To my teachers.

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About the Author

Martin Dege joined the Pratt Institute in 2020. He has worked at the American University of Paris, the University of Potsdam, the University of Konstanz, the University of Hamburg, and Yale University in the past. Dege is the recipient of various research scholarships, among others, the Marie Curie Program of the EU, the Fritz Thyssen Foundation, and the German Academic Scholarship Foundation. Dege's research follows three strands. On the empirical level, he investigates how concepts of crisis shape our everyday lives and the narratives we deploy to make meaning of the world and ourselves. On the theoretical level, he is interested in the historical emergence of psychology as a discipline, more specifically, how various theoretical ideas have been intertwined with political interests and power struggles to form the discipline as it stands today. On the institutional level, he explores concepts of digital humanities and how digitalization changes both research and teaching.

Chapter 1
Introduction: What Is Action Research?

This book is concerned with a particular mindset for doing research. It looks at how intervention and change can be incorporated into the research process. While traditional social science research generally tries to collect data and analyze the results to ultimately plan specific actions that might bring about change in a particular field, *Action Research (AR)* combines these separated steps into one process. The aim is to understand the *change* from the perspective of the participants in the field. These participants are understood as experts rather than objects of the research process.

More concretely, in this book, I undertake a closer look at this research tradition and investigate how and under which historical circumstances it emerged. This book is, however, not solely a story of praise. While I share most of the intentions of the Action Research tradition as it is rendered here, a nonpartisan evaluation of Action Research must also contest that failure is a big part of the tradition, be it on the empirical or the theoretical level. To partially remedy this situation, this book also looks at yet another research strand, generally called practice research, or, more specifically, the *German-Scandinavian tradition of Critical Psychology*. Action Research and this particular branch of Critical Psychology share many of the theoretical goals, and on the empirical level, they share many strategies. Critical Psychology, however, provides a more coherent theoretical basis. It is within this framework that this book emerged; my goal is threefold: *provide a historical perspective of the emergence of Action Research*, *analyze its theoretical content*, and *offer a pathway to rethink and reshape its content to overcome pitfalls of the past*.

The Action Research tradition as it stands today is a multiple contested field. In the words of one of its main protagonists Bjøn Gustavsen:

> It was, for instance, argued that action research is not "scientific" since it is unable to keep the distance and neutrality in relation to reality which forms an essential part of the concept of "science." Others argued that it was "too scientific" in the sense that it was built on a naive belief in the ability of "science" to solve essentially political problems, such as overcoming worker alienation. From management quarters action research was often seen as "radical" – characterized by researchers running around in the workplace and undermining

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> management authority. From other researchers action research was seen as "conservative" since its work place orientation did not make it sufficiently macro-oriented and revolutionary. (Gustavsen, 1992, p. 31)

As Gustavsen indicates, defining Action Research is a rather tricky task. It is not simply a program of a particular political stream, be it left-wing, right-wing, liberal, or conservative. It is certainly not a specific branch of (positivist) science. Quite the opposite, it runs athwart these classic categorizations. A first approximation might be rendered like this: *Action Research is research that aims at knowledge, and it is activism that aims at change.* Moreover, it carries the belief that these two aims can be combined in a mutually beneficial process.

In their widely read Introduction to Action Research, Davydd Greenwood, and Morten Levin define Action Research as

> a research strategy that generates knowledge claims for the express purpose of taking action to promote social analysis and democratic social change. … AR aims to increase the ability of the involved community or organization members to control their destinies more effectively and to keep improving their capacity to do so within a more sustainable and just environment. (Greenwood & Levin, 2007, p. 5)

This definition points to several essential characteristics of the Action Research endeavor. Most importantly, it emphasizes the idea of democratic social change through research. At the same time, it mirrors the pragmatic stance of its authors, lacking any reference to an overarching theoretical stance that drives the research process. I want to argue that Action Research is not simply a "research strategy," as Greenwood and Levin claim. Instead, it is an endeavor on all fronts, spawning various methodological and theoretical developments that combine theory and practice (often denoted as praxis, pointing to its sources in German Critical Theory) to create transformative social research.

A closer look at the tradition of Action Research today ultimately renders Action Research an umbrella term that encapsulates several traditions. Two main strands can be isolated: the *Northern Tradition*, or Pragmatic Action Research, and the *Southern Tradition*, or Critical Action Research (Johansson & Lindhult, 2008). The two traditions have developed largely independently from each other and even created their textbooks traditions. For example, William Whyte's *Participatory Action Research* (1991) and Orlando Fals-Borda's *Action and Knowledge* (Fals Borda & Rahman, 1991) share as few as three references related to the practice of AR. On the theoretical level, however, the various approaches to Action Researchers share frequent references to the works of Kurt Lewin, Karl Marx, and Jürgen Habermas. No surprise then that the actual research practice also shows many similarities: Action Researchers understand every research field to be in constant flux.

In contrast to more traditional research strategies, examining specific conditions, attitudes, opinions, traits, etc., is not at the center of the investigation. Instead, AR aims to investigate the processes that create specific living conditions. To achieve this, Action Researchers frequently get actively involved in the field and try to work on specific solutions for existing problems together with the research participants. In contrast to participant observation, this participation is not – or at least not primarily – focused on the experience of the research participants. Instead, the

conditions people are subject to and their concrete problems lie at the heart of the research process. The goal is to carve out specific concerns, disaffections, problems, limits, etc., which apply to the research field. Researchers and participants try to develop roadmaps and programs for actions to overcome those limitations. It is, as such, a fundamental necessity for Action Research projects that the research participants are concerned with the research question – it needs to be *their problem*. Researchers actively collaborate with the participants (often dubbed *co-researchers*) to first understand specific living conditions and constraints to be then able to implement change. This change aims to improve the research participants' concrete living conditions and possibly others in the same context. Action Research is fundamentally concerned with change. It is an inherently normative project. It tries to provide resources for the research participants to collaboratively change their situation toward a subjectively felt and objectively visible improvement of their living conditions. This improvement is thought to be achieved by bridging the gap between theoretical insight and practical intervention within a democratic framework of researchers and participants. In the ideal case, researchers only initiate a specific process of change that is gradually adopted by the research participants and ultimately continues without any intervention from professional researchers. In brief: Action Research aims at *empowerment*.

The Action Research tradition is anchored in psychology. The German-American psychologist and early Gestaltist Kurt Lewin is generally identified as the founding father. However, many of Lewin's concepts originated with the Austrian-American physician, psychologist, and inventor of psychodrama, Jacob Levy Moreno. The social worker and Native American rights activist John Collier is yet another source of influence for Lewin's thought. Even though Kurt Lewin sent the first Action Research projects underway, his untimely death in 1947 brought a sudden end to the AR efforts in the United States. Thanks to Lewin's close ties to the Tavistock Institute in London, his research concepts were not forgotten but revived during the 1950s in Great Britain, and thanks to relations between the Tavistock and a group of Norwegian researchers, the tradition continued to grow in Scandinavia after economic resources to conduct research in Great Britain had run out, or funding was cut. Norway remained at the forefront of developments in the Northern Tradition of Action Research for many decades.

Throughout its development, AR has always been subject to criticism. Already toward the end of the 1970s, the German Action Researcher Heinz Moser raised questions about the actual content of the Action Research process and the dilemmas Action Researchers face when trying to implement change:

> Often, you can only find meager allusions to 'emancipation,' 'social change,' or 'pleas for humanity.' The 'becoming a subject' that is so often discussed in Action Research seems to be nothing more than an abstraction from concrete social circumstances in which the becoming of a subject actually occurs. (Moser, 1978, pp. 185, my translation)

Moser identifies a detachment from social reality and claims that Action Research is too often blind to the concrete historical dimension of society and its reproduction. For him, the action-evaluation process is too easily simplified to mere actions

in the here and now. Moser argues that an implicit inductivism follows that "pretends immediacy, neglecting that all our experiences are structured by specific expectations" (Moser, 1978, p. 187). This line of critique culminates in the claim that Action Research – besides the many references to Neo-Marxism, Critical Theory in the vein of Jürgen Habermas, Communitarianism, and later also postmodernism – lacks an appropriate theoretical grounding (Kapoor, 2002; Markard, 1991). Consequently, action researchers are accused of being unable to justify their decisions for and against specific actions in the field. Instead, it appears that action research too often follows implicit ideologies that reduce the evaluative process to a pragmatic reconciliation of interests, a critique raised by a prominent reference in the field, namely, Jürgen Habermas himself (Habermas, 1978). Habermas contests that researchers' implicit ideology creates a gap of knowledge between the researchers and the research participants. Accordingly, in both the action and the evaluation phase of the research project, participants must rely on the researchers' judgments and are constrained in their efforts. Following this critique, researchers seem to more or less automatically take the leading role in the research project and effectively impede the generation of shared knowledge between researchers and researched. Hence, the subjects in the field cannot use research outcomes on their own, let alone continue the research process by themselves but rely heavily on the researchers to carry out new actions or at least provide the organizational structure to do so.

To be sure, this kind of criticism, which testifies to a lack of proper theoretical and methodological grounds for Action Research, has been invoked for many decades. And while the theoretical contributions to Action Research are filling bookshelves after bookshelves, there seems to be no reconciliation in sight. This book suspects that AR can benefit from approaching the problem from a different angle. In the following chapters, I argue that, far from suffering from a lack of theoretical grounding, Action Research evades a reflection of the kind of ideologies it takes for granted, such as the belief in democracy as the superior order, a specific deployment of Marxism, and deliberative concepts of communication. These underlying ideologies shape the understanding of "subjectivity," i.e., the capabilities and needs of the people involved, and "democracy," i.e., the various accounts of the interactional processes of these individuals and the goals their interactions aim at.

Too often, action researchers seem to rely on implicit ideologies to render their goals in terms of "improving society," "democratizing knowledge," and "humanizing working conditions." The suspicion would be that Action Research cannot fully account for the necessity of the changes it aims to invoke because its concepts of subjectivity and democracy are bound to a distinction between theory and praxis, where theory supplies the definitions of subjectivity and democracy, which are applied in empirical research. Action Researchers struggle to practically harmonize a theoretical cacophony to bridge a gap between theoretical/philosophical concepts of subjectivity and democracy derived from certain ideologies and the actual research process. I hope that I can address this issue with an attempt to specify the underlying concepts of subjectivity and democracy and tie them back to the overall effort to transcend the distinction between theory and practice.

To tackle this problem, I will first engage in a historical reconstruction of the various traditions of Action Research and their understandings of subjectivity and democracy. An evaluation of their legacy will eventually lead me to the works of Klaus Holzkamp, founder and arguably the main protagonist of the German tradition of Critical Psychology, and his take on subjectivity and democracy.

Holzkamp's conceptions of action and subjectivity rest on a developmental approach to human life. He understands the sociability of humankind as the product of a sociogenetic process which is, as it were, produced by phylogenesis but at the same time supersedes it (Tolman, 1994, p. 86). An understanding of subjectivity and democracy in this approach entails a reconstruction of the societal-historical embeddedness of these concepts. From there, the concrete life circumstances of subjects can be explored "from the ground up," i.e., *from the subject's standpoint*. Following these thoughts, Action Research could be rendered as the study of *the societal mediation of individual existence*, as Holzkamp would put it, i.e., human life as created, lived, and negotiated in a societal (*gesamtgesellschaftlich*) environment. The distinction between social (*sozial*) and societal (*gesellschaftlich*) carries much weight. Following a Critical Psychological framework, a fruitful analysis of a given field needs to take into account not only the local laws, rules, norms, habits, and so forth (i.e., the social – which Holzkamp terms "infrastructure") but also the superstructure (i.e., the societal), which provides the conditions of the existence of this social field.

In a nutshell, Holzkamp understands society not from the perspective of restrictions imposed on its members but conceptualizes it as a priori liberating. By being part of society, human beings collectively organize their lives, share labor responsibilities, and liberate resources for new activities. Against this background, the subject is always one step ahead of the restrictions put in front of her; every restriction imposed upon subjects is a source of new agency, for every restriction always also represents a hidden possibility: a possibility to act differently (a *Handlungsmöglichkeit,* literally: an opportunity/possibility to act). As I will argue with respect to Action Research in general, Holzkamp establishes a new model of change, one which is not driven by the specific needs of the subject or a general need for democratic structures but one which is based on the controversies of existing possibilities and restrictions in concretely given, empirical scenarios.

Plan of the Book

This book is devised into four main parts. Part I reconstructs the historical development of Action Research, starting with the founding fathers Jacob Levy Moreno, John Collier, and Kurt Lewin. Because Lewin is a general point of reference in the field, his contribution is examined in detail. At the same time, Lewin draws many of his ideas from Jacob Levy Moreno. The two are connected on various levels: Both came to the United States because of an ever-increasing anti-Semitism in Europe. Both are interested in groups and group dynamics. Most interestingly, they shared a

larger number of students who actively fostered an exchange of concepts between the two. For reasons I will explain later, Moreno's contributions to Action Research largely remain unreferenced in the literature. Instead, Lewin is usually praised as the sole originator of the main concepts. To reintroduce Moreno's contribution to the field, the concrete circumstances of the developments of what he called *interaction research* are discussed in chapter two.

After Lewin's untimely death, Action Research continued first in the UK and then in Norway, where several programs to increase workers' democratic participation in the production process were launched. Ever since the 1960s, Norway has been at the forefront of developments in the Northern Tradition of Action Research. To be sure, projects were carried out in other countries throughout Europe, Australia, and later also in the United States. Still, the actual theoretical and empirical developments unfolded in Norway, which offered favorable conditions of union and government support. For this reason, Norway remains the central focus of the discussion of the Northern Tradition. Part II of this book takes a second loop through the historical development of the field; this time, however, it focuses on the concepts of subjectivity and democracy as they emerged in the various traditions. To understand how subjectivity and democracy were shaped, the various approaches will be investigated alongside two sets of questions: *What theoretical concept underlies the approach? What are the concrete empirical procedures with which these theories are enacted?*

Against the background of a historical understanding of the development of Action Research as well as its underlying theories, ideologies, and practices, Part III turns to German Critical Psychology and the writings of Klaus Holzkamp to examine how he conceptualizes subjectivity and democracy. Specifically, Part III argues that Holzkamp introduces a concept of subjectivity that does not rest on an implicit ideology or a utopian idea of the future but a two-layered empirical procedure. To be sure, Holzkamp exhibits a specific utopia himself. But it is not a utopian concept of the future, be it the liberalist hope of deliberative democracy or the Marxist "association, in which the free development of each is the condition for the free development of all" (Marx et al., 1848/2008, p. 66), but a utopia in the present, one that "does not expunge the possible from what exists" (Markard, 2009, p. 67).

Part IV recapitulates the history and underlying concepts within the field of Action Research and attempts a reintroduction of Jacob Levy Moreno about Klaus Holzkamp. It advocates that Moreno's concepts can be recast and put to use in Action Research. The suggested catalyzer for such a reappropriation is Holzkamp's concept of subjectivity and intersubjectivity, as spelled out in Part III.

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Part I
The Emergence of Action Research

The emergence of Action Research as a research tradition is closely intertwined with the history of psychology. In the late eighteen-hundreds and the early twentieth century, academic psychology was primarily concerned with psychometric measurement and laboratory experiments. This situation changed with the political circumstances in Europe and North America during and after the two world wars. Psychology suddenly received attention from various stakeholders. The military had a keen interest in understanding the human condition from a psychological point of view in an attempt to develop strategies to manipulate soldiers' attitudes toward killing. Breaking the morale of the enemy also became a primary goal. At the same time, strategies were needed to keep the morale "at home" high. Governments also hoped for new ways to improve resource management, such as food supplies, to support their soldiers at war better. As such, psychologists were asked to take the knowledge they had gained from psychophysics and laboratory experiments into the "real world" to support the ongoing military struggle. Right within this transformation phase toward more applied forms of psychology, the German-born Jewish psychologist Kurt Lewin who had escaped from Nazi Germany to the United States, received a significant grant from the US government to research and improve food and cooking habits among American housewives. In this context, he developed the idea of a participatory research process in which researchers and research participants work together on specific goals. And the term he used to describe this research strategy was *Action Research*.

The history of psychological research into people's everyday experiences is older than the Action Research tradition. Psychoanalysis, for example, puts the person and the practical application of psychological knowledge at the center. This happened not so much because of external reasons and government demands. Instead, the methodological and theoretical underpinnings stemming from its origins in medicine provided the grounds. In the early 1930s already – and as such almost a decade earlier than Lewin – Jacob Levy Moreno, a Jewish-Romanian-born Austrian-American psychologist, and psychoanalyst, coined the term inter-action research, which he used to describe a research strategy with a focus on the production of change in collaboration with everyone who would be affected by that change.

Moreno's contribution to Action Research is marginalized today or overlooked entirely. There is, however, undeniable evidence that Moreno did not only come up with similar ideas as Kurt Lewin after him but directly inspired the work of the latter. At least from a chronological point of view, it was Moreno, not Lewin, who provided the original point of departure for the field of Action Research to unfold.

There is yet another early contributor to the development of AR: Social reformer, Native American advocate, and Kurt Lewin's friend John Collier. Collier was dedicated to the problem of *the treatment of those people who are not white*, as he regularly put it. Like Moreno and in contrast to Lewin, his approach was less focused on academic adequacy and coherence in methodology and showed a vital concern with democratic collaboration. In the historiography of Action Research, Collier, however, suffers from a similar fate as Moreno.

It was primarily Kurt Lewin's work, first in the *Food Habits Studies* and later in the *Harwood Manufacturing Company Studies*, that set the tone for future action research endeavors. After Lewin's untimely death in 1947, Action Research efforts in the United States entirely disappeared but were continued by the *Tavistock Institute of Human Relations* in Great Britain. The studies in industry and specifically in the Coal Mining sector conducted by Eric Trist of the Tavistock Institute provided the grounds for an elaboration of Lewin's theoretical approach to Action Research and the eventual emergence of a new theoretical basis called *Socio-Technical Systems Design (STSD)*. STSD was at the heart of the Norwegian *Industrial Democracy Project (ID-Project),* which launched in the late 1950s and early 1960s. Research projects in the British coal mining industry had shown that a democratic organization of the workflow was not only psychologically and physiologically healthier for the employees but also more productive than the old system, which was heavily built on a minuscule division of labor as fashioned by *Taylorism*. Despite these results, the fear among the managers of the National Coal Board in Britain of breaking up old hierarchies and eventually losing managerial powers was greater. Lobbying against the research efforts began, and ultimately the publication of the results was stopped.

Action Research only really took off publicly when the Norwegian government decided to invest in democratic structures of workplace organization and invited the Tavistock Institute of London to share their insights. With the beginnings of the Norwegian Industrial Democracy Project in the early 1960s, Norway emerged as the primary source of theoretical and practical advancements of the Northern Tradition. Both STSD and the subsequently developed dialogue approach became possible only because Norway offered a unique environment: A government that was generally supportive of efforts to strengthen worker participation and an equally unique and cooperative relationship between workers and employers.

Chapter 2
The Founding Fathers: Moreno, Collier, Lewin

Jacob Levy Moreno

Jacob Levy Moreno was born Jakob Levi, the son of Moreno Nissim Levi and Pauline Iancu, both Sephardic Jews. In his autobiography, he claims to have been born on May 16, 1892. According to him, his mother went into labor during the dawn of the holy sabbath on a ship in the Black Sea that carried no flag rendering him stateless from the very beginning of his life. Moreno describes himself as a "born citizen of the world, a sailor moving from sea to sea" (Moreno, 1989a, p. 16). A truly remarkable story, almost too fantastic to be credible. And indeed, a closer look at the historical documents reveals this story and, more generally, many of his biographical writings as a work of fiction. Moreno was, of course, very well aware of the fictional character of his personal life story. He, however, did not think of it as incorrect; instead, his autobiography exhibited a reality that was, as Moreno put it, "psychodramatically and poetically accurate" (Moreno, 1964, p. 7).

Moreno's birth certificate shows that he was born on May 18, 1889, in Bucharest, where his family lived at the time (Bratescu, 1975, pp. 7–8). His enrollment certificate at the University of Vienna shows the same birthdate. It reveals his nationality as Ottoman – per his father's nationality – not his birthplace, a common practice in the Austro-Hungarian Empire which relied on the *jus sanguinis*, the right of blood (Archives of Alte Universität, Postgasse, Vienna, Registry of Graduation (Medicine), 1917). Moreno began to claim 1892 as his birth year only after he had moved to the United States in 1925; all documents issued in Europe show his birth date as 1889. Altering facts about one's biography when moving to a new country was not uncommon at the time, specifically for people with a Jewish background. Moreno's story was, however, not just a random alteration for the sake of anonymity. His choice was informed by theory: human life, as Moreno understood it, always was a holistic co-construction: "The stories … exist in the minds of the people involved and told by them" (Moreno, 1964, p. 7). As such, the birth year Moreno chose reveals more

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about him than one might think: 1892 is the 400 years commemoration of the exodus of the Jews from Spain following the royal decrees, which ordered Jews and Muslims to convert to Catholicism or leave. It thus reflects his family history and origin as a Sephardic Jew. The year 1892 also marks the 400 years commemoration of Christopher Columbus' journey and subsequent "discovery" of America. Onboard Columbus' ships were not only adventurers and sailors. One-third of the crew were Jews who had to flee Europe to escape the Inquisition, just as Moreno sought refuge from an ever-increasing Anti-Semitism in Europe.

Moreno spent his early childhood in Bucharest before the family moved to Vienna in 1898 or 1899. His father was an unsuccessful businessman, often absent from home but unable to provide the necessary funds for the family to make a living. To revive his business efforts, Moreno's father relocated the family to Berlin in 1903 or 1904. When the success he had hoped for failed to materialize, the family allowed the young Jacob to move back to Vienna as he was convinced he could make a living on his own by tutoring younger school children. Soon after Moreno had left Berlin, his parents separated, and his mother and siblings returned to Vienna while his father moved to Istanbul.

Moreno's approach to tutoring was distinct from the typical approach of teaching specific school subjects separately, giving homework, and so forth. He focused on imagination and creativity. One of his students was Elisabeth Bergner, who became one of the most famous German actors of the 1920s. Her biographer Arthur Eloesser described Moreno's style of teaching and his influence on Bergner's later career in the following words:

> For a long time, the children were tutored by a Spanish-Jewish student of medicine. This apostolic character could have been the creation of the fantastic ethics of Jakob Wasserman. He was, first of all, a brilliant pedagogue, so naive or so much an artist that he could not keep the various disciplines apart. … There was no question about homework, results, or exams. He was a strange teacher, all the more strange as he did not accept any compensation from the people who hired him: "Give the money directly to the poor." … On their walks, the children were induced to give their few pennies to still poorer children, the pennies given to them for milk, fruits, or even for the carousel. The teacher had them fill their cheeks with air and assured them enthusiastically how wonderful the imagined piece of chocolate tasted, or they gave their balls away when playing in the city parks. Then they threw chunks of air at one another with empty hands. … The good poet would have taken him back at the right moment by letting him pass away through a noble kind of consumption or through a gentle heart attack. But now, being older, he may have abandoned his adolescent ideals. Oh no, he is a Wunderdoktor (miracle doctor) in a suburb of Vienna. He still treats the poor without accepting money. (Eloesser, 1926/1928, pp. 23–25, translated from original German)

Besides tutoring, in the summer of 1908, Moreno founded a group with others to help underprivileged people in Vienna. At that time, the Austro-Hungarian Empire suffered from racial conflicts and instability. Vienna was a melting pot of many nationalities, with people coming mainly from eastern Europe hoping to find employment, often dreaming of eventually making enough money to move on to the United States or Palestine (Mason, 1997, p. 31/42). Finally, the group had collected enough funds to buy a house in downtown Vienna. The *House of the Encounter*, as

it was called, offered shelter for homeless persons and refugees from other parts of the Austro-Hungarian Empire. The house was organized democratically with weekly assemblies in which problems were discussed. In his autobiography, Moreno claims that Leon Trotsky even used the place to hold meetings during his visits to Vienna (Moreno, 1989a, pp. 46–47).

In 1913, Moreno started another humanitarian project; this time, however, combined with a genuine interest in learning about the life circumstances of the persons he aimed to support. With a physician named Wilhelm Gruen and the leftwing Vienna newspaper *Der Morgen editor* Carl Colbert, Moreno visited prostitutes in their houses in the back-then red-light district of Vienna *Am Spittelberg* to discuss their life circumstances and find ways of self-help and self-organization.

With the outbreak of World War I in 1914, Moreno, like many others of his generation, volunteered for military service. Moreno was rejected, however, since his loyalty to the Austro-Hungarian Empire was questioned based on his Sephardi Jewish background and Ottoman nationality. Instead, in 1915 he was hired by the government as a medical officer and transferred to a refugee camp with over 10,000 detainees in Mittendorf, close to Vienna. The camp was occupied by Tyrolians, whom the Austro-Hungarian government relocated officially to protect them from the Italian army, but, at the same time, also to prevent them from changing loyalties given their assumed affinity with Italy and their Italian language proficiency (Steininger, 2003). In many ways, their situation was comparable to the Japanese-Americans on the Pacific coast in the United States during World War II (Burton et al., 2002). At first, Moreno was responsible for matters of hygiene, and later, he became the superintendent of the children's hospital on site. Beyond these duties, he acquired permission from the government to relocate individuals within the camp. To reduce corruption and animosities, he relocated people depending on friendship and personal interests and reorganized workgroups (Moreno, 1989b, pp. 65–67; Garcia, 2010, p. 5).

Because Camp Mittendorf was located only 30 minutes from Vienna, Moreno spent most of his spare time in the Austro-Hungarian capital and was exposed to the rich intellectual life of the time (Schorske, 1981). He attended lectures by Sigmund Freud and Albert Einstein. He met the likes of Martin Buber, Arthur Schnitzler, Jakob Wasserman, Robert Musil, Franz Lehar, Alfred Adler, and others in the famous Vienna Café Museum (Moreno, 1989b, pp. 61, 62, 70; Segel, 1993, pp. 25–27). Specifically, Buber greatly influenced Moreno's ideas of the encounter (Waldl, 2005). It was during that time that Moreno finished his medical degree, published his first journal *Daimon* (which was later renamed *Der Neue Daimon* and then *Die Gefährten*), and established his first theater for Psychodrama called *Stegreiftheater*, or "theater of spontaneity," in Vienna (Weibel, 2005, p. 529/530).

At the beginning of the 1920s, Moreno realized that antisemitism was to become an ever-increasing problem throughout Europe. He urged many of his friends to leave Austria and developed plans to emigrate to the United States himself. With the help of the engineer Frank Lörnitzo, he invented a machine to record sounds that was featured in a short *New York Times* article on July 3, 1925. The machine was described as "an invention [which] consists of disks on which the broadcast sounds

are recorded by a spiral consisting, not of deeper or shallower impressions as on a gramophone record, but of a continuous line of points more or less magnetized according to the strength or quantity of the sound" (39, p. 4). Moreno secured a patent for his invention in Austria and got the General Phonograph Company in Elyria, Ohio, interested in his invention. Moreno claims to have been able to sell his patent to the company (Moreno, 1989b, p. 92); however, no indication of such a contract can be found in the company records (Marineau, 1989, p. 180). Moreno likely overstates the importance and novelty of their invention. Moreno and Lörnitzo were probably given some money for their ideas – a common practice at the time – but certainly could not secure any substantive profit as they had hoped. Nonetheless, this invention enabled Moreno to leave Europe and come to the United States. He was, however, off to a rough start. Moreno spoke little to no English, which made it challenging to acquire a job. It was not until 1927, when he managed to pass the final exam in medicine of the state of New York that he could work as a physician again. He subsequently got hired by the Mt. Sinai Hospital, where he met Beatrice Beecher, a child psychology professor, who agreed to marry him under the condition that they get divorced as soon as Moreno was able to secure American citizenship (Marineau, 1989, p. 96).

Moreno's eventual breakthrough in the American scientific community came in 1932 when he conducted a study at the Sing Sing Correctional Facility funded by the *National Committee on Prisons and Prison Labor (NCPPL)*. He presented his results at the annual meeting of the *American Psychiatric Association* in the same year. Like his work with refugees in Camp Mittendorf, Moreno suggested reorganizing the facility according to his sociometric analysis in the hopes of improving the success of rehabilitation of the prisoners (Moreno, 1989b, p. 94; Marineau, 1989, p. 111). Through his work at Sing Sing, Moreno got acquainted with Fannie French Morse, then superintendent of the *New York State Training School for Girls in Hudson* (Firestone, 1944, p. 76). Morse appointed Moreno as Director of Research at Hudson, a position he held from 1932 to 1934. At Hudson, Moreno's research resembled many Action Research strategies. In contrast to his work with prostitutes in Vienna, research was one of the primary goals of his investigations. Compared to his research at Sing Sing or Mittendorf, the girls at Hudson took an active role in the research process. They decided which groups to belong to and voted on group leaders. Moreno made extensive use of video recording technology during this period, and parts of the material are still available today (Moreno, 1946, pp. 385–419, 1969, pp. 251–254).

In 1934 Moreno published his magnum opus, *Who Shall Survive?* (Moreno, 1934) and a revised and vastly extended edition in 1953 (Moreno, 1953). The book provides the foundation of Moreno's new science, which he called *sociometry*. Moreno's interdisciplinary approach is characterized by the psychiatrist William Allison White who contributed the foreword to *Who Shall Survive?*:

> Dr. Moreno comes back, apparently, to the position where the environment seems to have greater significance. Still, he returns to that aspect of the problem not on the same level as it existed originally but at a higher level. The interesting thing is that while he does come back to a consideration of the environment, that consideration includes the subjective

> aspect. … So we have one of those typical advances that swing from one point of view to another but, in doing so, includes that other. At the same time, Dr. Moreno emphasizes that he differs from the psychoanalytic approach in another very significant way, namely, that the analyst works backward to explain the individual's conduct while he takes the individual's conduct as the starting point and works forward. … Think of how much may be added to our capacity for dealing with our mentally ill patients in institutions by a more intelligent classification, a classification which shall not be just a simple matter of practically conducting the wards as administrative units but a classification that would go deeply into the individual problems of each patient and relate them one to another, and more particularly perhaps to the nurses, upon a basis which has definite therapeutic objectives. And think, further, if you have no objections to flights of the imagination, of what possibly it may offer to an understanding of the problems of democracy as they occur in a country like the United States made up of races from all the four quarters of the globe. (White & Moreno, 1934, pp. xv–xvi)

Who shall survive? was a successful publication – not so much in psychiatry but social psychology and especially sociology – and Moreno had finally gained attention in the American scientific community. In 1936, he started his own journal, the *Sociometric Review*, later renamed *Sociometry: A Journal of Interpersonal Relations*. The editorial board included Margaret Mead (1955), Gordon Allport, Hadley Cantril, and others (33). Moreno served as publisher, while Gardner Murphy and later George A. Lundberg took the role of general editors. Practical success followed: His theories were applied at St. Elizabeths Hospital in Washington (Moreno, 1989b, pp. 119,12; Herriott & Hagan, 1941; Hagan & Duval, 1943) and with the beginning of World War II, the American army – and later the British Army as well – became interested in Moreno's research on groups and supported him with research grants. This success, however, also marked the end of Moreno's productivity on the theoretical level. Instead, he started to travel the Western world to advertise his ideas. He could have probably accepted a position at a university, but in his account, his suspicion of institutions kept him from following that path (Moreno, 1989b, p. 38). At the same time, Moreno saw himself not simply as a psychiatrist or an academic. He was convinced of his peculiarity: Moreno saw himself as a God-like figure. At the same time, it seems that he was aware that such belief constituted a psychopathological condition, which he referred to as the *God Syndrome.* Following the psychodramatic method, the only way to deal with this condition was "to act it out" (Moreno, 1989a, p. 39). His travels throughout the United States and Europe were a messianic adventure, preaching to the world.

Moreno and Action Research

Throughout his career, Moreno contributed to several disciplines and developed several theoretical and practical approaches. He pioneered or wrote some of the first contributions to *psychodrama*, *sociometry*, *group psychotherapy*, *modern spontaneous theater*, and *art therapy*. As we have seen, he was even involved with the invention of a technique for sound recording. This complexity and variety of contributions

were reflected in his equally complex character. Clearly an egomaniac, he often described himself as God's incarnation on earth. He said about his autobiography: It "describes not only the transformation of a man into God but the reverse, the retransformation of God into man" (Moreno, 1989a, p. 15). Such depictions often made him an outsider and complicated many relationships, including the Lewin group. Unable to collaborate, the Lewin group continued to rely on Moreno's ideas, specifically when they turned to Action Research, however, without direct references to Moreno's work. However, the historical precedence of Moreno's ideas over Lewin's formulations of the Action Research strategy is undeniable.

A collective point of reference for almost every discussion of the history and development of Action Research is Kurt Lewin's concept of a *spiral of steps,* which he introduced in the late 1940s (Lewin, 1947a, b). Moreno described his project of sociometry in a very similar pattern in 1937. Sociometry is rendered as a combination of

> (1) the research procedure, aiming to study the organization of groups; (2) the diagnostic procedure, aiming to classify the positions of individuals in groups and the position of groups in the community; (3) therapeutic and political procedures, aiming to aid individuals or groups to better adjustment; and finally, (4) the complete sociometric procedure, in which all these steps are synthetically united and transformed into a single operation, one procedure depending upon the other. (Moreno, 1937, p. 211)

The development of this participatory research strategy is closely intertwined with Moreno's research on groups and group dynamics. He focused on the interaction of participants and first coined this approach *inter-action research* (Moreno, 1943). The earliest of these projects he conducted with Wilhelm Gruen in 1913. It explored the situation of prostitutes in Vienna. Moreno describes their situation as follows:

> Here was an entire class of people segregated from the rest of society, not because of their religion or ethnic character, but because of their occupation. They were unacceptable to the bourgeois, the Marxists, even the criminals. The criminal, after serving his prison sentence, is again a free agent. But these women were eternally lost. They had no civil rights. There were no laws, or even social mechanisms, for protecting their interests. (Moreno, 1989a, p. 48)

Moreno wanted to do for them "what LaSalle and Marx had done for the working class," namely, to give "them a sense of dignity" and possibly organize "them into labor unions" (Moreno, 1989a, p. 48; Sternberg & Garcia, 2000, p. 9). Moreno and Grün organized small meetings of eight to ten participants twice a week. In the beginning, the conversations consisted of everyday incidences such as overnight detention or harassment by clients. Over time, the participants understood that they shared similar life circumstances and could work together. They created a shared bank account for emergencies, organized a network of doctors who were willing to treat them, and by the end of 1913, had established an organization with elected officers (Moreno, 1989a, p. 49; Moreno & Fox, 1987, p. 209; Ameln et al., 2009, p. 522).

Moreno describes this process in his autobiographical reflections: "The conferences at first simply dealt with everyday incidents. … But gradually, they discovered the deeper value of the meetings, that they could help each other" (Moreno,

1955b, p. 5). Moreno and Gruen established a participatory research project in which the participants explored shared sets of problems that could be tackled with collaborative efforts.

Collaboration of the research participant is just one cornerstone of Action Research that Moreno seems to have pioneered for Lewin and others to draw on later. Like Lewin, when he turned to Action Research, Moreno also believed in the necessity to carry out research in the field rather than the psychological laboratory: "In the social sciences, the subjects must be approached in the midst of an actual life-situation," Moreno wrote in 1940 (Moreno, 1940, p. 317). Lewin conducted field research long before the 1940s, as did Moreno. It, however, was an explicit and theoretically embedded part of Lewin's general framework during the Harwood studies in the mid-1940s only. Also pioneered by Moreno were the different instances of role play Lewin used in his work in the National Training Laboratories in 1946.

The authorship of many concepts used by both psychologists is further blurred by a shared group of famous students: Ronald A. Lippitt, Leland Bradford, Alex Bavelas, and Alvin Zander. Moreno is the originator of the research on group dynamics (Petzold, 1980b), a topic closely intertwined with his concept of sociometry first introduced in 1934 (Moreno, 1934). However, in contrast to Lewin, Moreno saw his concept as irreconcilable with the Gestaltists and the psychologists in general of his time:

> The sociometrist, as a student of group dynamics and social configurations, is in a different situation from the Gestalt theorist. He does not approach something given, a Gestalt; he is himself the framer of a Gestalt and, therefore, the inventor of the framework. And it is within these frameworks that he approaches the social phenomena he studies and not outside of them. (Moreno & Jennings, 1938, p. 343)

Moreno envisioned research as participatory from the beginning and argued against an outside spectator role of the researcher. His work on social groups can, in many ways, be seen as groundbreaking and paved the way for Lewin's research. *Moreno and Jennings already introduced* laissez-faire*, autocracy, and democracy, three terms which are famously associated with Lewin's and Lippitt's work* (Lewin & Lippitt, 1938), in 1936 (Moreno & Jennings, 1936). They investigated how students in the New York State Training School for Girls chose specific seats in the cafeteria and discovered three patterns: the *laissez-faire sociogram* in which everyone chose their seat depending on personal decisions; the *autocracy sociogram* in which seats were assigned from the outside, and the *democratic sociogram* in which seating was negotiated among the students – the latter being the most successful model to prevent conflict. Moreno himself compared his work and Lewin's study almost two decades later:

> A good illustration is Lewin's experiment with democratic and autocratic atmospheres, which I have confronted elsewhere with an earlier experimental design of my own, dealing with the same problems [he is referring to the aforementioned 1936 study, M.D]. The question I tried to answer in my own mind was: which direction of research is more productive? Upon analytic examination I concluded that Lewin's approach is logically more elegant. … My experiment, on contrast with this, was done naively, without any pretense of logical elegance; but we had direct, empirical evidence. (Moreno, 1952b, p. 152)

This direct empirical evidence and straightforward research designs allowed Moreno to develop a closer involvement with the research participants. He was forced to rethink what he did repeatedly during the research process precisely because he could not rely on logical elegancy. It was a matter of his research design that the observed "objects" had to become subjects: alive and involved in the planning and creation of the research process. For Moreno, the researcher turned into a participant observer:

> The participant observer of the social laboratory, the counterpart of the scientific observer in the physical or biological laboratory, undergoes a profound change. Observing movements and voluntary association of individuals has value as a supplement if the basic structure is known. But how can an observer learn something about the basic structure of a community of one thousand people if the observer tries to become an intimate associate of each individual simultaneously in each role which he enacts in the community? He cannot observe them like heavenly bodies and make charts of their movements and reactions. The essence of their situations will be missed if he acts in the role of a scientific spy. The procedure has to be open and apparent. The inhabitants of the community have to become participants in the project in some degree. (Moreno, 1937, pp. 209–210)

The Sociometric Institute, founded in 1942, embodied many principles that define the field of Action Research today and shared many similarities with *The Research Center for Group Dynamics,* founded by Lewin three years later. Moreno described his institute as "the meeting-point of all the sciences in which it partakes: psychology, sociology, cultural anthropology, biology, psychiatry, and economics" (1942, p. iii). In the same context, sociometry is described as

> a theory and then a method – a method of how to gather the really vital facts about the interindividual relationships among people living in social groups and how immediately to remedy the frictions among their members with the minimum of effort. Its outstanding characteristic … has always been that it focusses its attention upon actual people, not upon abstractions of any sort, upon actual situations, like specific homes, schools, factories or communities, not upon abstracted and generalized situations, and upon situations in the present tense, not upon conjectures of past or future situations. (1942, p. vi)

Moreno envisions his approach as participatory. He is not interested in working *about* people; he wants to work *with* them and sees this as a pivotal characteristic of successful social research:

> The people who form the subjects of research are not used as guinea-pigs of some sort: it is their initiative, their spontaneity, their judgment and their decision which counts higher than anything else in the procedures applied in their behalf. It is obvious that the pertinent data about the human interrelations in a group cannot be found by one participant observer with any degree of certainty. The maximum possible certainty is, however, secured if every member of the group becomes a participant observer of all the others and of himself. (1942, p. vi)

The difference between the researcher and research participant diminishes, and the research process shapes collectively. Research in Moreno's understanding "approaches every new situation in a concrete way, re-shaping its tools for each specific situation" (1942, p. vi).

At the same time, the announcement shares a good deal of the critique Action Researchers have brought forward concerning the responsibility of the social sciences, namely

> the vast ignorance of the truly vital nature of interpersonal and intergroup relations. … The man in the street was brought up with a profound respect for Science and with the conviction that, in the moment of crisis and need it will come to his rescue. … It has not yet been shown that the social sciences have anything to offer – any program with which to counter this world crisis [Moreno refers to WWII] … There are a number of social researches in progress today which are worthy of their reputations and which will bear fruit. Notwithstanding their value from the point of view of pure science, their applicability to the pressing problems of the present is almost nil. (1942, pp. vi–v)

To be applicable to the pressing social issues, Moreno turns the researcher into a "social investigator." The social investigator follows the "observational interpretive (1)," i.e., observing the groups from the outside, "participant observation (2)," and "educating participants (3)" in the field to become researchers, that is, social investigators (Moreno, 1934, pp. 11–13, 1940). Social investigators are investigators as much as they are part of the investigation because "the 'uninvestigated investigator' constitutes … an ever-present error" (Moreno, 1940, p. 318). For this procedure to be successful, the research interest cannot originate with external researchers. Instead, it must emerge from within the research field (Moreno, 1934, p. 91). For Moreno, the researchers must "make a deliberate attempt to bring the subjects into an experimental state which will make them sensitive to the realization of their own experiences and action patterns" (Moreno, 1940, p. 317).

Similar to Lewin's concept of time, Moreno relies on the present moment, the concrete situation: "The psychology of action cannot divorce the act from the actor, the actor in situ, and the single actor cannot be separated from the ensemble of actors in situ" (Moreno, 1952a, p. 366). Therefore, researchers need to concentrate on each moment that happens in real life and is perceived as contingent and unpredictable. No abstract positions can be taken; researchers need to be involved. *The research subject cannot be researched if it is analytically separated from its concrete life circumstances, which themselves undergo constant changes*:

> The so-called social structure resulting from the inter-action of two thousand million individuals [the world population at the time of writing] is not open to perception. It is not "given" like an immense visual configuration – for example like the geographical configuration of the globe, but it is every moment submerged and changed by inter-individual factors. (Moreno, 1943, p. 316)

Moreno introduces many of the central characteristics of Action Research: interdisciplinary research strategies, the centrality of the concrete context, a collaboration of researchers and research participants, participation of the researchers in the field, development of adequate methods and procedures in a collaborative act during the research process, and, of course, the goal to produce a change to improve people's life circumstances. At the same time – and in contrast to Action Research and critical social sciences today – he upholds objectivity and neutrality of research and denies the influence of economic interests on the production of knowledge, a position he shares with Lewin (Moreno, 1942, p. vii; 1948, pp. 125–141, 1945a, p. 135).

While it is true that Moreno's version of sociometry – which he also referred to as a "hot sociometry" (Moreno, 1954, p. 186) in contrast to "cold sociometry," i.e., research focused on actual change on the societal level versus affirmative research, which reproduces the societal structures – started with lots of revolutionary impetus, this momentum was gradually lost and disappeared almost entirely by the mid-1950s when Moreno decided to drop the macro-level in favor of microlevel and psychotherapeutic work. In his early magnum opus, he still claims that a "true therapeutic procedure cannot have less an objective than the whole mankind" (Moreno, 1934, p. 3). And in 1949, he wrote in an article on Sociometry and Marxism: "The sociometric experiment aims to change the old social order into a new social order. … The sociometric test, in its dynamic form, is a revolutionary category of investigation" (Moreno, 1949, p. 114). In 1954, however, Moreno wrote: "In revolutionary sociometry research was a by-product. In cold sociometry research for its own sake became the central aim" (Moreno, 1954, p. 187). In the same paper, he describes hot sociometry as an era of the past, namely, 1923–1937, and even cold sociometry, for him, seems to be in its final stage to be followed by what he calls "perceptual" sociometry that is a strong focus on the individual. Moreno retired from his position as editor of the Journal *Sociometry* in 1955 and shifted the direction of the *International Journal of Sociometry* toward group psychotherapy (Moreno, 1955a).

John Collier

John Collier was born in Atlanta, Georgia, in 1884. He enrolled as a nondegree student at Columbia University in New York in 1902 to study French literature, drama, and biology under the guidance of Lucy Graham Crozier. He also studied psychology in Paris for a brief period to work with Pierre M. F. Janet. In 1908 he joined a community organization in New York's Lower East Side called the People's Institute (Rosenbloom, 1992). He was appointed to the executive committee in 1910, shortly after the founder of the Institute, Charles Sprague Smith, had passed away. During his tenure, Collier promoted a wide range of community development projects such as after-school use of school buildings for community projects, the growth of movie theaters in New York City, and the founding of what became known as The Home School, an education facility, which was based on the principles of John Dewey.

By the early 1920s, he had moved to San Francisco. He joined the California State Housing and Immigration Commission as director for Americanization, partly because securing funding for his projects in New York became ever more challenging. While developing an adult education program at San Francisco State College between 1919 and 1922, he became aware of the living conditions the native population in the United States had to endure. He became their advocate and most famously lobbied against the Bursum Bill, a land reform bill that would have taken even more land from the native population. In 1923, he organized the American

Indian Defense Association and moved to Washington, D.C., to lobby for Native American interests. According to Collier's memoirs, he successfully implemented the Indian Reorganization Act in 1934 – only one year after Collier was appointed Commissioner of Indian Affairs by President Roosevelt (Collier, 1963). The new law granted many rights and privileges, such as the right to conceive their constitution, to certain Indigenous American tribes (Myers & Smith, 2008, p. 39). He also promoted infrastructure improvements in Native American territories, which he described as "Indian-built, Indian-maintained, and Indian-used projects" (Collier, 1963, p. 187). After his retirement as Commissioner, he became an anthropology professor at City College, New York.

Collier advocated a combination of research and action during his work for the People's Institute in 1917 already. In a letter written to Charles Sanderson, then director of the People's Institute, he wrote: "I am determined the institute shall concern itself with social action rather than social friction: that is the improvement of social structure; … an enriched, rather than a merely extended democracy" (Collier, 1983a, p. 70). He is critical of method-driven social research and refuses to "intellectually recognize that truth must be efficient." Instead, for Collier, a researcher should "travel, as it were, unarmed and naked in the jungle" (Collier, 1983a, p. 71) with "a philosophy of perpetual adaptation" (Collier, 1983a, p. 72). For Collier, the kind of "experimental sociology" that he envisions "is action" (Collier, 1983a, p. 73). In a 1918 letter to the trustees of the People's Institute, he describes the community centers created under his guidance as "laboratories of method" and "demonstration centers for national influence" (Collier, 1983b, p. 86).

Collier's conceptualization of research combined with action was published in *Social Research* in 1945 – one year before Lewin's "Action research and minority problems" (Lewin, 1946). Collier calls his approach "action-research, research-action" (Collier, 1945, p. 294). Collier hopes that "research can be made a tool of action essential to all the other tools, indeed, that it ought to be the master tool" (Collier, 1945, p. 298). Naturally, such research would be interdisciplinary. Collier emphasizes "that this kind of research makes demands on the research worker that are far more severe than those made by the specialized and isolated kind. It requires of him a more advanced and many sided training" (Collier, 1945, p. 300). It relies heavily on researcher-researched cooperation since "the administrator and the layman always participate" in the process "impelled as it is from their own need" (Collier, 1945, p. 276). Collier is convinced that action research proved to be effective: "The government saved itself unknown amounts of money, probably millions, by using social science and relying on the principle of democracy" (Collier, 1945, p. 285). And most importantly, Collier is convinced that his approach carried out over the years "has pushed back our horizons of knowledge and understanding of a whole" (Collier, 1945, p. 276). For Collier, Action Research is best expressed in the following terms:

> And when the people acted upon are themselves made the true partners in the actions, and co-discoverers of the corrections of error, then through and through, and in spite of blunders or even by virtue of them, the vital energies are increased, confidence increases, power increases, experiences builds toward wisdom, and the most potent of all principles and ideals, deep democracy, slowly wins the field. (Collier, 1945, p. 298)

Kurt Lewin

Kurt Tsadek Lewin was born September 9, 1890, in Mogilno, Posen, Prussia. According to the Curriculum Vitae attached to his dissertation, his father, Leopold, was a merchant and landowner (Lewin, 1916, p. 37). Both his mother, Recha, and his father were of Jewish descent, and Lewin likely grew up speaking three or possibly four languages: German, Polish, Hebrew, and most likely Yiddish. Lewin was brought up in accordance with Jewish traditions (Lewin, 1992, p. 16).

In 1933 he wrote a letter describing his hometown to Wolfgang Köhler – the head of the psychology department in Berlin, fellow Gestaltist and outspoken anti-fascist. The letter was never sent and was discovered only after Lewin's death. The exact reasons why Lewin kept the letter remain unclear; it is, however, likely that the political situation in Germany played a role in his decision not to send it off. In the letter, Lewin writes:

> I don't know whether you realize to what extent the social exclusion of Jews and their forced restriction to very few professions existed before the war. My parents were among those few Jews who owned a farm. I, therefore, know that 100% anti-Semitism of the coarsest type was taken for granted and constituted the basic stance not only of the landed aristocracy, but also of the peasants in the surrounding area. (Lewin, 1986, p. 42)

As was the case for so many Jewish intellectuals of the time, Anti-Semitism was a constant struggle for Lewin, not only in his early years in Germany but also in his later life in the United States.

In 1905 the family relocated to Berlin, where Lewin attended the Kaiserin-Augusta-Gymnasium (today Ludwig-Cauer-Grundschule) and received the *Abitur* (the German high school diploma) in 1908. In 1909 Lewin enrolled as a medical student at the University of Freiburg but quickly moved to Munich to attend Aloys Fischer's classes on psychological topics. In the summer of 1910, Lewin moved back to Berlin to study with Rudolf Wirchow, Ernst Cassirer, and, most importantly, the neo-Kantian Carl Stumpf, who later supervised his Ph.D. thesis. With the beginning of World War I, Lewin enlisted in the army. In 1918, he was injured, discharged from the army, and honored with the iron cross. While in the military, Lewin defended his dissertation in 1916 and published his first article, (Lewin, 1917). During that time, he participated in a student group that organized so-called "*workingmen's courses*" to provide general education to the working class (Marrow, 1969, p. 6/7). It was in this group that he met Hedda and Karl Korsch. Karl Korsch later became an influential Marxist during the 1920s and worked on integrating humanist ideas and Marxism (Korsch & Gerlach, 1923/1966). With Korsch, Lewin wrote a critical paper on the mathematization of social psychology (Lewin & Korsch, 1939). Lewin also published an article on Frederick Taylor's concept of *scientific management,* which dominated labor management at the time, in Korsch's journal (Lewin, 1920). With Hans Rupp, Lewin worked on attitude measurement in the textile industry (Lewin & Rupp, 1928a, b). These early works and projects attest to Lewin's interest in labor and social justice issues. The fact that he picked up these issues

when he turned to Action Research later in his career seems logical rather than eclectic, as his student Ronald Lippitt claimed (Petzold, 1980a, p. 143).

To secure a position in academia, Lewin, however, dropped his applied research in favor of experimental psychology and problems of volition measurement (Lewin, 1916, 1922a, b). There is considerable evidence that this decision did not come lightly to Lewin (Métraux, 1992). The first version of his Habilitationsschrift (Lewin, 1921) – a second qualifying thesis required in Germany after the Ph.D., also called *venia legendi* (the right to teach) submitted in 1919 or 1920, was rejected by the Berlin department. The faculty thought it too philosophical and insisted on a more experimental approach. Lewin later passed his final exam to become a university professor based on an extended version of his dissertation (Métraux, 1983).

After passing the Habilitation, Lewin taught at the Berlin Institute, offering psychology and philosophy classes, and developed his field theory. Eventually, his work became known in the United States thanks to his American student Junius Flagg Brown who published a paper entitled "The methods of Professor Lewin in the psychology of action and affection" in the *Psychological Review* (Brown, 1929). Lewin's newfound fame brought him an invitation to the International Congress of Psychology at Yale University in New Haven the same year. Lewin left a considerable impression notwithstanding his lack of proficiency in the English language (Marrow, 1969, p. 51; Lück, 2001, p. 16).

In 1930, Lewin persuaded Donald K. Adams to translate "Der Übergang von der aristotelischen zur galileischen Denkweise in Biologie und Psychologie" (Lewin, 1931a). The paper had appeared in *Erkenntnis*, a leading philosophical journal. Adams had considerable difficulties with Lewin's writing style, which used many neologisms.[1] In 1931, Adams's translation appeared in the *Journal of General Psychology* with the title "The conflict between Aristotelian and Galileian modes of thought in contemporary psychology" (Lewin, 1931b). Lewin argued for psychology as idiographic science: Psychological laws, so he claimed, cannot be generated from studies with many subjects. Instead, the individual in its wholeness should take center stage in psychology. Shortly after this publication, Lewin was invited to serve as a visiting professor at Stanford University, which he accepted in 1932. Meanwhile, the situation for the Jewish population in Germany deteriorated to the point that Jewish professors and students became the subject of threats and, in some cases, physical violence.

With the end of the Fall semester at Stanford, Lewin took the long way around the globe back to Germany. He traveled across the Pacific and gave a talk in Japan. There he heard the news about the ongoing changes in Germany for the first time. The full meaning of the events was, however, not clear to him. Lewin only understood that Germany was not safe for him anymore when he met the German-speaking psychology professor and member of the cultural-historical

[1] See Adams interviewed by Marrow, specifically the creation of the term "valence," which translated the German "Aufforderungscharacter" and quickly became adopted by Tolman as a replacement for his concept of "demand value" and later even translated back to German as "Valenz" (Marrow, 1969, pp. 56–57).

school Alexander Luria in Moscow. Stuck in-between worlds, Lewin contacted friends Fritz Heider at Smith College and Donald MacKinnon at Harvard to acquire a teaching position (Marrow, 1969, p. 68). Eventually, Lewin joined Cornell University in August 1933. A fund from the Emergency Committee on Displaced Scholars in a corporation with the Rockefeller Foundation financed his position. Robert M. Odgen, widely considered the first American Gestaltist and Dean of Arts and Sciences at Cornell, had arranged for the grant to bring Lewin back to the United States, as he had for Gestaltists Koffka, Wertheimer, and Köhler before (Henle, 1984). Lewin stayed at Cornell for two years. During the summers of 1934 and 1935, he translated the *Principles of Topological Psychology* with the help of Fritz and Grace Heider (Lewin, 1936a). In the book, which he describes as the unfinished product of "a very long growth" (Lewin, 1936a, p. vii), Lewin presents his concept of a *Topological Psychology*, an approach to understanding human (inter)action based on his field theory. Lewin writes:

> The person is to be represented as a connected region which is separated from the environment by a Jordan curve. Within this region there are part regions. One can begin by distinguishing as such parts the "inner-personal" regions … from the motor and perceptual region …. The motor and perceptual region has the position of a boundary-zone between the inner-personal regions and the environment. (Lewin, 1936a, p.177)

At Cornell, Lewin divided his time between theoretical issues in psychology and practical application, like he did during his early days in Berlin. He was also involved with the foundation of a psychology institute at the Hebrew University in Jerusalem and strongly considered relocating to Palestine as the chair of the institute to be inaugurated. The dedication of *Principles of Topological Psychology* speaks to this fact: "I dedicate this book to a young scientific center at the meeting of the East and the West where I hope new productive collectives will arise" (Lewin, 1936a, p. ix}). As it turns out, Lewin was already in contact with officials from Hebrew University before he left Berlin for Cornell University and had discussed the possibility of a professorship there. This is indicated by a letter Lewin sent to Cyrus Adler (Dropsey College Philadelphia) on January 6, 1934 (Lewin, 1934). At the same time, university officials from Hebrew University were in contact with Sigmund Freud, who apparently dismissed the idea of hiring Lewin in favor of his friend (and fellow psychoanalyst) Max Eitingon. Judah Magnes informed Freud in a letter dated November 27, 1933, that "the University is considering the establishment of such a Chair [… with] Professor Kurt Lewin of Berlin being especially in mind" (Freud et al., 1954, p. 315). Freud did not reply to Magnes directly but instead took matters into his own hands and contacted the Hebrew University chancellor directly:

> I see no reason to assume that Professor Kurt Lewin will be the man to carry out the synthesis of psychoanalysis and psychology. Under these circumstances, the plan to establish a chair for psychology indicates a barely disguised rejection of psychoanalysis and the University of Jerusalem would thus have followed the example of other official teaching institutions. (Freud et al., 1954, p. 316)

Despite Freud's intervention, Lewin was offered a position at Hebrew University on February 5, 1934. Even though he must have seriously considered the offer, the conditions would not have allowed him to pursue his research agenda. No institute would be established, and his salary would have been only one-fourth of his payment during his sabbatical in Stanford (Lewin, 1934). Surely not without sadness, Lewin declined the position. He remained interested in a move to Hebrew University, though he visited Palestine in the summer of 1934 and subsequently tried to secure funds for a psychology institute at Hebrew University. Plans had gone far; in fact, Lewin was already listed on the academic catalog of the university (Bargal, 1998, p. 68), but the money did not come through. During February 1935, Lewin wrote two papers – both unpublished but available in the Archives of the History of American Psychology at the University of Acron (Lewin, 1935a, b) – in which he outlined his ideas for research in Palestine in general and for the envisioned psychology institute in particular:

> A Department of Psychology in the Hebrew University is an essential unit of the cultural, educational and practical services of the University to the people of the land of Israel and to Jews everywhere. … The planned Department will engage in teaching, research and other services. It will be directly linked with the work of the University and of the land in education, in mental hygiene, in medicine and in child guidance. Its research work will begin by concentrating upon the psychological problems connected with immigration, social adaptation and the processes by which Jews from different parts of the world integrate into a single cultural community. … The material for psychological research in Palestine is in many ways unparalleled in other parts of the world. Because Palestine is a concentrated area in which there come together a great variety of people of diverse and conflicting cultural backgrounds, it will be possible to undertake fundamental studies in certain problems of social fusion and adjustment which are characteristic of social and cultural history everywhere. The studies would pay special attention to the character of contacts and conflicts in the settlements; villages and towns, the obstructions and facilitations of cooperation and the new formations resulting from these processes. (Lewin, 1935b, pp. 1–2)

The second paper offers more detail on Lewin's plans for the institute:

> What gives such a study in Palestine a special scientific interest is the circumstance that Palestine does not now offer a well established cultural and social atmosphere. Unlike adaptation in other countries, adaptation in Palestine is therefore not mainly acculturation. The number of immigrants is especially large (about 40,000 Jewish immigrants this year as against 250.000 Jewish inhabitants). Thus quite heterogeneous groups are entering a country quite fluid and chaotic in its social and cultural structure. This situation provides the historically rare condition for studying the growth of a new cultural unity under relatively autochthonous circumstances. (Lewin, 1935a, p. 2)

Lewin suggests closely examining several topics: The relationship between immigrants from different countries, the lack of a common language, the impact of a more collectivist society in the kibbutzim on immigrants with more individualistic cultural backgrounds, and, among others, the influence of ideology – specifically Zionism – on the individual. Such a broad array of questions in Lewin's mind had to be approached with a similarly rich set of methods:

> Technically, the greatest success, should in my judgment follow a combination of statistical methods with case studies. In addition to various tests, psychological experiments in the

> narrower sense of the term should be set up. l personally find especially intriguing endeavours to coordinate with the field-studies experimental studies of the factors that condition the opinions and transformation of ideologies; of the effects of cultural and social homogeneity and non-homogeneity on the structure of a group; of the effect of cultural differences on work; etc. (Lewin, 1935b, p. 4)

These descriptions carry the seeds of Action Research. However, the plans for Hebrew University did not work out. External funding did not come through, and the university could not provide the salary Lewin asked for, let alone the research funds for Lewin's projects. In 1939 Joseph Bonaventura was appointed Professor of Psychology at the Hebrew University. According to the university archives, however, he did not teach psychology but focused on education. It took until 1941 for psychology to become a secondary field of specialization for BA students, and experimental psychology did not exist until the end of World War II (Bargal, 1998, p. 65). A psychology department was not established until 1957, ten years after Lewin's untimely death.

The negotiations with Hebrew University were, however, not entirely in vain. Specifically, Lawrence K. Frank, an officer with the Rockefeller philanthropies, wanted to keep Lewin in the United States and recommended him to George Stoddard, head of the Iowa Child Welfare Research Station (ICWRS) (Ash, 1992, p. 200; Marrow, 1969, p. 84; Bryson, 1998). In a letter to Robert Odgen, he wrote:

> I think it would be a very desirable arrangement, since the Iowa station could provide Lewin with exceptional facilities, equipment and personnel and a body of graduate students. I personally hope that he can stay in this country longer because of the value of his work for child research. (Frank, 1935)

Iowa's funding situation was much better compared to the situation in Palestine: $90,000 per year for the Center just from the Rockefeller Foundation (Ash, 1992, p. 201) compared to an overall university budget of £40,000 at Hebrew University (Bargal, 1998, p. 58). Also, ICWRS offered more freedom, given the institute's independence from the university administration. It was in the fall of 1935, after spending two years at Cornell University, that Lewin gave up on the idea of joining the faculty at Hebrew University (although there is some additional correspondence from 1937) and joined the faculty of the Child Welfare Research Station at the University of Iowa.

In his new position, Lewin gathered several students around him. Like his habits in Berlin, Lewin organized a weekly meeting called the *Quasselstrippe* – the hot air club or literary translated "chatter line" – where his students presented and discussed their research projects (Marrow, 1969, p. 88). Besides the *Quasselstrippe*, Lewin founded another group that met annually even long after his death; the last meeting took place in 1964 (Marrow, 1969, pp. 111–115). The so-called *Topology Group* first met at Smith College in Northampton in 1933, where the Gestaltist Kurt Koffka had a professorship. The inaugurating members were Lewin's colleagues from the Berlin days. In its third year, the *Topology Group* had grown into a hub for some of the most influential thinkers in psychology and the social sciences: Donald K. Adams, Karl E. Zener, Edward C. Tolman, David Krech, Tamara Dembo, Erik

Erikson, Lawrence Frank, Fritz Heider, Grace Heider, Wolfgang Köhler, Donald W. MacKinnon, Margret Mead, William Stern, and others.

While Lewin was still focused on theoretical problems in psychology during his first years in Iowa, he also worked on more applied and political ideas. In his 1936 paper, "Some social-psychological differences between the United States and Germany," (Lewin, 1936b) he questions the American concept of *Personality* and the German concept of *Character* (Danziger, 1994, p. ch. 10). Instead, he explains the differences between Germans and US Americans based on his field theory and the concept of life space. One year earlier, Lewin's first publication on groups and minority research had appeared in the same journal. There, he expands his concept of *life space* to *social space*, indicating his growing interest in social psychology (Lewin, 1935c). At the same time, his main concern was still theoretical in nature. He wanted to formalize psychology according to mathematical principles. The fruits of these efforts appeared as "The conceptual representation and the measurement of psychological forces" (Lewin, 1938b).

Most of his other publications during that time followed a similar trajectory. In 1939 he published a paper together with Karl Korsch. They describe the task of psychology on three levels: *experimental research*, *formalization and mathematization*, *and inventing dynamic constructs*. The most significant issue for Lewin was to "find a mathematization which adequately represents this dynamic interdependence between psychological processes" (Lewin & Korsch, 1939, p. 398). Yet another article published in the *American Journal of Sociology* that same year advanced a formalized approach to the social world. Lewin writes:

> [The commensurability of various approaches] can be accomplished by using constructs that characterize objects and events in terms of interdependence rather than phenotypical similarity or dissimilarity. It may seem that emphasizing interdependence will make the problem of classification even more difficult because, generally, it is more difficult to describe a fact in terms of its effect on others and its being affected by others (its conditional-genetic properties) than in terms of its appearance (phenotypical properties). However, as soon as one grasps the idea, it becomes evident that if one characterizes an object or event by the way it affects the situation, every type of fact is placed on the same level and becomes interrelated to any other fact which affects the situation. (Lewin, 1939, p. 888)

In 1940, Lewin published an article in the *University of Iowa Studies in Child Welfare* entitled "Formalization and Progress in Psychology." Metaphorically laden, he outlines his idea of psychological research:

> [T]o make oneself master of the forces of this vast scientific continent, one has to fulfill a rather peculiar task. The ultimate goal is to establish a network of highways and superhighways so that any important point may be linked easily with any other. This network of highways will have to be adapted to the natural topography of the country and will thus itself be a mirror of its structure and of the position of its resources. (Lewin, 1940, p. 11)

The underlying notion of Lewin's concept of science clearly draws on Cassirer and the German neo-Kantian tradition.

Besides the more epistemological discussions, Lewin was also active in experimental psychology during his Iowa years. He investigated the effects of frustration and regression on emotional and intellectual behavior with his students Barker and

Dembo (Barker et al., 1941). The well-known experiments on autocracy and democracy were conducted during the same period (Lewin et al., 1939; Lippitt, 1940; White & Lippitt, 1960).

The democracy/authority studies significantly influenced Lewin's research trajectory. His turn to the social world and to groups, which had already started with his paper on minority research in 1935 (Lewin, 1935c), unfolded entirely as he lost interest in the individual or the relationship of the individual to groups – although the latter continued to play some role. Lewin instead became more interested in the relationship of groups to groups. This is indicated most clearly in a preliminary note on the Autocracy/Democracy project published in 1938:

> However, one should break away from the rather narrow aspect of studying the effect of the group influence on the individual (e.g., the effect of various groups on the suggestibility of the individual) as the main problem; one should consider not only one effect of a given social situation (e. g. the influenceon productivity). Rather one should try to approach an experimental procedure: (1) where group life can proceed freely: (2) where the total group behavior, its structure and development can be registered. (Lewin, 1938a, p. 292)

This could indeed be read as a departure from the dominant discourse in American psychology at the time toward a more transdisciplinary and action-oriented approach. However, Lewin also started to adopt more concepts from American psychology. For example, the studies on frustration and regression, which continued the Berlin studies on action and emotion, did not stress the theory of action and emotion but relied on behavioral theory instead. Maybe because of Roger Barker's influence, who came as a postdoc from Stanford University, where he had been educated in psychological measurement and intelligence testing (Barker, 1979, pp. 2140–2149), Lewin adopted correlational matrixes to illustrate his findings (Barker et al., 1943). This starkly contrasts his earlier critique of Aristotelian science as a method of evaluating similarities. Lewin had always favored a Galilean approach that investigated actual connections between phenomena (Lewin, 1931b).

Beyond academia, Lewin was also politically active at the time. Like so many, he was concerned with the situation of the Jewish population in Germany. And like many other refugees, he had to leave friends and relatives behind: His mother still resided in the Netherlands. In 1939, shortly before the outbreak of World War II, he wrote a clairvoyant paper. In "When facing danger," Lewin writes that "those who are interested in democracy realize that there is but one of two alternatives, either to live as slaves under Fascism, or to be ready to die for democracy" (Lewin, 1997, p. 116). The fate of the Jews in Europe and elsewhere and the political developments in general in the northern hemisphere are directly related to Lewin's research. The study of the interaction of groups with other groups, specifically the interaction patterns between minority and majority groups, becomes the center of Lewin's attention. He writes:

> If it has ever been a question whether the Jewish problem is an individual or a social one, a clear-cut answer was provided by the S. A. in the streets of Vienna who beat with steel rods any Jew irrespective of his past conduct or status. Jews all over the world now recognize that the Jewish problem is a social problem. Thus we will have to turn to sociology and social psychology, if we wish to get scientific help for its solution. (Lewin, 1997, p. 117)

Also, Lewin realized that change could not be brought about by changing the minority or majority alone. Instead, it "should be understood that any underprivileged minority is preserved as such by the more privileged majority" (Lewin, 1997, p. 117). There is a "need of the majority for a scapegoat." This scapegoat "grows out of tension, e.g., from an economic depression. … No 'logical' argument will destroy these basic forces" (Lewin, 1997, p. 118). Lewin realized that the problem of the Jewish people and, more broadly, the problem of minorities could not be single-handedly resolved by science proper. Instead, "the Jew will have to realize that for him (sic) as well as for any other underprivileged group the following statement holds: Only the efforts of the group itself will achieve the emancipation of the group" (Lewin, 1997, p. 118). As such, Lewin believed that research needs to invest in the strengths and inner cohesion of the group because such a "group will have an organic life of its own. It will show organization and inner strength." In contrast, a "minority kept together only from the outside is in itself chaotic. It is composed of a mass of individuals without inner relations with each other, a group unorganized and weak" (Lewin, 1997, p. 119). The emancipation of a group "would be more than a self-centered act. It would have a direct bearing upon the struggle of the majority for the solution of their economic and political problems" (Lewin, 1997, p. 121).

Lewin's aspiration to contribute to social change as a researcher in social psychology did not remain on the theoretical level. *The Food Habits Study* was the first in a series of investigations that would eventually lead to the development of the Action Research concept. In 1941, M. L. Wilson, then Director of Extension in the U.S. Department of Agriculture and chairman of the Federal Interdepartmental Nutrition Coordinating Committee (Wilson, 1942, 1943), asked Margret Mead to serve as secretary for the Committee on Food Habits of the National Research Council (Guthe, 1943, p. 16). Mead knew Lewin by way of the Topology Group and decided to bring him in. The Committee was established in December 1940 alongside the Committee on Food and Nutrition. While the latter's primary purpose was to determine what a healthy diet would consist of, the former focused on "finding the most effective ways and means of adjusting habits to needs, of getting people to wish what they need" (Guthe, 1943, p. 10).

Margaret Mead initially favored approaching the topic from the viewpoint of cultural anthropology. Still, she quickly realized that facing World War II, studies like these would be too expansive and time-consuming. Instead, she approached Lewin, who "developed a test based upon intensive interviewing, which … can be used rapidly with groups … in any area" (Mead, 1943, p. 22). The Food Habits studies carried out by Lewin addressed two questions: (1) Why do people eat what they eat? and (2) what methods can be used to change these habits? Lewin issued a questionnaire about food habits to 2300 school children and conducted qualitative interviews with housewives. To change food habits, Lewin compared three approaches: (1) the effects of group decisions versus (2) education (in the form of a lecture) and (3) a simple request to change food habits in the face of societal needs during the war. The studies yielded two significant results. Lewin showed that the food habits of a family depended on a gatekeeper. It was the housewives who decided what to

buy and what to cook, a somewhat surprising result at the time. Lewin had expected the head of the household – in most instances, the father – to decide the meal plan. Also, it turned out that group decisions were much more effective in changing actual food habits than information brochures or lectures given by an expert.

The combined insights of the Food Habits study and the Autocracy/Democracy project were mirrored in Lewin's first Action Research project at a new manufacturing plant of the Harwood Manufacturing Corporation in Marion, Virginia, in 1939. Lewin was invited for a three-day visit to the company by Alfred J. Marrow, grandson of the founder of the company founder. Marrow and Lewin had known each other since 1934 when Marrow asked Lewin for advice on his Ph.D. project (Burnes, 2007, p. 215). The plant had approximately 300 employees and produced pajamas. The problems at the plant were multiple: The factory was opened in a region previously shaped mainly by agriculture. The newly hired workers – mostly female – had no experience with factory work. And though they were highly motivated, they couldn't meet the production standards of similar plants in the industrialized north. After twelve weeks of training, the trainees produced only half the output of the overall company standards (Marrow, 1969, p. 141). Increased supervisor pressure did not increase outputs. Instead, people were quitting their jobs. In a time when the ideas of Frederick Taylor, as introduced in 1911 (Taylor, 1911/1919), were still predominant, the management soon ran out of ideas to increase productivity. Lewin came in and held problem-solving sessions with the administrators. He suggested several changes in how the workers were approached: (1) Employees should not be approached separately. Instead, management requests should always be addressed to small groups, thus putting pressure on several individuals instead of one. (2) Lewin specifically suggested giving goals to the workers that they actually felt were realistically achievable instead of simply giving out the goal of reaching the known company-wide standards. (3) Sixty highly skilled workers from another community were hired to train the employees.

Lewin brought his graduate student, Alex Bavelas, in, who started a series of quasi-experiments based on the assumptions of the Food Habits study and the Autocracy-Democracy studies conducted earlier. The baseline assumption for the experiments at Marion was that increased participation and democracy on the production level would increase productivity and output. During the seven years Lewin cooperated with the Harwood Manufacturing company (until he died in 1947), three of his students were responsible for the research process: First Bavelas, who was later succeeded by John R. P. French Jr., and eventually Lester Coch. The most important experiments were concerned with group decisions, self-management (both conducted by Bavelas), leadership training, changing stereotypes (both undertaken by French), and overcoming resistance to change (conducted by French and Coch). These experiments and the research conducted at the Harwood Manufacturing Corporation in Marion in general – which continued decades after Lewin's death under the direction of Alfred Marrow – represent the birth hour of Action Research in Industry as well as research on organizational change (Burnes, 2007, pp. 213–214).

While research at Marion was underway, Lewin also supported the US government in their efforts to win the Second World War. According to Marrow, Lewin was researching questions such as

> What was the state of morale and its probable future course both in enemy countries and on the home front? What techniques of psychological warfare would most effectively weaken the enemy's will to resist? What kind of leadership in military units was likely to be the most successful? How could more such leaders be found and trained? How could home-front consumption of foods in short supply be cut back and the use of more available foods be encouraged? How did human relations in office and factory affect war production in America's industries? What measures could be taken to care for and psychologically rehabilitate those injured in combat? (Marrow, 1969, p. 154)

There is some indication that Lewin himself applied for a job with the government to help the war efforts. According to his FBI file, he applied for a position with the Foreign Broadcast Monitoring Service, of which fellow social psychologist Goodwin Watson was the director. His application was, however, denied. Lewin did not seem to be trustworthy, given his German background. The Office of Strategic Services (OSS), a predecessor to the Central Intelligence Agency (CIA) (Cooke, 2007, p. 449), had a different judgment of Lewin. He worked for them until January 1945 (Marrow, 1969, p. 180). The starting date of his employment and his exact occupation, including the location of his office (it was located somewhere in Washington D.C.), are classified to this date. The last appeal to be granted access to Lewin's files at the CIA was made by Bill Cooke in 2006 and denied (Cooke, 2007, p. 453).

Even though little is known about his work for the OSS, it surely brought Lewin into contact with many people working outside of academia. It pushed his interest in concrete social issues. Inspired by the experience, Lewin began to realize that research on topics such as group dynamics (Lewin et al., 1939), social change (Lewin, 1943a), and action research (Lewin, 1943b, 1946), which he felt more and more inclined to pursue, were very limited in Iowa. Founding his research center, attached to a university but not integrated and not dependent upon university resources, seemed like the next logical step. Lewin started to apply for funding and, in 1943, was able to secure the Field Foundation as a first sponsor. In the spring of 1944, the American Jewish Congress made funds available for a research center concerned with minority problems and antisemitism. Irving Miller, then vice-president of the congress, approached Lewin concerning such a project. He promised a million dollars for a new research center, back then, a considerable amount of money and way more than what Lewin had managed to secure so far. The Field Foundation had promised a mere $30,000 for the first two years (Marrow, 1969, pp. 161–162). Surprised by his success, Lewin founded two centers instead of one: In the first months of 1945, Lewin established the Research Center for Group Dynamics at M.I.T. and the Commission on Community Interrelations (CCI) of the American Jewish Congress located in New York City.

Naturally, Lewin was interested in combining the work of the two institutes but with slightly different foci. Most of his students, such as Lippitt, Radke, Festinger, French Jr., and Cartwright, were involved in both projects (Marrow, 1969, p. 173).

Lewin also kept close relationships with Douglas McGregor and Charles Myers at M.I.T. and Henry Murray and Gordon Allport at Harvard. The Research Center for Group Dynamics was mainly concerned with positive and negative forces at work in human groups. Lewin himself described the goals and research prospects of the center in an article published in Moreno's *Sociometry* in 1945.

The Center should investigate "the forces which bring about change or which resist change." He adamants that "all aspects of group life would have to be taken into consideration." Such consideration should be enacted without reproducing existing stereotypes: "the study of group life should be independent of the way in which society is accustomed to classify a particular group phenomenon." To achieve that, Lewin envisions a "systematic scientific approach" for the Center which follows "comparative lines." This approach included the "use of whatever qualitative or quantitative psychological, sociological, or anthropological methods … are needed for investigation." This could only be achieved, so Lewin argued if theories are kept "abreast and partly ahead of the gathering of data" (Lewin, 1945a, pp. 130–131). As such, the Research Center for Group Dynamics was envisioned with a primary focus on theoretical development and empirical application to gain results that would explain the basics of group functioning (Lewin, 1947a, b).

The Commission on Community Interrelations, in contrast, had a strong focus on applying scientific knowledge to existing social issues. While Lewin would have liked the Commission to also focus on building theories and engaging in long-lasting developments of scientific knowledge, this was hardly ever possible because the American Jewish Congress (AJC) used the Commission as their tool to investigate ongoing struggles of public concern and had – as the provider of the funds – a great deal of influence on the decision process about the research topics being pursued. The AJC itself was initially established to support European Jews in America after World War I and to advocate civil rights and equality for Jews (Cherry & Borshuk, 1998, p. 121). This focus broadened, however, specifically during World War II when American liberals realized that fascism and racism became stronger in the United States as well. Gunnar Myrdal's study on race inequality in the United States was just one expression of that development (Myrdal et al., 1944; Jackson, 1994; McLean, 1946, p. 159). Following these results, "Jewish Congress leaders perceived the need for a more broadly based attack on discrimination and prejudice." Indeed, the American Jewish Congress' leadership adopted the principle of "'collective security'; an assault on anyone's constitutional rights was now just cause for the Jewish Congress to come to the defense of the injured party or group" (Frommer, 1978, p. 540). Such attacks on people's constitutional rights should now be contested not only in the courtroom but also with social science research. This new approach gave birth to the CCI in 1945. Its goal from the beginning was to use social science knowledge to improve the life circumstances not only of the Jewish population in the United States but of minority groups in general. A self-description published in the Weekly Congress, the leading Journal of the AJC, read like this:

> There are other organizations, Jewish and non-Jewish, engaged in this work. The question the layman is entitled to ask is: What innovation will the new Commission bring into the general strategy? The answer is knowledge of facts. In the struggle against anti-Semitism

> two fundamental issues are involved: the safety of Jewish life and – what is of greater importance to all people – the ability of democracies to build a better world. … What the Commission on Community Interrelations proposes to do is to acquire precise and thorough knowledge of facts and to proceed with action based upon the facts. (Lewin, 1945b, pp. 4–5)

The facts needed "to build a better world" were to be gathered scientifically. The aim was to "take the struggle against prejudice out of the realm of hope, faith, opinion, and guesswork and place it within the scope of scientific measurement and scientific fact" (Interrelations, 1945, pp. 3–4) while following the credo "no action without research, no research without action" (Lewin, 1945b, p. 5). For Lewin, the CCI offered a "new approach to old problems," and its methods could be the "infrared rays of social science" (Lewin, 1945c, p. 7). He hoped to effectively combine immediate action and long-term research to generate effective responses to social problems: "[W]e do not want that type of so-called 'realistic policy' which lives from day to day. … Any constructive plan must see both the long-range goal and the day-by-day action. It should see not only the local situation … but also the broader issues and social forces" (Lewin, 1945c, p. 6). In its years under the leadership of Kurt Lewin, the CCI was highly productive. Its main concerns at the time were discriminatory practices, actively responding to prejudice, and the education of Jewish-born children (Cherry & Borshuk, 1998, p. 132).

Three main projects were formed from these concerns: The Community Self-Survey, the Incident-Control Project, and the Jewish Self-Identification Project (Marrow, 1969, pp. 195–196). The CCI encompassed and continued Lewin's previous research on social issues. But the CCI could not maintain the pace for a very long time, mainly because of funding issues. Lewin's untimely death cut off CCI's leadership and took away the man who constantly acquired new funds. On top of that, there was an internal rivalry within the AJC. In parallel to the CCI, the AJC had established the Commission on Law and Social Action (CLSA), which grew out of the merger of two existing AJC commissions: The Commission on Law and Legislation and the Commission on Economic Discrimination in 1945 (Jackson Jr., 2000, p. 98). Lewin had initially hoped to incorporate the two existing commissions into his CCI efforts but failed with his proposal (Lewin, 1944a, b). The CLSA's primary focus was to counter prejudice and racism in the courtroom, a strategy that proved more effective with donors and the general public furor (Jackson Jr., 2000, p. 114).

Kurt Lewin was involved in yet another project, which came to bear on the future course of Action Research. In the summer of 1946, Lewin was asked by the Connecticut State Inter-Racial Commission to explore new ways in community leaders' training on problems of racial and religious prejudices. Lewin organized a workshop at Teachers' College, New Britain, Connecticut, where he would – together with his M.I.T. staff Lippitt, Gradford, and Benné (Marrow, 1969, pp. 210–211) – conduct what they called a *change experiment*. The goal was to produce a change in the prejudice structure of the participants and simultaneously record the causes of the change. Forty-one participants – professional educators or community workers – were invited to participate. Marrow, who was among the professional researchers involved in the workshop, reports that it was in this

workshop that Lewin realized the importance of participatory structures during the research process. The original setting for the workshop divided the day into meetings of the participants in the morning and afternoon and research staff meetings at night. But Lewin was asked by the participants whether they could sit in one of the researchers' meetings. According to Marrow, Lewin found these workshops to be particularly productive. He decided to repeat a similar event the following summer and acquired funding from the Office of Naval Research, with which he had been involved during his work for the OSS earlier.

New funding became necessary after both the AJC and the Connecticut State Inter-Racial Commission had decided to opt out of the project (Marrow, 1969, p. 150). The 1947 workshop was the first to carry the name National Training Laboratories in Group Development (NTL). The NTL was first headed by Leland P. Bradford, who had already participated in the 1946 workshops. He is one of the main protagonists in developing the T-Group concept (Benne et al., 1964) applied today in managerial training. Under Bradford's leadership, much of the original ideas, namely, countering religious and racial prejudices, were moved to the background in favor of delivering "powerful learning solutions for organizations, leaders, and practitioners who have a clear stake in accelerating individual, team, and organization effectiveness."

In the last year of his life, Lewin became involved with the Tavistock Institute in London, founded by Eric Trist, whom Lewin had met at Yale University, and the British psychologist A. T. M. Wilson. Together, they established the Journal of *Human Relations*. Lewin had initially planned to spend the summer of 1947 at the Tavistock Institute. On the evening of February 11, 1947, however, Lewin felt sick. The family doctor diagnosed a minor heart attack and advised Lewin to go to the hospital in the morning. But before Lewin could leave the house, another heart attack stroke him; this time, it was fatal.

Of all the psychologists forced to emigrate from Europe to the United States before and during World War II, Lewin was the only one to embark on a successful career and founded his school with a group of followers. His field theory, though not a leading theoretical concept in social psychology today, significantly contributed to the shift in American social psychology from a "race psychology" to the "studies of prejudice" (Samelson, 1978). Even though his concepts and ideas were stripped of the methodological framework and largely appropriated to the model of dependent and independent variables by his most prominent students (Festinger et al., 1950), his framework also contributed to the foundation of ecological psychology (Barker, 1968; Barker & Wright, 1954; Wright & Barker, 1967) and the development of Action Research, first at the Tavistock Institute and later throughout the world.

Moreno, Collier, Lewin

Despite the evidence in Jacob Levi Moreno's and John Collier's writings and except very few protagonists within the scientific community who reported Moreno's (Petzold, 1980a; Blake, 1954) and Collier's role in the development of the Action

Research (Pagès, 1974; Neilsen, 2006), Kurt Lewin is generally recognized as the inventor of the term as well as the originator of the basic framework still applied by most researchers in the field today. While the claim that Lewin was the originator of Action Research proves to be false, there is, of course, considerable relevance to his ideas worthy of a thorough investigation. The ignorance of the contribution of Collier and, more importantly, of Moreno is not just a matter of more or less imprecise historiography of the field; it is inscribed into the development of Action Research in that Lewin became a universal founding figure, a natural originator of all the basic ideas. Few researchers, however, do acknowledge either Moreno's or Collier's contribution. Robert Blake describes the relationship between Lewinian and Morenoian thought as follows:

> [T]he significant thing is the difference between, not what they show in common. For example, you can search Moreno's work for concepts like "cultural island," force field, quasi-stationary processes and they will not be found. Furthermore, look at the historical figures to whom they both give credit. Moreno acknowledges the importance of the thinking done by Freud, Marx, Comte, Bergson, but these men are not important in Lewin's thinking. Lewin, on the other hand, refers to Galileo, Cassirer, Wertheimer, Kohler, etc., men whose contributions are not dealt with in detail by Moreno. I feel it would be unfortunate to confuse the paternity issue. Both men have made signal contributions to the development of social science, but in each case the contribution is relatively unique. … It is my personal conviction that Lewin could not have contributed what Moreno has given social science. But the opposite also holds. Moreno does not think in the same conceptual terms as Lewin did. (Blake, 1954, p. 87)

The same counts for Collier. He adds yet another perspective to Action Research, namely, a political dimension that is not motivated by a general aspiration toward a social revolution as in Moreno's case or improved functioning of society through science as exhibited in Lewin's work but an aim to improve the living conditions of a neglected or exploited (racial) minority.

As such, Blake offers a partial explanation as to why Lewin does not mention his apparent predecessors in the field: His idea of Action Research stems from a different logic of thought. Before reaching such a conclusion, one would, of course, speculate that Lewin was simply unaware of the writings of both Moreno and Collier. However, this seems to be highly unlikely, if not false altogether. In Moreno's case, connections to the Lewin school are fairly obvious. Many of Lewin's students, including Lippitt, Benne, Bradford, Bavelas, and Zander, worked with Moreno in the early 1940s and even published in Moreno's journals Sociometry and Sociometric Review. At the same time, Moreno is not mentioned in Alfred J. Marrow's otherwise highly detailed and comprehensive biography of Kurt Lewin (Marrow, 1969). Even more confusing from this perspective is the fact that Moreno was mentioned in Lewin's obituary, also written by Alfred Marrow:

> [Kurt Lewin] talked and listened with equal respect to all who spoke sincerely. I recall the day when I first introduced Dr. Lewin to Dr. Moreno [in 1935, M.D.]. Both recent arrivals, they had known of each other, but had never met. It was not long after the publication of Moreno's Who Shall Survive? and of Lewin's Dynamic Theory of Personality. Both men quickly found common ground. (Marrow, 1947, p. 211)

In Marrow's Lewin biography some twenty years later, there is only a short paragraph that refers to Moreno's work as influential. It appears displaced within the framework of the text, almost as if Marrow was forced to add it:

> Again, group psychotherapy strongly influenced the development of group dynamics. Although commonly associated with psychiatry, group psychotherapy did not actually come out of psychiatry and certainly is not limited to it today, as the formation and role of Alcoholics Anonymous, for example, makes evident. Much in group psychotherapy does not derive from medicine or traditional psychoanalysis. Practitioners seek new insights from research in other human sciences of man, particularly in social psychology. (Marrow, 1969, p. 167)

Yet another Lewin student, who also worked closely with Moreno, describes the relationship of his advisor to Action Research in the following terms:

> Before the Research Center for Group Dynamics at the Massachusetts Institute of Technology was founded in 1945, Kurt Lewin's career had little to do with what we call Action Research today. … He had not yet thought about the idea of intervention to produce change within the system. (Petzold, 1980a, p. 143)

Ronald Lippitt further explains that Action Research "was conducted many years before Kurt Lewin entered the field" (Petzold, 1980a, p. 143). Unfortunately, Lippitt is not explicitly referring to a specific person. However, given his – at least for a certain period of time – a close relationship with Jacob Levy Moreno, it seems clear that Moreno was at least one of the people he had in mind.

Moreno himself goes even further. For him, it is not only Action Research as a term he originally coined; he claims to be the inspirer of Lewin's whole project on group dynamics. Whether this is true or not, Lewin published his first results in the research of groups in Moreno's journal *Sociometry* (Lewin, 1938b). Other bits and pieces of evidence can be found: In a book edited by Kenneth Benne and Bozidar Muntyan, which collects contributions of Lewin, Bavelas, Lippitt, Zander, Bradford, and others, Moreno is explicitly mentioned in the Preface: "The editors make special acknowledgment to Dr. J. L. Moreno who has pioneered in the areas currently referred to as psychodrama, sociodrama, role-playing, action dynamics…." (1951, p. x). And in his Preludes to his autobiography, Moreno himself claims: "The Sociometric Institute served consciously or unconsciously as a model for the Research Center for Group Dynamics" (Moreno, 1955b, p. 95).

However, it seems that at some point, Lewin's students turned away from Moreno – personally and academically – which might, at least to some extent, be a consequence of Moreno's tendency to force his students to engage only with his ideas and work. At the same time, Lewin exhibited a more open advisor role and tried to learn from his students as much as they learned from him (Petzold, 1978, p. 210). According to his son Jonathan, Moreno described himself as suffering from "fierce independence and megalomania [which] would keep him an outsider [with] a desire that his ideas be maintained as they sprang from his mind, fearing that their purity would be distorted by others" (Moreno, 1989c, p. 4).

As for Collier, the situation is somewhat different. Collier did not work in academia then; he was engaged in politics as the Commissioner for Indian Affairs

appointed by President Franklin D. Roosevelt and became, after his resignation in 1945, president of the Institute of Ethnic Affairs in Washington. There is no connection between Moreno and Collier, and it seems plausible that at least Collier was not aware of Moreno's work, and there is no indication that the opposite does not hold as well. Lewin, in contrast, knew about Collier and even referenced Collier's work in his first article about action research:

> It will be crucial whether or not the policy of this country will follow what Raymond Kennedy has called international Jim Crow policy of the colonial empires. Are we ready to give up the policy followed in the Philippines and to regress when dealing with the United States' dependencies to that policy of exploitation which has made colonial imperialism the most hated institution all over the world. Or will we follow the philosophy which John Collier has developed in regard to the American Indians and which the Institute of Ethnic Affairs is proposing for the American dependencies. This is a pattern which leads gradually to independence, equality, and cooperation. (Lewin, 1946, pp. 45–46)

Collier mentions Lewin in his autobiography as well. For him, Lewin inspired him to found the Institute of Ethnic Affairs (Collier, 1963, pp. 356–357). Moreover, Collier decided to add a photograph of Lewin to his autobiography. The caption reads: "Dr. Kurt Lewin, the gestalt psychologist who founded social action research and group dynamics, an intimate friend of the author until his death in 1947" (Collier, 1963, p. 426). In contrast to the Moreno/Lewin relationship, there is considerable evidence that Collier and Lewin not only knew of each other's work but had, in fact, a close and personal relationship.

However, Collier seems to imply a difference between his work and Lewin's. He acknowledges his friend as the founder of what he calls social action research, a term he never used to describe his approach. While Collier and Lewin shared the same humanist worldview, both advocating democracy, it seems that their concrete approaches were different. Collier was not interested in establishing precise scientific methods, which adhere to a specific vocabulary. His goal was to improve the life circumstances of the Native American population. Lewin, in contrast, saw his empirical work as a means to establish just that: a reproducible, transparent, and objective scientific approach inspired by his previous work in social psychology. One might think of Lewin's approach to Action Research as "action research on action research, with the self-reflexive social psychologist leading the way" (Neilsen, 2006, p. 397).

From a historical point of view, Jacob Levy Moreno must be acknowledged as the originator of the term Action Research as well as several of the key concepts, which were later picked up by Lewin and his students. John Collier formed his own ideas about "action-research, research-action" independently from Moreno's work. His thoughts had an influence on Lewin's concepts, as is acknowledged by Lewin himself. It is, however, Kurt Lewin who first developed a thorough and elaborated concept of Action Research as a scientific method. As such, he is rightfully acknowledged in the community as an important thinker, even though the claim of him being the founder of the tradition proves to be false.

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Chapter 3
Pragmatic and Participatory Action Research – The Northern and Southern Traditions

The Tavistock Institute of Human Relations

When Kurt Lewin left Germany in August 1933, never to return, he first went to the University of Cambridge following an invitation by Frederick Bartlett. There, he first met Eric L. Trist, then a graduate student at the university working with Bartlett. Trist had come across Lewin's article about Aristotelian and Galilean modes of thought (Lewin, 1931a, b) and was thoroughly captured. In his autobiographical essay, Trist writes: "The last day [Lewin] was there was one of the high points of my life" (Trist, 1993, p. 5). A year later, when Lewin revisited Cambridge, he and Trist had the opportunity to discuss their research interests. Lewin's English had vastly improved, and *Principles of Topological Psychology* (Lewin, 1936) was in the translation process. Again, Trist was highly fascinated by Lewin's methodological ideas. In an Interview with Marrow, Trist describes his fascination: "I always look at books like Topological Psychology and the ideas of vectors and hodological space as the first drawings for a Michelangelo picture, the sketches being the design of a theory" (Marrow, 1969, p. 69). In 1936, Lewin and Trist met again at Yale University, where Trist was working on his Ph.D. with Edward Sapir, then head of the Anthropology Department. After Trist had finished his Ph.D. studies at Yale, he returned to the UK to join the British Army in the war against Germany. Being a psychologist by training, he was appointed to the *Tavistock Clinic*, an organization founded after World War I by Hugh Crichton-Miller, a Scottish psychologist who treated shell-shocked soldiers (Dicks, 1970, p. 3). After the war, the Clinic inaugurated a program to help rebuild society. Based on this program and with the help of a grant from the Rockefeller Foundation, the *Tavistock Institute for Human Relations* was first founded as a sub-division (in February 1946) of the clinic and later (in September 1947) as a separate entity (Marrow, 1969, p. 222, Trist & Murray, 1993). Shortly after the Institute was founded, the Journal of *Human Relations*, organized in conjunction with Lewin (Human Relations, 1947; Trist & Murray, 1993), who also

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contributed to the first articles (Lewin, 1947a, b), released its first issue. Lewin had planned to spend the academic year 1947/48 in London to intensify the bonds between his Center at M.I.T. and the Institute. Still, his untimely death prohibited further collaboration (Marrow, 1969, p. 223). The Institute was headed by psychologist Tommy Wilson and included research staff from anthropology, psychology, economics, education, and mathematics (1993, p. 7).

After the war, the British economy suffered from a severe crisis. The British Pound was devalued by 30.5% in September 1949 compared to the dollar to which it had kept a fixed rate during the war as part of the *Bretton Woods System* (Dawnay, 2001; Eichengreen, 2008). Devaluation became necessary because of a shortage of dollars in the UK (Clark, 2000, p. 24). Given this situation, the British government looked for cheap ways to stimulate economic growth. Productivity of the existing working force, specifically in export-orientated industries, was one goal to secure an inflow of dollars (Clark, 2000, p. 34). The *Committee on Industrial Productivity* was founded, and a *Human Factors Panel* was established, handing out grants to research institutions. The Tavistock Institute received research money and started two larger projects in industry: The *Glacier Metal Company Project* and the now famous project in the *British coal mining industry at the Haighmoor seam in South Yorkshire* (Trist & Bamforth, 1951).

The stage for the projects was set by the limits of Taylorism, which had become apparent in all attempts to improve production output. An increase in labor division and direct financial incentives based on the piece-rate system as the primary means of increasing efficiency had apparently lost their power. From this point of departure, the Glacier Metal Company Project investigated the consequences of dropping the piece-rate systems in favor of a fixed hourly pay rate. The Tavistock researchers were able to introduce some participatory methods, such as group discussions. The overall setup, however, was different from an Action Research project. Researchers appeared as consultants to the various levels of hierarchy and used their observations to conclusions about ongoing developments in the organization of production lines. They showed that discussions about concrete issues such as wage structures often covered conflicts related to the organizational structures and unuttered distrust among the various stakeholders involved (Jaques, 1951, 1993).

Like the Glacier Metal Company Project, the Coal Mining project was not primarily set up to utilize Action Research – even though participatory elements were applied, among other things.

It investigated productivity changes in the coal mining industry since its nationalization under the umbrella of the National Coal Board in 1946 (1). The study was carried out in a phenomenological manner drawing heavily on Lewin's field theory (Trist & Bamforth, 1951). The results showed that the concept of Taylorism as it was applied in the *longwall system of coal mining* (a specific technique of coal mining with heavy labor division) effectively decreased productivity. It also deprived the workers not only of their autonomy at work but also influenced their social life.

Lewin's work in industry tackled production issues from the perspective of process optimization. The Tavistock Institute included such factors as worker-to-worker relationship, hierarchy within the company and the appendant tensions, integrity of

the production process from the perspective of the worker, as well as the interplay of work and the greater social life context. As promising as the Tavistock's research seemed, its future failed to materialize. The funding initially provided by the Human Factors Panel of the Industrial Productivity Committee ran out after three years, and applications for extension were denied, indicating a lack of interest in pursuing Action Research projects by the government (Trist, 1993, p. 16, 1993, p. 8).

The Development in Norway

Action Research effectively got underway when the Tavistock Institute established contact with researchers in Norway. In many ways, the conditions in Norway for social science research in industry were favorable. Other projects were spawned by Lewin and the Tavistock Institute, of course, but Norway allowed for research on a national scale. When the country joined the European Economic Community (EEC), new markets for Norwegian-based companies opened. At the same time, the industry had to face many competitors at home. Productivity would now be measured against other European companies, not just rivals from the Norwegian market. At the same time, the *Arbeiderpartiet* (the Norwegian Labor Party) under prime ministers Einar Gerhardsen and Oscar Torp and specifically the Minister of Social Affairs Olav Bruvik were very well aware of the dangers of Taylorism, which was considered to be in strong opposition to the Norwegian culture of autonomy and self-reliance (Nordby, 1993).

Norway had entered comparatively late into industrialization, and elements of the traditional culture were still present. For example, the working day was structured around the traditional main meal in the late afternoon. The *skippertak* allowed workers to continue farming or fishing part-time while working in industry. In contrast to other countries, the *Trades Union Council (LO)* and the *Norwegian Confederation of Employers (NAF)* shared a history of productive collaboration and the kinds of transformational experiences of child labor. Large-scale barracking of workers typical for industrialization in the United States, the UK, and other countries was avoided by early welfare laws and agreements (Lieberman, 1972). In 1948, the government made workers' representatives mandatory on the board of every company wholly or partially owned by the state. Against this background, the Institute for Industrial Social Research at the Technical University of Norway, Trondheim, initiated a project on industrial democracy called the *ID Project* (Emery et al., 1969; Emery & Thorsrud, 1976) in 1962. The project was co-sponsored by the LO and the NAF (Emery et al., 1969, p. VIII). Both organizations also formed the Joint Research Committee to supervise the research process (Emery & Thorsrud, 1976, p. 3; Emery et al., 1969, p. 99). A cooperation with the Tavistock Institute of Human Relations was established to bring in their expertise. Eric Trist, who had left Tavistock and was now a Professor of Organizational Behavior and Social Ecology at the Wharton School, Pennsylvania, joined the research team (Trist, 1993, p. 19).

The ID Project in Norway was largely built on the experiences at Tavistock. As was the case with the Glacier Metal Company Project (Jaques, 1951, 1993); (1993), they introduced participation schemes for the workers beyond traditional representative structures. The explicit goal was "to promote organizational change that is not just humanizing but also democratizing" (Elden, 1983, p. 21). The initial project spanned over one decade and was split into two phases. The first phase investigated formal systems of participation (Emery et al., 1969), and the second focused on experiments and socio-technical changes to establish participatory structures (Emery & Thorsrud, 1976). The role of the shop floor worker was emphasized in particular since this level suffered the most from a lack of participation, specifically under taylorized conditions. The particularization of tasks imposed on the workers left no room for negotiations. Also, it forced supervisors and management into the role of control and coordination to uphold the entire system. To break this spiral, it seemed necessary to implement changes on the shop floor level first to liberate the confinements imposed on the higher levels of the company hierarchy.

A central element of the studies was the concept of "semiautonomous groups" (e.g., Herbst, 1974, p. 32), a concept directly derived from Lewin's work on group dynamics (Lewin, 1938, 1943). If work was to be reorganized from small, highly specialized, and repetitive tasks to a more inclusive process, it was necessary to coordinate several workers in one group. This group would be responsible for additional production tasks, such as process control and information handling. These new groups should be semiautonomous and handle a certain amount of changes while participating in the larger organization democratically.

Democratic participation was introduced as an essential concept and consequence of Lewin's work and the projects accomplished at Tavistock. As was shown by Jaques in the Glacier Metal Company Project (Jaques, 1951, 1993), relatively static hierarchies prove inefficient in handling change. By introducing autonomous, democratic structures, the ID project hoped to establish a form of organization that would not merely replace an old one but inhabit "reorganizational ability" to support a "self-sustaining process of organizational learning" (Emery et al., 1969, p. 2). These thoughts provided the grounds for what was later called *Socio-Technical Systems Design* (Trist, 1981). The ID project redefined *productivity* to include production output and the general ability of a company to adapt to changes in the mode of production as a function of input resources and external demand. Within this framework, a productive enterprise would create a "steady state" (Emery & Thorsrud, 1976, p. 5) – what Lewin called a "quasi-stationary equilibria" (Lewin, 1947a, p. 13) – by granting autonomy and selective interdependence to its employees.

The first phase of the study went underway in 1962. It was descriptive in nature and investigated existing participatory structures in Norway and Europe (Emery et al., 1969, p. Appendix III). The second phase had a strong participatory focus. Stemming from the underlying socio-technical approach, it seemed clear that different industries would need different participatory structures since different working techniques and machines allowed for different autonomy settings for particular work groups.

The project's first phase showed that existing participatory structures were largely inefficient. Workers' representatives on company boards could not bring their interests to bear. Most interestingly, this was not attributed to a failure of individual representatives nor a suppressive atmosphere created by the management. Inherent structural differences in how representatives were appointed and the general function of the board made it impossible to discuss workers' participation issues. Moreover, it became clear that *the workers* were not a unified group. Quite the contrary, because of their confinement in highly particularized tasks, no group cohesion or collaborative voice existed.

Phase two of the project tackled these issues and aimed at implementing democratic change at the workers' level. Four companies were chosen from a greater pool, each supposed to serve as an *exemplar case* for that particular branch of industry. The joint committee of LO and NAF made the final decisions based on national needs for improved productivity (Emery & Thorsrud, 1976, p. 28). The research took place at (1) *Christiana Spigerverk*, a wire drawing mill; at (2) *Hunsfos*, a paper-making and chemical pulp factory, (3) *NOBØ* in metal fabrication; and (4) *Norsk Hydro*, a hydro-electric company that already served as a site for interviews in the first phase of the ID project.

The socio-technical approach carried high hopes. Eric Trist and his colleagues hoped to transform more than concrete working conditions. The hope was to reshape society at large. While all efforts fell short compared to these goals, success on the empirical level was directly visible: semiautonomous groups were established, the general productivity and the quality of work and working life were increased, and the turn away from Tayloristic models became more manifest. As a result, Action Research projects along similar lines appeared in various other European countries and even in the United States WorkingOnTheQuality 1979.

However, the hopes for a general change in the modes of production throughout Norway did not manifest. The effectiveness of semiautonomous groups in labor organization did not provide enough momentum for other companies to follow the example. Also, the differences between the various companies, even in the same industry sector, were too large to simply transfer the results from one company to another. At the same time, STSD provided tight theoretical grounds, which prescribed the necessary steps to implement change. The concrete mechanisms as to why a large-scale change was not materialized were never systematically researched, allowing only for speculations about the failure in that particular area.

Toward the end of the 1970s, a new generation of researchers entered the scene and broke the theoretical unity of STSD in favor of what they called *local constructivism* (Gustavsen, 2008, p. 425). The idea was to create local theories applicable to the concrete case. Again thanks to governmental support, specifically the *Work Environment Act of 1977*, improvements in the social-psychological conditions of the workplace and democratization through local company-level training programs were a collaborative goal of all the stakeholders. Within this framework of local theorizing, the model of the *Search Conference* emerged (Gustavsen & Engelstad, 1986; Emery & Purser, 1996; Emery, 1999) and established a new theoretical framework, but not in the kind of rigid way as was the case for STSD. The central element

of the Search approach was dialogue, and this dialogue was supposed to include the opinions of all stakeholders involved.

Favorable conditions in Norway further backed this development: In 1982, the LO and the NAF, in reviewing their original agreement for the ID Project, decided on a supplementary which would organize financial support and professional advice for the planning of search conferences (Gustavsen & Engelstad, 1986, p. 106). The socio-technical design had lost its role as the primary theory in favor of more dialogue-oriented approaches. The idea of national example cases, which others in the industry would follow, was abandoned entirely. Instead, every Norwegian company was invited to apply for participation. Most importantly, the focus shifted from implementing democratic structures to providing the space for a search for ways to implement democratic structures. A single conference was not limited to one company to include a connecting element between the hierarchy levels of different companies.

The project was a success and a failure at the same time. While over 450 organizations participated in at least one search conference over a two-year period, not even 10% initiated actual change in their modes of production (Gustavsen, 1993). This was reflected in the project evaluation in 1991/1992, which documented the failure to produce large-scale structural change. Given the lack of success, the ID Project was eventually terminated. The LO and the *Confederation of Norwegian Enterprises (NHO)*, successor to the NAF, which had been closed in 1989, continued their cooperation in conjunction with the newly formed Research Council of Norway. A new cooperation agreement called *Employers-Workers Joint Action Program – Enterprise Development (HFB)* was initiated, and a new project called *Enterprise Development 2000 (ED 2000)* went underway in 1994 (Gustavsen et al. 2001, Gustavsen, 2004, 2008).

Under the newly established HFB, the LO and the NHO were now each responsible for 50% of the project funding. They also established a general secretary office at the HFB that oversaw the research process in the different projects. While the LO and the NHO had cooperated on research councils in the past, the HFB was a novelty, and this was the first time they incorporated an institution.

The new efforts connected with ED 2000 differed from the ID Project's first (STSD) and second (Search Conferences) phases in many significant ways. First, the primary focus was now placed on the cooperation of various companies instead of promoting change in single example enterprises as in the early phase of ID or discussing concrete change within a large number of companies as in the search conference tradition during the ID project. Networking, thus, became the primary target. In addition, there were structural changes to the coordination of the research process. While research in the ID Project was mainly coordinated by two research centers in Oslo and Trondheim, the new approach also targeted a networking process on the researcher's end. Local companies should connect with local research infrastructure interwoven within a greater network, abandoning the idea of highly specialized research centers. As such, diffusion should no longer be reached by implementing a theory about improvements derived from research in example cases but by expanding network connections. Accordingly, the main task of Action

Research would be to help to establish these networks. When the overall project went underway, seven projects – called modules – were started, covering the whole of Norway and forming ten different network configurations involving a total of close to 100 organizations and the same amount of professional researchers involved (Levin, 2002). After the project ended, an extensive evaluation documented the accomplishments. The evaluation showed the project to have been successful in many ways. Multiple connections between companies were established, interdependencies of companies in the production chain were better accounted for, research and industry managed to establish closer collaboration, and general productivity increased (2001). However, at the same time, theoretical unity was lost. Concrete fields were approached from the standpoint of individual research institutions deploying a multitude of methods, some of which were participatory and some only operating on the managerial level. An evaluation of the projects beyond the quantitative implications proved difficult. In essence, the researchers evaluated their approaches from their perspective, giving narrative accounts of success and failure in comparison to their goals (Bye et al., 2001; Colbjørnsen & Falkum, 2001; Elvemo et al., 2001; Fine, 2001; Hansen & Claussen, 2001; Hanssen-Bauer, 2001; Knudsen & Garmann Johnsen, 2001; Pålshaugen et al., 2001). Speaking in general terms, the ED 2000 project was successful on an economic level and produced a change of varying degrees for the workers involved.

Participatory Action Research

Largely independently and provoked by social and political concerns of anti-colonialism and economic development prevalent in Third World countries in the 1960s and 1970s, a tradition of action research developed, which would unknowingly revitalize some of the radicality expressed in people like Moreno and Collier. While Lewin either remained on the sidelines or was treated condescendingly, an entirely different interpretation of Marx, such as the one advocated by Lukács and Gramsci, was picked up by influential social workers, teachers, and community activists such as Paulo Freire in Brazil and Orlando Fals-Borda in Colombia. They adopted the focus on action and participation from the action research tradition – though only rarely explicitly – but radicalized its central attempt to improve work and life conditions through knowledge. This meant knowledge production was no longer identified with the search for truth. Instead, knowledge was tied to the demand for justice. A certain facticity of social oppression and unequal power relations was assumed, and conflict was considered an unavoidable necessity, given the apparent interests and hegemonic structures in place.

> Participatory researchers explicitly join one set of actors in a social system fragmented by conflicting interests. That choice is expensive, for their opponents often have more access to authority and resources. Participatory researchers seek "fundamental transformations" of societies, and the price may well conflict with existing authorities and resource holders. (Brown & Tandon, 1983, p. 287)

The significant choice of "within" or "outside" the system mattered for the justification and application of social science research and methodology.

> [S]ocial research was to be transformed from an abstract, detached, disinterested, and objective science conducted by outside experts into an emancipatory process centered on what Freire called conscientisation, where the poor were to become agents of social and political transformation aimed at creating just, peaceful, and democratic societies. (Kapoor & Jordan, 2009, pp. 16–17)

Paulo Freire and his colleagues in Latin America developed widely influential concepts for adult education among the urban and rural poor. Freire's dialogic approach to adult education engages individuals in critical analysis and organized action to improve their situations (Freire, 1970/1996, 1974/2005). In these dialogues, educators and students move toward a critical consciousness of the forces of oppression and the possibilities for liberation (Brown & Tandon, 1983, p. 279).

Similar principles of inquiry have been developed in Africa and Asia (Hall, 1981), mostly independently. Fals-Borda recalls the following major events in the 1970s that eventually led to the organization of the first Participatory Action Research Symposium in 1977.

- The birth of Bhoomi Sena (Land Army) in Maharashtra, India, with a peaceful-disobedience land take-over led by Kaluram, a social scientist who never finished school but helped in articulating the basic principles of Participatory Research (de Silva et al., 1979).
- The establishment of one of Colombia's first NGOs, the Rosca Foundation for Research and Social Action, founded by a group of social scientists who had quit university posts and were proceeding to co-operate with poor peasants and Indians organized to fight latifundia (Fals Borda, 1979).
- The completion of a five-year participant immersion project in Bunju village in Tanzania by Finnish scholar Marja-Liisa Swantz opened the gate to consider alternative ways of doing social research in Africa and other parts of the world (Swantz, 1970).
- The civil resistance, an underground organization in Brazil that facilitated reading, in manuscript form, Paulo Freire's classic work, Pedagogy of the Oppressed (Freire, 1970/1996), before it was published abroad during the same year. The exiled Paulo found an intellectual home at the lDAC Documentation Centre at the World Council of Churches, Geneva, Switzerland, with educators Rosisca and Miguel Darcy de Oliveira.
- Like in Brazil, in Mexico during the same year Guillermo Bonfil, and a group of colleagues led critical operations inside the National Autonomous University to revise the role of anthropology (Bonfil, 1970; Warman, 1970).
- Another of those critics, Rodolfo Stavenhagen, was in Geneva at the Institute of Labour Studies finishing his epoch-making essay on "Decolonialising applied social sciences" and getting ready to return to his country to found the innovative Institute for Popular Culture (Stavenhagen, 1971).

Spawned by these developments, the first Symposium of Participatory Action Research in 1977 laid the grounds for an international network of participatory researchers, which was officially formed in 1978 under the sponsorship of the International Council for Adult Education (ICAE) and the leadership of Budd Hall who has summarized the characteristics of participatory research as follows:

> Participatory research is … an integrated activity that combines social investigation, educational work, and action. … Some of the characteristics of the process include the following:
>
> – The problem originates in the community or workplace itself.
> – The ultimate goal … is fundamental structural transformation and the improvement of the lives of those involved.
> – The workplace or the community [is involved] in the control of the entire process.
> – The awareness in people of their abilities and resources [is strengthened] and mobilizing or organizing [is supported].
> – The term "researcher" can refer to both the community or workplace persons involved as well as those with specialized training.
> – [Outside researchers] are committed participants and learners in a process that leads to militancy rather than detachment (Hall, 1981, pp. 7–8).

The ICAE organized a Participatory Research network with nodes in Toronto, New Delhi, Dares-Salaam, Amsterdam, and Santiago de Chile and started to publish the influential participatory research magazine Convergence. Almost simultaneously, at Deakin University in Australia, a group of professors headed by Stephen Kemmis began to work with Yothu-Yindi Aborigines (Carr & Kemmis, 1986). Fals-Borda describes how already at the first Symposium, some people would privilege action over participation, while others would privilege participation over action. One result was to combine both with the formula "participatory action research" (PAR), but the abbreviation PR or even P(A)R would remain common, too (Fals Borda, 2001, p. 32).

Besides ICAE, the European Association of Development Research and Training (EADI) evolved in 1978 from the institutional basic needs approach to PR thanks to Marja-Liisa Swantz's Jipemoyo Project in Tanzania. In 1979 the United Nations Research Institute for Social Development (UNRISD) in Geneva started with anthropologists Andrew Pearse and Matthias Stiefel a comprehensive series of studies and publications on people's participation. The International Labour Organisation (ILO) and UNESCO did something similar with economist Anisur Rahman. The Research Committee on Social Practice and Social Transformation of the International Sociological Association opened a section on PR with the leadership of philosophers Peter Park and Michal Bodemann. PR organizations were established in South America (the Latin American Council for Adult Education) and Australia (the Action Learning, Action Research and Process Management Association (ALARMP)). Some of these organizations were initially founded through universities that also started their classes and programs. Thus, teaching at universities began in Massachusetts, Calgary, Cornell, Caracas, Dar-es Salaam, Campinas, Managua, Pemamabuco, Bath, and Deakin (Fals Borda, 2001, p. 35).

Besides the 1977 Symposium, another ten world congresses have been held often at academic centers. Many universities now accept graduation theses on PAR topics,

and some, such as Ithaca, Uppsala, Bath, and Melbourne, have introduced post-graduate programs in this field (Fals Borda, 2006, p. 354).

Some contributors and researchers describe what happened within these thirty to forty years as three periods or waves of Participatory Action Research. While the first wave was dominated by social and political concerns in South America, Africa, and Asia, the second wave marked the emergence of global awareness, which led to the increased adaptation of PAR projects to European and North American contexts. The latter development has given way to a certain convergence of action research approaches, which is either hailed from certain representatives (Fals Borda, 2006; McIntyre, 2007; McTaggart, 1997; 1991) or criticized as a form of co-optation of participants' knowledge for neoliberal processes of capitalist accumulation and, in turn, an increase of inequality by others (Brown & Tandon, 1983; Kapoor & Jordan, 2009).

While it is clear that these thirty to forty years led practitioners of PAR to engage in a variety of research projects in a variety of contexts, using a wide range of research practices that may be related to an equally wide range of political ideologies, the underlying tenet of each project is a joint decision to engage in individual and/or collective action in favor of marginalized groups. This decision expresses a certain willingness to face conflict.

In addition to the normative question of whether both traditions should possibly form part of a larger whole, another question is whether both traditions stem from similar concerns, research practices, and theoretical backgrounds that might have even mutually benefited from each other so that both traditions can form part of a larger whole.

It is undoubtedly true that the label participatory action research has been broadly used. But the researchers do not consider themselves Southerners, and their research does not share much overlap with participatory action research as its founders have conceived it. Thus, representatives like McIntyre, McTaggert, and Whyte, who refer to themselves as participatory action researchers, mainly share the emphasis on participation: They believe collectivity is key to any effective improvement of life conditions. However, the dimensionality of transformation and conflict seems almost absent, a fact which squarely positions them on the side of the Lewinian and Northern Action Research tradition. To better understand the differences between the traditions and derive from this investigation the tradition's concepts of subjectivity and democracy, I will focus on those who have advanced an Action Research approach distinct from the Northern tradition.

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Part II
Subjectivity, Desmocracy, and Action Research

The various approaches to Action Research all make an effort to produce change. Furthermore, there is an agreement about the tools for this change, namely, "research" to gain "knowledge" and participation of the people in the field in that research process. This participation is not merely functional but creative; people "bring themselves in" with their own opinions and ideas. The course of action is not determined by the researcher but is the product of a participatory process. However, the concrete conceptual and theoretical underpinnings, and thus the concrete procedures of bringing about such a change, differ widely. This chapter scrutinizes the action research concepts introduced so far concerning their understanding of "subjectivity" and "democracy." Why these two concepts? All approaches to Action Research aim to change a specific field by working with the people toward a "better" future. To discern this "change" and the "better" future implied therein achieved through and with the participants, questions like "Who is this change for?" "What structures will it promote?" "What are the underlying assumptions about people with which this change is supposed to be achieved?" are relevant on the empirical and conceptual levels.

I will analyze these questions in two ways. First: What are the core assumptions about the people with whom change is produced (their capabilities and needs), and what are the organizational principles of participation for the research project (total participation, majority vote, etc.). In other words: What are the underlying concepts of subjectivity and democracy in the various research traditions?

This chapter intends to answer both questions. In doing so, the various approaches will be closely examined concerning their theoretical underpinnings and the actual projects conducted. From this standpoint, subjectivity and democracy will be discerned and linked to the extent to which both contribute to the success and failure of the various projects. As will become apparent, the developments of the Action Research approaches came with changes in the respective concepts of subjectivity and democracy. Specifically noteworthy is the shift from a functional perspective in which both democracy and subjectivity are tools for a general improvement of the situation (Lewin) to subjectivity as a set of core needs of every human being and democracy, understood initially as the logical consequence of an attempt to satisfy

those needs (STSD) to democracy as a necessary and preexisting structure for subjectivity to unfold (Dialogue approaches).

Chapter 4
The Lewinian Tradition

Lewin and Subjectivity I: Lewin's Concept of Science

Kurt Lewin's student Ronald Lippitt claims that Lewin "had not thought about the idea of intervention to produce change within the system" before he started his first Action Research project in the mid-1940s (Petzold, 1980, p. 143). However, while Lewin had incorporated "change" into his research strategy before, the development toward a more political and more socially relevant research agenda was gradual. First, the two World Wars significantly changed psychology as a discipline. Before World War I, American academic psychology was strongly shaped by the leadership of Harvard University's experimental approach under German-born Hugo Münsterberg, whom William James hired in 1892 as the head of the Harvard psychology laboratories. Münsterberg died from a stroke in 1916 while giving a public lecture (Münsterberg, 1922) and was eventually succeeded by Edwin G. Boring, who had received his education at Cornell University under the guidance of Edward Bradford Titchener, a former Wundt student and strong advocate of experimental psychology (Boring, 1961). And indeed, experimental psychology was growing strongly in the United States: The total budget spent on experimental equipment in psychology grew from $30,000 in 1893 to over $1,000,000 in 1925 (Ruckmick, 1926). Boring, however, deviated from that line of thought when he got involved with the army intelligence testing program, where he worked with Robert M. Yerkes. Yerkes later became a professor at Yale University, where he co-founded the Institute of Human Relations (Morawski, 1986; Richardson, 1999). Both researchers were at the forefront of the development of applied psychology in the United States with the ideal of controlling and predicting human behavior. In the words of James McKeen Cattell, who delivered the opening address of the International Congress of Psychology at Yale University in 1929 in the presence of Lewin:

> The chief contribution of America to psychology has not been large philosophical generalizations, but the gradual accumulation from all sides of facts and methods that will ulti-

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> mately create a descriptive and of human nature and human behavior. … [The aim of psychology is] to describe, to understand and to control human conduct. (McKeen Cattell, 1930, pp. 21, 31)

The rise of applied psychology after World War I can be spelled out in numbers: Before the war, the main occupation of psychology graduates was university teaching and research. Less than one in ten members of the APA in 1916 held a position outside academia. By 1926, this number had doubled (Finch & Oderoff, 1939, 1941). In the same timeframe, the number of psychologists who reported teaching applied psychology grew from 11% to 29%, while the numbers for experimental psychology slightly declined (Fernberger, 1928). Experimental psychology now had a counterpart, which took place outside the well-controlled boundaries of the psychological laboratory. Within this framework, Lewin's work in the Food Habits study took the experimental methodology to real-world contexts.

Secondly, how Lewin approached this new research strategy is a product of his academic work dating back as early as 1917, notwithstanding a shift in Lewin's theoretical stance during his years in America. During his time in Germany and his first years in America, Lewin had strong affinities with the Left and was inspired by Marxist thought. When he began to develop Action Research, however, Lewin had become an advocate of American liberal democracy. This shift occurred not only in Lewin's thought. Still, it seemed to be a general phenomenon of the time and how events that shaped social psychology in the United States before, during, and after the Second World War were interpreted. The specific understanding of democracy and subjectivity shaped by these events in conjunction with Lewin's worldview before he arrived in the United States heavily influenced the conceptualization of Action Research shortly before his death in the mid-1940s.

Lewin's field theory as a psychological meta-theory (Lewin, 1936a) is partly a product of his prior training in phenomenology in Berlin under the guidance of his Ph.D. advisor Carl Stumpf (1992, p. 15) and his exposure to the American psychology of the time. Lewin's psychology offers a combination of phenomenology (Lewin, 1917), sociological meta-theory (Lewin, 1943b), empirical (that is also mathematical) rigor (Lewin, 1939b), and applied psychology (National Research Council, 1943). It is essential to understand these various parts of his theorizing to come to terms with his idea of democracy and subjectivity in Action Research and the developments in the field after Lewin's untimely death in 1947.

As such, for Lewin, psychology consisted of a tripartite system: theorizing, which for the psychologist was enriched by experimental research and applied psychology. The often quoted line "there is nothing so practical as a good theory" (Lewin, 1997b, p. 288) is frequently misread and seems to suggest that Lewin gave primacy to theory. However, the full quote reveals Lewin's critique of the "pure theoretician" in favor of a mutually constitutive relationship between theory and practice:

> It would be most unfortunate if the trend toward theoretical psychology were weakened by the necessity of dealing with natural groups when studying certain problems of social psychology. One should not be blind, however, to the fact that this development offers great opportunities as well as threats to theoretical psychology. The greatest handicap of applied

> psychology has been the fact that, without proper theoretical help, it had to follow the costly, inefficient, and limited method of trial and error. Many psychologists working today in an applied field are keenly aware of the need for close cooperation between theoretical and applied psychology. This can be accomplished in psychology, as it has been accomplished in physics, if the theorist does not look toward applied problems with highbrow aversion or with a fear of social problems, and if the applied psychologist realizes that there is nothing so practical as a good theory. (Lewin, 1997b, p. 288)

Lewin was well aware of the necessity to bridge the gap between theory and applied research and realized that a systematic approach to the problems of the social sciences could only be successful if it meaningfully combines theory and praxis. And this combination is, for Lewin, possible because "Kant in his Copernican Turn, transformed the question of 'is insight [Erkenntnis] possible' into 'how is insight [Erkenntnis] possible.'" For Lewin, it is the transformation from speculative science to phenomenological science, "a science which – instead of being based on a few presupposed axioms – takes its starting point in the concrete, existing [vorliegenden] objects" (Lewin, 1927, p. 375).

It is thus the phenomenologist Kurt Lewin who appears on the scene at an early point in time: In 1917, one year after he defended his dissertation (Lück, 2001, p. 14), Lewin published an article entitled Kriegslandschaft (The landscape of war) in the *Zeitschrift für angewandte Psychologie* (Journal of Applied Psychology). In this article, Lewin develops the distinction between facts and ideas derived from perception: "If you see a hill in front of you, it is also possible to see this hill as a curvature of an otherwise flat landscape. … The factual phenomenological reality of the landscape is changing" (Lewin, 1917, p. 440). Lewin points out that it is not the physical facts that solely define the psychological reality. It is the psychological reality that produces the facts. A change in perception from "hill" to "curvature in a flat landscape" does not mean that the previous picture disappears. Instead, Lewin argues that both perceptions exist and can be exchanged at any given moment. Reality, for Lewin, cannot be determined by the exclusive collection of physical facts, it is the psychological perception of reality which shapes it, and this perception is multifaceted not only for multiple subjects but varies within one person. The concrete form the perception takes is defined by the field the person is situated in. Having served as an infantryman for the German Army in World War I, he exemplifies his thoughts with a powerful description of his experiences when entering the landscape of war. "A landscape," he argues, "expands in all directions. … The landscape is circular, without end and beginning." However, this perception changes entirely when the landscape of peace is transformed into a landscape of war. Suddenly there appears to be "an end in front of you after which there is only nothingness" (Lewin, 1917, p. 441). The frontline becomes the field's border; the landscape suddenly appears vectored [Gerichtetheit] toward one central point: the frontline.

The changing landscape also changes the objects therein. Objects of peace [Friedensdinge] become objects of war [Kriegsdinge]: "To demand from an object in the war zone something you would normally only demand from an object of peace, is senseless" (Lewin, 1917, p. 444). The object's meaning and perception are

determined by the field in which it is located. "A destroyed village, for example, loses its horrors when located in the war zone. It becomes a war ensemble [Gefechtsgebilde] instead of a peace ensemble which is destroyed." The field inhabits its meaning and attributes meaning to objects present in the field. Subjects in the field are equally determined by this meaning, creating a perception cycle in which the subject produces meaning by perceiving the field, which in turn determines the perception of objects and the self-perception, which in turn determines the field: "The individual Soldier perceives himself as an object of war (Gefechtsgebilde)." When injured or commandeered to a different location, thereby facing his own not-participating-any-more [Nichtmehrmitmachen], "he suddenly and emphatically realizes that war is quite dangerous" (Lewin, 1917, p. 445). And if the battle moves on to a different location, "it is not only the frontline which is moved, on the contrary: Without realizing actual [eigentlich] change, where there was the war zone [Stellung] moments ago suddenly appears acre, grassland and so forth which are promptly connected to all the surrounding landscapes such as farmland and forests" (Lewin, 1917, p. 446).

For Lewin, the subject's perception is strongly connected and determined by the given field. Moreover, the subject itself and its self-perception are defined by this field. An object or a subject does not offer a meaning per se and cannot be studied as such. Instead, it is part of a "Gestalt in space [Raumgestalt]" (Lewin, 1917, p. 441), a wholeness as the starting point of psychological investigations.

This initial "wholeness" is central to Lewin's approach and can be related to Ernst Cassirer's work, with whom Lewin studied in Berlin (Lück, 2009, p. 83; Lewin, 1999f, p. 23). Lewin hoped to utilize Cassirer's philosophy of science for his approach to the social world (Lewin, 1931, 1947a, 1949). If it was Carl Stumpf and the phenomenological tradition that shaped Lewin's general approach to the world and perception (Bargal, 2006, p. 370), then clearly, it was Ernst Cassirer's work that strongly contributed to Lewin's understanding of science. Specifically, Cassirer's ideas of *Substanzbegriff* and *Funktionsbegriff* (Cassirer, 1910) – "the term of substance" and "the term of function" – translated to English simply as "Substance" and "Function" (Cassirer, 1910/1923) – proved to be invaluable concepts for Lewin.

In Substance and Function, Cassirer investigates knowledge production in the natural sciences and develops a concept of scientific progress. Lewin subscribes to these ideas and renders scientific development based on an interplay between the development of theory (function) and the acquisition of facts (substance) for "the term 'scientific development' refers to levels of scientific maturity" (Lewin, 1999a, p. 26). Lewin believes that Cassirer's model initially developed for mathematics and then broadened to encompass the, as Cassirer says, "exact sciences" (Cassirer, 1910/1923, p. iii), applies to and would indeed benefit the social sciences as well (Lewin, 1999a, p. 24). Cassirer thinks of science as a system. Stable facts can only be understood as part of a greater whole in which changes occur:

> [I]f it were true that exact proof were only possible of that which always maintains itself in the same form, then change could be tolerated as an auxiliary concept, but could not be used as an independent logical principle. … [B]ut this unchangeableness cannot be defined

> unless we understand, as its ideal background, certain fundamental changes in opposition to which it gains its validity. (Cassirer, 1910/1923, p. 90)

Cassirer holds that change within science produces new knowledge and stabilizes existing facts. Facts are validated within a system of change: "In truth, no physicist experiments and measures with the particular instrument that he has sensibly before his eyes; but he substitutes for it an ideal instrument in thought, from which all accidental defects … are excluded" (Cassirer, 1910/1923, p. 144). The general law, posed as a conclusion of an experiment is not meant to describe a single repetitive occurrence, but "the unity of a concept," which "has not been found in a fixed group of properties, but in the rule, which represents the mere diversity as a sequence of elements according to law" (Cassirer, 1910/1923, p. 148). These laws themselves are, in Cassirer's view, not meaningful in their relation to the given – which is subject to constant change – but only in their relation to other scientific laws:

> The meaning of [a scientific concept] cannot be comprehended, as long as we seek any sort of presentational correlate for it in the given; the meaning only appears when we recognize the concept as the expression of a pure relation, upon which rests the unity and continuous connection of the members of a manifold. … All that the "thing "of the popular view of the world loses in properties, it gains in relations; for it no longer remains isolated and dependent on itself alone, but is connected inseparably by logical threads with the totality of experience. (Cassirer, 1910/1923, p. 166)

This does not mean that Cassirer denies phenomena altogether; on the contrary, he sees the task of science as creating a better understanding of phenomena in the world. He opposes the existence of a direct connection between scientific concepts and the phenomena in the world in such a way that the phenomenon makes plausible or proves the concept. Instead, the concept gains its validity from its place within the logical structure of scientific description. It is proven by its consistent application in the face of the ongoing scientific change via discoveries and experimental evidence. From this viewpoint,

> matter and force, atom and ether, can no longer be misunderstood as so many new realities for investigation and realities whose inner essence is to be penetrated—when once they are recognized as instruments produced by thought for the purpose of comprehending the confusion of phenomena as an ordered and measurable whole. (Cassirer, 1910/1923, p. 166)

And the comprehension of these phenomena is based on an empirical approach. But for Cassirer, "the experiment never concerns the real case, as it lies before us here and now in all the wealth of its particular determinations, but the experiment rather concerns an ideal case, which we substitute for it" (Cassirer, 1910/1923, p. 254). This does not mean, however, that.

> [t]he individual case is … excluded from consideration, but is fixed and retained as a perfectly determinate step in a general process of change … of serial order. We do not isolate any abstract part whatever from the manifold before us, but we create for its members a definite relation by thinking of them as bound together by an inclusive law. (Cassirer, 1910/1923, p. 20)

Science studies is the connections between concepts: general laws but concerned with the individual case. And the juncture in history at which Cassirer sees this new methodology arising is Galilei, who opposed the Aristotelians not so much with finally displacing the earth from the center of the solar system (Galilei, 1632/1953) – most of the astronomical and mathematical groundwork for this was done by Kepler and Copernicus – but by developing his methodological framework for physics (Cassirer, 1910/1923, p. 354). It is not a coincidence that Lewin describes his methodology for psychology as "Galileian," which stands in stark contrast to what he calls the old "Aristotelian" view (Lewin, 1931).

In line with neo-Kantian ideals of "philosophy as a consultant to science" (Lewin, 1999a, p. 24), Lewin uses Cassirer's philosophy to develop his methodological approach. His general critique of psychology reflects that of William James:

> A string of raw facts; a little gossip and wrangle about opinions; a little classification and generalization on the mere descriptive level; a strong prejudice that we have states of mind, and that our brain conditions them: but not a single law in the sense in which physics shows us laws, not a single proposition from which any consequence can causally be deduced. We don't even know the terms between which the elementary laws would obtain if we had them. This is no science, it is only the hope of a science. (James, 1892, p. 468)

Lewin understands, however, that this problem cannot be resolved by additional data, as James had hoped for when he established the first experimental laboratory in the United States at Harvard in 1892. For Lewin, a methodological shift is needed. Following Cassirer's model of science as a system, he identifies the researcher's daily hassle with the "particular state of development of his science" as the major struggle. In empirical research, "methods have to be adjusted to the specific state of affairs at a given time" (Lewin, 1999a, p. 25), rendering "the basic character of science as the eternal attempt to go beyond what is regarded scientifically accessible" (Lewin, 1999a, p. 26), to take "the next step from the known into the jungle of the unknown" (Lewin, 1947a, p. 6). Progress thus always means to invent new methods and new theories that will at first inevitably be considered "unscientific" or "illogical" because "like social taboos, a scientific taboo is kept up not so much by a rational argument as by a common attitude among scientists: Any member of the scientific guild who does not strictly adhere to the taboo is looked upon as queer; he is suspected of not adhering to the scientific standards of critical thinking" (Lewin, 1999a, p. 28).

In many ways, Lewin's understanding of science and the scientific process anticipates what Thomas Kuhn explicated more than a decade later in The Structure of Scientific Revolutions (Kuhn, 1962). For Lewin, too, scientific progress starts with a new method, which is first thought of as being unscientific. However, unlike Kuhn, Lewin believed in a final revolution that would bring unity to the whole discipline or even the social sciences. Galilei achieved this for physics – in Cassirer's interpretation which Lewin follows – and clearly, Lewin felt that his field theory was the Galilean/Copernican revolution for the social sciences in "that it has shifted the previous logical constants …, that it has set them at another place than before" (Cassirer, 1910/1923, p. 373). The Aristotelian mode of thought, which in Lewin's view, dominated psychology in the early 1930s (Lewin, 1931, p. 158) – that is

before the publication of his field theory in 1936 (Lewin, 1936a) – but diminished later (Lewin, 1947a, b), is characterized as "anthropomorphic and inexact. … It classes many things with very slight or unimportant relationships together and separates things that objectively are closely and importantly related" (Lewin, 1931, p. 142).

For Lewin, distinctions in psychology such as "pathological" and "normal" or personality and social psychology are an expression of this Aristotelian, anthropomorphic view, since logically all these fields are heavily intertwined (Lewin, 1936a, pp. 5: "Psychology speaks of the 'errors' of children, of 'practice,' of 'forgetting,' thus classifying whole groups of processes according to the value of their products, instead of according to the nature of the psychological processes involved" (Lewin, 1931, #35351@143). In line with Cassirer, Lewin sees these definitions according to value as substantial concepts derived from a certain preexisting substance. They attempt to create a linkage between the scientific concept and the actual phenomenon, thereby failing to produce scientific advancement. Instead, this procedure creates piles of unrelated and inexact descriptions (Lewin, 1936a, p. 4). To avoid the accumulation of unrelated material, psychologists must "yield to a conception which seeks to derive the same laws for all these fields and to classify the whole field on the basis of other, essentially functional, differences" (Lewin, 1931, p. 144). "We are to return to the making of speculative 'systems' [to avoid] a blind collecting that splits the field of psychology into a number of unrelated branches" (Lewin, 1936a, p. 5). As such, a concept can be established by setting "[the previous logical constants] at another place than before" (Cassirer, 1910/1923, p. 373):

> The system of concepts capable of bringing together the different fields of psychology in an empirical manner would have to be rich and flexible enough to do justice to the enormous differences between the various events and organisms with which it must deal. It would therefore have to be oriented in two directions, namely, toward theoretical connectedness and toward concreteness. In other words it would have to be equally suitable for the representation of general laws and of the characteristics of the individual case. (Lewin, 1936a, p. 5)

Analog to the change process Cassirer attests in physics via the Galileian revolution, Lewin advocates for a change in psychology on the methodological level. The distinction between lawful behavior and chance – that is, the idea that parts of the variance are explained while others are random occurrences – must be reversed. Instead of looking for empirical instances that follow a scientific law, Lewin sets the law as the premise and argues that everything follows a particular law. Most importantly, this means that laws are not established because of a specific frequency of occurrences of the same instance. Instead, the single case must be considered lawful regardless of its occurrence frequency. While an Aristotelian mode of thought would define a law based on frequent occurrence, that is, from a historical perspective of actual events, the Galileian mode is not concerned with frequency at all. Instead, it is concerned with establishing, that is, finding the law for the particular occurrence in relation to other laws within the system of science. It is the task of psychology to find these laws in relation to the existing body of knowledge (Lewin, 1931, p. 150). In the same way, the nature of an object or construct under investigation cannot be

determined by its "membership in a certain conceptual class." Objects in the Galileian mode of thought are not determined by their classification but by their relation to each other (Lewin, 1931, p. 149). From this, one might conclude that the problem is to be found in the quantitative methods approach. However, Lewin is not arguing for a purely qualitative approach in psychology – the problem is not simply one of mathematization; it is how mathematics – that is, statistics – are applied in the Aristotelian mode: "Lawfulness is believed to be related to regularity, and considered the antithesis of the individual case." As such, psychology must decide whether an event is considered lawful, and the basis for lawfulness is a large n with a strong connection of investigated occurrences: "In terms of the current formula, lawfulness is conceived as a correlation approaching r = ±I." Since such a correlation is practically never achieved, psychological laws are considered "only regularly valid." Hence, a clear distinction is made between physics as the realm of actual laws and "psychological propositions," which only hold the status of "regularity" (Lewin, 1931, p. 154). This means, first of all, that psychology lacks exactness; "it is satisfied with setting forth mere regularities." The complexity of social life typically excuses this lack. Outliers and exceptional cases must be ignored to establish some scientific assertion: "[P]sychology does not regard exceptions as counterarguments so long as their frequency is not too great." (Lewin, 1931, p. 156).The reliance on frequency poses yet another problem: empirical data collection is historically and geographically determined (Lewin, 1936a, p. 30), i.e., it relies on data of past occurrences in specific locations.

> The determination of the cases to be placed in a statistical group is essentially on historic geographic grounds. For a group defined in historic-geographic terms, perhaps the one-year-old children of Vienna or New York in the year 1928, averages are calculated which are doubtless of the greatest significance to the historian or to the practical school man (sic) but which do not lose their dependence upon the "accidents" of the historic-geographic given even though one go [sic] on to an average of the children of Germany, of Europe, or of the whole world, or of a decade instead of a year. Such an extension of the geographic and historic basis does not do away with the specific dependence of this concept upon the frequency with which the individual cases occur within historically-geographically defined fields. (Lewin, 1931, p. 157)

For Lewin, "the content of a law cannot be determined by the calculation of averages of historically given cases." For one, this means that a law is produced that "applies to an 'average' situation. It is forgotten that there is no such thing as an 'average situation' any more than an average child" (Lewin, 1931, p. 172). Furthermore, the collected data are stripped from their historical specificity. From a Galilean perspective, historical frequency is a mere "accident." Consequently, the properties determined from this averaged frequency are not helpful to establish laws but merely a "matter of chance" (Lewin, 1931, p. 162).

To avoid such Aristotelian methodology, Lewin turns to the investigation of situational dynamics because "only by the concrete whole which comprises the object and the situation are the vectors which determine the dynamics of the event defined" (Lewin, 1931, p. 165). The Collection and recording of instances and the recording of those that fit a specific category becomes useless in this new framework. Instead,

to "comprehend the whole situation involved, with all its characteristics, as precisely as possible" becomes the goal of empirical investigations (Lewin, 1931, p. 166). Again, Lewin does not argue for qualitative over quantitative approaches, nor does he question the generalizability of empirical research results. On the contrary, "this step to the general is automatically and immediately given. … Instead of referencing the abstract average of as many historically given cases as possible, there is a reference to the full concreteness of the particular situations" (Lewin, 1931, p. 166).

From the Aristotelian perspective, psychology cannot make propositions about the individual case. In individual life, in subjectivity, "actual repetition, a recurrence of the same event, is not to be expected" (Lewin, 1931, p. 152). If individuality is, however, approached in the Aristotelian mode, it is approached from the perspective of frequency:

> Present-day child psychology and affect psychology also exemplify clearly the Aristotelian habit of considering the abstractly defined classes as the essential nature of the particular object. … The fact that three-year-old children are quite often negative is considered evidence that negativism is inherent in the nature of three-year-olds, and the concept of a negativistic age or stage is then regarded as an explanation … for the appearance of negativism in a given particular case! Quite analogously, the concept of drives, for example, the hunger drive or the maternal instinct, is nothing more than the abstract selection of the features common to a group of acts that are of relatively frequent occurrence. This abstraction is set up as the essential reality of the behavior and is then in turn used to explain the frequent occurrence of the instinctive behavior, for example, of the care of infant progeny. (Lewin, 1931, p. 153)

Lewin attests a circular process to Aristotelian psychology: historical-geographical data are collected and averaged to produce scientific propositions, which are then used to describe and explain an individual subject's behavior, which is subject to abstraction from the actual situation in which its behavior took place. Consequently, "lawfulness and individuality are considered antitheses" as long as the particular individuality cannot be subsumed under an average frequency produced from historically geographically specific data. Individuality is reduced "to a treatment … in terms of mere averages, as exemplified by tests and questionnaires" (Lewin, 1931, p. 155). Even if an investigation in an Aristotelian manner is concerned with a single person, she is still approached from a perspective of historical incidences instead of approaching situations holistically. "Such … investigations are consequently unable as a rule to give an explanation of the dynamics of the processes involved" (Lewin, 1931, p. 157). Even in qualitative analyses, Lewin identifies the same flaw, for example, in the study of important life decisions in the psychology of will. For him, the study of these important decisions relies solely on historical, anthropomorphic data: "[T]ransferred to physics, [this] would mean that it would be incorrect to study hydrodynamics in the laboratory; one must rather investigate the largest rivers in the world" (Lewin, 1931, p. 158). "[T]he Aristotelian immediate relation to the historically regular and its average really means giving up the attempt to understand the particular, always situation-conditioned event" (Lewin, 1931, p. 166).

Lewin instead advocates studying the lawfulness of the individual case. And he exemplifies his concept with Galilei:

> The mere fact that [Galilei] did not investigate the heavy body itself, but the process of "free falling or movement on an inclined plane" signifies a transition to concepts which can be defined only by reference to a certain sort of situation (namely, the presence of a plane with a certain inclination or of an unimpeded vertical extent of space through which to fall). The idea of investigating free falling, which is too rapid for satisfactory observation, by resorting to the slower movement upon an inclined plane, presupposes that the dynamics of the event is no longer related to the isolated object as such, but is seen to be dependent upon the whole situation in which the event occurs. (Lewin, 1931, p. 164)

Individual, subjective behavior can only be understood if the historically regular average is substituted for the particular in the whole situation and when "it is just the same whether the situation is frequent and permanent or rare and transitory" (Lewin, 1931, pp. 166–167). For Lewin, "the dynamics of the processes is always to be derived from the relation of the concrete individual to the concrete situation, and, so far as internal forces are concerned, from the mutual relations of the various functional systems that make up the individual" (Lewin, 1931, p. 174). The Gestaltist roots of this thinking couldn't be more obvious (Wertheimer, 1922, 1923, 1925).

"Situation," as a central term, does much work for Lewin. He argues that the concrete situation needs to be investigated. This way, "a picture that shows in a definite way how the different facts in an individual's environment are related to each other and to the individual himself [sic], can be acquired" (Lewin, 1936a, p. 13). For Lewin, a situation is never a single incidence but always a process in which some change occurs. To understand the situation is to understand that change. The complexity of such an approach becomes apparent in one of Lewin's examples (Lewin, 1931, p. 167): If a child is standing within visual range of two equally desirable objects – say a toy and a piece of chocolate – the initial data point would suggest that the situation cannot be resolved since both objects are equally desirable but at the same time in different places and, as such not reachable. However, empirically, the situation will most likely be resolved in one way or another. To understand how this problem is resolved, it is not sufficient to record the initial equality of both desires. At the same time, it is not sufficient either to record the moment in which the desire is changed – since both these data points are single occurrences that do not speak to the dynamics of the situation itself. From here, it would only be possible to empirically collect several instances to speculate about future behavior and give a regular explanation. Only the investigation of the whole dynamic situation of a girl being confronted with two equally desirable objects and finally deciding on one of those can explain the decision process. The investigation of the full concreteness of the situation reveals the psychological law, which is at once particular to the very case under investigation and general to all the situations in which the same conditions apply:

> The accidents of historical processes are not overcome by excluding the changing situations from systematic consideration but only by taking the fullest account of the individual nature of the concrete case. It depends upon keeping in mind that general validity of the law and concreteness of the individual case are not antitheses, and that reference to the totality of the

> concrete whole situation must take the place of reference to the largest possible historical collection of frequent repetitions. (Lewin, 1931, p. 175) [T]he opposition between universal concept and individual event is overcome. Law and single occurrence enter into an intimate relationship. (Lewin, 1936a, p. 8)

The methodological difference between Aristotelian and Galileian modes of thought Lewin presents here significantly impacts the exploration of subjectivity. In understanding that subjectivity or individuality is potentially confusing for research based on historic-geographically specific data in which "the exception proves the rule," (Lewin, 1931, p. 156), Lewin develops a model based not on a single occurrence of an event, but on single meaningful situations and their dynamics, where "the situation is to be regarded as the total of possibilities" (Lewin, 1936a, p. 15) – what Lewin calls psychological life space (Lewin, 1936a, p. 12) and later social space (Lewin, 1939a) or group life space (Lewin, 1935, 1947a, p. 12). From here, the subject becomes an actor in a situation in which she is confronted with various (inner and outer) forces, which eventually determine behavior. As such, the individual is determined by the situation and the concrete possibilities of action therein. At the same time, the individual determines the situation by producing effects that determine the psychological reality: "What Is Real Is What Has Effects" (Lewin, 1936a, p. 19). The only viable conclusion from this insight for Lewin is that "the situation must be represented in the way in which it is 'real' for the individual in question, that is, as it affects him [sic]" (Lewin, 1936a, p. 25), i.e., *from the standpoint of the subject*. Rather than singularized data points, Lewin sees the potential to fully understand the subject's decisions and behaviors. Not specific attitudes and traits are relevant, but the very situation in which behavior is shaped dynamically as an interplay of the subject and the situational forces: "A dynamic psychology has to represent the personality and the state of a person as the total of possible and not possible ways of behaving" (Lewin, 1936a, p. 4).

The subject is not a composition of historical facts or "nothing more than an abstraction – a being who properly should be described as a cross-section of the groups to which he belongs" (Lewin, 1939a, p. 21). Instead, it is the subjectivity produced in the dynamics of a specific situation that presents the starting point of psychological investigations. From there, psychological research, be it with individuals or groups, follows a pattern: The subjective life space is investigated concerning its "facts," i.e., everything that "matters" for the particular person or group. With this procedure, an account of the actions taken by the person or group and the subjectively following steps are created. In case of a conflict, the resolution depends on the analysis of the subjective life spaces of the parties involved, paired with an analysis of the objective situation, i.e., all the possible actions in the very situation that is used to "communicate to each other the structure of their life spaces with the object of equalizing them" (Lewin, 1947a, p. 12) to create a shared subjectivity. As such, the analysis of group conflict "swings from an analysis of 'perception to that of 'action,' from the 'subjective' to the 'objective' and back again" (Lewin, 1947a, pp. 12–13). The methodological background against which this procedure is formed is Lewin's famous "circular causal process," which underpins the development of the field of Action Research:

> Any kind of group action or individual action, even including that of the insane, is regulated by circular causal processes of the following type: individual perception or "fact-finding"—for instance, an act of accounting—is linked with individual action or group action in such a way that the content of the perception or fact-finding depends upon the way in which the situation is changed by action. The result of the fact finding in turn influences or steers action. (Lewin, 1947a, p. 13)

On a methodological level, Lewin reintroduces subjectivity to psychology. It is the psychological world – that which affects the individual – that psychology, in Lewinian terms, aims to study. He redefines concrete individuality as based not on attributes but on situational dynamics; the central element is the subject within the situation. These situational dynamics are a priori lawful and do not need to be established based on recording a maximum number of occurrences. They are generalizable because they are bound by law. The ideal environment to study these situational dynamics remains the psychological laboratory. Yet, the laboratory is not chosen because it represents ideal conditions for repetition. Instead, the experiment, in Lewin's view, must be modeled so that it resembles the real world as accurately as possible; its significant advantage is the ability to control for unwanted interruptions of situational dynamics.

As revolutionary as this may sound, Lewin cannot be read as a liberator of the subject. In the Aristotelian mode, the subject and concrete individualities are a scientific taboo, not to be studied because of the complexity of the black box. In Lewinian terms, the subject occupies the central role as the actor in the situation, who determines what is relevant to psychology in the first place. At the same time, the subject remains bound by law. A subject can be fully understood if the underlying laws are disclosed. And because these laws are procedural, the subject can be controlled and predicted, very much in line with Cattell's depiction of psychology as "to describe, to understand and to control human conduct" (McKeen Cattell, 1930, pp. 21, 31).

Despite these limitations, Lewin's understanding of science and empirical research provides the grounds for an Action Research methodology. Lewin created justifications for developing a (social) science with the individual subject at its center. Moreover, he introduced a distinction between historic-geographic data, accumulated and averaged, and the concrete situation as the empirical focus. He concluded that only the subject in the concrete dynamic situation – as investigated in Action Research – could be a useful source for creating new scientific knowledge. New psychological knowledge could only be gained if the co-production of the field and the subject were considered. As it became a practice in Lewin's Action Research projects, the Raumgestalt needed to be studied. Lewin's systems approach to research and science is strongly connected with these thoughts. He did not believe in a direct relation between scientific clauses and the real world but in systemic relations of the descriptions of reality, which had to be in concurrence with each other. The Tavistock Institute and Eric Trist and Frederick Emery later picked up this systems approach. Lewin firmly believed that these systems of scientific terms had to be tested in real life – or in laboratory situations that resembled real-life situations as closely as possible. And he believed that scientific progress could only occur if

creative research methods were applied. Standard methods would only lead to standard results. Instead, as in Action Research today, theories about situational dynamics (i.e., change) can only be studied in changing situations where new situations demand new research methods.

These convictions, in conjunction with the developments above in American psychology, namely, the rising importance of applied research, enabled Lewin to spell out his concept of a "circular causal process" as an underlying concept for the future development of Action Research.

Lewin and Subjectivity II: The Early Foundations of Lewin's Work in Industry

Kurt Lewin entered the history books as a psychologist. However, he was only employed in a psychology department during his years in Berlin. And while he knew that he was generally regarded as a psychologist, with his career advancement, he thought of himself as an interdisciplinary scientist, maybe a social scientist. Indeed, he believed it was possible to unify all the social sciences under one meta-theory, a claim he made in several of his published papers (Lewin, 1921, 1927, 1931, 1936a, 1939a, b, 1943b, 1947a). In his later years, he felt ever more compelled to cross-disciplinary boundaries: "Being officially a psychologist, I should perhaps apologize to the sociologists for crossing the boundaries of my field. My justification for doing so is that necessity forces the move, and for this the sociologists themselves are partially to blame" (Lewin, 1939a). Presumably, in the face of World War II, Lewin turned this possibility of unifying the social sciences into a necessity if democracy was to survive:

> Obviously social management in the various areas of modern society has to face a tremendous task. Its solution presupposes social fact-finding of an unheard of magnitude. It requires basic research about social steering systems. The fear of fascism seems to have driven some people into the greatest kind of misunderstanding which identifies democracy with planlessness. The survival and development of democracy depends not so much on the development of democratic ideals which are wide-spread and strong. Today, more than ever before, democracy depends upon the development of efficient forms of democratic social management and upon the spreading of the skill in such management to the common man. (Lewin, 1947b, p. 153)

In many ways, the above quote from the second volume of Lewin's Frontiers in Group Dynamics series – published posthumously, generated by the editors of Human Relations from a manuscript, which was "far from finished and in very preliminary form" (Lewin, 1947b, p. 143) and entailing many unreferenced sections of Lewin's (1946) article Action Research and Minority Problems (Lewin, 1946) – stands at the endpoint of the development of the concept of democracy in Lewin's writings and work. Lewin first raised issues of democracy and participation as early as 1920 when he published an essay in Karl Korsch's journal *Praktischer Sozialismus* (Practical Socialism) entitled "Die Sozialisierung des Taylorsystems" (Lewin,

1920), published in 1999 as "Socializing the Taylor system" (Lewin, 1999e). Lewin's goal is to change the role of psychology in industry; to end the Taylor system, which he describes as follows:

> "Taylorism"… refers to the relentless exploitation of the individual in the service of production, with the consequence of rapid aging; claiming the highest achievable performance level as the average level; whipping the workers to the most intense effort by all available means; degradation of work by driving it to the most extreme division of labor without regard for the worker's psyche, in short, a "consumption" of the worker in the service of production according to calculations of wear and tear and amortization rates applied to machines. (Lewin, 1920, p. 17; 1999e, p. 307) – wording changed.

Lewin does not deny the role of (Aristotelian) psychology in the propagation of Taylorism. However, he points out that applied psychology itself is "by nature merely a method that could serve any goal in changing work processes" (Lewin, 1920, p. 18; 1999e, p. 308). The decisive factor is the theoretical stance behind the application of certain techniques. And from Lewin's standpoint, which in this 1920 paper is the standpoint of socialism, "Taylorism … is a method which cannot be justified …, even if a community interested in high production figures takes the place of the individual entrepreneur because an arrangement of this kind would have to be rated as a one-sided exaggeration of the interests of the consumers of labor output, thus as consumer capitalism" (Lewin, 1920, p. 18; 1999e, p. 307). Lewin identifies two "equally privileged tasks" of psychological intervention in labor organization which become emblematic of his later work in industry: "(1) … increasing the production value of labor and of professions and (2) … increasing their consumption value for the worker" (Lewin, 1920, p. 30; 1999e, p. 317). For Lewin, a psychological intervention was supposed to increase efficiency and production output – like Taylorism. It should, however, also increase the personal satisfaction of the workers with their tasks. This interplay is important because the "decrease in labor's consumption value is accompanied by a decrease in work satisfaction" (Lewin, 1920, p. 18; 1999e, pp. 317–318). In strong opposition to the Taylor system, the worker needed to be recognized not as a machine more or less capable of relatively simple tasks but as a subject with idiosyncratic ideas and strategies in the accomplishment of their labor:

> Seemingly monotonous tasks are often not monotonous at all to the worker concerned, and, vice versa; seemingly varied ones are extremely boring. Only when a close examination of the mental factors involved in the different tasks identifies in detail the concrete operations and goals that enhance the life value of labor will such problems be solved, whether directly by psychological means or by general technical improvements. (Lewin, 1920, p. 20; 1999e, p. 309).

It would be the task of psychology to examine the individual case and make the results available for the "utilization for the general public" (Lewin, 1920, p. 21; 1999e, p. 310) because they have "a right to the universal introduction of such improvements in work, whether they increase productivity or consumption value" (Lewin, 1920, p. 33; 1999e, p. 319). Lewin's concept of labor and labor management goes beyond increased efficiency and increased production. It incorporates

the workers, their satisfaction, and society's general production needs. The mediation of the demand for expanded production and the demand for personal satisfaction, in Lewin's view, can only build on a process that renders the workers the experts of their tasks and in which new procedures are negotiated among all the affected stakeholders involved: "In order, on the one hand, to protect the life value of work from diminution and, on the other, from a decline in profitability, work consumers, as well as performance consumers, must have a guaranteed right to take part in decisions about the introduction of changes in work methods (Lewin, 1920, pp. 22–23; 1999e, p. 311). Research in this context "cannot … be done in the remote laboratory with just any subject but must take place in the factory itself with the skilled workers concerned" (Lewin, 1920, p. 19; 1999e, p. 308) and with "the specific industry, both as a work site as well as the aggregate of those who work in it, be made closely involved in the psychological studies" (Lewin, 1920, p. 33; 1999e, p. 319).

Lewin here subscribes to the Marxist notion of labor as "being indispensable to the human being" because it "endow[s] each life with meaning and importance" (Lewin, 1920, p. 12; 1999e, pp. 302–303). The 1920s Lewin believes in a transformative value of work not only for production but for society as a whole if it is achieved "to shift the goal, replacing the interests of the private economy with those of communal economy …, by the emphasis on aggregate aspects of the economy in contrast to private economic interests" (Lewin, 1920, p. 30; 1999e, p. 317).

In his reflections about Taylorism, several key issues stick out and ground his later work in industry and the Tavistock studies. Two elements seem specifically central. First, Lewin claims that labor and work organization belong to psychology – as opposed to psychometrics – and that this psychological relevance entails more than measuring human abilities for increased output production. Instead, Lewin introduces the dimension of *health in the workplace*. Work should not only be productive, but it should also be healthy, that is psychologically healthy. And psychological health is equated with subjective satisfaction. To achieve this, the worker needs control over her tasks and ultimately needs to have a say in the decision-making process of production, which is the second central element Lewin introduces here. This participation needs to take place on two levels: The worker must be able to decide to some extent which job she wants to do, and the amount of production, as well as the tasks to be fulfilled, need to be agreed upon in a broader participatory process in which both the consumers and the producers are involved. Essentially, Lewin calls for a process of democratic dialogue that negotiates the needs and requirements of the individual versus the society at large. Labor, so the claim, must be taken out of the private sphere in a double sense: It cannot be for the sole benefit of private capital, and the concrete work conditions cannot be a solely private matter. Instead, labor needs to be placed in the public sphere where concrete conditions are to be negotiated. Lewin imagines applied psychology's role as a scientific tool that helps facilitate these processes.

Lewin and Democracy I: The Concept of Participation in Lewin's Research Projects

Lewin got the chance to practically implement his high ambitions expressed in the Taylor paper during 1923–1925 when he collaborated with Hans Rupp, a Berlin colleague, on a project to increase productivity in three textile companies in Northern Germany. His colleague Hans Rupp had been working in the field of suitability testing (*Eignungsprüfung*) for new applicants to the company and tried to improve these tests so that they resemble the actual work tasks during the production process as accurately as possible. Given the mismatch between the daily jobs of the workers and the abilities they were judged upon, Rupp developed a series of apparatuses that resembled the various tasks on the production line. Applicants were ranked according to the speed and accuracy with which they fulfilled the various functions. Rupp criticized the older tests because they centered on tasks related to the actual production of goods – the productive work in the narrow sense – such as stitching, spinning, and so forth, while leaving other mandatory tasks, such as regular adjustments to machines, routine repairs, or the acquisition of raw material out of the equation (Lewin & Rupp, 1928a, pp. 8–23). In essence, Rupp's work is an early version of the socio-technical analysis, which became prominent in the Norwegian ID Projects 40 years later. Instead of judging labor in terms of output as in the Taylor system, he analyzed the work process fully to understand all the tasks involved. Rupp differentiated actual production tasks, contributing to the final product, and maintenance tasks, such as repairing machines. This distinction carries significance, as it is for the first time that these maintenance tasks became recognized as actual labor, which remained unpaid since the wage system relied on piecework.

In his part of the project, Lewin questioned the logic of sustainability tests. Lewin assumed that the productivity of a laborer was related to the techniques used rather than their suitability for the job. He decided to investigate the concrete techniques of the employees in fulfilling their tasks, carrying the suspicion that differences in work output stemmed not so much from different inherent abilities or inabilities but from how the tasks were approached (Lewin & Rupp, 1928a, pp. 51–63). While this seems to be a relatively simple suspicion, it already hints at a significant methodological shift within the framework of labor organization, which only came to the surface more than 20 years later when the Tavistock Institute launched its research project into coal mining. Within the Taylor concept of increased labor division, humans essentially carried out the tasks machines could not perform. The logical consequence was that the machine – and as such technical advancement – determined the worker's concrete labor tasks as a mere machine operator. In the same way that a machine was judged as good or bad, i.e., productive or unproductive, the worker was evaluated according to the output produced. It was assumed that every worker would, like every machine, follow the same repetitive steps in the production process. Therefore, as in Rupp's experiments, the general measure was "speed." Lewin, in contrast, reintroduces a distinction between the machine and its operator. While the machine was following the same repetitive steps, the worker was able to

produce *variance*. As Lewin was convinced, how she utilized the machine could easily vary from worker to worker, thereby rendering productivity not as a simple function of speed but as a function of the concrete techniques applied.

In accordance with his theoretical standing, Rupp tried to break down the various tasks into repetitive units of action. At the same time, Lewin was concerned with the *Geschehensganzen* (Lewin & Rupp, 1928a, p. 51), i.e., the whole dynamic process and situation in which the various single actions of labor as parceled out by Rupp took place. In addition to Rupp's distinction between productive and maintenance tasks, Lewin differentiated between tasks solely dependent on the worker – for example, stitching which only happens if the worker actually does something and therefore renders her self-determined in her actions – and tasks that involved automated machines. For these, Lewin thought it necessary to treat "the person and the machine in their specific interaction as a *dynamic unit*" (Lewin & Rupp, 1928a, p. 52). The failure in a specific task or a reduced output of a worker compared to previous achievements or the achievements of others was not attributed to the worker's inability but to the failure of the entire unit. A breakdown into simple tasks would not be helpful since the complexity would only become apparent if the whole situation remained in view. It was no longer possible to ascribe a specific repair time to specific defects since often several defects happened at the same time or one failure spawned another, which produced complex situations not comparable to incidents that had happened before or to other workers and which were not simply the sum of an equation of all the individual failures. Instead, multiple failures create a psychologically different situation: The worker has to decide which failure needs to be fixed first, operates under stress, trying to get back to productive work as fast as possible, and so forth.

With this approach, Lewin was able to define situations of complete system failure, for example, because multiple defects took place at the same time or the machine ran out of necessary raw material in various places. From this perspective, it became clear that the failure to meet production goals could not be attributed to the worker. Instead, it depended on specific strategies to deal with complex situations and system failure (Lewin & Rupp, 1928a, p. 56). While Lewin managed to show that differences in output did not necessarily stem from the ability of the worker, he at the same time acknowledged differences in productivity across employees. However, he was able to show that this was mainly due to different ways of organizing the workflow. He observed a connection between productivity and the "performance milieu" (*Leistungsmilieu*): employees working in close proximity tended to match their output. More productive workers decreased their output, while less effective workers increased their production speed. Lewin showed that this effect occurred because of an approximation in the techniques applied.

In essence, Lewin was able to show in his first project in industry that productivity is not primarily the achievement or fault of the person. He created an understanding of the necessity to help each other in situations of multiple failures; that is, he helped establish a common interest in increased production. He could record various working techniques, which proved to have different efficiencies for different workers. As such, he could make implicit strategies explicit and enable the

employees to consciously decide and try various techniques to see which would suit them best. While the study documented that workers were largely freed from attributions of specific inherited performance levels and enabled to decide between different techniques independently, the workers were never directly involved in the research and decision process or asked for their opinion. Instead, all the input came from the researchers. However, there are other interesting implications for future research in industrial production. First, it has to be recognized that this is a field study, i.e., an early instance of research carried out in the real world instead of the laboratory. Second, the classical experimental approach was dropped partially by Rupp in his suitability tests in that he did not rely on a control group for his modifications of the test setting. Even though it is most likely that these deviations from the classical experimental approach did not happen due to the methodological design but were simply motivated by pragmatic reasons (specifically because Lewin reintroduced control groups and separation from the actual production line "to experiment" in his later Harwood studies), they nonetheless represent an early instance of fieldwork where researchers manipulate real-life processes to "improve" a specific life context.

Until this point, Lewin had unfolded a theoretical understanding of subjectivity, an ideological basis, and a practical concept of applied psychological research in the field. What his theory lacked was a concept of participation in the strong sense, a concept that goes beyond a mere "right to take part in decisions" (Lewin, 1920, pp. 22–23; 1999e, p. 311) and spells out how this participation should be realized. It took three more research projects – the Autocracy/Democracy Studies by Lippitt and White (Lewin & Lippitt, 1938; Lewin et al., 1939; White & Lippitt, 1960), which Lewin supervised, the Food Habits Study (Guthe, 1943; Mead, 1943; Lewin, 1943e), and the Studies in Leadership, which led to the founding of the National Training Laboratories (Lippitt, 1949; Marrow, 1967), all of which I will examine concerning their contributions to the ideas of democracy and subjectivity in the following before turning to the first fully-fledged Action Research project under the guidance of Lewin.

The Autocracy/Democracy Studies undertaken by Lewin, Lippitt, and the political scientist Ralph White marked a return to the research lab. Nonetheless, the experiments were closely related to the social issues Lewin was concerned with at the beginning of the war and were going to shape Lewin's concepts of participation and democracy in future projects. Also, in line with his concept of a new Galileian era of psychological research, the experiments were not constructed to ensure reproducibility and variable measurement. The return to the laboratory is justified differently. The complexity of influences on groups in real-life settings provided the main reason. At the same time, Lewin's goal was to "approach an experimental procedure: (1) where group life can proceed freely: (2) where the total group behavior, its structure, and development can be registered." This further meant that "[a]ny specific problem such as group ideology should be approached in the experimental setup and the analysis of the data as a part of this greater whole," (Lewin & Lippitt, 1938) where the experiment resembles actual life circumstances as accurately as

possible while at the same time offering ideal conditions for the researchers to study situational dynamics.

After a preliminary investigation conducted with two groups of ten-year-olds by Lippitt showed inconclusive but interesting results with respect to autocracy and democracy in group behavior (Lippitt, 1940), a second series of experiments was started in 1939. A third test condition called "laissez-faire" was added to the democracy and autocracy conditions and tested with four groups of ten-year-old boys who met once a week to work on different projects (such as creating theatrical masks, mural painting, soap carving, etc. (Lewin et al., 1939, pp. 273–274). The projects forced the boys to work together to achieve their goals. The study lasted five months, and the group leader (an adult) changed every six weeks. The group leaders' task was to emulate the three different forms of leadership: democracy, autocracy, and laissez-faire. The democratic condition worked as follows: In the beginning, the group members were introduced to the various activities and followed by a discussion about what they wanted to do first. The leader moderated the group discussion and developed a tentative group goal for the activity. A vote facilitated the eventual decision. The division of labor and the concrete techniques to be applied were up to the group members. The group leader generally behaved as a group member and only gave advice in situations where no solution could be found. The authoritarian group, in contrast, had to work on the same group goal upon which the democratically organized group had agreed, but without any discussion. The autocratic leader decided upon the group's policies, concrete procedures, and labor division. The leader did not actively participate in the activities but praised and criticized individual members. Finally, the laissez-faire group was presented with all the available materials and free to decide what they wanted to do. The leader stayed out of the activities but offered instructions for possible activities with the materials if asked (Lewin et al., 1939, p. 273). All groups were subject to the three leadership styles at different times. In addition, single individuals were transferred from one group to the other, for example, moving from the autocracy to the democratic group and so forth.

The main findings of the experiments are related to a previous set of experiments conducted by Roger Barker, Tamara Dembo, and Lewin, which showed that if people (in the case of the experiment, young children) are prevented from achieving a goal, they develop frustration, which eventually leads to aggression (Barker et al., 1941a). Based upon the analysis of the boys' dialogues during the democracy/autocracy study, the highest amount of aggression occurred during the laissez-faire group condition. In contrast, the least aggression was found in the autocratic groups. However, it became clear that the autocratic groups' lack of aggression was due to oppression from the authoritarian leader. Intense frustration was present, but the leader's authority controlled the outburst of aggression. Whenever the leader left the room, or, more significantly, when the group changed to a new leadership style, intense aggression patterns emerged (Lewin et al., 1939, p. 283). If aggression occurred in the autocratic groups, it was regularly focused on "a substitute object" (Lewin et al., 1939, pp. 297–298), i.e., a single person as a scapegoat, an object, or someone outside the group. Aggression was never directed

at the group leader. High aggression in the laissez-faire group was explained by the frustration stemming from a lack of leadership and, consequently, a lack of accomplishment in the tasks compared to the other groups. Group bonds were low in both the autocracy and the laissez-faire groups. In the qualitative interviews conducted with all participants after the experiments, a strong preference for democratic leaders was found (Lewin et al., 1939, pp. 284–285). Also, the transition from democracy to the laissez-faire and autocracy setting went smoothly. However, it was extremely difficult for the groups from one of laissez-faire or autocracy to adjust to the democratic setting.

While Lewin is cautious with generalizations, he nonetheless believes to have discovered some basic pattern – laws in his framework – of societal processes:

> The varieties of democracies, autocracies, or "laissez-faire"atmospheres are, of course, very numerous. Besides, there are always individual differences of character and background to consider. On the other hand, it would be wrong to minimize the possibility of generalization. The answer in social psychology and sociology has to be the same as in an experiment in any science. The essence of an experiment is to create a situation which shows a certain pattern. What happens depends by and large upon this pattern and is largely although not completely independent of the absolute size of the field. This is one of the reasons why experiments are possible and worthwhile. (Lewin et al., 1939, p. 297)

And indeed, the gist of Lewin's understanding of democracy that will emerge in the coming years stands in clear relation to these experiments. He opposes any autocratic system, be it country governments or company directors in industry. He believes in a democracy based on elections that exhibit competent leadership to avoid chaos, as in the laissez-faire approach. And most importantly, he realizes that "autocracy is imposed upon the individual. Democracy he [sic] has to learn" (Lewin, 1939a, p. 31). It is his belief in democracy and the possibility of learning democratic behavior that drives his efforts. The study further exemplifies Lewin's understanding of development in science. The frustration/aggression study provided what Lewin identified as a fundamental law of the relationship between frustration and aggression and served as the underpinning for the democracy/autocracy study: Wherever there is aggression, there is also frustration, and vice versa. The aggression in the laissez-faire group is explained by frustration caused by the failure of achievement. In contrast, the aggression outbursts in the leader's absence in the autocratic group are explained by the frustration caused by repressive leadership. Although the experiment lacks an actual control group, it could be argued that the democracy group, in which everything is "just right," serves this function. Democracy is thereby elevated to the general societal standard against which autocracy and laissez-faire represent polarized ends on a continuum.

The Food Habits Studies went one step further. The goal was to change the behavior of the participants (Lewin, 1943e, p. 35). And that was to be achieved by influencing a group and its dynamics rather than the individual: "Closer scrutiny shows that … the individual approach place[s] the individual in a quasi-private, psychologically isolated situation with himself [sic] and his [sic] own ideas. Although he [sic] may, physically, be part of a group listening to a lecture, for

example, he [sic] finds himself [sic], psychologically speaking, in an 'individual situation'" (Lewin, 1999b, p. 273).

The first step of the study was a thorough description of the field to be examined. Specifically, the channels through which food made its way into the family household were investigated via qualitative interviews (Lewin, 1943e, pp. 37–39). A central theoretical concept stemming from these investigations was the "gatekeeper." Lewin discovered in the 107 interviews conducted (Lewin, 1943e, p. 36) that it was, in all instances, the housewife who controlled the channels, thus taking the function of the gatekeeper who controls the principal availability of different kinds of food in the family household. But the gatekeeper does not only control the type of food that comes into the family, but she also controls how it is distributed (i.e., in Lewin's study, meat for the husband was the number one preference, whereas children were preferably served vegetables – this proved true across all income groups) (Lewin, 1943e, p. 42). Carving out the role of the gatekeeper was instrumental in inducing change since there was now a single person per family instead of incorporating all family members. In other words, if the gatekeeper could be persuaded to buy healthier and – more important, during the war – readily producible and available food, this would affect the rest of the family as well. To compare different approaches to inducing change, it was first decided that small groups would be the optimal target since group dynamical processes would foster discussion about change. At the same time, Lewin thought of this as a chance to prove that "[d]emocratic methods regarding changes of groups [are not] wasteful" (Lewin, 1943e, p. 55). His "interest in the group decision method stemmed from the realization that even strong motivation may not suffice to change the action of a person …. Motivation leads to action only if it is brought down from the level of wishes and sentiments to the level of a 'decision'" (Lewin, 1943e, p. 60). The concept of a "group decision" that Lewin developed for these experiments was defined as follows:

> A group decision has one aspect in common with group discussion: a free interchange of ideas takes place, and in many respects, the initiative lies with the group. No attempt is made to force a decision on the group or to use high-pressure salesmanship. On the other hand, unlike a mere group discussion, group decision leads to a setting up of definite goals for action. These goals may be set up by the group as a whole for the group as a whole. (Lewin, 1943e, p. 55)

The first study set up by Lewin compared his idea of "group decision," i.e., "the group decides for itself whether and to what degree it wishes to change its habits," and what he called "request," i.e., "the group is requested to make a change to a certain extent" (Lewin, 1943e, p. 56). The subjects for the investigation were university students in Iowa who lived in dormitories. The goal for all groups was to reduce the consumption of white bread in favor of wheat bread. A total of eight student co-ops were matched in four pairs according to their consumption of white bread. One pair was asked to make a group decision about reducing white bread consumption in favor of wheat. In contrast, the others were asked to reduce their consumption for a definite amount, which equaled the amount the paired group had voluntarily agreed on in their group decision process. The experiment lasted for a week and showed that group decisions were more successful in case of external

requests; the wish to succeed or not to succeed depended upon whether or not personal preferences happened to be in line with the request. A group decision, on the other hand, seemed to establish a sufficiently strong group goal to be accepted by the members and overruled personal preference (Lewin, 1943e, p. 59). It seemed that requests were inefficient if the requested subjects were not already inclined to do what they were asked to do. The group decision, in contrast, enabled subjects to overcome their personal preferences in favor of a group goal.

In a second experimental variation, Lewin compared the effectiveness of a "Lecture Method" (Lewin, 1943e, p. 60) with his group decision concept. This time, the participants were housewives from the original interview study. Again, groups of about 20 were chosen, with a total number of 120 participants (Lewin, 1943e, p. 63). Lewin invited nutrition experts to lecture on the value of a different diet. The lecture stressed the importance of a healthy diet in times of war and pointed out the health and economic aspects of replacing regular meat with kidneys, brains, and hearts – which were chosen because of the general resistance to these kinds of food and in the hope of ruling out any of the participants already subscribed to the advertised diet. In addition, recipes were discussed and distributed, which helped avoid the aversive characteristics of the new ingredients, such as odor, texture, and appearance. Also, the (female) lecturers shared personal success stories of preparing delicious dishes with these kinds of meat, which seemingly convinced their own families.

In the group decision setting, a group facilitator was present together with the expert. A discussion about the new recipes was facilitated, and attempts to persuade the participants to try out the recipes were avoided. Instead, the discussion was introduced by a short statement that explained the problems of the government in changing food habits in the face of the increasing needs during times of war. The expert was introduced only once the group reached a state in which the discussion focused on problems of preparing the new kind of meat and to "help out" with expert knowledge. At the end of the meeting, the group voted on whether they wanted to try the new recipes the following week. In the lecture setting, 10% had tried one of the recipes after one week, while the number came down to 52% for the participants in the group discussions. Of the participants who had indicated before the experiments that kidneys, brains, and hearts were highly disgusting to them, nobody in the lecture groups tried out a new recipe compared to 29% in the group discussion setting. Group discussion and group decisions were far more effective, and participants who were originally highly averse to these new cooking methods were convinced. Lewin showed that if "the individual is approached in a group setting, and democratic discussion rather than lecture is stressed" (Lewin, 1943e, p. 63), change can be produced.

Besides showing the effectiveness of democratic decisions, the food habits studies point to "one of the fundamental problems of action-research, namely, how to change group conduct so that it would not slide back to the old level within a short time" (Lewin, 1999b, p. 265). For Lewin, it is crucial to create a high degree of involvement combined with freedom for a personal decision, as is achieved in the group decision situation (Lewin, 1999b, p. 271). Lewin realized that he needed to

reconsider the relationship between action and motivation. It is essential, so he points out, that we "study the particular conditions under which a motivating constellation leads or does not lead to a decision or to an equivalent process through which a state of 'considerations' (indecisiveness) is changed into a state where the individual has 'made up his [sic] mind' and is ready for action, although he may not act at that moment" (Lewin, 1999b, p. 272). This "making-up once mind" is what Lewin calls "freezing" (Lewin, 1943e, p. 64) with which he tackles one of the significant problems in Action: How is it possible to ensure "[p]ermanency of the new level, or permanency for a desired period?" (Lewin, 1999b, p. 282).

The Food Habits Studies marked Lewin's return from the laboratory to the "real world." However, apart from the interview study, Lewin still relied on experiments in that he tried to show social processes "in principle," not about actual social issues. The attempt to convince students to eat more dark instead of white bread was convenient for the research process – the two kinds of bread did not represent issues at the forefront of a change process for a healthier diet. Instead, they served as "substitutes" to show a process "in principle," which could then be applied to a "real problem" in the "real world." This is even more obvious when substituting certain kinds of meat. The types of meat chosen in the study characterized a deliberate exaggeration, forming an implicit statement: If it were possible, with the methods applied, to convince people to eat food that is generally regarded as disgusting, then it would a fortiori work with products that were already on the general list of eatable things. To show this, Lewin applied for the first time a concept of democratic decision-making as a research method for change. To him, democracy was superior because people could be convinced of change if given a chance to discuss issues in a broader group they felt part of. He showed that discussions had to occur between the relevant stakeholders, a role he termed "gatekeeper." He changed the researcher's position from an observer or detached investigator to a group facilitator who would moderate the discussion. At the same time, expert knowledge was introduced from an expert who otherwise shared the fundamental experiences of the group (being a woman with the typical "housewife responsibilities," having to convince other family members of the new kind of meat, etc.). Moreover, he introduced a distinction between democratic discussion and democratic decision-making by vote, which for him, was the key element in a change process. Also, he invoked for the first time the idea of lasting change, a concept he termed "freezing," in opposition to a short-lived change process that would immediately snap back. The concept of a democratic change via the decision processes of the participants implies another critical issue: Such projects can, in principle, fail concerning the predetermined goal. People can stay "unconvinced," i.e., decide against a change process.

From the workshop in leadership that Lewin organized in 1946 and which led to the foundation of the National Training Laboratories (NTL) one year later, a single occasion sticks out, which is described by Ronald Lippitt in an Interview with Alfred Marrow:

> Sometime during the evening, an observer [a member of the research staff] made some remarks about the behavior of one of the three persons sitting in – a woman trainee. She broke in to disagree with the observation and described it from her point of view. There was

> quite an active dialogue between the research observer, the trainer, and the trainee about the interpretation of the event, with Kurt an active prober, obviously enjoying this different source of data that had to be coped with and integrated. At the end of the evening, the trainees asked if they could come back for the next meeting at which their behavior would be evaluated. Kurt, feeling that it had been a valuable contribution rather than an intrusion, enthusiastically agreed to their return. The next night at least half of the fifty or sixty participants were there as the result of the grapevine reporting of the activity by the three delegates. The evening session from then on became the significant learning experience of the day, with the focus on actual behavioral events and with active dialogue about differences of interpretation and observations of the events by those who had participated in them. The [research] staff were equally enthusiastic, for they found the process a unique way of securing data and interpreting behavior. In addition, the staff discovered that feedback had the effect of making participants more sensitive to their own conduct and brought criticism into the open in a healthy and constructive way." (Marrow, 1969, pp. 212–213)

This was the first time participants of an ongoing project did not only participate in the research process but were invited to join the evaluation sessions. Thus, participants became "co-researchers" who entered the general discussion about research results and jointly evaluated their situation and behavior.

While the above studies were underway, Lewin was also involved in the now-famous Harwood Studies in conjunction with the Harwood Manufacturing Corporation. The Harwood Studies are generally considered the first Action Research project (Burnes, 2007, p. 217). In contrast to the projects Lewin worked on before, which were either laboratory studies or studies "in the field." Still, without direct intervention, the emphasis here "was to be on action, but action as a function of research. Each step taken was to be studied. Continuous evaluation of all the steps would be made as they followed one another. The rule would be: No research without action, no action without research" (Marrow, 1969, p. 90). In many ways, the Harwood project can be regarded as the culmination of Lewin's previous efforts. The general goal at the Marion, VA, plant was to increase production and improve quality. Lewin and his colleagues initiated several new procedures. They reduced the number of units expected to be produced by a newly trained worker. They established a longer training period in which they were supposed to eventually reach the company average – a measure to reduce frustration and the potential of aggression among the workers toward the management. As a result, the quit rates during training drop drastically.

Another goal Lewin pursued at the plant was to introduce a "rapid retraining of mediocre leaders into efficient democratic leaders" (Bavelas & Lewin, 1942, p. 115) following the insights gained in the Autocracy/Democracy studies. Lewin's student Alex Bavelas selected a small group of highly productive operators and met with them several times during the week to discuss their individual working methods. The pluses and minuses of the various approaches were discussed, and changes to the production line were suggested to the management. After several meetings, the group started trying to improve its output. The group members talked about a certain increase to be targeted and then voted on it. Over the course of five months, the group managed to increase their productivity significantly. Again, the hypothesis for the success of the changes was that instead of mere group discussions, the concept

of group decision would be applied. Bavelas tested this hypothesis by arranging group meetings with other groups who merely discussed possibilities to increase productivity without setting a definite goal. As a result, the increase in production for these groups was marginal. In Lewin's words, the process of unfreezing, changing, and freezing was only accomplished in the first example (Burnes, 2007, p. 218). In another experiment, Bavelas eliminated regular working hours. Instead, the workers were allowed to set their own goals during a typical day. Since the workers were paid according to the number of units produced, they were interested in achieving high outputs. The ability to decide how much they would produce led to an increase in overall performance (Burnes, 2007, p. 220).

To manage the problem of decreasing availability of workers, Lewin's research group suggested hiring older workers as well. This was met with strong resistance at the management and supervisor levels. Older workers were believed to be slower in learning and production, less reliable, and had more sick days. Even the introduction of data that proved the opposite could not convince the decision-makers. To answer the existing prejudices, a project was formed under the supervision of John R. P. French. Several company supervisors were asked to join a project to determine the productivity of older workers already working in the factory. All the decisions concerning the research process were left to the team members; the researchers merely served as consultants. The results proved that older workers were not only as effective as their younger counterparts but, in many cases, even outclassed them. The supervisors thus faced a dilemma between their prejudices about the abilities of older employees and the data which they had collected themselves. They had to realize their preconceptions were mistaken and even started to organize workshops for the other company supervisors to share their results. Consequently, the company began to drop the age restriction for new hires (Coch & French Jr., 1948; Marrow & French Jr., 1945; Marrow, 1972).

Alterations to the production line provided challenges. Workers were dissatisfied with giving up a task they were good at and resisted new production modes. Lewin suggested establishing meetings between the workers and the management every time changes to the production line were coming up rather than handing out instructions. The sessions were used to explain the necessity of changing the production line to the workers and to have the workers make suggestions about the change process. Instead of the usual drop in productivity after modifications to the assembly line, production increased by 14% within three weeks after the new procedure was established (Marrow, 1972, p. 97).

Although detailed documentation of the Harwood studies is scarce – most of the reports can be found either in very few and short publications by Bavelas, French, and later Lester Coch (Bavelas & Lewin, 1942; Marrow & French Jr., 1945; French Jr., 1945; Coch & French Jr., 1948) or in various side notes scattered throughout Alfred Marrow's publications (Marrow, 1964, 1967, 1969, 1972; Marrow et al., 1967; Marrow & French Jr., 1945) – the studies show how Lewin continuously applied his insights from previous studies. Like his early studies in Berlin, the research process started with analyzing the production tasks in which the operator and the machine were seen as productive interrelated unit (Lewin & Rupp, 1928a,

b). From there, the first goal was to reduce frustration and aggression (Barker et al., 1943) as a prerequisite for productive democratic change (White & Lippitt, 1960). Eventually, workers were granted more self-determinacy in their work process and were included in the "decisions" about production change (Lewin, 1943e). On the management level, supervisors were encouraged to carry out their research projects to "see for themselves" which group of workers proved effective, a consequence drawn from the leadership studies.

Lewin and Democracy II: Lewin, Culture, and Change on a Large-Scale Level

Before I draw some due conclusions about Lewin's Action Research concept, there is yet another instance of Lewinian thought to be considered. The Lewin we get to know in these publications is not so much "scientific" as he is "political," more concerned with social theory than empirical research. It is a Lewin who responds to the Second World War and the Nazi genocide.

As mentioned above, Lewin turned political early in his career. Already in the 1920s, he criticized the Taylor system from a socialist perspective. Early on, he argued that the work sphere needed to be understood as part of the public sphere and, as such, democratic negotiations. After years of developing his psychological meta-theory and empirical laboratory research, he returned to the discussion of societal issues spawned by his theory and research and the onset of the Second World War, anti-Semitism, and the suffering his own family had to endure. Alfred Marrow reported from a conversation with Lewin toward the end of Lewin's life when he begged Lewin to "take it easy," given his workload and signs of worsening health conditions. Lewin replied: "When you have to go to sleep each night, hearing the anguished screams of your mother as the brutal Nazis tortured her to death in a concentration camp, you can't think of 'taking it easy'" (Marrow, 1967, p. 146). Specifically, Lewin's confrontation with prejudices toward Jews in the United States compared to what he had already experienced during his childhood in Germany filled him with grief (Marrow, 1969, p. 100). Lewin published several papers on Jewish issues during his first years in the United States (Lewin, 1997a, c, 1999c).

Probably the most radical paper was published in 1939 and deals with the ever-increasing threat of fascism in Europe. Lewin realized that the war was unavoidable; that "those who are interested in democracy realize that there is but one of two alternatives, either to live as slaves under Fascism, or to be ready to die for democracy" (Lewin, 1997d, p. 116). For democracy seemed to be the only way to solve the problem of anti-Semitism and oppression of small groups in general because "anti-Semitism is partly the need of the majority for a scapegoat." However, Lewin argues that often "it is not the majority as such but an autocratic group ruling the majority that needs the scapegoat as a means of distracting the masses" (Lewin, 1997d, p. 117). In this case, as in many others, Lewin's explanations for social phenomena

stem from the Autocracy/Democracy and the Frustration/Aggression studies. He knows that generalization from these studies does not hold for every aspect of life. Yet, he states:

> Although groups of different sizes have specific problems, nevertheless, certain dynamic characteristics seem to depend more on the structural properties than on the absolute size of the group. Therefore, we might be able to investigate the properties of large groups on relatively small-scale models. We don't need, for instance, to study whole nations to find out to what degree our perception of the ideals of other persons depends on our own culture. We can study the same phenomenon in the 8- and 11-year-old child who perceives the degree of egoism, generosity, or fairness of his [sic] surroundings according to his[sic] own degree of egoism, generosity, or fairness. (Lewin, 1999d, p. 344)

In accordance with the results of the Autocracy/Democracy studies, Lewin relates anti-Semitism to prejudices spawned by societal tension stemming from a lack of democratic structures. And he is convinced that these prejudices cannot be done away with by logical arguments – just like it was impossible to persuade supervisors to hire older women in the Harwood studies; or in the attempts to change food habits. The only field of operation left for the Jews Lewin identified as the realm of the "Jewish life" (Lewin, 1997d, p. 118). A primary obstacle to doing so represented the constitution of the group, which mainly happens through external forces. If the Jews wanted to survive, so Lewin concludes, they had to realize that they were a group, not by their own decision alone but by the decision of the majority – bound together by external forces – and that oppression could only be overcome by acting together, i.e., by organizing themselves democratically.

In a paper entitled The Special Case of Germany published in 1943 at a time when the defeat of Germany in World War II was already graspable – Nazi Germany had just been defeated in the Battle of Stalingrad – Lewin discusses the importance of the social sciences in restructuring the World after the war. Like before, Lewin cautions against generalizations from psychological studies and introduces other influencing factors, such as culture. At the same time, he shows the role psychology can take in the restructuring of the world. He points out that simplified explanations for the reasons for war, which flourished specifically in psychology and associated violence with societal tensions, neglect the whole picture. For Lewin, war was a product of economic, political, and psychological/sociological factors. However, "[i]n planning the peace and in thinking of the future international conduct of other countries and our own, we must also realize that the psychological and particularly the cultural factors are in the long run essential" (Lewin, 1943d, p. 555). This is in congruence with his reflections on the autocracy/democracy studies, where he states that "Indeed, individual habits, as well as cultural patterns, have dynamically the character of restraining forces against leaving the paths determined by these patterns. … [T]hey determine the cognitive structure which a given situation is likely to have for a given individual." Because "[w]hether or not a given amount of tension and given restraining forces will cause a person to become aggressive depends finally upon the particular patterns of action which are customarily used in the culture in which he lives" (Lewin et al., 1939, p. 269). And with "Hitlerism … [as] an extreme edition of that traditional militaristic Prussian culture," Germany proved to

be "a special case: … It is a culture which is centered around power as the supreme value and which denounces justice and equality of men again and again as the disgusting remnants of a decadent democracy" (Lewin, 1943d, p. 555). Lewin realizes that the war will be won on political and possibly economic grounds but that long-lasting change for a "new German culture," which "should become thoroughly democratic" (Lewin, 1943d, p. 556) could only be achieved by re-education (Lewin, 1999c) because "the social atmosphere of groups can be changed profoundly by introducing different forms of leadership" (Lewin, 1943d, p. 557). And even autocratic leaders could be taught democratic attitudes within a short time frame. But all this, so Lewin was convinced, could only unfold against the backdrop of the exiting German culture he described in the following terms:

> A culture that considers peace to be an unavoidable but unattractive pause between wars; a culture where, for generation after generation, the military caste has had the highest status in the social hierarchy; a culture that does not know the concept or the term "fairness"; a culture which … is thinking mainly in terms of "rule or obey." (Lewin, 1999d, p. 335)

Culture, for Lewin, represents a system of relative stability – a quasi-stationary equilibrium (Lewin, 1943d, p. 558; 1947a, p. 13) – consisting of interacting forces and cannot easily be changed. It predetermines not the actions but the core of the possibilities of action in a situation. Moreover, "everyone considers as 'human nature' what actually is the character of his [sic] own specific culture" (Lewin, 1999d, p. 335). Consequently, "[t]o be stable, a cultural change has to penetrate more or less into all aspects of a nation's life." The change must, in short, be a change in the "cultural atmosphere," not merely a change of single items. As such, Lewin hoped for a revolution in Germany that could go deep enough to change the culture and get "rid in a very thorough fashion of a large group which has developed to perfection the most ruthless methods of suppression." But even after this group is gone, democracy would not simply emerge. Even if there was a democratic leader, the followers still had to learn their role as "democratic followers, " since "[t]o apply the principle of 'individualistic freedom' merely leads to chaos," i.e., the laissez-faire state. In contrast, "people must rather forcefully be made to see what democratic responsibility toward the group as a whole means." While Lewin outright accepts that democracy cannot be learned through autocratic methods, he at the same time argues "that to be able to change a group atmosphere toward democracy the democratic leader has to be in power and has to use his power for active reeducation" (Lewin, 1943d, p. 561). Moreover, democracy needs to be able to defend itself. "[C]ultural uniformity" (Lewin, 1943a, p. 168) is not desirable and, given the difficulties of changing a culture, not even possible. Germany could not "be a copy of the English or the American way of living." Instead, "[w]hatever occurs, the resulting culture will be something specifically German. It will show the traces of its history and of the present extreme experiences of war and Nazism" (Lewin, 1943d, p. 556). At the same time,

> any democratic society has to safeguard against misuse of individual freedom by the gangster or – politically speaking – the 'intolerant.' Without establishing to some degree the principle of tolerance, of equality of rights, in every culture the 'intolerant' culture will

> always be endangering a democratic world organization. Intolerance against intolerant cultures is therefore a prerequisite to any organization of permanent peace. (Lewin, 1943a, p. 168)

In the case of Germany, this also means that the "task [for the USA] is to create a minimum degree of democracy" because "the naive belief that people 'left alone' will choose democracy [has to be avoided]" (Lewin, 1943a, p. 171).

The Lewin revealed here is political. At the same time, he draws from his experimental work. Democracy is the main category and the quasi-stationary social equilibrium to be achieved. The obstacles are the difficulties of implementation; since democracy cannot be implemented from the outside, it has to be learned and is hampered by "culture." Thus, Lewin's subject becomes more determined by multiple frameworks: It is principally granted its subjectivity and individuality, yet the situation and the concrete forces in the field bind it. Moreover, how the forces play out is ultimately defined by "habits," which are based on an underlying, almost unchangeable substructure termed "culture."

Action Research or Social Engineering?: A Critique of Lewin's Concepts of Subjectivity and Democracy

With Action Research, which Marrow defines as "the experimental use of social sciences to advance the democratic process" (Marrow, 1969, p. 128), Lewin hoped to introduce a method of applied psychology that would advance the production of positive social change and utilize democratic participation. It was conceived as "research which will help the practitioner" (Lewin, 1946, p. 34). With his concept of unfreezing, changing, and freezing (Lewin, 1999b, p. 282), he hoped to tackle "one of the fundamental problems of action-research, namely, how to change group conduct so that it would not slide back to the old level within a short time" (Lewin, 1999b, p. 265). This was to be accomplished by proceeding in "a spiral of steps, each of which is composed of a circle of planning, action, and fact-finding about the result of the action" (Lewin, 1946, p. 38; 1999b, p. 269). Group manipulation is inevitable in this process because the planning of action depends upon the groups' perception of the situation "and therefore can be influenced by a change in this perception" (Lewin, 1999b, p. 270).

In many ways, Kurt Lewin managed to reintroduce the subject to psychology. In critiquing the general praxis in the psychology of his time, he carved out a specific understanding of subjectivity as an interlinked process of concrete situational dynamics, biological or inner activities, and a larger cultural framework. He attests that psychology in its pre-Galilean state has dealt with the subject only from a historical-geographical perspective and the subject "on average," thus rendering concrete subjectivity or individuality as a potential outlier.

In contrast, Lewin's psychology of situational dynamics starts with the subject and its concrete subjectivity. The situation under investigation consists of all the

facts that matter to the subject. As such, there is no way of understanding the subject without considering the whole situation. And the whole situation can be understood if all the possibilities of action are accounted for. If this is achieved, the various forces behind the number of choices available can be determined, and change can be induced by manipulating the various forces in the given field.

As shown above, Lewin's concept of democracy stems mainly from the Autocracy/Democracy Studies. While Lewin cautions against hasty generalizations of these results on several occasions (Lewin, 1938, 1939a, 1947a), he is certain that basic patterns found in these studies are readily applicable to phenomena in the society at large (Lewin, 1943b, c). Lewin defines three basic patterns of social organization: Autocracy, Laissez-faire, and Democracy. Autocracy is a state in which the leader determines the actions of their followers. Democracy also depends on a leader, but it must be a "good" leader who believes in democratic values and supports and fosters the co-determination of goals. Laissez-faire is either defined as a "pseudo-democracy" (Lewin & Lewin, 1999, p. 323), "individualistic freedom" (Lewin, 1943a, p. 170), or "anarchy" (Lewin & Lewin, 1999). It shows a lack of leadership exhibited by the leaders. Lewin creates a continuum ranging from total control of the leader (autocracy) to no control (laissez-faire), with democracy taking the ideal middle position where leadership is just right. He sees democracy as the preferred mode, for one, because the results of his experiments prove that people prefer democracy over the other forms of leadership and because it is the most effective form, which can supposedly be seen from the various studies in industry. At the same time, Lewin attests that democracy is difficult to achieve since, in contrast to laissez-faire as the absence of leadership and autocracy as an imposed leadership, democracy has to be learned by both the leaders and the followers. As such, democracy poses a paradox: "The democratic leader does not impose his goals on the group as does the autocratic leader: the policy determination in democracy is done by the group as a whole. Still, the democratic leader should 'lead'" (Lewin, 1943a, p. 168).

Another attempt to explain the difference between the three forms of leadership more in line with Lewin's field theory goes as follows (Lewin et al., 1939): In an autocracy, the "space of free movement" is extremely limited by the leader, which causes frustration and aggression as determined in the studies by Braker and Dembo (Barker et al., 1941a, b, 1943). In laissez-faire, movement is also limited, not because of a strong leader but because of the absence of a coordinating authority and, thus, a constant restriction of one's actions by the actions of others. In a democracy, because of the structure of the majority vote, it is possible to decide on collective action, and the social space is organized in a way that provides the maximum space for free movement to every individual and, thus, the highest possible reduction of aggression and frustration is achieved. As such, democracy, in Lewinian terms, calls for both a group focus and a maximum amount of individuality (Lewin et al., 1939). In many instances in his later writings, the ideal-type democracy is the United States, the prototype autocracy is Nazi Germany, and a prototypical laissez-faire state can be found in the post First World War Weimar Republic (Lewin, 1936b, 1943d, p. 559; Graebner, 1987).

The objection that an ultimate determination of the subject might arise because it is placed in the context of specific situations, its actions are described as a priori lawful, and because it is itself subject to a specific leadership style, Lewin answers in two ways. First, he introduces the concept of culture and corresponding habits: "The different styles of living can be viewed as different ways a given problem is usually solved. A person living in a culture where a show of dominance is 'the thing to do' under certain conditions will hardly think of any other way in which the solution of this problem may be approached" (Lewin et al., 1939, p. 296). There are various versions of autocracy, democracy, and laissez-faire based on the cultural system upon which these different leadership styles rest. Yet, the various leadership styles share some basic patterns across cultures. For democracy, these basic patterns would be.

> the essential equality of man. This sentiment has become still stronger and more determinant since the all-out war of Nazism to establish, philosophically and practically, an all-out inequality of man. However, let us not forget that the democratic belief in the equality of man means – as you well know – the granting of equal rights to individuals of different character, race, or creed. This principle acknowledges essential differences between individuals and between groups; indeed, it encourages and safeguards these differences by promising tolerance for all but the intolerant. In other words, the democratic equality of man means the right of individuals or groups to be different. (Lewin, 1999d, p. 335)

As such, the changes produced by research in group dynamics merely reflect changes on the surface, leaving the underlying culture intact, because there are certain dynamic relations between the various aspects of the culture of a nation – such as its education, mores, political behavior, religious outlook – which interact in a way that tends to bend any deviation from the established culture back to the same old stream." To change a culture, one "has to penetrate more or less into all aspects of a nation's life. The change must, in short, be a change in the 'cultural atmosphere,' not merely a change of single items" (Lewin, 1943d, p. 557). In essence, the only way to change a culture would be by means of a revolution or complete re-education.

The second response through which Lewin avoids an ultimate determination of the subject and social processes lies in his idea of quasi-stationary social equilibria:

> A culture is not a painted picture; it is a living process, composed of countless social interactions. Like a river whose form and velocity are determined by the balance of those forces that tend to make the water flow faster, and the friction that tends to make the water flow more slowly – the cultural pattern of a people at a given time is maintained by a balance of counteracting forces. The study of cultures on a smaller scale indicates that, for instance, the speed of production or other aspects of the atmosphere of a factory has to be understood as an equilibrium, or more precisely, as an "equilibrium in movement." (Lewin, 1943d, p. 558)

The idea of quasi-stationary social equilibria explains that a situation can be entirely stable from the outside and still exhibit an ongoing back-and-forth between opposing forces, which produces an inner movement. In essence, Lewin developed an early version of open-systems theory: Society is conceptualized as a framework consisting of various nuclei with well-defined borders. Their social function can

only be understood by investigating them as a whole, while their inner functions are in constant flux.

The consequence of this conception of subjectivity, culture, and the different forms of leadership is that – because of the lawful character of all actions and the practically unchangeable equilibrium of culture – change from within is hardly possible. Lewin seems to acknowledge some historical development that changes culture, but evidence for this is relatively thin (Lewin, 1943a). Certainly, people will adapt quickly to new life circumstances when entering a new culture. But this is, in Lewin's theory, related to the new forces in the field generating a new mode of determination for the subject. In essence, this means that Lewin's concept of the social order allows only for change coming from the outside, either by means of a revolution that fundamentally changes the preconditions or by means of a change in leadership. Changes from within are not possible. Further, the changes from the outside cannot happen on the level of single individuals since they will become followers of the new reference system they are subject to. Instead, to produce change always means to introduce new ways of leadership: "For the status and power of the leader or of the leading section of a group make them the key to the ideology and the organization of the life of that group" (Lewin, 1943d, p. 561).

Against this backdrop, it is of no surprise that the terms often used by Lewin to describe his research approach, besides the now famous Action Research, are "social engineering" (Lewin, 1945b, p. 6) or "social management" (Lewin, 1947a, p. 6). And indeed, there is evidence that Lewin had a great interest in using social science research to manipulate behavior. Edward Tolman notes in his obituary for Lewin:

> [Lewin] felt that, if we could but correctly conceptualize the a-historical, situational factors determinative of behavior, then we could manipulate these contemporaneous situational factors and produce the sort of behavior which all persons of good will would desire. If we can but discover the 'systematic laws,' the laws of the 'pure case,' i.e., those laws whereby a given 'life-space' inevitably produces a given behavior, then we can know how to change persons and groups to remake their behavior according to our heart's desires. (Tolman, 1948, p. 23)

Lewin himself is less straightforward, though, for he maintains that "the student of group life should be aware of this danger … of becoming a servant of very one-sided social interests" (Lewin, 1945a, p. 132). At the same time, he realizes that "the practitioner is interested in change experiments because he wants to reach certain objectives" (Lewin, 1945a, p. 133) and that these objectives are necessary because otherwise "we cannot judge whether an action has led forward or backward, if we have no criteria for evaluating the relation between effort and achievement, there is nothing to prevent us from making the wrong conclusions and to encourage the wrong work habits" (Lewin, 1947b, p. 150). Equally, Lewin realized that applied research would always be caught up in the political game:

> The boss of a powerful organization might be eager to have certain problems of his [sic] organization studied, but he [sic] might be as eager to prevent the appearance of any data which, to his [sic] mind, would be detrimental to the prestige of his [sic] organization. We might be dealing with such highly organized, at the same time, rigid and extremely sensitive

> social bodies, as the army or a factory. In such, and in many other "hot" situations, the psychologist is bound to wonder whether he [sic] has not stretched out his hand a bit too far into the "real social life." After all, he [sic] had planned to make science and not politics his [sic] work. … We might as well be clear that the psychologist is caught, for better or for worse, in a situation which is unavoidable. (Lewin, 1999d, p. 345)

It seems that Lewin was very much caught in this unavoidable situation himself. For him, this situation manifested itself in a gap between theory and applied research, which he would have so enthusiastically liked to close. His theoretical models confine subjectivity to an ultimately determined state, besides inner fluctuations; the person is the victim (or benefactor, for that matter) of her concrete situation, group, leader, and culture. Democracy is rendered the ultimately desirable form of leadership. Still, it is only achievable if a strong leader lays out the course and the person surrenders their opinion to the group's majority vote. Autocracy and anarchy (laissez-faire) cannot be changed by any other means but by introducing an experienced leader from the outside who defines the concrete goals and manipulates group behavior by granting votes on minor, non-decisive facts. All these problems are reflected in Lewin's studies. The autocracy/democracy study defined a simple continuum ranging from no leadership to rigorous leadership and yielded the predictable result that the "truth lies in the middle." The Food-Habits study introduced a vote in the end, but its outcome was meaningless since it did not affect the research process in any way. The goal was set from the beginning, namely, to convince housewives to try out new kinds of meat. The Harwood Manufacturing Corporation was not typical for US-American Companies, specifically in the low-skill branch; quite the contrary, as Coch & French effectively show:

> The policies of the company in regard to labor relations are liberal and progressive. A high value has been placed on fair and open dealing with the employees and they are encouraged to take up any problems or grievances with the management at any time. Every effort is made to help foremen find effective solutions to their problems in human relations, using conferences and role-playing methods. Carefully planned orientation, designed to help overcome the discouragement and frustrations attending entrance upon the new and unfamiliar situation, is used. Plant-wide votes are conducted where possible to resolve problems affecting the whole working population. The company has invested both time and money in employee services such as industrial music, health services, lunch-room, and recreation programs. In the same spirit, the management has been conscious of the importance of public relations in the local community; they have supported both financially and otherwise any activity which would build up good will for the company. (Coch & French Jr., 1948, p. 513)

In all instances, the studies did not put forward a primary goal of improving workplace quality. Instead, the increase in production and productivity was the main objective. Workers were granted a say in the process only as it was helpful for productivity. Self-determination concerning hourly production was only granted after Bavelas had realized the same concept with a small experimental group and showed that productivity increased. In general, company-wide changes were usually based on quasi-experimental studies conducted with a small portion of the workers in advance. Older women were not hired because of a high unemployment rate among women 30 years and older but because the company was short of personnel (despite

a tremendous unemployment rate in the community). The self-guided investigation into the productivity of older workers was a setup to convince the supervisors since results were already known to the professional psychologists and management (who introduced the idea of hiring older women precisely because of this knowledge in the first place). Ultimately, "democratic decision" was a mere tool to achieve goals set out by the researchers in conjunction with the management in advance.

And yet, as Bernard Burnes put it:

> Lewin was a humanitarian who believed that only by resolving social conflict, whether it be religious, racial, marital or industrial, could the human condition be improved. Lewin believed that the key to resolving social conflict was to facilitate learning and so enable individuals to understand and restructure their perceptions of the world around them. (Burnes, 2004, p. 981)

And Lewin himself states:

> In a democracy, the respect for human dignity and a feeling of decency has led to a peculiar dilemma: the "decent" citizen apologizes for his lack of active participation in group affairs by condemning group manipulation and leaving the business to the politicians. We do not want group manipulation, but we do need that amount of management of groups which is necessary for a harmonious living together. We want this group management to be done "by the people, for the people." This presupposes that not only the social scientist has to know more about all the factors which make for good or poor relations among groups in a community; this knowledge will have to be common knowledge to the ordinary citizen. To my mind, there is hardly anything more essential for the survival and the progress of democracy than that every citizen understands more clearly how the "right to be different" and the "cooperation for the common good" can and should be integrated for harmonious group relations in a democracy. (Lewin, 1945c, p. 7)

It was this spirit that spawned future Action Research even if Lewin was – maybe also because of his early and sudden death – not able to put it into practice himself. His thinking reflects the problems, questions, and desires of Action Research in multiple ways: On the practical level, he raised the questions: How do we produce change? How do we make it stable? What are the concrete power struggles we have to expect? On the methodological level, he asked: What is the best method of approaching the field: Do we focus on groups or individuals? Do we incorporate the participants in the research process? And on the theoretical level, he finally asked: What kind of change do we desire to build a better world?

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Chapter 5
The Northern Tradition of Pragmatic Action Research

The Tavistock Institute: A Phenomenological Approach

The Tavistock Institute provided a stepping stone that linked Lewin's work to the efforts of the next generation of Action Research projects in the Norwegian ID Project. While Tavistock was undoubtedly strongly influenced by Lewin's thinking, they nevertheless fell behind his comprehensive concepts and empirical achievements. Their projects are not interventionist but remain on a descriptive level. There are several reasons for this. They did not have the managerial and governmental support Lewin had in the Harwood and Food Habits studies. Their financial resources were considerably smaller. They lacked the kind of theoretical framework Lewin could bring to the field. At the same time, however – and this holds specifically for the studies in Coal Mining – they provided the underpinnings for a new theoretical framework, the Socio-Technical Systems Design (STSD),[1] which would later provide the grounds for the Norwegian ID Project. I will examine two Tavistock projects in the following to understand the transition process from Lewin to STSD. The two projects contribute to the new framework in two different ways. While the Glacier project can be seen as expanding the social/political horizon, the coal mining study adds to the social/technical component of (action) research in industry.

In 1949, Research at the Glacier Metal Company was carried out in collaboration with the Service Department of the company, a relatively independent subsection with 100 employees responsible for repair work and the sale of replacement parts.

[1] The socio-technical approach was also referred to as socio-technical systems (STS) (Emery, 1972), socio-technical design or socio-technical systems design (STSD) (1993). The latter was introduced by Eijnatten in his 1993 review of the literature in the field to avoid confusion with the field of Science and Technology Studies (STS) (Winner, 1992). With the same goal, I will use Eijnatten's abbreviation in the following even though STS or any of the other terms referring to the approach might appear in quotations.

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Until 1943, the payment structure of the Service Department relied on a pay-per-job system; that is, employees were paid a certain amount for specific repair assignments. In 1943, the management dropped this system in favor of a Payment by Results on Time Basis Scheme (Jaques, 1993, p. 380). Rates were now calculated based on a standard minute scheme. This new payment method was used until 1948, even though it was never entirely accepted by the workers. Since the steps for individual tasks kept changing, the standard minutes needed constant adjustment. Inequalities were created that sometimes created higher wages for untrained workers compared to trained craftsmen. To avoid these scenarios, management considered introducing an hourly flat rate for the workers. This flat rate should, however, be lower than the average payments based on the minutes system to account for an expected loss of productivity (Jaques, 1993, p. 381). To address upcoming issues with the new wage system and to investigate changes in productivity, a Wages Committee consisting of the Shop Committee (a workers' council), the divisional manager, the shop superintendent, and the shop accountant was formed to discuss the implications of the new system. A research team from the Tavistock Institute led by Elliot Jaques was also brought on. From the beginning, there was considerable tension between the manager, the Shop Committee, and the researchers. While the Shop Committee wanted to negotiate the "size of the package," management wished to utilize the research team to establish group discussions with the workers about the new wage structure. The Shop Committee was concerned about the researchers and believed they would take sides with the management to persuade the workers to accept a deal that would disadvantage them (Jaques, 1993, p. 383). In a private meeting of the Chairman of the Shop Committee and Jaques, these issues could be resolved. The Chairman uttered his suspicion of the new format. He explained that he was unsure about the attitude of the workers toward it precisely because the new structure meant that some workers would earn more than before. In contrast, others suffered from sometimes heavy losses. Group discussions of 1 hour were set up, including all the workers affected by the shift from the minutes system to the flat rate (a total of 40 workers divided into 5 groups). In the group discussions, distrust of the workers with the researchers surfaced. Moreover, it became clear that the workers were also suspicious of their own Shop Committee, which they saw as possibly collaborating with the management (Jaques, 1993, p. 386). The workers called for an increase in payment since the company was now saving on administrative costs previously caused by the constant readjustment of the standard minutes according to new work procedures. Further, the workers saw an increase justified because they believed productivity would increase since collaboration on jobs and helping each other out would be possible under the hour-based system. Consequently, the discussions concluded with the workers demanding to have the new flat rate set at the level of the basic pay under the minutes system plus 65%, which was believed to be the average bonus paid in the old system (i.e., the average portion of the salary based on the minutes system in addition to the old base payment). Management refused and pointed to the average bonus in the old system at a rate of 58% (instead of 65%). Even though management was right with the calculations and able to prove it, the Shop Committee was unwilling to agree to the proposal until the management

gave up on their earlier plans to reduce the basic salary by the expectation of reduced productivity under the new system. Finally, an agreement was reached, and a letter was prepared and handed to the workers. It read as follows:

> Management started by considering that the Shop would be ready to accept a slightly lower wage earning on average because of the advantage of working on a stabilized basis, not subject to the hazards of piece-work, and because it was considered that production might suffer to some slight extent when the direct piece-work incentive was withdrawn. Your representatives, however, objected to this view, suggesting that we ought to pay the same amount of wages in the Shop on the new system as on the old. , and that it would be up to the Shop itself to give as high an output on the new basis as on the old. Management consider this an extremely responsible attitude to take and accept the principle put forward by your representatives as a basis for discussion. The principle of payment proposed is that each worker should receive a new flat rate made up of his present basic rate, plus a 57 per cent increment, which is equal to the average bonus earnings of the Shop. This would mean that there would be some levelling out of wages, with less spread between the top and bottom, although an adjustment might be made in the case of a few people where it is considered that injustice would be done by adopting such a basis. … The position at the moment is that you are being asked to come to a decision as to whether you would regard the basis outlined above as being satisfactory in principle. There are still important matters to be solved and discussions are continuing on these. … [W]e have to consider to what extent, if at all, there should be any adjustment in rates established on a new basis in accordance with falling outputs, or alternatively, increased output, whether this arises from greater or lesser productivity on the one hand, or greater or lesser volume of work on the other hand. Subject, however, to final decision on these issues, we should like to know if you think the general basis proposed would be satisfactory. On this proposed new basis your own new rate would be as shown on the bottom of this note. Signed, Divisional Manager [and] Chairman of Shop Committee. (Jaques, 1993, p. 392)

The divisional manager devised a plan how to move forward:

1. A guarantee from the shop that production would not suffer.
2. A guarantee from management to the shop concerning benefits from increasing productivity.
3. The establishment of some mechanism for making possible increased participation of the total shop in the making of departmental policy. (Jaques, 1993, p. 396)

It was decided to have the workers vote on the establishment of a Shop Council in conjunction with the vote on the switchover to the new wage system. The goal was to address questions like: “Who got the savings on overheads? What happens if productivity went up or down? … What guarantees were there that the agreed flat rate would be secure and that there would be no change back to piece-work at a lower level?” (Jaques, 1993, p. 397). The ballot paper read like this:

> Do you wish to change the method of calculating your wages from the present piece-rates to a flat hourly rate? If you agree to this change, you must leave to the findings of a Shop Council (to be set up as representative of all London Service Station personnel) further discussion as to what is to be done if production rises or falls as a result of the change. The Council, when constituted, will discuss and decide on all problems arising out of this issue. (Jaques, 1993, pp. 400–401)

All 40 piece-rate workers of the company voted, and 28 agreed, while 12 voted against. In a special meeting, the Shop Committee decided that the majority of 28 votes was good enough to switch the system on the following grounds: “it would

allow more equitable payment on the basis of skill; it would overcome the present difficulties in assessing proper piece-rates, and remove many pay anomalies in the department … and hence would lead to increasing harmony" (Jaques, 1993, p. 402).

This was, however, not the end of the wage negotiations since an unexpected difficulty arose from the so-called "Holiday Incident" (Jaques, 1993, p. 403). Every worker at the company received two weeks of paid vacation per year. The vacation time was the same for everyone during which the company closed down entirely, with very few workers taking their vacation a couple of weeks earlier to ensure a basic amount of staff being present during the actual vacation weeks in case of emergencies or necessary repairs, which were only possible while production was interrupted. The workers demanded that their vacation pay be the same flat rate they would now receive under the new system. In the old system, only the base rate was paid, and any bonuses (which on average made up 57% of the salary) were lost for the two weeks vacation period. Management readily agreed to that demand. However, when talking to the finance office, management had to learn that the vacation payment was calculated by taking the average of a worker's basic rate on July 1 of the preceding year and June 30 of the vacation year. Workers who took their vacation earlier were excluded from this rule. For them, the vacation rate was calculated as the average of the base rate of July first of the preceding year and the base rate on their last day of work before the vacation started. Depending on the actual date of the switchover to the new system (in which the basic pay was substantially higher), workers would receive a higher vacation payment. Also, if the switchover would occur before June 30 but after some workers had taken their holiday already, it would be to their disadvantage. Even if the department switched to the new system before anyone took their vacation time, the vacation salary would still not be the same as the new wage based on the hourly system since the base rate of June 30 of the previous year was part of the calculation. This complex situation led to a stop in negotiations within the department. Now, negotiations had to take place between the department and outside administrative bodies. The members of the shop council had to work together in a joint effort to achieve a shared goal. They were eventually successful in renegotiating the calculation system for everyone to benefit from the new wage structure during their vacation times. The joint effort was appreciated by the workers in the company and helped reduce tensions and suspicions among the stakeholders.

The Glacier study ignored the concrete circumstances of production and how tasks were organized (Trist, 1981a, p. 21). However, the study introduced a new dimension, which had not appeared in Lewin's work so far: it investigated the structural conditions upon which the company's hierarchical system rested. In comparison to Glacier, the Harwood plant had a relatively simple structure. In essence, there were two parties involved: management and workers. Supervisors played a role, but only since they were the prolonged arm of the management on the shop floor. The research team stood on the side of management; their goal was a higher production output. This may not have necessarily been a negative situation for Harwood, given the general attitude in the company as described by Coch and French (Coch & French Jr., 1948, p. 513). The structure at Glacier was considerably more complex.

There were workers on a piece rate, workers on an hourly rate, supervisors, a worker's council, department management, and several administrative bodies outside the department, which nonetheless influenced departmental policies. The researchers' roles were largely undefined. While brought in by management, researchers were not collaborating with management interests. Instead, their interest was to observe and possibly facilitate discussions. At the same time, management was afraid that the researchers could collaborate with the workers. In turn, the workers and the workers' council feared the researchers would advocate for management interests.

To further complicate the issue, there were instances of mistrust in the workers' council among the workers, suspecting the council would secretly team up with management against them. Eventually, a collective effort in a conflict with an outside entity (the payroll office) unified the stakeholders. In this process, the researchers were not involved; they were merely documenting the incident.

Several developments are noteworthy: Change was not a simple matter of experimenting with new ideas. Quite the contrary, "experimenting" with the new structure was impossible since the new wage system would only unfold its potential if all workers switched at a time – since, under the old system, everyone had to get their work done to receive wage benefits. In contrast, the new system was supposed to enable collaboration on various job tasks. At the same time, management gave up a tool to reward high productivity. As a result, the democratic processes in Glacier were much more complex than Harwood's. In Harwood, democracy was used as a tool. Workers discussed the consequences of decisions made by management. For example, management decided to start the production of a new product, and the workers were allowed to discuss the implications and how they might deal with these, leaving room for minor adaptations such as how to organize the production process in detail. These discussions did not occur because of a genuine "interest" in the workers' opinions but because it was – according to Lewin's insights in the Food Habits study – believed that such decision processes would increase production output – which they did. At Glacier, the decision process about the new wage structure was also facilitated by vote. The vote was, however, "genuine" in that the eventual decision carried actual consequences, and the workers were participating in formulating general company policies. The shop council even discussed whether the majority vote could be accepted, given the closeness of the decision and the potential hardship for some workers stemming from the new system.

The relative structural complexity in the Glacier study yields another essential element. The various bodies involved in the negotiation process were apparently following a specific pattern of discussion. It was the "job" of the workers' council to oppose management, the workers' "job" to doubt the other parties' sincerity, and management's "job" to suspect that the workers have no interest in their work other than being paid for it. These general patterns (called "discursive formations" in the Search Approach) came to the forefront when the researchers entered this otherwise stable opposition of the parties involved. It became clear that the wage discussion was, to some extent, a scapegoat discussion covering more fundamental problems of mistrust and uncertainty among the parties. This is indicated by the quantitative measures provided by the research team. In all the meetings of the Wages Committee,

the actual time spent discussing the wage issue was considerably shorter compared to the discussions about company structures and hidden, unuttered interests by opposing groups. Real democratic change or, in the words of Jaques, tackling the "underlying morale issues" Field (Jaques, 1993, p. 393) is only achieved after a new council, the Shop Council was established. This new council allowed for a change in the discursive formations, which eventually facilitated cooperation among management and the workers' council for the much-appreciated benefit of the workers on the shop floor. As such, the Glacier study invited a shift from a strong emphasis on "group decision" as pronounced by Lewin toward group discussion and dialogue among the stakeholders involved. In other words: Glacier marks a shift from "democratic decision" to "dialogical participation" in direct and representative forms and with the participation of everyone who has a stake in the final decision. This dialogue had to take place outside the common structures to unfold and tackle the "real" issues. This procedure is reminiscent of the general tendency in the Northern Tradition to establish roundtables or group discussions with all stakeholders involved outside of the regular production process.

The Glacier Metal Company project points to another significant issue in Action Research: The researchers cannot take a neutral position for granted. Instead, it is more likely that intervening researchers will be regarded as the representatives of particular interests. And in the worst case – as seen from the Glacier example – these allegations are multiple and contradicting, depending on the groups that bring the allegations forward.

The Tavistock Coal Mining project began in 1948, and its initial phase lasted two years. Its primary goal was to analyze productivity in coal mines in the South Yorkshire coalfield in Great Britain. The coal mines in Great Britain were nationalized on January 1, 1947, following the Coal Industry Nationalisation Act (1946). The reasoning behind this nationalization was the immense importance of the Coal Industry as the primary provider of natural resources in Britain. The Tavistock researchers were interested in huge observable differences in productivity output from coal mines. Also, reports about epidemic-like occurrences of psychosomatic disorders among workers had shown a relationship between psychological illnesses and mechanized work conditions (Morris, 1947; Halliday, 1949; Dickson, 1936) but so far, the relationship between health, productivity, and the social structure was not investigated – which was partly due to the difficulties stemming from private ownership and the unwillingness of mining companies to collaborate and partly due to the relative novelty, at least in the magnitude of the phenomenon ever since the mechanization of coal mining and increased labor division. Comparable to the Glacier Metal Company attempts to increase productivity via wage adaption and improved equipment failed. Eric Trist was the primary researcher and worked closely with K. W. Bamford, a coal miner for eighteen years (Trist & Bamforth, 1951, p. 3). Given Trist's indebtedness to Lewin's work, he approached the study from a field theoretical perspective:

> In the account to follow, the longwall method will be regarded as a technological system expressive of the prevailing outlook of mass-production engineering and as a social structure consisting of the occupational roles that have been institutionalized in its use. These

> interactive technological and sociological patterns will be assumed to exist as forces having psychological effects on the life space of the face-worker, who must either take a role and perform a task in the system they compose or abandon his attempt to work at the coal face. His contribution to the field of determinants arises from the nature and quality of the attitudes and relationships he develops in performing one of these tasks and taking one of these roles. Together, the forces and their effects constitute the psycho-social whole which is the object of study. (Trist & Bamforth, 1951, p. 5)

For Trist, the field is a psycho-social whole, and forces active therein do not simply produce a specific behavioral outcome as in Lewin's logic. Instead, they force the person in the field to take a particular role and fulfill the tasks attached to that role. While for Lewin, the forces active in the field determine the will of the person (in conjunction with the underlying personality, which appears in the form of "habits" rooted in the person's "culture"), Trist invokes a notion of oppression. The subject, in his case, seems to have a choice, namely, to "either take a role and perform a task in the system … or abandon his attempt to work at the coal-face" (Trist & Bamforth, 1951, p. 5). If it was Lewin who reintroduced the subject to (applied) psychology as an object of scientific study by claiming the lawfulness of the individual case and thus the particular situation and its dynamics as the beginning of psychological research, it could be argued that Trist reintroduced the subject as subject to the investigation of labor conditions by adding the idea of "choice" and implying a conscious relationship between the subject and the forces that govern behavior. However, due to political pressures, Trist fails to spell out his theoretical framework. Trist and Bamford's conclusions implied the necessity for a radical shift in the organizational hierarchy of the coal mining industry, a change the National Coal Board was unwilling to make. They subsequently agreed to the publication of a highly censored research report and prohibited further investigations in the South-Yorkshire coal field (1993, p. xxi). Trist and Bamford's 1951 publication was the only one reporting from their research project for over a decade. It was in 1963 that Eric Trist eventually published the conclusions of the Coal Mining Studies in a book (Trist, 1963).

The original study Trist and Bamford carried out was a comparison of two coal seams, both in the South-Yorkshire coal field. One seam was characterized by very low productivity, high absenteeism rates, and sickness. The other seam painted a contrasting picture: productivity was extremely high, absenteeism practically did not exist, and reported illnesses were below that of any other coal seam with available data. The primary difference between the two mines was the size of the "coal front," i.e., the width of the area that provided coal. Two coal extraction methods were applied in the two seams; one operated according to the "longwall method" while the other was forced to operate in terms of the "short wall method." The short wall method proved more successful, but not because of its technical superiority. As it became clear, the form in which labor was organized provided the advantage. Because Trist and Bamford were not allowed to publish these results in the early 1950s, they concealed their comparison of the two mines behind a fictional comparison of the working methods before and after the introduction of the longwall method. That which the report labels a description of labor organization in coal

seams from supposedly ancient times – that is, before the technical and industrial revolution – is a detailed description of the work coordination at the Haighmoor seam and the short wall method was practiced (1993, p. 23).

The conceptual grounds for the study are drawn from Lewin in two ways. First, the conditions in the seams are analyzed in field theoretical terms and forces governing individual behavior. Second, workers and machines – or technical equipment – are conceptualized as a single unit. To understand the forces in the field, it was thus necessary to understand the concrete conditions of work in a seam, including the technical necessities implied by the technology in use. Trist argued that the shortwall method allowed for a social equilibrium to be established among the coal workers. They worked in small teams of two to four with relative autonomy. One so-called semiautonomous group was responsible for the whole process, from facework to delivering the coal outside the seam. And while these small groups worked independently in the mine, there was a larger social connection among the workers as a whole stemming from strong social bonds of the families where families of injured or killed workers, for example, were taken care of by other workers and their families. The longwall method, in contrast, interrupted this social equilibrium and failed to establish a new one (Trist & Bamforth, 1951, p. 10).

In the longwall method, a direct advance into the coal on a continuous front is made, hence the name "longwall." Instead of a small team of workers responsible for the whole process, labor is divided into three shifts, every shift lasting seven and a half hours. A complete cycle of coal getting can be accomplished in 24 hours. The first shift is responsible for the cutting, the second for ripping, and the third for filling the coal. One cycle extracts an average of 200 tons, with 40 workers needed for the process. An average mine in England in the 1940s operated between 12 and 15 longwall faces at a time (Lowrie, 2002, p. 240). This layout bears several significant effects on the organization of labor. First, the unity of the task of coal extraction is broken into three parts, with a single worker never completing the whole cycle on their own. Second, the small groups of the short wall method are replaced with larger groups of different organizational structures. While the small groups work together closely, that is, in close physical proximity, and are thus able to communicate with each other despite darkness, dust, and noise, greater spacial distances between the workers make communication in the longwall setting nearly impossible. Thus, even though the group of workers is larger, workers are, in fact, more isolated. The work in small groups consists of different tasks and allows for specialization within the small group and recognition of workers for proficiency in specific tasks. The longwall method, in contrast, forces all workers of the same shift to fulfill the same repetitive task from the beginning to the end of a seven-and-a-half-hour shift. Trist speaks of

> psychological and social difficulties of a new order [that] appear when the scale of a task transcends the limits of a simple spatiotemporal structure. … Once a job is too big for a particular group, a multiple group comes into existence, composed of several sub-groups of the singular type. In these differentiated organizations of intermediate social magnitude, problems of inter-group relationships are superimposed on and interact with the intra-group tensions of the primary components. In the longwall production unit, the scale of the task

> introduces the contradiction of spatiotemporal disintegration as a condition of multiple group integration. (Trist & Bamforth, 1951, p. 14)

The workers are still part of an interdependent network but are singularized given their tasks. Payment still relied on piece rate, but since the second shift and particularly the third shift depended on the quality of the work done by the previous shifts and the first shift relied on the work of the third shift from the previous day, workers can easily suffer an income loss they are not personally responsible for. Since workers were usually trained only in one of the several tasks, they always worked the same shift. This breaks the bonds of the larger social groups and even prevents groups from participating in ongoing social activities due to their work responsibilities at different times of the day. While the job description of a short wall worker calls for a miner who can perform all the tasks, thus constituting one large group of equal miners as colleagues, this description is split into three jobs under the longwall condition. It, therefore, replaces the unity of the miners' community with a hierarchical order depending on the concrete labor task of the individual. In addition, the kind of Taylorism introduced with the longwall method differs from labor division in a factory setting. While conditions in factories are usually relatively stable, with changing conditions only occurring if, for example, a machine fails or new products are introduced, work in the mine is heavily defined by constantly changing environmental conditions: coal might be hard or soft, the floor may rise or the roof collapse, air ventilation might be inadequate in specific places, and so forth. A change in condition usually means more work, which is not productive in the strong sense and thus usually not paid. Workers have labeled such tasks "bye-work" (Trist & Bamforth, 1951, p. 20). The amount and frequency of such potential problems result in constant anxiety among the workers since different problems might add up to a point where either shift runs behind schedule to the extent that the fillers cannot fill anymore and the whole cycle is interrupted, which leads to a loss in production and a loss in payment of at least one day per person; a process which Trist calls "the magnification of local disturbances" (Trist & Bamforth, 1951, p. 21). The only person per shift who is officially responsible for avoiding significant disturbances in the production cycle is the deputy. However, given the conditions below ground, such as high noise, density, and limited sight, the deputy cannot supervise the work processes; they are confined to be the messenger of bad news to the management.

Trist summarizes the conditions underground as "the norm of low productivity:"

> The crises of cycle stoppages and the stress of the deputy's role are but symptoms of a wider situation characterized by the establishment of a norm of low productivity, as the only adaptive method of handling, in the contingencies of the underground situation, a complicated, rigid, and large-scale work system, borrowed with too little modification from an engineering culture appropriate to the radically different situation of the factory. (Trist & Bamforth, 1951, p. 23)

Bearing heavily on Kurt Lewin's field theory, the Cole Mining study by Trist was the first large-scale investigation into the connectedness of work and social life. It

showed that work based on some form of formal small-group organization with relative autonomy is superior in the context of mining because it provides more flexibility to handle unforeseen difficulties, makes possible strong social bonds between the workers, and reduces illnesses caused by psychological issues or physical injuries, partly because workers in small groups feel responsible for one another and partly because the ability to fulfill the whole production cycle provides a sense of purpose to the workers.

On a methodological level, the Cole Mining study provided the groundwork for later socio-technical analyses. It offered an understanding of the conceptual unit worker/machine based on both elements and their interplay. This is in line with Lewin's concept developed during the studies in the German textile industry.

However, there is a crucial difference between Lewin's worker/machine unit and the concept developed by Trist. For Lewin, optimization was the reason to look at the two components as a single unit. He showed that workflow optimization is not simply achieved by replacing a bad machine or operator. Instead, he showed that unforeseeable situations of multiple failures had to be accounted for to increase productivity. Trist relies on two factors, one theoretical and one empirical. On a theoretical level, he added the element of "choice" into the Lewinian force field. Specific forces do not simply govern the person; she is a conscious being who can respond to and act upon the forces that affect her. Empirically – and very much to his surprise – Trist showed in another investigation carried out together with Frederick Emery in the mines of North-West Durham during the years from 1955 to 1958 the successful "working of a conventional, semi-mechanized, three-shift longwall cycle, by a set of autonomous work groups" (Trist, 1981b). Groups of 40–50 miners organized themselves, exchanged various tasks, and took responsibility for their shift schedules. While officially paid at piece rate, they collected all earnings and redistributed them by labor conditions, eliminating the effects of bye-work. Compared to an identical longwall situation with a traditional work organization, the output was 25% higher, the costs lower, and absenteeism had been cut in half (Trist, 1963). Trist realized that he had found empirical evidence that the longwall method could be productive, given a different form of labor organization, that there was a moment of conscious choice not only about the role a person decides to fulfill but also about the way this role should be fulfilled: Human and machine were dependent and independent at the same time. Dependency arises from the human–machine interaction as a production unit. However, they are independent on a conceptual level because there is a conceptual choice about how labor is organized. This conceptual choice reveals that the machine does not determine the mode in which it is used. Instead, technical requirements can be dealt with in multiple ways that depend on choice. Thus, it is necessary to think of machine and person as an entity in analytical terms and to analyze them as different units on the conceptual level – with the persons organized in groups always exhibiting the capacity to make decisions about their relationship to the technological world.

From an Action Research perspective, it must be noted that the Coal Mining studies, even though they utilized continuous contact over a period of two, and in the second case, three years with some 20 key informants who represented between

them the various coal-face occupations and held repeated discussions and extended interviews with this group, interviewed the management on all grades of personnel up to area manager and even used three psychiatrists with experience of miners' problems as informants, it cannot be considered an Action Research project in the narrow sense (Gustavsen, 2008, p. 423). Due to monetary, time, legal, and political restrictions, the researchers could not engage with the workers during the work process.

Nonetheless, the Coal Mining study, as well as the Glacier Metal Company project – which equally fails to implement researcher participation beyond the role of a consultant – yield several critical theoretical insights for the formation of the Northern Tradition of Action Research. In adhering to Kurt Lewin's field theory approach, the work of the Tavistock Institute at the same time widened the scope in several key areas. For Lewin, the subject was important in the here and now as a function of the situation. His goal was to manipulate the subjects in the field – the forces determining the behavior – to produce a positive outcome for all. Democracy was a vehicle to achieve this since his experiments showed that democracy proved to be more productive and more satisfactory – reducing frustration and thus violence, and as such, increasing the "space of movement" for the individual – than other forms of government in the local as well as the global arena. Democratic vote was superior to autocratic structures because individuals seemed to accept changes more readily if they were brought about by democratic vote – democratic participation was not an end but a means. The goal was to manipulate behavior based on the forces in the field. "Manipulation," for Lewin, was a tool of science that could be used for good or bad ends.

Jaques added a new level to these socio-spacial dynamics: He differentiates between a surface structure of dialogue – which in his study was centered on the negotiations of a new wage structure – and an underlying conflict, which was not made explicit but, in fact, the actual battlefield – namely, the fear of management having some "hidden plans," which would be to the disadvantage of the workers. Unless this underlying conflict was harbored, there was no way of conflict resolution. This implicit sphere is not equivalent to Lewin's concept of culture as a basic, underlying, ultimately determining structure for a person's behavior. Instead, it resembles an unconscious framework for substitute conflicts. Solving these surface conflicts will not alter the conflict situation. Rather, implicit assumptions need to be made explicit. Consequently, direct worker involvement in the decision process must be organized outside conventional committees and representational structures since they reproduce underlying conflicts.

The Coal Mining study adds yet another important feature. While it was concerned with the investigation of productivity and ultimately able to show that the longwall technique of coal extraction necessarily rested on low productivity of the individual worker to prevent the whole line of production from crashing, it went off from a different point of departure: It is a lost "social balance" that results in psychological illnesses among the workers that provided the primary reason for his study (Trist & Bamforth, 1951, pp. 4–5; Morris, 1947; Halliday, 1949; Dickson, 1936). The Lewinian whole of the situation is thus amplified by another factor: the

psychological question of mental well-being. The consequences of this move are far-reaching. Had psychiatry previously been the treatment of the sick in designated places and psychiatric intervention limited to the treatment of the "insane," is it now entering everyday social life, "looking beyond its traditionally limited role and addressing the management of society and its problems" (Miller & Rose, 1988, p. 182). The Coal Mining study successfully manages to incorporate the societal structure upon which work rests into the complex factors under investigation in the measurement and improvement of productivity; it introduces work regulations to protect from physical injury and a favorable climate of interaction with others, a sense and understanding of the whole process of work, identification with formal groups, and the incorporation of human choice to protect and support mental health as a primary source of productivity for the worker. As such, an altered perception of the meaning and significance of work was central to the "Tavistock Mission" (Dicks, 1970): "The integration of all actors in production in a joint partnership for productivity, contentment and democracy" (Miller & Rose, 1988, p. 182; Rose, 1990). Conceptualizing production as a social process inextricably intertwined with the greater social realm allowed for a different rationale for democratic structures. Democracy evolved from a means to an end, from a tool to a value for society (Taylor, 1952); Brown, 1954). The Lewinian strong leader who manipulates their followers to vote for the right cause disappears in favor of essentially "leaderless" autonomous groups (Miller & Rose, 1988, p. 184). This new framework gave rise to what is now called the Socio-Technical Perspective (Trist, 1981a).

Socio-Technical Systems: The Theoretical Basis

STSD developed as a consequence of the coal mining studies in Britain. Eric Trist and Frederick Emery, two of the main protagonists of these studies, were responsible for the theoretical underpinnings. Thus – precisely because of Trist's influence – it connects to Kurt Lewin's thinking and carries a similar ideology. Emery and Trist developed what Gustavsen termed a "structurally oriented theory" (Gustavsen, 1985, p. 461). A theory that offered concrete roadmaps for society to be changed for the better. The essential components of this theoretical framework can be summarized as follows: There is a social side and a technical side to work in particular and to life in general, and they function together as a system. This system can be modified; that is, it can be *designed*. Undoubtedly, Lewin himself was interested in redesigning work processes to make them more efficient and democratic – even though democracy served, as we have seen, as a tool to facilitate decision. Moreover, his Galilean mode of thought shows the basic characteristics of a system theory. As I have shown earlier, this idea derives from Cassirer's concept of science. While Lewin actively reflected on the systems character of science, he never explicitly tackled the consequences of this underlying concept of his field theory. However, it seems that the systems idea is present in the field approach: Following the Gestalt theorists' dictum of a qualitative difference between the whole and the sum of its

parts – what Max Wertheimer called the *Ganzeigenschaften* (Wertheimer, 1922) – Lewin realizes that a group follows different laws compared to an individual subject. Moreover, there is no direct connection between the attributes of the parts and the attributes of the whole. One cannot be deduced in any way from the other. Groups have to be regarded as a stable whole even though there is movement on the inside; groups are visible as groups if they are in a state of equilibrium, an open system.

Lewin's field theory as a system-theoretical approach thus connects to a larger movement of system-theoretical thinking in the twentieth century, most prominently perhaps Talcott Parsons's Action Theory, Niklas Luhmann's System Theory, and Bruno Latour's Actor-Network-Theory (Parsons, 1951/1970; Luhmann, 1970/2009; Latour, 2005). Emery and Trist argue that the Austrian-American biologist Karl Ludwig von Bertalanffy (Trist, 1981a, p. 25) offered the first general systems theory in a paper on open systems in physics and biology published in *Science* in 1950 (Bertalanffy, 1950). Bertalanffy thinks of any entity as a system with internal self-regulating processes that produce a "steady state," – what Lewin called "quasi-stationary equilibria." This steady state is the product of a regulated flux. The whole system is not closed, i.e., not autarchic but dependent on constant input and output from the surrounding environment. The crucial element is the regulation of the boundaries of this system. Emery transfers this concept from the natural sciences to the world of social production in the following terms:

> The technological component, in converting inputs and outputs, plays a significant role in determining the self-regulating properties of the enterprise. It functions as a primary boundary condition of the social system in mediating between the ends of an enterprise and the external environment. Because of this, the materials, machines, and territory that make up the technological component is usually defined as "belonging" to an enterprise. They represent, as it were, an "internal environment." Thus, it is not possible to define the conditions under which an open system achieves a steady state unless the "system constants" include mediating boundary conditions (cf. von Bertalanffy, 1950). The technological component has been found to play this mediating role. It follows that the open system concept, as applied to enterprises, ought to be referred to the socio-technical system, not simply to the social system. (Emery, 1972, 1978, 1993, p. 2)

The socio-technical design approach was heavily influenced and shaped by the coal mining research projects in Great Britain. Had jobs previously been designed as a function of technological advancement with humans being reduced to mere operators of distinct tasks and increased labor division, was it now the challenge to reintroduce the human being into the work process. A challenge that is thought to arise from the supposedly lost unity of the working experience as a consequence of the industrial revolution: (Thorsrud, 1968; Emery, 1972; Engelstad, 1972; Herbst, 1974; Trist, 1981a; Pasmore, 2001):

> The outstanding feature of the social pattern with which the pre-mechanized equilibrium was associated is its emphasis on small group organization at the coal face. The groups themselves were interdependent working pairs to whom one or two extra individuals might be attached. … Leadership and "supervision" were internal to the group, which had a quality of responsible autonomy. The capacity of these groups for self-regulation was a function of the wholeness of their work task. (Trist & Bamforth, 1951, p. 6)

In contrast, during the industrial revolution

> [i]t was found that most of the known industrial processes could be analysed into a sequence of simple operations. Once that is done, the task is no longer an art, but becomes a predictable determinate mechanism. It does not matter at this stage whether the operations are performed by a machine or a human being. (Herbst, 1974, p. 14)

The small group design was based on Lewin's experiments on group decisions and Wilfred Bion's concept of leaderless groups (Bion, 1946). Bion worked with the British Army during World War II and was involved in selecting army leaders. He proposed to the army that the selection for promotion by superiors should be supplemented by peer groups, which choose individuals from within their group to climb up the ranks. The reasoning behind this was that peers were equally or even better qualified to judge the leadership abilities of individuals in the group. Later, Bion revised his recommendations and advised the army to drop leadership appointments for small groups altogether since leadership would unfold by itself (Foresti, 2011, p. 104).

In the Taylor System, the technological system determined the human task. Technological progress prescribed how machines were applied. Technology determined how humans grouped around the machines. However, "since human beings, unlike machines, are capable of an extremely wide range of behaviour and variability, coercion has to be introduced to elicit from the operator the required set of responses and to prevent him from engaging in any other type of behaviour" (Herbst, 1974, p. 14). Thus, it was necessary to introduce a hierarchical system of control where one level is responsible for the level below. Entirely new job profiles, such as foreman or charge-hands, had to be established to guarantee the working force engaged in production. As a function of the necessity of control, tasks were further broken down into smaller units to allow for comparison and supervision. Efficiency was either improved with new machines or with (financial) incentives for the workers to increase output.

The STSD tradition, in contrast, offers a tripartite argument against the Tayloristic structure: First, STSD researchers had data to show that work under the Taylor system was psychologically and physically unhealthy (Dickson, 1936; Morris, 1947; Halliday, 1949). Second, as was shown in the Yorkshire coalfield, new technology and increased labor division did not necessarily improve efficiency. On the contrary, it could lead to a "norm of low productivity" (Trist & Bamforth, 1951, p. 23) to prevent interruptions of the production cycle as a whole, and third, as was shown in the North-West Durham coalfield, technology did not determine the mode of work; instead, the organization of production was a matter of choice (Trist, 1981b). Moreover, increased labor division and specialization create a rigid and inflexible system with difficulties adapting to changes in the production line (Emery & Thorsrud, 1976, p. 2).

What is more, STSD scholars argue that efficiency and productivity cannot be measured by output rates alone. Productivity and efficiency are based on the actual production output and the machines used, management decisions, workers' representatives, union strategies, the input from and output to the environment, etc. Based

on the systems approach, productivity cannot be judged or improved based on the optimization of a single component; instead, the optimization of one component to its maximum might be counterproductive to the efficiency of the whole system: "Inherent in the socio-technical approach is the notion that the attainment of optimum conditions in any one dimension does not necessarily result in a set of conditions optimum for the system as a whole." Quite the contrary: "The optimization of the whole tends to require a less than optimum state of each separate dimensions" (Trist, 1963, p. 7) because

> [t]he technical and social systems are independent of each other in the sense that the former follows the laws of the natural sciences while the latter follows the laws of the human sciences and is a purposeful system. Yet they are correlative in that one requires the other for the transformation of an input into an output, which comprises the functional task of a work system. Their relationship represents a coupling of dissimilars that can only be jointly optimized. Attempts to optimize for either the technical or social system alone will result in the suboptimization of the sociotechnical whole. (Trist, 1981a, p. 37)

The social and technical systems are based on a different logic. For analysis, their interplay is at focus. The technological side seems to follow engineering laws – machines can be optimized based on these laws. For the social side, Emery posed a list of needs that had to be accounted for in a job: (1) The job needed to be demanding and provide some variety, (2) the job needed to provide chances for continued learning, (3) areas of individual decision making, (4) recognition and social support, (5) relation of the work to social life, and (6) a prospect of some desirable future. These universal and axiomatic requirements were seen only to be fulfillable if interlocking occurred, i.e., work in groups would be the norm (Emery & Thorsrud, 1976, pp. 15–16; Trist, 1981a, p. 42). These groups would be autonomous in that they form open systems. The foremen (sic) take the role of boundary control but not in the sense of supervision, as in the Taylor system. Still, in the form of input-output control, i.e., they provide the necessary resources for the groups to function. Group functioning, then, depends on the ability to self-regulation. Self-regulation is increased with learning and thus control of the "space of free movement." The groups together form the primary work system. Within the primary work system, one member is usually part of several overlapping work groups, i.e., membership in the regular working group, task forces for specific purposes, union representation, etc. The resulting network is described by Trist as follows:

> The development of self-standing primary work systems containing mixes of groups with commonly shared skills, matrices whose members have partly overlapping skills, and networks of mainly specialist skills constitutes a new basis for the effectiveness of sociotechnical organizations. They create organizational units of considerable robustness, which compose microsocieties having intragroup, intergroup, and aggregate relations with a whole operational task. These microsocieties provide considerable space of free movement to the individual and are open to the interorganizational environment. (Trist, 1981a, p. 49)

Following this organizational pattern, the whole-part relationship is inverted in that it is not the individual and their task that is reflected in the whole, but the whole that is reflected in the potentialities of the individual group. This leads to a state

where only "minimum critical specification" is required to assure the functioning of the whole system (Herbst, 1974, p. 19ff).

Emery, Trist, and later P. G. Herbst spell out procedures to achieve the desired changes in a system through researcher intervention. These procedures were developed and refined during the Norwegian ID Project. One of the crucial points is that change cannot be achieved if the governmental structure – the board as distinct from the management – denies changes, as was experienced in the Coal Industry in the UK. If the board of an enterprise is willing to accept changes, the first step would be to identify subsections of the corporation where change is most needed. Within these subsections, departments are chosen, and local management and workers are included in the process. A task force is constituted to undertake a socio-technical analysis and recommend changes. The idea behind this procedure is that individual departments will function as "outstanding examples" and set in motion an automatic effect of diffusion that will take hold of the whole enterprise, other enterprises in the same industry, and other related sectors until the state level is reached. Nation-states become models for other nation-states, ultimately leading to a new industrial revolution (Herbst, 1974).

The ability to produce change on the macro-level is embedded in a social ecology best spelled out in Emery & Trist (1973) and Emery & Trist (1965). Therein, the development of a society is described as a constant increase in complexity. By way of this process, more and more units of society become interlinked until a stage they call "turbulent" is reached. In turbulence, the effects of one system on another affect the effect produced by the system from which the effect originated. In this stage, relatively modest changes are continuously amplified and disturb the equilibrium of the whole, eventually creating significant upheavals and crises. These problems are not solvable by conventional means of rational planning or hierarchical organization. Instead, as it seems to Emery, the existing concepts of social organization are responsible for these problems. The only way out of this vicious cycle is a set of values that all agree on (Emery & Trist, 1965, p. 28). These values are, however, themselves subject to constant flux and can, therefore, not be determined from the top or decided upon by an intellectual elite. Instead, they must be the product of a continuous negotiation process with everyone's participation.

Based on this framework, Emery underlines the importance of democracy in the workplace: To give control to the workers in small groups instead of supervisory power increases flexibility and adaptability of the system toward changes in the environment. Turbulences will inevitably occur everywhere in the future. It is, so Emery explains, like a company of soldiers marching in step over a bridge. The coordinated movements of the soldiers bring the bridge into oscillation and affect the soldiers: "The 'ground' is in motion" (Emery & Trist, 1965, p. 26; Herbst, 1976, p. 20; Herbst, 1974, pp. 31–32). Changes in the environment produced by the system affect the system itself. Consequently, "planning in organizations and communities must be done by the people, for the people, and with the people." Because turbulence produces "a paradox: the more society changes, the more we need to plan, but the knowledge we need keeps receding from view." It seems that "we have been looking for knowledge in the wrong sort of places – and for the wrong sort of knowledge" (Emery & Purser, 1996, p. ix). STSD aims to provide a plan for action to counter these developments.

In summary, the fundamental principles of socio-technical design can be characterized as follows:

1. STSD is a holistic approach. The whole is an open system that allows for investigating the system itself and the system–environment relationship.
2. On the technical side, a labor system, i.e., a set of activities that define a functioning whole, is seen as the basic unit of the work process.
3. On the social side, the group is the basic unit instead of the individual.
4. Changes should be regulated internally by the group instead of supervisory changes from the outside, following the idea of self-regulative systems.
5. Instead of looking for the redundancy of parts (i.e., fewer workers with more workload or faster machines), redundancy of functions is the goal
6. Development of multiple skills per individual is aimed at (and seen as a human prerequisite) to increase the response repertoire of the group.

Against this background, STSD follows the Lewinian approach. Socio-Technical Systems Design is, however, not just a theory about groups. While Lewin's theory of group dynamics, in comparison, certainly had broader underpinnings which are reflected in his theory of culture and re-education, these underpinnings ultimately served the purpose of backing up his ideas about group dynamics in a narrow sense. His broader reflections on society, in turn, were derived from an effort to generalize the results of his experiments. The STSD approach comes, so to say, from the opposite direction. Instead of creating a greater social theory framework to incorporate, explain, and justify empirical results, STSD provides a theoretical framework to which the empirical world must be adapted.

Frederick Emery broke with Marxism in the 1950s to develop his theory (Emery, 1982). Marxist thought can, however, be found in STSD theory. Prominently among these is the role of production as fundamental to human life. The most robust expression of this conviction in STSD can be found in P. G. Herbst's summary of the STSD approach. The book's epilogue is entitled "The Product of Work is People" (Herbst, 1974, pp. 212–218). Herbst writes:

> That which we have achieved is always in the past, and as soon as it is achieved, it is no longer truly related to us. However, what we have become – our qualities as human beings and our potential for future development – is always in the here and now. After everything else is gone, that which we have become remains, whether we recognize this or not, the product.

In this context, democratization is set against the fear of alienation:

> However, one can use one's work and one's inner strength to produce no more than an outer shell of success, possessions, and pretensions, and then towards the end of one's career, one may quite literally suddenly experience oneself as an empty, burnt-out, and rigid shell, and find that one has gained little of value. (Herbst, 1974, p. 213)

Emery and colleagues hoped to form an all-encompassing theory that would explain production in the narrow sense and society at large, incorporating the modes of production as the most fundamental axiom (Emery, 1977):

> In the sociotechnical field as a whole, the knowledge base is unevenly distributed. Much is known about primary work systems and a good deal about modeling new plants. Far less is

> known about transforming existing work establishments and even less about sociotechnical processes at the macrosocial level. The payoff from directing research attention to this level would be considerable. (Trist, 1981a, p. 60)

In a concretization of Marxist principles, STSD theory is supposed to show concrete action to pursue, namely – and this is in line with Kurt Lewin – actions that would foster democratization. While for Lewin, this belief derived from the Democracy/Autocracy Studies, for Emery, democracy was a function of his theoretical superstructure. The justification for democracy as a goal cannot be found in empirical results in Emery's case; instead, it is legitimized by basic human needs. They are spelled out as follows:

1. The need for the content of the job to be reasonably demanding (challenging) in terms other than sheer endurance and yet providing some variety (not necessarily novelty).
2. The need to learn on the job and go on learning (which implies known and appropriate standards and knowledge of results).
3. The need for some area of decision-making that the individual can call his [sic] own.
4. The need for some minimal degree of helpfulness and recognition in the workplace.
5. The need to be able to relate what he [sic] does and what he [sic] produces to his [sic] social life.
6. The need to feel that the job leads to a desirable future. (Emery & Thorsrud, 1976, p. 14)

From within this framework, it is possible to introduce such distinctions as necessary (provision of recourses) and unnecessary (control) management and such requirements as "semi-autonomous groups," "participation in company decisions," "top management as the leader of the company – which ought to be a democratic leader in the Lewinian sense – instead of being the administrator" and so forth.

While there are several overlapping features between Lewin and STSD, such as the aim for democracy, the systems approach, the focus on small groups, the element of participation, etc., there is a conceptually incommensurable difference between the two. Lewin believed in a gradual development of society and culture, which culminates in democracy, and a gradual development of science which – in Galilean form – could help to foster societal progress. The components of his theory are defined along the lines of empirical findings, which need to be understood so that they can be influenced. STSD, on the other hand, poses an overall goal: The radical restructuring of society according to a participatory ideal. The components of the theory are defined downward from this overarching ideal. The subject in Lewinian terms appears underdetermined; it is the sole product of forces in the field, changeable only by an alteration of the forces, which proves to be an endeavor of gradually increasing difficulty depending on the level at which change is supposed to take place: within the experiment, changes to the forces are easily made while on the cultural level, the only solution is revolution or reeducation. The STSD subject, in contrast, is overdetermined in that it is based on a concept of an ideal state the theory aims to achieve. For a world in which everyone participates, a subject must have an inherent will to learn to control their environment; subjects need to be recognized and given recognition to understand the general production process, and so forth. This subject is not an empirical but an ideal construction – a subject ideally pictured for this new societal structure.

Most importantly, though, the STSD approach assumes the empirical subject as the ideal one, a conceptual flaw that, in its overdetermining effects, partially explains the ultimate failure of the Norwegian ID project.

Socio-Technical Systems: The Application of Democracy in the ID Project

As mentioned above, the ID Project failed not because of structural or empirical issues but because of a theoretical problem buried in the conceptualization of subjectivity. This conclusion, however, needs further differentiation: While it is true that the ID Project failed in comparison to its high standards and goals, namely, to change how work is organized in the Norwegian economy at large, it was at the same time thriving in several, more small-scale levels. First, change was achieved – not in all cases, but out of four projects, only one was a complete failure. Second, it provided further empirical evidence that participatory modes of work are, in fact, more adaptive to changes, more productive, and healthier than Tayloristic working conditions. And third, it provided an essential cornerstone for the development process of theory and practice in Action Research. In the following, the failure and success of the ID Project and its two phases will be examined more closely.

The first phase of the study had an exploratory character and tried to achieve a general understanding of existing participatory structures. For that purpose, extensive interviews were conducted in Norwegian companies with basic participatory structures such as worker representatives on the board of directors as a requirement for Norwegian companies entirely or partially owned by the state. The decision against a quantitative approach was made because of the limited number of companies and the relatively low number of people with experience in worker participation. The Companies chosen were Norsk Hydro-Elektrisk Kvaelstrofaktieselskab (Norwegian Hydro-Electric Nitrogen), Norsk Jernverk A/S (Norwegian Iron Works), Ardal og Sunndal Verk A/S (a company producing aluminum), and Raufoss Ammunisjonsfabrikker (a producer of firearms) as primary sources. Minor investigations took place at Norges Kooperative Landsforening (today, the Coop NKL, a cooperative that produced various consumer goods, such as food, tobacco, shoes, light bulbs, etc.), and some interviews were conducted at the national water and electricity company. In addition to the interviews, which always included workers, worker representatives on the board, union leaders, shop stewards, members of the management, and in every case, the managing director – summing up to a total of 30 people interviewed several times and covering all the ranks of the companies – access was given to written material including memos of past meetings of the board and documentation of worker participation activities (Emery et al., 1969, Appendix I). Interviews were semi-structured with a fixed set of questions (45 mandatory questions). The questions centered on the following topics: Activities before becoming a board member as well as the process of becoming one (appointed/elected,

etc.), preparation material/introduction provided for the task, relationship to other stakeholders on the board (such as management, union), relationship to fellow employees, personal family, and friends, conflict situations, subjectively most interesting topics discussed, personal assessment of the functions of the board, qualifications needed to be a good board member, and suggestions to improve the system (Emery et al., 1969, Appendix II). In addition to the interviews and the written material provided by the companies, the researchers analyzed the general political attitudes toward industrial democracy – they conducted what could be called a "discourse analysis." Speeches of Prime Minister Gerhardsen, The secretary of the workers Union Alf Andersen the Minister of Social Affairs Olav Bruvik, the director of Akers Mek (a major ship-building company), the president of the Norwegian Association of Manufacturers S. Walter Rostoft, brochures published by the unions, proceedings of the Aspengren Committee (a committee of the LO), as well as other speeches, articles, and publications concerned with democracy in industry were analyzed.[2] The results of this analysis showed unanimous agreement across all interest groups that democracy in industry needed to be strengthened. Specifically, the laws in place at the time to ensure worker participation were recognized as unfolding a dual function: On the one hand, they guaranteed participation; on the other hand, they confined participation to the committees, not the actual shop floor. Politics in Norway at the time thus aimed at democratizing the workspace as part of the public sphere. The hope was, among other things, to create higher commitment from the workers, and higher productivity. While all sources analyzed argue for more participation and more democracy beyond a representational system, it is striking that no practical plans to achieve these demands are given.

The interviews showed a basic shared notion of democracy among the participants. For the interview study, the ID Project relied on Karl Mannheim's definition of democracy:

1. That men are assumed to be equal human beings. If, on the contrary, some are considered to be of inferior caste or to be second rate, then despite any formal arrangements, they will tend to be deprived of effective representation.
2. That all men have such freedom of movement in their daily lives that they may, if they desire, make an autonomous contribution to the life of the community. If, on the contrary, men are extremely restricted by the need to earn a living, by censorship, or in other ways, then no formal arrangements for representation will create an effective democracy.
3. That the leadership is removable by, and responsible to, the many. If the available leadership is, on the contrary, controlled by some political party or machine, or loyal to a narrow social stratum, then elections and the like will not ensure effective democracy. (Mannheim, 1956, pp. 177–179)

The central conclusion about the function of representation drawn from the interviews were: Participation implements the right of the workers to be heard on the managerial level.[3] Workers would, to some extent, control management and restrict

[2] A detailed account including quoted sections of the material can be found in Emery et al., 1969, Chapter 1.

[3] Excerpts of the interviews conducted with workers' representatives can be found in Emery et al., 1969, Chapter 2.

their actions. The probability of decisions in the interests of the workers could be increased because of the additional expertise brought to the board. The questions about the individual conduct of the representatives however revealed that there was little communication between workers' representatives and workers and that many representatives felt that it was unavoidable for them to take "a company view" when it came to matters of production. These contrasts casted doubt on the effectiveness of the representational system.

The evidence found in the interviews, in conjunction with the analysis of the general discourse about democracy, rendered the existing model of participation as representation problematic. Even though the workers' board members were, in all instances, full members of the board, i.e., their participation was not limited to certain work-related topics, their actual participation highly depended on their personal interpretation of their role. For the representatives, the concrete question often was what they believed to be representing, that is, whose interests they understood to be promoting and how they conceptualized the group they were a representative member of. These varying interpretations can be captured on three levels:

1. Employees as a social class (including potentially all workers in the world)
2. Employees as a particular occupational group (i.e., metal workers, coal miners, etc.)
3. Employees of a particular company

Depending on the level the representative would associate with, different interests, arguments, and actions would result. On the first level, for example, it would be the goal of the representative to influence the power of management over the production process to influence society as a whole. Democratic processes within the company would most likely not be of much concern since the company is regarded as one of many companies within the – generally difficult – societal production process. On the second level, the representative would mainly be concerned with social welfare issues pertaining to his particular group, much in line with the efforts of labor unions. On the third level, a representative would be concerned with the concrete circumstances within the company: the boundary conditions of the workplace, such as problems with new production techniques, safety and comfort issues, issues of career development for the workers, and so forth.

While levels one and two call for political advocates, the third level relies on engagement with co-workers and awareness of specific problems and issues in various parts of the company. It speaks to a failure of the representational system that this kind of representative was not found in the empirical results. Indeed, the failure could also be attributed to a simple lack of appropriate staff. Still, overall, structural problems seem to be most strongly related to the setup of representationalism: The politicization of the workers' representatives appeared to be a result of the appointment process. In all instances, the representatives were appointed by the owners – in case the government entirely owned the company, the government appointed the representative in agreement with the labor union. In all interviews, the reason for the appointment was that the union had asked the particular workers to act as representatives (Emery et al., 1969, p. 69). This situation further complicates the matter

since many representatives saw their task as "union work" or the representation of "governmental interests" in the cases where the government appointed them. Others were unsure about their role in the beginning and later decided – or were even told by the general manager – that as board members, it would be their job to "act as such" and even make decisions that were "right against what you were there to represent" (Emery et al., 1969, p. 73).

In sum, the workers' representative on the board seemed to be an artificial addition – created by law – to an existing equilibrium. The process of appointment on the one hand and a lack of connection to fellow workers on the other rendered effective representation of the particular employees of a specific company impossible. Emery diagnosed a "failure to achieve democratic handling of the day-to-day problems [that] undermine[d] the work at higher levels of representation" (Emery et al., 1969, p. 42). In Lewinian terms, the lack of space for movement excludes the regular worker from participation since they are occupied with the narrow, highly specialized portion of the workflow they are responsible for. And it is the lack of cohesion of the group of workers that deprives the representative of an actual group with concrete interests to be represented.

The results of this first phase did not come as a surprise to the researchers. In effect, they resemble what had already been shown in the Glacier project: Actual dialogical participation could be achieved within the given structures of a company. Additions to existing committees are generally unsuccessful because a single person cannot alter the function of a specific body. Instead, the person becomes subject to the existing culture – in the Lewinian sense.

With this conclusion in hand, it seemed necessary to try new ways of participation that would not rely on representationalism in existing company bodies. The chance to conduct not just one Action Research project but four in four different companies provided ideal conditions to put the general methodological framework developed in STSD theory into practice. In the following, I will examine two projects in detail to highlight different aspects of the STSD approach in practice.

The first research site was the wire drawing department at Christiana Spigerverk, located in the traditional industrial area of the Oslo Valley. The company had recently undergone changes on the management level. The senior management that rebuilt the company after near bankruptcy in the 1920s had retired, and a new generation of managers came in. The company employed about 1200 workers and had a good standing on the national market though the competition was gradually increasing. Research went underway after the Union, the researchers, and the management had unanimously agreed to do so at a meeting on January 17, 1964 (Emery & Thorsrud, 1976, p. 28).

The wire drawing process at the time can be summarized as follows: A wire drawing bench is 10–12 meters long. Raw material in the form of large bundles of thick wire is stored at the beginning of the bench, which subsequently needs to be placed on the benches to run across for reduction in thickness – the goal of the process. At the end of the benches, the wire is taken off and bundled to be picked up and taken to the storage facility. The operator's main tasks are to weld bundles of wire together at the input end of the bench, fit the wire onto the bench through the

reducing dies, and bundle the finished wire for transportation after it is processed. The tools available to the worker are the welding machine, heavy tongs, and electrical motors that help push the wire on the bench. The drawers themselves are, for a considerable amount of time, inactive. Routine tasks are putting the wire on the machine, welding together, and bundling it for storage. However, immediate action is required if the wire breaks. If this happens, the worker needs to turn off the machines to prevent further breakages, force the wire back into the dies, and weld it together to restart the normal process. Thus, even though the operator is inactive for a great deal, they must constantly watch the machines for possible failures. The frequency of failures is not in the hands of the operator since breakages occur as a function of the quality of the metal (Wright, 2011, pp. 1–18). The necessary repairs in case of failure are comparable to the "bye-work" described in the coal mining studies: It needs to be done as quickly as possible to have the production process going again; it does not belong to the "productive work" in the sense of actually producing a consumer good, it is not within the control of the operator to determine when or how often these breakages occur, and it produces, even if absent, a constant level of stress for the worker – they need to be prepared to act quickly at any given moment, shifting from "slack time" to "rush time" (Emery & Thorsrud, 1976, p. 30). One worker is typically responsible for one bench (sometimes for two). Even though there were 20 benches in parallel next to each other at the Christiana Spigerverk, workers were expected to stick to their bench even if they have nothing to do and major hazards occurred on a bench right next to them. Communication is the privilege of the foreman and the repair staff (if needed); workers communicate during incidental meetings in the tool room where they would get new dies. Payments were made on a piece-rate basis, but the rate could be negotiated with the foreman based on the amount of "bye-work" that occurred, which meant that good negotiators would earn more money than others. Also, the quality of the benches differed as some benches were known to create more trouble regardless of the quality of the raw material or the operator. Thus, people with "good benches" were eager to protect their position, while workers with "bad" benches had to be good negotiators with their foreman. Typically, workers developed a specific attachment to their bench as their workplace. Due to this setup, the researchers encountered a paradoxical situation in their interviews with the workers. While nobody liked their job – often it was described as boring, dull, and lonely – only 2 out of a total number of 24 workers would have been willing to "give up" the system of having their "own bench" of which they were "the master" (Emery & Thorsrud, 1976, p. 3).

From the socio-technical analysis of the research process, which took a total of three months, the researchers drew suggestions about possible improvements and communicated them to the management:

1. Groups of workers should be at least four in size, with more workers than machines, to ensure a breakup of the old one-worker-one-machine system.
2. Wages should be based on a guaranteed minimum, which would ensure that the workers do not have losses of payment over the course of the experiment. Payment should be increased in case the workers could increase productivity.

3. Recruitment to the experiment should be handled voluntarily. Groups should constitute themselves.
4. The welding task at the input should be cut off from the rest of the process and done in rotation by two team members. The rest of the work organization should be left to the teams themselves.
5. It should be communicated that this was only an experiment, and participation in all parts, including the discussion of findings, was entirely voluntary. A duration of three months was aimed at.
6. The layout should be modified, i.e., barriers between the benches should be removed, and a central control station to shut off benches operated by one group should be installed.
7. The group should take up simple maintenance tasks.
8. The group should be provided with knowledge about their progress.

Several of these key suggestions could not be realized. The union insisted that the machines chosen for the experiments should be maned with the same number of workers as before, i.e., one worker per bench. This insistence stemmed from a fear that had developed among the workers that the program was essentially conceived to find ways of justifying layoffs. Increased efficiency was equated with reduced manpower to do a job. Moreover, workers had developed an attachment to their machines because of the one-worker-one-machine principle, which had been in place since the company existed. It was felt that efficiency for the worker depended on this relationship, that the same worker would be less productive on a different machine. Conversely, workers working on a new machine in the experimental situation felt they were unjustly taking somebody else's place. The matter was further complicated because it was impossible to buy new machines for the study, which meant that workers had to give up their working place to make room for the experiment. The consequence of this situation was that with one exception, the workers who had worked on the one-worker-one-machine system on the machines chosen for the experiment were equal to the group who enlisted in the experiment to stay with their benches. The experiment started with two groups (termed group A and group B) working in two shifts. As a consequence of the setup, the workers continued in the same vein as before, having one worker take care of one bench and only accepting to take turns on the welding. This forced some group cooperation since the workers who remained on the benches had to take responsibility for neighboring benches, even if this only meant stopping the machine in case of breakage. This slightly modified the work process since the welding off was no longer the worker's responsibility. The worker was instead required to watch two benches for possible interruptions.

At one point in time, when one of the workers in group B was absent, a researcher suggested running the worker's bench even though the group was one person short. The suggestion was followed, no problems were caused, and production ran well. However, when group A heard about the incident the next day, they complained since this was seen as a breach of the one-worker-one-bench agreement, and the bench of the absent worker was left idle again. Because workers, specifically in

group B, insisted on staying with their machines, only excepting to take turns in the welding process, work was still unevenly distributed and depended upon the frequency of machine failure. At the same time, the possibility to negotiate wages with the foreman in case many machine failures occurred was no longer in place since workers had switched from a piece rate to the hourly rate system creating a feeling of being disadvantaged compared to workers who had high outcome figures. As a result, in week four, the workers met and ended their participation in the experiment. This marked the end of phase I.

A new experiment was set up, this time with only one group on an entirely voluntary basis – allowing the workers who wanted to leave the experiment to stick with their machines under the condition that they would not work in the same shift as the experimental group. The existence of only one group nullified some form of competition, or at least a certain standard to be measured against, which made the workers decide to have a bench running even though the primary worker on this machine reported being sick and did not show up for work. Productivity in this group was higher than what would have been achievable under the old system with the same number of workers. However, it was lower than what the group would have been able to achieve with one more worker on the same number of benches. Since the workers had agreed with the union and the management on a group incentive system for the wage structure calculated by the group output, salaries for this group went up considerably. Spontaneously, a second group formed to take over another shift. Since the production output of the groups was written on a blackboard visible to everyone every day, the workers working on the old system could see that the productivity per worker in the group setting had increased. The group system had worked if success was equated with higher productivity. The workers felt that this was not in their interest since it could be used to lay some of them off.

Consequently, the experiment broke down after only six weeks, with the workers returning to the old one-worker-one-machine system where the individual worker would stick to their machines. Instead of group work, the workers now decided to take alternate breaks to have the machines running through the whole shift to increase productivity, with the individual worker watching two machines only if a colleague took a break. The union opposed these changes since they were not part of the original setup, and the workers decided to return to the system in place before the experiment. Although the experiment officially continued until the end of the three months, which meant that workers were theoretically free to form new groups and try new ways of production during the remainder of the time, production essentially returned to the traditional model after only six weeks.

The experiment at Christiana Spigerverk failed to achieve its goals on all fronts. Nonetheless, it marks several important insights for Action Research. Workers did feel that their job had become more “exciting” or at least more varied when working in a group. The qualitative interviews with the workers after the experimental period showed that most preferred the time spent in group work over the old system. Moreover, the role of the foreman, who was previously busy with negotiating wages and controlling the workers, turned to a group manager who helped provide materials and coordinate the availability of raw materials, transport of end products,

coordinate repair workers, and so forth, thus contributing to the actual workflow instead of being occupied with wage management and control. From there, two questions arose: Why was the change not implemented beyond the six weeks? What could have been done differently, and what could be done differently in the future to avoid such situations?

It seems that the workers were highly suspicious of the changes to be implemented. While they all agreed to a general notion of more democracy and more participation, and while they all disliked most of the characteristics of their job setup and enjoyed the participatory factor of the experiments, they were unable to estimate whether the proposed changes would meet their desires for improvement. As such, they preferred "the devil they knew over the one they didn't" (Emery & Thorsrud, 1976, p. 54). Moreover, they were afraid to be tricked into a situation that would eventually be to their disadvantage, much like the reports from the Glacier Metal Company project. As such, the problem of socio-technically redesigning work does not seem to rest so much on the technical side of redesigning the workplace but on the social side of implementing these changes. Consequently, the researchers established so-called "Action Committees" that would guarantee the participation and information of all stakeholders involved from the beginning to the end of the project.

The second project took place in the Hunsfos Pulp and Paper Mill, a major employer in the Vennesla community in the very South of Norway. Statistics from 1963 show that about 50% of the male working force in the community was employed at that company. The production of paper included all steps, from the preparation of wood to the eventual cutting, sorting, and packaging of the paper. In 1964, the company produced about 54,000 tons of paper and employed between 900 and 1000 workers. After intensive interviewing of workers and detailed surveys, the Chemical Pulp Department (CPD) was chosen as a suitable site because it was relatively autonomous from the other departments, contained an opportunity for improvement because it had to deal with the variance of the quality of timber and thus had to make constant adjustments according to the raw material. Moreover, since the department had the middle spot between the wood preparation and the paper mill with buffer storage of the raw materials and their end products, a temporary change in productivity would not immediately affect the rest of the company. In contrast, an increase in productivity could very well provide effects if production in the other departments also increased.

The chemical preparation of paper consists of five steps: boiling, screening, bleaching, boiling acid preparation, and bleaching liquid preparation. First, spruce pieces, fir, and hardwood are boiled separately with magnesium bisulfite, a process by which lignin and cellulose, the major components of the wood, form a compact structure. The lignin is slowly resolved, and the cellulose fibers are released. The lignin is a waste product, while the fibers and undissolved material undergo further screening. For that purpose, fresh magnesium bisulfite acid, created from magnesium oxide and sulfur dioxide in a separate process, is added. A system of screens increases the purity of the fibers by removing impurities. From there, the spruce pulp goes to buffer storage, while the fir and hardwood are bleached with a liquid

prepared from chlorine and sodium hydroxide. The three pulps together form the end product of the chemical process, which provides the raw material for the paper mill (Bajpai, 2010, pp. 8–44).

The department had 29 employees working in shifts, with one worker outside the shift system responsible for preparing the bleaching agents. The other four subprocesses were controlled by senior operators on each shift who were at the top of the hierarchy on the production level, with the rest of the workers being considered on the same hierarchical level. After incidences of poor-quality output, management introduced foremen to every shift. Repairs and process control, i.e., the right mixture of the chemicals – were provided from services outside the department. Above the senior operators were the production engineer, the pulp mill manager, and the company's general manager.

The wage consisted of an hourly rate, shift bonuses, regular overtime payment, payment for extra hours, and a production bonus. The production bonus was paid according to the final paper output of the company, which was only indirectly in the hands of the workers in the chemical pulp department since the pulp's quality influenced the paper mill's processes to some extent. Extra hours payment was reserved for odd jobs such as cleaning or minor repairs, an attempt by management to account for the "bye-work" issue. Workers below the senior operator level were highly specialized in their tasks, which created immense problems in case of absenteeism. Moreover, this specialization produced an implicit hierarchy in the department based on the perceived skill level needed for particular jobs and the attractiveness of the tasks involved. Furthermore, the newly introduced foremen were recruited from the existing senior operators, which made it difficult for them to establish themselves at a new level in the hierarchy. Instead, they quickly took over the work previously reserved for the extra hours regulation, thus further increasing labor division in the department.

The researchers conducted a socio-technical analysis over the course of the second half of the year 1964. Because of the relatively high level of skill involved in the production processes, the main issues differed from the study at Christiana Spigerverk. Specifically, the organization of information flow proved to be a hindering factor for higher productivity. For example, the results of the laboratory tests about the quality of the different chemicals involved were sent to the foremen to be disseminated to the operators. Because of the nature of the foremen's job, they were often absent from their desks. Thus, information about necessary adjustments provided by the laboratory was significantly slowed down because it arrived at the foremen's office first. Management and the union studied the results of the researchers' analysis in detail. After joint meetings brought forward a document – the so-called Easter Document because it was released around Easter 1965 – to outline possible changes for the CPD, the proposed changes and the conditions for changes can be summarized as follows:

1. The purpose of the study should be to improve conditions of personal participation in decision making.

2. Increased autonomy for extended groups, i.e., groups not only limited to people working in the same shift but people working on the same process, should be established.
3. Problems should be solved in small steps by a committee consisting of workers, supervisors, and management in consultation with the researchers. Members will be elected from each shift. Changes will only be applied in accordance with the workers concerned.
4. Boundaries for the partly autonomous groups should be the environment, i.e., the responsibilities and autonomy of other groups, definitions of the quality measures, and group bonuses based on measurable or observable improvement.
5. Groups will be constituted by meetings of workers and supervisors whose tasks are interdependent. Researchers will take part in those meetings.
6. To facilitate group work, the management will provide a dedicated repair worker for the department who cooperates directly with the supervisors and operators, operators will be offered training for all tasks involved in the production process if they so wish, and an information center will be established to make all information including output and quality measures immediately available to everyone, regular working hours will include time for meetings, telephones will be installed at every operator work station.
7. The Union will be given full transparency of all the actions.

The Easter Document was accepted by vote of all status groups involved, though the decision was carried only by one vote in the workers' group. To address this, a meeting with researchers, workers, the union, and management was organized to assure the workers that this was not a measure to reduce the working force. The opposition subsequently decreased to a small but existing minority.

From the Easter Document, the practical suggestions were established quickly. Telephones were installed, and an information center was created. However, at the evaluation meeting in September, all parties agreed that progress was little. Consequently, an Action Committee consisting of the training officer, the assistant foreman, and one operator was established, and the research team was asked to withdraw from the forefront. The Action Committee was not in any way thought of as a representative body; instead – and that was reflected in its composition – it was set up to provide a resource of knowledge. A continuous problem-solving process was followed between the workers and the action committee. For example, operators readily accepted training for different tasks, but when management suggested a rotation procedure for the various jobs, the workers declined. Operators were not ready to give up their primary task they identified with strongly – an attachment similar to what was witnessed with the personal benches in the first study. With the Action Committee, a rotation plan within one shift according to needs stemming from absenteeism because of vacation times or illnesses was developed, with the operators keeping their primary tasks. The information center was improved after several suggestions were made and several parts of the technical process, including communication between the production units and machine improvements, were achieved. All these steps led to increased production and quality, and the foreman

role was abandoned, giving the individual worker more responsibility according to their skills. By 1968, the ideas first initiated in the chemical department had spread to the other departments of the company, further dissolving various levels of the company hierarchy. However, the company went through a major economic crisis during the early 1970s, and management reverted most of the changes to the old system.

In comparing the two projects, it is notable that the experiments at the wire drawing department at Christiana Spigerverk failed. In contrast, the Hunsfos Pulp and Paper Mill project was successful on all levels, at least for a limited time. Where does this striking difference come from? Both projects were approached with the same theoretical framework; research followed the same steps of interviewing, socio-technical analysis, change recommendation, and change implementation. At the same time, some fundamental differences apply. At Christiana Spigerverk, the changes were implemented in a classical quasi-experimental fashion. Instead of changing the organizational structure, a small portion of the production was cut off as an "experimental site." In contrast, for the rest of the workers, the regular production process continued. Their function resembled that of a control group. Even if this was not necessarily the researcher's intention, the workers constantly compared the production standards achieved in the experimental situation with the regular production process. At Hunsfos Pulp and Paper Mill, by comparison, a whole unit was subject to changes. The workers at the CPD had an Action Committee of workers of the department to be on their side, which served as a council for the implementation of changes and replaced the researchers who, in the wire drawing case, stayed "in command" of the entire project. The production tasks at CPD were more complex, allowing for more variations simply because the number of tasks to be accomplished was significantly higher. The CPD required a greater extent of external communication, for example, with the lab that attested to the right balance of the chemicals, or in case of the wood preparation department which assigned the quality of wood, and in case of the paper mill which responded to the quality of the CPD's end products. In the wire drawing case, external communication and coordination were nonexistent.

However, for a short period in the Christiana Spigerverk case, workers switched to group work, and this drastically increased productivity per worker. However, internal pressures forced a change back to the old system. The same effects count for Hunsfos Pulp and Paper Mill. This observation seems to suggest that both projects failed and that this failure is better explained by the similarities of the two cases instead of their differences, speaking to a general failure of the ID approach. To substantiate this claim, I will briefly introduce the remaining research sites of the ID Project.

The third study started in 1965 at a newly opened production side of the NOBØ company in Hommelvik, outside Trondheim. At the time, the company produced metal sheets, office furniture, electric panel heaters, and traditional radiators. In Hommelvik, approximately 75 workers were employed, and 30 were part of the experiment. Instead of foremen, the company had appointed "contact men" for every major production area. The new site was headed by a production engineer

who made his decisions in close contact with the company headquarters, which also handled sales and acquisition of raw materials. The project began with a socio-technical analysis of the production process. The production line adhered to the traditional way of increased specialization in conjunction with piece-rate payments. The panel heaters went through three production stages: (1) pressing, welding, and grinding, (2) Surface treatment, cleaning, painting, and drying; and (3) assembly work and packaging. Production was organized so that the semi-finished product was pushed to the next station whenever the individual work step was completed, forming a simple, specialized production line of observable units. There was little quality dependence between the individual steps; the quality of the products was assessed at the end of the production line. The only room for improvement seemed to be the speed at which the parts were assembled. The general attitude of the workers toward the company was positive; they found their jobs easy but boring. In many ways, the production process was comparable to that at Christiana Spigerverk in that the tasks were simple, quality variance due to "bad" or "good" work by the workers practically did not exist, the wiggle room for changes was rather limited, and change was first implemented within an experimental group, leaving some of the workers on the old system. After finishing the socio-technical analysis, the researchers suggested changes in accordance with the "application of the general psychological criteria of job design" (Emery & Thorsrud, 1976, p. 90). Three autonomous groups were set up, each responsible for one of the three production tasks. The wage system was changed from piece rate to hourly rate with bonuses if the production output would increase under the new conditions. The so-called "contact men" [sic] were now used as boundary control of the autonomous groups (thus organizing the functionality of the machines, the availability of raw material, and replacement workers in case of absenteeism instead of a mere control function under the old system). The experiment ran until 1967 and proved to be successful. Production went up, absenteeism decreased, and the workers preferred the new system over the old. The group system successfully spread throughout the plant, but management blocked it from being applied in the main plant. Production based on semi-autonomous groups continued in Hommelvik until the early 1970s when the company closed the plant and moved the production to a newly opened, larger plant (Emery & Thorsrud, 1976, p. 97).

The fourth study of the ID Project, which started in early 1967, was unique in that it took place in a newly opened fertilizer plant belonging to the Norsk Hydro, one of the major employers in Norway, until today. The new plant was located in Herøya to produce artificial fertilizers. Before worker recruitment for the new plant had begun, an Action Committee was established. It consisted of the head of the fertilizer department, a union representative, a foreman from another fertilizer factory of the same company, a representative from central management, a representative from the local personnel department, and a representative from the researcher group.

Few limitations existed concerning the job design to be proposed. However, the factory was built in a way that would suggest the following setting: Shift work would be in place, and every shift would be headed by a foreman [sic] who would

be responsible for the whole factory. The factory itself could be divided into three parts, and so would the shifts, with each part reporting to a charge hand. The shift workers would again be subdivided according to different skill levels. Two highly skilled operators would be in charge of the central control room. A special group working only day shifts would be responsible for cleaning and transport activities. A special maintenance force would handle maintenance and repairs.

Based on interviews and analyses in an existing factory, a different organization was proposed: the charge hands were considered redundant. Since maintenance and repair work made up a considerable part of the daily work, every unit should have at least one highly skilled maintenance worker to promote autonomy. The shifts would also handle cleaning and transport. The new proposal saw a requirement of 57 workers to run the plant, while the old one calculated a total of 94. The exclusive crew of central control room operators should be replaced by a rotational system of all the operators in the plant with the required skills. The new wage system based payment on proven skills and experience on the job. Every worker was given the right to take training to learn any other position in the company up to control room operator. Learning programs were set up where individual workers functioned as teachers and students according to their skills. The plant was commissioned in the summer of 1967 and operated successfully based on the semi-autonomous group design. However, diffusion to other plants did not take place.

The NOBØ company project shows remarkable similarities to the Christiana Spigerverk experiments. They differ in their outcome, though. While success cannot be attested to the NOBØ plant at Hommelvik due to its shutdown by the general management, it operated successfully under the semi-autonomous group paradigm for several years. To be sure, there are many speculative reasons for the difference in success. For example, the researchers managed to get the workers' trust, while at Christiana Spigerverk, they failed to do so. Also, the fluctuation of workers at Hommelvik was higher, and a higher percentage of the workers had not been employed in other companies before. Therefore they lacked expertise and experience. Hommelvik had a higher diversity among the workers (30% women, for example) than Spigerverk (0%). Many such variables could be listed. Interestingly, however, the researchers followed the same basic procedure in both cases.

The project at Norsk Hydro was successful on all ends – besides the fact that diffusion to other plants failed. Again, the procedure applied by the researchers followed the same principles. The notable difference here, however, was that no classical change process had to be initiated since the plant was newly opened and started to operate based on semi-autonomous groups from the beginning.

Speculations about the influence of various measurable variables in the concrete cases put aside, it appears that the STSD approach suffers from two significant theoretical shortcomings. First, it exhibits a rigid model of the change process. As a result, STSD uses the same template for every project: singularized work is replaced by group work, workers are given more responsibility and incentives to learn more tasks, foremen-like positions are removed, and the wage systems are changed to a time-based model. This rigidity is a problem "by design": The goal of the approach is to establish a general scheme which can be, in principle, transferred to every

Norwegian company – and eventually the rest of the world. Second, STSD lacks a conceptual framework to explain the failure or success of its projects. Situational dynamics are not accounted for in the STSD approach.

This lack seems to result from a twofold problem: First, the approach carries a strong democratic ideal. Participation is the ultimate solution enforced on various ends. This is justified by a general agreement of the Norwegian society to foster democratic change (as assessed in phase 1 of the ID-Project). This agreement is taken as "proof" that the workers "want" democracy. However, the democratic ideal is not transferred to an empirical level of participatory co-construction. Instead, semi-autonomous work groups are established as a pre-given ideal form of democratic and participatory development. The democratic implementation of democracy, or the participatory implementation of participation, is not addressed. Second, the concrete needs, goals, and wishes of the subjects in the field are not assessed. Or, if they are assessed, they only enter the research project as "real" needs, goals, and wishes if they are in accordance with the six psychological principles upon which STSD rests. Whenever there is a lack of concurrence, this is attributed to some kind of false beliefs among the workers, which will be "cured" once they experience the power of the new system. In other words, the theoretical, idealized subject is superimposed on the subjects in the field. It thus eliminates the possibility of an empirical subject that could unfold its potentialities in a collaborative action process based on their concrete, empirical needs, goals, wishes, etc. The change framework is thus introduced from outside.

From this perspective, the only truly successful project, one may argue, is the NOBØ company project. From an Action Research perspective, such an argument would be flawed. NOBØ did, in fact, not produce substantial change. Instead, it was a new plant set up and organized according to STSD principles. This is not to say that the conditions for the workers at NOBØ were inferior to other companies; quite the contrary: all the available data indicate that the workers were better off than their colleagues in other companies. They were happier with their jobs, had a higher salary, were able to conduct more tasks on average, and so forth. However, they did not participate in establishing new structures at the plant; they "were lucky" to be employed in a place with better working conditions built by socio-technical design principles.

Local Constructivism: The Search Conference

By the mid-1970s, the limitations of the STSD approach became increasingly apparent as more STSD research projects failed to carry momentum and were adopted by other companies. A period followed when less money was available for Action Research projects, and theoretical development stagnated. However, this situation changed when, toward the end of the 1970s and then prominently during the 1980s, a new generation of researchers got involved with Action Research in industry. Confronted with the failure of STSD, they forged their approach, which

was based on dialogue and supposed to tackle one of the significant shortcomings of STSD, namely, the inability to democratize the democratization process.

One of the initial drives came from politics, similarly as had been the case for the ID Project. During the late 1960s, a general debate about health and safety at the workplace emerged all over Europe and led to new protection laws in most countries in Western Europe (Ashford, 1976). This debate was most prominent in Scandinavia during the strike in the Luossavaara Kiirunavaara AB mining company in Sweden in 1971, where workers demanded better working conditions. Their strike was motivated by a large number of accidents and the high degree of illnesses related to long-term exposure to toxic substances in the workers' environment. In most cases, the answer to these concerns was implementing standards to prevent accidents or negative long-term health implications. This was the case, for example, in Sweden.

In Norway, however, a different approach was chosen: Instead of spelling out specific standards, the legislation demanded the installation of specific rights and duties and called for a reasonable degree of worker participation in developing these local standards and duties (1977). This new law provided the grounds for a shift from the creation of standards for democracy to the democratic creation of standards. The Work Environment Act granted democratic participation for workers in all companies by law – without defining the mode of participation. As such, the question of how to implement such democratic participation was left for researchers to answer. STSD, of course, had an answer to this: semi-autonomous groups. But the new generation of Action Researchers was looking for approaches that would democratize those processes that can produce (democratic) structures and not impose democratic structures through researcher intervention.

The emergence of the Search approach – also dubbed "democratic dialogue" (Gustavsen, 1992, p. 3) or "dialogue conferences" (Pålshaugen et al., 1998, p. 25) – marked the beginning of a methodological shift in Action Research, which would take ten years to complete. The new generation of Action Researchers in Scandinavia was led by Bjørn Gustavsen, who was also responsible for the theoretical underpinnings of the Search approach. In this new theoretical framework, STSD was not dropped completely. The goals to introduce democracy, flexibility, expansion of the number of tasks the individual worker is capable of, etc., remained at the forefront of research activities. But the general explanatory framework of STSD and, more importantly, the impetus it carried for specific actions were dropped. In realizing that the STSD approach worked in some instances – at least for a period – while failing in others, researchers concluded that such a general framework could not explain the concrete processes at work in specific projects. As a result, and influenced by the broader developments within qualitative research field Field (Glaser & Strauss, 1967/2008), "local theory" emerged to account for the concrete circumstances within specific projects that led to success or failure. STSD as an overarching framework represented too strict of a model, its presuppositions, steps, and explanations confined data collection, modes of action as well as the presentation of the results in a specific but not necessarily all-encompassing mode of thought – namely that of open systems thinking – began to reveal its shortcomings and limitations. Moreover, the theoretical part of Action Research remained exclusively in the

hands of the researchers. There was hope that an effective development by the participants to arrive at an "indigenous evolution of 'local theory'" (Elden, 1983, p. 21) would improve this situation.

This theoretical shift would also strongly affect the modes of research. Throughout the Harwood studies, interventionist research took the form of "field experiments," i.e., experiments in the classic laboratory sense, which had to be carried out in the real world only because it was not feasible to bring the issues to the laboratory. The design usually included a concentrated and well-defined effort on a clearly defined workplace – which was mostly cut loose from the actual production chain to "try something out" to test and enrich a preexisting theory that initially derived from more basic laboratory experiments. Typically, these field experiments involved a control group, and the researchers were the research site's primary input and representatives of knowledge. Results were typically defused within the academic community from which they also originated.

The ID project, in principle, copied this quasi-experimental approach to research – the only difference being that there was no clear concept of "control groups." The goal was to produce significant data with a successful experiment showing the superiority of a different organizational approach, namely, in all instances, a locally adapted variation of the semi-autonomous group concept. The significance of improvement shown in the experiment would then diffuse to other workplaces and change processes in one giant leap.

In the Search approach, instead of well-defined interventions, field research was conceptualized broadly to cover all major issues, including general social and political issues. This also meant that the idea of "national exemplars," which were supposed to show the rest of the industry what they should do (as in the ID Project), was dropped. Instead, many enterprises were to take part in research projects, with the effect that collaboration between various companies was encouraged. Research became complementary instead of leading, it contributed to a different knowledge instead of the knowledge, and the dominance of a single theory disappeared in favor of a possible emergence of theory from within the research and action process itself. The hope was to create local approaches which primarily served the workers and helped them to understand their concrete working situation. Since the theory-building process was now collaborative, researchers and workers were viewed as possessing complementary bodies of knowledge. The workers emerged as the expert of their working place – in stark contrast to STSD, where the workplace was analyzed and described by the researcher using socio-technical analysis – and the researchers as possessing some form of general knowledge (theory) as well as knowledge about organizing change (process knowledge). The form of communication that emerged in the new approach was dialogue, not instruction, with the goal of synthesizing both realms of knowledge instead of reformulating findings within the boundaries of the STSD discourse. This also meant that broader dissemination channels had to be utilized.

However, a general theory – or an overarching theoretical framework – was not lost but moved to the background as a form of "generative theory" (Gustavsen, 1985, p. 467), based on dialogue. It is within this dialogue that local theories emerge

that would have to live up to the expectations of the researchers and workers. Changes and modifications are discussed, evaluated, and criticized within this dialogue. In short, dialogue becomes the central empirical and theoretical cornerstone: It is the context in which the workers become "aware of how much they really know" when "they put their heads together" (Elden, 1983, p. 29). The precondition and borderline for such dialogue is that it be organized democratically:

> There are, of course, limitations to what forms can be developed and used within a framework that bears the label 'democratic.' There are certain frame conditions that cannot be transcended. For instance, patterns of concrete organization must not destroy the generative ability as such because this would imply a destruction of the chief democratic resource. (Gustavsen, 1985, p. 467)

It could be argued that the Search approach upholds the democratic ideal as it was developed by Lewin and passed on to STSD. However, as was shown before, Lewin's democracy was a tool that proved to be superior in terms of functionality. In contrast, in the STSD approach, democracy was ultimately achieved by means of restructuring: Researchers sketched out a socio-technical analysis, which would be discussed in a committee consisting of representatives of the stakeholders involved to recommend changes. Change plans were voted upon and, depending on the vote, inaugurated. Change recommendations always included the facilitation of small groups as the basis for democratic processes. An Action Committee helped during the restructuring process and was usually disbanded once the change was achieved. In other words, with STSD, an existing structure is substituted with a new one, while in the Search approach, democracy "is no longer defined in terms of specific patterns of the organization but in terms of how the patterns are created" (Gustavsen & Engelstad, 1986, p. 101).

In the STSD approach, an understanding of the subject informed how democratic structures were institutionalized. As elaborated above, this understanding derives from a theoretical idealization rather than empirical work. In reformulating this concept of democracy, the Search approach also reconstituted the concept of subjectivity. According to Gustavsen, the striving force behind the democratization efforts of Action Research is the "generative capacity" of human beings "to develop solutions," which are "one's own solutions" (Gustavsen & Engelstad, 1986, p. 104). A model based on this framework would no longer strive for specific organizational patterns, such as autonomous groups or a representational system, and would not use the installation or diffusion of particular patterns. Instead, the central element would become democratic structures that help unleash every subject's generative capacity.

The evolution of the concept of subjectivity in relation to democracy from Lewin to the Search approach could be described in the following terms: Lewin defines the subject in relation to surrounding forces. Democracy was understood as a historically developed concept that seemed to work best within the social force field to guarantee the proper functioning of social bodies. In STSD, subjectivity expressed an ideal of democracy, i.e., democratic needs were inherent to every person. In the Search approach, the subject remained undefined. It has no further attributes other

than a generative capacity that can unfold under the conditions of democracy. Democracy becomes dialogue with others.

A central reference for the dialogue concept as the starting point of democratic change is Jürgen Habermas's Habilitationsschrift "Strukturwandel der Öffentlichkeit" (Habermas, 1962/1990) published in English as The Structural Transformation of the Public Sphere (Habermas, 1989). In this book, Habermas examines the emergence of a public sphere that developed a mode specific to modern society. For him, the public sphere is a mediator between private and state authority, making a critique of both possible. A precondition of this public sphere is its openness for all. It symbolizes an open discussion in which everyone carries the same rights. For Habermas, because it developed within the realm of ownership rights, work belongs to the private sphere and is thus excluded from critical reflection. It is the effort of the dialogical approach in Action Research to include the work sector in the public sphere and make it available for critique in open discourse. To this end, the greater, overarching theory that takes the place of STSD in the Northern Tradition is the aim to contribute to what Habermas would call the unfinished project of modernity (Bernstein, 1997), i.e., "to introduce work experience as a main resource for participation in [public] dialogue" (Gustavsen & Engelstad, 1986, p. 105).

Dialogue as the prerequisite of democratic co-determination rests on 13 normative core assumptions that derive from the debate on deliberative democracy and are spelled out by Gustavsen as follows:

1. Dialogue is a process of exchange: ideas and arguments move to and from between the participants.
2. It must be possible for all concerned to participate.
3. […] Each participant has an obligation not only to put forth his or her own ideas but also to help others to contribute their ideas.
4. All participants are equal.
5. Work experience is the basis for participation. This is the only type of experience which, by definition, all participants have.
6. At least some of the experience which each participant has when entering the dialogue must be considered legitimate.
7. It must be possible for everybody to develop an understanding of the issues at stake.
8. All arguments which pertain to the issues under discussion are legitimate. No argument should be rejected on the ground that it emerges from an illegitimate source.
9. The points, arguments, etc. which are to enter the dialogue must be made by a participating actor. Nobody can participate "on paper" only.
10. Each participant must accept that other participants can have better arguments.
11. The work role, authority, etc. of all the participants can be made subject to discussion – no participant is exempt in this respect.
12. The participants should be able to tolerate an increasing degree of difference of opinion.
13. The dialogue must continuously produce agreements which can provide platforms for practical action. Note that there is no contradiction between this criterion and the previous one. The major strength of a democratic system compared to all other ones is that it has the benefit of drawing upon a broad range of opinions and ideas which inform practice, while at the same time being able to make decisions which can gain the support of all participants. (Gustavsen, 1992, pp. 3–4)

The Search Conferences represented an effort to create a place outside the company structures where members of all hierarchical and functional levels could meet – the so-called vertical slice principle – to have a discussion where everyone would be principally equal.

A conference typically centered on three major topics:

1. Evaluation of environmental trends
2. Scanning of current problems
3. Discussion of participants' personal experience

To ensure the participation of all with equal rights, Gustavsen and Engelstad carved out five major rules for such conference proceedings (Gustavsen & Engelstad, 1986, p. 109):

1. The arena(s) of discussion must be well-defined, i.e., it must be clear where the discussion takes place. Off-the-record agreements and so forth are counter to the idea of a Search Conference.
2. The legitimate issue is public, i.e., private interests are not, in any way, served.
3. Resource persons, i.e., researchers, advisors, etc., act on the public scene.
4. Decisions and conclusions must be based on the relevant facts of the discussions. The discussions define what is appropriate.
5. Personal grievances and problems are to be left aside or, if possible, investigated as to the causes stemming from a possibly problematic underlying structure.

Subgroups are used for various purposes: groups of persons from various hierarchy levels meet, persons from the same level meet, diagonal slices if more than one company or several departments of one company are involved (workers of one company meet managers of the other), free choice based on the personal interest for a topic, and groups based on an action-criterion.

One might argue that such a relatively rigid structure is opposed to the idea of democratically creating structures for democracy. However, Search conferences, in principle, take place outside traditional company hierarchies. Their goal is to unleash the general human capacity to create something new. As such, the first step into a change process does not occur in the sphere in which change is supposed to happen, as was the case for both Lewin and STSD. Rather, it occurs in an artificial setting where the various stakeholders can meet "on new grounds." Within these discussions, the results and ideas of STSD are often used as a toolbox, for example, the set of core human needs – the need for demand and learning, variation, decision-making, social contact, understanding the work process, and a desirable future (Emery et al., 1969, p. 105) – might be used to facilitate discussion about their applicability in the concrete case.

The turn to dialogue also entails a general turn to language as the primary tool for initiating change processes. The distinction between what happens in reality and how this reality is constructed discursively by actors is increasingly highlighted:

> To be able to create a common reality we have to operate through a common language. We are all familiar with those conflicts, both in family life and at work, in which people who share the same practical reality still do not share the same perception of reality, and who

> thus find themselves at loggerheads in conflicts which give the impression of being unresolvable. In such cases we often say that the two sides 'do not speak the same language'. Being able to speak the same language is an important precondition for the creation or re-creation of a community. (Pålshaugen et al., 1998, p. 15)

Influenced by a Foucauldian perspective (Foucault, 1966/1994) – which dominated the scene in critical social sciences in the late 1980s and early 1990s – the Search approach highlights the power of discourses to shape reality, include and exclude actors as well as modes of thought:

> When we appropriate language we also appropriate, simultaneously, that (or those) perception(s) of reality which, to a large extent, are to be found ready made in the language community (communities) in which we grow up. If we term such institutionalized forms of language use discourses, we might say that our perception of reality largely depends on which discourses we are, and have been, involved in. (Pålshaugen et al., 1998, p. 16)

From this perspective, the general framework for Action Research can be described as follows: Several different discourses – often institutionalized and therefore protected from immediate change – compete for the dominant description of reality. However, these discourses also overlap, creating possible linkage points between them. In this context, the Wittgensteinian notion of "family resemblance" is often used as an illustration Field (Wittgenstein, 1953/2009). Discourses can be connected because language is unlimited, reproduces that which existed before, but includes a variation of this original – a notion prominently presented by Judith Butler at the time of the emergence of the Search approach (Butler, 1990/2002). Language and reality do not constitute two separate systems. Instead, language directly shapes our perceptions and experiences, for it is through linguistic constructions that we experience and build the world.

This turn to language resembles the influence of postmodernity on the Action Research Tradition. The researcher's and worker's knowledge are now conceptualized as two different discourses that cannot be placed in a hierarchical order. Instead, the goal would be to find the linkage points. On the level of organizational change, it is the task of Action Research to help persons identify their discourses and create new ones to re-describe their reality and thus produce change. In following Gustavsen (Gustavsen, 1998), one may be critical of the practical effects of this "new language" on how Action Research is carried out. However, the postmodern nexus with its general framework of a critique of hierarchies and specifically the critique of a hierarchy of different kinds of knowledge certainly contributed to the development of the Search Approach as a liberating factor from the bonds of "hard science."

The search approach turns away from the desire to identify specific general human needs upon which change could rest. Instead, the overarching motivating factor to conduct research is drawn from more general statements about the human capacity as the capacity to create something new (via means of language); in a way, the pendulum seemed to swing in the other direction, embracing a notion of idealism that brings out the best in humans if only the right structures are given:

> Noble visions and high ideals have spurred the birth of new organizations, educational and religious institutions, progressive social movements, flourishing communities, creative epochs, and even new civilizations. Visions and ideals bring out the best in people. People power. Collaborative social action. We can see it in the civil rights movement; we can see it when creative groups generate breakthrough ideas and turn them into winning products; we can see it in the solidarity of people united by a common cause. There is an unquestionable energy unleashed when people work collectively to bring their most desirable future into existence. This ability to seek and create a more desirable future for our social organizations is a unique human capacity. We might even say that such ideal-seeking behavior is the life-blood of healthy organizations, vital communities, and the good society. But this basic human need to create a more desirable future is often blocked, covered over, or frustrated. Unfavorable conditions can prevent groups from understanding their common ground, inhibiting their ability to work toward a shared purpose. (Emery & Purser, 1996, p. 3)

The Search Conference method aims to create action based on basic human ideals, which supposedly everyone shares. The main differences to previous approaches can be summarized as follows: Experimentally oriented Action Research followed a linear logic of the project and had as its chief theoretical source a robust theory of human attributes, i.e., either STSD or socio-psychological theories about organization. It was legitimized in its efforts by the content of the solution, embraced a few leading actors, namely, the researchers, and defined the initial conditions as the "zero-point" from which change would start. In addition, experimentally oriented action research embraced a highly structured way of proceeding by way of straightforward steps and aimed at a giant leap change toward the end of the project. In contrast, the dialogue-oriented approach embraces an interactive logic, is based on a theory of participatory democracy, and derives its legitimation from the participation of the actors in the field. As such, it has many leading actors – depending on whoever crystallizes as a leader in the democratic process. The project's starting point is already part of a more extensive process, and it is minimally structured for a creative process of change.

The subject in this approach is defined in two ways. First, it is attested to a principal capacity to generate something new using language in dialogical form with others. Secondly, however, this dialogue must be constructed in a specific way to ensure maximum participation; that is, it must happen outside the conventional pre-existing discourse formations at which the change is aimed. This suggests that, without this newly generated framework made possible by the Search conference, the subject is ultimately confined and defined by the discourses it is participating in.

The Search Conference Put into Practice

To understand the Search approach in more detail, I will briefly discuss one project. It seems interesting to examine how the human capacity to create something new is unleashed using discourse and analyze the extent to which the rigid structure applied in STSD projects is replaced by a concept of structural emergence.

Øyvind Pålshaugen conducted a research project based on the Search approach in the tobacco industry at J.L. Tidemann's Tobacco Factory in Oslo. The company employed approximately 300 workers at the time (1987–1989) (Pålshaugen et al., 1998). Pålshaugen neither relies on a set of preconditions for any change nor starts the project with a socio-technical analysis. Instead, the beginning is a dialogue. For that matter, the project is divided into three phases: (1) the foundation phase, (2) the project development phase, and (3) the institutionalization phase.

The Tobacco factory suffered from low productivity and high absenteeism. Efforts were made to improve the work environment. A job design committee was formed but operated unsuccessfully. The researchers were invited to help with the job design since current job descriptions were perceived as too static and tasks for individual workers were too narrowly defined. The primary hope of the committee was that the researchers would provide information on how to restructure the company by bringing in general theoretical knowledge and expertise they had gained from dealing with other companies. The company's representatives thus reflected a specific understanding of the role of the researchers, namely that of experts who could bring in their knowledge in the form of a "product" to be applied to the company structures. The researchers related to this demand by suggesting that the problems in job design were mere expressions of general organizational issues within the company, which should be tackled by looking at the factory and its functioning as a whole. This way, the researchers circumvented the idea of delivering a fixed product as suggested by the management while at the same time opening the discussion for possible changes. In other words: Instead of confronting two discourses, a linkage was created (Pålshaugen et al., 1998, p. 27).

The company management accepted the perspective offered by the researchers, and the decision was made to initiate two Search Conferences organized; one for management only – this was seen as a tryout of the method – and one that would include representatives from all departments and hierarchy levels of the factory and representatives from the general management. The second Search Conference was given the theme "What are the most significant problems in the company today?" (Pålshaugen et al., 1998, p. 38). This approach was chosen to give the workers a chance to express their unhappiness about concrete company structures. The groups were composed by the researchers, with persons in the same group who came from the same hierarchical level and a similar working context but differed in attributes such as age, sex, number of years working for the company, etc. The reason for this procedure was that people in different workplaces and with different tasks were considered to have a tendency to be faced with different problems stemming from the overall company organization. To get at the issues associated with specific workplaces, the chosen approach seemed promising because "[t]he company's local discourse is built up of several smaller, distinct discourses manifest within different arenas. … [T]his type of configuration of discourses [is called] the company's local discourse formation" (Pålshaugen et al., 1998, p. 39). Moreover, this procedure was expected to relativize specific subjectivities, i.e., "personal problems," to get at the problems the entire group shared. These are the problems that the researchers valued as most significant for the company's discourse formation.

This formation was subdivided into four general areas: The arena of the management, the cooperative bodies, the union, and the company's sub-public arena, which consisted of several sub-arenas, such as daily conversations between the workers in different settings. These sub-arenas are usually invested with a particular private character because they do not necessarily interact with the first three arenas. Within the Search approach, this sub-public arena poses the real problem since it communicates the company's daily business without being connected to the other three. Consequently, complaints, suggestions for improvements, etc., remain unnoticed within the company's discourse formation. The Search Conference is supposed to provide an internal public sphere (Habermas, 1962/1990, p. 357f), which allows protagonists to interlink with potentially all the discourse arenas in the company. In that respect, the second search conference seemed to be successful. Specifically, the following problems were raised by the workers on the shop floor:

> [T]he supervisors had too little contact with the people they were supposed to supervise; the information for, and the training of new employees was not good enough; people became involved in new projects at too late a stage; some felt they were under surveillance by supervisors and department heads; the organization and allotment of tasks was not as it should be (among other things, in the form of too much borrowing by people from other departments); and, in connection with absenteeism, there was an uneven distribution of the workload between those at work. It was also emphasized that communication between the workforce and the leaders was absent – in both directions. In addition, certain physical working environment problems were mentioned, such as dust and the need for ventilation. (Pålshaugen et al., 1998, p. 41)

It is specific to the Search approach that such a list produced at the company-wide Search Conference is not used to solve these problems. Instead, the goal is "to clear the ground so that the dialogue conference can facilitate a certain redistribution of the problems, in terms of their place and significance in the company's discourses" (Pålshaugen et al., 1998, p. 42). Consequently, the researchers started group discussions again, but this time across the hierarchy levels. The general outcome was a tendency to prefer working in groups over working alone, that authority was seen as too concentrated within the company, and that a more robust delegation of authority would be preferable. The third round of group discussions involved workers from the same departments and managers from different areas of the company (the so-called "diagonal groups"). The general task was to discuss short-term and long-term measures for the general development of the company.

These three rounds of group discussions created a clear picture of the company's problems and a number of solutions that could be aimed at. However,

> [a]t this stage of the company conference … it was vital to avoid the temptation to "give the participants what they want." … This transition from describing problems to describing solutions, which might appear as a 'natural development' of the conference, would represent precisely that step from using language in common to construct models of reality to using language to simulate actions and practical solutions without a sufficiently clear demarcation of the difference between the context of discussion and the context of practical action. The illusion that a linguistic act is identical to a practical action would therefore … threaten to cloud the discussion with an illusory content. (Pålshaugen et al., 1998, p. 56)

In other words, implementing the solutions discussed would ultimately fail because they were produced in a different discursive realm, namely, the Search Conference, and not the day-to-day company operations. Consequently, the next set of discussions focused on ways in which democratic and participatory dialogue could be implemented within the general structure of the company to initiate dialogue-based development work in which everyone participates. One suggestion by the researchers was the formation of production groups in which operators could come together for discussions about work-related issues. The groups' task was to envision how these production groups could be organized. The groups themselves were again organized by the researchers, this time according to the homogeneous group principle, i.e., workers with workers, managers with managers, etc. The idea was to bring out the contrast between the hierarchical levels by using the final group discussion section of the two-day Search Conference to have the lower hierarchy levels discuss the proposals of management from the previous group session and vice versa.

The results of the Search Conference were as follows: Production groups were initiated with a company-wide development committee, plenary meetings on the departmental level, and department committees were held. Two researchers were hired to help coordinate the process. This marked the beginning of phase two of the project. Over the next three months, 176 recommendations for change were made using the newly installed bodies, and 101 of these could be tackled directly, while the rest needed new acquisitions and budget adjustments. These numbers are particularly significant because they show that more than half of the existing issues – and many of these were raised before the change in organization – could have been tackled without additional resources. The fact that this did not happen before the Search Conference intervention was attributed to the existence of a sub-public arena in which the recommendations were "lost." After the structural changes, the sub-public arena seemed to be successfully integrated into the company's general discourse formation.

While at the beginning of stage two, the discussions in the production groups were led by the researchers, they slowly became independent, and some departments also reorganized themselves. This new structure eventually allowed for a central issue to be tackled: Leadership. Many workers were unhappy with their supervisors partly on personal grounds, partly stemming from the structural relationship of the supervisor as a control authority of individual work. Starting from the workers' initiative, another Search Conference was set up to discuss the role of supervisors in the company. Following that second conference, a department representative was established who now functioned as an intermediate between the supervisor and the workers. Later, the company organized another Search Conference – this time, the researchers only assisted in the preparation – to redefine the supervisor role as a whole. This marked the beginning of the final phase in which the new modes of organization in the form of dialogue became institutionalized, and the researchers were no longer necessary to keep the dialogue going.

Despite the strong theoretical claims associated with the Search approach, specifically the claim to help create structures that lead to the implementation of

democratic procedures, the project at J.L. Tidemann's Tobacco Factory shows different results. The Search Conference, as the researchers set it up, followed a clear-cut procedure that existed in advance. It could be argued that this is in line with the general theoretical claim since the Search Conference took place outside the company hierarchies and was, as such, not part of the field the democratization process aimed at. And indeed, the researchers intentionally steered this conference away from discussing practical solutions to company problems. The Search Conference was a "training ground" in democratic dialogue. It is, however, striking that the methods eventually applied in the company in phases 2 and 3 were very similar to the Search conference structures. The researchers interpret this as a sign of the success of the original Search conference (Pålshaugen et al., 1998, p. 110). But is this plausible? The Search conference that the workers experienced was their first contact with a procedure through which they could sit down and talk to various company members about the problems they encounter on a day-to-day basis. During the conference, they were instructed by a group of researchers about the concrete proceedings for the two days (i.e., who is going to discuss what with whom). At the end of the conference, they were asked to think of models in which democratic dialogue could be implemented in the company. Given the lack of elaborated alternatives to the Search conference format, it seems natural to transfer at least the gist of the discourse they were part of during the conference to the company structures. From this perspective, it is not just the structure to create a structure for democracy, which the researchers provide. Instead, the Search conference seems to serve as a training ground for structures to be institutionalized in the company. The researchers' idea of "minimal intervention" is at least questionable.

With respect to democracy, the Search approach nonetheless offers entirely new grounds. Democracy is no longer defined as a matter of vote. In fact, the vote does not play a role at all. Instead, democracy is dialogical with the consensus of the workers involved or affected by a particular change. The application of dialogue to produce consensus means that, at least in principle, everyone involved or affected can contribute to the decision process from their subjective standpoint. This is, to some extent, compromised by the explicit exclusion of "personal accounts" and individual troubles if there is no relation to the general company structure.

Enterprise Development 2000: Major Methodological Shifts

The ED 2000 Project brought with it several important conceptual and structural changes.

On the conceptual side, *learning* took center stage. The goal of STSD had been to create national examples, which other companies would copy. The Search Approach tried to establish spheres of communication in which principal consensus could be achieved. The ED 2000 focus was now set on difference: "There is actually little that can occur in terms of mutual learning between actors who are identical: among identical actors each is a replica of the others and meeting other actors is the

same as meeting oneself" (2001, p. 6). In other words: While the idea of national exemplars was already dropped when the ID Project entered the Search Conference phase, there was now a conviction that transferring knowledge gained in one project directly to another was not desirable. Instead, it seemed that the local conditions, even in highly similar industries in similar regions, would always be so drastically different that a superimposing of successful structures in one project would most likely fail in any other. Learning was redefined as a process of understanding differences. To realize a difference would also mean understanding one's organizational structures, often implicit and taken as a given. It is the verbalization of administrative procedures in the confrontation with other modes of operation that produces an identifiable difference from which new possibilities of action arise. To elicit these differences, it was now necessary to have forums for those different actors to meet. Hence the focus on networking emerged.

In addition, another significant change directly related to the concept of Action Research emerged. While congruent with past experiences, it seemed clear that the research involved could not be grounded in preexisting facts – but in contrast, these facts had to emerge from the research process, and the idea of a general project goal was dropped entirely. The classical cycle of fact-finding, action planning, and action, followed by evaluation of the new facts, was redefined to focus on the act of changing, a focus "on what is done at 'the moment of creation'" (2001, p. 8). Consequently, the evaluation of success or failure of actions did not consist of a comparison of achievements and goals. Instead, the aim was to facilitate a self-sustaining process: "'A good program' is a program that phases itself fruitfully into ongoing processes, helps improve on them for some time, and then waves farewell to processes that continue to gain in momentum, speed and quality. Any effort on any level must, ultimately, be seen within the context of this larger whole" (2001, p. 9).

As such, the ED 2000 project represents the final breakaway from an overarching general theory – or body of knowledge – to which Action Research in the Northern Tradition has contributed. This breakaway tackles one of the major pitfalls of past projects. It is intertwined with the new idea of learning from differences introduced in ED 2000: If the relationship of action and theory in Action Research is seen as a continuous collection of knowledge through Action Research, which gradually enriches the underlying theory, then Action Research is slowly abolishing itself, a movement visible in the research projects of the ID Project in the 1960s: Phase one of the project provided the necessary knowledge about the culture, society, and the general wishes and aims that should be followed. Over the course of four projects, phase two gradually enriched the researchers' knowledge about what needs to be done to establish democracy up to the point where, in the final project, a new factory was designed according to the standards of democratic participation, which derived from the general theory. Action Research was hardly necessary because the facts that would follow every action were already "known." In the ED 2000 project, learning from differences meant that the actual trigger for change was the realization of different forms of organization. The elimination of these differences by following the prescriptions of a general theory would produce a state at which no new

change could emerge since "meeting the other actors [would be] the same as meeting oneself" (2001, p. 6). This does, however, not mean that Action Research is – if it wants to be successful – reduced to a particular method without theory, a procedure used for random purposes whenever the conditions seem to call for it. The burden of generalization is not taken away. Instead, it is moved to the practical realm. It is no longer the researcher who is responsible for the dissemination of knowledge gained in Action Research projects. Instead, connections must be made in the practical world in networks where experiences flow. The relationship between theory and praxis is collapsed because both continuously enrich each other and spread their insights and achievements.

These methodological changes, specifically the focus on "processes where something new is emerging" (2001, p. 4), brought a change in vocabulary. Instead of "change" or "improvement" of existing conditions, the new umbrella term used was "innovation." On a practical side, this new term was adapted because it had crystalized as a significant expression within the everyday language of companies. Innovation suddenly seemed to be everyone's goal (1997). Innovation as a term appeared to grasp the kind of change companies aimed at. Due to the interconnectedness of supply chains, financing obligations, general business strategy, and so forth, few companies can break these ties easily and implement radical change. Moving forward in slow steps was the goal entitled "innovation."

This modification, however, reflected more than a mere introduction of a new, possibly more appropriate term. It reflected the new methodological approach: With Lewin and his close followers, change indicates a process that follows the pattern of unfreezing, implementing modifications, and freezing (Lewin, 1947, p. 34), i.e., a pattern of analyzing the existing state, which ought to be modified to implement a new state. In the STSD tradition, "improvement" resembles the kind of theory-driven change that predefined the desired state according to theoretical insights (Emery & Thorsrud, 1976, pp. 23–29, 47f). "Innovation" now was supposed to move the focus to a different level: In focusing on innovation rather than change or improvements, the project underlined its aim to capture change at the moment it is taking place. This is driven by the concept of turbulent environments as developed by Emery (Emery & Trist, 1965, p. 25) and subsequently applied in the Search Conferences (Emery & Purser, 1996, pp. 47–73). However, in the ED 2000 project, this notion is pushed one step further: If the environment has changed to an extent to which changes are continuously affected by their effect on the environment, then it follows that the phases of planning and implementation increasingly collapse into one another – or rather, the implementation phase is gradually absorbing the planning phase since a constant feedback loop from the environment to the implementation makes necessary a constant readjustment of the action plans. Instead of different phases of planning, acting, and evaluation, an overarching moment of creation that includes all the steps at once – and even beyond that: the regular tasks of everyday life come into focus as the prerequisites of innovation – is enacted (Totterdill & Hague, 2004).

On the structural level, it was decided that local projects had to be related to local research, i.e., the necessity for a strong research center, namely the Work Research

Institute (WRI) in Oslo, which had dominated all previous attempts at industrial democracy in Norway, was dropped. WRI became one of seven modules that would undertake Action Research projects in industry.

Enterprise Development 2000: The Projects

The program proposal for ED 2000 identified five challenges in the face of globalization the program would meet:

1. Improving performance in quality, logistics, customer orientation, productivity, and flexibility
2. Increasing product and process innovation rate
3. Increasing regional cooperation
4. Extending international connections
5. Improve intra- and inter-organizational solutions (Research Council of Norway, 1996).

These goals are reformulations of the goals of the 1960s projects. The innovation consists of the lack of a prescribed mode of achieving these goals. It was understood that the determination of how these goals could be mastered should emerge locally in the research process and through the connections between enterprises to be built. As such, "organization" was not understood as a technology to be implemented as in the case of STSD and, to a lesser extent, the Search approach. The focus lay in the development of new work relationships. Consequently, the project aimed at:

1. Bringing more research groups directly into contact with enterprises
2. Increasing the commitment of research institutions to the development of enterprises
3. Increasing recruitment of doctoral students in the field
4. Infusing educational institutions with the output of the projects (Research Council of Norway, 1996).

To facilitate this process, researchers should organize themselves into modules. A module was defined as a basic organizational unit consisting of researchers with a common research agenda and a set of enterprises they work with. A module could span several institutions, had to include doctoral students, and, once accepted, received a stable amount of funding for the project. Each module was required to work with at least five enterprises and held five full-time researcher positions. Furthermore, researchers on the project were required to stay on the project for five years. The Networks to be established were called "development coalitions" and should consist of companies committed to joint development and shared activities. At the same time, the modules had to be integrated partners of these development coalitions – instead of service institutions as in the previous projects. In the long run, networks of enterprises, research centers, and learning environments were hoped to be established. The research centers were free to set up their projects with the enterprises they partnered with. No restrictions were made as to the approach applied. However, the research teams were asked to answer six themes that reflected the past developments concerning the democratization efforts in Norway in general:

1. How can a viable development processes be created?

2. How can organizations be linked to each other?
3. How can improved integration between Norwegian enterprises and world market standards in quality, logistics, product development, etc. be achieved?
4. How does the Norwegian tradition of labor participation help to improve enterprises in an international context?
5. How can business strategies be developed that integrate the interests of the workers?
6. How do state programs support the work of enterprises (Research Council of Norway, 1996)?

The seven modules were as follows (Levin, 2002, p. 5): The Rogaland Research Foundation (RF) in the Norwegian oil capital Stavanger in the south-west constituted a module, which aimed at establishing business networks (Hansen & Claussen, 2001). The Work Research Institute (WRI) in Oslo established a module that focused on the interplay of research and work-life reforms (Pålshaugen et al., 2001). The Tromsø module in the very north of Norway, based at the local university in cooperation with Nordland Research (NF), a private nonprofit research organization in Bodø and the Institute of Marine Research and the Northern Research Institute (NORUT), both located in Tromsø, focused on cooperative research styles between different research institutes and the industry (Bye et al., 2001). The Agder Research (AF) in Kristiansand constituted the southernmost module. It focused on the constitutional and innovative aspects of enterprise development, combining structural, processual, and learning elements of development (Knudsen & Garmann Johnsen, 2001). The Trondheim module was a cooperation between the Norwegian University of Science and Technology (NTNU) and The Foundation for Scientific and Industrial Research at the Norwegian Institute of Technology (SINTEF) and engaged in in-depth analyses of five companies that were implementing significant changes at the time (Elvemo et al., 2001). The Norvest Forum Module, located in the regional business district of Ålesund in cooperation with the regional learning network (NVF), was interested in enhancing learning possibilities (Hanssen-Bauer, 2001). Finally, the second Oslo-based module stemmed from a cooperation of the Institute for Applied Social Science (Fafo) and the Norwegian School of Economics and Business Administration (NHH) in Bergen. It worked on collaboration in and between enterprises (Colbjørnsen & Falkum, 2001).

This decentralization of research efforts had consequences for the Action Research community in Norway. Previously, the Action Research approach was dominated by the WRI, and the dialogical approach developed there. With the decentralization of the research efforts, researchers who had previously not worked in the Action Research field were now invited to contribute. To exemplify the changes introduced by this new approach, I will briefly discuss two modules with contrasting approaches in the following.

The WRI's module in Oslo appears to be a natural representative of the efforts connected with ED 2000. This is not a coincidence; however, to arrive at the goals set out for ED 2000, the researchers at WRI had to go through a learning process themselves. This process involved learning from the other modules and their newly introduced approaches. The WRI has been the center for work-life reform in Norway and was responsible for the ID Project. Thus, it has been based on a tradition of Action Research since the 1960s and went through the stages of the classical STSD

approach to the Search Conference paradigm. Bjørn Gustavsen, Professor at WRI, was a central protagonist in the establishment of ED 2000 (Levin, 2002, p. 4). The WRI contacted a total of 20 companies – with most of them they had a history of collaboration. In line with a distinction of operational and developmental tasks developed earlier (Pålshaugen et al., 1998), the researchers tried to improve the participatory factor on the developmental side of the companies. Fifteen companies invited the researchers for a first meeting. Access to the field was ensured through dialogue conferences as developed within the Search approach. Ten companies participated in the first Search conference, while five entered a longstanding collaboration with the researchers (Pålshaugen, 2002). Among those five, the cooperation with the weapons production company Kongsberg Defense & Aerospace, which had been given a contract to develop a new Sea-target Missile (MSN), can be seen as a good example of the general efforts in this module's first phase. From the beginning, the project was set up to last eight years, the total time for developing the new system. As a development strategy, the company had decided to utilize "concurrent engineering" for the first time. In concurrent engineering, all the cycles of a product are part of the design process, which means that the conceptualization of a product takes into account all the various stages of production and the complete life cycle of the final product at the very beginning instead of designing the product to then conceptualize the mass production process and possibly handle recycling issues once they become prevalent (Hartley, 1992, pp. 123–142). Because Search conferences were utilized, the design team increased over the course of the development from 90 to 200 engineers, taking into account all the various parties involved in the eventual production process. The Search conferences managed to transport a general understanding not only of the design of the new product but also of the general steps of production involved, thus creating a holistic picture of the entire process for everyone. From this starting point, the engineers determined the necessary working groups and the general steps for the project.

Similar projects were carried out in the other four participating enterprises– VBK ASA, a supplier company for the transportation industry, Fellesslaktriet, a slaughterhouse co-op, ELKO AS, a producer of electrical fittings in conjunction with OSRAM AS, a light bulb producer, and OCEANOR, a producer of high technology products for oceanography. However, the evaluation report after two years of ED 2000 felt that the Oslo module's efforts were unsuccessful compared with the specified goals. Most crucially, as it seems, the WRI had continued with their general research strategy, that is, Search conferences with the researchers being consultants to the companies. No integration of the researchers in the production process was achieved, and no networks between companies were formed (Fine, 2001). As a result, the HFB forced the Enterprise Development Group at WRI, which was leading the research in the first half, to be integrated with the Process Industry Group, which did action research studies primarily in the oil industry and was also located at the WRI. As Øyvind Pålshaugen, director of the Enterprise Development Group put it, this "mark[ed] the end of the beginning, but by no means the beginning of the end" (Pålshaugen, 2002, p. 40).

The Nordvest Forum module was based on a network of local companies by the same name (abbreviated NVF) located around Ålesund in northwest Norway. The purpose of the forum was "to be an industry center for leadership and organizational development, which, by increasing management skills, developing organizations and establishing networks, contributes to an increased value for owners and users" (Hanssen-Bauer & Raabe, 2002). It was founded in 1989 and consisted of close to 150 companies at the time when ED 2000 went underway. Ålesund is one of the industrial areas of Norway with a strong export orientation and products related to fishing and furniture. Companies were small to mid-size and either had a long history of family ownership or were still family-owned. The application to form a module came from the board of directors of the NVF because they felt a need to tighten the connections with research institutions in the area. From the beginning, the module followed an Action Research approach "intended to nurture co-generative learning among researchers and practitioners, as well as between researchers from various institutes and disciplines" (Hanssen-Bauer & Raabe, 2002, p. 111). Because of the large number of companies involved and the relatively small size of each enterprise, it was clear from the beginning that the project's focus would not be on individual companies but the network. After an evaluation of the network's current capacities and abilities, two primary objectives were agreed on: (1) creating a learning agenda controlled and driven by the needs of the participating companies and (2) systematically collecting local experience from practical enterprise development work. Two doctoral students were hired – this move was hoped to support a link to the Norwegian University of Science and Technology (NTNU). Also, the network was connected with the International Institute for Management Development, Lausanne (Switzerland). The doctoral students were employed at the NVF and had teaching responsibilities at the university. As such, they were intended to function as reflection partners, bringing knowledge from the university to the network and vice versa. They were involved in the project on three levels: Teaching, administrating the network, and cooperation with the local companies. In addition, the doctoral advisors were brought in to organize workshops for the workers and company management. Workshops were used to share the knowledge gained during the research process in the individual companies. Specifically, the work with Ekornes and the Stokke Group became famous because it developed the first forms of Demand Chain Management thanks to the extension of the network and the establishment of close relationships with suppliers and external customers (Raabe, 1999).

The ED 2000 project is interesting in many ways. It introduced the first real large-scale effort to cover the entire Norwegian industry. Second, it changed the focus from individual companies to networks, specifically emphasizing the connections between various companies. Third, it pluralized the number of approaches and the number of research centers involved. However, at the same time, ED 2000 poses several questions concerning the concept of "research" underlying the endeavor. The lack of unity among the projects led to empirical and theoretical eclecticism, arriving at a state of pragmatism in which actions are justified because they "work." Consequently, the form of evaluation of the research projects was that of the

researchers' self-evaluation in narrative form (Levin, 2002). On the economic side, evaluative factors were the increase in productivity and efficiency for the various companies involved. It could be argued that the introduction of networks provides an overall theoretical framework specifically in comparison to the open systems approach underlying the first phase of the ID Project. However, "networks," i.e., the close collaboration of several companies in the production process, seems to reflect a general trend in economic development in the 1990s (King & Robinson, 2000). From this perspective, all that can be said about the ED 2000 project is that something was done that produced some form of change.

ED 2000 managed to introduce a focus on difference instead of sameness. The goal was to learn about one's project by explicating the differences to the approaches of others. At the same time, it abandoned Lewin's classical cycle of steps by essentially collapsing the steps of analysis, action, and evaluation into the action phase. Action from now on was seen as a unity of those three steps. A general approach to the various research sites was also considered to be unpractical since local circumstances were thought of as demanding different approaches. To provide a common ground, all participating institutions subscribed to the loosely defined concept of "innovation" as the shared goal. "Innovation" as defined in the ID 2000 project, however, is not a term backed up by theory. Instead, it is the common denominator for an underlying but implicit ideology of "improvement." All projects were "well intentioned" in that none fostered the maximization of profits or the increase of productivity at the disadvantage of the workers. Instead, the situation of all should be improved, that is, "innovated." However, the lack of an overarching theory increased the knowledge deficit about what should be improved.

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Chapter 6
The Southern Tradition

Conflict Intervention Instead of Consensus Intervention: The Focus on Empowerment in Participatory Action Research

Much different from the various understandings of subjectivity and democracy developed in the Northern Tradition and often formulated as a critique of the latter's particular rendering of participation as dialogue and organizational change, a tradition of Action Research became popular in the 1970s specifically in Latin America, Asia, and Africa, which emphasized the importance of the periphery and its role in creating knowledge to change the center of society. Although the label Participatory Action Research has spread and is increasingly applied to a whole range of action research projects, which believe in sustainable development through and with the people affected (Whyte, 1991; Castellanet & Jordan, 2002; McIntyre, 2007; Kindon et al., 2007; Kapoor & Jordan, 2009), the label first occurred alongside claims hardly reconcilable with a lot of participatory action researchers today. In this tradition, participation carries a radically different theoretical and practical significance, which this chapter intends to disclose.

Catalyzed by Paulo Freire (1970/1996, 1974/2005), who was appalled by the situation of the illiterate poor, education was conceived to be a tool for changing society structurally. Knowledge, which he equated with power, had traditionally been monopolized by an elite that sought to protect its interests. The learner was considered a passive recipient of knowledge deposited by an educator, just as money is deposited in a bank. Freire organized a multidisciplinary team to develop educational materials that would stimulate the poor to reflect on their lives and the underlying causes of their conditions. People were organized in "cultural circles" to recover their identity and indigenous knowledge. Dialogue on controversial issues such as land tenure rights was the central process, followed by reflection and action. Thought-provoking photographs were used to initiate this process, which Freire called concientização. This Portuguese term implies a liberating process whereby

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oppressed people evolve toward a state of critical consciousness. Threatened by his efforts to change society structurally, the Brazilian government jailed Freire, who eventually sought exile in Chile. He later worked in Portuguese-speaking African countries.

Another militant who sought to construct a new social order and further developed Freire's educational philosophy into an account of Participatory Action Research was the Colombian sociologist Orlando Fals Borda (Fals Borda, 1987). Coming from an academic setting, Fals Borda describes himself as being isolated from the realities of social life. Motivated by the desire to find a balance between reflection and action, he and a group of university intellectuals went out into the field to bring science to rural people. At first, communication barriers and differences between their concepts of reality caused the farmers to reject Fals Borda's group. Moreover, the technological solutions developed by the university researchers did not apply to rural conditions. After deep reflection, Fals Borda radically reoriented his work, no longer treating rural people as passive "objects" but encouraging them to become active "subjects" or agents of their liberation. He was involved in militant action research for a period, collaborating with an aggressive association of small farmers that had invaded lands belonging to large cattle ranches in northern Colombia.

Most characteristically, Fals Borda has been described as taking one of Serge Moscovici's maxims to heart: "When Social Psychology begins to be dangerous, it will then begin to be a science" (Moscovici, 1972/1994, pp. 66, my translation and Brandão, 2005, p. 45). And indeed, Fals Borda's understanding of action research – becoming popular in the 1980s and surviving until today – hinges on science being transformed into countervailing knowledge through an intervention in political and cultural power relations.

The key to this understanding is a holistic concept of participation indebted to the Spanish philosopher José Ortega y Gasset, who imported the German existentialist *Erlebnisphilosophie* into a philosophy of authentic life experience. Fals Borda supplements this philosophy of authenticity with that of a genuine commitment to those who are exploited or oppressed.

First, learning to interact and organize with PAR is based on the existential concept of experience proposed by the Spanish philosopher Jose Ortega y Gasset. Through the actual experience of something, we intuitively apprehend its essence; we feel, enjoy, and understand it as reality and place our being in a wider, more fulfilling context. In PAR, such an experience, called vivencia in Spanish, is complemented by another idea: that of authentic commitment. This combination of experience and commitment allows one to see for whom such knowledge is intended, in this case, the base groups themselves. Moreover, such a concept of experience recognizes that there are two types of animators or agents of change: those who are external and those who are internal to the exploited classes. Both types are unified in one sole purpose – that of achieving the shared goals of social transformation. These animators (internal and external) contribute their knowledge, techniques, and experiences to the transformation process. But their knowledge and experience stem from different class conformations and rationalities (one Cartesian and academic,

the other experiential and practical). Thus, a dialectical tension is created between them, which can be resolved only through practical commitment, that is, through a form of praxis. However, the sum of knowledge from both types of agents makes it possible to acquire a much more accurate and correct picture of the reality being transformed. Therefore, academic knowledge combined with popular knowledge and wisdom may result in total scientific knowledge of a revolutionary nature that destroys the previous unjust class monopoly (Fals Borda, 1991, pp. 4–5).

The Marxian tone in this account of commitment, mixed with a Lukácsian and Gramscian inflection of praxis, is hard to miss. The researcher needs to transform herself into a mediator who can transcend "the asymmetry implicit in the subject/object relationship that characterizes traditional academic research and most tasks of daily life," change it into a "subject/subject relationship" (Fals Borda, 1991, pp. 4–5) and break the asymmetrical relationship of submission and dependence.

In this light, participation is more demanding than in the Northern Tradition. In addition to the willingness for conflict and the awareness of its necessity, it requires authentic participation of both researchers and co-researchers that Fals Borda defines as

> rooted in cultural traditions of the common people and in their real history (not the elitist version), which are resplendent with feelings and attitudes of an altruistic, cooperative and communal nature and which are genuinely democratic. They are core values that have survived from original praxis in spite of the destructive impact of conquests, violence, and all kinds of foreign invasions. Such resistant values are based on mutual aid, the helping hand, the care of the sick and the old, the communal use of lands, forests and waters, the extended family, matrifocalism and many other old social practices which vary from region to region but which constitute the roots of authentic participation. (Fals Borda, 1991, p. 5)

Apparent in the quote's emphasis on core values that have survived their societal corruption is a Romantic bent which affirms a certain purity of folk culture and regional ethnicity – something Muhammed Anisur Rahman terms the organic structure of a particular society (Rahman, 1991, p. 22). Indeed, the techniques of PAR "useful in the establishment of people's countervailing power" are: regional knowledge derived from "meetings, socio-dramas, public assemblies, committees, fact-finding trips and so on," a critical recovery of history through the disclosure of oral traditions, which yield data and facts that "correct, complement or clarify official or academic accounts written with other class interests or biases in mind," and basic recognition of folk culture expressed in "cultural and ethnic elements frequently ignored in regular political practice, such as art, music, drama, sports, beliefs, myths, story-telling and other expressions related to human sentiment, imagination, and ludic or recreational tendencies" (Fals Borda, 1991, pp. 8–9).

The research project is less described as a step-by-step development of practical knowledge; the present world is not worked upon by new forms of knowledge applied through participatory practices. Instead, the current world is entirely reworked through the exposure of authentic life forms and hidden – at times repressed – cultures of knowledge that experience this knowledge as liberating. The application of this knowledge consists in realizing the need for a radical

transformation of society and providing for the dissemination of knowledge on a micro and macro level.

Fals Borda stresses "the obligation to return this knowledge systematically to the communities and workers' organizations because they continue to be its owners. They may determine the priorities concerning its use and authorize and establish the conditions for its publication, dissemination, or use." In general. However, it is the researcher's responsibility to seek for as many efficient forms to communicate the knowledge as possible, including "the use of image, sound, painting, gestures, mime, photographs, radio programs, popular theater, videotapes, audiovisual material, poetry, music, puppets, and exhibitions. Finally, there are material forms of organization and economic and social action developed by base groups (cooperatives, trade unions, leagues, cultural centers, action units, workshops, training centers, and so forth) as a result of the studies carried out" (Fals Borda, 1991, p. 9). Ideally, a language of "symmetric reciprocity" is created which "is based on daily intentional expressions and is accessible to all, avoiding the airs of arrogance and the technical jargon that spring from usual academic and political practices, including ideological elements from the current developmentalist discourse" (Fals Borda, 1991, p. 10).

The focus on ethnographic research, knowledge ownership, and natural language is crucial here. It can be seen as a political response to a specific understanding of the crises on the left and the right. To both camps, PAR would answer with a more authentic democracy that can neither be achieved through one grand-scale political solution with a vanguard party as its locomotive nor an undertheorized and thin concept of subjectivity entirely compatible with liberal societies. Instead,

> [t]he creative sociopolitical force set in motion by PAR may lead to the conformation of a new type of State which is less demanding, controlling and powerful, inspired by the positive core values of the people and nurtured by autochthonous cultural values based on a truly democratic and human ideal. Such a State would be neither an imitation of existing historical models, the failures of which are easy to recognize, nor a copy of earlier representative democracies. It would strive for a more even distribution of power-knowledge among its constituents, a healthier balance between State and civil society with less Leviathanic central control and more grassroots creativity and initiative, less Locke and more Kropotkin. In effect, it would seek a return to the human scale which has been lost in the recent past. (Fals Borda, 1991, p. 6)

Specifically, Fals Borda and Rahman, but also researchers like David Brown, Rajesh Tandon (Brown & Tandon, 1983), and Steven Jordan (Jordan, 2009) carefully point out the difference between PAR and action research in general. Although sympathetic to the further dissemination of participatory ideals, they are often skeptical about a possible convergence and emphasize the problematic effects of methodological and rhetorical co-optations.

> The growing application, status and cooptation of PAR in many quarters call for an attempt to promote greater clarity as to what it is and is not, both at the micro and the macro levels. At the micro level, PAR is a philosophy and style of work with the people to promote people's empowerment for changing their immediate environment – social and physical – in their favor. … Two elements of empowerment that are considered by PAR to be the most important are autonomous, democratic people's organizations and the restoration of the

> status of popular knowledge and promoting popular knowledge. … [At the macro level,] PAR has moved beyond the village cluster level, and is a multidistrict or province-level phenomenon with formal or informal structures linking the base level processes. … How far such multiplication processes may move in any given country and at what speed cannot be predicted, just as it cannot be predicted how far any other effort for social transformation, "revolutionary" or otherwise, may spread in any country where such effort has to move through significant resistance and also needs qualified manpower (sic) of its own to expand in scale. However, in terms of macro-social transformation, PAR at this stage may be viewed more as a cultural movement, independent of (in some countries in link with) political movements for people's liberation rather than a political alternative itself. (Rahman, 1991, pp. 16–19)

In light of this political response in favor of cultural transformations, the role of knowledge becomes even clearer. The Southern Tradition criticizes scientific truth but likewise embraces the significance of science in the production of a different truth. It criticizes reason but embraces the role of reason in transforming the relations of power and knowledge. Ultimately, the criteria are set alongside a specific interpretation of history, subjectivity, and democracy, as is nicely illustrated in Rahman's treatment of the question of leadership.

> While self-emancipation of the "working class" was, indeed, the original revolutionary vision of Marx and Engels, it may be suggested that the situational difficulty of the working class initiating its own liberation on a macro scale creates a vacuum in leadership which gets filled in by intellectual-activists trained in the schools rather than in life. It is a tragedy of the first order that these very intellectuals in their great wisdom not only fail to recognize the limitations of their knowledge and understanding. They also do not recognize the alienation between themselves and the people, overlooking or denying the new dialectics they introduce in the social scene by assuming revolutionary leadership even if this were fully well intentioned. (Rahman, 1991, p. 20)

He continues by showing that democracy was equally misunderstood as either neoliberal democracy or socialist democracy, that both thwart the true subjectivity expressed in free and creative action by bureaucracies. With the initiative resting in an administrative, nonproductive class, the crisis of the left and the right boils down to the dissociation between the productive forces and the leadership of concerned societies. In a final leap, Rahman connects democracy and leadership by first claiming that "democracy, in any case, is a necessity for a revolutionary development of the productive forces" and then analyzing that with the dissociation of productive forces and leadership "prevailing in any society, the need is therefore to generate social processes which would promote the possibility of an organic leadership ('organic vanguard') to emerge – a leadership which would organically and not merely intellectually, belong to and represent the interests of significant productive forces in the society" (Rahman, 1991, p. 2122).

Against this background of theoretical and practical underpinnings of participatory action research, which rest in a strong divide between alienated and authentic subjectivities and democracies, the following facts will no longer be surprising. Although researchers find much common ground between the Northern and the Southern Tradition of Action Research, for example, in their emphasis on practical knowledge that aspires to have a bearing on society, the critique of positivist

science, and the importance of developmental change as a consequence of inquiry, it is also attested that a true exchange of information and experience is almost inexistent. Indeed, it can be questioned whether the assumption of a general convergence between both traditions might have been adopted too hastily.

In a short article in 2006, Fals Borda comments on this perspective with encouraging words offering an almost rosy future for joint endeavors. A closer reading, however, suggests that his vision leaves him much more skeptical. Thus, he positions himself as a Southerner and comments critically on a "convergence" account that had tried to summarize participatory action research (Chandler & Torbert, 2003).

> [W]ith all due respect, from the standpoint of a Southerner I was somewhat disappointed for two reasons: 1) the choice of illustrations for the 'flavours' is uneven and limited, they refer mostly to Northern contexts—reminiscent of Kurt Lewin's limited social psychological approach—and are drawn from a single source, the otherwise marvellous Handbook of Action Research edited by Peter Reason and Hilary Bradbury in 2001; and 2) the authors postpone articulation of 'the necessary aims of social research' (p. 148) regarding the ethical and political aspects and the variance of human-action settings involved, that is, the connection with grand theory and the pursuit of alternative paradigms that would be of help in 'the transformation of the lives with others' (p. 148). (Fals Borda, 2006, p. 352)

He continues by acknowledging the successful efforts of Scandinavian researchers and integrates them into the second and third wave of participatory action research. Although the tone of a common struggle for justice and peace dominates the piece, it is equally clear that he refers back to a first wave, the heritage of which considers important to construct a meta-narrative that subsumes the rest of Action Research:

> [I]t is preferable for us to seek our own explanations for building an alternative paradigm, by studying our indigenous or founding regional groups and emphasizing their values of human solidarity. Some of these groups extend from one country to another without respecting present frontiers. Other groups in Latin America include the descendents of pre-Columbian indigenous populations, characterized by their values of cooperation and mutual aid; of runaway black slaves who established their own enclaves, characterized by their love of liberty; of peasant-craftsmen from Iberia, who were against the landed gentry and supporters of independence, characterized by their dignity; and the settlers who have been pushing the farming frontier into virgin land, characterized by their assertion of peaceful autonomy. The fundamental values of the founding groups provide a strong ethos identifiable with a deep-rooted non-violent socialism that is worth salvaging to repair our deteriorating social fabric. (Fals Borda, 2006)

This deep-rooted, nonviolent socialism capable of repairing our deteriorating social fabric does not necessarily square well with the turn to Habermas and Foucault in the Northern Tradition, Fals Borda's acknowledgments notwithstanding. To be sure, Fals Borda might indicate a move away from the emphasis on, at times, militant interventions in conflictual situations. Still, the essential worldview behind the choice of these specific means has not changed. For that matter, the analysis Brown and Tandon provided in 1983 still holds (Brown & Tandon, 1983). In this article, they stress differences in ideologies that are reminiscent of the above discussion of subjectivity and democracy.

According to Brown and Tandon, action researchers in the Northern Tradition frequently assume common interests and are surprised if top managers resist worker analyses. Participatory action researchers, on the contrary, do not expect cooperation from corporate landowners or company management and assume that those parties will resist the project and plan accordingly. Equally, while the Northern tradition might believe that enhanced efficiency and effectiveness will improve the situation for all system members, even if short-term effects concentrate wealth and power in relatively few hands, Southerners believe that the increase in equity of resource distributions and self-reliance of oppressed groups is critical and might be brought about at the expense of economic efficiency or growth.

In addition, while Northerners tend to stay neutral and aspire to enhance the output of a system as such, Southerners are explicit about client contributions; in fact, their immediate clients are defined as a "wide range of exploited or oppressed groups," and they start with the assumption that oppression is a central problem. Also, even if no violent means are applied, data collection and analysis look fundamentally different. While Northerners emphasize mutual trust and iterative data collection and analysis to develop shared diagnoses, Southerners might use militant observers who do investigative "conflict methodologies" to extract information from uncooperative adversaries. As a last difference, Tandon and Brown point out that results are used differently: While action research contributes to "knowledge" in general, that is, to the scientific and economic community more broadly conceived, PAR seeks to join one set of actors to change the status quo. While the last decision about implementing specific changes is entirely the management's responsibility in a Northern Action Research project, PAR tends to be suspicious of the system's affirmative forces (Brown & Tandon, 1983, pp. 284–287).

In an earlier article, Fals Borda replicated this line in which he calls for a reorientation of accumulating and systematizing knowledge in "teleological terms" leading "into more integrated academic and popular, or common-sensical, knowledge so that a new type of 'revolutionary science' (in Kuhnian terms) becomes a real possibility, not only a felt necessity" (Fals Borda, 1987, p. 330). It is specifically in the discourses of development and participation, both of which he links to Foucault, that he finds significantly different ontological conceptions to those expressed by Third World approaches.

> The developmentalist discourse, as is well known (Foucault's thesis of the archaeology of knowledge can help us in this respect), involves dealing with the concepts of poverty, technology, capital, growth, values, and so forth, as defined from the standpoint of rich, developed countries (where in fact the concept of development was first proposed), a discourse organised into a coherent intellectual whole for the purpose of rationalising and defending the worldwide dominance of those rich and powerful societies. The participatory discourse or counter-discourse, on the other hand, initiated in the Third World – quite probably as an endogenous dialectical response to the actions of the developed world – postulates an organisation and structure of knowledge in such a way that the dominated, underdeveloped societies articulate their own socio-political position on the basis of their own values and capacities and act accordingly to achieve their liberation from the oppressive and exploitative forms of domination imposed by opulent (capitalist) foreign powers and local con-

> sular elites and thus create a more satisfactory life for everyone. In this way a more human Weltanschauung, or world outlook, could be fashioned. (Fals Borda, 1987, p. 331)

To be sure, a distinction in critical versus practical action research approaches might work well for a whole range of researchers (Johansson & Lindhult, 2008); Brown and Tandon themselves suggest that the thrust behind the divide rests on the legitimacy of current distributions of power and resources (Brown & Tandon, 1983, p. 289). But both underestimate the importance of countering present conditions through regionalized forms of deeply embedded and authentic knowledge that, in a Romantic fashion, is more humane than what society can make us participate in. The democracy to come is ultimately mapped on a concept of authentic subjectivity, which essentializes humankind in purely natural conditions, a world without technology, capitalist growth, and economic progress. If the Northern Tradition is interested in participating in discourse, hoping to change its outcome thereby, the Southern Tradition is interested in countering discourse, hoping to reveal authentic structures of human social life thereby.

The 1979 Appalachian Land Ownership Study

The Appalachian Land Ownership Study is recognized as a pioneering effort in the field of participatory action research. In essence, the Study worked with the local population because they were seen to be subject to oppression by capitalist interests. The goal was to unite the people and produce a collective effort to overcome oppression, much as expressed by the first wave of Participatory Action researchers.

Appalachia is a region of the United States stretching from New York State to Alabama, one of the country's most densely populated rural areas. Appalachia carries large amounts of natural resources, such as coal, land, timber, and water. While the importance of these resources has declined today, the region has been subject to exploitation by large companies since the beginning of the twentieth century. As a result, the richness of the country in natural resources has soon been contrasted by a growing poor population. To a large degree, land ownership and use were the cause of the rise in poverty, for land ownership determines how the land is used and affects the jobs created, the taxable income in the region, and as such, the finances of the local communities.

In the late 1970s, when the Appalachian Land Ownership Study was conducted, large portions of land were owned by corporate, often absentee, interests, specifically by the coal mining and agricultural industry. In addition, tourism increased in some areas of Appalachia and fostered a particular kind of economic development, turning farms into golf courses, homes into condominiums, and wells into water slides.

These geographic and economic patterns created a growing class of rural industrial workers which, paradoxically, contributed to the ongoing destruction of land by strip mining. The loss of agricultural land and the heavy coal mining made the land

increasingly vulnerable to extreme natural conditions. In 1977, major floods left approximately 20,000 people homeless. While the government quickly provided relief trailers, it refused to seize corporate land to provide refuge. The companies, mainly of the coal mining industry, that owned the land had equally no interest in providing their land to offer shelter.

In response, citizens of Mingo County, West Virginia, started to gather groups from around the region to discuss possibilities of protest. Eventually, the Appalachian Alliance was formed. Their primary antagonist was the Appalachian Regional Commission (ARC), a government agency responsible for Appalachian development. Back then, the ARC's primary strategy was to foster growth in urban centers, encourage inhabitants of rural areas to move to the cities, sell their land, and leave even more land for corporate use. It was the goal of the Appalachian Alliance to develop ways in which they could effectively challenge land policies in the region. Together with the Appalachian Studies Association, a task force was established.

To challenge ARC policies, the task force decided to study land ownership patterns in the region and apply for funding from the ARC, arguing that their policies could only be effectively inaugurated with some grounding knowledge regarding land usage. The study specifically included the following goals, and the training was provided by John Gaventa and Billy Horton from the Highlander Research and Education Center:

- Provide comprehensive information that would be useful to local groups to influence regional and national policies on land-related questions.
- Envision a model for citizens to engage in research that grows out of their concerns.
- Train local citizens in obtaining information useful to them to influence policymakers.
- Develop a network of citizens and other organizations interested in land issues.
- Develop action plans to improve the situation of the local population.

To grant the funding, the ARC insisted on the following terms:

- A phased release of funding; new funding coming only after subgoals of the study were achieved.
- Requirement of approval from the governor's office in each state to conduct research.
- Development of a mutually acceptable framework of analysis, spelling out the details of the methodologies to be used. This process specifically prioritized a quantitative approach since this was regarded as "hard science."

According to John Gaventa and Billy Horton from the Highlander Research Center, the ARC wanted to deny funding from the very beginning. It purposefully attempted to derail any plans and projects the Appalachian Alliance put forth. Therefore, they compiled a preliminary report about the situation in Appalachia and threatened to go public with their results, including a range of criticism of ARC policies. The funding was granted (Gaventa & Horton, 1981, p. 32).

With the goal set to collect as much data about land ownership patterns in the region as possible, systematize this data, and compile an accurate report to foster further action, the study went underway. The Land Ownership Task Force and the Highlander Research Center coordinated the project. The participation of states relied upon self-selection, i.e., states were included if local citizen groups were able to recruit local researchers and a state coordinator. The local groups then decided which counties and major land-related issues in their states would be included in the study. Eventually, 80 counties from Alabama, Kentucky, North Carolina, Tennessee, Virginia, and West Virginia were included in the survey phase, and 19 of these participated in the case study phase. Researcher training took place at a workshop in May 1979 at the Highlander Research Center to discuss the methodology of the project as well as train participants to conduct further workshops with local citizens in their respective counties. Follow-up workshops decided on the format and content of the final report, the dissemination of the results, and further action strategies. Field research for the land ownership study was completed in the summer and early fall of 1979. At the same time, outside funding was secured in order to loosen the dependency on grant money from the ARC. Over the following 18 months, the results were analyzed and discussed among approx. 100 co-researchers. On April 3, 1981, the final report, entitled the "Appalachian Land Ownership Study," was released (Force, 1981).

Most interestingly, the study did not prove to be significant based on new insights. Instead, it consistently documented already known facts and translated those into the language of policymakers needing facts and figures.

Among others, these were the main results of the study:

- 1% of the local population, absentee holders, corporations, and government agencies, control 53% of the total land surface in the 80 investigated counties.
- Three-quarters of the surface acres and four-fifths of the mineral acres in the survey were owned by entities residing outside the counties or outside the state.
- 40% of the land in the survey and 70% of the mineral rights were owned by corporations.
- Mineral rights were greatly underestimated for property tax purposes.

In addition, the task force called for land reform. It provided specific policy recommendations to shift more of the tax burden onto corporations and the wealthy, protect land owners' rights against surface mining, increase the power of government to confiscate corporate land for alternative economic development and housing construction, protect agricultural land, and institute local planning and zoning to regulate land use and environmental impacts (Force, 1983).

As anticipated by the co-researchers, the APC did not produce a press release when they received the report, nor did they disseminate any of the copies to local newspapers, agencies, households, etc. Moreover, it refused to accept the county case studies (including several qualitative interviews) as part of the report because the project director regarded this information as "unscientific" and too subjective.

The task force had developed its own press and release strategy, however, and was thus able to bring the findings to the attention of the public. In addition,

sub-reports were produced for the counties and were disseminated to the respective neighborhoods. Both the task force and the research center highly valued education. They organized numerous local meetings in which they informed community groups about the findings of the land study and started participatory projects in which researchers and co-researchers would think about further possibilities for resistance across communities. They established a subgroup to develop ideas about land reform alternatives and conducted workshops. The issues they were concerned with included:

- How can global issues be structured, such as their impact on the local level becomes evident?
- How can re-education of the people be incorporated?
- What kind of measures of resistance can be deployed?
- What would a better society look like?

In a more recent article, Shoanna Scott evaluates the results and effects of the Land Ownership Study. She highlights its impact on political and juridical processes and the development of Participatory Action Research as a whole (Scott, 2009). While the Land Ownership Study remains of central importance as a pioneering effort and milestone in the field of Participatory Action Research, the results on the ground are divided. Almost none of the report's recommendations were carried out. However, some progress was made in taxing unmined minerals and protecting landowners from the negative effects of surface mining in Kentucky and West Virginia. But by the time the study was published in 1983, land study data had been used by citizens in all six of the participating states. Scott further specifies:

> In West Virginia, the land study findings were introduced as evidence for a court ruling which declared that West Virginia's rural counties had been unconstitutionally discriminated against due to shortcomings in the property tax system used to fund local schools. The Appalachian land study had its most significant impact in Kentucky, where it helped form networks and leverage resources that resulted in Kentuckians for the Commonwealth (KFTC). This organization continues to develop grassroots leadership in Kentucky and provide a voice for citizens in the state legislature. This project can also be linked, either directly or indirectly, to the emergence of other groups in Appalachia that continue to pursue community development, environmental protection, social justice, and democratization in Virginia, West Virginia, and Alabama. (Scott, 2009, p. 198)

In summary, the Appalachian Alliance maps directly on the Participatory Action tradition as described by Brown and Tandon (Brown & Tandon, 1983) above. It arose out of the need of oppressed groups to transform power structures; thus, both the Land Ownership Task Force and the Highlander Research Center and all the initiatives following from there strongly identified with one group of participants while regarding other actors such as the coal mining companies and the Appalachian Regional Commission as their adversaries. None of the research was done in accordance with systemic structures, instead,

> "the ultimate goal is 'the radical transformation of social reality.' For many of us involved in the land study, … the need for some radical restructuring of society seemed self-evident."

> Some were even doubtful to the extent that they believed it needed "radical changes in the way wealth is distributed in capitalist America." (Gaventa & Horton, 1981, p. 39)

To illustrate the contrast to the Northern Tradition, Brown and Tandon specifically mention the Appalachian Land Ownership Study as a paradigm case that explains the difference between conflict intervention and conflict mediation. None of the activists aspired to reach a common understanding with the landowners; they were skeptical that enhanced effectiveness combined with a worked-out consensus could improve the situation of everyone involved. Their strategy does rather consist of the attempt to counter the power of cooperations through the use of conflict methodologies, which can be nonviolent, such as advocacy research, geographic investigations, and qualitative data collection. But they can also involve militant observers and more radical forms of protests. The Appalachian project mainly involves countering discourse, restructuring power relations, and redistributing material resources and economic profit.

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Part III
Klaus Holzkamp and the Concept of Subjectivity

On different pathways, the Northern and the Southern Traditions of Action Research arrived at similar states. Theory slowly moved to the background or was consumed by a priori assumptions about human needs, right and wrong, or general ideas about societal development. In the foreground, action as the primary vehicle manifested itself. In the Southern Tradition, action as organized resistance against oppression prevailed. In contrast, in the North, action became the central category, which was now to include analysis and evaluation as a response to an assumed increase in the speed of socio-technical developments in turbulent times. The problem with both these developments is the problem of justification. Without an underlying theory of human development, capabilities, and needs, without an understanding of societal structures, the action carried out in action research projects become mere actions of interest groups or stakeholders. From this standpoint, I will, in the following, introduce Klaus Holzkamp's concept of Critical Psychology to show how the Southern, as well as the Northern Tradition, could benefit from the integration of Holzkamp's theory and praxis in Action Research.

Chapter 7
What Is German Critical Psychology

A Short Introduction

Morus Markard, one of the central contributors to the development of Kritische Psychologie in the past decades, starts his introduction to the field of German-Scandinavian Critical Psychology with the claim that "strictly speaking, critical psychology does not exist" (Markard, 2009, pp. 13, my translation). And indeed, besides very few claims of or hopes for a unification (see, for example, Jones & Elcock, 2001; Billig, 2008; Richards, 2010), critical psychology is a mere umbrella term for several, sometimes highly distinct, approaches. The list is long: There is critical developmental psychology (Morss, 1996), critical social psychology (1997), critical applied psychology (Prilleltensky & Nelson, 2002), critical sport psychology (Moran, 2004), critical health psychology (Lyons & Chamberlain, 2006), postmodern psychology (Hook, 2007), critical psychology of religion (Carrette, 2007), critical law psychology (Kapardis, 2010), discursive psychology or critical psychology of language (Forrester, 1996; Parker, 2002), critical pedagogic psychology (Stephenson Malott, 2011), and many more. The critique of a so-called "mainstream psychology" could be a uniting feature of these various approaches. However, once examined more closely, it becomes evident that the mainstream these approaches are set up against is not a unified construction either. Sometimes this mainstream is constituted by a specific theory (or lack thereof); sometimes, the methodological approach – namely nomothetic science – is the focal point of criticism; sometimes, it is the lack of political involvement or, alternatively, the conspiracy of psychology with governing powers. This assumed "mainstream" is thus perhaps best characterized as a boundary construction needed to uphold the critical position and a "mainstream psychology in crisis." As Jaan Valsiner puts it:

> [T]he "crisis talk" can expand into other rhetoric forms that establish their own existence in the idea-scape of the discipline, becoming encapsulated, and thus providing an arena for discourse that is immune to innovation. […] [T]his has given rise to the label "critical psychology" that has developed on the margins of psychology since 1960s and has developed

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> its own contrarian social organization. By creating a new consensus group on the basis of critique of its opponent, vaguely labeled the mainstream, the direction of critical psychology opens for itself a legitimate rhetoric domain of being critical of—while not attempting to change—the discipline. … [T]he critical genre … ends up supporting precisely the object of its critique. (Valsiner, 2012, p. 164)

Following Valsiner, what unites critique in psychology is indeed not so much a shared theoretical framework or a specific political agenda, let alone attempts to change the discipline, but much more a certain organizational formation, i.e., social networks, specific conferences, etc., where researchers meet who regard themselves as being "on the margins."

Ian Parker, in his attempt to unite the field of critical psychology, carves out four main strands critical psychologists should follow: First, critical psychologists have to insist that the historical development of psychology as understood in the "mainstream" is too narrow because it reduces the history of psychology to the history of an institutionalized discipline. To create an all-encompassing psychology, "the systematic examination of how some varieties of psychological action and experience are privileged over others, how dominant accounts of 'psychology' operate ideologically and in the service of power" must be included. This secondly includes "the study of the ways in which all varieties of psychology are culturally historically constructed, and how alternative varieties of psychology may confirm or resist ideological assumptions in mainstream models" (Parker, 1999, p. 13). At this point, thirdly, "the study of forms of surveillance and self-regulation in everyday life and the ways in which psychological culture operates beyond the boundaries of academic and professional practice" (Parker, 1999, p. 14) has to explore "the way everyday 'ordinary psychology' structures academic and professional work in psychology and how everyday activities might provide the basis for resistance to contemporary disciplinary practices" (Parker, 1999, p. 15). In essence, Parker describes a circular movement of critical psychology, starting from the discipline as it constitutes itself in academic history, moving along its practices and its (re)considerations of the world to eventually criticize the mainstream from the perspective of ordinary (or everyday) psychology. A psychological mainstream is defined that is concerned with affirming its position in society and supporting existing societal power structures. Thus, in a nutshell, critical psychology is the critique of this uncritical mainstream.

Thinking of the psychological mainstream as affirmative and uncritical might be problematic, however. Some of the most famous experiments from within the psychological "mainstream" are, in fact, critical: Solomon Asch's conformity experiments showed the effects of group pressure (Asch, 1956), Stanley Milgram's obedience/authority studies investigated the extent to which strict obedience to orders given by an authority was not a mere German phenomenon, but a "human" one (Milgram, 1974), Bibb Latané's experiments about the diffusion of responsibility were motivated by the 1964 killing of a young woman in New York, which was witnessed by 38 people, without anyone intervening (Latané & Darley, 1970). All these studies were critical of a societal status quo. At the same time, they certainly

belong to the realm of "psychological mainstream" as it is constructed from a "critical perspective."

Given the fact that there is a large number of psychological approaches that refer to themselves as "critical" and, at the same time, a probably even larger number of research projects that are "critical" according to the definition but would not necessarily characterize themselves as such – and would not be termed "critical" by "critical psychologists," – the term "critical," as it is used in the arena of critical psychology, seems to be the kind of boundary condition for a "consensus group" as described by Valsiner (Valsiner, 2012, p. 164).

Moreover, research (or "science" in the meaning of "Wissenschaft") is critical as a Conditio sine qua non: psychoanalysis is critical of behaviorism, idiographic approaches are critical of nomothetic research, neuro-psychological concepts are critical of socio-psychological explanations, and so on. In short: *Wissenschaft* as the producer of "new knowledge," is always critical in that this "new knowledge" either contradicts or at least questions previously existing beliefs.

Thus, the meaning of "critical" in the tradition of German-Scandinavian Critical Psychology must be different if it is not tautological or merely serves the construction of a consensus group. In German, this difference is marked by the distinction between "kritische Psychologie" and "Kritische Psychologie (KP)," the latter starting with a capital "K," thus often termed "Groß-K Psychologie" or "capital-K psychology." KP's understanding of critique derives from the concept of "critical science" (Kritische Wissenschaft) as it was developed in the Frankfurt School of Critical Theory. In the words of Max Horkheimer, critique.

> is a human activity which has society itself for its object. The aim of this activity is not simply to eliminate one or other abuse, for it regards such abuses as necessarily connected with the way in which the social structure is organized. Although it itself emerges from the social structure, its purpose is not, either in its conscious intention or in its objective significance, the better functioning of any element in the structure. On the contrary, it is suspicious of the very categories of better, useful, appropriate, productive, and valuable, as these are understood in the present order, and refuses to take them as nonscientific presuppositions about which one can do nothing. The individual as a rule must simply accept the basic conditions of his existence as given and strive to fulfill them; he [sic] finds his [sic] satisfaction and praise in accomplishing as well as he [sic] can the tasks connected with his [sic] place in society and in courageously doing his [sic] duty despite all the sharp criticism he [sic] may choose to exercise in particular matters. But the critical attitude of which we are speaking is wholly distrustful of the rules of conduct with which society as presently constituted provides each of its members. (Horkheimer, 1937, pp. 206–207).

KP is not critical in a manner that it takes a certain societal problem as its starting point (as, for example, in Latané's study). Instead of trying to establish psychological dispositions leading to the bystander effect, critique in this sense would ask about the concrete societal conditions in which such a situation is produced. Research and intervention would try to tackle these conditions instead of creating stabilizing factors that would explain such effects according to psychological dispositions. Similarly, "categories of better, useful, appropriate, productive, and valuable" are not ignored as unscientific (in a positivist understanding of "neutral" science); nor are they taken for granted. Instead, they become part of the

investigation. The description of a situation as "better, useful, appropriate…" would, in this understanding of critique, always raise the question of "Better, useful, appropriate… for whom?."

Against this background, Critical Psychology cannot rest on a critique of one or another form of mainstream psychology, although a critique of what he termed *Variablenpsychologie* (psychology of variables) played a role. Holzkamp imagined KP to become a new psychology that rests upon Critical Theory and Marxism.

The Development of German-Scandinavian Critical Psychology

As I mentioned in previous chapters, the two world wars influenced the turn from experimental to applied psychology in the United States and the UK. In Germany, this turn began to take hold of psychology at a later stage. After World War II, psychologists continued where they had left off during the war. To be sure, Nazi Germany invested in psychology and psychological diagnostics, including new fields of psychological research such as the "Wehrmachtspsychologie" (Geuter, 2008). At the same time, the Law for the Restoration of Professional Civil Service from April 7, 1933 () forced all Jewish professors to retire. In psychology, five out of fifteen professors had to retire: Adhemar Gelb, David Katz, Wilhelm Peters, William Stern, and Max Wertheimer (Lück, 2009). As German academia is traditionally organized in the form of "Lehrstühle," one professor typically heads a whole group of researchers working for them – and bound to the professor's fate – often constituting a "school of thought." Werner Traxel describes the general sentiment of psychologists in Germany after the war. In contrast to their American or British counterparts, German psychologists were largely opposed to experimental psychology. The tradition was seen to be a failure of the past. Only toward the end of the 1950s and under the influence of American psychology did experimental psychology return to German universities (Traxel, 1985, p. 105). With the Sputnik Crisis in 1957, *Bildungskatastrophe* (educational disaster) became a buzzword in reference to the German educational system (Picht, 1964), and psychology as a discipline found itself at the forefront of an educational reform pushed by the government. Increasing financial support for the universities combined with a rising number of enrollments and the first generation of German students born after the war helped unfold the student protests of the late 1960s. The "Brown [as the color of the NSDAP] Ivory Tower" was contested, and a more democratic and more political university was demanded (Leibfried, 1967). It was within this framework of critique that Kritische Psychologie emerged.

To understand the concrete interplay of the societal (*gesellschaftlich*) circumstances in West Germany at the time and the emergence of KP, it is helpful to shed some light on Klaus Holzkamp's personal development. Holzkamp underwent a gradual process of development, which culminated with the abandonment of what

he called traditional psychology (*überkommene Psychologie*). Holzkamp enrolled at Freie Universität Berlin as a psychology student in the summer of 1949, only one semester after the psychology department was founded by Oswald Kroh (Retter, 2001, p. 44). He was granted a doctoral degree in 1957 for an experimental study in Jasperian *Ausdruckspsychologie* (Bianchi, 1956, p. 263). In 1967, Holzkamp was appointed Professor of Social Psychology at Freie Universität after publishing two monographs (Holzkamp, 1964; Holzkamp, 1968). Both books investigated the interplay of theory and experiment in psychology. Their central purpose was not to disprove experiments in any way. On the contrary, Holzkamp believed that "the experiment constitutes a fully legitimate … and irreplaceable method with respect to the foundations of psychology (*grundlagenwissenschaftlichen Psychologie*)" (Holzkamp, 1964, pp. 4, my translation). The problem Holzkamp identified he termed *Repräsentanzproblem* (problem of representation): "While the theoretical clause expresses statements about a 'theoretical reality' in line with the coherency of the theory, the experimental clause is an immediate verbal statement of what is practically done in the experimental act" (Holzkamp, 1964, pp. 28, my translation). The experimental clause and the theoretical clause "cannot be unequivocally ascribed to one another in one or the other direction" (Holzkamp, 1964, pp. 270, my translation). An infinite number of experimental clauses can be ascribed to a theoretical clause. Put differently: How a hypothesis is operationalized for the purpose of an experiment cannot be logically deduced from theory. And because this relationship also holds true in the opposite direction, Holzkamp concludes that experimental results must be, in principle, ambiguous. It is the main effort of both books to develop a set of principles through which this problem of representation can be minimized. Both books were well-received within the scientific community and established Holzkamp as an important methodologist. However, the actual impact on the day-to-day business of experimentation was low. Symptomatic for this state is perhaps a letter from August 1965, sent to Holzkamp from Wolfgang Köhler, then at Dartmouth College:

> Your book should be understood as a call for self-reflection by all psychologists. At the same time, it serves as an introduction to this art. It seems to me that in this field your book is an unparalleled effort. … The only wish I have is that, above all, you bring forward a practical example of all the things you describe, for example, in the context of perception. I want to see how you yourself avoid all the mistakes you mention. (Köhler, 1987).

Holzkamp realized that Köhler was right: He had offered solutions to the problem of representation that were themselves caught in a representational problem; they were phrased theoretically although addressing practical problems. In the epilogue to the second edition of *Theorie und Experiment* Holzkamp writes:

> The boundaries and shortcomings of the book are visible wherever one tries to find a positive solution to the problem of representation, or more precisely: The failure of the book is that it believes in a solution to the problem of representation within the framework of the traditional variables schema of psychology. This belief is grounded in the constructionist understanding of the science underlying the whole argument. As such, it is an example of how far one can get and where one has to stop if one realizes, on the one hand, that human insight (Erkenntnis) is not contemplative but only achievable (zu gewinnen) in the framework of active intervention in reality, while on the other hand, one conceptualizes the

> researcher as an isolated individual who is facing a previously untouched reality, i.e. if one does not comprehend that insight (Erkenntnis) is one aspect of the objective (gegenständlich) appropriation of nature by means of societal labor in a historical process, which means that the individual is always facing a reality which was previously shaped and made identifiable by human labor. (Holzkamp, 1981, pp. 276–277, my translation).

Holzkamp felt that he had reached the boundaries of his discipline. The arbitrariness of the theoretical clause and the experimental clause does not allow insight (Erkenntnis) to be generated within the experimental setting.

Under the impression of the student movement, which successfully destabilized the hierarchical university system in Germany, Holzkamp decided to break with his prior work. A positive reception by the student movement was a supporting factor for his turn to a new psychology (cf. Markard, 2009, p. 37f).

The first article with a major impact on the student movement was published in 1970 (Holzkamp, 2009b) and based on a presentation given in 1968. Prior to the publication, the article had been distributed widely among the students in a collection of the so-called *Relevanzpapiere* (relevant papers). In this article, Holzkamp summarized the critique he had developed in "Theorie und Experiment" and "Wissenschaft als Handlung." But he added external relevance as another component to his critique of experimental psychology.

Holzkamp's critique of psychology resembles William James's critique before he turned from psychology to philosophy, as well as Lewin's critique, which led him to develop Topological Psychology. Holzkamp too assessed the state of psychology as a "multitude of micro-investigations, … where no human being can have an oversight of this aggregate, let alone ordering it or make sense of it" (Holzkamp, 2009b, pp. 16, my translation). Holzkamp did not regard this development, which gradually nullifies the external relevance of experiments, as accidental. For him, it was the logical consequence of the problem of inner relevance. Whatever happens in an experiment is not logically connected to the overarching theory; the researcher merely implies this connection. In contrast to Lewin, though, Holzkamp did not believe the solution to this problem could result from a Galilean turn that emulates physics since the very aim to be as exact as physics constituted, for him, a crucial part of the problem:

> The high degree of integration of theory in physics as a precondition for external relevance is, in substance, achieved through a unity-endowing subject matter (Gegenstand). The natural thing-world (Dingwelt) with which physics deals is, to a high degree, passive, ahistorical, and manipulable through consistent events. That is why physics was able to gradually develop instructions … through which the natural thing-world can be transformed into an artificial, physical world. (Holzkamp, 2009b, pp. 19, my translation)

Physics is successful because it is able to provide knowledge about the process of transforming naturally occurring phenomena, such as iron, stone, etc., into structures built by humans, such as a bridge, a house, etc. This is possible because of a specific subject–object relationship in physics; it is "based on an ontic, immediately given, and irreversible relationship." In psychology, in contrast, "the researcher, as well as their potential subject matter, are human subjects, i.e., individuals who exist

in and of themselves (*sind sich selbst gegeben*), to whom the world is given and who can actively produce an account of themselves and the world" (Holzkamp, 2009b, pp. 20, my translation). This relationship is not ontically given. Rather it is the product of "an agreement about specific constellations of social roles, which can be broken up or even reversed at any given point in time" (Holzkamp, 2009b, p. 21). The consequence for the experiment is that "the reactions of the test subject are not a direct consequence of the instructions; instead, the instructions attempt to modify the self-understanding (Selbstsicht) and world-view (Weltsicht) of the individual. … These viewpoints are … never fully given to the researcher" (Holzkamp, 2009b, pp. 21, my translation). Three distinct mechanisms achieve the modification of the subject's self-understanding and world-view:

1. Psychological studies or experiments are increasingly "parceled" to be able to create direct relationships between independent and dependent variables. To avoid confounding variables, independent and dependent variables are increasingly divided into sub-variables. (Holzkamp, 2009b, p. 22)
2. In the service of this "parcellation" and the avoidance of confounding variables, the phenomena under investigation are deprived of their real-world embeddedness. Holzkamp calls this a "reduction of the complex conditional structure (*Bedingungsgefüge*) of the real world," which usually results from the creation of an artificial situation outside of the participants' concrete life circumstances. For the most part, this artificiality is an effect of actual experiments and survey studies, but it applies to interview situations as well. The consequence for the subject is a reduced understanding of the research's purpose; it is impossible to discern the research project's relevance for 'real-life issues', and consequently difficult for the participant to create a connection to their self-understanding and worldview. The researcher takes the role of a guiding instance in this newly entered artificial realm and can shape the situation accordingly. (Holzkamp, 2009b)
3. "Destabilization (*Labilisierung*) of the stimulus situation:" The research participants are given as little information as possible about the research agenda and the context in which the study/experiment supposedly produces new knowledge. Concrete researcher instructions usually provide the only guiding post for the participants to create meaning and action. (Holzkamp, 2009b, p. 26–27)

Like Lewin, Holzkamp offers a critique of the subject as it is understood in psychology. Yet, a far-reaching difference exists between the two critiques. For Lewin, the problem was that of historic-geographical specific data that are averaged, thereby producing an individual "on average" that, as such, does not exist in the real world. The reason for its nonexistence is the simple fact that nobody is average in everything. For Lewin, then, all psychology needs to do, is to be more precise and exact and investigate the concrete individual in the concrete situation. Holzkamp would undoubtedly agree with this move. At the same time, his critique goes further than Lewin's. According to Holzkamp, the subject under investigation in a psychological study is constructed as what he calls a "norm-participant." Based on the idea that within a psychological study, there is a certain agreement about specific social

roles the participants and the researchers are, on principle, agreeing to take up, Holzkamp describes the norm-participant as follows:

> The norm-participant is an imagined person who behaves according to the previously determined agreement. This person consequently acts only according to whatever the research has 'put inside' this role – this counts for visible utterances and any hidden activities. A study is, then, designed successfully if it manages to eliminate or isolate any difference which exists between the concrete participant and this imagined norm participant. (Holzkamp, 2009a, p. 58)

While for Lewin, the problem with psychological studies consists in the inability to account for all the aspects that appear in the concrete experimental setting, Holzkamp goes one step further in arguing that the concrete agency for the participant is reduced in a way implied by the setup of the research situation. While for Lewin, research still investigates "real people" but fails to acknowledge all the factually appearing features of the person, Holzkamp believes that it is precisely these features that are reduced to a minimum. In other words: While for Lewin, it is still possible to create insights about human beings by means of psychological studies or experiments, Holzkamp is highly skeptical and claims that this very possibility is already taken away by how psychological studies and experiments are designed. Because experiments and studies only emulate real life, the subject is reduced to a mere "organism:"

> If we call beings Lebewesen who have a history and which can potentially be subjects of this history in a reflected manner and which can furthermore potentially create their own, not alienated world according to their needs and which can ultimately represent their own interests in a symmetrical and rational dialogue, if we call these beings human beings; and if we call beings which do not have a history and which are reduced to a certain number of behavioral reactions to specific stimuli, organisms; then we have to conclude that the concept of the norm-participant entails restrictive determinations that force beings, which have the potential to act like human beings outside of the experiment, to behave like organisms inside the experiment. (Holzkamp, 2009a, p. 61)

Because a psychological study is an artificial intervention, it deprives the participants of their history and the ability to consciously relate to that history. In the social world, they would use this ability, at least potentially, to organize the circumstances according to their own needs. Within the study or experiment, these circumstances are predetermined as the boundaries of the concrete study or experiment. To give some examples: A person who participates in a survey that uses the Likert scale adds a short explanatory note to one single item to explain why she cannot answer the item based on a scale ranging from "strongly agree" to "strongly disagree." A person in an autobiographical interview prefers to talk about her fiancé. A person in an experiment about self-perception refuses to order a number of photographic pictures that show her from various angles according to her personal liking because she cannot decide which image she prefers. All these situations would essentially violate the setup of the study or experiment. For Holzkamp, however, such behavior marks the expression of human potentiality to shape the world and, as such, represents a defining factor of the subject matter of psychology. In opposition to Lewin, Holzkamp would not argue that psychology shies away from the concrete

individual, rather it misconceptualizes the relationship between society and the individual:

> It is a characteristic of contemporary psychology that it specifies the particular individual (Einzelindividuum) as the "concrete" individual, which is set in contrast to concepts like "society" seen as a result of a generalized abstraction based on the behavior of "concrete" single individuals. Thereby, "society" is constructed as being merely imagined, with its only empirical foundation resting on the behavior of the single individual. (Holzkamp, 2009a, p. 108, my translation)

The consequence of this move is that societal issues are associated with the individual, and they can be solved simply by changing individual behavior. This understanding is one of the underlying notions of the STSD approach: By changing the individuals' behavior toward group work, the overall societal framework is supposed to change gradually. The change process would, ultimately, frame the whole of society as an aggregate of the behavior of single individuals. Much in difference to the latter approach, Holzkamp views the single individual as the abstraction, namely, an abstraction from "the concrete socio-historical situation of a human being" (Holzkamp, 2009a, p. 108 my translation).

From this perspective, Holzkamp reevaluates the external relevance of psychological experiments and studies. He realizes that this relevance is not effectively nullified but becomes merely "technical," (cf. Habermas, 1968b) because "the production of artificial and controlled situations can also be in the interest of everyday technical procedures" (Holzkamp, 2009b, p. 27). In this respect, the studies of Lewin and Rupp in the German textile industry, for example, carry technical relevance in that they experimentally explore certain modes of production, which are then applied to the real world as exact a reproduction as possible of the experimental setting. The same counts for the Norwegian ID-Project studies. The workers at Christiana Spigerverk took part in an experiment that should later be emulated as exactly as possible for the whole working process. The Search approach gives another example. Search conferences are, in effect, a practice ground for specific structures that are later applied to the company organization. In all these cases, not the experiment emulates reality, but reality mimics the experiment. Hence the experiment is only technically relevant as a kind of training arena driven by "an interest in gaining knowledge (*Erkenntnisinteresse*) about the technical domination (Verfügung) of objectified (*vergegenständlicht*) processes" (Habermas, 1968a, pp. 157, my translation). Holzkamp opposes the idea that this kind of transfer of a restricting setting from the experiment to the real world can be the goal of psychology. Instead, psychology should aim at emancipatory relevance:

> Psychological research would be relevant in an emancipatory way in so far as it would help to shed light upon the societal and social dependencies of the individual from the individual's perspective (Selbstaufklärung) and thus contribute to the improvement of the individual's situation by helping to remove these dependencies. (Holzkamp, 2009b, p. 37)

This distinction between technical relevance and emancipatory relevance is often taken as a distinction between the "good" and "bad" ends of research. For Holzkamp, it is not that simple. Research with technical relevance seems important in

general – given that all applied physics, for example, possesses technical relevance. Even in the realm of psychology, research with technical relevance is important. Holzkamp mentions the examples of "the concrete design of a workplace, the situation of an airplane pilot or car driver … etc." (Holzkamp, 2009b, pp. 27, my translation). However, he also concludes that psychological experiments and studies that tend to follow the traditional model fail to provide technical relevance in most cases, even if they imply doing so. Taking the example of intelligence tests, Holzkamp explains:

> If the practitioner wants to use such a test, he [sic] would, first of all, have to prove that the situations in which the test was validated provide sufficient structural similarities (Strukturähnlichkeiten) with the situations he [sic] is assessing according to the test results. Only in this way could the relevance of every possible test result be secured. Because the situation in which the test was validated is obviously radically different from the practical situation and because the practitioner, in most cases, does not have any methodological tools (Denkmittel) at hand to compare the structures, this is not how the practitioner proceeds. On the contrary, he [sic] simply behaves as if "validity" was a general characteristic of the test and thus regards the test results as "somehow" meaningful (aussagekräftig) for the situation he is confronted with. Based on this procedure, the assumption of the relevance of the test results seems illusory to the greatest extent. (Holzkamp, 2009b, pp. 32, my translation)

The focus on technical relevance might work well in physics, but it often seems to fail in psychology. This is because experiments in physics are created based on "the same principles" as "the artificially objective world-formation of everyday life" and thus necessarily provide a set of structural similarities. In psychology, this is not the case since the experiment or study works – as shown above – based on abstraction from the "historical and societal emergence [of the social world-form]" (Holzkamp, 2009b, pp. 30, my translation).

This leaves us with one crucial question: What results from the fact that psychological research is applied to a situation for which it is not relevant? Holzkamp answers that the mechanisms of psychological studies and experiments (partializing, reduction, destabilization) might be transferred to the real world, effectively eliminating the concrete historical becoming (*historische Gewordenheit*) of the situation and the individuals therein. Equally, their ability to consciously react to that history and the potentiality to organize the concrete circumstances according to their own needs would be hampered with (Holzkamp, 2009b, pp. 34–45). Holzkamp is convinced that this would produce a world that effectively resembles Herbert Marcuse's vision of a dystopian future. Marcuse identifies

> certain basic tendencies in contemporary industrial society which seem to indicate a new phase of civilization. These tendencies have engendered a mode of thought and behavior which undermines the very foundations of traditional culture. The chief characteristic of this new mode of thought and behavior is the repression of all values, aspirations, and ideas which cannot be defined in terms of the operations and attitudes validated by the prevailing forms of rationality. The consequence is the weakening and even the disappearance of all genuinely radical critique, the integration of all opposition in the established system. (Marcuse, 1964/2002, p. xii).

According to Holzkamp, "we have to ask ourselves whether psychological research should – unimpressed with these issues – continue on a path, which, potentially, transforms it to an instrument of the rulers' (*Herrschenden*) manipulative interests to control (*Kontrollinteresse*)" (Holzkamp, 2009b, p. 36). Of course, Holzkamp believes that this question is already answered. An entirely new psychology had to be envisioned, one which would carry emancipatory relevance in that it helps to realize the human potential to "shape the situation according to their needs" (Holzkamp, 2009b, p. 26).

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Chapter 8
Subjectivity and Democracy in the Tradition of German Critical Psychology

The Subject and Action As Central Categories: The Historical-Empirical Level of KP

The previous two sections aimed to show the specific understanding of critique that motivates *Kritische Psychologie* and the historical circumstances and theoretical developments that formed the concept. This section seeks to explain the concept of *Kritische Psychologie, focusing* on subjectivity, democracy, and action as they appear in Holzkamp's thought. As such, this section will not investigate a historical development but present the concept in its "final version," leaving the various debates through which the approach was formed to the side. A historical account of the development of Kritische Psychologie can be found, for example, in (Papadopoulos, 2009) and (Markard, 2009).

The previous section linked the endeavor of a different psychology to the alteration of the general methodological framework. As most favored by Holzkamp, experimental research would need to be replaced in favor of studying everyday life. To what extent, though, would such a shift unfold the kind of emancipatory relevance Holzkamp is trying to achieve?

Everyday life, for Holzkamp, is everything that is not a psychological experiment or a psychological study. To be sure, Holzkamp holds the former to be much richer and to carry more potential for action than the rigid setting of psychological research. However, emancipation, as Holzkamp describes it, does not necessarily follow.

Nonetheless, Holzkamp attempts to show that emancipatory potential is somehow part of the human condition. Again, he turns to history, the historical development of the concrete subject, and the societal circumstances in which the subject lives to illustrate this move. The historical approach is used to prove two basic assumptions. First, the concept of the subject is identified with the idea of the enlightenment as defined by Immanuel Kant:

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> Enlightenment is the human being's emancipation from its self-incurred immaturity. Immaturity is the inability to make use of one's intellect without the direction of another. This immaturity is self-incurred when its cause does not lie in a lack of intellect, but rather in a lack of resolve and courage to make use of one's intellect without the direction of another. (Kant et al., 2006, p. 17)

Holzkamp affirms that the concrete living conditions of a subject are self-made and can be transformed by one's own actions. To elaborate on this, Holzkamp differentiates between primary restrictions that are the cause of societal circumstances (such as the necessity of a certain educational degree to apply for certain jobs) or physical limits (such as the inability to be in two places at once) and secondary restrictions that appear to be naturalized conditions – i.e., external conditions one cannot influence – but that are, however, self-made. It is the primary task of psychology to uncover these secondary restrictions (cf. Holzkamp, 2009a, p. 39).

Such a historicizing mode of thought yields a certain perspective of contingency, a fundamental conviction that "it could also be different," i.e., a utopian moment. This is in line with the specific understanding of critique in KP: "It does not accept the status quo as the standard; it does not expunge the possible from what exists." (Markard, 2009, p. 67, my translation).

The Holzkampian endeavor to create a new psychology rests partly on the belief that there is "more" to being human than what is expressed and visible in everyday life; moreover, that it is possible to realize this "more" by means of concrete, psychological interventions.

Holzkamp spells out his new approach to psychology in his 1983 book "Foundations of Psychology (*Grundlegung der Psychologie*)" (Holzkamp, 1983). As mentioned above, his approach to psychology is historical. The concrete history he unfolds is that of the subject matter of psychology, which is defined as "*Das Psychische*" – the psyche. *Das Psychische*, for Holzkamp, refers to a broad range of phenomena that can be found not only in humans but, to a certain degree, in any life form. For example, Pavlov's experiments with dogs in which he showed the effects of conditional learning (Pavlov, 1927/2003) would necessitate a description in terms of *Das Psychische*. It is a psychic process that allows the dogs to learn to attribute the ringing bell signals to the arrival of food. Human beings, with their fully developed psychic life, are capable of much more complex ways of learning than dogs – "learning from meaning," for example, (Holzkamp, 1995). To be sure, under certain conditions, humans are also able to follow the path of conditional learning, which Holzkamp associates with an evolutionary idea of development: There is a connection between the psychic learning of the dog and that of a human being in that conditional learning in a dog marks a prior form of learning by meaning that takes place in humans. This evolutionary process proceeds dialectically in that the more developed version includes, the earlier one, i.e., more advanced forms of psychological processes include the more primitive ones instead of replacing them. This dialectical development is key to understanding Holzkamp's psychology: The quantitative culmination of new abilities eventually leads to a qualitative leap, which establishes a new qualitative level of development incorporated in the previous level (Holzkamp, 1983, p. 72, 1995, p. 283).

The basic idea behind the historical approach is that the human condition cannot simply be determined on an empirical level, that is, by observing human behavior. From this perspective, it would also not be sufficient to observe humans in interaction or the ontological and psychological development of a single human being. Such a development can only be understood if the human–world interconnection (*Mensch-Welt-Zusammenhang*) (Holzkamp, 1983, p. 187) is taken into account. This human–world interconnection is the product of a specific historical development that needs to be understood. Thus, a history that tries to discern the human condition needs to start at the very beginnings of the psychic life (*Das Psychische*) and show how it developed in its concrete interconnection with the world to arrive at the level of the human psyche in modern society. The starting point of such an endeavor is not a specific date in the past – since such a date would inevitably result from an arbitrary decision – but the concrete here and now. This here and now is the "manifest surface" (*Erscheinungsoberfläche*) or the "imagined concrete" (*Vorstellungskonkretum*), which supplies certain terms that can be used to describe the human condition, such as behavior, perception, attribute, anxiety, motivation, etc. (cf. Holzkamp, 1983, p. 51). These terms are, however, only "preliminary" (Holzkamp, 1983, p. 50). They must be taken through the historical process of their own emergence to clarify their meaning. This reconstruction aims to develop the basic terms – which Holzkamp calls "categories" – with which the human condition can be described. These categories then provide the basis for particularized theories about concrete empirical conditions.

Holzkamp's course of action seems complicated, maybe even unnecessary. Why not simply start at a certain point in the past at which the psychic life has reached a certain visible quality and go from there? There are two reasons for this. The first is mentioned above, namely, that such a date would be arbitrary. The second reason is that Holzkamp wants to understand the existing understanding of the psychic life, particularly the historical becoming of this understanding. And this is expressed in specific terms, which are nonetheless naturalized, i.e., deprived of their own historical becoming and thus render invisible the "sublated processes" from which they are constituted. These sublated processes are made visible by means of "showing the terminological form of the historicity which is buried in the present" (Holzkamp, 1983, pp. 51, my translation). The method Holzkamp applies here is that of historical materialism. Marx illustrates his method with the example of the term "population":

> The population is an abstraction if I leave out, for example, the classes of which it is composed. These classes are an empty phrase if I am not familiar with the elements on which they rest. E.g., wage labour, capital, etc. These latter presuppose exchange, division of labour, prices, etc. For example, capital is nothing without wage labour, without value, money, price etc. Thus, if I were to begin with the population, this would be a chaotic conception Vorstellung] of the whole, and I would then, by means of further determination, move analytically towards ever more simple concepts [Begriff], from the imagined concrete towards ever thinner abstractions until I had arrived at the simplest determinations. From there the journey would have to be retraced until I had finally arrived at the population again, but this time not as the chaotic conception of a whole, but as a rich totality of many determinations and relations. (Marx, 1978, p. 237)

The starting point of the historical reconstruction is the existing term, which is the concrete, i.e., the term "psyche" denominates the human psyche in its full concreteness. But it is only through the historical reconstruction of the emergence of that which the term denominates that this full concreteness can be understood.

According to this procedure, the psychic life (*Das Psychische*) as the fundamental category must fulfill the following standards: It must be the most general category of psychology, it must provide a distinction between psychic life and forms of life that are not psychic, and its basic characteristics must be found in all the more advanced forms of life, which rest upon this basic category.

Holzkamp shows this development of the psychic life in the first four chapters of his "Grundlegung der Psychologie." It is impossible – and for the purpose of this book also not necessary – to present the entire course of Holzkamp's reconstruction of the psychic life here. It is, however, essential to understand the methodological concept as spelled out above. In short, Holzkamp analyzes the emergence of the psychic life from the beginnings of life to socialization (*Vergesellschaftung*) of human beings on a functional-historical level, i.e., the accumulation of specific biological functions eventually leads to a combination of these in a new qualitative level from which a further accumulation of functions starts. All this is seen as an evolutionary process of phylogenetic developments at the end of which the "societal nature of humankind" unfolds (Holzkamp, 1983, chap. 2–4). It is essential, however, that socialization, i.e., the fact that human beings are inherently social beings, takes place on the phylogenetic level. This means, for Holzkamp, that socialization itself is not the product of a socio-historical process but belongs to the realm of phylogenesis: Human beings are inherently social. With the socialization of the human species, a qualitatively new level is reached at which evolutionary processes are no longer the primary factor for the development of the psychic life. The socio-historical development now occupies this position. From this point, the development of the individual is not determined by evolutionary processes but by the societal mediatedness of individual existence (*Gesamtgesellschaftliche Vermitteltheit individueller Existenz*) (Holzkamp, 1983, p. 144ff). This mediatedness reflects a "doubled relationship" between the individual to society (Holzkamp, 1983, p. 192). The individual's concrete life circumstances are shaped by societal production – not solely by their own doing – while this individual, at the same time, participates in this production process. It is the critical factor of socialized living conditions that these two elements "drift apart and become independent from each other" because "the produced goods become, in principle, available to all members of the society without regard of the fact whether the concrete individual was part of the concrete production process" (Holzkamp, 1983, p. 192). As such, the socialization of humankind represents a liberating fact compared to the previous stage at which every individual was concerned with their personal survival. The individual is consequently not reduced to the necessities of securing their individual existence but can use the societally produced circumstances to survive, i.e., they can focus on specific tasks while benefiting from the work of others to ensure their survival.

To differentiate between the activities that reproduce the societal structures and the broader connections of which an individual becomes part, Holzkamp introduces

the terms "position" and "life situation." The position is the "epitome of different and necessary labor tasks" for societal reproduction. It captures the concrete "labor division in a society and the possibilities for individual life-sustainment" (Holzkamp, 1983, p. 196) offered by this labor division. And life-situation "denominates the concrete circumstances from the standpoint of the subject … in so far and inasmuch as it stands in direct contact with these circumstances" (Holzkamp, 1983, p. 197). The concept of life circumstances as such includes the concept of position. It moves beyond it in that it also consists of all other activities of a human being, which in principle, of course, also reproduce the society but are at the same time not necessary to secure survival.

The distinction between the two terms also marks a different analytical angle. The position of a subject is analyzed from the perspective of society, i.e., the determining factor is the location of a person in the societal reproduction process, for example, a university professor, janitor, unemployed person, American president, etc. The life circumstances are analyzed from the subject's position, i.e., what does it mean to be this concrete university professor, janitor, unemployed person, American president, and what other concrete activities are included in this life. This differentiation is important on two levels. First, it provides an analytical tool in empirical research (discussed in more detail below), which allows capturing the subject from two analytical positions, i.e., its objective position in the societal reproduction process and the subjective life circumstances that determine how this objective position is dealt with. Secondly, it prevents the concept from becoming deterministic, thus removing the liberating factor that was proposed to be included in the socialization of humankind. To a certain extent, the individual is determined by society with respect to its position – i.e., its concrete participation in the reproduction process of society as a whole. But it is free, in principle, to modify its life situation, engage in activities that do not directly contribute to the reproduction of societal structures, or change its mode of participation. Of course, depending on the concrete societal structures, different restrictions apply – as will be discussed below – but that does not mean that the individual is fully determined by society; it always can act consciously toward these restrictions. In other words: The relationship between the individual and society becomes a possibility relationship (*Möglichkeitsbeziehung*) in which societal necessities becomes subjective agency the subject can consciously relate to. She can make decisions about her own actions. Again, for Holzkamp, this ability to relate to one's situation consciously (Bewußtes Verhalten-Zu Holzkamp, 1983, p. 237) is not a mere accident; it is a necessary consequence of the socialization of humankind because socialization entails labor division and labor division entails a gradual development of choice, i.e., a reflection about one's position within the societal framework and a realization that this position can, in principle, be changed. For two reasons, this principal space of free movement of the subject cannot be taken away completely by any kind of societal restriction imposed upon the individual. First, this principal ability to choose originates in the phylogenetic development; it is inherent to the human species (Holzkamp, 1983, p. 352). Second, the possibility relationship, as introduced above, inhabits a double character defined as "twofold agency" (Doppelte Möglichkeit

Holzkamp, 1983, p. 352). This twofold agency is conceptualized in line with the differentiation of position and life circumstances. The accumulation of all societal positions explains the concrete necessities for societal reproduction. Yet, from their standpoint of concrete life circumstances, the subject is free – in principle – to choose if and where it wants to participate. This creates a contradiction between individual choice and societal needs (as the driving factor of societal dynamics as an interplay of global needs and local action) and thus necessarily implies certain restrictions of action for individual subjects (for instance, in a capitalist society, labor provides money with which other activities can be financed, but without participation in the labor market, restrictions are imposed on the subject). These restrictions, however, do not determine the subjects' actions in their concrete life circumstances. They merely become premises in the concrete life circumstance. What does this mean? Restrictions must again be analyzed from two sides. Their first dimension is societal in that it determines the position of a person (i.e., with a job, without a job, high salary, low salary, etc.). Their second dimension is subjective in that it places the subject in its concrete life circumstance. On this basis, objective restrictions become subjective premises for action, i.e., not having a job can be devastating for a person, which means that it creates the premise "to find a new job," while for another person being without a job can unfold a feeling of "freedom" creating a premise of "finally doing what I always wanted to do" etc. In other words, an individual's concrete actions cannot be understood from the person's position within society because the position is not a determining factor for actions. However, it is, at the same time, obviously, not irrelevant. Rather it creates specific premises, that is, in principle, explainable plateaus from which the subjective meaning of the concrete situation is derived. Premises serve as the subjective reasoning for concrete actions: "The different and often contradicting manifestations of the psychic life thus stems from the difference or contradiction of the premises on which the individual reasoning rests" (Holzkamp, 1983, p. 352). Premises are the subjective interpretation of objective agency or restrictions of action on the societal level. They take into account the "personal sensitivities" (individuelle Befindlichkeit Holzkamp, 1983, p. 353) of the subject from which the net of personal meaning unfolds (i.e., "I cannot take this job they offered me because I would have to move to the West Coast and thus wouldn't be able to go to the Red Sox Games anymore," etc.).

At this point, the relationship between societal restriction and personal action is already implied. If the societal restriction is not a determining factor of personal life circumstances, then the subject can consciously relate to those restrictions by forming personal premises for action. If the premises are not mere subjective interpretations of objective restrictions of a person's position, then the person must also have the conditions of agency at her disposal (Handlungsfähigkeitsbedingungen Holzkamp, 1983, p. 354). This is the second character of the twofold agency, i.e., instead of simply following the concrete restriction ("I cannot take the job because of the Red Sox"), the person can consciously relate to the concrete restrictions (for example, "I'll drop the Red Sox and become a Dodgers fan," or "I'll find a Red Sox fan club in L.A. which buys group tickets to home games so I can fly back now and

then," or "I'll negotiate a higher salary to partially cover the costs for my trips," etc.). It might be helpful to demonstrate this twofold agency with another example taken from Jonathan Culler (Culler, 1982, p. 124). In some American airports, signs are posted at the security check that state: "All remarks concerning bombs and weapons will be taken seriously." The sign precludes the possibility of jokingly saying, "I have a bomb in my shoe." – thus restricting the possibility of "making fun" of the security check idea as a whole, which should "be taken seriously." This restriction could be addressed by interrupting my conversation about the construction of bombs with my physicist friend while going through the security check, i.e., acting in accordance with the existing agency. At the same time, a statement like "If I were to remark that I had a bomb in my shoe, you would have to take it seriously, wouldn't you?" would again ridicule the whole security check process while at the same time circumventing, that is disabling the concrete restriction of action because a conversation about "bombs" would have effectively taken place. Culler goes on to explain: "A metasign, 'All remarks about bombs and weapons, including remarks about remarks about bombs and weapons, will be taken seriously,' would escalate the struggle without arresting it, engendering the possibility of obnoxious remarks about this sign about remarks" (Culler, 1982, p. 125).

Hence, concrete restrictions cannot only be circumvented and effectively nullified by consciously approaching these restrictions, but new possibilities also emerge. The subject is, in principle, always one step ahead of every restriction (Holzkamp, 1983, p. 355).

In concluding these thoughts, Holzkamp realizes that a new central category had emerged, a new basic need of humans under the condition of societal mediatedness, which is different from what he had adopted from Marx. Marx's analysis only considered the subjects' position and excluded the concrete life circumstances. As an effect, he had to conclude that "labor" was the central defining category of humankind. With the inclusion of concrete life circumstances, Holzkamp can explicate the following:

> Not 'labor' as such is the primary life-need (Lebensbedürfnis), but 'labor' only in so far, as it secures the participation in the determination (Verfügung) of the societal process for the individual in that it gives the individual agency (Handlungsfähigkeit). Hence, not 'labor' but agency is the primary human life-need—this is the case because agency is the most general framework-quality (Rahmenqualität) of a human and humane Dasein. In contrast, a lack of agency (Handlungsunfähigkeit) represents the most general quality of human misery and dependence (Ausgeliefertheit), anxiety, bondage, and degradation. (Holzkamp, 1983, p. 243)

To this point, Holzkamp's analysis rested on a level of abstraction from concrete societal circumstances, i.e., the goal was to show how the human condition unfolds under the assumption of a societally mediated existence of individual life. The central categories to specify the human condition were *agency), personal sensitivities, twofold agency, consciously related conduct, position, and life circumstance*. In further concretizing his analysis, Holzkamp analyzes the human being in capitalism as the most dominant form of social order in the world today. To be sure, capitalism as a framework is an abstraction from the concrete social order of concrete cultures.

However, the goal is, as before, to analytically move from the abstract to the concrete to further specify the initial concept on an actual-empirical level.

One of the key elements of a capitalist order that Holzkamp emphasizes is an increase in labor division. Labor division has been a defining factor of the socialization of humankind in general in that it freed the individual from the necessary production of goods for their survival. In capitalism, however, societal complexity and labor division have reached a stage at which the subject, from their standpoint of individual life circumstances, cannot oversee the societal process as a whole. The subject only recognizes society insofar as they stand in contact with it in their life circumstance. At this point, agency for the individual is no longer defined on a societal level but becomes the product of what Holzkamp calls "infrastructures" (Holzkamp, 1983, p. 359), i.e., a network of connections with others. These networks create their boundaries and rules in the process of generalization of the concrete other. This generalized other becomes a "*man*:" … "that *man* does something encloses in its societal meaning immediately that 'I too" have to do this" as well (Holzkamp, 1983, p. 360). In other words, the principal freedom given by the twofold agency on the level of societal mediatedness of individual existence is effectively reduced to a smaller space of possible action, which depend on the concrete infrastructure a person in its concrete life circumstances is a part. The concrete agency is reduced to a specific "space of possibilities" (Holzkamp, 1983, p. 368) with the generalized other at its boundaries. The role of these boundaries and the role of these infrastructure networks, i.e., whether they play a confining role, effectively reducing the individual to a singular entity that is confined to the process of securing its own survival (i.e., the single parent who works three jobs to earn the required income to cover the monthly costs), or whether these networks effectively produce a collective sphere of interaction and learning for the individual to realize the societal mediatedness of its existence and engage by means of a collective amplification of agency for the whole group, is a question of local theories based on actual-empirical research.

To be able to differentiate agency in the actual-empirical situation, Holzkamp distinguishes between "restrictive agency" (Holzkamp, 1983, p. 370) and "generalized agency" (Holzkamp, 1983, p. 383). This distinction stems from the concept of a "twofold agency," as discussed above. Restrictive agency thus denominates the subjectively given possibilities of action smay they stem from societal structures, the generalized other within the infrastructure, or physical and biological limitations. Generalized agency can be understood as the possibilities that unfold in the discussion and transformation of the concrete restrictions, the circumvention of the restriction to talk about bombs, as mentioned in the above example.

On this basis, and given Holzkamp's definition of agency as the primary human life-need, the question arises as to why people so often follow the restrictive agency instead of taking up the generalized form. For Holzkamp, engaging in generalized agency precludes the restrictive one, i.e., they stand in an either/or relationship. Thus, taking up generalized agency produces the threat of losing a specific form of security provided by the infrastructural network of restrictive agency. To return to the examples: To say, "If I were to remark that I had a bomb in my shoe, you would

have to take it seriously, wouldn't you?" can effectively ridicule the sign that tries to rule out any jokes about bombs. It could, however, also be interpreted by the authorities as an actual remark about bombs making the person subject to intensified search or even exclusion from the flight. The decision to take up the new job in L.A. despite the Red Sox addiction could lead to frustration and misery, for example, because there is no Red Sox fan club in L.A. with members the person actually likes, or the new job is so busy that there is simply no time to fly to the East Coast, etc. In other words: The extension of agency at the subject's disposal by engaging in generalized agency can only be functional for the subject if the risk of losing agency that provides exit options from the given infrastructure is eliminated or at least reduced. Thus, for Holzkamp, the only way to effectively realize generalized agency is to take the whole infrastructure to a new level to fully realize new possibilities. "That means to create an oppositional power by connecting with others in concrete cooperation which is strong enough to counter the individual dangers of losing existing agency" (Holzkamp, 1983, p. 373), for example, to involve everyone waiting in line at the security check in a conversion about the ridiculousness of security checks in general or the "remarks about bombs sign" in particular; or the fan club which buys group tickets already mentioned above. This implies

> a subjective perspective of the realizability of such possibilities by exceeding individual subjectivity through concrete cooperation that is directed towards the interspersion (Durchsetzung) of generalized interests of conjoint self-determination against prevailing partial interests, i.e., a firmly 'intersubjective' relationship as a hallmark of collective, that is societal, subjectivity. (Holzkamp, 1983, pp. 373, my translation)

From this framework of intersubjectivity, it is possible to spell out the concrete relationship of two subjects to one another, i.e., the relationship of subject and concrete other in which subjectivity is enacted:

> 'Consciously related action' is, as such, always 'my action.' Consciousness is always a matter of the first-person perspective. … The other is not merely a 'social tool' in the collective achievement of goals; he is also not merely a 'communication partner' with whom I plan and control the efforts of creating collective life conditions…. The other is, in fact, the source of insight (Erkennen) and the consciously related action in that he 'resembles me' (mir gleicht). Herein lies the … fundamental characteristic of 'subjectivity' and 'intersubjectivity:' I realize the other as a coequal but different 'center of intentionality' in his [sic] relationship to societal agency and therein to himself [sic]. (Holzkamp, 1983, pp. 237-238, my translation)

From here, a fundamental difference between the accounts of Action Research and Holzkamp's concept becomes apparent. For Lewin, the smallest analytical entity is the group, which is different from the singular subject, i.e., it has other qualities than the accumulation of qualities of the subjects the group consists of. For STSD, the semi-autonomous group is the smallest analytical entity in that it can unfold democratic structures and a degree of flexibility that is not achievable for the singular subject. In the dialogical approaches, the discourse unit is the smallest analytical entity because only in such a unit can action unfold in the form of a specific language configuration. In the Southern Tradition, subjectivity refers to an authentic understanding of the pure self, best expressed by and through

traditions inherited from the past that can liberate the alienated subject of the present. For Holzkamp, subjectivity is defined as a dual relationship of me/other, which invests subjectivity; it allows for subjectivity in the first place. Groups have a different quality for Holzkamp as well, but only insofar as they act together in "creating collective life conditions." The subject and its "relationship to societal agency and therein to himself [sic]," however, remains the unconditional analytical category from which the analysis of a group takes off: "It counts for collective forms of consciousness, or that is to say interindividual-societal subjects, that these are coalitions based on shared objective interests, etc., which are nonetheless given to the subject as 'my' interests, which I share with others" (Holzkamp, 1983, pp. 238, my translation).

What is the role of this analysis in relation to empirical research? This question has two answers. First, the above describes what Holzkamp regards as empirical research, more precisely, research on a historical-empirical level. It investigates the development of the psychic life from the beginnings of life to today's capitalist society. It becomes apparent that Critical Psychology in Holzkamp's fashion is an inherently interdisciplinary endeavor. However, interdisciplinarity is not defined as combining knowledge of various disciplines to achieve a more complete picture of reality. Holzkamp is well aware of including disciplinary boundaries in the concept of interdisciplinarity. Thus, he develops a model of cooperation on four levels (Holzkamp, 1983, pp. 27–29; Markard, 2009, p. 105):

(a) The philosophical level.
(b) The level of social theory.
(c) The level of categories.
(d) The local theory level.

The philosophical level is that of materialism, and the social theory is that of Marx. The level of categories refers to the historical-empirical process as spelled out above, and the local theory level refers to the level of actual-empirical research. Technically, the level of categories has two sublevels: The categories Holzkamp advances for the development prior to the socialization of humankind (i.e., the existence of a psychic life, the human being as social) are taken as "facts," which determine the subject in the strong sense – only to be revised by new discoveries in the form of empirical data. The categories developed on the level of the socialization of humankind are defined as "mediation-categories" (Vermittlungskategorien Holzkamp, 1983, p. 356) because they are used to discern the meaning of objective conditions for the subject, instead of specifying certain attributes of the subject.

The levels are mutually exclusive in that problems on one level cannot be solved on another level. However, unresolvable conflicts on a lower level can be the consequence of a problem on a higher level, which must then be tackled at that higher level. The task of level (d) as the level of actual-empirical research is to show how the categories developed in the historical-empirical process are reflected in the concrete empirical situation. I will turn to this level in the following section.

The Actual-Empirical Level: Research from the Standpoint of the Subject

The actual-empirical level is the empirical level of Kritische Psychologie in the strong – or one might say "common" – sense, i.e., research with actual human beings. The goal is to understand the interrelatedness of life circumstances and subjectivity in the concrete case. Most significantly, it investigates the conditions a human being deals with in everyday life, the subjective meaning of these conditions, and how these conditions are transformed into premises of action. As such, this procedure is called "condition-meaning-reasoning analysis" (Bedingungs-Bedeutungs-Begründungsanalyse Markard, 2009, p. 268). While in Holzkamp's hierarchical model of levels of discussions, the construction, in principle, goes from the top to the bottom, i.e., from philosophy to social theory, this process is turned upside down on the actual-empirical level, i.e., the research process does not start with the categories to specify them for the concrete case, but the starting point is the concrete case from which, by way of a process of abstraction, the relevant categories are "reached."

The basic premise of this condition-meaning-reasoning analysis is that there is no action without reason. Irrational behavior does not exist in *Kritische Psychologie*. Human behavior always is consciously related action. That action is always reasonable does not preclude the possibility of appearing irrational to others. At the same time, from the subject's standpoint, there is always a reason for action based on subjective premises. Morus Markard usually exemplifies the absence of irrationality in KP with his famous (and fictitious) IKEA example – an example that Markard uses not to banalize the research topics of Kritische Psychologie (the KP approach has been used to research such topics as drug addiction (Vandreier, 2010), labor unionism in the United States (Schmalstieg, 2010), or racism (Laisney, 2008), among others), but to have a manageable case at hand from which the approach can be explained:

> A woman buys a new wardrobe at IKEA, which she manages to assemble in her sleeping room. However, the wardrobe collapses as soon as the city cable car passes her house. The woman gives it a second try, but again, as soon as the cable car passes the house, the wardrobe collapses. A friendly IKEA employee pays the woman a visit and takes care of the assembly. Once he is done, the two decide that the IKEA employee should wait for the next cable car to pass by – inside the wardrobe with a flashlight to be able to see what is really going on – to witness the possible collapse with his own eyes. While the woman leaves to get a beer for the friendly IKEA employee …, her husband comes home, which is unexpected at that time of the day. In the sleeping room, the husband sees the new wardrobe and opens it: "What are you doing here?" he asks the stranger inside the wardrobe. The stranger replies: "I am waiting for the cable car." (Markard, 2009, pp. 189, my translation).

For the reader of this humorous episode, who has witnessed the whole situation from its emergence to its end, its concrete meanings and premises are comprehendible. The husband is, however, doomed to believe that he has some lunatic – and possibly a rival in love – in his sleeping room. Without an intersubjective breakdown, (Aufschlüsselung Holzkamp, 1983, p. 356) of the concrete premises, this

situation seems unresolvable (at least in a cooperative way). From a research perspective, the concrete facts of the situation can be used as empirical data – for example, the narrative account of the woman, a recording of the telephone call the woman made to IKEA, etc. – to spell out the concrete premises in the situation. As seen from this example, the decisive character of data in KP is not to prove or disprove a hypothesis or theory but to visualize and concretize existing premises–reasoning relations. The existence of premises and reasonings for actions in the context is a given, and irrationality is not a condition inherent in certain actions but merely an assessment from the outside, articulating a lack of understanding of the premises and reasons of the acting subject.

Two more important factors figure as significant illustrations, specifically if we imagine the episode as a research situation for KP: First, the subjects involved, or "participants" – as they would be termed in the traditional language of empirical research, are ultimately concerned with the situation, i.e., it is their problem. As such, in KP, they are termed "persons concerned," (Betroffene Holzkamp, 1983, p. 531) instead of "participants." Secondly, the persons concerned are key for the research process not simply as the conductors of a certain action or as the resources for some form of generalized knowledge but with their full knowledge about their concrete subjective standpoints. The resolution of the situation – in the interest of the persons concerned – in a condition-meaning-reasoning analysis can only succeed with their participation. Thus, their role is qualified as "co-researchers." This does not mean that co-researchers and researchers are simply thought of as the same; it merely refers to the absence of a hierarchical structure between the two. Their relationship can best be described as complementary, i.e., the researcher brings a certain knowledge to the field, i.e., theory and research results are typically not readily available to the co-researchers.

On the other hand, the co-researchers contribute essential knowledge about their concrete life circumstances, without which KP research would not be possible in the first place. This complementary relationship also includes a necessity of reciprocal education: the researchers have to learn about the life circumstances of the persons concerned insofar as is relevant to the research project. The co-researchers have to learn about theory and research results within the limits of the concrete case – this does not preclude that the co-researchers can contribute knowledge from academia to the field and that the researchers have to reflect on their own life circumstances to uncover institutional restrictions. Holzkamp characterizes the relationship between researchers and co-researchers as that of "meta-subjectivity":

> The framework of communicative understanding (Verständigungsrahmen) itself must – by means of the research process – be pushed towards scientific (wissenschaftliche) reliability/validity/generalizability of the research results, … i.e., to a level of scientific (wissenschaftlicher) meta-subjectivity, which includes and at the same time supersedes the intersubjective relationship of researchers and persons concerned. (Holzkamp, 1983, p. 541).

As such, KP does not operate from the perspective of a conditional discourse (Bedingtheitsdiskurs, Markard, 2009, p. 274), i.e., the question is not how specific conditions affect people. Instead, KP operates from the perspective of the reasoning

discourse, (Begründungsdiskurs, Markard, 2009, p. 274) to uncover the meaning certain conditions unfold for individuals.

This reasoning discourse should not be confused with the "discursive formations" or the "sub-discourses" in the Search approach. While the latter are denominators of a more or less specified "language" in use, i.e., a particular way of talking to each other, the reasoning discourse in KP is clearly defined as the way in which subjects interpret objective life conditions as subjective premises from which they attribute meaning to their actions.

Holzkamp summarizes the above as follows:

> [In KP research], the subjects concerned are enabled to penetrate (durchdringen) their subjective sensitivities by usurping (Aneignung) the KP categories to reach the level of meta-subjective generalizability. … This also means that the … actual-empirical methods become methods in the hands of the persons concerned. (Holzkamp, 1983, pp. 543, my translation).

From here, the direction of actual-empirical research takes shape: The goal is to extend agency in terms of generalized agency because

> only in the practical attempt to realize new possibilities can the actual-historically given objective and psychological idiosyncrasies (Besonderungen) and restrictions be made empirically perceptible (erfahrbar) in the face of the resistive reality – which includes the meta-subjective arguability (Diskutierbarkeit) of the specific means which are applied to overcome the restrictions. (Holzkamp, 1983, pp. 562, my translation).

In this context, Holzkamp speaks of KP research as a "controlled exemplary praxis" (Holzkamp, 2009b, pp. 136, my translation) because the means to overcome restrictions as well as the concrete life circumstances in which these means were applied are to be described in a meta-subjective discourse, which in principle, enables everyone to usurp these descriptions for their own conditions and apply the means if they see fit.

At this point, it could be argued that KP resembles the STSD approach in that STSD tries to create examples of successful change, which would, in their understanding, naturally trigger a change in other parts of the (Norwegian) industry. As we have seen, this triggering effect did not occur. However, there is a fundamental difference between STSD and KP in that the STSD approach did not only provide a specific – and very limited – set of means but also predefined the concrete ends to be achieved by those means, let alone the fact that the approach does not operate on a level of the standpoint of the subject. In KP, meta-subjectivity aims at making the results of a research project communicable beyond the initial project. Thereby it attempts to translate the possibilities into academic and social discourses. Whether or not people would take over the means spelled out in a project for their own lives is a matter of personal decision and interest, i.e., depends on their subjectively experienced life circumstances. Unlike STSD, KP does not advance an imperative to apply or use their concrete results; they merely become – in the ideal case – new possibilities for others in similar situations.

In addition, the extension of agency – while being the general orientation of KP research – is not the primary goal, according to Holzkamp. Instead, it is an effect of

the necessity to try out how new possibilities play out "in the face of the resistive reality" (Holzkamp, 1983, pp. 562, my translation).

A prototypical design of KP research on the actual-empirical level was introduced by Markard (Markard, 1985b; Markard, 1985a). It proceeds in four steps.

1. Problem constellation: The problem the research will be concerned with is explicated. This is a difficult task since the verbal identification of a problem already entails a certain abstraction from the concrete life circumstances, for it includes certain aspects while excluding others, which might nonetheless play a certain role in the problem constellation. This first level also includes the assumptions that the persons concerned have already applied certain attempts to solve the problem – which, however, failed or caused other, more severe problems since this is the only way in which a problem can actually be identified as such. It is, therefore, also possible that, over the course of the research, the initial problem turns out to be a secondary issue caused by a failed attempt to solve a different underlying problem. Hence, the assumption behind this first level is that "problematic entrapments are functional in a restrictive way, i.e., the ways in which the persons concerned try to solve their problems are counterproductive … but nonetheless realized as being without alternative (*alternativlos*)" (Markard, 2009, p. 282). This does not mean that people, in general, cannot solve problems; it merely reflects that there are problems that are not solved despite attempts to do so and that these problems are most likely not singular entities but embedded in a problem constellation. This problem constellation might, in part, be caused by the very attempts at problem-solving, including the possibility that the initial problem of the research process is, in fact, the consequence of a previous attempt to solve another problem – which might, in the eyes of the persons concerned to be regarded as solved but over the course of the research process be identified as the actual cause of the problem constellation.
2. Rephrasing the problem in terms of premises–reasons connections: This second level entails the discussion of the categorial foundations of the beliefs of the researchers and co-researchers with respect to the problem constellation – this also entails a readiness to question one's own fundamental beliefs. The primary goal is to explicate all participants' subjective points of view. At this stage, researchers and co-researchers do not simply discuss the problem constellation to utter their personal points of view. Rather, the goal is to introduce and analyze as many data points as possible (which includes efforts of data collection before this step). The available data are sorted according to two criteria: (1) function and (2) modality. In the function analysis, data are sorted along the following categories: (a) primary-founding data, (b) secondary-founding data, (c) supporting-concretizing data, and (d) visualizing data. The data of type (a) refers to all the data without which the problem cannot be understood as such. Type (b) represents all the data that shows a process of problem deferral, i.e., data about the concrete consequences of the problem. Type (c) data relates to situations other than the one under investigation in the project in which the problem nonetheless occurs, and type (d) data is data that is technically not needed to describe

the problem but can serve the purpose of further illustrating the problem situation. Analyzing the data modality means classifying the data according to their origin, i.e., narrative accounts, written accounts, video documents, tape recordings, etc. This also includes the analysis of the data according to certain common beliefs or other forms of weeding out contradictions – this is specifically relevant to narrative and written accounts.

3. Alternative premises and practical test: The goal is to emphasize premises different from the ones the persons concerned were using before – under the condition that these new premises lead to new possibilities. The goal is to "try out" these new possibilities.
4. Evaluation of the changes in praxis and stagnation: The consequences of the applied changes are analyzed. Were they successful, i.e., was the problem solved? If not, what were the reasons? In the event of failure (stagnation), the first three steps are reanalyzed, which includes the possibility of rephrasing the problem under the inclusion of the insights generated during the research process thus far.

The fundamental problem of the results of such a research process is that of generalizability, similar to the issues identified in the Action Research traditions. In the Southern Tradition, the hope for generalizability ultimately rests on more or less conflictual steps toward more justice that lead the way to what is conceived as a utopian telos of human development. In the Northern Tradition, the idea of generalizability as such was gradually given up during the development. For Lewin, generalization was a natural fact of the accumulation of scientific laws by means of scientific development. The more humanity knows, the more it will be able to apply this knowledge – resembling the developments in physics in the social sciences. In STSD, the hope was that generalization of the results would simply "emerge" because society would eventually understand that work organization based on semi-autonomous groups is "better" in any aspect, i.e., more democratic, healthier, more flexible, more productive, etc. The Search approach hoped for a spread of a certain dialogical understanding, which would help to articulate problems and thus solve them – this happened, however, not in a generalized way since the only thing that was generalized stemmed from theory, namely, that humans have to engage in dialogue. The ED 2000 project, in contrast, followed the idea of "learning from difference," i.e., to understand one's own situation in the face of different situations of others in different but similar locations. For KP, the generalization process is captured by its concept of meta-subjectivity. Meta-subjectivity is supposed to ensure the understandability of research results beyond the concrete research context implying potential usefulness for others, who might or might not "try out for themselves" what they saw in another situation.

Generalization in KP is the generalization of agency, (Möglichkeitsverallgemeinerung, Holzkamp, 1983, p. 545). It stems from the relationship between the given societal agency and my concrete interpretation of this agency based on subjective premises as the reasons for action. Consequently,

> the many different individuals are not isolated from each other but have a factual relationship with each other expressed in shared societal agency. … Personal differences appear as various forms of the realization of this shared agency and can be realized by the individual as such. (Holzkamp, 1983, pp. 548, my translation).

Generalization thus means understanding that different modes of action result in effect from the same societal agency. By analyzing the premises of action, it is possible to form a certain space of possibilities that can be constantly enriched by new premises – contributed by new subjective realizations of societal agency and a collective realization of generalized agency. As such, KP offers a principal framework for "persons concerned" to empirically realize their interrelatedness and the possibility of acting together to enrich their space of possibilities in a generalized way (Holzkamp, 1983, ch. 9).

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Part IV
Action Research and Practice Research

At the beginning of this book, I outlined how the origins of Action Research date further back than often assumed in the literature. I have shown how most of the concepts applied by Lewin during the Food Habits and Harwood Studies originated in the works of Jacob Levy Moreno already during his time in Vienna and later in a more precise fashion during the 1920s, 1930s, and early 1940s when he arrived in the United States. I have also shown how this heritage was subsequently lost, most likely due to personal issues between Moreno and the Lewin group on the one hand and fundamental theoretical differences between the two protagonists on the other. Lewin believed in the possibility of change by means of scientific development; Moreno believed in human action and the power of spontaneity. This friction left Moreno's work on the margins and largely ignored the Action Research literature. His methods were influential, but his name – and with it, his theoretical heritage – were ignored and, in some instances, so it seems, deliberately left out of the picture. But Moreno's methodological ideas remained encapsulated in Lewin's general framework and were taken up in the development of Action Research after Lewin.

Lewin never claimed to have founded Action Research, or Group Dynamics, or any other concept he made use of – except Topological Psychology – but at the same time he remained unclear about the sources and inspirations of his concepts. It is due to his prominence in psychology – Gordon Allport and others described Lewin as one of the two most essential protagonists in psychology in the twentieth century, the other being Freud {Allport, 1947, #52559@47} – and his reputation as an original thinker that subsequent generations of Action Researchers readily accepted Lewin's position as the originator of the fundamental ideas of the approach. The "hidden" Moreno in Lewin's thoughts is not reducing Lewin's importance, for he developed an entirely new framework for research and managed to draw the attention of the social sciences away from either large-scale social perspectives or micro perspectives on the (averaged) individual toward the phenomenon of social groups. However, the theoretical framework of vector psychology and the Gestalt imperative that the whole is different from the sum of its parts, i.e., that the investigation of individuals cannot replace the investigation of groups which subsequently became the smallest logical unit Lewin's research rested upon, also brought with them the

exclusion of several aspects of group life, which were initially captured in Moreno's thought.

Klaus Holzkamp did not draw on Lewin, although it can be assumed that he was familiar with Lewin's work, nor does he reference Moreno at any point in time – and there is no indication that he knew about Moreno's work. And such connections cannot be expected since Holzkamp followed a different objective: His aim was not to further develop psychology; instead, he believed that the discipline as he saw it in the late 1960s and early 1970s had failed as a whole and that the only way out of this misery would be to build it anew from the grounds up – which he eventually hoped to achieve with his magnum opus Grundlegung der Psychologie in 1983. Moreno, with Sociometry, and Lewin, with Topological Psychology, pursued similar endeavors. The remarkable connection between the three is their conclusions for empirical work, which in all three instances involved the necessity to be carried out in the field, i.e., the real world, and which in all instances was aimed at producing social change. Beyond this similarity, however, many differences occur with Holzkamp and Moreno heading in a similar direction while Lewin's concept rests on a fundamentally different basis. From this perspective, the goal of the following concluding part is to show how, by re-appropriating the Morenoian origins of Lewinian thought, the basis of Action Research can be enriched from a Holzkampian perspective. More specifically, the aim is to show how the fundamental concepts of subjectivity and democracy are underconceptualized in the various Action Research traditions leading either to an unbridgeable gap between aspiration and reality – as is the case for the Southern Tradition – or a stepwise reduction of the aspirations to produce change on the macro level as illustrated by the Northern Tradition.

Chapter 9
New Grounds for Action Research?

The Lewinian Loss of Subjectivity, Its Perpetuation in the Northern Tradition, and Its Critique in the Southern Tradition

Lewin's theoretical reflections about Taylorism point beyond his later empirical applications in industry and the theoretical framework applied throughout his Action Research Studies. In his 1920 paper, Lewin refers to the problem of labor as an issue of the private sphere instead of the public – an idea that returns much later in the development of Action Research, namely, in the Search approach. He hints at the importance of psychological and physical health for the labor process, an issue not touched upon during his empirical work and only becomes a topic again in the Tavistock coal mining studies. He also understands that productivity cannot be measured solely according to the actual products produced but must be conceptualized based on the interplay of worker's satisfaction, production of goods, and societal needs – a reevaluation that only enters Action Research over the course of the ID Project and without the component of societal needs. He introduces a concept of democracy that goes way beyond the kind of functional relationship between group decisions and group action in his later studies. In the Taylor critique, democracy is necessary because the worker is the expert on her task. She can best decide whether production is functional, productive, and subjectively healthy. Democracy is needed because the workers need to democratically negotiate their actual productivity with society to determine the right interplay of production and societal needs. Essentially, Lewin, in this early paper, calls for a process of democratic dialogue to negotiate the needs and requirements of the individual versus society at large. Labor, so the claim, must be taken out of the private realm in a double sense: It cannot benefit solely private capital, and the work conditions are significant beyond the private matters of the individual worker. Labor needs to be placed in the public sphere where the concrete conditions are to be negotiated. Applied psychology, as a tool of science, is the

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facilitator of such a process, a concept that essentially resembles the dialogical concepts of the Northern Tradition. However, Lewin's actual research, and as such, the main reference points for the development of Action Research, turned out to be quite different.

This difference stems from the theoretical framework Lewin developed in the years after the publication of the Taylor article. This framework broaches the understanding of the individual and groups. It creates a framework to research and manipulate individual behavior and group behavior and, as such, also provides a framework to understand what this individual is, namely, an actor in a situation who is confronted with various (inner and outer) forces that eventually determine her behavior. As such, the individual is determined by the situation and the concrete agency therein: "A dynamic psychology has to represent the personality and the state of a person as the total of possible and not possible ways of behaving" (Lewin, 1936, p. 14). From this point of view derives a relatively clear-cut procedure for investigating the subject (or group): The subjective life space is investigated with respect to its "facts," i.e., everything that "matters" for the particular person or group. This procedure creates an account of the actions taken by the person or group and the subjectively following steps. In case of a conflict, the resolution depends on the analysis of the subjective life spaces of the parties involved, paired with an analysis of the objective situation, i.e., all the possible actions in the very situation to "communicate to each other the structure of their life spaces with the object of equalizing them" (Lewin, 1947, p. 12). As such, Lewin introduces a subject bound by psychological law and can be understood if these laws are understood and subsequently manipulated with this knowledge.

This kind of understanding is best exemplified in the Food Habits studies. Even though the studies took place "in the real world," they were used to illustrate fundamental principles and not bring about change in the subjects' interests. The food the housewives had to be convinced of was not chosen according to principles of healthiness or availability but based on the extremeness of the case. The reasoning behind this was that if housewives could be persuaded to use food considered disgusting, it would be possible – even easier – to introduce less disgusting food to be chosen in other cases according to more functional principles. The studies were supposed to be exemplar cases that reveal an underlying basic principle inherent in groups, namely, that group discussions with a democratic leader (the researcher who guides the discussion) can lead to a group decision that manifests a change of the existing forces in the field. Democracy in this concept is reduced to the actual vote in the moment of decision; it no longer includes the dialogical factor of actual negotiation, as it did in the Taylor paper. To be sure, dialogue takes place, but not as a form of actual negotiation. It is merely used to ensure active participation. Moreover, the decision becomes more or less predetermined in that the natural goal of the whole research endeavor is to have the housewives decide to use the new sorts of food. A negative decision could be explained within the Lewinian framework in two ways: Either the democratic leader, i.e., the researcher, did a bad job during the discussion phase in that he failed to "lead" the group toward the right ends, or the underlying culture as a quasi-stationary equilibrium does not allow for a change of such a

magnitude (i.e., the kind of food the housewives were supposed to be convinced of is plainly unacceptable for fundamental reasons such as religion, etc.).

According to Lewin, this cultural framework is the basic underlying structure that predetermines not the actions themselves but the core of agency within that culture as such. This agency is perceived as a natural boundary in "that everyone considers as 'human nature' what actually is the character of his [sic] own specific culture" (Lewin, 1999, p. 335). This culture is unchangeable from within; it can only be overthrown entirely by a (cultural) revolution or changed with the help of a strong leader who comes from the outside and actively promotes large-scale reeducation. This reeducation may even entail force – what Lewin calls the "democratic paradox" – —as long as the leader is, in principle, democratic (Lewin, 1943, p. 561). Again, democracy here does not mean "dialogue" but "vote" based on a specific education brought about by the leader. The subject in this framework is fully determined in multiple ways: While it is, in principle, granted its subjectivity and individuality – which in the situation determines that which has effects – it is at the same time bound by the situation and the concrete forces in the field. These forces are subject to manipulation by a leader. Moreover, how the forces play out is ultimately defined by "habits," which are based on an underlying, almost unchangeable substructure termed "culture."

As can be concluded, Lewin does not incorporate an element of actual choice, as described in his Taylor paper, in his conceptualization of the subject. He could not reconcile his idea of per se lawful behavior with the creative potential of humans to engage in active and open-ended dialogue. In Holzkampian terms: He realized only the first half of the twofold agency, only restrictive and not emancipatory agency. He understood that there are determining factors for behavior and that these factors are multilayered and complex. He realized that the cultural structure becomes naturalized, that it loses its human-made character and is perceived as unchangeable laws. He, however, failed to understand that there is a second level of discussion beyond a mere "obeying the rules," namely, the discussion of these rules as such. He thus fails to overcome the subject/object divide he endeavored to transcend and did not realize that research in psychology does not deal with other objects but with the other, who "resembles me (*mir gleicht*)" (Holzkamp, 1983, pp. 237, my translation).

Analogously, the treatment of subjects as objects is also the problem of Lewin's concept of groups. As discussed before, Holzkamp conceives of the group as consisting of subjects that remain subjects for themselves and only constitute a group with others in that they realize a common cause. For Lewin, the group is defined in different terms, namely, "interdependence" (Lewin, 1939). To be sure, Lewin does not believe that the group is a mere sum of its parts (i.e., the individuals it is composed of), nor is it an entirely new entity that replaces the individuals it consists of. Instead, it is constructed of the various interdependencies of the individuals in the group and, as such, different from the sum of the attributes of these individuals. However, "for Lewin, the existence of groups is … anchored in the structural sphere" (Pagès, 1974, p. 114) and the relationship among the members is not psychologically dynamic but physically static.

These theoretical underpinnings explain the dual character of Lewin's concept of Action Research. On the one hand, Lewin developed a psychological concept of change based on communication. By emphasizing the importance of the forces in the field and the life space of the person, he shows that change depends on the environment. These environmental changes are only effective if they matter to the subject, i.e., are of psychological relevance. The democratic leader manages to make topics matter; she introduces certain goals, methods, and norms to the group by means of communication. That way, change is not imposed on group members but rather "directed." On the other hand, democracy (or, instead, a special Lewinian version) is imposed on the people, and so is the communication about specific norms, goals, and methods. Dialogue about democracy does not exist. Instead, democracy is a technical procedure to achieve specific goals. It is introduced in an autocratic and not a democratic fashion. It leaves no room for the subjects to act based on personal needs and choices outside the given sphere of goals, methods, norms, etc., thus representing a mere restrictive system. In line with this conclusion drawn by Chein et al., in a state-of-the-art article about Action Research published in 1948, Action Research is "controlled research on the relative effectiveness of various action techniques" (Chein et al., 1948, p. 48).

The STSD approach inherits an underlying tenet from the Lewin approach, for in STSD, too, democracy is not a matter of democratic agreement. Instead, it is forced upon the workers as a superior organizational structure. The rationale behind this superiority differs from Lewin's approach, though. Lewin believed to have shown the superiority of democracy in his experiments. Democracy, under the guidance of a democratic leader, proved to provide the maximum amount of free space for everyone and could thus guarantee a better functioning of the situation in which it is enacted and ultimately for society as a whole. In STSD, in contrast, the reason for democracy is not functional but psychological. It stems from the assumed core needs of the subject: demanding and variable tasks, a chance for continued learning, individual decision-making, recognition and social support, a relation of work to social life, and a prospect of some desirable future (Emery & Thorsrud, 1976, pp. 15–16; Trist, 1981, p. 42). These needs were not empirically based but conceptually understood. They did not describe an empirical subject that carries these needs as a process of a historical (evolutionary) becoming. Instead, they formed an ideal subject, namely, the subject of the semiautonomous group, which replaces the Lewinian democratic leader as the central necessity for democratic structures.

For a semiautonomous group to function, it must consist of subjects that require demanding and variable tasks, constant chances for continued learning, and so forth. Thus, despite the conceptual application of this ideal character of needs, they were treated as empirically established requirements for a good life. The tension between the conceptual and empirical dimensions of needs has not been explicitly tackled by the STSD tradition. The empirical subjects in the field were assumed to be identical to an imagined, ideal subject. Consequently, the failure at Christiana Spigerverk was not attributed to an underlying misconception of the needs and wishes of the workers involved but to a general failure in the communicational process. From this perspective, the workers simply did not understand that the

suggested improvements were, in fact, in their interest. The forceful establishment of democracy was legitimized because it supposedly guaranteed the satisfaction of the workers' needs.

In contrast to this interpretation, the underlying reason for the failure at Christiana Spigerverk seems to lie in the unaccounted-for needs and wishes of the actual subjects involved. They were subject to a specific organizational structure which, in a certain way, restricted their agency prior to the experiment. The experiment came with a new set of restrictive structures, i.e., no longer being allowed to work on one's machine, being made to take turns on the welding tasks, etc. Adding to that, the new situation brought with it a certain insecurity about the consequences of such changes and created an existential threat (or at least the possibility thereof) for some because they feared layoffs for the number of people needed to run the existing machines was effectively reduced while productivity was increased. From this perspective, the introduction of an Action Committee in the second study at Hunsfos can be seen as a committee to foster the transformational process. It helped the workers to find their place in the new system faster and more effectively, i.e., provided guidance in understanding the newly imposed restrictions of action for the subject to establish new premises for their actions and meaning construction. The premises themselves, though, remain unreflected upon, the individual meaning structure of specific actions are not made explicit, and the restrictions of the semi-autonomous group setting do not become subject to discussion or dialogue.

The dialogical approach seems to finally break with these major issues in that it stops forcing a specific democratic structure upon the workers and instead attempts to provide the grounds for a democratic emergence of democratic structures. The turn toward a framework of "generative theory" (Gustavsen, 1985, p. 467) refers to a double shift. First, democratic structures should no longer be prescribed but result from a democratic process. Secondly, the subject is not defined as the carrier of a certain set of attributes and needs. Instead, the subject was now regarded as possessing a generative capacity, i.e., they can produce something anew. This production process takes place in the realm of language – in communication. Thus, it was necessary to establish a specific form of dialogue. The rules for such a dialogue enacted during the Search Conferences were essentially drawn from the concept of deliberative democracy as spelled out by Jürgen Habermas. As such, the problem of the subject is, in effect, dropped entirely in this approach and replaced by a discursive process. By establishing a certain discourse, namely, the "rational discourse" Field (Habermas, 1962/1990, p. 105), principally, everything can be uttered for the generative potential of the subjects to unfold. The place for these rational discourses was the Search conferences at which democratic structures were to be established by democratic means.

However, a closer look at the implementation of these Search conferences, as shown in the J. L. Tidemann's Tobacco case, reveals a different story. The assumptions about the existing discourses (or discursive formations as the authors call them) are relatively strict. The Search conference exhibits a relatively rigid organizational structure that is set up by the researcher-experts. The goal of the Search conference is not to find actual solutions to actual problems – problems are not even

analyzed but simply stated. The goal is not to "give the participants what they want" (Pålshaugen et al., 1998, p. 56). Instead, the Search conference is a training ground for specific forms of dialogue between various status groups. In other words, it is a training ground for a particular form of democracy which is – in the ideal case – enacted in the company after a (successful) Search conference. What is described as a "change in discourse" by the authors resembles the same kind of replacement of an old structure with a new one as already witnessed in the STSD approach. Because these dialogue forums are kept alive, however, this new structure is more flexible. It would potentially change over time according to the decisions made by the workers and workers' representatives. As such, the dialogue approach provides more flexibility and a less rigid structure compared to STSD. Yet, because the subject is reduced to its capabilities to produce different utterances in different contexts (i.e., to be able to "say more" in a less restrictive context and to "say less" in a more restrictive one) the twofold agency in the Holzkampian sense cannot be incorporated. Instead, an ongoing change process is established in which an existing structure of agency and restrictions is replaced with a new one. This replacement is the consequence of a dialogue about topics of company efficiency but does not involve a conscious relation to the restrictions to enact emancipatory change. In other words, a constant process of substituting specific restrictions for other ones – valued as the product of a dialogue – is established. Transformation is not produced because of a realization of specific premises for action, which would lead to a developmental process in consciously relating to restrictions and thus overcoming those, but by a dialogue that renders certain restrictions dysfunctional from a certain perspective, mostly the perspective of increased productivity, which thereby makes it necessary to substitute these specific restrictions for those which appear in a different form. To produce change in the interests of the subjects, however, it is necessary to establish a conscious relation to the restrictions instead of simply replacing structures based on the evaluation of some general points of view that are assumed to be in everyone's interest but in fact necessarily enact generalized interests of a specific discourse.

The Southern tradition, in contrast, tackles restrictions of action directly, and the underlying concept of the given social structure is equally clear-cut. In essence, many are forced to live a life of oppression enforced by the few. These few are the lost subject; they do not matter for the research process other than that they are the enemy one has to fight against. The many on the other side are – even though they are alienated to a certain extent – closer to their real subjectivity, which they can reach through education and a reorientation toward their cultural roots. The goal of this education and reorientation is not so much to teach the subjects democracy as in the Lewinian case. Education is supposed to do away with alienation to uncover the pure subject that existed before oppressive structures were established. This Rousseauian purity of the subject entails democracy already and will automatically exhibit it. However, to arrive at this purity, the Southern approach faces the same kind of democratic paradox Lewin described. Namely, to arrive at this democratic state, a leader who directs the subjects toward this purity is needed. The paradox is mitigated here, however, because democracy is not entirely foreign to the subject; it

is merely hidden "inside," or repressed, and needs to be brought to the forefront. This difference also provides the baseline for the Southern critique of Lewin's concept as (neo-)liberal and affirmative. From this perspective, Lewin forces a new structure upon a group – which he judges as superior – while the Southerners think of themselves as liberators of existing but hidden capabilities and needs. This presupposed underlying, yet hidden, constitution of the subject is, of course, not empirical in any way. It mainly derives from the kind of orthodox Marxism that grounds this tradition: Change is brought about in a class struggle, and the subject that possesses the true potential for change is the oppressed one. The utopian character of this concept becomes visible: it is the eventual replacement "of the old bourgeois society, with its classes and class antagonisms, [with] an association, in which the free development of each is the condition for the free development of all" (Marx et al., 1848/2008, p. 66). The research process is consequently not of much methodological interest; critique is carried out in very broad theoretical terms (making it easy to subsume anything or nothing), and research is a means, a mediator but in a way absent from and unrelated to that which cultures have to produce themselves. Thus, the oppressed own the knowledge of their emancipation; in some sense, it is embodied mainly by them.

As opened up by the Holzkampian Critical Psychology perspective, sustainable change might not be reached by democratic structures with which actual structures are imbued; neither seems a change in discourse capable of keeping its promises. More favorable might be a return to the subject as a critical concept in Action Research. However, this subject cannot be an imagined one or a concept derived from a certain theoretical perspective. Instead, it is conceptualized on a double empirical basis in that it is understood in its historical becoming, i.e., in the position it takes in the societal structure and the local infrastructure. It is understood on its actual-empirical level, i.e., from the perspective of the concrete life circumstances that make certain actions possible and pose restrictions upon others.

Moreno, Holzkamp, Subjectivity, and Democracy

Like the Southern Tradition, Moreno's aspiration for change was driven by a utopian moment: "Man [sic] is more than a psychological, social or biological being. Reducing man's [sic] responsibility to the psychological, social, or biological department of living makes him [sic] an outcast. Either he [sic] is co-responsible for the whole universe or his [sic] responsibility means nothing" (Moreno, 1949, p. 235). Moreno saw himself in a tradition of the great philosophers of history and the natural successor of Freud in psychology. He is believed to have continued where they left off: "Marx saw the position of man as that of a member of society, the struggle within it as his ultimate destiny. Freud saw the position of man (sic) as the one of a traveler between birth and death, the cosmos beyond was shattered. I moved man back into the universe" (Moreno, 1949, p. 235). It is from this

perspective that he aims at "the idea of a society in which our deepest selves are realized" (Moreno, 1949, p. 236).

As a result, he aims to change society because a "true therapeutic procedure cannot have less an objective than the whole of mankind" (Moreno, 1934, p. 3). And he sees the grounds for such a society in the evolutionary process itself: "If … 'Creative' Evolution should be true, it cried for a demonstration, for its continuity in the realm of action, … for the evolution of the creator. It was this which brought about our attempt to turn the elan vital into the reality of experimentation, the training of spontaneous personality" (Moreno, 1934, pp. 7–8). His sociometry was conceptualized as a "technique of freedom," which was supposed to balance the "spontaneous social forces to the greatest possible harmony and unity of all" (Moreno, 1934, p. 7).

In the research strategy that emerged from this belief, "the subjects must be approached in the midst of an actual life-situation and not before or after it" (Moreno, 1940, p. 317) because "[t]he psychology of action cannot divorce the act from the actor, the actor in situ, and the single actor cannot be separated from the ensemble of actors in situ" (Moreno, 1952, p. 366). The exact method could not be spelled out in advance. Instead, it was necessary "to let the direction and the expansion of the research grow out of the situation" (Moreno, 1934, p. 91). Research served as a "deliberate attempt to bring the subjects into an experimental state which will make them sensitive to the realization of their own experiences and action-patterns" (Moreno, 1940, p. 317).

For the researcher, this meant they had to reflect on their role in the research process because "the 'uninvestigated investigator' constitutes … an ever-present error" (Moreno, 1940, p. 318). If researchers reflect on their role, they become "less and less an observer and more and more an aid and helper to every individual of the group in regard to their needs or interests, the observer undergoes a transformation, a transformation from observer to auxiliary ego" (Moreno, 1937, p. 210). The participants on the other hand "become open promoters of the project; the project becomes a cooperative effort. They become participants in and observers of the problems of others as well as their own; they become key contributors to the sociometric research" (Moreno, 1937, pp. 210–211). The concrete methods developed in such a research process are "methods of discovery" (Moreno, 1951, p. 40) in that they are not tools in the hands of the researcher but available to all the participants to solve their problems. This means that "every member of the group [is given] research status" to make "[s]ociometry … the sociology of the people, by the people, and for the people" creating the "rule of universal participation in action" (Moreno, 1951, p. 38). Research is carried out "in such manner that it is itself a motive, an incentive, a purpose, … for the subject," which becomes an "active agent in matters concerning his [sic] life situation" because "[i]f the test procedure is identical with a life-goal of the subject he can never feel himself to have been victimized or abused" (Moreno, 1934, pp. 14–15).

In a similar vein as Holzkamp, Moreno draws his conceptual framework from a critique of traditional psychology, which "throw[s] the subject into a passive state, the subject being in a role of submission" (Moreno, 1934, p. 15). "Sociometric action theory," in contrast, "is not the outcome of an emphasis upon pragmatic and

empirical thinking but the result of a critique of the total methodology of social science" (Moreno, 1951, p. 134). Moreno's research strategy is a "hot sociometry" (Moreno, 1954, p. 186) which realizes that

> [t]here is a way in which man, not through destructiveness nor through economic planning, but as a biological being and a creator, or as an association of creators, can fight back. It is through a strategy of creation which escapes the treachery of conservation …. This strategy is the practice of the creative act, man, as a medium of creation, changing his products continuously. Spontaneity as a method of transition is as old as mankind, but as a focus in itself it is a problem of today and of tomorrow. (Moreno, 1934, p. 364)

Consequently, Moreno's research had to be "a theory of human relations … founded [on the idea of] propelling human groups into action" while at the same time combining the production of theory with empirical work to overcome the "controversy between scientific and applied" research (Moreno, 1951, p. 134), rendering "the sociometric procedure operational and observational at the same time" (Moreno, 1951, p. 20).

The group thus plays a major role in Moreno's thinking; in fact, "sociometry, because of the unity of the human group, studies the human group as a totality. It studies every part with a view to the totality and the totality with a view to every part." But the group is never the smallest entity of analysis, as in the Action Research tradition. Instead, "the gestalt is a function of the gestalter, social configurations function as groups of gestalters." As a result,

> Gestalt is not the "first" principle. The whole is not holier than the part. Gestalt is second to the "gestalter," its producer. There is a higher arbiter; a wider frame of reference than the principle of gestalt—the twin principle of creativity and spontaneity, the source of gestalts, of isolated parts as well as of wholes. If there is any primary principle in the mental and social universe, it is found in this twin concept which has its most tangible reality in the interplay between person and person. (Moreno, 1943, p. 317)

The similarities between Holzkamp's research concept and Moreno's concept of intersubjectivity and action as creation are striking. Both have a concept of equality among researchers and research participants in the research process. For both, research has to be concerned with the participants' problems, needs, and interests. Both believe in a method that becomes the participants' method for improving their life circumstances. Both have a concept of emancipation through research, and both believe in change through research against restrictions imposed on the individual. Also, both have a similar group concept in that a group constitutes itself along the lines of corresponding interests and needs. And most importantly, both start their research from the standpoint of the subject.

However, the reason for prioritizing the subject is different for Holzkamp and Moreno. Moreno's subject rests on a belief in creativity and spontaneity, which every human being can unfold. For this to happen, it ultimately needs a process of "not only the transformation of a man [sic] into God but the reverse, the retransformation of God into man [sic]" (Moreno, 1989, p. 15). Only from this plateau can "the idea of a society in which our deepest selves are realized" Field (Moreno, 1949, p. 236) be achieved. This gradual reunion of God and human ends in Moreno's future utopia on which his whole concept of subjectivity rests. Holzkamp's concept

is utopian as well. But his utopia takes a different form and a different place. It is a utopia in the true sense of the ancient Greek word, a "no place," which is not somewhere distant in the future but, in fact, nonexistent, only realizable in fleeting presents. It drives Holzkamp's theorizing in that "it does not expunge the possible from what exists" (Markard, 2009, pp. 67, my translation).

For Lewin, it was impossible to take the subject as a starting point in Morenoian terms. This is not so much because he adhered to the Gestalt tradition, from which he only borrowed concepts for his theory in a selective fashion. Rather it seems that Lewin was not even entirely convinced of the cohesiveness and strength of the Gestalt approach. But Lewin, the scientist, could not accept such spiritual notions of a utopia to come, brought about by a God-like figure. *Wissenschaft* showed him that there was a qualitative difference between the individual and the group and that the group was the smallest unit one would operate within the process of field research. Holzkamp, on the other hand, believed that it was possible to show that this creativity and spontaneity of the subject was, in fact, a consequence of an evolutionary process at the end of which he found the socialization of humankind as a new qualitative level of development. In a way, he immanentized Moreno's spirituality and turned it into a worldly potentiality. Once subjectivity is seen as not only the starting point but also the product of intersubjective processes, to be human means to possess a twofold agency to act. There always is a restrictive and an emancipatory mode of action.

Distinct from the Southern tradition, which privileges the past over future and present, and distinct from Moreno, who imbues human development with an all-encompassing spirituality, Holzkamp offers a response to the perpetual and naturalized interplay of subjectivity and democracy, mostly regarded as a constant struggle in the various action research traditions. To be sure, his perspective allows for Moreno's contribution to be reintroduced into or rather reevaluated in and for the field of action research. For Moreno, it is the needs and wishes, as well as the capability to be spontaneous and creative, which offer the core human factors for action and change. This understanding of human nature is derived from a theological tradition that holds humankind to resemble God. Certainly, the effects of such an understanding can be studied in various tragic outcomes of authoritarian politics, which led Lewin to turn his back on these messianic accounts. Yet, he thereby eschewed a deeper consideration of their values and usefulness for social science research. Holzkamp can provide a crucial bridge between Moreno and Lewin, which might, in turn, allow Action Research in general to develop a clearer sense of the underlying impasses their concepts of subjectivity and democracy imply.

Holzkamp's Critical Psychology can be understood as a successful attempt to turn Moreno's spiritual history of human development into an empirical one by examining the subject on the historical-empirical and actual-empirical levels. In rendering the subject and its premises and concrete life circumstances as the starting point of the research process from which its position in the local infrastructure and the society at large can be understood, the problematic notion of a desired democratic structure can be circumvented. The goal of research would no longer be a democracy (in one or the other form) but the empirical needs and wishes of the

subject and their reason for action. In creating a dialogue about these reasons and the concrete restrictions the subject faces, a discussion is put into practice, enabling subjects to consciously relate to these restrictions and thereby explore ways to change them. Against this background, democracy does not represent a desired structure that needs to be implemented by the researchers but an automatic by-product of the conscious discussion of the existing restrictive agency. In other words: Holzkamp changes the question from "How do we produce change towards a different, desired (democratic) future?" as it is prevalent in Action Research, toward: "How do we deal with the existing restrictions?" Change is thus not the primary goal of the research project in Critical Psychology because the existence of restrictions is not, per se, problematic but a necessary part of the societal structure. It is possible to consciously relate to these restrictions because every restriction entails a possibility, namely, the possibility to act differently (or not to act at all). Change is not the establishment of a new structure but a concrete transformative process in the concrete behavior toward restrictions and, thereby, a transformative process of the restrictions as such.

Unlike the Southern Tradition with a focus on counter-knowledge in the service of particularity, and unlike the Northern Tradition with an emphasis on improving knowledge in the service of universality, Holzkamp unites both on a third level: Action Research becomes a mediation through which democracy as its universal element and subjectivity as its particular element are both deepened. In fact, democracy is not possible without subjectivity and vice versa – they establish each other in an intersubjective process that takes place in the here and now. This intersubjectivity opens up possibilities that have never been completely eliminated or successfully repressed from what exists. It entails a counterfactual dimension, but this counterfactual dimension does not require a set of human needs (ID Project), a specific form of discourse (Search approach), or conflict to be pre-given (Southern Tradition); instead, it transcends social life worlds through a process of a conscious relation to existing restrictions. To be sure, for Holzkamp, the change process is a process of the subjects themselves; it does not take place by establishing new structures; it happens through the interactive and intersubjective participation of the subjects within the given framework of restrictions. From here, the Holzkampian contribution to Action Research could be an empirical subject that forms intersubjective relationships within a societal structure and can create change by consciously relating to existing restrictions, thereby enacting the twofold agency. The utopian fixation on either a wholeness gradually achieved through future perfection or a wholeness buried in the past and eventually reinstated in an authentic future world is replaced by a view of restrictions as historically developed and seemingly unified in the present but open to change through intersubjective practices. Far from representing a move that might seem to come with a certain thinness, the potential for transformation can be quite substantive. Thus, the radicality of change in projects carried out in Critical Psychology does not result from justifying a certain worldview and the question of how this worldview is brought about but is expressed in the subjectivities at work that consciously judges restrictions and possibilities to react to them.

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