

Outlining a theory of the social and symbolic function of evaluation of education

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Abstract

This article presents the results of a research project focusing on evaluations of education as a part of a new public management in the area of education.

The empirical material consists of:

- 8 state-sanctioned evaluations of the formal training programs for the positions in a medical field
- various texts on evaluations
- various examples of Danish evaluation research.

A field of producers of Danish evaluation research is constructed as part of a field of power: analogous to the analysed evaluations, Danish evaluation research forms a discourse legitimizing socially necessary administrative interventions. The evaluations and the evaluation research are constructed as parts of a mechanism performing and legitimizing a sorting to an existing social order.

The theoretical starting point is in theories primarily by Émile Durkheim, Pierre Bourdieu and Ulf P. Lundgren.

Sammendrag

Denne artikel præsenterer resultaterne af et forskningsprojekt med fokus på uddannelsesevalueringer som del af en new public management inden for uddannelsesområdet. Det empiriske materiale består af

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- 8 stats sanktionerede evalueringer af uddannelserne til positionerne inden for et medicinsk felt
- forskellige tekster om evaluering
- forskellige eksempler på dansk evalueringsforskning.

Et felt af producenter af dansk evalueringsforskning konstrueres som del af et magtens felt. Analogt til det som ses i de analyserede evalueringer, danner der sig inden for dansk evalueringsforskning en diskurs som legitimerer socialt nødvendige administrative interventioner. Evalueringerne og evalueringsforskningen konstrueres som dele af en mekanisme som udfører og legitimerer sorteringen til en eksisterende social orden.

Det teoretiske udgangspunkt er i teorierne først og fremmest af Émilie Durkheim, Pierre Bourdieu og Ulf P. Lundgren.

Nøgleord: evaluering, uddannelsesevaluering, social reproduktion, samfund efter det Moderne, meritokrat

Résumé

L'article présente les résultats d'une recherche qui voit les évaluations des éducation comme un cas de "Nouvelle Organisation Publique" dans le secteur de la formation et de l'éducation. Les données empiriques comportent:

- 8 évaluations, homologuées par l'État, de programmes de formation pour des positions dans le champ de la maladie/santé
- différents textes sur les problèmes de l'évaluation
- différents exemples Danois de recherches sur les problèmes de l' évaluation.

Le champ des producteurs de recherches scientifiques sur l'évaluation est construit comme faisant part du champ du pouvoir. De manière analogue aux évaluations étudiées, la recherche scientifique Danoise sur l'évaluation se présente elle aussi comme un discours pour légitimiser des interventions administratives d'avance socialement nécessaires. La recherche présentée dans l'article construit ces évaluations empiriques et ces recherches scientifiques sur l'évaluation, comme des mécanismes qui réalisent et légitiment l'allocation des positions professionnelles dans l'ordre social existant.

L'inspiration théorique de la recherche vient principalement de Durkheim, Bourdieu et U.P.Lundgren.

Mots clés: évaluation, évaluation de l'éducation / formation, reproduction sociale, sociétés d'après la modernité, méritocratie

Entry, focus and interest

The present article resumes the major findings of a research project reconstructing the social and symbolic function of evaluations of formal training programs. What is formulated in the project is a social theory *about* evaluations (science in a strict sense, cf. Durkheim, 1956), and even more specifically a social theory about the *social and symbolic function* of evaluations, i.e. about the *proper*

pedagogical function of evaluations. The project is documented in the author's dissertation (2008a) and in other texts by the author (cf. the list of references). The aim was never to formulate any kind of theory *for* evaluations in order allegedly to improve the evaluations. Within the chosen theoretical frame of reference this is done practically under the given circumstances.

In the project it is shown that a purely scientific stance formulating a theoretical theory about evaluations is absent in a field of evaluation research: However refined, because of certain implications evaluation research consists of practical theory (cf. Durkheim, 1956), and gives priority to (explicitly or implicitly) normative approaches; evaluations, meta-evaluations, meta-evaluating theoretical work, methodologies etc. In this research project it is shown that this is state of the art in evaluation research¹ This emphasizes the need for discussions such as the ones in the present article, and not only related to evaluation research.

In accordance with a new public management, a performance seems to only legitimately exist as a performance if it has undergone some kind of alleged standardization². In the last instance this implies an evaluation. This gives rise to complaints from the evaluated practitioners. Practical experience tells them that improvements to their practice originate from the practical meeting of demands from practical circumstances and from helping each other, and not from formal evaluations. In turn it gives rise to an interest in examining the social and symbolic need for specialized instances with the exclusive right to produce what has been produced between peers for innumerable years. Why is it necessary to produce this quasi-knowledge in a more or less *inapplicable* form if the ambition were to improve the practice of the practitioners whose practice is evaluated?

The article contains a seminal work, applying first of all the theories of Pierre Bourdieu, and it adds to the existing evaluation research a strictly scientific, sociological or pedagogical explanation of the presence of evaluations. Following Bachelard (1968) the interest and aim in any scientific analysis is to *break* with an immediate understanding of (in the present project) what an evaluation is, that an evaluation is necessary and offers something indispensable for the concrete practice, and that a task is to (normatively) improve the evaluation or to simply choose the right one (cf. Bachelard, 1968). This narrows the perspective to explaining and understanding evaluations as an occurrence in the world: Why are they here? Who performs them? What are the implicit or explicit presuppositions of evaluations? What do they do? What is the social and symbolic function of evaluations? What are the social consequences of performing or deciding to carry out an evaluation and what is the symbolic function of performed or planned evaluations? The aim is to construct the *practice* of evaluations and to construct the true pedagogical function of evaluations of formal training programs i.e. the ones analysed in the present research project. The research questions are: What do evaluations do, pedagogically and sociologically? What are the practices of evaluations? And, more fundamentally: Why are evaluations necessary?. The research project outlines a theoretical theory, a strictly scientific theory, about evaluations.

¹ In Callewaert (unpubl., 1976) the relation between evaluation and social reproduction is discussed. However, the discussions and explanations seen in this project, are not addressed in Callewaert's text.

² The examples are legion within a new public management: authorizations, standardizations, examinations, evaluations, risk assessments, quality standards, etc. etc. etc., and with the educational system as a prominent example of a system separating the right from the wrong and uniting and uniforming the right, involving intricate systems of evaluations, self-evaluations, meta-evaluations at any level, externally as well as internally etc. etc. etc. (see Bourdieu & Passeron, 1992 and even Nørholm & Brinkkjær, 2005). This work of evaluation implies and demands the cooperation of and is performed by a corps of specialists, most often trained in the social sciences (cf. below and Nørholm, 2001, p. 569, cf. also Callewaert, unpubl. 1976, p. 1).

Another focus is on the applied evaluation *method*, on the implicit or explicit presuppositions of the applied method. Who performed these evaluations? What are the qualifications of these individuals? What are the implicit assumptions regarding the relationship between directions and practice underlying the evaluations? Etc. At an immediate level it seems that the recommendations presented in the evaluations are in the interest of nobody: If a practical mastery is trained practically, i.e. under the given circumstances, why do the evaluations systematically recommend more practical theory reproducing ideas of how practice ought to be, a symbolic mastery? The attempt is thus to explain and understand how the recommendations presented in the evaluations are formulated and put through, apparently unanimously: What are the interests in what appears to be in the interest of no one?

The focus is neither on the content of the analysed evaluations nor on the content of the evaluated formal training programs. Nor is it on the advantages or disadvantages of different evaluation *methods*, or on *developing* such methods; nor is it on whether the methods are applied properly, what possible methodological mistakes have been made, what alternative methods might be more expedient etc. In other words: The focus is relatively narrow on the practice of evaluations of formal training programs, with perspectives on evaluations in a broader sense and on evaluation research as a socially and symbolically indispensable part of the evaluations, i.e. as an accompanying discourse to the evaluations. For a discussion of how an accompanying discourse - or a practical theory (cf. Durkheim, 1956) - is constructed as an indispensable part of a practice, see Nørholm & Brinkkjær (2005). Within the chosen theoretical framework, it is explained *why* these evaluations increasingly naturally suggest improvements that are contrary to a relationship between school, education, formal training programs (a symbolic mastery) on the one hand, and the acquisition of a practical mastery on the other (cf. Bourdieu, 1977, Bourdieu & Passeron, 1992).

Theoretical background and frame of reference

Because of the relatively narrow focus on the social and symbolic function of evaluations of education the analyses depended on the existence of a theory about human action including the social genesis, meaning and distribution (structure) of these actions. The analyses moreover depended on the existence of a theory that, in an anthropological sense, can be applied to explain and understand the symbolic function of formal training programs, a scientific theory for the reconstruction of a social and symbolic function of formal training programs, i.e. a theory *about* a social distribution of what is misleadingly designated as the 'results' of formal training programs. Misleadingly, because it is not obvious that formal training programs are instrumental in someone acquiring a mastery, at least not in the first instance (see the discussions in Nørholm & Brinkkjær, 2005).

The focus of the analyses required a sociological theory to explain a social distribution of different forms of practical practice or practical competences and of practical theory respectively; a sociological theory that could explain the social relations of dominance in these distributions. This is found in Durkheim (1956). The focus even required a theory about the social conditions related to sorting in a school or educational system: What is the social function of formal training programs? What is the social distribution of different forms of practical competences? What is the social distribution of the training and allocation of practical competences respectively? And especially: What role do evaluations of formal training programs play in this complex? This is found in (Bourdieu & Passeron, 1992). Already at an early stage of the analytical work, it is seen how one is forced to step by step construct the object by constructing the implied objects and by implying an

epistemological break (see Bachelard, 1968, Bourdieu et al., 1991). This is present in the theories of Pierre Bourdieu in particular; the relevance of which also depends on:

- limiting the scope to describing and explaining, and refrain from suggesting improvements
- choosing the theories as a result of empirical investigations and not from philosophical or other considerations
- including meta-theoretical considerations of the status of a theory as such

In other words, the theories Bourdieu are chosen because they offer a sociological theory of practice (a sociology of action, praxeology), and a theory of how knowledge about practice is obtained (a social theory of knowledge). Finally Bourdieu offers a sociologically reflexive theory about the practice of the researcher and about the function of this practice related to the investigated phenomenon as well as to the results of an analysis of the investigated phenomenon.

Even Bourdieu's notions of *habitus*, capital and field are applied

The structures of a particular type of environment (e.g. the material conditions of existence characteristic of a class condition), which may be grasped empirically in the form of the regularities associated with a socially structured environment, produce habitus, systems of durable dispositions²⁰, structured structures predisposed to function as structuring structures, i.e. as the principle of the generation and structuration of practices and representations. Consequently, these can be objectively "regulated" and "regular" without in any way being the product of obedience to rules, objectively adapted to their purposes without presupposing any conscious aiming of ends and an express mastery of those operations leading to these ends and, being all this, collectively orchestrated without being the product of a conductor's orchestrating action (Bourdieu, 1973, pp. 63-64).

Note 20 in the quote says:

The word "disposition" seems particularly appropriate for expressing what is covered by the concept of habitus (defined as a system of dispositions): firstly, it expresses the result of an organizing action, having a meaning very close to such words as structure; furthermore, it designates a manner of being, an habitual state (in particular, concerning the body) and, especially, a predisposition, a tendency, a propensity or an inclination (Bourdieu, 1973, note 20, p. 64).

Habitus and *capital* are theoretical notions employed in reconstructing the incorporated history of a person or institution, and how this person or institution is positioned within a social *field*. Far from being static and determined once and for all, the notions and theories constitute dynamic parts of an analytical apparatus: The work of constructing the scientific object is part of a continuous scientific process where the concrete theories are applied as the methods for analysis. This is opposed to a more common vision where the methods for analysis are determined at the outset as a set of procedures and systems of categorization seemingly independent of what they are applied to and under what circumstances, as if such considerations were not necessary and everything were given.

Bourdieu's notions *doxa* and *illusio* are also employed. *Doxa* is that which "goes without saying because it comes without saying" (Bourdieu, 1977, p. 167). The incorporated ideal of what can and must be done, unmediated by consciousness; *doxa* is what is done and perceived as the proper thing to do under the given circumstances. Fundamentally and arbitrarily, *doxa* is perceived of as the only thing to do, the *normal*, that which 'calls itself' and is recognized as the right thing to do, and indeed is the *only* thing to do. That which, pr. social forces, the forces of the social field in question, is the only thing to do, think, say etc.

Illusio is the accepted corporal, incorporated, practical belief of what is worth investing and worth fighting for; the stakes and what is at stake in a certain field. That which, *pr. doxa*, is worth investing in the game (Bourdieu, 1996, pp. 333-336).

Stating that the chosen theory is the method applied might look like a mystification; the presupposition and naturalization of a normal vision implies the separation of theory, method and (idea-based, pre-constructed) object, and it implies that using the method systematically guarantees the quality of the research. However, the discussions in Lundgren (1979) as well as in Lundgren & Franke-Wikberg (1980, pp. 142-152) of these implications related to evaluations or evaluation research suggest that applying uniform methods regardless of the circumstances only seems to ensure fundamentally *incomparable* results (cf. also Nørholm, 2008a, pp.85-94). In Lundgren (1979) it is also stated that evaluations and evaluation research theoretically move “...within an epistemological perspective that can be classified as a specific variant of logical-positivism [an instrumental positivism, MN], advocating objectivity in terms of *methods*” (Lundgren, 1979, p. 38, my italics, MN). Consequently, a precondition for the mystification and the need to explain it is the naturalization of scientific premises diametrically opposite to that of the present analysis. So, instead of a mystification, it is a way of speaking clearly: Under any circumstance, any method implies a theory; it is futile to maintain independence by claiming to do so. Stating that the chosen theory is the applied method is even a way of turning the limiting conditions connected to any theoretical starting point into an advantage by systematically trying to control them instead of relying on an, in principle, impossible neutral method.

Stating this explicitly might even suggest a vicious circle analytically: It might seem to imply circular arguments; stating that the theory is the starting point not only for the discussions of empirical findings, but even for the theoretical construction of these empirical findings, might seem to suggest that the research only *illustrates* the applied method and theories.

Firstly, a naturalized vision implies what is investigated because the starting point for the investigations is based mainly on pre-constructed objects, i.e. on ideas. *Secondly*, however, stating that the theory is the starting point does not imply a refusal to discuss, for instance, the chosen categories. It only implies that these discussions must involve systematic, empirically founded theoretical reflections on the conditions of possibility for an analytical work as part of the construction of the scientific object, using and explicating the chosen theory as a frame of understanding (cf. Bourdieu, 2004, esp. p. 85ff.; Bourdieu et al., 1991). Hence, the analyses present “...a counter-description of the phenomena instead of taking over the view found in everyday language in administrative or political documents or in the heads of the participants” (Callewaert, 2006, p. 128, my translation, MN).

In other words, the starting point is following a French epistemological tradition that says “no” to that which offers itself as evident for analysis, as part of an active, conscious defence against any kind of implicit or explicit normativity, cf. Bachelard (1968), Bourdieu et al. (1991), and cf. for instance Durkheim’s *maxime* that science studies “...facts to know them, and only to know them, *in an absolutely disinterested fashion*.” (1956, pp. 92-93, my italics, MN).

However, it does not suffice to break with everything ‘out there’ that presents itself as self-evident as an essential theoretical-methodological step in the construction of the object. It is also necessary to break with everything ‘within’, with a scholastic error of the researcher projecting an observed regularity or a reconstructed social function to the analysed agents as deliberate intentions. Moreover, it is even necessary “... to objectivate everything that is linked to the scholastic universe...” (Bourdieu, 2004, p. 94): As part of the result of the research project, this approach involves the second of a double epistemological break in which the researcher systematically compensates for his own place in the field he is part of, i.e. compensates for the social struggles in the scientific field in question (cf. Bourdieu et al., 1991). These breaks involve that the researcher

takes none of the declarations in and around the evaluations or evaluation research for granted (cf. even Bachelard, 1968), and involve an analysis of the work of the researcher (MN) in relation to the scientific field in question, i.e. a field of producers of evaluation research (see Figure 1 and Figure 2 below).

Setting out from the concrete: The practical implementation

The research sets out from a spontaneous astonishment over the research connected to the evaluation of the formal training of nurses (Evalueringssenteret, 1996, cf. Nørholm, 1997, 2001), an astonishment that gradually proved to be inspired by my familiarity with Bourdieu's way of conducting social science. Despite the fact that Bourdieu did not work with evaluations or evaluation research (unless the work presented in *Reproduction* (Bourdieu & Passeron, 1992), is regarded as such), this eventually led to a more explicit awareness of at least two important circumstances:

- social science is primarily seen as a study *of* social action, not a study *for* social action
- practice is regarded not as generated through the conscious and deliberate actions of individual actors (rational choice), but as generated in an interplay between position, dispositions (*habitus*) and positioning/position-taking (*prise de position*).

These two central points seem to be contrary to the perspective applied in any formalized evaluation. Therefore, it was necessary to also take a closer look at what is generally regarded as and calls itself evaluation research³, primarily in Denmark but also internationally, to determine whether a sociological approach had been applied in theories other than Bourdieu's, respectively in Lundgren's theory referring to Durkheim and the sociological tradition. Consequently, the *doxa* of evaluation research is resumed (see below): How are evaluations defined? What implicit theoretical assumptions lie behind an evaluation practice? Etc. These questions were applied to the most frequently referenced Danish evaluation research. This tradition, identified in the research project, involves a normative approach, and consists of research *for* evaluations. Hence, evaluation research does not offer scientific research *about* evaluations etc. (scientific in a strict sense, cf. Durkheim, 1956): Practice is seen as rational action rather than generated by a *habitus*-orientation within a field.

In other words, the *doxa* of evaluation research offers no explanations of the phenomenon evaluation, of its occurrence, or of its social and symbolic function beyond the implicit and explicit presuppositions of evaluation research. Consequently, it was important to analyse and discuss the social and symbolic function of a normal vision not only of evaluations but also of evaluation *research*. This was done by breaking with the normal vision and it is shown how the presuppositions of evaluation research hinder the very explanations sought for: Instead of as a source of explanation the *doxa* of evaluation research texts poses a symbolically necessary part of an evaluation practice. Hence, the *doxa* tends to negate the position of production of scientific knowledge about evaluations. Consequently, evaluation research was constructed as a part of the evaluation practice, i.e. as an accompanying discourse (cf. Nørholm & Brinkkjær, 2005).

³ Because of the forces of the social field in question, what is regarded as and calls itself evaluation research (*doxa*) is the only thing there is. These considerations led to abandoning both this rather clumsy notion and the notion normal evaluation research. Only the notion evaluation research is used.

The analysed evaluations

The empirical material consists first of all of eight evaluations of formal training programs for the professional positions within a medical or health field. The evaluations were required by the Danish state as part of a general evaluation of all formal training programs in Denmark, and were carried out in 1996-1998. During these 2-3 years, a generally accepted way of performing evaluations was established - a paradigmatic way, establishing how evaluations should be performed properly. Even though it is never explicitly requested in the terms of reference for the evaluations, it is - e.g. - established that an evaluation should also present recommendations for improving what is evaluated. Where there are no explicit standards, the evaluations are carried out *as if* the standards existed; when there are no explicit directions, the irregularities are seen regularly, and the evaluations are performed *as if* an explicit standardized method were required.

Seven of the analysed evaluations were carried out by the Centre for Quality Assurance and Evaluation of Higher Education (now The Danish Evaluation Institute) and a uniform method was employed. The evaluations were on the formal training programs of *dentists* (Evalueringsscenteret, 1998), *occupational therapists* (Evalueringsscenteret, 1997a), *physiotherapists* (Evalueringsscenteret, 1997b), *radiographers/radiologists* (Evalueringsscenteret, 1996a), *medical doctors* (Evalueringsscenteret, 1996b), *nurses* (Evalueringsscenteret, 1996c) and *midwives* (Evalueringsscenteret, 1994). The eighth evaluation was carried out by the Adult Education Research Group at Roskilde University, evaluating the formal training for *social and healthcare assistants and helpers* (Erhvervs- og voksenuddannelsesgruppen, 1996).

The evaluations present uniform recommendations: The parts of the formal training programs allocated to a school or university should consist of more theory and the parts of the formal training programs allocated to the working place should consist of a more controlled supervised practical training. However, both parts of the formal training are based on ideas of the practical work and not on results of scientific research in a strict sense, and the recommendations imply that both parts of the formal training programs should consist of practical theory (cf. Durkheim, 1956) or accompanying discourses (Nørholm & Brinkkjær, 2005).

Hence, despite immediate differences between the evaluations and between the evaluated training programs, the same fundamental assumptions are generally found. This is also true of the collection of information used in the evaluation. In the evaluations, uniform and standardized methods are applied to recording interviews with a number of representatives at many hierarchical levels. This exemplifies how a standard or a *normal* way of performing an evaluation, a *doxa* of evaluations is established, and it is an example of how a certain standard method is both developed and applied. This is a typically instrumental positivist vision: Standardized methods are seen as guaranteeing comparable results, whereas, in fact, the opposite seems to be the case (cf. Lundgren, 1979). This is also the case regarding the implication of a theory of action without a symbolic economy of human action including speech. A similar methodological vision is found in the analysed examples of Danish evaluation research (cf. below). However, the evaluators in the analysed evaluations never perform scientific empirical studies of their own, and they never refer to the scientific empirical work of others. Stating this is not only stating a fact that seems to be a prerequisite for the evaluations to function well and without friction, it also explains why the original Bourdieu tradition was maintained in the research projects. In addition, it explains how evaluation research only gives rise to additional questions rather than answering the questions: What is, e.g., the social and symbolic function of evaluation research? Furthermore, it is convincing in that what was initially just a presumption, later became a hypothesis and finally a conviction: Evaluations of formal training programs have a social and symbolic function rather than serving to generate cognition. However, this is unintentionally obscured by the fact that (as a part of the social and

symbolic function) the results of an evaluation are fundamentally socially and symbolically necessary for a policy, just as they are socially and symbolically necessary for a professional procedure, albeit in the second instance.

Danish evaluation research

Another part of the empirical work consisted of preliminarily identifying and mapping of the *doxic* evaluation *research*, arguing for the construction of evaluation research as a part of the scientific object evaluation. Applying primarily Bourdieu's theory of social fields (cf. Bourdieu, 1984) four distinctly different, relatively dominant positions in Danish evaluation research were identified. This resulted in the construction of four positions in a field of evaluation research, of producers of Danish evaluation research (cf. Figure 1 below). A social division of labour is signalled, including a discussion of a social distribution of what seems to be at stake: Defining how an evaluation is carried out properly and a rational theory of action. Based on a thorough reading of a great number of texts labelled evaluation research (*doxa*) the selection and reconstruction is considered representative for a tradition not only in Denmark but also more generally in an above all Anglo-American tradition. This analytical work is outlined below with reference to Nørholm (2008a, 2008b, 2005).

Under the given, never-challenged circumstances a coherent research approach is identified, a paradigm of Danish evaluation research. However, this research is present only under certain naturalized social, material, historical circumstances, implying a new public management in which the evaluations are employed as managerial tools that impose certain changes necessary only under the same circumstances that are also the conditions of evaluation research. However, no empirical or systematic theoretical arguments support the assumption that Danish evaluation research helps improve practice, whether this practice is an evaluation or an evaluated formal training program. Instead, Danish evaluation research seems to offer a socially and symbolically necessary administrative knowledge that legitimizes already socially and symbolically necessary administrative decisions, as a part of a new public management.

Doxa in evaluation research implies no distinction between evaluations and evaluation research. Both evaluations and evaluation research involve the same kind of normative investigations or estimations; inasmuch as it consists of practical theory for evaluations, evaluation research therefore functions meta-evaluating evaluating the evaluations. Instead of presenting scientific knowledge about evaluations, it rather involves reproducing a social division of labour between evaluation researchers and evaluators. And in any case, evaluation research consists of practical theories for evaluations and not theoretical theories (cf. Durkheim, 1956). In other words, evaluation research is produced as an evaluation of evaluations, offering seemingly value-neutral tools for improving the evaluations in order to rectify the deficiencies in relation to, for instance, an alleged value-neutrality of the evaluations. Evaluation research is a socially and symbolically necessary part of an evaluation practice.

In itself it is not problematic if one particular approach to the purposive research *for* evaluations (*doxic* evaluation research) excludes any other approach. However, it is problematic when the *doxa* of evaluation research (a practical theory *for* evaluations) implies that it is scientific in a strict sense, which is signalled by e.g. the fact that the research is conducted by university researchers or at universities. Inasmuch as *doxa* excludes a strictly scientific position it consequently negates the position of producing scientific knowledge about evaluations. In turn this negates even the counterposition, a position of the production of political position taking towards evaluations and towards what is evaluated.

In evaluation research, a rational theory of action is presupposed or implied, fundamentally viewing human action as enlightened, conscious and rational, i.e. as generated through the conscious and deliberate acting of individual actors (rational choice), and not, as in the theories of Bourdieu, generated in an interplay between position, dispositions (*habitus*) and positioning/position-taking (*prise de position*). In evaluation research this excludes viewing and explaining human action in the light of a symbolic economy of these actions; human actions mean no more than what meets the eye. Consequently, evaluation research offers no explanation of the social and symbolic function of evaluations that goes beyond the presumptions of the evaluations or the evaluation research, nor does it offer any explanation at all. Nothing reaches beyond the immediate idea that evaluations are tools for assessing and improving the evaluated. And, from a more general perspective, no reconstructions or scientific explanations are offered for the presence of the evaluations and for why they are carried out.

However, the *doxa* of evaluation research is not monolithic. It has a great variety (cf. Nørholm, 2008a, esp. pp. 135-178). Regardless of this variety, *doxa* implies that (normatively) determining the value of the evaluated is a central task. Claiming this is uncontroversial: See for instance Scriven (1991, p. 1): “Evaluation is the process of determining the merit, worth and value of things, and evaluations are the products of that process”, or, in a Nordic setting, Vedung (1998, p. 20): “To evaluate is to scrupulously judge. To judge involves determining the worth” (my translation, MN). As another finding, *doxa* implies a certain rational theory of *structure* (function) and *genesis* of human practical action; however, this is controversial because it functions as an ideology, as it is based on ideas of the structure (function) and genesis of the evaluated, and not on analyses hereof and of the symbolic economy of human practical action (including speech) (for examples, see Scriven, 1991, Vedung, 1998). In Nørholm (2008a, esp. pp. 135-178, but even pp. 106-135) a large number of texts on evaluation/evaluation research are analysed. It is shown that albeit that the texts show a great variety, they express a homogenous position (*doxa*). Even meta-studies of evaluations and the underlying assumptions seem to neglect to analyse this symbolic economy of human practical action, and consequently seem to reproduce *doxa* (see also for instance Leeuw & Furubo, 2008). In the research project, these assumptions are challenged in an attempt to formulate an alternative to the meta-evaluating *doxa*.

The above shows how evaluation research functions as an ideology for the evaluations that, in turn, function as an ideology for the evaluated, which, in turn, also functions as an ideology. The next time round, evaluation research is constructed as a socially necessary practical theory for evaluations produced by a number of specialists who possess a double privilege: They explain both how the evaluations should be carried out and how the evaluations should be interpreted. In turn, this displays a social division of labour that places the ones doing the practical work (evaluations) in a dominated position relative to the ones who produce the directions for the practical work and the interpretations of this practical work (evaluation research). See below, cf. even Durkheim (1956).

In general, as long as everything is kept within the implicit or explicit assumptions of the evaluations, and as long as these assumptions or premises are not constructed, no explanation *can* be produced by applying evaluation research. Instead of stating this negatively, it could be stated positively: The reason why this system functions so well is that everything is kept within a certain frame of reference, within a common pre-conscious (*doxa*); nothing else is necessary under the given social conditions. However, this becomes problematic when the presupposed naturalness is questioned, as it is done here. In other words: 1. as long as the actions carried out by the participants of the evaluations or evaluation research are not objectivated or constructed under the given social conditions, and 2. as long as no systematic epistemological *break* is carried out with these assumptions, including breaking with an implicit theory of human practical action without a symbolic economy of these actions, 3. even the explanations from evaluation research, produced with the

explicit purpose of conveying an understanding of the different mechanisms involved in the evaluations, does not offer any explanations. Instead, they too are part of what needs to be explained.

On the existence of a field of producers of evaluation research

After the analysis of Danish evaluation research and referring to Bourdieu (1984, esp. pp. 126-131, 1988), who in turn refers to Immanuel Kant and his outline of a Modern university (in Kant, 1798/1998) and to Kant's discussion of the need for a 'left' position, a lower faculty (philosophy) where every truth held by the 'right' position, the upper faculties (theology, law, medicine), is challenged. Kant (1798/1998) stresses that the struggle between the faculties (cf. the title *Der Streit der Fakultäten*) is university-internal, and that it is and only can be in the interest of a democratic society. After these analysis four distinct positions are identified/constructed and an outline of a social division of labour is reconstructed. The properties used in this reconstruction are: (partly sex), position, +/- employed at a university (incl. topic), +/- reflecting on many/few evaluations, +/- participating in evaluations, seniority in the field in this generation (cf. Figure 1 below), see also Nørholm (2005, 2008b, and even 2009):

- a dominant, '*left*' position (HFH): A relatively dominant-dominated, dominant feminine position, involving major empirical works and involving uniform tasks repeated in infinity (cf. Bourdieu, 2001)
- a middle position (FH): Relative to '*right-left*' as well as up-down, acting as a sort of Trojan horse, advocating for evaluations as the source of new scientific sociological knowledge
- a middle position, relatively '*right*', middle relative to up-down (PD-L): Formulating methodologies and participating in the Danish Democracy and Power Study (Togeby et al., 2003) argues for a relative '*right*' position
- a dominant dominant, '*right*' position (EA): Involving internationalization, representation, conference talks, contributions to international method books and review works. Seniority in the field argues for a more dominant position in relation to the other positions.

In every instance, the empirical work in Danish evaluation research is "impure" (cf. Bourdieu, 1988, figure A, p. 122). The work never involves a systematic *break* with the presuppositions regarding *structure* and *genesis* of human practical action, nor does it involve theoretically founded empirical studies characteristic of science in a strict sense (cf. Durkheim, 1956). The contribution of the present research project (see Nørholm, 2008a) is to suggest a purer explanation without suggesting any improvement of the evaluations.

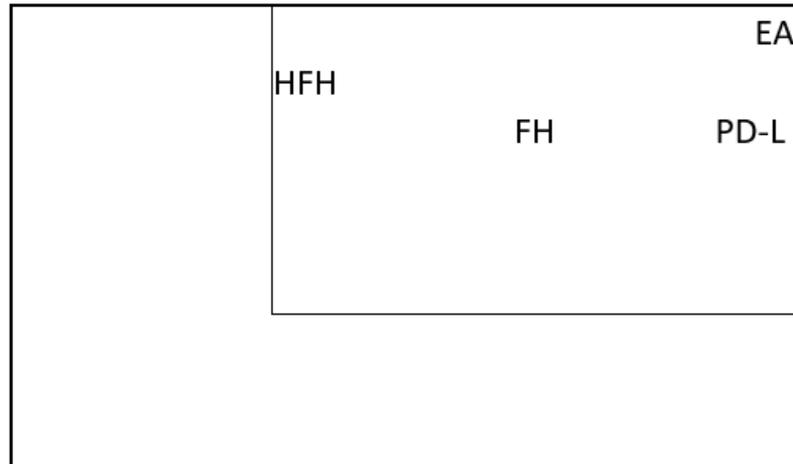


Figure 1. Preliminary construction of a field of producers of Danish evaluation research. HFH: Hanne Foss Hansen, FH: Finn Hansson, PD-L: Peter Dahler-Larsen, EA: Erik Albæk. Horizontal axis left/right: Non-temporal, scientific, cultural pole where scientific findings count in acquired positions (*'left'*), vs. temporal, academic, political-economic pole where honor and reputation count in inherited positions (*'right'*) (cf. Kant, 1798/1998, Bourdieu, 1988). Vertical axis up/down: +/- seniority in the field. The empirical work and the theoretical reconstruction behind this figure are presented in Nørholm (2008b, 2005). The properties used in this reconstruction are: (partly sex), position, +/- employed at a university (incl. topic), +/- reflecting on many/few evaluations, +/- participating in evaluations, seniority in the field in *this* generation.

Between the analysed researchers, something is *at stake* and *in play* (*illusio*). The stakes are articles, books, participation in conferences as well as in national and international networks etc.; the work is purposive, meta-evaluating to improve evaluations and indirectly to improve the order of society. Reconstructed, *illusio* is a sort of solicitude or care for evaluation research concluding firstly in improved evaluation research, secondly in improved evaluations, and finally in improvements of what is evaluated. In turn, this also implies a sort of solicitude or care for an existing society or an existing societal order.

There are certain common beliefs (*doxa*) that are seemingly uniform in the field: What characterizes a good evaluation: The same objective for a social order, the same underlying theory of action. There is a common *doxa* regarding the content of the notion of 'research': It is and should be practical theory. There is a common *doxa* regarding the relation between 'research', practical theory, and university: Practical theory is and should be produced with the premise that the directions

formulated in the purposive research are realized in practice.⁴ Different strategies for administering different, unevenly distributed capitals, and different strategies for obtaining goods are constructed. There are internal mechanisms for the distribution of reward or punishments, different mechanisms of being consecrated as an evaluation researcher in relation to the stakes mentioned. There is an exchange of references functioning as capital for the one referring as well as for the one being referred to. There are specific journals, associations, societies and notions with a relatively unique content. In other words, there is a mutual understanding, an explicit and implicit matter of course in the way something is spoken of and something else is not. But not only is something included and something else excluded. The way these certain included subject matters are discussed also shows a certain regularity. Hence, a number of characteristics for a social field are identified which in turn might argue for the existence of a relatively autonomous field of producers of evaluation research.

This repeats a self-understanding: Improving or caring for evaluation research, evaluations and for the evaluated formal training programs is a matter for the producers of evaluation research. This is the way it should be. It is claimed within the field and it goes without saying; it is part of the *doxa*. Hence, part of what is at stake is how this is done best. However, this is miscognized as arbitrary, and too, it is miscognized that both *doxa* and *illusio* is repeating and meeting demands, presumptions and implications found elsewhere, i.e. in a political-administrative field - the field of power. Neither *doxa* nor *illusio* is the result of an independent, field-*internal* work.

In other words, evaluations or evaluation research are reconstructed as tools for a public administration, as parts of a new public management, and not as tools for the production of scientific cognition. The *legitimacy*, however, is unchallenged when the production of evaluation research, functioning as an ideology ultimately for an arbitrary social order, is allocated to a field cognized and recognized as relatively autonomous - the university.

The second epistemological break

Performing a second epistemological break (see Bourdieu et al., 1991) involves subjecting the researcher (MN) to an objectivation similar to the objectivation of the analysed producers of evaluation research. In relation to the properties used in the reconstruction (MN) is positioned in

⁴ Even the implied assumption that a systematic application of the method shows how the method is applied according to the rules is uncertain: Following a rule always implies something else than just the rule: The rule never contains a rule for following a rule etc. (see Wittgenstein, 1960, §§81 ff, cf. even Taylor, 1992): A rule is needed to follow a rule: No rule is unambiguous without another rule to explain how to follow the rule. Hence, to follow a rule demands a rule for following a rule, which in turn demands a rule for following the rule for following a rule etc. *ad infinitum*. Because always and empirically, something happens and not nothing, following a rule requires something other than the rule itself; practical action is a *praxis* (Wittgenstein) or a *practice* (*pratique*) (Bourdieu) and not the following of rules (cf. even Nørholm, 2011, p. 33).

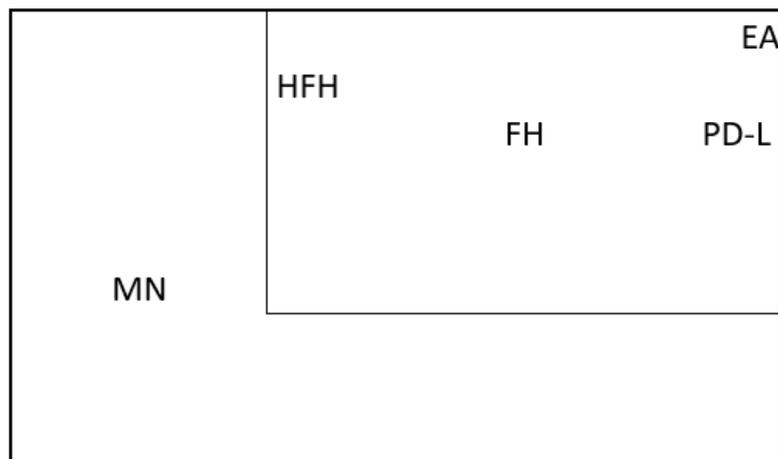


Figure 2. Preliminary construction of a field of producers of Danish evaluation research after the introduction of the researcher (MN). HFH: Hanne Foss Hansen, FH: Finn Hansson, PD-L: Peter Dahler-Larsen, EA: Erik Albæk. MN: Morten Nørholm. Horizontal axis left/right: Non-temporal, scientific, cultural pole (*'left'*), vs. temporal, academic, political-economic pole (*'right'*) (cf. Kant, 1798/1998, Bourdieu, 1988). Vertical axis up/down: +/- seniority in the field. The empirical work and the theoretical reconstruction behind this figure are presented in Nørholm (2008b, 2005). The properties used in this reconstruction are: (partly sex), position, +/- employed at a university (incl. topic), +/- reflecting on many/few evaluations, +/- participating in evaluations, seniority in the field in *this generation*.

- a dominated, extreme *'left'* position (MN): A relatively dominated, feminine position, having produced only few, strictly scientific empirical works with a very limited empirical material, not participating in evaluations, with low seniority in the field etc.

Figure 2 shows a number of significant points. 1. The work of the researcher (MN), outlining a theoretical theory about evaluations, lies outside the field of producers of evaluation research. Hence, what is produced in this position does not challenge the social order within the field of producers of evaluation research. 2. However, by showing how a theoretical theory about evaluations is absent in evaluation research, and by showing that evaluation research is produced in a political-administrative field, the field of power, i.e. decidedly *non-autonomously*, it is shown how the work of the researcher (MN) challenges the *legitimacy* of the producers of evaluation research. 3. The present research project was carried out as a Ph.D. project (in education), which might suggest the challenge were neglectable. However, challenging the legitimacy even challenges the reputation of the researchers in question, a challenge that is increasingly significant the you get closer to the temporal, academic, political-economic pole (*'right'*) (cf. Kant, 1798/1998, Bourdieu, 1988). This might suggest that the findings in this article will not be met in an open, ideal, strictly scientific

discussion but rather by means of keeping order, i.e. by the discussion of methodological questions etc. Or, rather, the findings will be neglected because they cannot be considered evaluation research (*doxa*).

In a radical interpretation of the analysis above of the field of producers of Danish evaluation research this field is constructed as a part of a political-administrative field, the field of power, i.e. decidedly *non-autonomous*. This implies that the production of an accompanying discourse (cf. Nørholm & Brinkkjær, 2005), of practical theory *for* evaluations, is also seen in a non-autonomous part of the field of power. However, the administrative allocation of this particular type of research to universities impedes seeing evaluation research as part of the basis for the legitimizing of certain education policy initiatives because these educational policy initiatives are put through *as if* they were scientifically necessary, based on research, or as the saying goes 'evidence-based'; references are made to the production of legitimacy as if it were produced elsewhere (university). An important prerequisite for these arguments is that the university is recognized as relatively autonomous and outside at least direct political control from a political-administrative power. This discussion shows how this is not always the case.

Gradually, it even seems that all research becomes purposive, practical theory. This undermines a relative autonomy of the university by negating both the position of the production of scientific knowledge and the position of producing political position taking. Generalizing the above emphasizes how scientific reason, a social conquest in line with a national health insurance, existing only under certain social conditions including a relative autonomy and hence at risk of disappearing if these social conditions change (cf. Bourdieu, 1985, p. 389), is under liquidation. This should not be seen as a movement *backwards* in time to pre-modern societies where everything is in everything, but as a movement *forwards* in time to societies *after the Modern*, after the dismantling of the characteristics of the Modern including for instance a relatively autonomous university. This is the case regarding the legitimate grounds for the imposition of administrative decisions connected to another essential tool for social sorting and reproduction: School, education, formal training programs.

In itself, it is not a problem if purposive research is carried out in the context of, for instance, university. The problems only arise when nothing else is produced, and when these produced practical theories are considered and claimed to be scientific in a strict sense. Under the given socio-historical conditions, conditions that are never taken into systematic consideration, and as if nothing else could be produced. The consequences of this process, a naturalization of one, in principle arbitrary position, are suggested here.

Discussing the findings

The analyses in the research project resulted in a construction of the social and symbolic function of evaluations of formal training programs as a societal tool for social sorting, that is reproducing already existing social differences. Furthermore, the analysis resulted in constructing evaluation research as a part of the scientific object evaluations of formal training programs as a socially and symbolically indispensable practical theory *for* evaluations produced in a non-autonomous part of the field of power. This also seems to be the case internationally: Evaluation and evaluation research seems to have the same preconditions and a similar social and symbolic function everywhere.

The analyses moreover show that an evaluation is not based solely on explicit written guidelines, for instance resulting from evaluation research. Under the given circumstances, something else seems to be employed; something implicit that ensures that the 'right', the 'natural' or the 'approved' follows by itself. If the purpose is to understand and explain this implicit 'something', it does not suffice to set out from evaluation research because evaluation research does not offer that

sort of explanations: In the evaluations the method is uniform as a result of *both* a deliberate development of a uniform method *and* a social necessitation hereof. However, the arbitrariness is never challenged; it is naturalized. The results of the research project stress how, instead of following explicit written guidelines, evaluations are carried out as position-takings in a political-administrative field, the field of power, and not in a scientific field and implying a scientific reconstruction. Fundamentally, within the chosen theoretical frame of reference every practice in any field is performed practically with *habitus* as a generative principle. In this case the *habitus*es are opposed or even antagonistic: On the one hand, there are political position-takings generated by a political-administrative *habitus*, on the other hand, there are scientific reconstructions generated by a scientific *habitus* and characterized by the researcher systematically compensating for his or her *habitus* to control the influence of the *habitus*. See also the discussion in Nørholm (2011).

The analyses show how the analysed evaluations imply a theoretical framework similar to that of evaluation research. However, the implication is *not* that the evaluations are carried out as they are *because* evaluation research looks the way it does, or vice versa. Rather, evaluations, evaluation research etc. are simultaneously necessary parts of an evaluation *practice*. In addition, although evaluation research, a practical theory *for* evaluations, is produced at what is cognized and recognized as *another place*, disconnected from the evaluations (e.g. at a university). It is not possible to explain and understand evaluation research separately from the evaluations or vice versa. Evaluation research is a socially and symbolically necessary part of the evaluation practice, just as this *other place* is constructed in an epistemological *break* with an immediate vision (cf. Bachelard, 1968).

Everything appears to be sensible and natural. However, as soon as the implicit assumptions are reconstructed and challenged, e.g. the implied notions of action, knowledge, science, evaluation, theory etc. and their implications, everything turns out as arbitrary. The intention in the analyses is not to show how the existing paradigm is *wrong*. However, this well-functioning societal mechanism turns out to be problematic, and it gives rise to problems when so many conditions and circumstances are presupposed and left unchallenged. In this article it is argued that when the arbitrary is naturalized, it has very specific social and symbolic consequences. This leads to another main point in explaining and understanding a new meritocratic model for public management.

Presupposing that a uniform method ensures uniform and comparable results seems rather to have the opposite effect: A uniform method applied generally ensures *incomparable* results, not comparable as it is implied. If the tools for examination are not adjusted to what is examined, this in itself prevents the investigation from producing the results claimed. And it even supports stating that administrative interventions are performed by something else than systematic knowledge or insight (i.e. by an administrative *habitus*). This is supported by Lundgren (1979) and Lundgren & Franke-Wikberg (1980). This should not be read as the pointing out of an error. It is merely an uncovering of yet another *regularity* that should be taken into consideration when the focus is on the social and symbolic function of evaluations of education or on any evaluating practice.

This social and symbolic function does not depend on the evaluation being carried out in accordance with externally or internally formulated criteria, at least not in principle, primarily because the evaluations are carried out *pr. habitus*. But the *strength* and *legitimacy* depend on the cognition and recognition of the criteria *as if* they were external. The social and symbolic function of the evaluations of formal training programs becomes to perform and legitimize a sorting of the evaluated formal training programs. However, this is uncontroversial: The whole idea and rationale of an evaluation is to perform some kind of sorting. But, because all that is said in the analysed evaluations consists of a dominant accompanying discourse (cf. Nørholm & Brinkkjær, 2005) constituting what *can* be said under the given, unchallenged conditions (*doxa*), and because everything is carried out as position-takings, produced practically with *habitus* as the generating

principle and not through scientific reconstruction, eventually the sorting reproduces already existing social differences, i.e. already existing social relations of dominance. This is, however, illegitimate if stated as an explicit purpose. A necessary precondition for the evaluations to be cognized and recognized as fair and natural is therefore that the *criteria* are cognized and recognized *as if* they were external. Another precondition is that the *instances* performing the evaluations are cognized and recognized *as if* they were external. The research project shows that this is not the case.

Concluding remarks

In order to understand and explain the analysed evaluations of formal training programs as appearances in societies after the Modern, it proved necessary to construct the relation between these evaluations and evaluation research. In turn it proved necessary to construct evaluation research as a part of the scientific object evaluation. This theoretical reconstruction points towards an outline of the social and symbolic function of evaluations as a part of a new public management in societies after the Modern.

In the analyses a coherent rationale is constructed. Everything seems to be functioning function-free, without friction: Arguing for the legitimacy of a normal approach does not only seem unnecessary under the given, unchallenged conditions; to do so would indeed amount to a profanation of the field's most sacred presuppositions, of its *doxa* (cf. also Lundin, 2008, p. 11). Under the given, never challenged circumstances nothing but *doxa* exists, nothing but *doxa* can exist. In the research project (see Nørholm, 2008a) it is also shown that all the different parts - evaluation, evaluation research, encyclopaedia, dictionaries etc. - constitute conditions that, under the given social circumstances, are separately necessary for designing the evaluations and the evaluated formal training programs as a continuation of and a condition for a certain vision of the relationship between theory and practice, or more precisely, between formal schooling and a practical mastery. Not only does this vision lie in the continuation of a normal, widespread vision, it is even an essential part of the foundation for the legitimacy of school and the educational system. Consequently, the social and symbolic function of evaluations of formal training programs is to reproduce and legitimize already existing social relations of dominance *as if* these relations of dominance were universal and so that the existing, in-principle arbitrary relations of dominance are cognized and recognized as universal, natural relations of dominance.

The specific way in which the formal training programs are designed may resemble an education policy formulated in advance. However, this policy could not be realized without evaluations that *neither* ask general questions *nor* offer general answers, but instead are asking the questions and offering the answers that *can* be asked or offered - under the given circumstances, which systematically are never challenged, analysed or taken into consideration. In turn, this implies a collective amnesia with regard to both the social history (*genesis*) and the function (*structure*) of the employed tools: Instead of appearing as socially arbitrary societal tools having socially arbitrary conditions and being designed at an arbitrary time and place, the evaluations and the evaluation research appear as absolute tools under absolute circumstances, out of time and place.

What is seen is a reproduction of one specific vision of the relationship between evaluation and evaluation research and between formal schooling and a practical mastery, implicating a so-called rational theory of action. More generally, this shows the reproduction of a social division of labour (cf. Durkheim, 1956):

- in a dominant position: The ones who legitimately *talk about* practical actions, who symbolically deal with how practical actions should be performed and understood, most often without performing them themselves; the practical actions are mastered *symbolically*
- in a dominated position: The ones who perform the practical actions in question; the practical actions are mastered *practically*.

The evaluations, evaluation research etc. contribute to the reproduction of this social division of labour and, more generally, to the reproduction of in principle arbitrary social relations of dominance. As well-functioning societal mechanisms evaluations and evaluation research seem to be parts of the way an economic-administrative pole is imposing and cementing one specific, existing, in principle arbitrary view and thereby imposing and cementing one specific, existing, in principle arbitrary social order at the cost of any other.

The analyses show how evaluations of formal training programs are but one of the steps in a sophisticated four-step societal tool for social sorting: Evaluation research is sorting the evaluations of formal training programs, sorting formal training programs, and in turn sorting students, apprentices and pupils. Because of the different administrative allocations of the various parts of an evaluation practice, the latter amounts to and implies a social division of labour in relation to educational level (cf. Durkheim, 1956, Bourdieu & Passeron, 1992) and in relation to an allocation to different places in a social hierarchy, a meritocratic stance; not only are the evaluations tools for sorting, which is uncontroversial, the evaluations are moreover sorting in accordance with already existing social differences and thereby cementing and expanding already existing social differences. In other words, this four-step societal sorting tool functions by and through an imposition, cementing, reproduction and expanding of an *existing* and *in principle arbitrary* social relation of dominance at the cost of any other. Because the sorting at any hierarchical level is performed in accordance with already existing social differences at any hierarchical level, the interests considered are the interests of the dominant groups and not of any group; the evaluations are partial. Consequently, an expert or representative *taht* takes part in performing the evaluations and a general imposition of dominant interests in favour of existing arbitrary social relations of dominance should be seen as two faces of the same coin.

Evaluations of formal training programs accompanied by evaluation research as an accompanying discourse (cf. Nørholm & Brinkkjær, 2005) are but one out of many societal tools active in uniforming or standardizing quality assessments. The evaluations are administrative tools in a new public management in which employees at any hierarchical level participate in the evaluations and hence become accomplices of, cognizing, recognizing *and claiming* the fairness of a social sorting by means of these administrative tools. The evaluations are tools for separating the 'wrong' from the 'right' and for uniting the 'right' by uniforming the way things can be done legitimately (*doxa*). The presupposition of a new public management model, that nothing exists unless it has been properly evaluated, adds to the strength and extent of the evaluations as partial administrative tools.

It is hardly surprising that a societal order is maintained via sophisticated social mechanisms. On the other hand, it is important to investigate why and how the strength of these mechanisms depends on nothing being said outright and is instead called and recognized as something else (education or training, evaluations, evaluation research etc.). It is also important to investigate why and how administrative and managerial initiatives are legitimized by applying tools (education or training, evaluations, evaluation research etc.) produced in contexts that are not immediately recognized as parts of an administrative or managerial system.

In the current research project the focus was on the social and symbolic function of evaluations of *formal training programs* and on a socially and symbolically necessary *evaluation research*, an accompanying discourse (cf. Nørholm & Brinkkjær, 2005) to the evaluations. However, in principle

there is no difference between evaluations of formal training programs and any other evaluation which gives rise to the claim that the social and symbolic function of evaluations as such is equivalent to that of evaluations of formal training programs. The same argument holds for the analysed *evaluation* research and any other purposive research.

In a broader perspective, the findings with regards to evaluations of formal training programs (see Nørholm, 2008a) may be applied to any institutionalized evaluation: Because they are performed *pr. habitus* and not *pr. explicit rule*, any judgement is fundamentally a judgement of *taste* and seems to presuppose social hierarchies (cf. Bourdieu, 1984); some judgements are more proper than others. The evaluations are performed by *habitus* which has the consequence that since they are applied by *habitus*, that is *pr. body*, the criteria are *secret*. This being the case *because* the criteria are explicit and not in spite of it.

All this is enhanced by a meritocratic, neoliberal new public management model.

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