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Ontography as the Study of Locally Organized Ontologies

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I first used the term »ontography« in the postscript of a special issue of the journal *Social Studies of Science* on »the turn to ontology« in the field of Science and Technology Studies (STS).[1] In their introduction to the special issue, the guest editors, Steve Woolgar and Javier Lezaun, noted that there had been an upsurge in the use of the term »ontology« in STS, anthropology, and other social science fields.[2] Woolgar, Lezaun, and other contributors to the special issue expressed both heightened interest in and skepticism about this »turn to ontology.« The »turn« in question was not a turn *back* to an ancient branch of metaphysics concerned with the constitution of reality, though some of the authors who embraced it did indulge in metaphysical discourse. To the extent that it was a metaphysical movement, the turn to ontology was a turn from one metaphysical orientation to another; from a humanist treatment of representation and meaning-making, to a post-humanist (re)distribution of agency among humans, non-human beings, and material things. This turn was an empirical turn that aimed to investigate particular domains of social action (including but not limited to scientific, technical, and medical actions) in which things (though not necessarily things-as-such; self-sufficient, non-relational entities) were highly salient, not only as objects but also as agents, agencies, and intermediaries.

The turn to ontology is sometimes presented as a reaction to an over-emphasis on epistemology in earlier work in STS and related fields, particularly work associated with social constructionism in the sociology of scientific knowledge, which seemed to reduce obdurate material realities to historically, socially, and culturally situated practices and knowledges. Turning to ontology did not necessarily represent a reversion to scientific realism, but it did invite renewed inquiry into the resiliency and robustness of things and a search for novel ways to conceive of things as other than docile expressions of purposive human practices and/or reified

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1   Michael Lynch: Ontography: Investigating the Production of Things, Deflating Ontology, in: Social Studies of Science 43/3 (2013), pp. 444–462.
2   Steve Woolgar and Javier Lezaun: The Wrong Bin Bag: A Turn to Ontology in Science and Technology Studies?, in: Social Studies of Science 43/3 (2013), pp. 321–340.

ideologies. For some writers, the turn to ontology was a way to turn away from an exclusively humanistic conception of action and agency in favor of a post-humanist treatment in which ultimate sources of agency are left open in fields or networks of humans, technologies, non-human beings, and things.[3]

In the postscript of the special issue, I suggested the term »ontography« as an alternative to »ontology« that would be more congruent with a descriptive orientation to social, cultural, and historical conceptions of things, beings, and relations among them. At the time, I thought »ontography« was a neologism, and only after I had begun drafting the paper did I conduct a word search that informed me otherwise. My search turned up various conceptions of »ontography« in philosophy, cognitive science, and other fields, all of which differed from what I intended with the term. However, I did not mean to propose an entirely original idea, because I derived »ontography« from another obscure term, »epistemography.« Before proceeding with what I mean by »ontography,« allow me to give a brief gloss on »epistemography.«

1. From -ology to -ography

Historian of science Peter Dear uses the term epistemography to draw a contrast between science studies (historical and social studies of scientific discourse and practice) and philosophical epistemology.[4] He emphasizes that the former term is descriptive while the latter one is normative. According to Dear:

»The term ›epistemography‹ is intended to bring some clarity to the discussion by proposing a loose grouping of the most central and characteristic kinds of work currently encompassed by the label ›science studies.‹ The grouping strategy relies on making explicit the following recognition: the field of science studies is driven by attempts to understand what science, as a human activity, actually is and has been. Epistemography is the endeavor that attempts to investigate science ›in the field,‹ as it were, asking such questions as these: What counts as scientific knowledge? How is that knowledge made and certified? In what ways is it used or valued? ›Epistemography‹ as a term signals that descriptive

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3   See, for examples, Bruno Latour: Science in Action: How to Follow Scientists and Engineers through Society, Cambridge, MA 1987; Bruno Latour: An Inquiry into Modes of Existence, Cambridge, MA 2013; Andrew Pickering: The Mangle of Practice: Time, Agency, and Science, Chicago 1995; and Annemarie Mol and John Law: Regions, Networks and Fluids: Anaemia and Social Topology, in: Social Studies of Science 24/4 (1994), pp. 641–671.
4   Peter Dear: Science Studies as Epistemography, in: Jay Labinger and Harry Collins (eds.): The One Culture? A Conversation about Science, Chicago 2001, pp. 128–141.

focus, much like ›biography‹ or ›geography.‹ It designates an enterprise centrally concerned with developing an empirical understanding of scientific knowledge, in contrast to *epistemology*, which is a prescriptive study of how knowledge can or should be made.«[5]

Dear adds in a footnote that »[t]he suffix ›–ography‹ should not be taken to indicate anything more specific than ›description‹ in the widest sense; it need not imply spatial description (akin to ›cartography‹), for example, although it could well do so in particular cases.«[6] The suffix also resonates with the ethnographies of laboratory and field science that made important contributions to science studies in the 1980s and afterwards, and also with a particular understanding of historiography. As usually understood, historiography is the study of methods of historical writing, but epistemography turns attention to the performative ›writing‹ of history enacted by historical actors. This is not writing *about* history, though it could include such writing; instead, it refers to the writings (and practices) of natural philosophers and scientists that constitute the historical record.

The contrast to epistemology is especially clear when we consider the highly influential (in public discourse, if no longer in philosophy of science) writings of Sir Karl Popper. Dear points out that Popper not only formulates methodological recommendations, he loads those recommendations with moral and political significance. Popper's demarcation criterion of falsifiability, for example, lends itself to being applied well beyond the philosophy of science as a basis for political reform.[7] Falsifiability, according to Popper, allows a critical analyst to distinguish between ›confirming‹ the doctrines of a pseudoscience and successfully contributing to the progress of genuine science. For Popper, the robust popularity of a doctrine or belief system is no measure of its scientific value. Popper's negative examples (Freudian and Adlerian psychoanalytic theory; the Marxist theory of history) have been highly influential and can be said to have had long-lasting (if mixed) success in public life as well as academia. Astrology and alchemy persisted for thousands of years, despite (or, rather, because of) their lack of falsifiability. Even though Popper's recommendations might seem to imply that neglecting falsifiability will lead to failure, his negative examples indicate quite the opposite. Belief systems that cannot be proved false may be sustained indefinitely, whereas falsifiable theories are forever liable to be shot down. If one adds to the picture the possibility of ›false refutation,‹ then the picture gets even darker. However, for Popper, his demarcation criterion enables a wedge to be driven between popular beliefs in and beyond academia and genuine (if tentative) knowledge.

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5 Ibid., pp. 131–132.
6 Ibid., p. 131, n.
7 Karl S. Popper: Conjectures and Refutations, London 1963.

It is no accident that on the rare occasions when high courts invoke the philosophy of science, the name of Popper tends to be cited (Edmond & Mercer, 2002).[8] For example, in a key US Federal District Court Case, *McLean v. Arkansas*, the judge (William Overton) ruled against the Arkansas state legislature's attempt to provide equal time for the teaching of »creation science« (a secularized version of the creation story in the Book of Genesis) whenever the Darwinian theory of evolution was taught in public (state supported) school science classes.[9] One basis for ruling that creation science was a religious doctrine masquerading as a science was a list of »essential characteristics of science,« one of which was that genuine science was »falsifiable.« Judge Overton's ruling cited philosopher of biology Michael Ruse, an expert witness in the case who testified that creation science was unfalsifiable.[10] Ruse was criticized by other philosophers for promoting Popper's demarcation criterion, which by then had been rejected by many philosophers of science, and for confusing the issue as to why creation science should be rejected.[11] Laudan argued that many of the doctrines of creation science *were* falsifiable, and in fact *false*, and that Darwin's theory, when taken as a whole, was unfalsifiable but highly credible and unquestionably scientific.[12]

Another US Supreme Court case in which Popper's demarcation criterion was invoked concerned the question of how courts should rule on the admissibility of expert testimony. In the landmark *Daubert v. Merrell Dow Pharmaceuticals* case, the court reviewed competing standards for admitting scientific and technical evidence.[13] (Admissibility refers to judicial allowance for specific evidence to be presented in a trial court.) The court received *amicus curiae* briefs from a number of STS scholars and philosophers of science on questions pertaining to the criteria for counting a domain of practice as »scientific.« Once again, the issue of falsifiability (more broadly construed in connection with »testability«) was cited as an important consideration. And, once again, Popper was mentioned as an authority

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8   See Gary Edmond and David Mercer: Conjectures and Exhumations: Citations of History, Philosophy and Sociology of Science in U.S. Federal Courts, in: Law & Literature 14 (2002), pp. 309–366.

9   *McLean v. Arkansas* (529 F. Supp. ED Ark. 1982).

10 Michael Ruse: Response to the Commentary: Pro Judice, in: Science, Technology, & Human Values 7/41 (1982), pp. 19–23; Michael Ruse: Commentary: The Academic as Expert Witness: in Science, Technology, & Human Values 11/2 (1986), pp. 68–73.

11 For criticisms of Ruse's testimony, see Larry Laudan: Science at the Bar: Causes for Concern, in: Science, Technology, & Human Values 7/41 (1982), pp. 16–19; Philip Quinn: The Philosopher as Expert Witness, in: J. T. Cushing, C. F. Delaney, and G. Gutting (eds.): Science and Reality: Recent Work in the Philosophy of Science. Notre Dame, IN 1984, pp. 32–53.

12 Ibid., p. 16.

13 *Daubert v. Merrell Dow Pharmaceuticals* (509 U.S. 579, 1993).

on the subject, and once again some philosophers criticized the court's citation of Popper as irrelevant and misguided.[14]

Rather than try to take up the philosophical challenge to specify criteria for demarcating science from non-science, a different project is possible for social-historians and sociologists, which is to treat debates about such matters descriptively, as themselves *phenomena* of interest. This perspective is concisely articulated by Thomas Gieryn under the theme of »boundary work.«[15] Gieryn points out that »[e]ven as sociologists and philosophers argue over the uniqueness of science among intellectual activities, demarcation is routinely accomplished in practical, everyday settings.«[16] An effort to describe boundary work makes no initial commitment to whether or not, and if so how, such boundaries should be drawn in general or in any specific case. In line with Dear's conception of epistemography, the task is a descriptive one: to provide documentary accounts of how courts of law and other institutionally embedded agencies constitute epistemic boundaries in particular cases.

Such a descriptive project contrasts to normative approaches to epistemology. Dear's epistemography is ›conservative‹ in the sense that it does not critique the practical and historical circumstances in which epistemological categories and distinctions are invoked, refashioned, and instituted. In another sense, however, epistemography is radical in the way it resists the demand to posit (or presuppose) historically and culturally transcendent grounds of inference and action. It is an explicitly relativistic approach, but the relativism is a restricted version that is compatible with making local judgments about what is, was, and can be taken as real and true. (How else could it be descriptive?) Such local judgments are contingent, and do not propose any general formula or argument that would provide a stable basis for making discriminations between true and false knowledge.[17] Moreover, epistemographic investigations would not necessarily provide general, historically stable, criteria for certifying knowledge. Dear's epistemography also is broadly compatible with an ethnomethodological »respecification« of key methodological and epistemological topics. Both approaches refuse the epistemic privilege so often assigned to, or presumed by, the ›professional analyst‹ to specify the *real* reasons,

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14 See, for example, Susan Haack: Trial and Error: The Supreme Court's Philosophy of Science, in: American Journal of Public Health 95/S1 (2005), pp. S. 66–73.

15 Thomas Gieryn: Boundary-work and the Demarcation of Science from Non-science, in: American Sociological Review 48 (1983): 781–795.

16 Ibid., p. 781.

17 In this respect, epistemography is consistent with the Strong Programme in the Sociology of Scientific Knowledge, as espoused by David Bloor: Knowledge and Social Imagery. London 1976, p. 6.

motives, and social circumstances that supposedly drive situated actions.[18] Recurrent *topics* associated with epistemology—and, not incidentally, social science methodology—become situated phenomena to be described in circumstantial detail rather than posited as generalized schema. In studies of scientific practices, such topics include observation, representation, coding, classification, measurement, analysis, and the reproduction of findings. Along these lines, Christian Greiffenhagen, Michael Mair, and Wes Sharrock introduce another ›graphy‹ term, *methodography*, that would describe the ›on the ground‹ performance of methodological practices in social science.[19]

Epistemography, as I understand it, is not a branch of philosophy, though it reacts to and selectively addresses topics that often are central to philosophical investigations. Philosopher Peter Winch in his landmark *The Idea of a Social Science*, argued that studies of human actions cannot be scientific in the same sense as studies of non-human phenomena, because social science descriptions and explanations employ concepts of a »second order« that are drawn from, and intelligible in terms of, the vernacular language that constitutes the phenomenal fields that social scientists study.[20] Winch recommended that, instead of supposing that they are characterizing and explaining previously unknown objects in, and properties of, the world, social scientists (or rather social philosophers) should recognize that they are engaged in an epistemological project. Accordingly, they should seek to *explicate* rather than *explain* the conceptual and discursive constitution of social actions. Epistemography proceeds from such a recommendation by rejecting armchair inquiry and the philosophical baggage of epistemology, undertaking instead to compose historical and ethnographic descriptions of particular historical productions and contemporary cases. I once used the term »epistopic« to bring into relief the way topics of epistemology were subject to respecification in specific case studies.[21] Examples of such topics and studies that respecified them as contingent social–historical productions are »matters of fact«; replication of experiments; observation; perception; objectivity; pluralized and located »knowledges«, and

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18   See Harold Garfinkel: Ethnomethodology's Program: Working out Durkheim's Aphorism, Lanham, MD, 2002; and Graham Button (ed.): Ethnomethodology and the Human Sciences, Cambridge 1991.

19   Christian Greiffenhagen, Michael Mair and Wes Sharrock: From Methodology to Methodography: A Study of Qualitative and Quantitative Reasoning in Practice, in Methodological Innovations Online 6/3 (2011), pp. 93–107, available at http://journals.sagepub.com/doi/pdf/10.4256/mio.2011.009 (30.01.2019).

20   Peter Winch: The Idea of a Social Science and its Relation to Philosophy (1958), London 1990.

21   Michael Lynch: Scientific Practice and Ordinary Action: Ethnomethodology and Social Studies of Science, Cambridge 1993, pp. 275 ff.

representation.[22] It is notable that many of these studies were done decades ago, at a time when the field of Science and Technology Studies (STS) was still forming and becoming institutionalized. Currently, the field has a relatively stable and expanding literature, professional associations, journals, and so forth, which provide a recognized organizational identity. Normative and politicized approaches are now in the ascendency, though they are not necessarily incompatible with the descriptive approach.

2. From Epistemography to Ontography

Like epistemology, ontology carries the baggage of metaphysics. Indeed, the very distinction between epistemology and ontology imposes a classical distinction upon inquiry into what Latour dubs »modes of existence.«[23] Karen Barad hyphenates »onto-epistemology« in order to signal a refusal to separate being from discourse, and agency from materiality.[24] This refusal to impose a stable demarcation sets up Barad's effort to elucidate the particular ›cuts‹ between agency and reality that are imposed in particular cultural and historical circumstances.[25]

If we take seriously the refusal to assign ontology to a broad domain of things-as-such, and epistemology to the human historical project of knowing the world,

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[22] On matters of fact, see Steven Shapin and Simon Schaffer: Leviathan and the Air Pump, Princeton 1985; on replication, see H. M. Collins: Changing Order: Replication and Induction in Scientific Practice. London 1985; on observation, see Trevor Pinch: Towards an Analysis of Scientific Observation: The Externality and Evidential Significance of Observational Reports in Physics, in Social Studies of Science 15/1 (1985), pp. 3–36; on perception, see Jeff Coulter and E. D. Parsons: The Praxiology of Perception: Visual Orientations and Practical Action, in: Inquiry 33 (1991), pp. 251–272; on objectivity, see Lorraine Daston and Peter Galison: Objectivity, Brooklyn, NY 2007; on knowledges, see Donna Haraway: Situated Knowledges: The Science Question in Feminism as a Site of Discourse on the Privilege of Partial Perspective, in: Feminist Studies 14/3 (1988), pp. 575–599; on representation, see Michael Lynch and Steve Woolgar (eds.): Representation in Scientific Practice, Cambridge, MA 1990; and Catelijne Coopmans, Janet Vertesi, Michael Lynch and Steve Woolgar (eds.): Representation in Scientific Practice Revisited, Cambridge, MA 2015.

[23] Latour: An Inquiry into Modes of Existence (as note 3).

[24] Karen Barad: Agential Realism: Feminist Interventions in Understanding Scientific Practices, in: Mario Biagioli (ed.): The Science Studies Reader, New York, NY 1999, pp. 1–11.

[25] See Latour: Science in Action (as note 3), and Pickering: The Mangle of Practice (as note 3) for efforts to avoid preferentially assigning agency to humans and materiality to non-human entities, and Hans-Jorg Rheinberger: Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube, Stanford, CA 1997, for a hybrid conception of »epistemic things.«

then the »turn to ontology« would turn full-circle, returning to the phenomenal fields in which *knowing* and *what* is known are grammatically and materially in-separable and only provisionally differentiated. This would not be a turn back to epistemology, but rather to a primordial being-in-the-world from which innu-merable distinctions spin out. Consequently, ontography would occupy the same initial standpoint (or standpoints) as epistemography. Studies of the production and use of *evidence*, disputes about *matters of fact*, and descriptions of the technologies and networks through which *data*, *facts*, and *metrics* are disseminated, would be no more ontological than they ever were epistemological. Ontography would not be an inquiry into things as such, any more than epistemography is an inquiry into ideas. And, if we throw ethics into the picture, we have a more elaborate nexus of implications: epistemography would invariably cross over into ontography, and both would involve ethical considerations (spurring investigators to engage in ›ethigraphy‹ (or, perhaps, ›ethicography‹) as well.[26]

Appending the –ography suffix to a root word previously appended by –ology does not always produce a predictable derivation, but following the example of epistemography, my conception of ontography is intended to cover investigations that aim to describe the contingent and organizationally embedded work of social agents to propose, inscribe, or dispute particular ontological matters. In both epis-temography and ontography, the aim is not to transcend the particular discursive fields but to characterize their practical operations. Such characterizations cannot transcend *our* vernacular usage, however broadly or narrowly ›we‹ conceive of ›us,‹ and regardless of how much or how little technical language such characteriza-tions deploy. A possibility that arises from such characterizations is that they will not trace back to metaphysics, but will instead float in a sea of ordinary language in which conceptual distinctions and variations in usage abound. Granting the ultimate intertwining of subjects with objects, concepts with observations, values with facts, and knowledge with what the knowledge is of, the question is whether ontography would provide a *relatively* distinct descriptive shading when compared with epistemography.

It may be preferable for the time being to leave this question unanswered. If we take, for example, *matters of fact* as a topic, we might suppose that such facts clearly are ontological matters. But, once we begin to look into the matter, it gets more complicated. For Nobel Laureate Steven Weinberg, a physical law is like a rock in the field: »we did not create the laws of physics or the rocks in the field,

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26   On ›ethigraphy‹ see Michael Lynch: The Epistemology of Epistopics: Science and Tech-nology Studies as an Emergent (Non)discipline, in: American Sociological Association, Science, Knowledge & Technology Section (ASA-SKAT) Newsletter (Fall, 2001), pp. 2–3.

and we sometimes unhappily find that we have been wrong about them, as when we stub our toe on an unnoticed rock, or when we find we have made a mistake (as most physicists have) about some physical law.«[27] But, for Ludwig Wittgenstein, facts (and this would also apply to physical laws) are true propositions, necessarily expressed in language (albeit in some cases mathematical symbols), and thus they differ from things like rocks, which can neither be true nor false.[28] One might distinguish a real rock from a fake rock, such as one used as a prop for a theatrical production, but this distinction requires situational framing. Also, consider such topics as »reality,« »raw data« (big or small), and »nature«. Are they on the ontological side of the ledger, while ideas, interpretations, and linguistic expressions are on the epistemological side? Initially, it might seem so, but very quickly we can find it difficult and contentious to separate *real* reality from contestable claims about what is real, and we may also find similar problems with distinctions between data and interpretations, facts and values, and natural entities and artifacts. Questions about such concepts and distinctions are not only of interest to philosophers, as they also are subject to ethical, legal, and social disputes. For example, with words similar to Weinberg's, Chief Justice Phil Hardberger, of the Texas Supreme Court, delivered the verdict in the case, *Littleton v. Prange* with the pronouncement: »There are some things we cannot will into being. They just are.«[29] Hardberger was referring to plaintiff Christie Littleton's gender, insisting that she had no right to inheritance as the spouse of her deceased husband, because her status as a male at birth was a baseline fact that remained unchanged and negated the legality of her marriage under Texas law, despite changes in her name, birth certificate, social-identity, and anatomy that Littleton had undergone since her birth. Needless to say, Hardberger's immutable fact has proved to be more mutable than he claimed.

Although Hardberger's pronouncement invites spirited objections (and, indeed, the ruling was a topic of dissent at the time for one of the three justices who heard the case, and for long afterwards in legal and popular commentaries), an ontographic account would examine and describe the social, historical, and legal circumstances of such pronouncements. Similarly, ontographic consideration of particular assertions about »raw data« and »what the data show,« would not begin by contesting such assertions with counter-claims to the effect that there is no such thing as raw data; instead, the aim would be to examine the practical and polemical relations in which such usage is embedded.[30] Nor would *nature* be de-

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27 Steven Weinberg: Sokal's Hoax, in: The New York Review of Books 63/13 (1996), pp. 11–15.

28 Ludwig Wittgenstein: Tractatus Logico-Philosophicus (1922), London 1961, Sec. 1.1.

29 *Littleton v. Prange* (9 S. W. 3d., 1999).

30 Compare to essays in Lisa Gitelman (ed.): Raw Data is an Oxymoron, Cambridge, MA 2013.

clared dead on arrival, or claims about what is *objective* be dismissed as meaningless or misleading. The initial problem would be to recover the intelligibility of such ontological categories and distinctions based on how they are used in particular settings, and to investigate the *work* they do in such settings.

3. Artifacts and Natural Things

One widely employed ontological distinction is between artifacts and things of nature. It is a distinction used in many natural sciences, and in an inverse way in archaeology. A variant of that distinction also comes into play in law, particularly in connection with intellectual property claims, and also in connection with disputes in the USA concerning the question of whether the U.S. Constitution prohibits publicly funded schools from teaching creationist »theories« of life on earth, or even presenting the secularized doctrines of »Intelligent Design« as a »scientific« alternative to the theory of evolution by natural selection. Using a variant of the argument from design used by Rev. William Paley in the early 19th century, proponents of Intelligent Design assign natural order to the handiwork of an unspecified intelligent designer. According to such an assignment, there no longer is a distinction between artifacts and natural things; instead, there is a distinction between human-made artifacts and those designed by the transcendental agent.[31]

In many natural sciences, the ability to detect artifacts and to discriminate between artifacts and »natural« phenomena is a technical achievement. A close study of how it is done in particular cases does not necessarily contribute to general ontological definitions of how artificial kinds differ from natural kinds, but it can be informative for how participants in various fields of science and law, as well as in many other social practices, organize domains of discourse and praxis. That distinction was featured in an ethnography I conducted decades ago in a neuroscience laboratory which used electron microscopy and other techniques to examine brain plasticity—in this case, rearrangements of neuronal organization following a lesion to the mammalian brain (with laboratory rats being the most often used animal model).[32] At the time, there was a great deal of discussion and debate in science studies about the »social construction« of scientific practices.[33] Constructivist

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[31] The status of Intelligent Design as a scientific theory or religious doctrine was adjudicated in a Federal District Court in Pennsylvania: *Kitzmiller et al. v. Dover Area School Board* (500 F. Supp. 2d 707 M.D. Pa., 2005).

[32] Michael Lynch: Art and Artifact in Laboratory Science: A Study of Shop Talk and Shop Work in a Research Laboratory, London 1985.

[33] For examples and discussions of constructivist ethnographies, see, Bruno Latour and Steve Woolgar: Laboratory Life: The Social Construction of Scientific Facts, London

interpretations were readily (and some would say, deliberately) confused with the vernacular distinction used in many scientific laboratories between provisionally accepted »natural« features and those entities and evidential features consigned to »artifacts« (akin to electromagnetic »noise« that interferes with the signal detected by an instrument). After noting that archaeologists preferentially treat artifacts as discoveries, while discarding the material that surrounds them, while electron microscopists attempt to eliminate artifacts that intrude upon fields of anatomical features and sometimes can be mistaken for subcellular entities, I proposed to conduct an »archaeology of artifacts«—what I would perhaps now dub an »archae-*ography* of artifacts.«[34]

For that study, I interviewed and recorded discussions among members of a small team of researchers conducting an electron microscopic project, and paid close attention to what they said about the artifacts they came across, and how they handled those they noticed. I also familiarized myself with the visual materials, technical language, and relevant background literature. I then developed a rough typology of different artifacts and their consequences for the project at hand. Some were consigned to visual defects that marred the appearance of visual documents. Such artifacts did not necessarily inhibit the analysis of the micrographs in which they appeared, although the micrographs in which they were found would not be selected for display in public presentations and published articles, since they would expose the laboratory to negative assessments of the preparatory work that rendered brain tissue into micrographic data. Other artifacts were assessed as potential sources of confusion with the subcellular anatomical entities under study, and as ruining the specimen materials from usability as data. Although I did not use the term »ontography« at the time, my study exemplified an effort to write about particular deployments of an ontological distinction for practical and epistemic purposes in an organized domain of activity.

In the very different field of intellectual property law (an area of law that often has some connection to the sciences), a related distinction comes up. This is the distinction between »products of nature« and »compositions of matter« in Anglo-American patent law (similar distinctions also appear in other national and international legal systems). The application of the distinction in particular cases is highly consequential and also fraught with controversy. Patent examiners and judges in the U.S., Canada, the E.U. and elsewhere do not fancy themselves to be engaging in metaphysics when they make judgments about particular cases, but

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1979; Karin Knorr-Cetina and Michael Mulkay (eds.): Science Observed: Perspectives on the Social Study of Science, London, UK / Beverly Hills, CA 1983.

[34]   Lynch: Art and Artifact in Laboratory Science (as note 32), pp. 81 ff.

they *perform* ontological work and constitute ontological distinctions when they examine patent applications and adjudicate disputes about patent claims.

Recent disputes about gene patents, culminating in a 2013 US Supreme Court decision,[35] make up a rich field of research on the diverse and subtle (and sometimes baffling) ways in which the product of nature / composition of matter distinction has been deployed. In addition, the parties to the cases that concerned questions about genetic patents debated about the specific ontological question of whether genetic sequences should be considered as chemicals or as information.[36]

Briefly, a scholarly consideration of a line of cases involving patent claims on entities, such as adrenalin, plant fibers, genetically engineered organisms, and genetic sequences is *ontographical* in the sense that it delves into the historically and institutionally situated way agents decide the ontological status of living organisms. Such questions invariably turn on judgments about identity and difference; for example, on whether or not extracted genetic sequences used in test systems are the same as ›native‹ DNA sequences. Court judgments about what an entity is *essentially* are bound up in legal precedents, though they take into account arguments by scientists, clinicians, and various stakeholders. The classifications and judgments about relations of identity and difference are highly particular, and do not constitute a stable, context–independent ontology. However, the very specificity of such judgments is highly informative for ontography.

5. Conclusion

In this paper, I suggested that *ontography* offers a way to re–specify the metaphysical category of ontology for more localized investigations into specific social and historical practices that inscribe or otherwise organize divisions among kinds of things. This suggestion followed along the lines that Peter Dear had previously outlined would be the case for »epistemography«: it would be a descriptive approach to historical and contemporary discourse and praxis in which topics

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35   *Association for Molecular Pathology, et al. v. United States Patent and Trademark Office, et al.*, (569 U.S. 576, 2013).

36   The case also is a relatively rare instance in which STS researchers who had written about relevant historical and sociological issues submitted declarations which were featured in the case. In the 2010 Federal District Court trial that preceded the Supreme Court decision. Shobita Parthasarathy and Myles Jackson wrote separate Declarations for the Federal District Court phase of the litigation: *Association for Molecular Pathology, et al. v. United States Patent and Trademark Office, et al.*, US District Court, Southern District of NY (July 2010).

of epistemology are perspicuously featured.[37] In an earlier article on the topic of »ontography,« I mentioned that this approach would be »deflationary« in the way it examined the pragmatic use of what Ian Hacking has called »elevator words« (e.g., reality, representation, and other key philosophical terms) in specific organizational settings.[38] The point of such an investigation would be to elucidate how ›ontological‹ distinctions play a constitutive and consequential role in the production of social and natural order. Examples I discussed in the present paper are from the natural sciences and law, but there are many other possibilities. Perhaps the most obvious domain that I have not discussed is the explicit technical use of *ontologies* in computer and information science. Such usage invites ethnographic investigation of how practitioners in those fields constitute »ontologies.« Although such an investigation would necessarily take into account the technical work involved in the fields studied, it would open up broader questions about the organizational conditions and consequences of such work.

Two issues that remain to be discussed further are, first, whether or not »ontography« would differ substantially from »epistemography,« and whether it would be limited to description. On the first question, my tentative answer is that there would be no clear methodological (methodographic?) difference, but that the discourse and practice in a given case might highlight ontology as an explicit theme (such as in computer and information science), or deploy an ontological distinction (such as the one I discussed between artifacts and natural phenomena) that provides a point of leverage for disputes and decisions that become embedded as findings and precedents in a domain. On the second question about the limits of description, it is difficult to resist the call to conduct a normative and active engagement in disputes studied, but my recommendation is to *begin* with a descriptive orientation, in anticipation of gaining some original insight into contentions about, for example, »facts« (and categories of »fact,« including the category of »fact« itself), before committing to a position about what the facts happen to be. This differs from assuming that »we« are now living in a »post-fact« era, and it does not relieve us of the responsibility for living in the world we find.[39]

Finally, an ontographic investigation would not embark on an ambitious project of developing a novel ontology; one that would emancipate itself from the subject-object dichotomy and various other binaries that hold modern Western

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37 Dear: Science Studies as Epistemography (as note 4). On »perspicuous phenomena,« see Garfinkel: Ethnomethodology's Program (as note 18), p. 141.

38 For a discussion of the »deflationary« implications of ontography, see Lynch: Ontography (as note 1), p. 452. On »elevator words,« see Ian Hacking: The Social Construction of What? Cambridge, MA 1999, p. 22.

39 Michael Lynch: STS, Symmetry, and Post-truth, in: Social Studies of Science 47/4 (2017), pp. 593–599.

metaphysics in their grip. Instead, it would begin without assuming a common metaphysical system that underlies an entire era or a massive and vaguely defined region. This (presumed lack of) assumption is likely to be disallowed by those of us who insist that there is no escape from a ›deep state‹ of metaphysics. Faced with such objections, I can only plea ignorance about such a ›deep state‹ and to confess an inability to expose and reform its operations.