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Thick and Thin Characters: Organismal Form and Representational Practice in Embryology and Genetics

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Abstract

In this thesis the family of concepts, including characters, traits and phenotypes of organisms is analyzed according to the various roles these concepts play in different disciplines and in different historical periods. The ways the concepts are construed in order to fulfill these roles are spelled out in terms of the representational practices in which these concepts are embedded. In particular, I will look at classical genetics, in the period spanning from Mendel to Morgan, and compare the character concept that gradually developed in this period, and which I characterize as a thin concept, meaning that characters appear as values of variables that are represented in articulated symbol systems, with a thick concept of parts, properties and processes as it prevails in anatomy, physiology and embryology. In order to illustrate that both concepts still play their role in modern biology and to address the question of their possible integration, I will, by way of conclusion, briefly look at current developmental genetics.

I introduce the topic of organismal characters by pointing out that characters are such an important category in the life sciences, as well as in broader social contexts, because the parts and properties of organisms they specify can be seen as the interfaces between human interests and organisms, including humans themselves. Since from the plurality of interests results a plurality of character concepts, I will address the general issue of scientific pluralism in Chapter 1. Chapter 2 and 3 introduce the methodological concepts necessary to analyze the representational practices that embody the various character concepts. Representation will be characterized as having a systemic structure, in which alternatives of labels are coordinated with alternatives of referents. The notions of thick and thin phenomena that mark an important difference in character concepts, and the idea of syntactically and semantically dense or articulated symbol systems on which the distinction is based, will be introduced here. Furthermore, a notion of practice will be developed that relates actions to symbol systems. Chapters 4, 5, 6 and the Conclusion contain the discussion of the character concepts in the disciplines taken into consideration.

Introduction: Characters as Interfaces Between Organisms and Human Interests

> Science and signs are inseparably interconnected, since science both presents men with more reliable signs and embodies its results in systems of signs. Human civilization is dependent upon signs and systems of signs, and the human mind is inseparable from the functioning of signs. (Charles Morris [1938] 1971, 17)

Some concept of a character, trait or phenotype, or parts, properties or capacities of an organism by any other name (e.g. homology, adaptation, organ, behavior, etc.), plays a central role in almost every branch of the life sciences. In taxonomy organisms are classified according to their similarity or disparity with respect to certain characters. In evolutionary ecology organisms are considered to be adapted to certain environments in that they possess certain characters. In developmental biology parts and properties of organisms are studied as outcomes of developmental processes. In genetics the characters appear as inherited from the parental generation. Also in today's molecular biology, the understanding and manipulation of the organism's pathological or desirable traits is the ultimate motivation for research. And even in the behavioral sciences, the objects of investigation are often treated as characters of organisms.

This ubiquity of character concepts does not come as a surprise, given that humans have vital interest in organisms, including themselves. Organisms are predators, prey, pathogens and pets, tools, food and goods, they are the source as much for inspiring fiction as for interesting facts and, most notably, being an organism is one aspect of human self-understanding. Characters can be said to be the interfaces between the various human interests and the integrated whole of the organism, they are the relevant aspects of an organism in a given situation. Therefore, to study the way parts, properties and capacities of organisms are conceived should be the right angle to understand the interests that drive particular human practices, inside and outside the sciences. Furthermore, the fact that characters play a role in so many contexts makes them the ideal focal point for questions concerning the relations between scientific disciplines, for instance, with respect to reduction, integration or uncircumventable plurality. In addition, it allows philosophers, historians and sociologists to address the question how the life sciences are influenced by broader social issues, and how those are in turn affected by scientific results.

My interest in the concept family of characters, traits and phenotypes originated from assisting in the editing of the book "*Heredity Produced,*" by Staffan Müller-Wille and Hans-Jörg Rheinberger (2007). In their introduction to the volume the editors put forward the thesis that "the knowledge regime of heredity started to unfold where people, objects, and relationships among them were set into motion" on a global level, due to the exchange of goods, colonialism and industrialization around the mid-eighteenth century (Müller-Wille and Rheinberger 2007, 16). Another way to put this is to say that the cultural history of heredity is a history of characters becoming visible. Plants and animals were brought home from expeditions, were imported or exported from and to colonized territories and, unfortunately, also humans were objects of this trade business. New organisms entered agriculture and breeding and racial systems were established to organize the colonial societies. At the same time, legal systems became more elaborated and with them notions of kinship and ways to track genealogies. Also medicine became more institutionalized and relied increasingly on statistical tools. All these developments materialized in various practices and new ways of representation and, above all, enabled new contrasts between organisms and within organisms. The new contexts and the associated practices rendered certain parts and properties of organisms visible and turned them into characters.

A trait, or character, is any part or property of an organism that plays a role in certain cultural contexts or practices, including particular scientific interests. In the history of heredity, the focus lies on the fact that once they are recognised, characters can be tracked along the generations. Doing so is to establish a concept of heredity. But there are other roles that characters play. For instance, one might say, once they are recognized one can ask questions about their function, or wonder how they arise from the apparently homogeneous mass of the early embryo.

Given the relevance of character concepts, it is remarkable how little there has been written about the issue from a general perspective. Of course, there is much literature about characters in their particular roles, concerning some roles more than others. Insofar, the literature that has to be taken into account to provide a more synoptic view would be vast. Since in this thesis, I take a more narrow view myself, I will only give a brief overview on what types of texts are available. Homology and adaptation are examples of character concepts that are widely discussed (see e.g. Griffiths and Brigandt, eds. 2007; Forber, ed. 2009, for recent special issues about these topics in a philosophical journal). Systematicists in general have the most explicit discussions about the characters, because what counts as a character, but also how characters are weighted, determines their proposed taxonomy (see e.g. Ghiselin 1984; Colless 1985; Richards 2003). And they are most likely to use the term, because it entered modern biology via natural history. A notable collection that takes a broader perspective on characters in development and evolution is provided by Wagner (ed. 2001). Much of the literature by biologists, as well as philosophers, is normative, though, in the sense that it argues for a particular understanding of a concept in a given field. My concern is rather descriptive. I want to analyse how character concepts are construed in the disciplines I look at in certain historical periods. With respect to genetics and development, much writing focuses on the concept of the gene, but since the issue is intrinsically connected to the concept of a character, trait or phenotype, this literature contains much discussion on those concepts as well (see e.g. Beurton et al., eds. 2000). The contribution to the history and philosophy of characters in genetics, which is probably most closely related to my project, has been made by Sara Schwartz (1998).[1] Another notable article on traits in genetics is Falk (1991). Not everywhere, where parts and properties of

[1] The text is an unpublished Ph.D. thesis. Since the thesis is written in Hebrew, my knowledge of the work is restricted to an extended English abstract and those parts that were published as articles (Schwartz 2000, 2002). I thank Sara Schwartz for sending me the abstract.

organisms are considered, the terms “character” or “trait” are used, e.g. in anatomy and physiology, organisms are dissected, literally and conceptually, in terms of organs and capacities. Winther (2006) is an important contribution to the study of those disciplines that investigate body parts.

It is impossible in the scope of this thesis to investigate the roles characters play in all disciplines and social contexts at all times in history. But I see the thesis as part of the larger project to map at least a large part of that territory. Here, I restrict myself to taking a closer look at the role of characters in the study of heredity in the context of its “disciplining” (Rheinberger and Müller-Wille 2009, 169). In particular, in Chapter 6, I will center on Wilhelm Johannsen’s phenotype concept (1909), in order to show how thick and thin interpretations of characters were negotiated in genetics. I will introduce the distinction in Chapters 3 and 4, but roughly speaking, thick characters are parts of organisms in their morphological detail, while thin characters are abstract variables. I will follow the trajectory from Mendel’s introduction of thin characters (1866) in contrasts to the Darwinian-biometrician thick notion of variation, via Bateson’s interpretation of Mendel’s model in terms of characters as parts (unit-characters) that fall somewhat in between a thick and a thin interpretation, from the perspective of Johannsen’s interpretation of Mendelian characters. Johannsen embraces a thin character concept in his rejection of gradual variation and criticizes the morphological unit-character concept. He distinguishes between thin properties as the appropriate category for genetics and thick morphological parts as the result of epigenetic development. Morgan and his followers (1915, 1926) can be seen as completing the thinning of characters in genetics and reducing their role to mere data for chromosome level processes. I will argue that, in the light of the distinction between thick and thin characters, the so-called separation of genetics and development appears differently from how it is usually interpreted.

In order to make the distinction more clear, I will show in Chapter 5 how anatomy and classical embryology deal with thick notions of parts and processes that bring them about. I will also take the opportunity, to discuss the plurality of decompositions in those disciplines that operate with thick characters (Winther 2006) and the role of function in the individuation of parts in biology (Laubichler 1999).

In Chapter 4, I set the stage for the discussions in Chapters 5 and 6 by giving a brief overview on the history and etymology of the relevant terms and introducing the conceptual distinction between characters as parts and characters as variables that take different values (Colless 1985).

What is needed to analyse the way characters are conceived in different contexts, is an account on human practice in general and scientific practice in particular. Although a linguistic, pictorial, or generally semiotic turn, as well as a material or practical turn in the study of science has often been called for or declared by philosophers, historians and sociologists of science, it is still difficult to find the analytic instruments to represent representation as the result of certain activities and at the same time as informing or motivating action and behavior. The reason is that philosophers and linguists often have developed elaborate formal theories of representation, sometimes also indicating a role for action in such theories, but did not care much about the applicability of such theories to actual practical contexts. On the other hand, most historians of science are concerned with the details and specificity of their case. They provide sensitive and fine-grained descriptions of representations and practices, but usually have little interest in making general claims about scientific practice. For those philosophers and historians who hover in between these two approaches and hope to catch some generalizable pattern in scientific practice that is still informed by and capable to inform the particular cases, it is often difficult to find the right balance between principles and details. In the first part of this thesis I attempt to sketch an account of scientific practice. I say “account” and not “theory,” because I conceive of such an endeavor as enabling scholars of science to describe practices and the representations they are related to, to track their transposition and transformation between disciplines and across times, instead of defining what a practice or representation is. In a nutshell, the argument I develop is the following.

Scientific results necessarily take some semiotic form. Also characters will in some form be named, described, depicted or pointed out. I will argue for an understanding of representation, where the entities scientists deal with, as any entity any person recognizes as existing, has to be understood as part of the symbol system in which it is represented, instead of thinking about things in the world as pre-delineated and perceived as such, that get labelled, drawn, photographed or described, or in any other way re-presented. I find in Ferdinand de Saussure (1959) a thinker who has forcefully argued that in semiotic systems, the units are delimited through the other units in the system and that this applies as much to the representing sign-vehicles as to what is represented. Saussure, however, in rejecting the image of words labelling given things in the world, conceives of words or other sign-vehicles as not representing things in the world at all, but relating to concepts. I will recruit Hilary Putnam (1975) to make the point that one has to bite the bullet, especially when talking about scientific representations, and admit that words and other forms of representation are about the world. If terms like "gold" and "tiger," but also "eye" and "red," are not taken as referring to things in the world, the account cannot be adequate to describe the way language and other semiotic systems are used in science. Putnam, however, misses the systemic character of language and thus his theory lends itself to essentialist interpretations. Instead, a view is needed in which things become individuated within the semiotic system in which they are represented.

In the light of the discussion of structuralist and extensionalist approaches, Nelson Goodman's theory of symbols (1976) seems to do justice to the requirements for a theory of representation that is useful to describe the practice of science. First, Goodman's account is extensional; labels denote things in the world. Second, it is systemic, in that it describes labels as parts of sets of alternatives that organize a realm of things such that it becomes partitioned or classified. A scheme of labels and the realm it organizes are a symbol system in which the units gain their significance in that they delimit each other. Third, things are not only individuated within symbol systems, but they can themselves figure as symbols, as exemplars that refer to labels and thus indirectly to other things. Fourth, Goodman takes into account non-linguistic symbols, and with the notions of syntactic and semantic density or differentiatedness provides precise tools for the analysis of different forms of representation.

However, Goodman, as the other authors discussed, does not explain how symbol systems come into being. This is even more regrettable since on this account, — and this is especially important in the study of science, — the individuation of things depends on the introduction of symbol systems, which must happen through some form of activity. Furthermore, actions usually relate to given things, represented in some symbol system, in one way or the other and thus they must relate to symbol systems. I therefore attempt in Chapter 3 to develop a notion of practice that describes actions coming in sets of alternatives as well, that either map on systems of labels that organize a realm, or sort a realm themselves. To describe the relation between the objects in a realm and the actions, analogue to exemplificational reference in the case of labels, I introduce James Gibson's concept of affordance (1986).

I further describe a particular kind of actions that are capable to introduce symbol systems; these might be called epistemic actions. Since such systems consist of schemes of labels or behaviors that organize a realm into ranges of extension, such actions must have a contrastive nature. They must introduce a difference between the objects in the realm. I discuss Ludwig Wittgenstein (esp. 1953), David Gooding (1990) and Hans-Jörg Rheinberger (1997), in order to understand ostensive definitions as procedures of that kind, which can also include difference-introducing interventions. Those interventions that explicitly aim at detecting differences in the causal relations between objects, I call experiments and I use Woodward's (e.g. 2008) counterfactual-interventionist approach to emphasise their contrastive nature. Such contrastive procedures can be further distinguished in those that aim at attributing parts and properties to an object and those that aim at classification or partitioning of objects according to their parts and properties. Additionally, contrastive procedures can be used to introduce new differences or to demonstrate known classification or partitioning schemes.

Scientific phenomena become represented when they are made perceptible as different from something else, that is, as part of a symbol system and through a contrastive procedure. The contrastive constellation of material, including the phenomenon itself, is a representation of the phenomenon (Rheinberger 1997; Gooding 1990). But the procedure can also bring about various other forms of representations, such as diagrammatic or photographic images. Furthermore, within the system, the phenomenon can be represented by a label, a behavior or by its causes or effects, if causal differences are highlighted. These different forms of representation can be dense or differentiated. Phenomena represented in differentiated symbol systems are taken as units and afford certain actions as such. In these cases I speak of thin phenomena. Concerning epistemic actions, they afford external contrasts, that is contrasts between the realm they are part of and another realm. A representation in a dense symbol system can always be interpreted under a differentiated scheme. But if a phenomenon is represented in a dense symbol system, it affords different actions than a thin phenomenon. In particular, it affords internal contrasts, in which the phenomenon itself is the realm that gets organized by a scheme in the course of contrastive procedures. I speak of thick phenomena here. As indicated above, this distinction is relevant in the investigation of character concepts. The question always has to be: What is the contrastive procedure? That is, what is contrasted (organisms with organisms, parts with parts, effects with effects) and which method is used to introduce differences (e.g. looking under a microscope, touching, staining, irritating, manipulating)? What are the syntactic and semantic properties of the representations produced (such as exemplars, photographs, diagrams, names), that is, of the symbol systems to which they belong? And what do the represented phenomena afford (e.g. further contrast, e.g. of organisms with organisms or parts with parts, or any other action such as selecting, eating, selling etc.)?

The account of practice and representation offered suits the plurality of character concepts alluded to above. Characters can play various roles according to the context. They can be desired or unwanted, explained or explaining and classified or used in classifications. Different roles will be associated with different contrastive procedures and representations and lead to different ways to divide the organisms into parts or to determine properties or capacities. In Chapter 1, I will take a general philosophical position concerning the interpretation of such plurality in the sciences. I will defend a version of pluralism, which I refer to as anthropocentric pluralism and which I see in accord with Goodman's pluralism (1983, 1978). Since proper monists are hard to find, at least in the philosophy of the life sciences, I will specify my view in opposition not to a monistic position, but to a different view of pluralism, which I find to be almost consensus in the field. The view can be characterized as grounding scientific pluralism in the complexity of the living world, which is usually taken as a fundamental property of this domain that derives from the evolutionary process. Instead, I maintain that pluralism stems from the open nature of human practices, which allows for always-new contrasts and thus representations through transporting and transforming natural material and relating it to artifacts and cultural concepts.

The sentence that serves as a motto of this Introduction was written by Charles William Morris in his *Foundations of the Theory of Signs*, his contribution to the *Encyclopedia of the Unified Sciences*. While I deny the possibility of a unification of the sciences, as the editors of the Encyclopedia envisioned it, and advocate a disunity of science instead, I embrace the possibility of achieving some unification in the description of scientific practice. Morris' claim subsumes scientific observation, the "reliable signs," and the formulation of results in the form of images, diagrams and language, under the things that can be described by semiotics. Semiotics thereby promises a unified description of science. It does so, however, only if action is seen as part and parcel of the semiotic process. Experimental interventions and the use of language do no longer appear as separate realms, but are both described as semiotic processes. Characters must be seen as appearing in such semiotic processes.

PART I. FOUNDATIONAL ISSUES: PLURALISM, REPRESENTATION AND PRACTICE

Chapter 1. Pluralism: Complexity and Human Practices

Chapter Introduction

The zoologist Olivier Rieppel writes:

> The notion of what a 'character' is will vary with the perspective from which a character is being analysed. An embryologist will have a different notion of a character than a systematist, and an ecologist may look at 'characters' in a somewhat different way than a functional anatomist. (Rieppel 2001, 59)

A "different notion" of a character can be understood as the different relations the term holds to other notions that are relevant to a given perspective, while a different perspective can be spelled out in terms of observational practices and the overall goals of the respective disciplines.

What is expressed here is a pluralist account of characters, according to which the way characters are individuated depends on the interest and practice of a discipline. How a notion of character, and the particular character categories it identifies, arises in the context of the disciplinary perspectives of classical embryology, genetics and modern developmental genetics is the subject of the thesis. Anatomy in the broadest sense plays a role in all disciplines and will therefore be discussed as well. Other disciplines such as systematics and ecology are not considered in the scope of this thesis.[2] By showing how

[2] A caveat is in place here. The term "discipline" invokes the impression that perspectives are fixed, that there is a limited number of interests that map on different aspects of organisms, the domain of the disciplines, which defines the disciplinary boundaries. It is a consequence of plurality that this is not the case. The juxtaposition of embryology, genetics and developmental genetics already makes clear that, historically speaking, interests and methods merge and divide and give rise to ever new perspectives. But even within what we perceive as a discipline at a given time, many strongly diverging perspectives might exist in parallel. As much as I don't want to commit myself to the existence of separate domains, I am not regarding disciplines as institutional entities, because those are also obviously subject to change. I still use

character categories arise in the context of other concepts and the material practice of these disciplines, it should be possible to draw some general conclusions about what it means that categories arise in the context of certain interests and practices. By showing that the disciplines indeed have very different notions of character and that the categories they define depend on the specific contexts in which they are meaningful, pluralism should be indirectly justified. Before I turn to the analysis of the plurality of character concepts in biology, I should say some words about the concept of plurality in philosophy itself. I will present a particular view of pluralism in contrast, not to monism, but mainly to another form pluralism, which I diagnose to be common to most writers in philosophy of the life sciences.

Pluralism

Stephen Kellert, Helen Longino and Kenneth Waters, in the introduction to an anthology on "*Scientific Pluralism"* (Kellert et al. 2006), make a distinction between plurality in, and pluralism about the sciences. The former is hard to deny. There is obviously a great diversity of scientific representations, classifications, theories and explanations, some of which are seen as applying to different domains, some as concerning the same phenomenon. Monism is characterized by Kellert et al. as the position which holds that "the nature of the world is such that it can, at least in principle, be completely described or explained by [...] a single, complete, and comprehensive account of the natural world (or the part of the world investigated by the science) based on a single set of fundamental principles." (Kellert et al. 2006, x) This view also entails that there is a method of inquiry through which such an account is established. Methods, representations, classifications, theories and explanations are evaluated with respect to their contribution to the quest for such a unified account. It seems that the attenuator "at least in principle" is deemed

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the term to indicate broad differences between perspectives, because no more fine-grained term, such as research programme etc. would be more precise. The point is that the boundaries have to be determined on a case by case basis.

necessary because such an account could be too complicated to handle in practice. Physicalism, for instance, is a form of monism, but a complete physical description of the world, even if there would be such a thing, would not be feasible for achieving many practical goals, and even as a description, devoid of any application, it would not be possible to handle for human minds. Nevertheless, for a physicalist other descriptions are mere short cuts to explain specific phenomena that would still be fully explained only in physical terms and their physical basis should also explain why they can be captured by such short cuts.

The bracketed alternative in Kellert et al.'s definition of monism — single accounts for a "part of the world," — is meant to address positions that acknowledge that there might be different domains of phenomena in the world (e.g. living vs. non living, or organic vs. mental), that require different and specific scientific accounts, but that for every such domain there is a single, complete, and comprehensive account. This view can be described as a form of pluralism, because it denies the reducibility of all phenomena to one set of fundamental principles, but it may also be called "domain-monism."

Kellert et al. describe pluralism as a position that acknowledges that there is no *a priori* reason to believe that there is such a unitary account, not even with respect to separate domains of phenomena. They emphasize, however, that there is equally no *a priori* reason to deny that a unitary account can be found, at least with respect to partial domains. Therefore, they describe their view as the "pluralist stance" (Kellert et al. 2006, xiii). This position is open to unification, if it is empirically justified, but guards against a hastened move towards unification on the basis of a metaphysical assumption that runs the risk of overlooking important differences, or neglecting scientific approaches that lie outside the scientific mainstream, a strategy, which could be detrimental to science (Kellert et al. 2006, xii).

This view is different from a view that embraces diversity in the sciences only for the reason that it makes the occurrence of a successful unificatory account more likely, and which Sandra Mitchell summarizes as holding that it is a “rational strategy to adopt for the scientific community as a whole in order to hedge its bets against empirical uncertainty” (Mitchell 2002, 56). This view, of course, would ultimately be a form of monism. Kellert et al., instead, see a real possibility that a unified account might not be available. But the latter conclusion is to be drawn on an empirical basis: “The only way to determine whether a part of the world will require a plurality of accounts is to examine the empirical results of scientific research of that part of the world.” (Kellert et al. 2006, xxiii)

It remains unclear, however, how the success of unification, or the need for plurality is decided empirically, that is, what would be evidence for a legitimate unification or a justified plurality? Would it be evidence that supports some principle of identity of entities from different domains, e.g. brain functions and mental states? It is clear that such evidence is only evidence in the context of a particular theory that incorporates the identity principle, and which therefore is an competing alternative to the separate theories related to the different domains, rather than a justification for replacing them, because they instead have their own criteria to decide what counts as evidence. Or is the evidence for or against unification historical evidence on the social success or lack thereof of unitary theories? If conflicting, or, at least alternative views are maintained in parallel by different groups, this would be evidence that they are not unifiable, whereas the closure of controversies or the extinction of alternative accounts would be evidence for the legitimacy of unification. But history shows that unification is often only transitory and that new alternatives emerge. And not rarely, it is even the old alternatives that arise in new guise. From their text it appears as if Kellert et al. think of the empirical evidence that justifies unification or the maintenance of plurality, respectively, as the practical success of some theories (making good predictions, allowing for intended interventions etc.). If a unified view is more successful in this sense, it should replace alternative views. If those, however, yield practical knowledge that is lost in the unified account, they should prevail. But practical success is always restricted to particular needs (e.g. medical cure) and the success with respect to one need does not entail that the same account facilitates practical success with respect to other needs. That is, the success of a unified neurological theory of the mental and the organic with respect to some surgical or chemical intervention on mental disease would not entail the successful application of this theory in other cases where mental categories are used, say in legal contexts. This is not a problem for the pluralist stance, because here the need for pluralism would be empirically justified. The real problem is that if separate accounts are abandoned because of a unified account is useful in all fields of application of the particular theories, it will never be known if they would have had applications under new circumstances and the circumstances always change.

Even if it does not become clear how exactly it is decided on empirical grounds whether unification is possible or not, the important point with respect to the kind of pluralism I wish to defend in this chapter is that Kellert et al. see the reason for the plurality in science and the possibility that unification might turn out to be impossible, at least in some domains, in the complexity of the world: "It appears that some parts of the world (or situations in the world) are such that a plurality of accounts or approaches will be necessary for answering all the questions we have about those parts or situations." (Kellert et al. 2006, xxii) With respect to the world's "being such that," they explain, that there might be different reasons for different parts of the world. With reference to some of the case studies collected in their anthology, Kellert et al. gesture towards "the complexity of the phenomena—whether associated with crossing levels of organization or multiple factors within the same level of organization" (Kellert et al. 2006, xvi), as one reason. They also name different "explanatory interests," but the way they put it, a plurality of interests has to be understood as resulting from the fact that the "parts of the world in question are such" that we have different interests concerning it.

Complexity

Complexity, by Kellert et al. and many other authors, is taken as a fact about the world. Therefore, a plurality of scientific accounts must co-exist to explain the different aspects of the complex world. Mitchell, for instance, writes:

> This 'fact' of pluralism, on the face of it, seems to be correlated not with maturity of the discipline, but with the complexity of the subject matter. Thus the diversity of views found in contemporary science is not an embarrassment or sign of failure, but rather the product of scientists doing what they must do to produce effective science. Pluralism reflects complexity. (Mitchell 2002, 55)

Also Ronald Giere, in his contribution to the anthology edited by Kellert et al., expresses a similar view:

> Complexity raises more serious issues in biology. In this respect, biology may provide a better paradigm for the sciences than does fundamental physics. Here the traditional way of dealing with the overall complexity of biological phenomena has been to distinguish *levels of organization*: molecules, cells, organs, whole organisms. The same strategy is used in the social sciences: individuals, small groups, communities, corporations, nation states. Even a metaphysical materialist who presumes that there is ultimately nothing but elementary particles must agree that there is little hope of finding a usable reductive theory in either biology or the social sciences. So models are constructed at various levels, resulting in a pluralism of perspectives at different levels. [...] It seems an understatement to say that this plurality of perspectives at different levels is just a pragmatic response to complexity. It looks to be a more fundamental and relatively permanent feature of the biological and social sciences. (Giere 2006, 34)

Kellert et al., Mitchell, and Giere, all emphasize that different models are still models of one single phenomenon, or that different perspectives are still perspectives on a single world. I see two fundamental problems with putting things like this.

First, complexity cannot be taken as a fact about the world. It is just another human concept. It comes with particular representations — representations that represent the world as complex. These representations can be many kinds of things, from pictorial diagrams to linguistic metaphors, and the talk of "levels of organisation" is part of these representations of complexity. Thus, I think, it is necessary to exchange the claim of Kellert et al. that "parts of the world (or situations in the world) are *such* that a plurality of accounts or approaches will be necessary for answering all the questions we have about those parts or situations" by the claim that *humans* "are *such* that a plurality of accounts or approaches will be necessary for answering all the questions we have about those parts or situations." I will argue for that view below.

Second, if the different models, representations and perspectives are versions of the world, "we cannot test a version by comparing it with a world undescribed, undepicted, unperceived" (Goodman 1978, 4). Thus the notion of "the world" of which different versions exist becomes devoid of meaning. The only thing that can be said is that the claim that two representations are representations of the same phenomenon amounts to having a representation that represents the two representations as being representations of the same phenomenon. These points are, of course, rather fundamental, touching on the core of philosophical epistemology. And the authors discussed are certainly aware of the problem and develop sophisticated arguments to defend their respective views against this objection.

As I will specify below, I will opt for an anthropocentric pluralism, which holds that ever-new worlds are generated through human practice, as opposed to a view where a certain number of perspectives are required due to the complexity of the world. The diversity of interests, accordingly, is not imposed upon humans by the complexity of the parts of the world they have interests in, but comes with the diversification of human practice. Before I further elaborate this position, I shall discuss some alternative views, to clarify why I deem such a strong anthropocentrism necessary.

Kellert et al. seem to read John Dupré as holding a position similar to the one I wish to put forward, when they write: "According to his 'promiscuous realism,' there are an indefinite number of ways of individuating and classifying the objects in the world, each of which is responsive to different interests, and no one of which is more correct than the others." (Kellert et al. 2006, xiii) A closer reading, however, reveals that Dupré does not hold the kind of anthropocentric pluralism envisioned here, but, instead, is in line with the positions discussed above that ground pluralism in the complexity of the world. His pluralism as well as his antireductionism are ultimately justified by the fact of complexity in the biological world. On pluralism he writes: "It is that the complexity and variety of the biological world is such that only a pluralistic approach is likely to prove adequate for its investigation." (Dupré 1993, 53) And with respect to reductionism he states: "I suspect that the complex interdependencies of entities at many different levels of structural complexity characteristic of biology is sufficient to show the implausibility of the reductionistic project." (Dupré 1993, 103)

The impression that Dupré grounds his pluralism in an anthropocentristic way in human interests stems from the structure of his argument. Dupré begins with the observation that ordinary language classifies organisms quite differently from scientific taxonomy. There is, for instance, no term in ordinary language co-extensive with "*angiosperms*" (an ordinary language term would be "flowering plants", but this is neither a commonly used category, nor is it likely to be understood as having the same extension as "*angiosperms,*" but would probably be understood as co-extensive with "flowers"). Instead, there is the category tree, which includes, for instance, oak trees and pine trees, where the former, but not the latter would fall under scientific category *angiosperm*. Dupré comments: “for most purposes it is much more relevant whether something is or is not a tree than whether its seeds develop in an ovary.” (Dupré 1993, 35) Thus ordinary language and scientific taxonomy produce conflicting classifications. Furthermore, there are areas of specialization other than science that produce very specific classifications of organisms. Dupré gives the example of the category of *cedars*, which does not correspond to any category in taxonomy, but classifies a kind of timber for the purposes of timber merchants and carpenters. None of these classifications is in itself better or worse, they apply in their context, but not in others.

Dupré then moves on in his argumentation to show that even in scientific taxonomy distinctions are drawn with respect to different purposes. Interest in ecology as opposed to evolutionary history, for instance, will lead to the preference of a concept of species as classes as opposed to species as individuals (Dupré 1993, 42). Furthermore, it could favour morphological criteria as opposed to phylogenetic criteria for classification (Dupré 1993, 45). Within a phylogenetic taxonomy (cladistics), it could motivate a divergence from the cladistic orthodoxy, for instance, to rescue the distinction between *reptiles* and *aves* that is not preserved by the strict application of cladistic criteria (Dupré 1993, 49).

Despite this emphasis on different interests, Dupré shares the view characterized above, that parts of the world, such as the domain of organisms, are such that they can give rise to a variety of human interests. And the property that makes parts of the world amenable to a plurality of interests is their complexity, the fact that they are not the product of a uniform process, governed by a single law, but in the case of organisms, of the process of evolution that necessarily produces heterogeneity. It is complexity that fosters different interests, not different interests that make the world appear complex. In contrast to this view, I content that pluralism grounds in the plurality of interests, in the complexity of human practice.

The fact that Dupré’s pluralism is not anthropocentric in this sense does not so much show in his use of the label “realism,” which after all does not mean much, but in his maintenance of the classical distinction between natural and artificial classifications. Carl

Gustav Hempel rejects the notion of essential characteristics that distinguish natural from artificial classifications. Instead he offers as the "rational core" of the distinction "the consideration that in so called natural classifications the determining characteristics are associated, universally or in a high percentage of all cases, with other characteristics, of which they are logically independent." (Hempel 1952, 53) Accordingly, he states that the distinction between natural and artificial classifications is a matter of degree. He also derives a form of pluralism from this observation, when he claims that "a particular classification may well prove 'natural' for the purposes of biology; others maybe fruitful for psychology or sociology, etc., and each of them would presumably be of little use in some of these contexts." (Hempel 1952, 54) Whether a classification is fruitful, for Hempel is an empirical question, where the number of predictions and deductions that can be drawn from the classification are the empirical measure of its naturalness. Even though Hempel denies the reality of essential characteristics, he still suggests that those characteristics that yield fruitful classifications mark real boundaries in nature, which explain the fruitfulness. A classification "of humans according to the first letter in their given names, or according to whether their weight does or does not exceed fifty pounds," he cannot imagine to be fruitful (Hempel 1952, 53). Thus he somehow maintains the idea implicit in essentialism, that artificial classifications are based on "superficial resemblances or external criteria." (Hempel 1952, 52)

In the same vein, Dupré abandons essentialism and endorses pluralism, but maintains the distinction between natural and artificial kinds. He also thinks that useful classifications can be distinguished from others because they capture many similarities and allow predictions:

> The class of creatures with wings and feathers, for example, is more natural than that of creatures that are gray and over one foot long. This is so because when we know that a creature belongs to the first class, we can make numerous further predictions about it: that it or its female counterpart lays eggs, is warm-

> blooded, relatively large-brained, and so on. Membership in the second class carries no such benefits. (Dupré 1993, 64)

Several classifications can correspond to natural kinds, which are real, according to Dupré, even if they cross-classify a domain of objects, such as organisms. An individual can be of various kinds and this does not make those kinds less real. However, Dupré's view implies that there are other possible classifications that do not capture natural kinds, such as the example given in the quotation. The term "natural kind" does not imply essentialism on this account, but still distinguishes "butterflies, coprolites, volcanoes and suchlike somewhat homogeneous naturally occurring kinds of objects from kinds that are either not naturally occurring (artefacts) or not remotely homogeneous (property of the U.S. government, fast food, bigger than my head, and so on)." (Dupré 1993, 83)

The problem with such examples for unnatural kinds is that they are presented without any context. Take Hempel's example: if we imagine a context where the classification of humans according to weight classes is used, there will be many properties that are shared between the individuals that fall in the same class; boxing is a case in point. And whether a class is homogeneous or heterogeneous depends on what properties we look at. Of course, the members of the class that is characterized as "property of the U.S. government" are very heterogeneous with respect to material and form, but since the context in which the classification is used is the legal context, we have to look at the legal properties of these objects and with respect to these, they are very homogeneous. For instance, they all fall under 18 USC Sec. 1361 02/01/2010 "Government property or contracts." It is easy to make predictions if one knows that an object belongs to this class, for instance, concerning what happens if one "wilfully injures or commits any depredation against" such an object.

Dupré might want to admit that there are legal kinds, but maintain that they are different from natural kinds. But there are two points to be made in this respect. First, Dupré is very concerned with showing that there is no sharp boundary between scientific and non-scientific, expert and natural language classifications. This can be interpreted in two ways.

Either one sees a sharp line between the domain of nature and the non-natural things, but thinks that both scientists and non-scientists classify objects in the domain of nature, or one does not draw this line. It would be more in the general thrust of Dupré's anti-essentialist argumentation not to postulate such a sharp line, or more precisely, to expect that different classifications of objects as falling in the natural or unnatural domains can co-exist. The use of tools might be a case in point. Usually we see our artefacts as outside the domain of the natural, but in the context of tool-use in animals in general, the tools appear as natural objects. Thus their status is doubtful at least concerning the early tool uses by *Homo sapiens*. But even if a sharp line is drawn between natural and unnatural objects, then cedars are natural objects. They fall in the domain of organisms that are differently classified by taxonomists and timber merchants or carpenters. Still, if one realizes how the latter are involved in the unnatural world, cedar appears as very unnatural kind as well: there are almost certainly industrial norms regulating which timber can be sold as cedar, as well as norms that suggest which timber can be used for which purposes etc.

The second point that is important if the distinction between natural kinds and other kinds of kinds such as legal kinds is maintained is that also natural kinds only become comprehensible if the context is taken into account. One might argue, that while I could come up with a context in which "creatures that are gray and over one foot long" is used as a category and also allows some predictions, — I imagine some 19th century British club of noble men, hosting a hunting contest in colonial territory —, in the case of "creatures with wings and feathers" it is clear without suggesting any context that they allow for further predictions. But this is only because we already know the context. Would we know nothing about the context of biology as a science, the second classification would be as absurd as the first.

So what is the context? Is it other kinds, the kind in question interacts with, which makes recognizing this kind fruitful, or is it the humans that recognize the kind as such, for whatever reasons. It seems that in the case of hunting for gray creatures that are over one food long, the interactions of this kind with other kinds is deeply entwined with the recognition of this kind by human actors. Whereas in the case of creatures with wings and feathers, the interaction of this kind with other kinds in an ecological context is independent of the kind being recognized. But first of all, humans hunting animals is an ecological interaction. Secondly, the causal interactions in nature are manifold. Even if they exist without humans detecting them, they do not mark boundaries of objects. Instead, it seems that by focussing on some causes will result in some ways to recognize objects or focussing on some objects will highlight some causal relations.

Pluralism Concerning Simple Things

I will now address one further instance of the type of pluralism that rests on the notion of the complexity of the world, and use this position to introduce my own position by contrast. The dependency of the context in which a classification or decomposition is natural in terms of the interactions among natural kinds on the context of human interest in them, can be illustrated with an example provided by William Wimsatt, a lifelong thinker of complexity. Though both Dupré and Wimsatt look at organisms, the former is mostly concerned with pluralism concerning classifications of organisms, whereas Wimsatt looks at their decomposition into parts. Wimsatt distinguishes two forms of complexity: descriptive and interactional. Wimsatt defines the former in the following way:

> Assume that it is possible to individuate the different theoretical perspectives, T_i, applicable to a system. Each of these T_i's implies or suggests criteria for the identification and individuation of parts, and thus generates a 'decomposition' of the system into parts. These decompositions, $K(T)_i$, I will call 'K-decompositions'. The different $K(T)_i$, may or may not give spatially coincident boundaries for some or for all of the parts of the system. The boundaries of two parts are spatially coincident if and only if for any two points in a part under $K(T)_j$ these points are in a single part under $K (T)_k$, and conversely. [...] If all of a set of decompositions, $K(T)_j$, of a system produce coincident boundaries for

all parts of the system, the system will be called *descriptively simple* relative to those $K(T)_j$,.

If two parts from different $K(T)_i$ are not coincident, but have a common point which is an interior point of at least one of them, then there are a number of different mapping relations which can hold between their boundaries, each of which contributes to its *descriptive complexity*. (Wimsatt 1972, 70)

Wimsatt then illustrates descriptive complexity by comparing a piece of granite with a multicellular organism. One can take several theoretical perspectives towards a piece of granite distinguishing sub-regions according to chemical composition, thermal conductivity, electrical conductivity, density, and tensile strength. All of these decompositions yield sub-regions with coincident boundaries. In a multicellular organism such as a fruit fly, the theoretical perspectives might offer criteria to decompose the organism into "anatomical, physiological, biochemical, or evolutionary functional system; into cells having common developmental fates or potentialities, or into phenotypic features determined by common sets of genes," thus defining parts, that do not have coinciding boundaries (Wimsatt 1972, 70). Wimsatt remarks that the different descriptions of the piece of granite are basically perspectives derived from one theoretical perspective that describes the physico-chemical properties of the object. So it is no surprise that the boundaries they define coincide! The fly lends itself to many more theoretical perspectives that are not sub-perspectives of the physico-chemical perspective, and which are "not meaningfully applied to the granite." (Wimsatt 1972, 71, in the annotation to Wimsatt's Figure 1) For sure, a piece of granite has neither anatomical parts nor cell fates, but the claim that the physico-chemical perspective is the only perspective that can apply to a stone is not as straightforward as it seems.

Take a common wrench (my Figure 1). Being made of one material, the physico-chemical analysis along the lines suggested for the piece of granite will yield no sub parts. However, from a functional point of view we can distinguish the handle from the head. As

Figure 1 shows, the application of a geometric grid also defines sub-regions. And finally, the colors indicate parts that are affected differently by stress forces when the wrench is used with a given displacement.

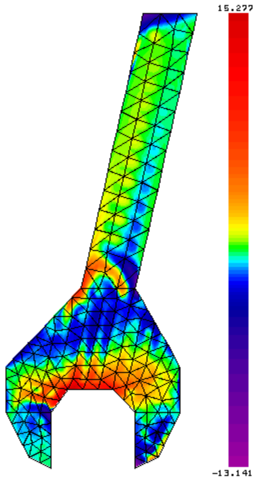

Figure 1: Different ways to partition a simple wrench

It might be argued that the distinctions of the sub parts are due to the fact that the wrench is an artefact that has a given function, and thus a given interaction with other objects. But first, there is no reason to exclude artefacts from the discussion of the complexity of objects. Secondly, stones do just as well appear in particular contexts. If the stone is investigated in an ecological context, for instance, and the interaction with algae or lichens is investigated, then the surface of a stone will certainly be distinguished as the relevant locus from the rest of the stone, and rugged surface parts might be distinguished from smooth parts. If it is about big granite blocks in rivers, the surface regions exposed to water will be distinguished from those outside the water. Ecologists might use categories from geology, for instance, when they call the parts of a granite boulder not covered by soil the "outcrop," but they are interested in outcrops as habitats, while Geologists will have a different perspective on the object, being interested in weathering (where in turn the lichens might be one factor). This decomposition of a variety of sub-regions of the surface, or parts inside or outside water or soil, produces boundaries that are not coincident with the physico-chemical decompositions. Let us look at the inside/outside the water decomposition. It might be said that it is extrinsic, as opposed to such properties as to chemical composition, thermal conductivity, electrical conductivity, density, and tensile strength. But first, other decompositions mentioned such as the rugged/not-rugged surface are not due to external factors, and second, some functional features in organisms are also individuated on the basis of the interaction with external ecological factors. It might also be said that the property is transitory, but also many parts of a fly are transitory, occurring and disappearing during the life cycle of the organism. It might be further objected that the inside/outside the water decomposition does not apply to all blocks of granite. But it applies to all river-bed-granite-blocks and objects in this class would be descriptively simple according to Wimsatt. Finally, it might be said that these parts do not allow for generalizations. But riverbed ecologists certainly draw generalizations on the basis of such decompositions.

Just as Hempel's and Dupré's artificial kinds, Wimsatt's simplicity is a rhetorical trick. By neglecting the context in one case (grey creatures over one foot long, piece of granite) and relying on the fact that for the other example (creatures with wings and feathers, multicellular organisms) the context is immediately activated (as a frame of semantic knowledge, Fillmore 1976, which in this case is the frame of a typical reader of *PSA*) one side of the comparison is rendered absurd or simple, respectively. If we put ourselves in the shoes of someone who does not know about the many perspectives on an organism that biology has to offer, a fly would appear very simple, just as the stone appears simple, if we do not take the possible ecological, geological or geometrical descriptions into account. It seems that descriptive complexity depends on the perspectives. Thus the plurality of perspectives cannot be explained through complexity.

I am not denying that a multicellular organism is more complex than a piece of granite. But descriptive complexity is not a good measure for the complexity that is meant when we intuitively agree with that statement. Maybe interactional complexity is. I will not discuss this point here. Interactional complexity is defined as interaction among the parts identified in different K-decompositions. Thus it depends on descriptive complexity. The question is when things are descriptively simple or complex. I content that there are no descriptively simple things, because there are indefinitely many perspectives possible concerning every object, some of which will yield non-coincident components. Only a certain limited subset, such as the set of physico-chemical perspectives, can be said to render the object descriptively simple. From 1972 paper it does not become fully clear whether Wimsatt holds the same view or whether he thinks that a piece of granite is in some ontological sense descriptively simple and that it lends itself to no other perspective than the physico-chemical.

In any case on my account a stone is descriptively complex. This might be due to the fact that it is part of an ecological or geological system. The whole geological system might compete with a fly in complexity. Then the descriptive complexity is determined by the system it is part of. The complexity of the object thus depends on whether we represent it as a part of a system (and of which system) or not. Every object is, however, ultimately part of a causal system of the same complexity (earth, universe, or wherever the boundary is drawn). We can easily represent a piece of granite as isolated from any systemic context. We could do the same with a single molecule of the fly, as biochemists sometimes do when they investigate the chemical properties of the molecule without taking its functional role into account. This is in line with what I said before, that complexity is a way of representing things, namely as parts of systems. The bottom line is, descriptive complexity follows from the way things are described. The apparent circularity goes away if one takes a cultural perspective according to which it is the generation of new ways of description that make objects descriptively complex.

Anthropocentric Pluralism

The persisting idea of natural and artificial classifications, even if interpreted in a non-essentialist way, as well as the idea that only intrinsic but not extrinsic properties (if the distinction is sustainable at all) provide criteria for classification, are the subject of attack of Goodman's new riddle of induction. "New riddle of induction" is therefore a bit of a misnomer. Before I explain this claim, I shall briefly introduce it in its original form of Goodman's short note in the *Journal of Philosophy* of 1946, because the way he puts it there is less prone to misunderstandings than the more well known formulation in *Fact Fiction and Forecast* (Goodman 1983):

> Suppose we had drawn a marble from a certain bowl on each of the ninety-nine days up to and including VE day, and each marble drawn was red. We would expect that the marble drawn on the following day would also be red. So far all is well. Our evidence maybe expressed by the conjunction "$Ra_1 \cdot Ra_2 \cdot \ldots \cdot Ra_{99}$," which well confirms the prediction "Ra_{100}." But increase of credibility, projection, "confirmation" in any intuitive sense, does not occur in the case of every predicate under similar circumstances. Let "*S*" be the predicate "is drawn by VE day and is red, or is drawn later and is non-red." The evidence of the same drawings above assumed maybe expressed by the conjunction "$Sa_1 \cdot Sa_2 \cdot \ldots \cdot Sa_{99}$." By the theories of confirmation in question this well confirms the prediction "Sa_{100}"; but actually we do not expect that the hundredth marble will be non-red. "Sa_{100}" gains no whit of credibility from the evidence offered. (Goodman 1946, 383)

The problem is that, depending on what predicate (red or *S*) is applied to the marbles in recording the data, there are two sets of data that support different hypotheses. The formulation of the riddle in *Fact Fiction and Forecast* often gave rise to an interpretation of the riddle as stating that the objects might change their color and therefore corrupt prediction (the most prominent instance of this misreading is Barker and Achinstein 1997). But under this interpretation the "new riddle" would be a version of David Hume's (2004 [1748]) problem of induction, and therefore hardly new. The formulation above, instead, has the form of an urn model as a cognitive tool well known from probability theory. To be precise, it represents an urn problem without replacement. This means that the marbles, once drawn, are not reconsidered. It does not matter, therefore, if they change their color. The point is not, as in Hume, that the uniformity of nature cannot be assumed, but the different ways by which we can classify objects. In one case the objects are classified by color and put in baskets, so to speak, that have color terms written on them. One can see these baskets as metaphors for our cognitive categories. All red marbles go into the basket that has "red" written on it and that is attached to the rule: "Put in this basket all and only those marbles that are red." In the other case objects are classified according to the scheme S/not S. We have a basket that has "S" written on it and is attached to the rule: "Put in this basket all and only those marbles that are either drawn by VE day and are red, or drawn later and are non-red." If one draws a marble before VE day and it is red it goes in the basket labelled "S" and after VE this marble is still red and it is still in the basket, where it belongs, because it was drawn before VE day and found to be red. Now in the first case, the prediction will be that, since all marbles were such that they go in the basket labelled "red," the marble drawn on the first day after VE day will be such that it goes in that basket as well, which requires that it will be red. In the second case, we will predict that, since all marbles so far were such that they go in the basket labelled "S," the hundredth marble will also be such that it goes in the basket labelled "S," which would require that it is non-red.

Many arguments have been put forward in order to show that there are unique features of the predicate "*S*" ("grue" in *Fact Fiction and Forecast*) and that all predicates that share those features have to be excluded from contexts where predictions are made, such as science (see e.g. Stalker, ed. 1994 for an annotated bibliography, which already contains more than 300 articles). The main lines of argument can be classified according to whether they focus on syntactic, semantic or epistemic differences between predicates of the same type as "*S*," and proper predicates such as "red." All three aspects have already been addressed in Carnap's first response to Goodman's 1946 paper (Carnap 1997). I will not discuss all arguments here, but just give an example to illustrate how difficult it is to define "proper predicates," if one feels the need to do so. Carnap suggested that proper predicates that can be used in scientific hypotheses should not be positional, that is, they should not contain in their definition any special or temporal restriction, or, to put it differently, the definition should not contain any individual constant. But this would make it hard to find any proper predicate in sciences such as geology and biology. Think of predicates such as "Arctic" or "Precambrian." (Goodman 1997) The restriction would render those disciplines not proper sciences, a conclusion that any philosophical account on the sciences is well advised to avoid. Furthermore, science is not the only context where hypotheses are formulated and most contexts in which predictions really make a difference in the sense of being life saving happen outside of science.

One might argue that the definition of "*S*" requires human judgement and action. A marble must be drawn in a certain time interval and found to be red by a human agent to be *S*. One can also say that a marble must be drawn at any time and found to be red in order to be red, but it seems that in the case of "red" one can somehow omit human agency from the definition. Things are just red. But are they? Is something red in the absence of light? This depends again on the definition of red that, however it is construed, will contain many other predicates that have to be defined as well. But one way to put it would be that red is a relational property between an object, human senses and a set of circumstances.

Be this as it may, we have no reason to exclude predicates that depend on human judgement and agency in their definition as good candidates for being used in hypotheses. The classification of objects according to the S/non-S scheme has real causal consequences. We can imagine a situation where marbles go in different literal baskets that might be further processed. The *S* marbles might be sold, while the *non-S* marbles might be destroyed. These categories then can well describe the relations in the ecology of humans and the objects in their environment. It shall become clear now, why I termed the kind of pluralism that sees no need to get rid of *S*-like predicates, that rather sees them as a virtue, not as a vice, an anthropocentric pluralism.

The difference between “red” and “*S*” is according to Goodman just that in our current world, “red” is used and “*S*” is not. A predicate is projectible, that is, it can be applied to unobserved cases, if it has been successfully used in projections before. Such predicates Goodman calls entrenched (Goodman 1983, 94-96). As Goodman writes: “The reason why only the right predicates happen so luckily to have become well entrenched is just that the well entrenched predicates have thereby become the right ones.” (Goodman 1983, 98) Projectibility then involves another inductive inference, of course: The predicate was appropriate before, so it will be appropriate again. But there is no need to justify this projection. It is just the way that we proceed. We might be wrong in particular cases, if we apply the predicates that we have successfully applied before. The point is that predicates are used in the context of many other symbols. What makes them successful, and this is a point that Goodman emphasizes only later, is that they fit in a network of symbols, or, as we can also say, the categories fit in a network of categories. Goodman writes:

> We have seen [...] that rightness of categorization, which enters into most other varieties of rightness, is rather a matter of fit with practice; that without the organization, the selection of relevant kinds, effected by evolving tradition, there is no rightness or wrongness of categorization, no validity or invalidity of inductive inference, no fair or unfair sampling and no uniformity or disparity among samples. (Goodman 1978, 138, 139)

It is entrenchment and fit that make a predicate projectible, or a category right. It is not that the right categories are natural kinds. Goodman speaks of relevant kinds instead.

> I say ‘relevant’ rather than ‘natural’ for two reasons: first, ‘natural’ is an inapt term to cover not only biological species but such artificial kinds as musical works, psychological experiments and types of machinery; and second, ‘natural’ suggests some absolute categorical or psychological priority, while the kinds in question are rather habitual or traditional or devised for a new purpose. (Goodman 1978, 10)

Categories, or the predicates that name them, fit not only with other words, but with all symbolic practices, whether they are pictorial or performative. Goodman suggests a number of criteria that explicate fit, such as coherence (Goodman 1978, 125) and utility (Goodman 1978, 123).[3] And, of course, the description of fit requires a theory of symbols that enables one to detect the connections between verbal, pictorial and other symbols — the nodes in the network, — which Goodman also provides (Goodman 1976, see also Chapter 2). Given an account on the fitting of categories, one can also explain how new categories that are obviously not entrenched, can be introduced. They might either have no rivals, if they apply to unexplored realms, or they might fit well with given categories. In the latter case they might conflict with some categories or explicitly rival with some others, but if their overall fit is good, they may prevail and in the long run their rivals may vanish. With Wittgenstein we can say that categories are right within a given "form of life" (Wittgenstein 2001, § 19, for a pluralistic understanding of the term see e.g. Haller 1999). Predicates and other symbols are used successfully if the interaction between the sign users goes smoothly, which it usually does (see Abel 2004, 173). Words are answered and actions are responded to in a continuous way. Only sometimes the flow of interaction is interrupted by misunderstanding and confusion. In this case the categories have to be readjusted.[4]

Let me finish this rather incomplete description of Goodman's pluralism with a fictional example of what Wittgenstein would call a form of life. We live in a language community where "red" is used and we cannot imagine a situation where "S" is used on a regular basis. But imagine a feudal society in which the lord, *qua* authority, establishes the rule that every calendar year, he receives all pigs born before summer solstice and all cows born after. They have a word for this category of things, which is "duty." If an ethnologist observes 99 piglets being brought to the lord before summer solstice, she will predict that

[3] On fitting, see, for instance, Goodman (1978, 19), as well as Goodman and Elgin (1988, 46), concerning the network metaphor, see Goodman (1976, 72), and Goodman and Elgin (1988, 8).

[4] Concerning fitting as "making fit," see Goodman and Elgin 1988, 185.

on the next day, another piglet will be delivered. If, instead, a calf will be delivered, she will understand that she has the wrong category. What is delivered is not a piglet or a calf, but a duty and a duty is defined according to the rule mentioned above. “Duty” is an entrenched predicate and duty is an entrenched category in this society. An inhabitant, who does not understand the predicate and brings a piglet after summer solstice when he is asked to bring a duty, causes a problem and his category system will be adjusted. If the ethnologist wants to describe the society she has to describe duties as kinds of things that exist.

If I said that “the new riddle of induction” is a misnomer, we can now see why. The choice of categories precedes every kind of inference; it has consequences that go far beyond the narrow problem of induction. As Hacking remarks with respect to the new riddle: „It is not peculiarly connected with induction“ (Hacking 1997, 163). Every result of a cognitive operation (induction, deduction, abduction, comparison etc.) depends on what categories we take as given. The example of inductive inference is just a very powerful illustration of this fact: „Induction is no more than a crisp way to pose a general difficulty“ (Hacking 1994, 194). Also Günter Abel emphasizes the significance of the new riddle beyond the problem of induction and writes with respect to the distinction between projectible and non-projectible predicates: „The results of this move are of an importance that clearly goes beyond the problem of induction in its narrower sense. They concern fundamental aspects of human cognition and practise“ (Abel 1997, 2). The important point is that there are no predicates that stand out from others in that the objects they apply to are connected by a closer connection than the objects in the extension of other predicates. If predicates have an extension in a given language, the objects are alike in that they fall in the extension. Predicates gain their projectibility only „from our practice of using predicates, from habit, and from the degree of entrenchment in historically and culturally given practices and standards“ (Abel 1997, 2). Another way to put it is to say that there are no natural categories. Goodman articulates the more general importance of the distinction between projectible and non-projectible predicates himself already in *Fact Fiction and Forecast*:

> It may give us a way of distinguishing 'genuine' from merely 'artificial' kinds, or more genuine from less genuine kinds and thus enable us to interpret ordinary statements affirming that certain things are or are not of the same kind, or are more akin than certain other things. For surely the entrenchment of classes is some measure of their genuineness as kinds; roughly speaking, two things are the more akin according as there is a more specific and better entrenched predicate that applies to both. (Goodman 1983, 122)

This, of course, is a statement with much more far-reaching consequences than only that there is no way to know which predicates yields good predictions and which not.

Chapter Conclusion

We saw that most forms of pluralism available in the philosophy of the life sciences literature base the fact of plurality in the complexity of nature, instead of the principal openness of human practice. Where does the belief in the fact of complexity stem from? It seems that most of the philosophers discussed above do not derive the fact of complexity from the fact of plurality in the sciences, because this would be overtly circular. Instead they see evolution as a process that has the particular characteristic of bringing about complexity, and they probably think that there are other processes that are capable of generating complexity as well. Thus the belief in the fact of complexity is dependent on a belief in a particular idea about the process of evolution. This idea is grounded in the science of biology, thus in one (or rather a bunch) of the perspectives or models that make up the plurality of science. This is again a form of circularity. But it is, I guess, even if it might be problematic, unavoidable.

Even if this form of circularity is unavoidable, there are other reasons to reject the justification of pluralism through the (evolutionarily informed) belief in complexity. Most importantly, as we saw, this view is unable to capture the plurality of decompositions or classifications that applies to objects that are not considered complex by anyone. If we instead locate the complexity that gives rise to a plurality in human practice, any object can become subject to a variety of perspectives. A pluralism that is grounded in the belief in the open-ended creativity of culture instead of evolution is also dependent on one particular perspective on culture, among the many available, and thus not less circular than evolutionary pluralism. It is just one world-version, although, I hope, a right one.

I said that I call my position, which I take to be in line with Goodman's, "anthropocentric pluralism" (although I do not know if Goodman would subscribe to this label), because Goodman's critique of genuine or natural classifications based on intrinsic properties operates with properties that involve human action (the property of a marble of being drawn before VE day and found to be red). One might read this as saying that there is no reason to exclude such properties (or predicates) from the properties that can define relevant kinds. A strong reading would be that any property involves a relation between an object and its human observer. A weaker reading would allow for having an ontology, which is independent of human observers, but still include properties that depend on humans as on a par with any other properties and as subject to scientific investigation. One cannot have, for instance, an ecology that treats humans as animals and just operates with non-anthropocentric categories, leaving out predicates such as "property." The fact that humans have property systems causally influences their ecologies and it can therefore only be described with the help of such categories.

To give an example, and thereby come back, finally, to the question of characters of organisms: Shelf life is a character. It is a property of a part of a plant (the fruit) and it becomes recognized outside and inside the biological sciences. It can be subjected to biological analysis, for instance, when its heritability is investigated. And yet, there is no shelf life without a shelf. And there is no shelf in the relevant sense without the whole culture of division of labour, food production, trade and storage, without monoculture, trucks, supermarkets and fridges, without working hours and salad recipes without the whole of our culture from the architecture of our kitchens to zucchini-tomato gratins. It is in the network of these symbols, artefacts and practices that the category of shelf life becomes recognized, relevant and right. And the fact that attempts have been made to reduce this character to the activity of certain enzymes only shows that this is actually another character. It is a translation, but as every translation it does not go without loss. Agriculture will play a role when the history of genetics is discussed below, but it should become clear that also in non-applied research — that might appear as isolated from the broader changes in life style and culture (which it is not of course), — change and novelty in the form of new technology or any form of integration of knowledge from other fields results in new perspectives and thus new decompositions and classifications.

Chapter 2. Theories of Representation: Systems, Reference and Dense vs. Articulated Symbol Systems

Chapter Introduction

If a part or property of an organism becomes individuated, such that it can figure as a character in some context, it becomes possible to refer to it with a term of a given language. The referent of a term can even become individuated through the use of the term. Furthermore, many non-linguistic symbols will be involved in delimiting and representing parts and properties of organisms. There is, however, much dispute concerning the question whether the possibility of using a term to refer to an object is part of its meaning at all, or whether there is even nothing more to meaning apart from reference. And if there is more to meaning than reference, is this more a function of the reference, or is reference a function of these other aspects of meaning. Given that I am interested in the ability of language and other symbol systems to refer to characters and sometimes even to individuate characters, I will emphasize the referential potential of symbols. I will opt, that is, for an extensional theory of meaning. I contend, however, that there is more to the meaning of a term than its relation to its referents and that is that terms are parts of systems. The systemic character of language and signs in general has been made prominent in linguistic and semiotic structuralism, but has often been neglected in the Anglo-American tradition of philosophy of language. I will therefore first present the structuralist view on signs in its original formulation by Ferdinand de Saussure. The problem with the structuralist tradition, however, is that it tends to see words or other sign-vehicles as being related to concepts, thereby rejecting the idea of reference, relegating it to pragmatics, that is to the use of signs that, on this account, has nothing to do with a theory of meaning. I will contrast Saussure's ideas with Putnam's account that represents an extensional theory of meaning. Unfortunately, Putnam shares with many of his peers the ignorance towards the systemic nature of language (which might stem from an ignorance towards the structuralist tradition). One author who unites systemic and extensional aspects is again Nelson Goodman. His theory of symbols has the additional advantage that it applies not only to language, but also to non-linguistic symbols. Given that science operates as much with images and diagrams as it operates with words, such a theory is an invaluable resource for the analysis of scientific practice. Among other things, I will derive the notions of thick and thin concepts from Goodman's work, which will figure prominently in my comparative study on characters in several biological disciplines.

Saussure: Signs are Organized in Systems

Ferdinand de Saussure, the founding figure of structuralism[5], was unsatisfied with a view of language as a list of words that correspond to things, what he called a *nomenclature*. First of all, Saussure maintains that what is associated is the sensual impression of an expression (the signifier) – he focuses on sounds, but it might also be marks of ink or many other things – and a concept (the signified). Those elements are associated in the brain as he puts it (Saussure 1959 [1916], 65-66). Together they constitute a sign. But even on this

[5] Considering that this thesis engages in philosophy of science it should be noted that structuralism here must not be confused with the account on scientific theories that is associated with Patrick Suppes and his followers.

account, sounds are not just “means for expressing ideas.” (Saussure 1959, 112) Saussure thus opposes a reduction of a theory of language to a theory of names, which he associates with philosopher’s accounts on language at his time. Claudine Normand observes that for Saussure, philosophers are unaware “of two important aspects: on the one hand that the most important function of language does not consist in designating things, but in relating and combining words in different ways; and on the other hand that language is continually moving and transforming itself.” (Normand 2004, 97)

With respect to change in language Roland Barthes points out that Saussure was concerned with – and we might even say about – the stability of language (Barthes 1988, 161). Since the connection between the word and the thing, or, for Saussure, the impression and the concept, is arbitrary, it seems to be vulnerable to changes. But instead, we observe that language, even if subject to change, is rather conservative. There must be something that holds the arbitrary association in place. The solution Saussure finds has two dimensions, the social character of language and, – his greatest innovation, – the idea of language as relating systems of pure values. The first point is rather clear. In that the connection between the signifier and the signified is conventional, it cannot be changed by the individual speaker (Saussure 1959, 73-74).[6] Language does, of course, change over time. Saussure speaks about the nature of changes that can occur, and what is systematic about them flows from the systemic character of language. He does not say much, though, about the causes and conditions of change, because of the great variety of factors involved (Saussure 1959, 76-77). In any case, the factors of change are constrained by the community of speakers. Changes are therefore mostly unconscious gradual deviations that

[6] For Saussure the social character of language mainly concerns the constraints of conventions on the individual speaker. The social determination of language in the sense of language reflecting the life-world of a people (the environmental circumstances, the resulting habits and the social order) that was emphasized by William Dwight Whitney when he speaks of language as a social institution, although acknowledged by Saussure, for him was not part of the core problem of linguistics, but rather an external issue to be dealt with by ethno-linguists (see Normand 2006, 95).

sweep through a community of speakers. Or they are the result of changes in the cultural influence of one language community on another. Accordingly, they are always social phenomena. Finally, even if change is due to a conscious act by an individual speaker, such a speaker only has the power to change conventions, if she is licensed to do so by the community of speakers. That is, when its members are ready to follow her. Scientists, speaking to a conservative audience, but with a certain authority, might under some circumstances be in the position to change language, but just as well might a teenager on the streets change the language, speaking without authority, but to an open audience, ready to pick up the new slang. Given the aims of this thesis we shall come back to the conditions that allow a scientist to introduce a new signifier and thus a new signified. But authority alone is not sufficient to explain how changes are introduced and convention alone is not sufficient to explain stability. This is where Saussure's concept of languages as relating systems of values comes into play.

Languages combine systems of signifiers with systems of signifieds. According to Saussure, what makes a set of signifying or signified units a system is that the units are determined only through their position among the other units. That is only within the system they have a value. The relation they hold to other units is that of being different. With respect to concepts, or ideas, the units of thought, he contends:

> Instead of pre-existing ideas then, we find [...] values emanating from the system. When they are said to correspond to concepts, it is understood that the concepts are purely differential and defined not by their positive content but negatively by their relations with the other terms of the system. Their most precise characteristic is in being what the others are not. (Saussure 1959, 117)

And with respect to phonemes, the units of sound he writes:

> Phonemes are characterized not, as one might think, by their own positive quality but simply by the fact that they are distinct. Phonemes are above all else opposing, relative, and negative entities. (Saussure 1959, 119)

Saussure rejects the notion of a sound expressing an idea; instead it is the sound as opposed to other sounds that is capable of expressing the idea as opposed to other ideas. A sign, however, the unit of a signifier and a signified, is not defined negatively as what it is not, but is a positive unit. It has meaning in that it links a sound to a concept, within the system. Nevertheless, it stands in opposition to other signs in that the sounds and concepts it associates do.

Saussure imagines sound and thought as unstructured continua, which, using his own metaphor, later became called the plane of expression and the plane of content (Saussure 1959, 112). If we look at a sign, however, we can isolate the signifier and the signified as units of expression and content. Such units alone have no value, that is, no meaning. The value of a unit is not intrinsic and that is to say that the unit itself is not a given. For Saussure units of expression as well as content only exist in that chunks of the expression or content plane come to stand in certain relations, which means that they acquire a value. These relations thus have two dimensions (Saussure 1959, 115). First, a unit of expression has a value only in that it is linked to a unit of content and vice versa. Second, it has a value in that it is different from the other units in the same plane. The first relation is that of signification and the other can be called, with Saussure, the relation of opposition. The first relation is characterized through interchangeability. In communication, the speaker can, and indeed must, exchange the content unit for the expression unit. The second relation is characterized through the fact that exchange would result in a different sign. If we exchange the signifier in a sign with another signifier of the same system, a new sign would result and the system as a whole would be changed. It follows from the first dimension of value, that, even if we speak of systems of expression units and systems of content units, the systems on each plane are only systems in that they are combined in a system of signs (what semioticians later will call a code).

The difference between signification and opposition must not be confused with the well-established difference between the object a sign refers to and the way it refers to the object.[7] First, reference does not come into play here, since Saussure only speaks about the relation of an expression (or more precisely the sensual impression of an expression) and a concept, which is understood as a mental phenomenon, not an object in the world. Second, the two relations apply as much to the signified as to the signifier.

The way Saussure argues for the fact that units (of expression or content) are not determined by their intrinsic properties, but instead have a value only in relation to a unit of the respective other plane and to other units of the same system in the same plane, is as follows (Saussure 1959, Part Two, Chs. III and IV):

First, isolated signifiers or signifieds have no value outside a system. While the idea of an isolated unit of content might be difficult to conceptualize, we can clearly see that a sound of which we do not know what it signifies, nor what to compare it with, would give no hint on what it signifies or whether it is part of a language at all (the same goes for a mark of ink or any other potential signifier).

Second, while it has been said that units cannot be exchanged with other units to which they are opposed within a system, at least for expression units, they can be exchanged with other units that are not, but then become part of the system. Imagine a language some scientists use in a lab in which the object //red card// applied to a cage signifies *animal treated with x*, //blue card// *animal treated with y* and //green card// *animal not treated*. If //red card// and //blue card// are exchanged their value has changed. The system of expression units is conserved, but the code has changed. If //blue card// occupies its old position plus the former position of //red card//, we have another system of expression and another code in which //blue card// signifies *animal treated*. But if //red card// is exchanged by the sound "red", the later takes on the value of the former, and the sign system remains

[7] This distinction appears in various ways and with different terminology in philosophy (*Bedeutung* and *Sinn*, extension and intension, denotation and connotation).

intact. In fact, //red card// could also be exchanged by the sounds “blue,” “Torre di Pisa” or “uzdiblak.” The important point is that the signifier is different from all other units in the system. We could also exchange the whole system of expression by a system consisting of the sounds “red,” “blue” and “green,” or any other set of three distinct sounds. That shows, that it is the place in the system that determines the value, or, as we might say, grants the functioning of the unit. It is less clear, however, what it would mean to exchange the content unit *animal treated with x* by the content unit *Torre di Pisa*. The first two arguments for a relational view of the value of units therefore seem to reveal a certain asymmetry between expression and content that is not fully acknowledged by Saussure.

A third point applies to both planes: The value of a unit can even be changed if neither the unit nor the corresponding unit on the other plane is changed, just by changing any other unit in the system on one of the planes. Saussure gives an example:

> Within the same language, all words used to express related ideas limit each other reciprocally; synonyms like French *redouter* 'dread,' *craindre* 'fear,' and *avoir peur* 'be afraid' have value only through their opposition: if *redouter* did not exist, all its content would go to its competitors. [...] The value of just any term is accordingly, determined by its environment. (Saussure 1959, 116)

This means that if the expression unit *redouter* gets lost, the content unit 'dread' is not signified by the other expression units, but this content unit does not exist any more. What happens is that the content units 'fear' and 'be afraid' change their value in that they cover more of the content plane. This is also implicit in the above example. If the signifier //red card// gets lost from the code, all expression and content units change their value. A similar argument can be derived from the comparison of different languages. Saussure goes on: “If words stood for pre-existing concepts, they would all have exact equivalents in meaning from one language to the next; but this is not true.” (Saussure 1959, 116) The different ways in which languages segment a content plane is famously illustrated by a diagram of Louis Hjelmlev (1961, see also Eco 1979, 73):

<table>
<tr><td rowspan="2">træ</td><td>Baum</td><td>arbre</td></tr>
<tr><td rowspan="2">Holz</td><td rowspan="3">bois</td></tr>
<tr><td rowspan="3">skov</td></tr>
<tr><td rowspan="2">Wald</td></tr>
<tr><td>forêt</td></tr>
</table>

Table 2.1. Different languages segment a content plane differently, after Hjelmslev (1961).

The diagram shows how three different languages (French, German and Danish) distinguish different content units. “Bois” for instance signifies *timber* and *small forest*. Even if one maintains that here one expression unit signifies two concepts, it sounds plausible to say that the German language does not codify a difference between a large and a small forest. Of course, people can make the distinction *ad hoc*, using the diminutive “Wäldchen” (*small forest*), but then they would actually introduce a new local code, in which “Wald” would indeed have a different value.

The systemic character of language contributes to the explanation of its stability, because if a change is introduced the whole (sub-) system is changed, which must meet much stronger resistance in a community of speakers than a change that only applies to one unit. On the other hand the systemic character explains much of the patterns in which change occurs if it happens. It also makes us sensible to what happens if changes are introduced, namely that they never affect only one sign, but have repercussions in the whole system of systems, which is particularly important if conceptual change in science is studied. This brings us to the other motivation for emphasizing the systemic nature of language apart from understanding change and stability. It relates to the first critical point against the philosopher’s view of language that Normand attributes to Saussure, their occupation with naming. This criticism is strongly motivated by the observation that there are not the same units in all languages or at different times, which was just illustrated with the help of Hjelmslev's example. Comparing the sets of units given in different languages suggests the image of the continuum on both sides, expression and content, that can be divided differently. Since the division seems not to be determined by the "stuff" the continuum is made of – because otherwise it could not be different in different languages, – the units are not defined by the chunks of the continuum they separate, but only negatively through their difference with respect to other units. Thus it is the units as having a differential value that define the chunks of the continuum. The continuum is segmented only in that there is a system of units delineating each other. Saussure writes:

> Whether we take the signified or the signifier, language has neither ideas nor sounds that existed before the linguistic system, but only conceptual and phonic, differences that have issued from the system. (Saussure 1959, 120)

Language as a list of names does not fit with these observations about language. But we need not follow Saussure in rejecting that signifiers signify things, just because we reject the view of language as a list of names for things. Instead we can think of a plane of materiality, which is only divided into units, the things, in a system of signs. From this perspective, it should become clear why Saussure's theory is attractive for the type of pluralism envisioned in this thesis. A good guide of where I want to go results if one exchanges the terms in Hjelmlev's diagram for terms signifying characters of organisms, and the different languages by different disciplines, research programs or scientific projects of whatever scale.[8]

Since the goal is to speak of things in the world, in particular, of parts and properties of organisms, as units that are not determined by internal features, but delimited by the other things and properties that are individuated, it will be necessary to complement the language of content and concepts in Saussure's approach for an extensional language, while at the same time retaining the systemic nature of language put forward in structuralism.

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[8] Again, how local such a system is – that is, how small and how transitory the community sharing the code is, — is a matter of degree and has to be determined on a case by case basis.

Critical Interjection: The World is Around

Before extensionalist theories of meaning are discussed, it is important to understand why the structuralist tradition is so reluctant to introduce reference into its theory of meaning and to point out what would be the potential benefits of doing so.

Saussure intended to describe *la langue* as the conditions for language as abstracted from all *langues*, the particular languages that are given to the linguist in the form of individual speech, *parole*. As Umberto Eco will put it later, referring to things in the world is something that can be done with language and thus belongs to *parole* (Eco 1979, 58). And it seems that Saussure held a similar view. *La langue* is the capacity to associate systems of differential values. In a given language a semantic field is segmented into units differently than in another language. The signs, an expression unit united with a content unit, can be used to refer to things in the world. The question whether things in the world that fall in the realm of one content unit are of the same kind or not, for Saussure, was not a question of linguistics. A Normand puts it:

> Saussure's concern was not whether or how a language represents the world, nor how it is related to thinking. Such philosophical topics were not within his purpose. As a linguist, thinking about the nature of language (*langue*) and how best to portray it, his concern was to make apparent how a language works as an everyday mechanism, at anyone's disposal. (Normand 2004, 97)

Saussure rejected a theory of meaning where words are names for things. He seems to think that if this would be the case, and concepts would thus just be mental representations of the things, then concepts would be given and thus fixed and the same in all languages. The only aspect of a sign that would be different in different languages would be the sounds that are attached to the concepts that represent things. But since it can be observed that languages display different systems of concepts that divide a semantic field differently, concepts do not represent things and the meaning of a sign is not given by its function to name things, even though, it can be used in this way by the individual speaker. This worry however, only occurs, if the world is though of as consisting of given and fixed kinds of things. If things are thought of as being delimited within the system, just as content units, the reason for Saussure's worries disappears.

Umberto Eco, writing within the structuralist tradition, even if he is also strongly influenced by Peirce' pragmatic semiotics, is more explicit about excluding reference from a theory of codes. He argues that even if states of affairs in the world might be the reason to establish a code, and a sign can be used to refer to them, they are not relevant for the functioning of a code. A message containing a term that we take to be referential would still be understood if the state of affairs would not be the case. This is shown by the possibility of lying. Eco distinguishes between the conditions of signification and the condition of truth, a distinction that maps on the distinction between intensional and extensional semantics (Eco 1979, 58-59). For Eco, the conditions of truth fall under a theory of sign production as opposed to a theory of codes. We can reply to this argument by saying that telling a lie is in the domain of sign production anyway, but if a theory of codes should be able to explain why in a situation of lying the receiver reacts with a behavior that does not fit the facts, then it must connect the signifier with a state of affairs that the receiver expects.

Eco further argues that if a word such as "dog" is used, it refers to all dogs. He writes: "But «all existing dogs» is not an object which can be perceived with the senses. It is a set, a class, a logical entity." (Eco 1979, 66) First, it is not clear why a set is not a material entity. The set of all dogs is nothing but all dogs. If dogs are material entities, the set of all dogs is one as well. Second, a term like "dog" should not be taken to refer to the set as an entity, but to every dog severally (at least according to Goodman, see below). But furthermore, it is not clear why what is referred to must be perceptible or even perceived. Both of Eco's arguments for content instead of things as signifieds resemble that of Saussure for sensual impressions instead of the sounds as signifiers. Saussure says that we can have the impressions of the sounds in our head (speech-thinking) even if there is no sound. Eco argues we can have the things as concepts in our head without the things being around. But that is not an argument against the fact that words refer to things even if those are absent when the word is used, as will become clear in the discussion of Putnam's view. One might say with Putnam that referring terms have an indexical component, but they are not like deictic gestures (see below). The question is not so much whether we can have a satisfying theory of language that can deal with words like "mouse," – maybe this can be achieved without reference to referents – but whether we can have a theory of how mice are recognized, as different from, say, rats. Philosophers want to know the conditions for knowledge about mice and not only about "mice." And since language seems to play an important role in the generation, acquisition, structuring and maintenance of knowledge, they should make their theories of language speak to their epistemology. Now linguists and semioticians are not philosophers, but if language has a role in epistemology, then epistemology should have a role in a theory of language as well. The world is around and it has to be taken into account in any theory of meaning.

My main argument here, and that goes orthogonal to the usual disputes between intensional and extensional semantics, is that the whole idea of segmenting a continuum fits better with an extensional theory of meaning. The idea that thought is a "shapeless and indistinct mass" is plausible just because thought is difficult to access and thus can be described as anything, even as a "vague, uncharted nebula." (Saussure, 1959, 111, 112) But for the same reason it is not particularly informative. Instead of thinking of a continuum of thought that gets structured into a system of content units consisting of *forêt* and *bois* delimiting each other, it is easy to speak of a material continuum going from a single tree to a group of millions of trees covering many acres of land. Where the idea of a segmented continuum of thought becomes untenable is with respect to the differences that define a unit. Even if a unit is not defined by some intrinsic property, but instead by its position in a system, in the case of sounds, we can still say when two sounds are different. But when are two content units different?

This is why I contrast the structuralist version of an intensionalist semantics with an extensionalist view presented below in form of Putnam's account, instead of other intensional theories such as those of Frege, Russell or Carnap that are usually contrasted with extensional theories such as Putnam's. Since I am interested in the very notion of units delimiting each other in systems of units and intend to apply it to entities in the world (which might be objects, events or entities that fall in other ontological categories), I want to retain the idea of systems from structuralism, but show that it works also, and even better, in an extensionalist framework. A view that combines the virtues of structuralism with extensionalism is Goodman's that is therefore the last position discussed in this chapter.

Putnam: Signs Refer to Things

Putnam develops his model of meaning in opposition to views that, at least on his interpretation, suggest that the extension of a term is fixed by its intension, which is associated with a psychological state of the person using the term.[9] Knowing the meaning (intension) of a term, according to this view amounts to the ability to give a description of the objects the term refers to or to name the properties, the possession of which are necessary and sufficient criteria for an object to fall in the extension of a term.[10] As an alternative to this view, Putnam formulates his socio-linguistic hypothesis, which states that someone who uses a word to refer to objects in the world does not need to know of

[9] Putnam's view is often associated with Kripke's; Hacking (2007a), instead, argues that what their accounts mainly have in common is the rejection of such views and that apart from that they differ in many respects. I will not discuss Kripke's theory here.

[10] The view is often called descriptivism and usually associated with the accounts of Frege and Russell. Putnam interprets also Carnap as building his account on the speaker's knowledge. Structuralist semantics in the Saussurean tradition, for Putnam would have to be rejected for the same reasons as being mentalistic. It would fare even worse in not even giving an account on extension at all. Nevertheless, the amount of ignorance it takes to write a paper with the title *The meaning of "meaning"* and not even mentioning such an important tradition in linguistic semantics is surprising.

any object if it is in the extension of the word. There must, however, be people who are experts with respect to the objects that are in the extension of that word. In this way, the reference is not fixed through the psychological state of the speaker, but instead through the speaker's membership in a community of language users, which also comprises the experts who can fix the reference (Putnam 1975, 227-229). What the experts are capable of is to identify objects as being of the same kind. Of any given object the expert must be able to decide if it is of the same kind as those objects to which the word applies. This implies that objects are the same with respect to a particular property. Putnam's examples focus on "microstructures" or "hidden structures," (Putnam 1975, 232, 235) because he focuses on those words that have a meaning in natural language as well as in science, such as words for substances (water, gold) or for biological genera or species (tigers). In these cases the experts are the scientists and the properties might be microstructural.

This view needs not necessarily to be interpreted as essentialist, monistic, or scientistic. Putnam is not ignorant towards Goodman's (e.g. 1972, Ch. IX, 2) remarks that any two objects have similar properties. It is not the fact that two objects have any property in common that makes them being of a kind, but the fact that the possession of this or that property became a reason to attach a label to all possessors. This is compatible with the pluralist conception that an object can be of many kinds, or even that objects become delimited differently. Which property is chosen depends on the respective interests.[11] There is also no need to read Putnam as saying that the expert must in any case be a scientist (as e.g. Elgin 1983, 15 does). It can easily be musicians, lawyers, or polo players. Finally, the property that establishes the sameness relation is not necessarily a hidden or microstructural property, neither within nor outside the sciences.

[11] Hacking (2007a) emphasizes the non-essentialist and pluralist reading of Putnam (1975), especially in the light of Putnam's later writings. There is also the possibility to advance a less (from my perspective) charitable reading, but I am not interested in determining what Putnam actually meant. What is important is that if I integrate aspects of his theory in my account then only under the assumption that it is compatible with a non-essentialist, pluralistic and non-scientistic account.

According to Putnam, a (natural) kind term like "water" has "an unnoticed indexical component: 'water' is stuff that bears a certain similarity relation to the water *around here*." (Putnam 1975, 234) If someone utters the word "water," it is not required that she has a special knowledge that determines the extension of the term, but instead she uses the word to pick out everything that is water, that is, everything that is the same as what is called "water" by the linguistic community she belongs to, due to its possession of a certain property, in this case the chemical constitution, which can, in principle, be determined by an expert who is part of that community.[12]

The non-expert speaker can use a word to refer to objects in the world, because someone has used the word as a name for things of a certain kind and there is a (causal) chain of communication connecting any speaker with the event of naming. Invoking the origin of a term in this way is problematic though. It is hard to imagine that there was a single event where water was recognized as a substance and given a name. But even in the case of relatively recent terms that originated from a scientific context such as "gene" it is not straightforward. Even if we know that it was Johannsen who introduced the term in 1909, the story of the different kinds of entities the term is or was used to refer to and the various alternative terms that have been used to name them is quite complicated (Kitcher 1982). But it can be argued that for Putnam much less hinges on the event of baptizing the kind than is often suggested. The recognition of a kind might be a rather blurred process, and if there was a single baptist she might have had a very imprecise way of fixing kind membership from the perspective of later experts. The important thing is that there are now experts in the community who can say if an object belongs to the kind of stuff we came to attach a label to. Therefore, Putnam also speaks of collective reference instead of causal

[12] maybe we can compare the situation to a case where a person who was led to a place being blindfolded still can meaningfully use the word "here," relying on the fact that the place can be determined. (Whether there is an analogue case where someone uses "I" without knowing the referent depends on the notion of personal identity one assumes.)

reference. What matters is not the uninterrupted causal chain from the moment of introduction of the term, but the possible chain from a speaker to an expert.

The point in Putnam's argument that probably lends itself most to an essentialist reading is not his constant invoking of microstructures, but the postulated constancy of kinds. Putnam lets us imagine that experts today have a better method to identify gold than Archimedes had (Putnam 1975, 235). Accordingly, there might be some pieces of metal that Archimedes called "gold," but that are, according to today's standard not gold. Being good pluralists, we should think that, since the gold and non-gold pieces had many properties in common, in particular the property that Archimedes used to identify what he called "gold," they were of one kind and Archimedes used the term "gold" (I will use the English instead of the Greek term) to name this kind. Today we just divide the world differently, seeing a common property among what we call gold that non-gold lacks. Archimedes categories are as right as ours (fitting and entrenched). Putnam, however, argues that when Archimedes applied the term "gold" to what we now recognize as non-gold, he was wrong. This seems to be in conflict with the pluralism suggested here. But a reconciling reading is possible. Archimedes was indeed wrong, if it is assumed that he was interested in substances as a natural philosopher. Today's experts separate non-gold from gold, because it "behaves differently" in many (possibly rather artificial lab-) situations. If they were now to perform such experiments where gold behaves very differently from non-gold in Archimedes' presence, he, being a natural philosopher, could not help but being interested in this difference (Putnam 1975, 237). If there is a difference in behavior of a substance, there is a difference in kind. There are, of course, differences between any two things, but the question is whether these differences have been made visible and if the kind of difference is of interest for an observer. Under the assumption that Archimedes had an interest in "physico-chemical reactions," we can say he would agree on the modern use of "gold."

There might be other interests. If one piece of gold has been mined on a Monday and the other on Tuesday, there is a difference, but it will not be visible unless the pieces come with a paper-label indicating their place and time of mining. If someone were to tell to Archimedes that gold mined in place A can be distinguished from gold mined in B, because it has a slightly higher amount of copper traces, which makes it more reddish, then Archimedes, being a polymath and thus also interested in geology as well as trade issues, would immediately agree that there are two kinds of gold, gold from A and gold from B and he would agree that different names should be used. How the names are applied if a new difference is made visible is another story. One can use "gold" for gold or for non-gold or use it to name the kind made up of non-gold and gold, and use two new names to address the difference.[13]

Returning to the expert/non-expert relation, Putnam notes that the non-expert is still required to possess some kind of knowledge associated with that term, which mainly comprises some characteristic properties of the objects in the extension of the term, what he calls the stereotype. It allows the non-expert to understand statements that make reference to these objects, but it is not what fixes the reference. Indeed it can even be false. Though the speaker might use the term in the absence of objects in its extension to refer to them, she might also encounter an object, of which she needs to determine of which kind it is. She will settle the case by relying on stereotypic properties.

Putnam concentrates on the expert's capacity to determine kind membership and the role of stereotypes for the non-expert language user. It is important, however, to realize that also the expert has a specific knowledge associated with the kind, about the properties that settle kind membership and the operations that test for these properties. Since the knowledge itself is not meant to be the intension of the term, we have to think of it as

[13] Hacking (2007b), commenting on jade as one of the other examples of kinds given by Putnam wonderfully shows — drawing on work by La Porte (2004), — how different names have been attached to track different types of differences, ranging from differences in superficial color, via place of origin to chemical reactions.

having the same format as the stereotype, just being more refined. Thus on the one hand, experts have specific knowledge attached to the name and they also speak about objects when they are not present. On the other hand, even if non-experts might often use terms in the absence of the objects they refer to, they also apply operations to determine the kind membership of an object if they meet one, just not as reliable ones. If a lay speaker uses a term like “gene,” she refers to genes, although she will never encounter genes (although she might encounter the gene’s effects, which are also the scientists’ access to genes), but in the case of a piece of gold, the non-expert speaker has her ways of recognizing the object as belonging to the kind, which consist in testing for the properties that are part of the stereotype. Imagine a person who encounters a piece of metal and wants to test whether it is gold by looking for the property yellow color, which is part of the stereotype. It can easily be seen that looking at the object is an operation just as in the case of the expert, just an unreliable one. For instance, the person will hold the object in appropriate light, so that she will be able to evaluate the color. The operational character becomes even clearer when she bites on the object to test the degree of hardness. The relation between the experts and the non-experts with respect to their knowledge and operations can be summarized in Table 2.2.

<table>
<tr><th>Expert</th><th>→ Communication links →</th><th>Non-expert</th></tr>
<tr><td>Deciding operation</td><td rowspan="2">→ Simplification or refinement ←</td><td>Hinting operation</td></tr>
<tr><td>Knowledge</td><td>Stereotypic knowledge</td></tr>
</table>

Table 2.2. Comparison and relation between experts and lay speakers

There are two shortcomings of Putnam’s account with respect to an account of meaning that is supposed to speak to the problem of the individuation of objects or properties. First, the expert’s “ways of recognizing” something as being of a certain kind, that is, the notion of operation, is not explicated. Second, the theory of the common knowledge the speaker employs (the markers and the stereotype) is not well elaborated. Both shortcomings result from ignorance towards the systemic nature of meaning. I will concentrate on the role of operations below. Concerning the second shortcoming, linguists and cognitive scientists have very elaborated theories about the format of mental representations to offer, which will not be discussed here.

I follow Putnam, however, in maintaining that there are terms that are not only used to refer to stuff in the world, but that do refer. Reference cannot be relegated to pragmatics. Instead the distinction between semantics and pragmatics should be taken as what it is: a broad indication of dimensions of the same process, not as designating two separated domains that require separated theories. Putnam's arguments do not only show, contra Eco, that it is possible to refer to things if they are absent or make up an indefinite set of individuals, but that we can even refer to them without having the content unit. A non-expert can use the term "gene" and refer to genes as what experts would determine to be genes, without even having a proper stereotype at all. What terms refer to is part of their meaning, even if the kind of object referred to is not around or if the speaker would not be able to recognize it if she sees one. I pursued a pluralist interpretation of Putnam, that makes his extensionalism more compatible with Goodman's, which will be discussed next, in order to see how the systemic nature of language and other symbol systems that was emphasized in structuralist semiotics can be retained in an extensional semantics. The first shortcoming of Putnam's view, a missing explication of operations, is shared by Goodman. A fix will be attempted afterwards (Chapter 3.).

Finally, it should be remarked that Putnam's sociolinguistic theory adds another dimension to language as a social institution, apart from the maintenance of conventions that was emphasized by Saussure. In the context of this thesis, I will only speak about the limited social group of scientists. But, of course, scientists are always only experts with respect to some kinds of things and most of the time use kind terms as non-experts. Putnam's thesis of the division of linguistic labour, which on the present account implies a division of operational labour, should therefore be kept in mind in order to make the analysis of scientific practice sensible to the question whether a user of terms referring to parts and properties of organisms is speaking as an expert or non-expert with respect to this term.

Goodman 1: Things in Systems

For Goodman, as for Putnam, the most important aspect of meaning is extension.[14] Reference is the fundamental relation between symbols and the world. It works in both directions. Labels (verbal, pictorial or other) denote the objects they apply to and refer to them. Objects instantiate the labels they comply with and under some circumstances can be used to exemplify the labels. In this case they are samples (or exemplars) and refer to the labels.

This is in contrast to the structuralist tradition, where a label (signifier or expression unit), or its sensual impression, denotes a concept (signified or content unit), instead of objects in the world. Furthermore, the way a sample signifies would be spelled out in terms of denotation as well in a structuralist account, such that an object in some circumstances would be said to denote the property it exemplifies as a content unit.

Thus Goodman's theory of reference does not only warrant to speak about labels as applying to things in the world, but also introduces an important difference in the way objects refer to others. Exemplification is particularly important for complementing Putnam's theory, because what the expert does when ascertaining the kind membership of an object, is to use it to exemplify the kind by letting it exemplify the property that defines the kind (or rather the label that applies to all objects that possess the property). But most importantly, Goodman, and that distinguishes him from most philosophical accounts on meaning in the analytical tradition, especially from other extensionalist accounts, has a notion of system, that was emphasized so strongly in the Saussurean tradition, but which is

[14] To be precise, for Putnam meaning is extension plus stereotype, while Goodman just speaks of the extension of a term and reserves meaning for another aspect (see below).

in his case applied to the relation between labels and objects, and not to that of sounds and thoughts.[15]

Before we turn to Goodman's notion of a system, some details of his account on reference are in place (Elgin 1983, 19-23, provides a convenient summary). In the case of singular denotation, a label denotes one object or a group of objects. Thus "Mont Blanc" denotes the Mont Blanc and "Mont Blanc Group" denotes the 18 peaks of the Mont Blanc Group. A general label such as "peak" denotes all the objects in its extension (all peaks). It does not denote the class of peaks. If one is not a strict nominalist, as Goodman is, one can allow for labels that denote classes. Elgin gives the example of the label "human" that denotes all humans severally and "humanity," which denotes the class of all humans. If a label denotes distributively, such as "human" or "peak," it denotes all humans or peaks that exist, that have existed and that will exist. If a label denotes an object or event in the future, or in the past, such that we have no secure knowledge about it, its extension is unsettled. If the future arrives and the object fails to exits, the extension is null.

Exemplification is reference of an exemplar to a label that denotes it.[16] An object possesses as many properties as there are labels that apply to it, which means, in principle, indefinitely many (but of course not all). An object can only exemplify those labels that

[15] Apart from remarking that his general theory of symbols can be seen as an extension to structural linguistics (Goodman 1976, xi), Goodman makes no reference to the structuralist tradition, not in form of its American branch, let alone in form of the French (or Italian) heirs of Saussure. In particular, he neglects other attempts to construct a general semiotics that is going beyond the linguistic sign systems.

[16] Goodman uses "sample" instead of "exemplar," because he introduces the semantic concept with examples that we would readily classify as samples, such as a tailor's swatch. But once the relation is described, it becomes clear that it appears in many contexts where we would not call the exemplifying object a sample, such as when a work of art exemplifies. We can, of course, still use "sample" as a *terminus technicus* that has to be understood differently from the ordinary language word (namely in the way Goodman defines it), but still, "exemplar" seems more neutral. In science for instance, the term "sample" is used frequently, but even if all samples in science exemplify, not everything that exemplifies is a sample in the sense that scientists use this term.

apply to it and thereby be an exemplar of the other things to which this particular label applies. But it does not exemplify all the labels it complies with in every situation. While in the case of labels “to denote is to refer” (Goodman 1976, 52), in the case of objects mere instantiation is not reference. An object that complies with many labels might be perceived and used for many things, but only if it is used to refer to one of the labels it instantiates, is it an exemplar of the kind of thing the label refers to.

Reference cannot only go from labels to objects or objects to labels, but it can also go indirectly from objects to objects or more remote labels, or from labels to other labels or more remote objects. Goodman speaks of chains of reference (Goodman 1984). To explicate the notion, he introduces a denotational hierarchy. On the lowest level are objects and labels with null-denotation (fictive labels); on the next higher level are labels that denote and on the next level labels that denote denoting labels. Above there are labels that denote labels that denote labels that denote and so on. Now “any referential chain from an element to another at the same denotational level must pass through at least one other denotational level.” (Goodman 1984, 63) Take the following example of showing to someone the color of one’s house on a card with small fields of sample colors: the label “top right field” will denote and thus refer to the actual field, while the field in this situation exemplifies and thus refers to the label “red,” which in turn refers to the house.

Having explained how Goodman understands reference, I can now turn to his notion of system. In Goodman’s terminology, labels belong to families of alternative labels, that form a scheme, which organizes a realm, the part of the world it is applied to, into ranges of extension of the alternative labels (Goodman 1976, 71-72). A realm organized by a scheme is what Goodman calls a system. We can clearly see the parallel to Saussure’s systems of sounds and thoughts. The labels delimit each other’s ranges of extension, thus we might say with Saussure the labels as well as the kinds of objects have a value only within the system. The organization of a realm, or field of reference, can be the sorting of objects or the individuation of objects (Elgin 1983, 38-39). The realm can be thought of as one object or class of objects that constitutes the range of extension of one label in a scheme that organizes a larger realm, which figures in another system, and so forth. Concerning the sorting of objects for example, the scheme consisting of the labels "Gram-positive" and "Gram-negative" organizes the realm of bacteria, which is the extension of the label "bacterium" that forms a scheme with "archaea" that organizes the realm of prokaryotes that results from the schema consisting of "eukaryote" and "prokaryote" organizing the realm of organisms and so forth. If we have a eukaryote cell as a realm, which is the extension of the label "eukaryote cell," which forms a scheme with "prokaryote cell," a sub-scheme consisting of "organelle" and "cytosol" can organize the realm in the sense of individuating objects (parts) instead of sorting them (decomposition). Though "organelle" can be part of a scheme that organizes the realm in the extension of "eukaryote cell," the labels "organelle," "cell" and "tissue" could form another scheme, organizing the realm of the organism in a hierarchical decompositional fashion.

Accordingly, although Goodman does not make this point, a scheme (a set of labels) can be thought of as belonging to another label, which is either a hypernym (rendering the labels in the scheme hyponyms) or a holonym (where the labels in the scheme are meronyms). The first semantic relation pertains to labels that denote objects or properties and a label that denotes a more general class ("yellow," "green" and "blue" are hyponyms and "colors" is the according hypernym, but also "mice," "rats" and "squirrels" are hyponyms, where "rodents" is the hypernym). The second semantic relation applies where the labels denote parts and the other label denotes the whole ("organelle," "cytosol" and "cell wall" are meronyms of the holonym "cell"). It has to be noted that the hypernym or holonym to which a scheme belongs does not necessarily have the realm the scheme organizes as its extension. Thus "yellow," "green" and "blue" can organize the realm of flowers, but "flower" is not the hypernym to the color-terms, but "colors" is. I suggest therefore taking "colored" as the hypernym, because in this way the objects sorted by the scheme of hyponyms are in the extension of the hypernym. Since this is not the way the term “hypernym” is usually used, I will speak of meta-labels to which a scheme belongs.

A scheme that functions in a given system can be applied to a different realm. And a realm that is usually structured by one scheme can be organized by another scheme. In that case the labels are used metaphorically (Goodman 1976, 74ff.). Thus also metaphorical reference has the same systemic nature as literal reference. Applying a folk botanical scheme that usually organizes the realm of indoor plants in a metaphorical way, one might say that one person instead of another is a cactus instead of a flower. One label can function in several schemes, such as when “green” is opposed to other color predicates in one scheme and to “ripe” in another. And obviously parts of one realm can also be part of another realm. The DNA of an organism is part of the realm of the cell that is organized by a complex scheme of labels for cellular constituents, but it is also part of the realm of factors influencing ontogeny, which also comprises many labels that apply to factors of the environment.

According to the above said, exemplification must be systemic as well. To modify an example from Elgin (Elgin 1983, 72): A free sample packet of breakfast cereals might be distributed per mail to all households in an area. A customer might understand that this sample is supposed to exemplify “crunchy” and “healthy,” and not “arrives by mail” let alone “free.” This is because the customer is familiar with the convention of product samples. But if the packet is presented together with the description of its distribution in a marketing seminar, the participants might not immediately figure out which of the labels that apply to it are exemplified until the instructor announces “You have to bring the product to the people instead of waiting for the people to come to the product!” What he says is: This object and its mode of distribution by mail instead of another object that is given away for free in the supermarket exemplifies “bringing the product to the people” instead of “letting the people come to the product.” From this contrast the listener can derive the realm (modes of food sample distribution) and the scheme. Grasping the system, she can also make the following contrast: This object exemplifies "mode of distribution" instead of "eating experience," and thereby exclude "crunchy," as one of the labels that organizes the realm of eating experiences.[17] What I want to put forward is that no object alone is an exemplar, but only an object that is, at least implicitly, in contrast with another object (or many), just as no label has an extension except in contrast to the other labels in a scheme.

It has to be remarked that the meaning of a label for Goodman cannot be equated to its extension (this is why I said he uses "meaning" differently from Putnam). This becomes obvious in his discussion of synonymy. Two predicates such as "being gram-negative" and "having an outer membrane" are co-extensive, when taken as labels of schemes organizing bacteria. Nevertheless, they do not have the same meaning. Goodman suggests the following test to determine same or different meaning. Two labels have the same meaning, if all their parallel compounds, that is all compounds of both labels with a same third label are co-extensive (Goodman 1976, 204-205). Consider the compound labels "test for being Gram-negative" and "test for having an outer membrane." Since there is certainly another test for determining whether a bacterium has an outer membrane except from Gram staining, that falls in the extension of the second compound label, the two labels will not be co-extensive, and therefore the two atomic labels are not synonymous, that is, they do not have the same meaning. Note how meaning, which is used here in a context that is the classical motivation for intensional semantics (*Sinn* and *Bedeutung*) is, even if not reduced to, still spelled out in terms of extension.

The above was mainly concerned with linguistic labels. Pictures and other non-linguistic labels, according to Goodman, represent through denoting as well (they can also exemplify, but then they do not function as labels but as exemplars). Similarity has often

[17] Elgin uses the terms scheme and realm in the direction of reference. Thus a set of exemplars is a scheme that organizes a realm of labels (Elgin 1983, 122). I will not follow this suggestion, but speak about realms always as the extension of a set of labels that make up a scheme. Otherwise the description becomes too complicated when speaking of reference from exemplar to label and from label to exemplar together.

been advanced as a way to explicate the representational force of pictures. Goodman gives four arguments against this view (Goodman 1976, 4). First, the relation of similarity is reflexive; the relation of representation is not. Although an object is similar to itself, it does not necessarily represent itself. Second, similarity, but not representation is symmetrical. An object is always as similar to another object as this latter one is to the first, but still the first can represent the second without the second representing the first. Third, things can be similar without being in the relation of representation at all. And finally, an object, also a representing picture, can be very dissimilar to the object it represents, but still represent it. Goodman does not deny that often times there are similarities between a picture and the object it represents. His general concern, which underlies his whole philosophy, is not that there are no similarities, but that there are too many (Goodman 1972, Ch IX, 2). His mantra goes: Two objects share as many similarities with each other than any of them with any other object.[18] So the point is, just like similarity between objects does not determine a kind and thus the extension of a label, but, instead, the label groups the objects of a kind and thus points out the relevant similarities, pictures do not represent due to similarity, but instead, in denoting the object can point out the similarities between the picture and the object and thereby make the properties in which they are similar obvious, which might have gone unnoticed before.[19] The argument is important, because images are ubiquitous in

[18] An insight that Peirce attributes to Augustus de Morgan: "A great oversight which had vitiated the entire discourse of logicians about marks, and had prevented them from fully understanding what marks are, was corrected by Augustus de Morgan when he observed that any collection whatever of individuals has some mark common and peculiar to them. That it is so will appear when we consider that nothing prevents a list of all the things in that collection from being drawn up. Now, the mere being upon that list, although it has not actually been drawn up, constitutes a common and peculiar mark of those individuals. Of course, if anybody tries to specify a number of individuals that have no common and peculiar mark, this very specification confers upon their common and peculiar mark a new degree of actuality." (Peirce 1905, 43)

[19] Goodman's famously illustrates the point with the following anecdote: "To a complaint that his portrait of Gertrude Stein did not look like her, Picasso is said to have answered, 'No matter; it will.'" (Goodman 1976, 33; see also Robert Schwartz 1985)

science and also important with respect to the representation of characters. Besides similarity (iconicity) there is causality (indexicality) as a candidate to explicate the notion of representation in these cases. Goodman is silent about this, but an argument, parallel to Goodman's arguments against similarity as determining representation can be construed with respect to causality in indexical representation. An index cannot represent by virtue of its causal relation to the object it signifies, because it might actually stand in a causal relation to many objects without representing them and the signified object stands in a causal relation to many objects that do not represent it. Rather this particular causal relation becomes salient, because the index is used to represent the object that is because it refers to it. In the following, I will discuss the concepts that Goodman offers to analyze different symbol systems, including non-linguistic forms of representation, which are so abundant in science.

Goodman 2: Dense and Articulated Symbol Systems

Any utterance or inscription or other occurrence of a symbol that is produced or perceived, Goodman calls a "mark." A "character" (a symbol theoretic term used by Goodman, not to be confused with biological characters which are the subject of this thesis) is a class of marks. Marks can have some features that are constitutive and others that are contingent. Constitutive features are those that marks that belong to the same character must share to be character indifferent. Marks of the same character might have contingent features and even share them, but every such mark could be replaced by a mark that does not posses these additional features. Thus marks of a character are replicas with respect to the constitutive features. (Goodman 1976, 131-132; Elgin 1983, 97-98) Characters can also be compounds, "if the marks that belong to it are composed of marks that belong to other characters," otherwise they are atomic (Elgin 1983, 99). Words in English such as "do" are compounds. The mark "do" belongs to the character [English word do] and is composed of the marks "d" and "o" that belong to the characters [fourth letter of the Latin alphabet] and [fifteenth letter of the Latin alphabet], respectively.

Systems (in a broad sense, comprising many scheme/realm systems) can differ in their syntactic and semantic properties. In particular they can be disjoint or non-disjoint and, independently of that property, they can be differentiated or dense, both syntactically and semantically. On the semantic level they can also be ambiguous or not, while on the syntactic side they can be attenuated or replete. Thus there are a variety of different types of systems, although not all of these properties can be freely combined. The major distinction, however, is that between syntactically and semantically differentiated and syntactically and semantically dense systems, which can be captured by speaking of digital and analogue systems, or, as I will prefer articulated and dense systems. It has to be noted that being dense or differentiated is not a property of marks or their referents. The same mark can be interpreted under a dense or under an articulated system (Goodman and Elgin 1988, 127). For instance, while our Latin letters are usually interpreted under an articulated system, such that we can replace a handwritten letter by a printed one, a forensic document examiner will apply a dense interpretation to the same letter mark. With respect to the different actions that relate to dense and articulated representations, I will later speak of thick and thin phenomena as their referents. Now I shall explain in more detail these syntactic and semantic properties of systems.

A system is syntactically disjoint, if its characters are disjoint, meaning that none of the marks that belong to a character also belongs to another character (this requirement holds within a system — a mark might well be additionally interpreted under another system and thus belong to another character under this interpretation). Accordingly, if a mark belongs to two characters, but its replicas do not all belong to both characters as well (which would render the two characters one character) then these two characters would be non-disjoint.

Syntactic differentiation of a system is the case if "for every two characters *K* and *K*', and every mark *m* that does not actually belong to both, determination that *m* does not belong to *K* or that *m* does not belong to K'" is theoretically possible. (Goodman 1976, 135-136) This complicated definition is easier to understand if we look at cases of non-differentiation or density: Systems are dense if they "provide for an infinity of characters so ordered that between any two there is a third." (Elgin 1983, 99) That is, the system allows for the infinite construction of marks such that any of them belongs to a different character and this is why it is impossible to determine whether it belongs to one or another, as it is required for differentiation. The ordering matters here. Take a gauge without graduating dots or figures on the dial that measures some continuous quantity. If we determine the position of the pointer (a mark), every position constitutes a character, because every different position would be a mark that differs from another in a significant respect and thus belongs to another character. But how can we determine the exact position of the pointer? It seems that every time we look more closely, we would assign the mark to a new character (is its position at 12° or 12,1° or 12,09° etc.?) and thus between every two characters there is a third. Therefore, we cannot determine that the mark belongs to one and not another character. There might be perceptual or technical limits to our ability to discriminate the actual pointer position ever more finely, but the symbol system itself does not set such limits. Such a meter is analogue, because it is syntactically dense. The fact that it is not the mark in itself that is analogue, but the system, is easily illustrated if we imagine a dial that has graduating dots and the position of the pointer is interpreted such that it indicates the position of the dot closest to it. The same mark is now interpreted in a syntactically differentiated system.

Differentiation and disjointness are independent. Our letters (*qua* characters) are differentiated and disjoint, but if a letter-mark would belong to two letters, but its replicas would each belong to only one, the system would be non-disjoint, but still differentiated (the examples given by Goodman and Elgin are fairly artificial, and it seems that the combination of syntactic non-disjointness and differentiatedness is rather rare). And even if we have problems determining to which character a mark belongs in the segmented dial, every mark belongs to only one character, thus the system is disjoint even if it is dense. (Goodman 1976, 137)

Concerning semantic disjointness, we have to look at the relation between a mark and its extension or compliance class, that is, the class of objects it denotes as well as at the relation between an object and the compliance class it belongs to. A system is semantically disjoint, if marks of different characters have different compliance classes and if no object belongs to more than one compliance class. There can also be a situation where marks of different characters have different compliance classes, but there are some objects that fall in more than one compliance class. A system where many marks of different characters have the same compliance class is redundant. A system is only semantically disjoint if both criteria are met that is if it is not redundant and every object is in the extension of marks of only one character; otherwise it is non-disjoint (Goodman 1976, 149-152; Elgin 1983, 102-103).

Ambiguity is the case when marks of the same character differ in extension, that is have different compliance classes. The marks of unambiguous characters all have the same compliance class. There can also be a situation where a single mark has more than one compliance class. Being ambiguous or unambiguous is a property of systems different from semantic disjointness (Goodman 1976, 147; Elgin 1983, 101-102).

Semantic differentiatedness requires that "for every two characters of the system, and every object that does not actually comply with both, it is theoretically possible to determine that the object in question does not comply with one or that it does not comply with the other." (Elgin 1983, 103) In parallel to syntactic differentiation we can understand this definition by looking at semantic density: Here compliance classes (and thus the characters to which the marks belong that have the compliance class as their extension) are again so ordered that between every two there is a third. That means for every object that is in a compliance class that we cannot say whether it is not in another compliance class instead, if we look more closely. Elgin gives the example of the color of a damson plum. Is it purple, dark purple, halfway between dark purple and deep purple, halfway between dark purple and the shade that is halfway between dark purple and deep purple and so on?

(Elgin 1983, 102) The problem is not with plums, but with the system of discursive language, which is semantically dense in that its characters are so ordered that between every two there is another so that we cannot determine that an object does not fall in the compliance class of the one, or that it does not fall in that of another. A specialized restricted language (or subsystem) that some plum farmers and wholesalers might have agreed on could provide only the characters “dark purple” and “deep purple”; such a language would be semantically differentiated.

Systems that are both syntactically and semantically disjoint and differentiated as well as unambiguous, are called notations by Goodman. Western standard musical notation comes close to this type of system as well as blueprints for electrical circuits. Such notations are determining their compliant performances or circuits. Other notations are determined by what complies with them. This is the case with measurements performed with a digital (in the sense of syntactically and semantically disjoint and differentiated) gauge and also with diagrams that register such digital measurements. Languages (in the narrow sense of ordinary languages such as English) are syntactically differentiated, but semantically dense (as shown by the plum example). However, notational systems that are semantically differentiated can be derived from languages and often are in more specific contexts. In science, language is often used in a notational way, that is, semantic density is reduced. Ordinary language with its openness that stems from its semantic density Goodman calls “discursive language.” There are, as we saw, gauges that record analogically, and, accordingly, there are diagrams that are syntactically and semantically dense. Many pictures (such as paintings and drawings, but excluding notational diagrams, of course) are syntactically and semantically dense as well. Here a last distinction is in place that Goodman illustrates with the following example (Goodman 1976, 229-230): A momentary electrocardiogram and a Hokusai drawing of Mt. Fujiyama could consist of indistinguishable marks (black zigzagged lines on white background) but they are still part of different types of symbol systems. Both systems are analogue, but whereas in the electrocardiogram, the only constituent features are the relations of each point in the line to the x and y axis (even if we cannot say how many points there are, because the system is syntactically dense), while all other features such as the color or thickness of the line or the type of ink or paper (within the technically possible limits) are contingent. In the drawing all features are constitutive. Thus a drawing (and also a painting or a sculpture and probably many other symbol systems) has a large amount of constituent features (probably indefinitely many, although it still can have contingent features). It belongs to a symbol system that is syntactically replete, while an analogue diagram is syntactically attenuated in that it has only a few constitutive features. The difference is obviously gradual. Goodman calls attenuated systems "diagrammatic," although we saw that some of the symbols we call "diagrams" are notational. Replete systems can be called "representational," but it has to be kept in mind that the term "representation" is often used in a broader sense. Table 2.3. summarizes the rough classification of symbol systems, although it has to be noted that the combinations of the discussed criteria would allow for a more fine-grained classification.

	digital (articulated)		**analogue (dense)**	
syntactic	differentiated	differentiated	dense	dense
semantic	differentiated	dense	dense	dense
synt. repleteness			attenuated	replete
system type	notational	discursive	diagrammatic	representational

Table 2.3. Types of symbol systems according to their syntactic and semantic properties

Since this is particularly important for my reconstruction of different character concepts, I have to explain how these syntactic and semantic features also apply to exemplificational systems. Imagine a micrograph of a cell. First I will consider denotation again. The image functions as a label, that is, it denotes the cell. We can take the picture as a whole denoting the cell as a whole. In this case the image (probably with other images) constitutes an articulated system. But usually a scientist will take the structure of the image to represent the structure of the cell. In this case the image is interpreted as an analogue system. Every fragment of the image can be a mark so that it is impossible to say that a mark does not belong to one character instead of another, for between every two characters there is a third. Again, technical resolution might put a limit on the density of the system, but this is not part of the interpretation.[20] Such a system is also semantically dense, because for every object (fragment of the cell) it is impossible to say that it does not belong to one compliance class or not to the others, because the compliance classes, that is the extension of the characters of the image are so ordered that between every two there is a third. The image is also relatively replete, because many of its features are constituent. But all of this concerns the denotation of the cell by the image. When it comes to exemplification, we could look at the way the cell exemplifies the image, but this way of symbolization rarely takes place, because often the only access to the cell is the image.

I will instead look at the way the image exemplifies the labels that describe the cell via indirect reference. Now the fragments of the image are not denoting marks, but exemplifying marks. Instead of speaking of marks and characters in the case of exemplifying symbols as well, as Goodman does, I will speak of partial objects and kinds of partial objects, but it has to be kept in mind that there are no objects and kinds outside a symbol system, whether it is denotational or exemplificational. For every object such as a fragment of the image it is impossible to say that it belongs to one kind of object rather than the other, because under a dense interpretation, the kinds are so ordered that between every two kinds there is a third. The system is thus syntactically dense. It is also semantically dense in that, for every object (for every fragment of the image) it is impossible to decide if it exemplifies one label or the other. The labels that describe the objects are so ordered that between every two there is a third. That is, the image is analogue or dense and its description is dense as well. An analogue representation allows for an articulated reading. Therefore, in cases where a phenomenon, e.g. a cell or an organ,

[20] The interpretation might be said, instead, to dictate to move to a technique with higher resolution if the limit is reached.

is actually interpreted as exemplifying in a dense system, I will speak of the cell or organ as representing as a thick phenomenon, or of a thick concept of a cell or organ (see Chapter 3).

A diagram of a cell, schematically showing some main components and their relative spatial relations (in one of the possible cell states or phases) is articulated. The objects that make up the cell in this system can be taken to exemplify in a semantically differentiated way the labels that describe it. It is not a property of the world to be dense. If the world is represented as consisting of differentiated objects, or as exemplifying differentiated labels, then it appears differentiated. If it is represented as undifferentiated and as exemplifying labels in a dense scheme, then it appears undifferentiated. And articulatedness is not a property of denoting labels. Even the schematic diagram of a cell can be interpreted as symbolizing densely. Imagine a historian of biology, who wants to find out whether a certain diagram was drawn by Professor X or by her assistant. The historian will pay attention to differences that do not symbolize in the biological use of the diagram. Concerning the other direction of changing the interpretational scheme, of course, it is possible to arrive at an articulated representation of a cell, starting from an analogue representation (micrograph), but this means to interpret the image under a different scheme, as differentiated and attenuated. This interpretation could lead to the diagram of the cell. But obviously this does not go without many decisions about how to fix the characters and which aspects to take as constitutive, and different interpretations could yield different diagrams.

The distinctions presented in this section are of particular importance, because in the second part, I will analyse the character concepts of the different disciplines mainly in terms of the ways characters are represented, that is I will ask whether they are represented in dense or in articulated symbol systems. As I mentioned above, if phenomena figure in articulated systems, I will speak of thin phenomena, but also if they are represented by a symbol that allows for a reading in a dense system, but is actually interpreted as articulated. Only if they are represented and interpreted in a dense system, I will speak of thick phenomena. The distinction between thick and thin thus covers more than the syntactic and semantic properties of representations that figure in concept formation. It captures different attitudes towards the represented that articulate themselves in different actions that follow the acts of representation. This idea will be elaborated in Chapter 3.3.

Critical Chapter Conclusion: Missing in Action

In this section I want to make the point that all of the theories discussed above are missing an account of the role of action in the context of meaning. Saussure's structuralism has been explored in order to gain a sensibility for the systemic nature of signs. However, the rejection of the view of words as names for things by Saussure, led to a theory about the relation of sign-vehicles and concepts, which is not suitable for my purposes, that is to analyze how people speak about things in the world, in particular, about characters of organisms. I have argued that things should be introduced in the systemic picture. The point is not to keep things outside of representation in order make representation less rigid, as Saussure did, but to keep them inside in order to present the recognition of things as a dynamic process. Things are only there within symbol systems. And this is to say that they exist only in opposition to other things. The system of oppositions imprints on, organizes and correlates all layers from expression, via content to behavior and things.

Putnam's theory emphasizes the role of reference, but lost the systemic perspective. It is, however, attentive to the type of knowledge that is attached to a label (the stereotype, the expert's knowledge about defining operations). Goodman instead does not provide the means to speak about the conventional knowledge a sender or receiver of a symbol has to have and that determines her choice and interpretation of signs. But just because one wants a theory of meaning to include things, one does not need to throw out thoughts. Extensional theories of meaning will not fully get rid of talk implying some form of mental content, and they are better off if they make that explicit as Putnam does, than by trying to hide it as an embarrassment. Goodman also does not emphasize the social character of language as much as Saussure and Putnam, although it is implicit in his account.

Goodman is not interested in the genesis and maintenance of meaning. He, quite explicitly, rejects a historical perspective on language, looking purely at the synchronic relations between symbols and things. But even if there are rarely distinct baptismal events of the type that Putnam mentions, descriptive means are needed to account for the generation, maintenance and adjustment of meaning over time, if one analyses science. And since Goodman's notion of entrenchment is diachronic, and is meant to provide a criterion for the rightness of a symbol, a way to describe the history of symbol systems should be part of a theory of such systems.

The disinterest in label introducing event, is also the reason that Goodman does not provide a way to describe the operations that make an object exemplify what ever it shall exemplify. All theories presented so far lack an account of the role of action in meaning, that is, its relation to labels, concepts and referents. Before I specify the role of action, let me briefly use Putnam's gold example to illustrate why such an account is needed. Putnam's expert has to perform certain actions in order to make a piece of gold exemplify the properties that define gold, that is, its chemical constitution. Since the chemical constitution is not perceptible, it will be exemplified through other properties that are exemplified, which will usually be certain chemical reactions with other substances, that only gold can undergo. Or, what makes gold gold is that only gold can undergo those reactions, which allows one to infer to its chemical constitution. A piece of gold exemplifies the property to react with another substance if the other substance is applied to it. It reacts with the other substance, but since the other substance must be applied, it reacts to human actions in a way. It takes an action to let gold exemplify "reacts with test substance" and thus "is AU" and thus "is gold." A test substance can perform this function, because it only reacts with gold and not with other substances. This knowledge implies the action of applying the test substance to all chemical elements. In a particular test, it is applied to all candidates of goldhood. The test is a differentiating action in that it implements the scheme consisting of "gold" and "other substances," organizing the realm of chemical elements in gold and other substances. Once these categories are in place, differential action can become attached to them. For instance, in some situations, someone might keep gold and discard other substances. The action of applying the test substance to a realm of substances introduces a difference between things in the world and thus different categories. The actions of keeping or discarding are applied differentially, that is the things are treated as something different. Thereby the categories become entrenched. Actions, it becomes clear, just like labels and the objects they apply to come in families of alternatives that map on the semantic systems. They are indeed part of those systems. Very roughly, and omitting the content level, a system in a unified account of reference and action can be represented like this (Table 2.4.)

differentiating action	applying substance to A	applying substance to B
realm (metal pieces)	gold	other metal
scheme (labels)	"gold"	"other substance"
differential action	keep	discard

Table 2.4. The places of action in semantic systems.

Other actions will render other labels as exemplified and thereby divide the realm differently. Applying the action of tracking their provenance to gold pieces, for instance, could make them exemplify "being from country A" and "being from country B" and, if the appropriate knowledge is available, "being mined exploiting children" and "being mined in a fair way." Things that were of one kind under the other scheme are now in the extensions of different labels. These new categories might catch on in the sense that the actions of buying/not-buying become attached to them differentially. If nothing like this happens, the categories will not become entrenched and the differentiating activity of provenance tracking will probably be abandoned. The example makes clear that the fitting and entrenchment, the two factors that contribute to rightness or relevance, do not apply to single labels or kinds, but to symbol systems including sets of action alternatives.

Entrenchment can be spelled out not only as a history of labeling things differentially, but also as a history of treating things differentially. Fitting implies that a system of categories fits in a network of many other categories, such as "child labor" in the example.

With respect to gold, I spoke of the expert's activity in determining the kind membership concerning already entrenched kinds, but the example suggests that the same structure underlies the discovery of new kinds. In the following chapter, I shall elaborate further on the structure of differentiating and differential action and show how they give rise to thick and thin representations of phenomena. This will be the last step in setting the stage for the comparative analysis of character concepts in biology.

Chapter 3. Towards a Systemic Account of Practice

Chapter Introduction

David Gooding in his book *Experiment and the Making of Meaning* (1990) calls for a theory of observational practices that explains how scientific terms are meaningful: "A theory of the meaning of scientific terms should show how scientists invent new representations of natural phenomena." In the following I sketch an account on scientific practice that differs from Gooding's in that it emphasizes the systemic character of meaning that was discussed in Chapter 2. Additionally, it will not only account for actions that bring about new representations of phenomena, but also for those that relate to known phenomena.

In Section 3.1. I will first specify the distinction between differential actions, which are alternatives of actions that apply differentially to different objects, and differentiating actions, which are those actions that lead to the recognition of differences and thus enable the organization of a realm by a scheme, that is, actions that introduce systems. Differential action will be explicated in terms of affordances. In section 3.2. I will then discuss ostensive definitions and experiments as two closely related types of differentiating actions or procedures, as I will call them, to acknowledge the structural complexity of this type of actions. In particular, I will show how these procedures share a contrastive structure – that is why I will call them contrastive procedures –, and provide a more fine-grained typology of such activities according to their aims, focus and use. The main argument is that with respect to the systemic nature of knowledge, those actions that generate knowledge must be regarded as system introducing (rather than mere baptizing) events.

In the course of section 3.3. I will discuss in which sense contrastive procedures, enable, bring about and relate to various ways to represent phenomena. In particular, I will use Goodman's distinction between dense and articulated symbol systems, which are taken as results of contrastive procedures, as the basis for a distinction between thick and thin phenomena. The latter distinction reflects also the affordances of objects that become represented through contrastive procedures, that is, it is a distinction in terms of the differential follow-up actions.

The account of scientific practice sketched out in this chapter will provide an analytic instrument to investigate how characters are represented in different fields of biological research and at different times in the history of biology.

A Caveat: Philosopher's Language and Ontological Categories

We have to distinguish the descriptive language of a philosopher from the descriptive language in other fields such as art or science that might come under philosophical investigation. When it comes to ontological categories, such as object, property, event, process, state, class, whole etc., they have to be taken not as features of the world, but as features of representations, because the world appears only in representations. Whether we describe the world such that there is one object that over time goes through different states, or such that there are two or more different objects encountered at different times does not depend on the world, but on the way a representation is constructed. Take the famous children's book *The Very Hungry Caterpillar* (Carle 1969). It tells the story of a caterpillar that hatches, eats a lot, forms a cocoon and emerges as a beautiful butterfly. This is a kind of biographical narrative, focusing on the individual life history. We could imagine another children's book, that focuses more on the relations and conflicts among the (anthropomorphized) inhabitants of a garden, where a caterpillar and a butterfly are two different characters in the narrative (which is not absurd, given that life cycles of different species need not be synchronized). Scientists construct narratives as well and while a developmental biologist will clearly speak of the caterpillar and the butterfly as states of the same organism, an ecologist, being interested in the synchronic relations among organisms might speak of caterpillars and butterflies as different types of organisms, even though he will, of course, be perfectly aware of the fact that caterpillars become butterflies.

In a similar way, properties might be represented as objects or objects as properties ("red" becomes "pigment" and "wing" becomes "winged"). These different ways of putting things are not true or false, but right or wrong, where the latter distinction pertains not to the relation between a representation and the world, but the relation between a representation and other representations and the particular overall purpose of constructing representations in a given situation. But this means that the different ways of representation are not just matters of picking different grammatical forms, such as adjectives instead of nouns. The adequate form has to be picked to construct a narrative that fares well according to Goodman's various criteria of rightness, alluded to in Chapter 1. Thus if we want to investigate science in order to see how interests develop, how they interact with technologies, how certain entities appear and vanish in these contexts and how they are explained or ordered, we have to be attentive to the way scientists use language beyond single terms or predicates. To realize whether they speak of different states of one object or rather of two different objects will give us an indication on how their interest is structured or in which way their perception is influenced by the instrumentation they use. If they switch from one type of narrative to another, this might indicate some important change in practice or theory, but it might also just indicate a more local switch between sub-tasks that are guided by the same aim.

Here, I will not provide an account on how grammatical forms are used, how discourses can be classified (e.g. diachronic vs. synchronic narratives) etc. The reason I am mentioning this grammatical aspect of representation through language is that, if the way that scientists speak is described, there is always also the language of the describing author, whether she is philosopher, historian or sociologist. A commentator on science has her own descriptive goals and constraints. For instance, she might write about different scientists, one of them using an object/state description, while the other uses a two objects description. In order to make the two scientific accounts comparable and increase the coherence and tractability of her text, she will restrict herself to one type of representation, for instance she might always speak of distinct objects. This will yield sentences like "Scientist X describes the objects A and B as different states of the same object." By writing this, she would not say that scientist X is wrong and that there are actually different objects in the world that are mistakenly perceived as different states of the same object. Instead, she is just applying her own style of description that serves different purposes than the description of the scientist.

Especially when philosophers of science speak very generally about things that are also investigated by scientists – though by the latter in form of particular occurrences – such as causal relations, the philosopher will usually start with sentences like "I take the relata of a causal relation to be …," where "..." can be objects, events, variables or whatever is considered most suitable. Philosophers might actually dispute among each other which is the best way to go, and some of them even seem to imply that their ontology is *the* ontology, that their way of describing causality captures the right ontological categories and any other talk of causes is just imprecise *façon de parler*. But usually, they will just imply that their way of speaking is the best for the purposes of philosophy, leaving the language of scientists who investigate causal relations, let alone the world, untouched. It is important to make this distinction here between a philosopher's descriptive language and the descriptive language of a field that is subject to the philosophical analysis, such as the biological sciences, because sometimes confusion occurs concerning this point (that is because some philosophers actually make strong ontological claims and others do not and this is not always indicated). In the following, I will say some general things about meaning, action and causation, and I will use a particular language to express some structural features of the ways they are represented and used to represent. I want to avoid the impression that I think that the biological world has to be described using the ontological categories that I use here. The same goes for the terms I use to describe actions, such as the term "experiment." Biologists will use the ontological categories that they find appropriate. Sometimes they will have disputes about the right way of putting things. Scientific work is a collective endeavor and therefore, there is a continuous negotiation and adjustment concerning different modes of representation that can certainly be as abstract as discussing whether something should be considered an object or a property. The way I understand philosophy of science implies to analyze the underlying differences, but not to take a stance on such disputes.

3.1. Differential Action and Differentiating Action

Terminology

In this sub-chapter, I shall first show that there are actions or behaviors attached to objects, or, to put it differently, that actions or behaviors always stand in some relation to objects. I said that objects have to be understood as units in a system in which labels organize a realm. And if we want to speak of concepts, they too have to be construed as units in the system. If this view is accepted, then actions or behaviors just like objects, labels and concepts have to be considered as units in such systems as well. I will further argue that there is a special class of actions that result in recognition of new objects that introduce or change the systems. These usually quite complex actions all have a contrastive structure that allows new differences to become recognized and therefore will be called contrastive procedures.

"Action" usually connotes intention, while "behavior" connotes conditioning. These can probably be seen as extremes on a gradual scale. Since what I say in this chapter applies to both, I will use the terms largely interchangeably, although in the case of differentiating activities, action seems more appropriate; there will in most cases be an intention involved. However, the actual result might be quite different from the intended one, for instance, when a scientist stumbles upon a causal effect when handling some material for different purposes than finding its effects. And even if effects are sought they might be investigated in an explorative manner, without any preceding idea about what the effects are. I will later introduce contrastive procedure as the appropriate term for differentiating action. In the case of differential actions, I tend to use the term "behavior," because they are usually strongly conventionalized.

Many thinkers in many different contexts have emphasized the role of action in theories of meaning and in epistemology. Usually philosophers of language focus on the way actions reveal what words refer to. This reasoning is often closely linked to behaviorism (see e.g. Quine 1960). They usually have not much to offer when it comes to those actions that introduce meaning, except that they speak of "baptizing" events and discuss the role of deictic gestures, as we will see, without paying attention to the systemic nature of language. Philosophers and historians of science instead tend to focus solely on those practices that generate knowledge (for a position that emphasizes the role of action for a theory of the meaning of scientific terms, see e.g. Gooding 1990), but pay less attention to actions that relate to objects as already known. I will not review every such theory here, but focus on the constructive task of sketching a perspective on action that best fits with the systemic view of representation introduced above. I will speak about actions as coordinated with objects and labels in terms of affordances and about actions that yield new knowledge in terms of contrastive procedures.

Affordances

Every action is related in one way or the other to the kinds of things and relations between things that are recognized as existing in the world and thus also to labels and concepts. And if the latter come in alternatives, actions have to come in alternatives as well that map on these three partite systems of units. Units in the system have to be used, either by using the words, which is an action, or by using the things. But "using" here can mean many types of possible relations in which things and actions or behaviors can stand, including, for example, running away from something, which we usually do not call a use. maybe the best notion available in the literature to cover them all is that of affordances (Gibson 1986). Even though the connotation of the actual verb "afford" equally rules out actions or behaviors like running away from something, let alone falling off a cliff, the noun, being a neologism, can be taken as what it is defined to mean.

Gibson characterizes an affordance as a relation that holds between an organism and its environment. The environment can be natural or artificial, but this is a distinction Gibson wishes to overcome, arguing that the artificial world is not a separate world, but just manipulated areas of the natural environment (Gibson 1986, 130). The important point is that the environment has several perceptual dimensions. It consists of substances, which are not the pure chemical substances, but the actually occurring mixtures and compounds that have various qualities (such as soft, hard, solid, fluid, and infinitely many more). The substances are organized such that they have surfaces. Any region of substance has, of course, interfaces with other substances, depending on where the boundaries are drawn, but the perceptually most important interfaces, which are called surfaces, are those that a region of substance has with the medium that fills the space in which the organism can move or perceive, such as air for human beings. Surfaces have layouts, which means that they have geometrical properties such as being horizontal or vertical, flat or rugged, and many more. Now the properties of the substances and surface layouts combined are such that they afford certain behaviors. The terrestrial surface, for instance, being horizontal, extended and rigid, affords support and is thus perceived as ground. The ground, similar to the medium, has a special role, because it is the basis for all other behavior. But there are also all kinds of objects in the environment, some of which are attached to the terrestrial surface, while others are detached. Such objects again have many properties according to substance and surface layout and therefore, can afford various behaviors that can be as general as being graspable or portable, or more specific. One of Gibson's examples is that "a rigid object with a sharp dihedral angle, an edge, affords cutting and scraping; it is a knife." (Gibson 1986, 133) The affordances can be benefits or detriments. The knife affords cutting or being cut.

It can be asked why one should speak of "being cut" as a behavior at all? But it is exactly this kind of passive affordance that points to the symmetric relational character of affordances. "An affordance points both ways." (Gibson 1986, 129) An object can possess properties that can potentially affect humans, but affordance is only given if the human agent recognizes the object as having these potential effects. If the knife affords being cut, this relation only holds if the actor knows that she can be cut. The interaction of the object with the agent does not need to become realized, the important point is that it is perceived. But, apart from knowing that the interaction of being cut can happen, the actor will also display a behavior of taking care. One could also say that the knife affords taking care not to be cut or acting at the risk of being cut etc. Gibson wants to avoid presenting affordances as something merely subjective. What an object affords is constrained by its properties. One of the characterizations that Gibson provides goes: "These positive and negative affordances are properties of things *taken with reference to an observer* but not properties of the *experiences of the* observer." (Gibson 1986, 137)

Though Gibson does not explicitly say so, it seems thus that affordances cannot only be classified as positive or negative, but also according to the activeness (as opposed to passiveness) of the behavior and the direction of affecting. Humans can be affected by an object (e.g. being cut), behave in a particular way in the face of an object (run away), acting on an object (sit on it), or properly manipulate it (cut it). If manipulation results in an object with different affordances, Gibson calls the activity manufacture (Gibson 1986, 133). Thus some objects or substances afford being manufactured and the result of the behavior, if actually applied, is another object or substance, where the change indicated by "another" is a change in what the matter involved affords.

Affordances are obviously relative to the particular organism. What affords support for a water bug does not afford support for a human being. The concept is thus thoroughly pluralistic; an environment "offers many ways of life." (Gibson 1986, 128, 138) There will be different sets of affordances specific to a species, to a society and maybe also to an individual. And always-new affordances can emerge (evolutionarily or culturally) if the environment or the behavior changes (Gibson 1986, 129).

Gibson develops his theory in the context of an ecologically informed theory of perception. In the sense that he takes affordances to be directly perceived as such, instead of being derived from the perception of isolated qualities, his theory is a descendant of *Gestalt* psychology (Gibson 1979, 134, 138). This notion of direct perception is in line with the view that the objects are recognized as parts of a symbol system that relates objects, labels, concepts and behaviors (although there need not always be a label if there is a behavior). I will not enter the debate on the nature of perception here, but suggest that some elements of the environment that were either significant in the evolution of the species or are sufficiently conventionalized, are perceived immediately as objects affording certain behaviors. But perception will be more analytic in unfamiliar circumstances or can be purposefully be put in an analytic mode. This can lead to the recognition of new objects that can again become conventionalized, which implies that they acquire affordances. In a way, entrenchment can be described psychologically as the genesis of *Gestalt*.

Gibson's explication of affordance is very general, because it is meant to apply to many (though probably not all) organisms. I am interested in human action and behavior and I will interpret Gibson as implying that there are also purely conventional affordances, which are in no way determined by the properties of the substances or surface layouts of the objects in question. If in a given culture a kind of object that is identified by certain properties is taken to be valuable, it affords such activities as buying, selling, collecting etc., while the same kind of object might not be considered valuable in another community and therefore not afford the same actions. But even here affordances are not merely subjective. First, the object's properties still constrain the affordances. Mountains and rivers will not serve well as collector's items. Second, such affordances, though less physically constrained are more socially constraint. Selling and buying require a mercantile culture.

If affordances can be conventional, a distinction between sub-classes of objects or parts of a whole can also be enforced by activities, even if no difference in properties is recognized. If, for instance, members of a community come to treat some objects in a class as the property of group A and the other objects as property of group B, there will be two sub-classes. The objects in these sub-classes can become subject to very different causal interactions and in the long run, the objects in these sub-classes will probably acquire distinct properties apart from being owned by one or the other group. The belonging of the objects need not be declared verbally. It is sufficient that a pattern of behaviors gradually becomes established in which members of group A do use some objects and do not use some others, which in turn are used by members of group B, who in turn do not touch the objects the A group uses, or at least not without permission. We can call this process coordination (see Lewis 1969), and it establishes affordances and kinds; it establishes a system. The interesting point to observe is that affordances are not only relative to a community, but also to the roles that different individuals or groups take in a community. If we go back to the example of valuable objects, there are different roles, such that between the objects and people in different roles different affordance relations hold. A piece of gold affords selling to a jeweler, buying to a customer and stealing to a burglar. This also requires the reciprocal understanding of what an object affords to the respective others.

This understanding is part of human interaction that Gibson describes in a way that renders people as parts of other people's environment that afford certain behaviors just as other objects and also animals. In general organisms have affordances with respect to other organisms, which can be reciprocal or not (Gibson 1986, 135).[21] Affordances can also be relative to contexts. If certain context markers are present, an object might afford behaviors that it does not afford if the actor perceives other context markers together with the object. Affordance can thus also be described as perceiving an object in the context of other knowledge, for instance, about other objects.

The term "affordance" became used in many contexts, ranging from sociology to product design. Many uses of the term deviate from Gibson's original characterization. The way I use the term here is also not fully licensed by Gibson's text, but it shares the major intuitions. Objects do not come in "fixed classes" (Gibson 1986, 134). They are recognized by a population of actors and different populations can come to recognize different objects in the same material region of the world. Any object has a bundle of properties that have to be sensually perceived in one way or the other, even if the perception is very indirect, and the properties often explain the affordances, even if they are not perceived separately. I possibly start to deviate when I say that objects have affordances that are not explained by their properties, but coordinated with some properties. And further, that yet other objects have affordances that do not even apply uniformly to the objects in the class to which the affording object belongs due to its recognized properties. In these cases, I will say that differential behavior has been imposed on them by fiat.

The recognition of distinct objects is always connected to the behavior it allows or requires. The affordance perceived in a context determines that the object is perceived as this rather than that. One object can have general affordances and various more specific affordances. A stone can be graspable and moveable, but it can also be "a paperweight, a bookend, a hammer, or a pendulum bob." (Gibson 1986, 134) So one object can afford

[21] To be clear: affordances do not explain conventions or social roles, but they help to describe them.

several behaviors, but a given behavior can also be afforded by many objects (pieces of wood can be paperweights as well). This is analogue to the fact that one object can exemplify many labels and a given label can be exemplified by many objects.

But the most important analogy between the relation of objects and labels and objects and behaviors, between reference and affordance[22], is that behaviors like labels come in sets of alternatives that delimit each other in their range of application. Gibson alludes to this fact several times without making it explicit. For instance, when it comes to resting (a class of behaviors), some layouts (from the realm of reachable surfaces) afford sitting, while others afford kneeling or squatting (Gibson 1986, 128). In the context of the question of direct perception of objects as opposed to the perception of distinct qualities, he states: "Those features of a thing are noticed which distinguish it from other things that it is not – but not *all* the features that distinguish it from *everything* that it is not." (Gibson 1986, 135) This can be read as saying that from the perspective of perception in terms of affordance, objects are contrasted with those objects that are in the same realm but afford different behaviors, not with every other object.

At least most affordances need to be learned. Instructing someone to answer to an object with a certain behavior can be compared with instructing someone to use a label. This procedure of introducing or teaching a label is usually called an ostensive definition and it involves objects as exemplars. I will discuss ostensive definitions in detail below. Here I will just quickly point out that in an ostensive definition of a label and also in an ostensive instruction, as we might say for behaviors, not single labels or behaviors are taught, but schemes of labels or behaviors. This object rather than that object is called X rather than Y. And analogously: This object rather than that object allows or requires this action rather than that action.

[22] Reference has two directions: Denotation goes from the label to the object, while exemplification goes from the object to the label. Since affordance goes from the object to the behavior, I will call the complementary relation "answering." A behavior will be said to answer to an object if the object affords the behavior.

The parallel between schemes of labels and schemes of behaviors organizing realms of objects should make clear why such a view on actions and behaviors is important for the study of science, if one has already accepted that the study of reference is. But let me be explicit about why this can be an important tool in the analysis of scientific practice. In short, Gibson's theory is interpreted in a way here, that warrants speaking of objects like electrons and genes, but also human livers and *Drosophila*'s white eyes, as affording certain actions in certain disciplinary contexts. Many actions in science result in the recognition of new objects and I will speak about these actions soon. But there are many other actions that have to be accounted for if scientific practice is analyzed, actions that are performed when objects are encountered that are already known. Observing differential behaviors of scientists can help to understand which objects they recognize, which categories they have formed, just like observing the way they use labels does. And it can help to make pluralism explicit by showing that different groups of scientists either recognize different objects, or answer with different behaviors to commonly recognized objects. Without explaining how different disciplines, interests or perspectives in science come about, the notion of affordances as interpreted here, can still help to describe how they manifest themselves in patterns of action. An object that affords throwing-in-the-trash for one scientist can afford further-investigation for another, according to the different interests. An object that affords further-investigation for one scientist can afford using-as-a-tool (or instrument) for another, according to the point in history. An organism might afford contrasting-with-an-organism-of-another-species for one scientist, but contrasting-with-an-organism-of-the-same-species-in-a-pathological-state for another, according to the respective perspective. A cell might afford observation-with-a-microscope to one scientist and homogenizing-for-biochemical-analysis to another, according to the methods available, and so on.

But it is important to get the roles of alternatives right. Here I presented the situation from the perspective of the object. If we look at one object, different actors answer with different actions to it. However, that implies that from the perspective of one actor, she has alternative actions to apply to different objects. If one scientist throws away one object, she will keep another. The alternative actions are afforded by distinguished classes of objects that form one realm that is thus organized by the scheme of alternative actions. It will most likely also be organized by a scheme of labels.

These different pathways of actions could, of course, be described without referring to affordances. But I want to address any action as part of a family of alternative actions that maps on a system of objects, labels, and concepts. To emphasize this systemic nature of actions that is not always recognized, it is appropriate to employ a special terminology. Gibson's approach suits my purpose, because he makes clear that actions do not just apply to objects, but are part and parcel of the very concept of the object in question and, most importantly, he provides us with a term for the relation between the object and the action, analogue to reference as the relation between objects and labels. And even if he does not make the systemic character of actions or behaviors explicit, and does not mention the according contrastive character of actions that introduce new objects, he seems to imply at least the former. I will now turn to the particular kinds of actions that can bring about new categories.

Contrastive Procedures

Action is always reaction to some environment in a particular situation. When acted upon, parts of the environment react and they might react differentially, although having been perceived as uniform, or at least heterogeneous in an unspecified way before. In this case new classes of objects are recognized. Since differences appear in contrasts, differentiating actions must be contrastive and I will refer to them as "contrastive procedures." Concerning this terminology it should be said that there are alternatives. Above, following Putnam, I spoke of operations. However, the term might be associated with the program of Bridgman's operationism, which was prescriptive rather than descriptive, in that it aimed at fixing the meaning of scientific terms through describing an operation that defines the term

(Bridgman 1928). The account offered here, instead, is meant to allow for the analysis of the dynamic establishment and vanishing of systems of objects, labels and actions. Whereas Chang (2009, 27 ff.) suggests to read the term "operation" more liberally in order to understand the role of actions in discriminating objects and thus in fixing the reference of terms in science, Gooding rejects the term due to its problematic history and suggests "procedure" instead (Gooding 1990, 8). Before I turn to this term, I shall mention another term that has been used in this context, namely "intervention." Hacking (1983) uses the term very broadly, to indicate that human actors affect and are affected by the world and then discusses specific forms of interventions separately under various headings. This use of the term is not specific enough to characterize the actions I have in mind and it is also too much connected with the discussion of scientific realism, which I intend to avoid. Woodward (e.g. 2008) and others use the term more narrowly to refer to the coming about of changes in a variable that reveal causal dependence between variables. Woodward prefers the term over another term used in this context – "manipulation" – because for him the change in a variable cannot only be brought about by a human actor, as manipulation traditionally is taken to imply, but can also be the effect of a cause that is external to the causal relation studied. Both terms, however, are strongly connected to the detection of causes and – even if causal dependence will play an important role in my account on contrastive procedures – an appropriate term shall cover also actions that contrast objects to discover properties apart from their causal powers. I will use "intervention" to designate parts of complex actions, in particular in the context of causal analysis, but opt for "contrastive procedure" as the more comprehensive term.

Gooding's "procedure" seems appropriate for two reasons. He defines a procedure as "a sequence of acts or operations whose inferential structure is undecided," which means that "rationales for actions often emerge as the account unfolds." (Gooding 1990, 8) First, this definition emphasizes that actions are usually decomposable in series of sub-actions.[23] Second, it takes into account that sometimes the goal of the actions change along the way in the context of scientific investigations.

Gooding, however, even if he talks about concept formation, that is, about how labels come to denote phenomena in science or how the phenomena become known phenomena, and even if he rightly emphasizes the role of human agency, still fails to do justice to the contrastive nature of scientific practice, which stems from the systemic nature of knowledge. Phenomena – objects, their properties and capacities, the events they are involved in, are what they are in that they are different from other phenomena. What becomes a referent is referred to as part of a realm that becomes organized by the schemes of labels and behaviors when differences become recognized. If the labels and behaviors that the objects exemplify and afford come in families of alternatives, then the actions that introduce phenomena must introduce them as different from other phenomena. They must not only connect an object with a label or a behavior, but also organize a realm of objects into ranges of extension by connecting it with a scheme of labels or behaviors. Accordingly, the procedures of introducing and teaching labels through ostensive definitions, identifying causes and effects in experiments and transferring the knowledge in demonstrations, introducing a behavior and instructing someone to behave appropriately, all must have a contrastive nature. Because knowledge is symbolically constituted and has a systemic nature, knowledge-generating actions introduce systems in the sense discussed

[23] The action of performing a PCR, for instance, usually includes running an agarose gel electrophoresis, which includes preparing the gel, which includes adding agarose powder, which includes taking it from the place where it is kept, which includes grabbing it etc. What is the appropriate level of analysis depends on the context. A protocol shared by practising scientists will probably just mention, “run a gel,” while a students manual will go through the steps of gel preparation. On the level of taking the agarose, idiosyncrasies come into play concerning the particular place in the lab where the agarose is located. This will therefore not appear in any protocol, but might become relevant if mistakes have to be detected. (Did you take the right container? etc.)

above. I will start to gather some features of contrastive procedures by discussing what philosopher's have said about ostensive definitions and experiments.

3.2. The Contrastive Nature of Epistemic Practice

Classification and Ostensive Definitions

Contrastive procedures aim at finding similarities and differences. These define classes. This is to say that these procedures result in recognizing classes of objects, whose members usually become denoted by a label or answered to by a certain type of behavior. There are as many classifications possible as there are similarities and differences to be found. What similarities and differences become noticed depends on the schemes already in use and the technologies available. But not every similarity or difference that becomes recognized will be useful or important from every perspective. A classification is useful if it "resonates" with other classifications.[24] In Goodman's terms one can say that only if it fits, the scheme will become entrenched. Classifications then point out relevant kinds. If a classification is performed and the similarities and differences resonate with others, someone acquired some unit of knowledge, what I call a concept. The similarity and difference relations can be demonstrated to other people in that the labels in a scheme can be exemplified by the objects in their respective ranges of extension, such that a collective of actors possesses the concept. Since the detection of a relevant kind and its demonstration both have to make the same contrast, I will talk about both situations.

The upshot of Chapter 2. was that, if we look at meaning, we cannot look at the relation between single labels and their referents. We have to take the system into account in which they function. This means two things, first, we have to take into account the syntactic and semantic properties of the system and second, we have to take into account the other labels that constitute the scheme together with the label in question, as well as the other referents that are in the realm that is organized by the scheme. I will come back to the difference

[24] The notion is Rheinberger's (1997) and will be further discussed below.

between dense and articulated representation later. Here, I will focus on how the relation between labels and referents depends on the relations among labels and among referents, that is, I will focus on the systemic nature of reference and affordance that corresponds, as will be shown, to the systemic nature of causal concepts. Because labels and behaviors are parts of schemes, the introduction and teaching of labels and behaviors must be contrastive.

Discussions of meaning, like Putnam's, focus on the label as referring to an object. But when they speak of baptismal acts, they implicitly not only acknowledge the role of action, but that the object plays a particular role in such a situation. Goodman has pointed out that reference can also go from objects to labels and I suggested that this way of symbolization must be in place when new terms are introduced, as well as in cases where a given conventional relation between a referent and a label is learned.[25] If we want to know how labels can be about the world we need to make the world part of the language and start our thinking from the direction of the object towards the label. The relation between a label and an object, which is introduced in baptismal acts, restated in an expert's test procedure or taught through ostension, will therefore be expressed as exemplification. The use of exemplars to explain how a label is to be used in general will be called ostensive definition.[26]

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[25] A special case of learning is given, when the use of the label is not taught, but has to be derived from its use. I suggest that the way an observer has to sort her various observations recapitulates the structure of ostensive definitions.

[26] Goodman seems to imply that in ostensive definitions objects function as exemplars (samples as he says, Goodman 1976, 53, fn. 5). Wittgenstein also emphasizes that in ostensive definitions, objects figure as signs, as part of the language. As he puts it: "It is most natural, and causes least confusion, to reckon the samples among the instruments of the language." (Wittgenstein 2001, § 16) His idea seems to be that one states synonymy in an ostensive definition (i.e. one makes a grammatical statement that is about signs) rather than referring to the object. The approach followed here is different, but this might after all be just a terminological matter.

It does not matter whether the label (and the class) is already conventionalized, but taught to a novice, or if a new class of objects is baptized and introduced to a community. In any case, one takes an epistemological stance towards language. Either one wants to create connections a concept, or makes other people acquire a concept, by representing the object and the label as standing in a reference relation that shall become or is a convention.

To say that something is represented requires some representational act. The act in question, the ostensive definition, is in itself a complex procedure; it involves activities on both sides of the relation. On the one side it requires to refer to an object, and on the other to refer to a label. The former act might involve a deictic gesture (holding up an object, pointing to it etc.), but not necessarily has to, since any deictic gesture is conventional anyway, and referring to the object with another conventional label that is "appropriately exogenous," will perform the same function.[27] The referential act concerning the label, might simply be its mentioning[28], however, a phrase like the "the word I told you yesterday" might also work under the appropriate circumstances. Since a deictic gesture or the act of mentioning, as opposed to the use of the word, are not automatically understood, but have to be learned as conventions, they can be misunderstood (on misunderstanding pointing gestures, see Wittgenstein 2001, § 185). It is unproblematic, however, to assume that ostensive definitions only work if these conventions are known. But even if someone understands the gesture of pointing (that she is not supposed to look at the hand, for instance), she does not necessarily understand what the gesture points to.

The argument I want to make in this section is that an ostensive definition requires a contrast. If someone wants to establish or teach (or discover in a foreign language) what gold is or what "gold" refers to, she needs to establish or teach (or discover) what is *not* gold and what alternative labels are used to refer to things that are not gold.

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[27] The expression is Woodward's (2008) and will be explained later.

[28] If a label is mentioned it refers not to the referent of the label, but to the label, e.g. >>"gold"<< refers to "gold" (Goodman 1984, 62).

Since I said that in ostensive definitions objects figure as exemplars, the notation used for the relation that is established or taught will be "is the exemplar for". (The finite article is necessary, because there are other objects that could be used as exemplar as well, namely all other instances of the label.) It has to be remembered though, that the relation is symmetric. What exemplifies a label is also denoted by it and even if an object that is denoted by a label does not automatically exemplify it, it still could be used to exemplify. An ostensive definition establishes the relation in both directions. The object will figure as exemplar (E) and the word as label (L) (especially, because the exemplar need not be an object, but could be an event, and the label need not be a word, but could be a picture or something else).

Now, if an ostensive definition requires to show what the object in question is not, if it requires to introduce a scheme instead of a single label, a procedure is needed, that contrasts at least one reference relation, ε_1, between E_1 and L_1 with another relation, ε_2, between E_2 and L_2. A full ostensive definition, understood as a contrastive procedure would thus consist of four acts. 1) Referring to an object E_1, say with a deictic gesture; 2) referring to a label L_1, say through mentioning it. Together 1) and 2) form one ostensive exemplification, OX_1, which, however, does not count as a full ostensive definition. A second pair of acts is required. 3) Referring to an object, $E_{2;}$ 4) referring to a label, L_2. 3) and 4) form a second ostensive exemplification, OX_2. The four referential acts have to be appropriately coordinated, of course, in order not to confuse OX_1 and OX_2. The question is how E_2 and L_2 need to be chosen such that the contrast between OX_1 and OX_2 introduces the relations ε_1 and ε_2 and thus the system in which the scheme L_1/L_2 organizes the realm to which E_1 and E_2 belong.

It is interesting to look at Wittgenstein's discussion of ostensive definitions in order to understand why they must be contrastive and how the contrast has to be construed. Wittgenstein never fully acknowledges the contrastive nature of ostensive definitions, but he seems to grasp its necessity and hints in that direction. Thereby he also hints at what needs to be contrasted. He writes:

> So one might say: the ostensive definition explains the use — the meaning — of the word when the overall role of the word in language is clear. Thus if I know that someone means to explain a color-word to me, the ostensive definition "That is called 'sepia'" will help me to understand the word. (Wittgenstein 2001, § 30)

The problem here is, of course, that, if the sender points to an object and mentions "red," the recipient of the ostensive definition cannot know if the sender points to the color, the object itself, its shape, its position or whatever properties it has. Since, as Wittgenstein argues, the problem cannot be solved by assuming that the sender *means* the color and not the shape or any other property when pointing to the object, this information has to be given. "Color-words" is a label that denotes labels such as "red," "green," or "sepia." Above I have interpreted a scheme in a way that its members to a meta-label which is a constituent of another scheme. Accordingly, I will say that what the recipient of an ostensive definition needs to know is the meta-label to which the scheme the label introduced, taught, or discovered is part of belongs. This does not imply to know the full scheme however, because one does not know the other labels in the scheme if one knows one label and the meta-label.

Usually, labels and objects (or their relation) will be introduced in a more specific context, or as a particular language game in Wittgenstein's words, and not as parts of language as such. One might introduce, teach, learn or discover how color words are used in house painting, for instance, because one is the product developer of a paint company, an apprentice of a painting firm, a customer of the firm or just an observer. Accordingly, the realm that is organized by the scheme of which the label is part is that of wall paints. To take another example that involves parts instead of properties, and which brings us closer to the subject of this thesis, let us imagine a person who teaches the body parts of a beetle to someone else. The label taught might be "antenna" and it will form a scheme with other terms such as "eye" or "leg." (We are talking folk entomology here.) The meta-label that someone has to know in order to understand what the teacher is pointing to would be "beetle parts" or "beetle organs." These meta-labels could form a scheme with "beetle behaviors," but just as well be part of another scheme with, say "fish parts." The scheme could also belong to the holonym "beetle body."

But Wittgenstein's solution to the problem of specifying the gesture, to assume that the person who is taught the label has to know label denoting the words in the scheme, or, alternatively, the meta-label, is unsatisfactory. We already had to grant that the conventions of the deictic gesture and of mentioning have to be known. But these at least apply to all ostensive definitions. In this case, the convention that has to be known to understand the definition is different for every definition. It would be more satisfying to have a picture, where the meta-label, to which the scheme belongs can be inferred by the recipient, in order to account for the power that ostensive definitions have in acquiring knowledge about conventions and producing them.

An alternative solution might be implied in the suggestion that full ostensive definitions are contrastive (consist of at least two ostensive exemplifications). The receiver needs to know other labels from the scheme and other objects from the different ranges of extension into which the realm is organized by the scheme, such that she can infer the realm and the meta-label to which the scheme belongs from the contrast.

There are several occasions where Wittgenstein suggests that expressions are parts of systems, where "system" can be interpreted as scheme (he uses the term system in several ways, however), and that to know the meaning of a label requires that we know which other labels are in the scheme. In this section, for instance he emphasizes the fact that terms belong to a "system of expressions":

> In a particular system of expressions we can describe an object by means of the words "satisfied" and "unsatisfied." For example, if we lay down that we call a

> hollow cylinder an "unsatisfied cylinder" and the solid cylinder that fills it "its satisfaction." (Wittgenstein 2001, § 439)

In other words, the labels "satisfied" and "unsatisfied" form a scheme that can be used to organize a realm of objects, in this case the class of cylinders. On other occasions he seems to suggest that to understand how a word is used requires knowing in "opposition" to what other words it is used. For instance, if one wants to understand how someone uses the word "sentence," denoting everything that is a sentence, in order to understand if she would count a valid mathematical expression in the extension of "sentence," (that is, to know how she draws the lines around the classes) one needs to know if she uses the word in opposition to "mathematical formula" and maybe some other terms like "logical formula," or instead only in opposition to something like "string of signs without meaning." (See Wittgenstein 1984, § 80)[29] If this implies, however, that one has to learn all the other labels in the scheme of which a label is part, the requirement is too strong. Indeed, to fully grasp the extension of a label, one would need to know all labels in a scheme, but most ostensive definitions are more modest. They introduce labels as parts of a scheme, but not the whole scheme. They are steps towards a complete understanding of the meaning of labels. Sometimes however, ostensive definitions can also be complete. In order to derive the meta-label to which the scheme in question belongs and the realm it organizes, it is sufficient to contrast two objects and two labels in a particular way.

Wittgenstein alludes to, but does not make perfectly clear that the ostensive definition relies on contrasts, when he says that it depends on "what happens before or after."

[29] Another interesting passage that brings together the notions of system and opposition is the following: "'Now' is a word. For what do I use the word? 'Now' — in opposition to what? — In opposition to 'in one hour,' 'half an hour ago' etc. etc. 'Now' does not stand for a system, it belongs to a system. It does not function magically; like no word does." (Ms-153a,67v, http://wab.uib.no/cost-a32/153a/Ms-153a_norm.html, my translation.) Indexicals like "now," one might say, like other labels refer to the object (time slice) as parts of a scheme. Only the scheme will tell the receiver how the object is construed (e.g. how large the time slice is).

(Wittgenstein 2001, § 35, see also 1984, § 27) The point is made more clearly, though also not fully appreciated in its function for the ostensive definition, in Baker and Hacker's comment on this paragraph. They write: "So, the learner may have asked «I know that *that* ☞ ■ is called 'square,' but what is that ☞ ● called?»" (Baker and Hacker 2005, 109) I will understand ostensive definitions as including the contrast, instead of referring to the contrast as the right context for the ostensive definition to work (single ostensive acts I have called ostensive exemplifications). I shall now clarify how the right contrast is picked. With respect to the different possible contrasts Baker and Hacker write a few lines later:

> The contrast between pointing to the table and pointing to the chair is between pointing at two different objects of the same general type (pieces of furniture, material objects). But to point to a color rather than to a shape involves a contrast in category. In the first case we change the direction of our gesture. In the second we need not, just because to point to the color (shape) is to point to the object which has it, and the same material object may exemplify both color and shape in question. (Baker and Hacker 2005, 109)

What they call "category," on my account is captured by the meta-label to which a scheme belongs or the realm that is sorted, which makes no difference if the meta-label is construed such that it has the realm that is organized by the scheme that belongs to as its extension (e.g. if the meta-label for color terms is not "color", but "colored"). Pointing at a table and a chair could, of course, involve a change with respect to the meta-label as well, if the meta-label in question would be "chair" not "furniture" and in this case a contrast that would not involve a change with respect to the meta-label could be that between a kitchen chair and office chair. To point at color and shape, on the other hand, is not necessarily a change with respect to the meta-label, if the latter is something like "having determinable properties." The contrast of different objects of the same "general type" (denoted by labels that belong to the same scheme and thus to the same meta-label) indeed requires a change in direction of the gesture, which is just to say that the contrast has to involve two objects (which could also be two parts of an object). But “a contrast in category” is not a contrast at all. Only objects can be contrasted and then the labels that denote them are automatically belong to the same scheme.

If we introduce, teach or discover the relation between an object an a label, we need to do this with respect to a meta-label and a realm, which is just to say we introduce it the label as part of a scheme that organizes the realm in question. Thus we have to make the contrast such that we do not make a change with respect to the intended meta-label, or, put differently, that we do not exemplify a label outside the scheme we wanted to introduce. We cannot introduce a label by contrasting it with a label of another scheme. However, if someone points at two objects separately, one of them being yellow, the other being blue, and mentions the labels “yellow” and “blue” accordingly, the receiver still cannot know whether she points to the colors or shapes or any other property of the objects, unless the objects are the same in all properties except color. And this is indeed the requirement, although it is an ideal that cannot be reached, and if it is only because two objects are either at different places or pointed to at different times. Nevertheless, if an ostensive definition should be successful, this ideal has to be approached. In cases were a restricted realm is given, such as when the labels denoting different wall paints are introduced with a collection of single bricks painted with the different colors, the realm can be appropriately uniform. The bricks have the same shape, size and weight and are alike with respect to many other properties. If the teacher points to two of them and mentions the color-labels it is very likely that it will be realized that she points at their color. She does not have to announce that she will now teach color words as Wittgenstein suggested. Instead, even if the receiver does not know the word “colored,” she can derive the concept. Misunderstanding can never be ruled out. This is not a problem in so far as labels (and classifications) are used in practical contexts. If those fail to function smoothly, misunderstandings will become obvious and the interpretation of the labels will be adjusted.

Usually, ostension works, often even without making the contrast explicit. But there are always reasons why an ostensive definition works. With respect to the contrast, the instructor may rely on the receiver grasping “what happens before or after.” Concerning the uniformity of the contrasted exemplars, it can be said that strictly speaking, the contrasted objects can never be more or less uniform, because no matter how many similarities they exhibit, they will still be different in innumerable respects. But first, the salience of properties can be exploited. In the above example, the bricks are uniform with respect to the salient properties. They still can differ with respect to properties such as having been painted on a Wednesday and a Thursday, respectively. Usually, the instructor can rely on the fact that the receiver has no access to this information. This will never be save, however. If the receiver was present when the two exemplar bricks were painted, recognizes them as such she might interpret the pointing gesture as indicating the difference concerning the day of the week where the bricks were painted. Secondly, if the exemplars are not uniform, the instructor can retreat to Wittgenstein’s solution and make the receiver understand that a scheme that belongs to the meta-label “colored” is explained, in the hope that the receiver already knows what it means for objects to be colored.

In many cases the properties concerning which objects are similar or different will not be salient. That is that they are not properties that humans happen to be receptive for. Humans are visual animals. They will easily grasp differences in color or shape. When it comes to weight, the instructor would have to let the receiver hold the objects, or, alternatively, let him look at the scale of an instrument, which of course requires that the receiver is familiar with reading such instruments. Here properties are made perceptible. The advantage in such situations is that the receiver is more likely to grasp the relevant difference because of the procedural character of making it visible. If the instructor operates the instrument the receiver is likely to guess that she is not supposed to pay attention to the color of an object. Still she requires the contrast to understand what the instrument shows. Such complex ostensions are often performed when scientific objects are introduced. Therefore, I will pay closer attention to the case.

Ostensive Definitions and New Scientific Objects

I have drawn a parallel between situations where a competent speaker teaches someone to use a label and thus to recognize the objects in its extension and situations where a scientist recognizes and introduces a novel phenomenon and attaches a name to it. But I maintained that what actually happens in the first situation is that a scheme is taught and thus a way to organize a realm of objects into ranges of extension. Accordingly, in the second type of situation differences are found among objects in a realm and also here a scheme is introduced, instead of a single label. The latter situation is thus not well characterized as a baptizing event.

Gooding (1990, Ch. 3), in discussing the difference between the two types of situations additionally emphasizes the point that the recognition of new objects is often a long process of stabilizing certain experiences, rather than a single event. And this process often involves various scientists. This offers another solution to the problem of ostensively picking out one property. If someone points to objects in a contrastive manner, another person does not necessarily have the same experience as the former, that is, she will probably not pay attention to the same properties. If we speak of two or more observers instead of one, communicating their findings, then we can say that the observers negotiate what there is to see, which differences are interesting.

More importantly, however, Gooding emphasizes the role of agency in discovering novel phenomena. "The 'act' of observing something for the first time presupposes the activity that rendered it visible." (Gooding 1990, 74) It is the active interaction with objects that draws attention of the observer to some properties instead of others. And it is also this interaction with objects that draws the attention of those people with whom an observer shares her experiences. "Where the manipulation of an image or object conveys just that aspect of a phenomenon intended by an observer, this makes it *possible* for observers to share a way of seeing the phenomenon." (Gooding 1990, 71) If someone applies a substance to several pieces of metal and observes that they react differently, it is likely that another observer's attention is also drawn to this difference, instead of, say, to the different shapes of the metal pieces. This is not only because the chemical reaction is an event that as such draws more attention than a steady state, but also because the agent applied the substance so what happens with respect to the substance must be important, under the assumption that the observer aims at establishing a shared experience. The application of the substance in this situation is a communicative act and it is received as such by the other observer.

One might ask if contrast is still that important if a procedure involves a manipulation of objects. If someone applies a substance to a single piece of metal and observes a reaction, another observer's attention will be drawn to the property of the metal to react with that substance (instead of its color or shape). However, the second observer will not know if she is supposed to learn something about the metal or the other substance (the active observer instead might have started her investigation from an interest in the metal) and both observers will not know if any metal reacts with the substance in this way. The act does not convey any such information. This is not to say that it conveys no information at all. Indeed something can be learned about the reaction between metal of this kind and this substance (no matter whether the same reaction will take place with every metal or not). But this implies that a contrast actually has been made. The metal without contact to the substance and the metal in contact with the substance have been contrasted. A classification can be established according to which contact situations differ from no contact situations in that the former but not the latter lead to the observed reactions.

Scientific practice and the observational or experimental systems in which it is organized can be described as a "groping and grasping for differences."[30] (Rheinberger 1997, 75) And often the differences are hoped to match those already established. To put it differently, it is hoped to find a scheme that organizes a realm of interest in the same way that it is organized by a scheme that already organizes the realm according to certain interests. For instance, Rheinberger discusses a case where the realm of tissues is already organized by the scheme healthy/malignant and differences are sought that map on this distinction. Those new properties might have some explanatory force with respect to the known properties and potentially serve as a handle to alter them, or at least serve for predictions. But as Rheinberger also points out, the differences that are found in such an investigation might split the realm differently or apply to a different realm. This can "reorient" the research program in question (Rheinberger 1997, 97)

Even if the same kind of contrast that is involved in instructive ostensive definitions is involved in knowledge-generating actions, we can only call them ostensive definitions if labels are applied to the recognized differences and if they are communicated. But unless they are communicated they will not be relevant at all, because science is after all a communal activity. The question rather is, if the procedures provoke differences or require instruments to render them visible, why not call them operational definitions? Apart from the mentioned history of the term that loads it with the connotation of normative definition, the reason is that this would introduce an undue difference between ostension and interventions. The point is that even simple ostensions are operations so they should not be treated separately from the complex operations discussed in this section. Whether the difference is in color that is visible, in weight that requires a balance or in reactivity, which requires the application of a test substance, the teacher or introducer of a label (i.e. a

[30] To find similarities can, of course, also be part of scientific concept formation. I will argue below that finding similarities depends on finding differences, because only differences make properties salient and delimit their scope.

scheme) will have to make a contrast and channel his and the other's attention to the difference in question.

Contrastive ostensive acts might involve all kinds of complex procedures, such as dissecting a whole animal to derive parts (ostension as isolation) or juxtaposing carefully prepared and tagged specimens from a botanical collection (ostension as filing), or relying on the differential ways the objects are affected by a cause or cause effects (ostension as experimental manipulation) etc. One could classify ostensive procedures by all kinds of criteria, but the important point is that they are always contrastive.

If reactions are tested or instruments applied to discover differences, there are causal relations involved. These causal relations might be of interest in themselves, but they might also be only vehicles to establish some difference. If a biologist weights the seeds of a plant, the causal relation between the seed and the balance will not be of any interest. The type of causal interaction is studied in physics, is well known, and taken for granted. They belong to a background observation theory. If the seeds have a differential effect in attracting birds that feed of them, this causal relation might instead be very important to the biologist.[31] I will call contrastive procedures that aim at discovering causal relations that are explanatorily relevant in a given context experiments in the narrow sense (although the term is usually used in a much broader sense, including all kinds of procedures).

Before I turn to experiments, I shall mention again that I consider those procedures that introduce classes of objects that afford certain behaviors, or instruct someone how to answer appropriately to certain objects as analogous to the procedures that introduce classes of objects that instantiate certain labels, or teach someone to denote objects appropriately with certain labels. That is they have to be in the same way contrastive.

[31] The boundary between these types of situations is blurry, though. A dye might be used to make different structures visible, but the fact that it stains a structure will also contain information about the chemical features of the structure.

Experiments and Causality

Some contrastive procedures aim at differential causes or effects, that is, at highlighting causal relationships. Causation is a notoriously difficult concept. This also has to do with the fact that the noun “cause” and the verb “to cause” are ambiguous, with respect to what kinds of entity they apply to. Accordingly, much has been written about causality by philosophers as well as scientists. These groups typically pose different questions. Philosophers typically ask "What does a claim such as “A causes B” mean?," while scientists ask, "When has an experiment shown that A causes B?" Although distinct, answers to both questions have benefited from answers to the respective other. My focus with respect to causality is: How does the detection of causal relations contribute to classification and decomposition? That is: How does it contribute to the identification and individuation of objects and properties? The procedures that allow the discovery (and demonstration) of causal relations are contrastive procedures, whether the focus on the causal relation itself or on the classification of causes and effects. I will discuss causality only to the extent that allows me to compare its experimental investigation to the procedures of classification by finding similarities and differences. The motivation for this interest in causation is, of course, that biological characters appear in many contexts where they are seen either as being caused by certain factors (evolutionarily or developmentally) or where they are causes themselves (as e.g. in ecological interactions). As we will see, sometimes they are seen as effects in the sense that instrument readings are seen as effects of a measured object.

One way to clarify the meaning of causal claims is through counterfactual analysis. According to this approach, A is a cause of B, if it is true that if A had not occurred, B had not occurred. This analysis, however, is not restricted to presences and absences. It became common to articulate causal relations as holding between variables. If V_1 and V_2 are variables that pertain to the same or different objects, we can say that V_1 having the value x causes V_2 having the value y, if it is true that if V_1 would have a different value $x' \neq x$, V_2 would have a different value $y' \neq y$. But this formulation still leaves open the possibility that the changes in V_1 and V_2 are due to a change in a third variable, that is, that V_1 and V_2 are merely correlated. One can rule out this possibility, by stating that V_1 having the value x causes V_2 having the value y, if it is true that if V_1 were to be changed by an intervention from the value x to x', the value of V_2 would change from y to y'. Intervention here does not imply a human intervention, but just refers to another causal relation that holds between V_1 and another variable V_3, which should be "appropriately exogenous" to the causal relation in question, which means that it can change the value of V_1, while not having an effect on V_2 except via V_1 (Woodward 2008, 1, 28-30). If such an intervention on V_1 would change the value of V_2, then V_2 is causally dependent on V_1.

Note the implicit contrastive character of causal statements. We can only understand A as causing B or V_1 having the value x as causing V_2 having value y, if we contrast this fact with the counterfactual situation of the absence of A or V_1 having another value x' (on the contrastive nature of causation see also Schaffer 2005). The situation is counterfactual, because A cannot at the same time be present and absent and V_1 cannot at the same time have value x and x'. In an actual experiment, we can only observe two distinct situations, and the experimenter has to create a situation that approximates the counterfactual situation. If in one situation V_1=x and V_2=y, while in another situation V_1=x' and V_2 =y', it is possible that it is one of the other circumstances that are different between the two situations apart from the changed value of V_1 that causes V_2 having the value y'. This uncertainty can in fact never be avoided; this is why experiments, or quasi-experimental observations[32] have to be so carefully controlled. For a philosopher like Woodward, being interested in the analysis of causal claims, "the characterization of an intervention tells us what should be envisioned as changed and what should be held fixed when we evaluate a

[32] Quasi-experiments mimic an experiment by choosing two situations suitable for contrast, without creating them through the experimenter's intervention (the classical text on qausi-experiments and natural experiments is Campbell and Stanley 1963).

counterfactual." (Woodward 2008, 29) But the notion of intervention also provides a guide for approaching the ideal contrast expressed in the counterfactual in an actual experiment. An intervention is understood to be "surgical," which means "that no other causal relationships in the system are changed" (Woodward 2008, 16). Usually the way this ideal is approached in laboratory sciences is through standardization of research environments and materials that allows the researcher to interpret the variable V_1 in one object as identical to the variable V_1' in another object or at another point in time and thus contrast different values in that variable.

Although there are many intricate philosophical problems related to the issue, this brief glimpse on the analysis of causality and the experiments through which causal relations are detected should be sufficient to see some similarities to the case of classification through detecting differences and the procedure of ostensive definition through which the classes are fixed. We saw that pointing to a property, the value (e.g. yellow) of a variable (color) as we can also say, requires contrast, and the same is true for the detection of a causal relation. This is not surprising since every property can only be perceived if it somehow (even if very indirectly) has a causal effect on the observer. On the other hand, being-caused-by-A, or affecting-B are properties of B and A, respectively.

Often when causal relations are investigated, the purpose is to learn something about the function of the causing object or variable in a larger system, or to explain the object or variable affected, even if it has to be said that stating a single causal relation rarely counts as a full explanation. It can, however, justify predictions. But it is important to see that there are also many occasions where a causal relation is established, but the investigator has no interest in explaining the effect or investigating the function of the cause with respect to the effect. The causal relation can be used to differentiate a class of objects, in those that have a certain effect and those that do not, or that have another effect. It can also be used to sort another class of objects, in those that are affected by a certain causal agent and those that are not, or that are affected by other causes. Then these classes might be investigated in other respects, for instance, by looking for other properties the objects in the thereby established classes share.

Having thus pointed out the close link between investigating causes and investigating properties as motivating classifications, we can further see a parallel between the kinds of contrastive procedures of ostensive definition and experiment. What is an intervention in causal experiments is the referring act (e.g. deictic gesture) in ostensive definitions. The act of referring to the exemplar in a single ostensive exemplification must be "appropriately exogenous" to the referential relation between the label and the exemplar, in the sense that it should not refer to the label at the same time (for instance, if the label is written, the exemplar and the label should not be held up at the same time). The whole ostensive definition, as we saw, has to be understood as consisting of two referential acts (belonging to the two ostensive exemplifications). The shift between referring to the one exemplar and referring to the other must be "surgical," in that the second exemplar should ideally differ in nothing but the property that motivates the classification that is captured by the scheme of which the labels in question are part.

Ostensive definitions and experiments are thus both contrastive, require an "appropriately exogenous" act of establishing the contrast and aim at the ideal of a surgical contrast. Again, this must be the case, because after all, all ostensive definitions involve a causal relation between the contrasted objects and the sensual system of the observer or an instrument, test substance, or whatever renders a property perceptible. And on the other hand, every causal relation is a form of classification. It ascribes the property of causing this or that effect to an object *qua* cause or the property of being affected by this or that cause to an object *qua* effect. Nevertheless, it is useful to keep the situations apart, because it is important for the analysis of scientific practice to be sensible to the different foci of contrastive procedures that indicate different broader goals of the application of contrastive procedures. The different foci will be compared in the next section.

The Aims and Foci of Contrastive Procedures

Every time a difference is found between two objects that belong to the same realm, a scheme can be applied that organizes the realm into ranges of extension. Thus the detection of a difference automatically gives rise to a classification. The class that constitutes the realm now contains sub-classes that constitute the ranges of extension under this scheme. But the goal of a contrastive procedure need not always be to classify the objects in a realm. Sometimes the contrast will just serve to make the properties of an object visible, the focus is not on the realm that becomes organized, but on the objects to which parts and properties are attributed that become salient in the contrast with other objects. One of the objects in a contrast might be a mere vehicle to contrastively investigate the other. Sometimes both objects will matter, but the resulting classification is still only a by-product. Comparative anatomy, for instance, need not always serve the aims of taxonomists, let alone indicate phylogenetic relations. It can simple be a strategy to make parts and properties of organisms salient, e.g. in order to investigate their function. I will call this non-classifying mode of contrasting "analysis." It is about the relation between an object and its parts and properties.

A note on properties is in place: In that a property becomes visible in a contrast, it becomes perceived as a determinate state of a determinable, which is to say that the label becomes introduced as part of a scheme that belongs to a certain meta-label. In fact the word "property" is used for both determinables and determinates.[33] The knowledge gained

[33] The distinction is due to Johnson (1921-1924). The stock example is color, which is a determinable, while red, blue, green etc. are determinates. As opposed to the distinction between hyponyms and hypernyms or labels and meta-labels, which are a semantic relations, this is a logical distinction. "Species are often thought to be definable in terms of a genus and a differentia. But determinates are not definable in terms of a determinable and a differentia; indeed, they are not conjunctive properties of any obvious sort. Determinates under the same determinable are incompatible; nothing can instantiate both of them at the same time, and anything that exemplifies a determinate must exemplify its determinables as well." (Swoyer and Orilia 2011, 42)

is not only that a certain object has a certain property (determinate), but also that there is that kind of property (determinable).

If the aim of a contrastive procedure is to make salient properties of the type *being the cause of a certain effect*, or alternatively, *being the effect of a certain cause*, that is to establish causal relations, I will call it "analyzing experiment" (again, this is a narrow use of the term "experiment", not everything that is usually called experiment falls in this category). In the former case, where the causing object is in focus, I will call the procedure "analyzing intervention," because the intervention part of the procedure lies on the side of the cause. When the effect is in focus, I will speak of "analyzing observation," just because the effect is what must be observed in an experiment (but, of course, also in an analyzing intervention the effect needs to be observed, and in an analyzing observation causes have to be intervened on). These activities aim at highlighting causal relations in which an object stands.

I said that the recognition of a difference between objects results in establishing sub-classes. If to classify the objects in a realm is also the aim of a contrastive procedure I will speak call the activity "classification." Since the procedure sorts the objects in a realm in sub-classes, this form of classification can be called "sorting." There is, however, also the case where objects previously recognized as belonging to different classes will be found to share a property, which establishes a super-class that all objects belong to. This will be referred to as "grouping." Also grouping depends on contrasts. Not only because two objects have to be compared to find a similarity, but most importantly, because on this account, properties only become perceptible in contrasts, where different determinates make the construal of a determinable possible. Thus if two objects are found to have the same determinate property, this requires that one of them has been contrasted with an object that has a different determinate property with respect to the same determinable. This also implies that the new class, which is the realm that is organized into the two classes we started with, is itself a range of extension of the label that captures the similarity and that belongs to a scheme with a label that denotes the object that exhibits the alternative determinate property, such that the ranges of extension of the new scheme organize a larger realm. Also grouping requires introducing a whole system.

Sorting and grouping are about the difference and similarity relations between objects. Another type of activity aims at establishing part-whole relations, I will speak of "partitioning." If a whole is divided into parts, the activity will be called "decomposing." It is applied to a realm taken to consist of a class of wholes. One picks exemplary wholes and applies some method of observation to find that their parts have different properties. Decomposing is a form of sorting in that two classes of parts are established. The difference between detecting parts through analysis and decomposition is that in analysis, two objects are compared to see that they differ with respect to the presence/absence of a part or some property of the part. In decomposition, parts of one object are contrasted with other parts of the same object.

What about detecting that two objects can in some respect be seen as parts of the same object (what can be called "composition" in opposition to "decomposition")? In principal, it has the same structure as grouping. With respect to classes there is no requirement that they should have any significance. Whereever common or distinct properties are found among objects classification happens. Subsequently, the classes, or as should be said the labels (or, even more precisely, the schemes) can become entrenched, or not, they can fit or be made fitting in a network of systems, or not. It seems that a whole is a certain kind of class, the class of objects as parts that becomes interpreted in a certain way in certain circumstances.

If objects in a given realm are grouped or sorted on the basis of having the same or different effects, I shall speak of "grouping intervention" or "sorting intervention," respectively, because the objects as causes are in focus. If objects in a given realm are grouped or sorted on the basis of having the same or different cause, I shall speak of "grouping observation" or a "sorting intervention," respectively, because effects are in focus. Parts having different effects or causes can also motivate a decomposition ("decomposing intervention", "decomposing observation"). Finally, having the same causes or effects can in some circumstances motivate composition ("composing intervention", "composing observation"). The different types of contrastive procedures, distinguished by their aim and the according focus are summarized in Table 3.1.

		Object – Property Relation	**Object – Object Relation**			
			Classification		Partitioning	
Attribute		Analysis	Sorting	Grouping	Decomposing	Composing
Causal relation	Causes	Analysing Intervention	Sorting Intervention	Grouping Intervention	Decomposing Intervention	Composing Intervention
	Effects	Analysing Observation	Sorting Observation	Grouping Observation	Decomposing Observation	Composing Observation

Table 3.1. A typology of contrastive procedures

The Uses of Contrastive Procedures

The last section distinguished possible aims and foci of contrastive procedures. I already alluded to the fact that contrastive procedures, additionally, can have different uses. In particular they can be used to introduce, teach or create differences. Here these uses shall be distinguished again more clearly.

A) *Concept formation* is the discovery of causal relations and relations of similarity and difference among objects that lead to the introduction of properties, super-classes, sub-classes, classes of wholes or classes of parts. It involves also the naming of the new classes with labels and probably the development of a set of behaviors that answer differentially to the objects in the new classes.

B) *Concept demonstration* is the act of making other people recognize the classes of objects, the similarity, difference and causal relations that specify them and teach them the labels and afforded behaviors. Labels and the classes they take as their extension, when they are already entrenched, are taught, just like they are introduced in the first place, through ostensive definitions. If causal relations are involved it requires a demonstrational (or reported) experiment. Teaching behavior, what can be called "instruction," works like ostensive definition, except that a behavior is performed instead of a label mentioned.[34]

C) *Concept introduction by fiat* is the act of applying labels or behaviors differentially to some members of a given class or regions of a given object, thereby creating subclasses or parts (or applying the labels or behaviors to objects that are not previously considered members of a class, thereby creating a super-class). The classes and parts are introduced by *fiat*, because their differentiation is not motivated by their possession of the same or different properties. We can distinguish two cases:

First, if the objects in a realm vary gradually with respect to a variable, such that it takes values covering a certain range, we can stipulate that a label L_1 denotes objects that take values in a certain sub-range, while L_2 denotes objects that take values of another defined range and so on.

Second, if no difference at all is recognized between the objects in a realm consisting of the objects A, B, C, D, E and F (although there will certainly be differences), it can still be stipulated that objects A, B and C are denoted by L_1, while objects D, E, and F are denoted by L_2. One can just as well lump together objects that were not considered members of the same class before by stipulation.

The same goes for the stipulation of behaviors. One can start to treat the objects that take values in a certain range for a given variable differently from those that take values that fall in another range. And one can treat A, B and C differently, from D, E, and F. One can also treat a bunch of objects alike that have not been considered as belonging to a class before. It might be said that here we have properly artificial classes (even more so than in

[34] A special case is the situation of learning the way labels and behaviors are applied, that is, of learning which classes are recognized, by observing the use of labels and other behaviors, of people who do not perform definitions or instructions, just by making the right contrasts through arranging the observed events such that they can be contrasted (this will most likely work only, if other labels and behaviors of the relevant community are already known; see the discussion of radical translation, Quine 1960).

Dupré's example of creatures that are gray and over one foot long). But it has to be emphasized that an accurate physical description of these classes can be given. And one effect of this stipulation of classes might be that some unrecognized similarities become salient. Finally, the fact of treating things differently or alike will almost certainly make them acquire similarities or differences, making the classes less artificial. These stipulated systems can become as entrenched as any other system, if they have a place in the network of systems and if they are used and accepted by a community.

Since the activities A) and B) are structurally similar it is possible to explain one by explaining the other, as I did when discussing ostensive definitions. Ostensively defining a scheme to document a new difference found among objects in a realm, and the teaching of the application of a scheme well entrenched in a community to a novice, both require the same contrast, just as making an experiment to produce new knowledge, and demonstrating the experiment to make other people acquire the same knowledge have the same contrastive structure.

Making people acquire the causal knowledge might work through the description of an experiment in an article. It has been pointed out by historians and sociologists of science that articles are narratives that present experimental procedures much more linearly than they were performed, as a direct comparison with lab notebooks shows (and even those might be the result of some filtering) (see e.g. Gooding 1990, 5). But the ordering and cleaning that takes place in the construction of the article is similar to the ordering and cleaning the scientist performs cognitively to filter, evaluate and connect the significant contrasts from all the manipulations and observations performed.

C) is different from A) and B) because in the case of concept introduction by fiat, objects do not immediately function as exemplars. The introduction of stipulated sub-classes, for instance, requires that for every object it is stated by which label it is denoted. But once the system is conventionalized the objects can very well function as exemplars.

In the following, I will discuss in which sense contrastive procedures enable, bring about and are themselves representations.

3.3. Representational Practice and the Distinction Between Thick and Thin Concepts

Reference in Systems

Once the relations between objects, their effects, the labels that denote them and the behaviors that answer to them have been established, it is possible to use the object to refer to the other items and vice versa. That the label refers to the object is obvious and that the object in turn can refer to the label if it exemplifies it has been discussed before.

Effects can, if the causal connection is known, refer to their cause. This is usually called – with respect to Peirce' typologies of signs (e.g. Peirce 1998) – "indexical signification." What is less often mentioned is that the cause can also refer to the effect. If I see smoke I know there is a fire, but if I see a fire but the smoke is concealed, because it goes immediately in a chimney, I still know that it is there. Since "index" is mainly used for effects, referring to their causes – although Peirce uses it more broadly –, one might call a cause referring to its effect an inverse index. Effects that refer to their causes play a particularly important role in cases of experiments or sorting or grouping experiments when the cause is only perceptible via the effect. But it has to be emphasized that not just any object or event, or whatever can be an effect, refers to all its causes. It refers to a cause, if the causal relation has been previously highlighted and the effect is used or interpreted as referring to the cause.[35]

If the relation between an object and a behavior is known, the object represents the behavior. This is the whole point about affordances. A chair cannot only be sat on, but, if one is part of a chair using community and has learned the behavior, a chair refers to the behavior of sitting. This is comparable to the situation where we know of an object what

[35] Above (Chapter 2), I discussed the parallels between iconic and indexical representation in this respect. Objects do not refer to everything that is similar to them, but only when they are used to refer, and thereby, one similarity relation among many becomes relevant.

label denotes it, that is, what it is. But there might be more specific cases where an object exemplifies a behavior (just like an object exemplifies a label only when used as exemplar). For instance, in a situation of instruction, say, how to use an instrument. At least some behaviors (as opposed to labels) cannot be present when the object they answer to is not present. Therefore, one might ask if the notion that a behavior refers to the object it answers to makes sense. The situation can be interpreted in parallel to indexes, which also can not refer to their causes if the cause is absent (a cause might refer to its effect in the absence of the effect if the appropriate background conditions are not met to realize the effect). If a behavior is observed, but the object is concealed from the observer's view, the observer will still know about the object if she knows the affordance relation. But also some quasi-pantomimic gestures might represent an object.

Reference does not require contrast, but the introduction of the systems that enable it does. Once the relevant relations have been made available through contrastive procedures, reference can travel along these routes.

Thin vs. Thick Concepts

So far I used "object" in a very general sense (just as "entity," "thing" or "phenomenon"). I will now use "phenomena" as the most general term for what becomes represented in a contrastive procedure. Objects will be characterized as a certain type of phenomenon.

Above, I introduced Goodman's distinction between articulated symbol systems that are syntactically and semantically differentiated and dense symbol systems that are semantically and syntactically dense. I pointed out that a symbol in a dense system can be re-interpreted under an articulated scheme. Here, I want to distinguish the ways symbols of dense and articulated systems are used. This is a distinction not in terms of differentiating action, that is, contrastive procedures, that bring about representations of objects, but in terms of differential behaviors, that is, in terms of what the represented objects afford in a particular research context.

Contrastive procedures bring about representations. First of all, the contrastive constellation of phenomena in itself is a representation (I will argue for this claim below). It is a dense representation, because the phenomena as symbols can be interpreted such that between every two of their fragments there lies a third. Verbal schemes that are applied are always syntactically differentiated, but can be semantically differentiated or dense. The procedure might also give rise to various kinds of secondary representations, such as reactions, instrument readings, or photographic images. While some measurements are articulated, photographs are dense. They might be translated, however, into articulated systems. Such secondary representations can be seen as data for phenomena.[36] But the phenomena in question can be data for yet other phenomena. For example, some difference between phenomena might become visible through the application of a staining dye and a microscope and recorded on a micrograph. The micrograph is data for the objects. But the objects themselves might be effects of yet other objects, and thereby can function as data for the latter.

Concerning the activities, the primary and secondary representations afford, they are, of course, specific to the particular situations. But it seems likely that phenomena that are represented in a dense symbol system afford different actions from phenomena represented in an articulated system. When it comes to epistemic practices, there are roughly two responses: performing internal or external contrasts. In an external contrast the phenomenon that became represented in a previous contrastive procedure is treated as a unit. I will speak of a thin phenomenon or a thin concept in this case. The realm of which it is part can be compared to other realms. If an internal contrast is performed, the phenomenon appears as complex in itself. It relates to its parts, properties and their organization. The phenomenon figures as a realm to be organized itself. Here, I will speak of thick phenomena or thick concepts. Representations in an articulated system do not

[36] See e. g. Woodward (2000) for the discussion of the distinction between data and phenomena.

afford internal contrasts, but representations in a dense system afford both, internal and external contrasts.

The distinction is best illustrated with an example. A micrograph of a cell is in principal a representation in a dense system. But if, for instance, an organelle is identified as a darker region in contrast to its lighter surrounding and an articulated scheme is introduced that organizes the realm of the cell, by naming the organelle and putting it in the context of other organelles, and the partitioning is further used to classify cells according to the possession of this organelle as a variable, then a thin concept has been formed. The organelle can be denoted by an arbitrary label in a diagram or by a description. Cells can be compared with respect to the possession of such an organelle. If instead, the micrograph is interpreted as containing information about the particular shape, components and organization of the organelle or its structural relation to other visible particles, an internal contrast can be performed that yields a thick concept of the organelle. The micrograph will not be translated in a notational language, but rather manipulated to gain more information, or replaced by more sensitive representations.

A contrastive procedure can produce a representation of a momentous state of affairs. If it is taken as a thin phenomenon, we can call it an “object.” If what becomes represented as existing atemporally is a thick phenomenon, it can be called a “structure.” But a contrastive procedure can also introduce difference between instances of changes over time. If the change is taken to pertain to thin phenomena it can be called an “event.” Causation in the sense of counterfactual dependence is a case in point. If one object as opposed to another object is represented as causing one effect rather than another effect, a difference between two events is made. If the phenomena of change involve structures, that is, thick concepts, we can speak of “processes.”

	state	**change**
thin	object	event
thick	structure	process

Table 3.2. Types of phenomena

The distinction between thick and thin phenomena (temporal or atemporal) is a distinction not between different kinds of things in the world, nor between representations. It is a distinction between what is afforded by phenomena as they appear in representations brought about in contrastive procedures, but that is constrained by the syntactic and semantic properties of the representation. The thickness or thinness will be revealed in the follow-up actions that answer to the representations, in what people do with the objects as represented to them in dense or articulated symbol systems. The distinction will thus be an important tool in the analysis of the disciplinary perspectives in which character concepts figure.

Representations, Models and Discourses

Contrastive procedures bring about material constellations that, in virtue of their particular arrangement, make particular differences, similarities and causal relations stand out from the manifold relations that can be detected. Thereby, they represent phenomena as standing in these relations. This can imply to put an already individuated phenomenon into a new context (classification), but often implies to discriminate the phenomenon itself from a four dimensional material continuity (partitioning). I use the verb "to represent" in this context for the following reason. If a phenomenon appears only within the kind of material constellation that is brought about in contrastive procedures, but the constellation itself is always more than the phenomenon itself, then the constellation as a whole, even if it includes the phenomenon in question, is not identical with and thus represents the phenomenon. Representation is the relation between a representing entity, in this case the material constellation that is brought about in the contrastive procedure, and the represented phenomenon, the object, event, structure or process in question. But this use of representation does not license to speak of the phenomenon as unrepresented in the sense of something unperceived or unobserved. In other words, it does not presuppose that there is something – in a way pre-individuated – that is represented. The phenomenon is part of the representation but has no meaning outside the constellation in which it is represented. Rheinberger writes:

> Scientific activity notoriously consists in undercutting the opposition between representation and referent, between "model" and "nature." Representation is not the condition of the possibility of getting knowledge about things. It is the condition of the possibility for things to become epistemic things. The scientific object is re-*presented* in being produced; and it is *re*-presented in the sense of a repetition, of an iterative act. (Rheinberger 1997, 112)

By "being produced," we can understand the introduction of differences that characterizes a contrastive procedure "Repetition" here seems to mean what Rheinberger also calls "reproduction." It refers to the fact that "the generation of new phenomena is always and necessarily coupled to the co-production of already existing ones." (Rheinberger 1997, 75) Restated in the terminology used here, the introduction of a system, that is, a realm organized by a scheme, always happens in the context of other systems. The phenomena that are used in the contrastive procedure to refer to, intervene on, or observe the new phenomena have themselves been individuated or manufactured at one point and are specified in systems, that is they are themselves parts of one or more realms, organized by schemes. In particular the realm that becomes organized or reorganized through a contrastive procedure is a phenomenon that is taken for granted when a contrastive procedure is applied, but has been represented itself in a material constellation before. Sometimes, the physical isolation of this phenomenon for further investigation might require repeated application of a differentiating technique. In other cases, it might be available, but the fact that it is salient as such means only that the category became entrenched. The contrastive constellation in which it is individuated as a phenomenon is still implicit if it affords contrastive procedures itself. When it becomes organized as a realm this might disrupt its status as a phenomenon and can lead to a reconsideration of the constellation in which it was isolated in the first place.

If the term “representation” implies that there is something outside the representation, that is represented, I suggest to understand the claim that the constellation of material and instruments in a contrastive procedure represents a phenomenon it includes, as saying that it represents the phenomenon as it appears in non contrastive denotation and exemplification (i.e. reference). I said above that once the phenomenon is individuated as part of a system, the label can denote the phenomenon and the phenomenon can exemplify the label, without there being a contrastive constellation. This use of symbols usually works in an unquestioned manner, fitting in with the use of other symbols and other behaviors. If the flow of symbolic practice breaks down, the system has to be reaffirmed or adjusted in a contrastive procedure. The constellation of the contrastive procedure represents the phenomenon as what is denoted or used to exemplify in other situations. When the label “gold” is used to refer distributively to all pieces of gold, or singularly to a particular piece of gold, or when a piece of gold exemplifies “gold,” then this usually goes without contrast. But when doubt is raised and an expert is consulted, gold becomes represented contrastively; it becomes specified by showing what it is not. If some concept is newly formed, the contrastive constellation represents a phenomenon as it figures in the future uses of the phenomenon in explication or in its role as referent of a denoting label. It is in this sense that we can understand Rheinberger’s appropriation of Jacob’s claim that experiments are “machines for making the future.” (Rheinberger 1997, 28) He points to the openness of experiments to unpredicted phenomena that once they become entrenched will belong to the interior of the world a community of language and practice lives in. But the interior of the world are phenomena that can be referred to, that exemplify and that afford behaviors. Thus it is an interior of symbols. “Representation,” then, is a relation that holds within symbolic practice (between material constellations that highlight a phenomenon and the phenomenon in non-contrastive reference relations) and not between symbols and the unrepresented world. This is what Rheinberger means when he says that representation is symmetrical, that "both terms of the relation are representations or models of each other." (Rheinberger 1997, 112) The world is around, but only insofar as it is broken down into units that exemplify, contrastively or conventionally – if one of the possible differences it embodies is realized.

Also Rheinberger requires for representations that they are made "coherent and resonant," which is reminiscent of Goodman's criteria for fitting (Rheinberger 1997, 112). These properties do not pertain to single representations, but to networks of representations. Two important ways in which representations can contribute to coherence and produce resonance in a network of representations can be distinguished. If representations cannot be compared with what they represent, they can be compared with other representations. If differences are introduced in a particular realm by different means, the resulting schemes can be aligned. If, for instance, differences are introduced in the realm of a certain type of cells by using an electron microscope in combination with a certain staining dye and another population from the same realm is subjected to homogenization and centrifugation, the two schemes can start to resonate if ranges of extensions isolated by the different methods are subsequently subjected to the same contrastive procedure such that similarity relations can become established between them. If this happens the labels from different schemes that organize the same realm become linked in a relation of co-extension, although they do not have the same meaning.

Another form of resonance can be described as the combination of constellations in which phenomena are individuated and specified. The most relevant ways in which representations of phenomena are combined are the formation of taxonomies and the formation of explanations. If contrastive procedures are applied to a realm and always the same criteria are applied in each set of contrasts, a taxonomy might be constructed. The taxonomy will have a material correlate in collections that constitute the constellations in which the classes are represented. On the other hand it will be an ordered set of schemes.

Although there are many forms of explanations, the way representations of phenomena are combined to larger explanatory representations can be illustrated out by looking at mechanistic explanations. Machamer, Darden and Craver famously explicate a mechanism as follows:

> Mechanisms are entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions. (Machamer et al. 2000, 3)

The entities and activities here are phenomena that become represented through contrastive procedures in material constellations that individuate and specify them. They can be thick or thin, that is, they can be objects, events, structures or processes. If the material constellations that represent those phenomena are combined, a representation of the mechanism as a phenomenon is achieved. But why is the resulting representation explanatory? The point is probably simply that a link between phenomena always provides more information than the phenomena separately. The whole point about contrastive representations was that a phenomenon is never represented alone, but at least put in a relation to other phenomena that make up the realm in which differences are introduced. As a result, a causal event, even if it cannot explain a structure, still has some explanatory force with respect to a thin object. It can explain why the object occurred in a particular background. Also a represented structure can have some explanatory force. If, say, the cell wall is represented as different from the inside and the outside of the cell, then the continuity of the inside of the cell is explained partly through the containment that is provided by the cell wall, without there being any events, processes or mechanism invoked. Containment is a structural feature. Finally, if a process is traced, for instance, if a population of cells is marked with a dye in an early embryo and traced through development, the movement of a population stands out at least against the rest of the embryo, no matter how differentiated our perception of the rest might be due to other contrasts. If the population migrates, then it migrates to a position relative to the whole embryo. This phenomenon alone should thus have some explanatory force concerning the state resulting from the process.

Even if one is not willing to call such relatively simple representations of phenomena explanatory, one at least has to admit that the boundary between descriptions and explanations is not very sharp. The representation of a mechanism requires that one of the phenomena represented is taken as an outcome (explanandum). To "take it as an outcome" indicates that a set of alternative actions is applied that map on the distinctions made between objects. To set the focus on some phenomena as opposed to others is an act. Then phenomena (the entities and activities in the explanans), probably from a range of phenomena, are selected such that not only their manipulation, but also the manipulation of the relations among them can be used to manipulate the outcome. This manipulation requires to make the explanans phenomena accessible in the respective procedures. Representing a mechanism is representing its components. In a way a mechanism is represented, when all its wheels and levers are represented and thus made available to operate the mechanism.

The components of a mechanism can be thin or thick phenomena. A mechanism can be heterogeneous in this respect. And there is no preferred direction of developing a mechanism in the sense that all thick phenomena have to be replaced by thin ones or *vice versa*. If complex systems like organisms are explained, there will always be thin phenomena functioning as black boxes. The mechanism might be refined if they are turned into thick phenomena, but if a mechanism is represented outside the material constellation in the form of an discursive explanatory model, the thick phenomena have to be turned into thin phenomena in that they have to be made available to some notation.

I will speak of the material constellations brought about by a contrastive procedure as models. Models are distinct from the observational systems employed. Observational, and experimental systems in particular, are constructed to model phenomena. A model of a phenomenon is a particular experimental setup within an experimental system. For biology, Rheinberger puts it as follows:

> The term 'model' is here taken in its generic meaning as an experimental setup that appears to be particularly apt for the analysis of a particular phenomenon and that usually involves a corresponding model organism. Every valuable experimental system is a model system in this sense. (Rheinberger 2007, 38)

The phenomena are epistemic things, embedded and contained in an experimental system (Rheinberger 1997, Ch. 2). The important point is that epistemic things "embody what one does not yet know" (Rheinberger 1997, 28). Experimental systems are thus understood as generating knowledge, as opposed to only arrangements for testing hypotheses. That is, because they not only introduce previously unnoticed differences, but in doing so, make them afford further activity that can yield further differentiation or unexpected resonances in one of the ways mentioned above.

When I speak of models here, this form of model has to be distinguished from other models. What I call model is a more or less complex arrangement of samples, instruments and the imagery that they produce. It is a material model. What is peculiar about such models is that they include what they model. If molecular interactions are represented in a model organism, the organism contains these molecules and the interactions take place in the organism. But they are discriminated and highlighted – carved out from the complex net of causal interactions – only in this arrangement. This is why they are represented and why the arrangement is a model. The modelling of ecological interactions by means of collections of appropriately annotated specimen as it is described by Griesemer (1990) similarly includes the phenomenon modelled, even if only in form of "remnants." because the model contains the phenomenon it represents which in the model is an exemplar, such models might be called exemplificational models. They can be distinguished from other material models that do not contain the phenomenon in question. A balls and sticks model of a molecule does not contain the molecule it is a model of. It does not even have any indexical connection to the phenomenon in a way a photographic image does. It might be called an analogical material model (where no reference is made to semantic and syntactic properties, but to reasoning by analogy). It nevertheless has phenomenological qualities, which make it informative. With Gooding, one can probably speak of "phenomenological models" in both cases (Gooding 1990, 88). Phenomenological models can be distinguished from theoretical models that will take some notational form, either discursive or mathematical.[37]

This chapter has introduced activities in an account of representation. On the one hand the activities that produce representations, which have been found to be characterized by a contrastive structure that follows from the systemic nature of knowledge, which is in turn derived from the systemic nature of symbolization. On the other hand there are those activities that answer to the represented phenomena. They come in alternatives, just as the signs, things, and thoughts. Furthermore, distinctions have been introduced concerning the aims, focus and use of contrastive procedures. Thick and thin, as well as simple and complex representations, and different notions of model have been distinguished. These distinctions should be helpful to analyze why and how characters are identified and

[37] This classification is not elaborated enough to account fro all types of models in science. Is an anatomical wax model less connected to the phenomenon than a photographic image, just because the information goes through the modelers eyes and hands? Does not every analogical model or simulation start with at least some data derived from the phenomenon? Is a schematic diagram of a cell derived from a micrograph a material model, or in its notational character more akin to a theoretical model? (For case studies in material modeling, see de Chadarevian and Hopwood, eds. (2004), and for a more general discussion Griesemer's comment therein.) Are data models, which are statistically treated mathematical notations of data theoretical models? What is the difference then between them and mathematical models that fit the data? (Concerning models of data, see Suppes (1962).) My only aim here was to point out that contrastive constellations are models.

individuated in biology. Before I turn to the historical case study, let me, by way of conclusion of the foundational part, come back to pluralism as discussed in the beginning of Part I for a moment.

Part I Conclusion

It might appear as if I have made concept formation depend on given properties and causal relations. How is this picture reconciled with the anthropocentric pluralism suggested in Chapter 1? First of all, even Goodman, who criticized the idea of privileged properties for classification and rejects the many roles that similarity had to play in philosophy, with respect to classification and representation, does not deny that there are properties and, accordingly, similarities. But there are indefinitely many properties. And what properties can be detected as same or different depends on which objects are individuated in the first place and this in turn depends on what properties are recognized. The point is to extract a coherent network of objects and properties from the many possibilities to individuate them. Some properties maybe salient for the contingent reasons that the human sensual system reacts to certain features more than to others, but that does not mean that those properties are the most relevant in any context. In general one cannot rely on the relevant properties to be obvious and guide us to the relevant concepts. They have to be made perceptible and many different properties can be made perceptible. This does not only imply that they have to be somehow translated into perceptible properties, but also that there has to be a contrast. This requires that properties are construed as determinates that belong to certain determinables. A contrast in the relevant sense is the juxtaposition of phenomena with different determinate properties. If we have thus defined a determinable among the indefinitely many there are and if we are able to discriminate some of the determinate properties, we can also say that things are similar with respect to that determinate property. This implies that we individuate similarities only as relations in systems of similarities and differences that are given in particular constellations of objects that have to be arranged in a certain contrastive way to represent the similarity relation. In this arrangement, which we can call a model, the objects are exemplars. They can exemplify labels (or behaviors) in a scheme. Similarity appears thus appears in a semiotic system and does not motivate it. Once a scheme is established, the fact that the label denotes these objects might be the only indication that they are similar and makes us attentive to look for the similarity. In this sense similar is what is denoted by the same label. The same goes for causality. The point is not that there is no causality; there is a dense net of causal relations, but a particular causal relation appears in exemplificational models that makes particular contrasts. Thus also here, causality alone does not motivate concepts, but a concept forms if one of many causal relations is made accessible and intelligible in a semiotic system.

Furthermore, human action brings about ever-new objects or puts objects in new contexts. Pure substances such as gold do not occur in nature. If they are produced, there will be new similarity relations. The same goes for more composed artifacts. And if an object is brought in a new environment, there will be new causal interactions. One can speak of causal dispositions, of course, as it is done when it is known that an object belongs to a class of objects, exemplars of which have been observed in actual causal relations. But to say that a fruit that grew before *Homo sapiens* evolved had the disposition to react in a certain way, if put on a *Wall-Mart* shelf sounds, albeit intelligible, grotesque.

Finally, I pointed out that classes can be formed by *fiat* if no difference in a property is observed and that labels and behaviors can become attached to these classes by stipulation. This can alter the causal properties of the objects in the classes in question. Examples are, if things are treated as "valuable" vs. "invaluable" or "mine" vs. "yours," which, as we know all to well, has many causal effects in this world.

A stone might be interactionally simple. But since it can be contrasted with many things, put in many contexts and become the object of stipulated behaviors, it is certainly descriptively complex. It follows that descriptive complexity is not a function of whatever it is that makes us call things complex. It is a function though of the interactions between humans and things and among humans. The plurality we observe in the decompositions and classifications, explanations and taxonomies, and the definitions of the boundaries of the discipline itself, that we observe in a science like biology is thus not a result of the complexity of nature as the complexity pluralists have it, but instead stems from the complexity and principal open endedness of human practice.

The parts and properties of organisms that become individuated, and the many roles they play, are unsurprisingly an instance of plurality. Different actors might cross-classify and cross-decompose the units that make up organisms and they will be interested in doing so for different reasons. To investigate how parts and properties of organisms become subjects of knowledge will provide a case study for pluralism. Furthermore, it will provide a case study for the systematic constitution of knowledge explicated above and for the contrastive structure of epistemic practices. At the same time, the analysis of knowledge and epistemic practice in this respect will help to address historical questions about the conceptual development of biology. In particular, it will help to understand why there are different disciplines, what separates them, and what the splitting or merging of research fields or strategies amounts to.

PART II. CASE STUDY: CHARACTERS IN DISCIPLINARY PERSPECTIVE

Part Introduction

The term “character” has been used for parts or properties of organisms in many different roles. Characters can be the parts or properties that serve to classify organisms. As parts, they can be the objects subjected to classification themselves. They can also be the parts or properties of parts that motivate classification. Sometimes a determinable is called “character” and sometimes the determinate property. Characters can be contrasted with other characters within an organism or between organisms. Characters can be causes or effects. They can be the phenomenon of interest and they can be data for another phenomenon, which might itself be a character or not. Characters can be represented in dense and articulated symbol systems and they can be taken as thin or thick phenomena. They can be objects, structures, events or processes. Finally, when they figure in explanations, they can be part of the explanans or the explanandum.

Given this variety of roles in classification, explanation and representation of organisms, it is no surprise that the realm of organisms becomes organized in many different ways in different research contexts or that same character according to a particular scheme, can figure in different complex representations.

In the following, I will look at a very limited range of the whole variety of biological research. Nevertheless, but even in this limited comparative study it will be possible to show that characters are individuated and represented very differently and play very different roles in contexts. To understand this will also help to clarify some misunderstandings that result from not realizing that talking of characters is not always talking about the same concept. Furthermore, the case will illustrate the usefulness of the analytic vocabulary developed in Chapter 1.

Chapter 4. Characters: Terminology and the Distinction Between Parts, Variables and Values

Terminology

The most general term concerning the descriptions of organisms, which is frequently used by biologists, is “feature.” It is used in the most general sense to refer to parts (e.g. bones, muscles or legs), properties or qualities (e.g. leggedness, coat color, brown), processes (e.g. respiration, metabolic pathways) and activities or behaviors (e.g. swimming, escape response) observed in organisms. The term is often employed in definitions of other terms such as “character.” The ornithologist Kurt Fristrup for instance writes: “An observable feature, a correlated set of features caused by a single developmental or ecological process, a historic event in the evolution of a feature: all of these are characters.” (Fristrup 1992, 45) Apart from their more or less specific name or description (e.g. “bone”, “incus”, “coat color”, “brown coat color”), features of organisms are often referred to in a more abstract manner as “part”, “property”, “process”, etc., respectively. But there is also an important family of terms that is used in specialized ways, though differently in various contexts, namely “character”, “trait”, “(partial) phenotype”, “phene”, “morph”, and maybe some more. Sometimes features are also referred to with the term that indicates their theoretical, e.g. evolutionary, role (e.g. “adaptation” or “homology”).

“Character” and “trait” are the terms most frequently used for features of organisms in specific contexts.[38] According to the *Oxford English Dictionary*[39] both words originally

[38] The term “phenotype”, being a neologism, has a much shorter history. It is certainly more tied to the term “genotype” and the according conceptual changes with respect to the gene. This will be the focus of Chapter 6.

[39] "character, n.". OED Online. September 2011. Oxford University Press. http://www.oed.com.pros.lib.unimi.it/viewdictionaryentry/Entry/30639 (accessed September 23, 2011); "trait, n.". OED Online. September 2011. Oxford University Press. http://www.oed.com.pros.lib.unimi.it/view/Entry/204450?redirectedFrom=trait (accessed September 23, 2011).

referred to artificially produced signs before they changed their meaning such that they could designate naturally occurring signs that allow to recognize an object as an individual or as belonging to a class of things[40]. The meaning of "character" changed from "a distinctive mark impressed, engraved, or otherwise formed; a brand, stamp," to "a distinctive significant mark of any kind; a graphic sign or symbol" and from there on the one hand to "a graphic symbol standing for a sound, syllable, or notion, used in writing or in printing; one of the simple elements of a written language; e.g. a letter of the alphabet," a meaning that persists, and on the other hand to "a distinctive mark, evidence, or token; a feature, trait, characteristic" in general and finally to the specialized meaning in Natural History: "One of the distinguishing features of a species or genus." "Trait" changed its meaning from "a stroke made with pen or pencil; a short line; a touch (in a picture)" via "a

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[40] Similar in its etymology, and, due to the language's rank as a language of science and scholarship in the 19th century, very important in the history of this family of concepts, is the German word "*Merkmal*". It is a compound of the verb "*merken*" and the noun "*Mal*". According to the *Duden, Herkunftswörterbuch* the former has the same roots as the English "mark" in a Germanic word for "border", which later acquired the meaning of "sign" (marking the border), that was artificially introduced. Today "*merken*" has the meaning of "to notice" or "to memorize." "Mal" has the meaning of "stain," in a sense that historically associates it with an event like a sin, which it indicates. In this sense it is a naturally occurring, or god-made sign. Today it can also refer to a mole or something similar, but also to artefacts that remind of an event, like memorials. Within the compound it designates any naturally occurring aspect of a thing that allows to recognize it. So in a way both parts of the compound have the same meaning but the first part emphasizes the cognitive role, while the latter emphasizes the material nature of the sign. (*Duden, Herkunftswörterbuch*. 2nd ed. s.v. "*Mal*", 436; *Duden, Herkunftswörterbuch*. 2nd ed. s.v. "*merken*," 454.)

The meaning and use of the term in natural history are very similar to that of "character" or "trait" and thus the latter two are often used in translations from texts originally written in German, such as in Bateson's translation of Mendel's paper (Bateson 1902). "Merkmal" on the other hand is often used as translation for "trait" or the Latin or English form of "character", as for instance in translations Darwin's work, although in the case of the *Origin of Species* "character" is sometimes translated as "*Merkmal*" and sometimes as "*Charakter*", which indicates that there are a number of concepts designated by these terms (Darwin 1967, translation by H.G. Bronn and J.V. Carus).

line or lineament of the face; a feature," to "a particular feature of mind or character; a distinguishing quality; a characteristic." It seems that it was the particular forms of the faces, or other body parts, or behaviors of people (or animals) that where recognized as shared, but more importantly, as being different, among people (or animals), especially among parents and offspring, that let "trait" become a biological term.[41] This happened when the knowledge of traits as shared or differing among individuals became increasingly organized under the metaphor of inheritance, which was taken from the legal sphere in the first half of the 19th century.[42]

Due to their different etymology and entry point in the language of the emerging biological sciences, the word "character" is still more associated with systematics and evolutionary biology, while "trait" is more frequently used in genetics. Accordingly, "character" pertains more to features of species, while "trait" is used more often to refer to variation among individuals. Nevertheless, both terms are used in both contexts and sometimes even with the same meaning in one text (Allen 2002, 373-374). The words themselves are thus no guide to the conceptual distinctions that can be made in the areas where they are applied. Some of those distinctions are discussed in the following, using the word "character," but it should be kept in mind that they might also be relevant when the word "trait" is used or another of the above mentioned terms.

Distinctions: Features vs. Characters and Parts vs. Variables

A first notable distinction made by Fristrup is that between a feature and a character. "Feature," being the most general term, "denotes a description or measurement of a part, quality, or action observed in an entity." (Fristrup 1992, 47) Characters instead, according

[41] I have argued in Chapter 3, that similarity presupposes difference, in that properties only become salient in a contrast.

[42] The book *Heredity Produced: At the Crossroads of Biology, Politics, and Culture, 1500–1870* (Müller-Wille and Rheinberger, eds. 2007) provides a number of case studies that show how certain traits of people (but also features of animals) first became visible in a social and medical (or agricultural) context and then became conceptualized as "inherited".

to Fristrup, are relations among features. This means that characters are features that play a particular role in classification or explanation, where they related to other features. According to the typology of contrastive procedures provided above, one can say that features are the result of analysis, while characters figure in classifications and partitionings (though again, the use of the words is not consistent, what matters is the conceptual distinction). The criteria used to define characters are a matter of conventions pertaining to different fields and might include similarity of form, correspondence of internal structure, congruence of external associations, developmental congruence or functional relation to other features (Fristrup 1992, 47; Crovello 1970). Of course, a feature might be observed and subsequently be assigned a particular role, but in most situations, certain roles will be selected as interesting by the research program and this will influence the choice of observational means.

As the etymological history of the terms “character” and “trait” showed, both could and can be used to address individual peculiarities. But as biological categories, they usually refer to a type of thing. Fristrup writes (Fristrup 1992, 45):

> Characters are always defined with reference to a sample of biological entities (organisms, populations, species). Characters are named to express a perceived or hypothetical likeness among entities in the sample. Characters provide the basis for relating or comparing these entities. By recognizing likeness, we define a character, but its importance lies in emphasizing variations: differences in the character values assigned to entities. In fact, characters are never defined unless an observed or probable variation exists.

This passage sounds contradictory. One the one hand Fristrup says that likeness defines characters and on the other hand variation is required to define them. The statement is best understood as saying – in accordance with the analysis given in Part I – that characters become defined in contrast, that is when differences point to the part or property, but that they are types only in that there is likeness among the instances of a label. What the passage also points out is that one has to pay attention to the realm that becomes organized by a scheme of character labels. Furthermore, the passage makes clear that characters pertain to individuals in a class and not to a class. Sometimes, a character is attributed to a species, for instance if it is said that "Homo *sapiens* has an upright stance" but such a formulation is to be understood as a metonymy (Colless 1985, 34).

The entomologist Donald Colless makes an important distinction between characters as parts and characters as variables taking values (Colless 1985). His discussion, like Fristrup's is mainly concerned with the role of characters in taxonomy, but his results are important for all character concepts as used in various contexts. He starts from the observation that the term "character" is used differently in different contexts even within taxonomy and distinguishes three semantic categories subsumed under the term "character": Parts, attributes, and variables.

Characters as parts are the physical building blocks, such as body parts, but also segments of behaviors, or processes, of which an organism or its behaviors or activities are composed. In predicating statements about organism they appear as either subject or object (Colless 1985. 230).

Characters as attributes are the distinctive qualities or properties of a form, structure, material or function. They are possessed — in the logical sense — by an organism or part. They constitute a fact about an organism. A statement assigning an attribute to an organism is a description that can be true or false (Colless 1985, 230-231).

Characters as variables are a set of mutually exclusive values (Colless 1985, 231-232). Logically, the relation between a variable and its values is not to be thought of as a class and member relation, but rather as that of a determinable and a determinate (see Fn. 33 above). A variable is not possessed by, present in, or true or false about an organism. It can be said to be applicable to a part or an organism. A character as variable corresponds to the Aristotelian notion of a *fundamentum divisionis* with the values as *differentia*, which traditionally underlies taxonomic reasoning in natural history (see also Ghiselin 1984, 105). Another common distinction that maps on the variable-value scheme is that of character and character states. (In genetics characters are also construed as sets of alternatives, see Chapter 6.) Characters as variables can be defined intensionally through a part name and a determinable (e.g. eye color, seed size), which is useful especially for metric characters, or extensionally through a list of the values (e.g. blue, brown, green for eye color), which makes explicit the actually observed character states. In general characters that are values of the same variable are judged homologous.

To summarize Colless' distinction with the help of an example, "eye" would designate a part, "eye color" a variable. A variable comprises a set of values such as "blue" and "brown". The description of an organism (or populations, or species) as "has blue eyes," assigns an attribute to it.

Colless' text sometimes gives the impression that he equates attribute and values, but this is not the case. He says that values must be given as "brown" or "blue" instead of "blue eyes", whereas attributes have the form "has blue eyes" (Colless 1985, 232). Attributes are thus not the determinates of determinables, but predicate a determined variable over an organism (or population, or species). But even if we have to be sensitive concerning the difference between a character and the attribution of a character, this distinction is not on the same level as that between characters as parts and characters as variables that take different values, since both, characters and values of a particular variable can be attributed. Here I will focus on the distinction between parts and values of variables and interpret it in terms of thick and thin concepts.

What Colless describes as parts is the result of the contrastive procedure of decomposition. Decomposition has been characterized above as a contrast between parts of the same structure. A character as part results from the decomposition of an organism taken as a structure, that is from contrasts between its parts. A part can be become a variable, if it is used in a contrast between organisms. Then it is treated as an object, that is, as a thin phenomenon. As a variable it can take as values either "presence" and "absence," or different states. But as long as it is treated as a part, it is seen in relation to other parts of the same structure and it is itself a structure that is open to further decomposition. It is thus a thick phenomenon and affords manipulation.

The identification of a variable and the values it takes results from a contrast between organisms the variable is applicable to, what I have called analysis. Even if a part is identified in contrast with other parts of the organism, when it plays the role of a value, it only becomes identified in contrast with another value of the same variable. If a part is not previously identified in a decomposing contrast, but identified as a thin value of a variable (as an object), the attention can shift from the analysis of the object to the analysis or decomposition of the part. In this case the part will become treated as a structure. But as long as a part or property figures as values of variables, they are thin phenomena. They afford comparison and being recorded and can serve as data.

Variables can either be thick or thin. If the scheme that organizes the realm is semantically dense, there will for every two values a value that lies between them. An object under such a scheme affords ever more fine-grained determination of the value and furthermore cannot be an exemplar, but for itself. If the scheme is discontinuous, instead, a label will cover a range of variation and the objects in its extension can represent each other as exemplars.

In some contexts, characters might afford being treated as a part, in others they will afford being recorded as a value of a variable. Since the term "character" is used for both categories, confusion might arise either if the categories are not separated conceptually, or, even if used consistently, when the uses are not specified in the literature (Colless 1985, 232). One situation where it would be problematic not to distinguish between parts and variables would be the following. The character "eye" can be described as having the being "blue" or "brown". Since other determinable properties such as shape can be applied to the eye, the character that takes values is the variable "eye color," not the part "eye." A failure to make that distinction also leads to the, according to Colless mistaken view, that the character/character state distinction is just a matter of hierarchical levels. Of course "brown" and "white" could be character states of the character "wing" while "winged" and "non-winged" are character states of the character "limb-form" etc., but it is important to notice that "brown" and "white" are states of the variable "wing-color," which is applied to the part "wing", whereas "winged" and "non-winged" are states of the variable "limb-form" that is applied to the organism. That is, it is not a simple hierarchy where a character on one level is a character state in the other, but a character on one level switches it status from part to a value of a variable while ascending in the hierarchy. The distinction is also necessary to understand the concept of a dependent character: the variable wing-color depends on the value "wing present."

A failure to distinguish variables and values can be equally problematic. The difference between "eye color" and "blue eyes" is relatively easy to grasp and not much harm is done if the two uses of "character" are not made explicit. A more difficult case is the absence of a structure. If a variable such as "leggedness" is not applicable at all to a species this is a different case from that of a snake, where the variable is thought to be applicable due to snakes' position in a phylogenetic classification, but the value of the variable "leggedness" is "absent". To make this difference comprehensible requires to be explicit about characters as variables or values (Ghiselin 1984, 105).

Chapter 5. Parts: Pluralism, Function and Characters as Thick Phenomena

Pluralism Concerning Partitionings

Rasmus Winther (2006, 472ff.) distinguishes two styles of biological theorizing (in the sense of Hacking 2004, Ch. 12): formal and compositional biology. Whereas formal biology focuses on mathematical models that express quantitative relations between parameters and variables that represent relations between the entities they map on, compositional biology looks at the relation of parts and wholes and their respective functions and capacities (the notion of variable here is more narrowly restricted to mathematical representation than the notion of variable discussed above). This distinction crosscuts biological disciplines, however defined, in that all make use of both styles. But some disciplines might predominantly make use of one or the other style. For instance, theoretical population genetics can be seen as a science relying on mathematical modelling, and developmental biology, according to Winther, is a science driven by the compositional style, even if there is elaborate mathematical modeling of developmental processes[43]. This distinction seems more appropriate than a distinction between theoretical and experimental biology, because theoretical biology makes use of experimental data and experimental biology is certainly connected to theorizing, and it is exactly the theoretical side of compositional biology that Winther intends to look at.

Relying on an institutional notion of discipline, Winther takes "theoretical perspectives" as his unit of analysis (Winther 2006, 475ff.). Accordingly, within an academic discipline such as developmental biology, which is mainly pursued in a compositional style, but might also harbour formal approaches, researchers might have different theoretical perspectives. What makes up a theoretical perspective is the "partitioning frame" and the "explanatory account," which are guided by a certain set of biases, commitments and norms. "Partitioning frame" describes which parts are identified and at what levels in part-whole hierarchies they are located, where parts can be structures, processes, mechanisms, functions or time periods, everything that is delineated as a part of a whole. "Explanatory account" is supposed to capture the type of question that can be asked and the explanatory resources that can be exploited to answer them with this theoretical perspective. "Explanatory resources" refers to the types of models that are constructed, the mapping of the explanatory roles of the parts recognized etc. If it is a compositional style theoretical perspective, explanations will explain properties of the whole through parts, their

[43] On the history of mathematical modelling in developmental biology and the conflicts or misunderstandings between the physical and the experimental cultures, as she calls it, resulting from differences in what is regarded as a satisfying explanation, see Keller (2003).

properties and their organization. (“Organization” here implies that this style of reasoning can take emergent properties into account.)

Winther discusses three disciplines: Comparative Morphology, Functional Morphology and Developmental Biology. He shows that within these disciplines different theoretical perspectives are taken. Accordingly, the explanations given in these disciplines appear to be very different from each other concerning the type of question asked and the types of answers accepted, as one would expect. But furthermore, and maybe less obvious, different partitioning frames can be observed. For instance, Winther remarks that a leg is regarded as a part, an integrated whole, which is made up of bones, muscles and other sub-parts, from the perspective of functional morphology, where it plays a causal role in the systemic behavior of locomotion. In comparative morphology, on the other hand, muscles and bones are delineated as parts, and investigated concerning their status as homologies, but the leg does not appear as a part (at least in the account studied by Winther) (Winther 2006, 486).

Winther’s notion of compositional style matches the idea of a context in which thick conceptions of phenomena prevail and his idea of a partitioning frame can be read as corroborating my view that characters can only be individuated in a system, that is as part of a realm that gets organized by a scheme. The point I want to emphasize here, however, is that his analysis supports pluralism concerning characters as parts. From different perspectives, the organism will be divided into parts differently. And even parts that seem to be delineated in the same way they can still play different roles in explanations. It must be said, however, that Winther’s pluralism is motivated by the assumption of complexity.

Some critical notes on Winther’s account shall be mentioned here: First, Winther writes: “once a perspective has identified the parts pertinent to it, specific explanatory accounts can be adopted” (Winther 2006, 476). And it seems that he is not only heuristically separating these aspects, but thinks of them as discrete steps in reasoning and even sees them as appearing in chronological order. Instead I contend that what Winther calls the “partitioning frame” will in many cases be determined by his “explanatory account.” A way of defining types of parts can become established because these types of parts are necessary for the type of explanation that is envisioned. Explanations should be seen as constructed in a delicate process involving material and theoretical practice, in which the explanandum and the explanans, the parts on both sides and their explanatory relations are determined together and in mutual adjustment. The explanation sought will most likely influence what contrast is made and by which means differences are introduced. This leads to the second critical point, the neglect of the practice of partitioning.

The procedural character of part delineations is missed. Winther's reliance on textbooks as sources clearly has drawbacks here. But even from the textbook quotations Winther is presenting it becomes clear that a part is determined by the *activity* that is isolating it or making it visible. In a passage from the Comparative Morphology textbook he is analysing, muscles are separated conceptually by describing how the scalpel has to be used to separate them physically.[44] (This is reminiscent of the importance of cutting and touching for Aristotle's delineation of parts; see below.) Also comparing animals — necessary for the construction of parts as homologies — is an activity of holding objects (body parts or drawings) against each other. In other cases it is even more obvious that the delineation of characters depends on a procedure of detecting it, for instance when staining reveals differences among tissues. The account of contrastive procedures given above and their distinction by aim and focus should be helpful to further analyze the practices that result in a partitioning frame.

In the following I will take a closer look at how explanatory aims, especially concerning functional explanations influence the delineation of parts.

[44] "In studying the muscles it is necessary to separate each muscle from its neighbours. This is done by searching carefully for the white lines of connective tissue, which mark the boundaries of muscles and slitting along these lines with the point of the scalpel. ... After freeing the margins of a muscle the fingers should be worked under the muscle until it is separated from its fellows." (Hyman 1942, 222, cited from Winther 2006, 481)

Biological Objects, Function and Models

In a series of papers Manfred Laubichler and Günther Wagner discuss the relation of biological objects, including characters and the (functional) models that refer to them (Laubichler 1999; Laubichler and Wagner 2000; Wagner and Laubichler 2000).

Laubichler (1999) emphasizes the distinction between an object identified by means of some measurement procedure and the biological object individualized through its functional role. This is important, because often the functional terms are used to designate the object. Thus, even if "*ß-galactosidase*" is used to refer to the object in question, the object is a protein and the term refers to it in its functional role in a given context. The context in this case would be a theory of cellular metabolism. Generalized theoretical models will have more general characterizations of the functional role such as "metabolic enzyme".[45] Nevertheless, the protein as an object can be measured independently and function might only be ascribed to it subsequently. Analogously, a character as part (e.g. the vomeronasal organ), as detected by an anatomical procedure (histology), can have the function (olfactory sense organ) in one or more theoretical context (e.g. neuro-physiology or behavioral biology), where it has a generalized role (sense organ or signal receptor, respectively). It is useful to keep the measurement and the ascription of functional roles separate, because often functions are taken to individuate characters.[46]

When it comes to ascribing roles, it seems, however, that Laubichler puts too much weight on functions. An object might figure in a theoretical model but not as having a function, e.g. when a protein or a morphological character is used in taxonomy. In this case

[45] Laubichler frames his distinctions in terminology from Peircian semiotics, with the object given in the measurement as the object sign, the functional role as the representamen and the generalized role in the theoretical (interpretative) context the interpretant. But also a non-functional concept would be a representamen and the functional interpretation is better understood as a further semiotic process.

[46] Karen Neander writes: "… most biological categories are only definable in functional terms." (Neander 1991, 180, see also Neander (2002) and Griffiths (1994) and (2006), as well as Amundsen and Lauder 1994 for further discussion of the role of function for character individuation).

the protein, for instance, would be considered as sequence in relation to other sequences. Laubichler also uses the term “relational property” for functions. This could be adopted as the more general term and (causal) functions should be treated as one type of relation among others (I prefer, however to speak of roles, functional and other).

Nevertheless, Laubichler's initial distinction between the object and its relational properties is important, because one object can be relationally characterized in various ways. The protein for instance can be seen as homology or as enzyme. So the object might be considered in theoretical contexts that assign different relational properties (roles) to it. It can be said that still the same object is considered, when the same method of identifying the object is used, what I have specified as a contrastive procedure (e.g., in the protein case the same chemical way of measuring, isolating or sequencing the protein). It is also important to see that a relational property (role) is interpreted in, but distinguished from a theoretical model in the sense that one relational property can be considered in different theoretical contexts (for instance neuro-physiology and behavioral biology in the above example of the vomeronasal organ as an olfactory sense organ). Of course, since the functional role is defined by, and thus not independent of the theoretical context, the latter point implies that there is a recognized connection between the two theoretical contexts that allows to regard them as being concerned with the same function of the object (e.g. that behavior coordinated through signals requires neuronal activity). In other theoretical contexts the object might not be regarded as having any biologically relevant relational properties at all.

The important point is that the role of an object, such as a character, in a functional, classificatory or other type of model will still influence the individuation. Laubichler and Wagner (Laubichler and Wagner 2000; Wagner and Laubichler 2000) propose that the behavior of a character as a variable in a mathematical model of a process (again this use of “variable” is more narrow than the one discussed above) determine whether an individuating scheme is adequate in a given context.

According to the authors, natural processes, interpreted as a mapping of an initial state to a subsequent state of a system, can be represented through mathematical models, which map one set of numbers to another (this is a different notion of process from the one I use in this thesis). The former mapping is determined by the physical properties of the process, while the latter mapping relation consists in mathematical operations (Laubichler and Wagner 2000, S291). The functionally relevant objects are those that are represented by variables in the equations that remain constant under certain important transformations. This holds for characters as much as for any other biological object that plays a role in a model of a biological process. Accordingly, there might be different character decompositions, depending on which biological process is mathematically modelled. Wagner and Laubichler give an example for the modelling of natural selection, where characters are construed as adaptations, but a model of the development of a characters might individuate them differently.

What can be learned from this discussion is the following. While some authors seem to suggest that characters are individuated according to their function, Laubichler (1999) points out the relative independence of the measurement procedure that makes a character visible and thus amenable to functional reasoning. However, his and Wagner's arguments concerning modelling emphasize that the intended model might still suggest the measurement method or select among alternative individuation schemes in a given situation.

Thus even if alternative decompositions are achieved by different anatomical methods, a model might determine which one is to be used. However, not all research programs that involve characters aim at mathematical models. Furthermore, if the model not only selects among different ways to decompose a whole, but also suggests how the variables it contains should be measured, the pathway leading from the theoretical modeling activity to the measuring activity must be described to understand how models determine the individuation of objects. A last important critical point is that it might still be possible to change the model if there is reason to cling to one partitioning scheme. Whether the practice in a given research context is driven by theoretical models or rather by procedural interaction with the material is to be judged on a case-by-case basis.

To see how methods of introducing differences are used in the individuation of characters as parts and what other factors play into the process, I shall look at the *locus classicus* concerning the "*Parts of Animals*."

Aristotle and the Parts of Animals: Dissection, Contrast, Function

Aristotle's writings on biology are among the earliest systematic descriptions of organisms.[47] It's worth looking at the way he perceived parts of animals, because his work already contains most of the issues that are present in different forms every time when the parts and properties of organisms are under investigation and his ideas had a far reaching influence on later schools of natural history, anatomy and physiology. In particular, this short excursus in the early history of biology might serve to show that the delineation of parts is historically conditioned, dependent on the observational practice (the physical delineation of parts as well as the choices of contrasts), and intermingled with the explanatory aims of an inquiry.

A first observation is that even Aristotle did not start from nothing, but practiced as someone deeply embedded in a tradition. Many parts and properties he discusses had names in common language, be it because they are salient with respect to human senses and dimensions, be it because they were relevant with respect to some human practice, for example related to food production. Aristotle often uses the expression "what is called" when he refers to parts. What can be learned from this is that a partitioning or classification

[47] It is common practice among philologists to refer anachronistically to Aristotle's writings on animals as his "Biology" (see e.g. Gotthelf and Lennox, eds. 1987). Aristotle's Biology mainly consists of *History of Animals (HA), On the Parts of Animals (PA),* and *Generation of Animals (GA)*. In HA he limits himself to description of parts of animals as they appear in different animals. In PA he gives functional explanations of the forms and qualities of the parts. In GA he discusses the reproductive organs and the generation of the parts from the fertilized egg.

usually starts from a situation where some probably competing partitionings and classifications are already in place. The entrenched systems often provide the realm that becomes organized (sorted or decomposed) by a new scheme.

Aristotle also refers to more specific knowledge concerning the anatomy of men and animals, in the form of written sources from preceding scholars, like Diogenes, for instance, or oral reports from practitioners like fishermen. As a matter of fact it is not always clear which observations Aristotle made by himself and which stem from testimony. But not only is the delineation and designation of parts influenced by prior knowledge; also aspects of their theoretical explanation are taken from the natural philosophy of his predecessors, Empedocles for example, which Aristotle, of course, developed further. A last role of the tradition lies in the fact that his methods of observation were partly taken over from preceding practice.

The second point to observe is his observational practice. Even if he included testimonial knowledge in his reasoning, his biology relied to a large extent on observations, which shall now be described in more detail. Aristotle mainly studied animals. However, he comments also on the parts of plants and many things he says about the parts of animals he seems to think of as being true also for plants. He obviously practiced dissections of dead animals. Probably he also dissected living animals. It is debated but unlikely that Aristotle dissected human corpses, but he might have used evidence from lesions or diseases in humans as evidence for the individuation and functional characterization of organs.

Aristotle makes a distinction between the internal and the external organs. The most striking point about dissection is that it allows to observe the inner parts of organisms and even the inner structure of the inner organs. How important this material practice was for the development of conceptual distinctions can best be seen when Aristotle introduces a distinction between the homogeneous and the heterogeneous parts. The former might be seen as roughly coextensive with what in the 18th, and especially in the 19th century became characterized as tissue types, whereas the latter corresponds to organs or complex body parts. However, the way that Aristotle defines homogeneous parts is as those parts of the body, a fraction of which falls under the same description as the original part. If a piece of flesh is *cut* away from a larger piece, it is still flesh. This is not true of the heterogeneous parts of the body, like the eye, nostril, the whole face, the fingers, the hand and the whole arm. If such an organ is cut apart, the fraction will not be of the same kind — a piece of a hand is not a hand. Some parts, especially the inner organs are both in that they are made of only one homogeneous material, but are structurally complex, like the heart with its various chambers and valves (Aristotle PA, Book II, Part 1, in Lennox 2001). It can be seen here how the method of cutting determines the conceptual distinction.

Obviously the anatomical procedure also implied to touch the organs. Touch, as Christopher Cosans points out, for Aristotle had a priority over visibility concerning the knowledge of things made of matter and thus informs more reliably about the qualities of parts than vision, which might yield different impressions for various students of animals or under different conditions (Cosans 1998, 312). But touching the parts of the body did not only determine the dryness/wetness, hardness/softness or hotness/coldness of the parts, but also its form. The delineation of elongate organs, for instance, like the oesophagus or sinews or vessels had to be traced out with the fingers in order to determine their way through the body and the organs they connect to. Touching, by Aristotle is used as a technique in contrastive procedures, in particular in decompositions.

Aristotle's practice exemplifies the ubiquitous role of contrast. Aristotle performs decomposing contrasts (between parts of the same animal) as well as classifying contrasts (between different animals). Observations require contrast and establish schemes that organize a realm. Aristotle's observations on different temperature in different sides of the body, for instance, invokes a planned simultaneous or subsequent touching of those areas (Aristotle PA, Book III, Part 7).

Sensations like temperature and degree of hardness or dryness, resulting from the material composition of parts serve as a basis for decomposing contrasts. But "Aristotle also considers how parts are differentiated other than by their composition; namely, by their presence or absence, by arrangement or position, by ''the more or less,'' and by differences in their features or qualities (for example, coloration)." (Blits 1999, 61) This implies that Aristotle delineates parts by contrasting parts in various animals, however without necessarily intending a classification of animals. Parts in different animals are contrasted on the basis of material and shape (by analytic rather than decomposing cutting and touching) and than further determines them according to a variable (*fundamentum divisionis*) such as length, width, texture, density, arrangement and number that can have discrete values, but is also often described in a quantitative manner as "more or less" (Lennox 1987, 342).

Another aspect of Aristotle's biology is the way it fits in his general philosophy of nature and his view on how knowledge about nature is to be achieved. This is not the place to reconstruct Aristotle's philosophy but the picture that emerges from Aristotle`s biology concerning parts and properties of organisms can be roughly summarized as follows: The constituents of animals and plants exhibit three degrees of composition. The homogeneous parts are composed of the elements, earth, air, water, fire, or of their forces as he specifies. These constituents of material are — although different from the actual stuff (earth, air, water, fire as it is found in experience), which just consists of the respective elements almost exclusively — not thought of as atoms, but rather as continuous matter. The homogeneous parts of animals (as other stuff, like stone) do consist of a combination of these elements, which Aristotle seems to think of as a compound or at least alloy, rather than a mere mixture in that it acquires new properties according to the proportions of their combination (Lones 1912, 91). The heterogeneous parts are composed of different homogeneous parts. The hand for instance is made of flesh and bones. This theoretical framework can be seen as influencing Aristotle's choice of contrastive procedure. Touch was though to be particularly apt to detect the properties determined by the elements (dry, wet, hot and cold) and thus allowed to determine which element was dominant in which homogeneous part.

Similar things can be said about the notion of function. Concerning the last step in the hierarchy of composition, that of the whole body from the parts, every part has its function as an instrument of the soul (this is the original meaning of the word organ), although this does not require that every organ is operated by the soul as Aristotle illustrates with the metaphor of a state in which, once an order is established there is no need for a monarch to permanently intervene in the actions of its members (Cosans 1998). The qualities of the parts that result from their specific composition are explained by the role the parts play in the specific life the animal lives (Lennox 1999). Here the explanatory goals will also influence, but not fully determine the partitioning frame.

In the following, I want to cite some examples to show how disciplines such as anatomy and classical embryology worked with a thick concept of characters as parts. This take on characters is certainly influenced by the functional characterizations and other relational properties they the practitioners were interested in, but is best investigated in the ways that representational practices are linked.

Characters as Structures: Thick Phenomena in Anatomy

Anatomy in the broadest sense[48], is concerned with characters as parts, even if the term character is rarely used in this context. In terms of representations and affordances, it is concerned with structures. A contrastive procedure as a setting within an observational system can represent structures. In the discussion of Winther's analysis of compositional biology and Aristotle's practise of this style of investigation, the role of cutting, touching and seeing in contrastive procedures, in particular in decompositions was mentioned. If these observational techniques are applied in a contrastive procedure that aims at

[48] The boundaries between Morphology, Anatomy, and Physiology are not always easy to draw (see Nyhart 1987; Cunningham 2002, 2003).

decomposition, a part becomes represented as a structure. The structure itself in its materiality is a representation, in that it exemplifies and it does so only in virtue of the contrastive constellation, even if it is isolated from the whole in the course of the procedure. Such an exemplar is always capable of exemplifying in a dense symbol system. For every sub-part such as a fragment of the isolated organ it is impossible to say that it belongs to one kind of sub-part rather than the other, because under a dense interpretation, the kinds are so ordered that between every two there is a third. The system is thus syntactically dense. It is also semantically dense in that, for every sub-part (for every fragment of the organ) it is impossible to decide if it exemplifies one label or the other. The labels in a descriptive language are so ordered that between every two there is a third.

The organ can of course also be interpreted in an articulated symbol system. In such a system the organ as a whole would be one object belonging to a kind of objects and exemplify one label. If such a scheme would be applied, the object affords to figure in a contrast between different organisms, where the mere presence or absence of the organ is recorded. In this case the organ is a thin phenomenon. But if the dense scheme is applied, the organ as a structure affords to be further investigated with respect to the details of its relation to other parts and its structural properties. It affords further decomposition.

If anatomists produce secondary representations, descriptions and images, their descriptions of the structure must be at the same time a description of how to isolate the structure. Thus they provide a way for other anatomists to avail themselves of the structure, which then, as I said, affords further decomposition. With respect to images, they might be dense such as photographs, which in principle, and this is the best prove for their thickness, enable someone to discover sub-structures that have gone unnoticed in the situation of their production. Other images, such as *camera lucida* drawings are more diagrammatic. But if we compare them to a mere name that denotes the object, which belongs to an articulated scheme and renders the object thin, in that it affords no further investigation, the structure represented in the drawing appears thick. The drawing might be schematic, but it offers the possibility that someone relates e.g. the depicted shape to some function, that the producer of the drawing did not know of. This is not afforded if the part in question is represented in a sentence such as “There is an organ which I call *xy*.” The drawing as opposed to the name cannot be replaced without a change of representation. The best indicator for a representation of a thin object is that it can be replaced by another symbol.

As an example for a thick phenomenon in anatomy, I will sketch the history of the adrenal gland, an organ that is attached to the kidneys, and consists of two sub-structures, the adrenal cortex and the medulla, both of which release hormones.[49] The adrenal gland was recognized in dissections at least since Galen (129-199), but he described them as “loose flesh,” that was not seen as fully distinct from the kidney, but rather as some extra kidney material. maybe the situation is best described in the way that he detected a difference between the adrenal gland material and the “other” kidney material in terms of integratedness, but he was not able to make the scheme resonate with other schemes that organize the same realm, thus the distinction did not gain significance. The adrenal glands were described as structures properly distinct from the kidneys by Eustachius (1520-1574), although he considered them as supporting the function of the kidneys and at his time the existence of this separate structure was still contested.

When by the early 17th century, the concept of the adrenal gland as a structure distinct in substance and function from the kidney was finally well entrenched, it afforded further investigation of the way it is attached to other parts such as the kidneys, blood vessels and nerve fibres, as well as further decomposition. Every aspect of the organ mattered for the anatomist and also for the physiologist, who asked for the function. Morphological differences between the sub-structures were recognized e.g. by Georges Cuvier (1769-1832). In 1836 N. Nagel described the medulla as a substance distinct from the adrenal cortex. Mainly due to the use of compound microscopes, anatomists, who now became

[49] My historical sketch relies in large parts on a review by Stephen Carmichael (2005).

histologists, founded their decomposition of organs on the different ways these were composed of cells (see Coleman 1971, Ch. 2).

Albert von Kölliker (1817-1905), in his influential *Manual of Human Histology* describes the adrenal glands and their internal structure in detail. Concerning the distinction between the adrenal cortex and the medulla, he writes:

> The former, *substantia corticalis*, is more compact, 1/3-1/2''' thick, tearing readily in the direction of its thickness, and, when torn, presenting a fibrous aspect. Its color is for the most part whitish-yellow or yellow, in the innermost third, however, usually passing into brownish-yellow or brown, so that in a transverse section, two layers maybe distinguished, an external, bright-colored layer and an internal narrow, dark border. The *medullary substance* is, normally, of a brighter color than the *cortical*, being of a greyish-white with a tinge of red, although when its numerous veins are full of blood, it may assume a darker and more venous hue. Its consistence is softer than that of the cortical substance, though not so much so as is usually believed, and with respect to its thickness, it is very inconsiderable (1/6-1/3''') at the thin borders and at the upper and outer extremity of the organ, whilst in the middle, and in the lower and inner half, it amounts to as much as 1''' or even 1 1/2'''. (Kölliker 1854, 214-215)

He also describes the different cell forms that are found in the structures and the connections to the vessels and nerves. Kölliker manages here to introduce a number of differences by applying various contrastive procedures, looking at one region of the gland, looking at the other, tearing one piece of the material and tearing the other, applying a measuring tool to one and to the other. Once there is one difference, for instance in visual impression, others can be applied in a ways that more and more schemes organize the realm in a consistent way and resonate with each other. This will increase the fit of the category and lead to its entrenchment.

Function does play a role. “As regards the *functions* of the suprarenal glands” he writes:

> *I consider the cortical and medullary substances as physiologically distinct*. The former may, provisionally, be placed with the so-termed “blood-vascular glands,” and a relation to secretion assigned to it; whilst the latter, on account of its extremely abundant supply of nerves, must be regarded as an apparatus appertaining to the *nervous system*. (Kölliker 1854, 219)

But function can hardly be described here as defining or individuating the structures. What happens is that Kölliker detects similarity (between medulla and the vascular system and cortex and nervous system, respectively) on the basis of the properties distinguished through contrast. Similarity as suggested above presupposes difference. The similarity relation here motivates a composition rather than a grouping, rendering the structures components of the vascular and nervous system.

The next step in the history of the adrenal glands was the application of contrastive procedures that relied on histochemical means. The preparation of sections and the use of the microscope made Kölliker’s observation already quite manipulative. Now another level of manipulation was introduced that relies on causal relations, which are not, however, themselves the subjects of investigation. I will now turn to processes as thick phenomena as they are studied in classical embryology in order to discuss similar kinds of manipulations.

Characters as Outcomes of Processes: Thick Phenomena in Embryology

James Griesemer describes the epistemic practice of “*Tracking Organic Processes”* (Griesemer 2007) by looking at several case studies in embryology and genetics in the late 19th and early 20th century. In discussing his approach, I will first emphasize the contrastive nature of such practices, and then show that in the case of embryology, processes as well as the characters as parts that are the outcomes of such processes are construed as thick phenomena. Classical genetics will be discussed in the next chapter.

Experimental embryology emerged as a discipline in the course of the nineteenth century together with other specialized biological disciplines that were all driven in one way or the other by the notion of organismic parts being composed of cells (see Coleman 1971, Ch. III). Thus, other than comparative embryology, which stood more in the tradition of natural history, experimental biology did not focus on the contrast between organisms, but on contrasts within organisms, in particular on contrasts between cell populations.

Griesemer characterizes the practice of tracking organic processes as follows:

> Tracking work provides the basis for causal narrative accounts of prospective significance, which involves two shifts of attention: (1) from developmental outcome to some earlier stage of a central subject significant to the narrative from which to begin tracking, then (2) tracking the historical process forward in time, conceptually "back" to the future developmental outcome from which the narrative account began. (Griesemer 2007, 399)

The developmental outcome, "the development of organized heterogeneity out of the apparent homogeneity of the fertilized egg," (Griesemer 2007, 397) is the explanandum of embryology. The local explanandum might be a certain stage of the embryo, which is explained by processes leading from an earlier stage to the stage in question, but ultimately, these explanations aim at the form of the adult plant or animal (although the notion of adulthood is not very precise or rather arbitrary, given the continuity of the life history of an animal). What is important is that the adult form is perceived as being composed of distinct structures. These structures, as we saw above, came to be conceptualized as being composed of specialized, in the language of embryology, differentiated cells. Cells in earlier stages of development came to be conceptualized as determined. Griesemer writes:

> These "cell lineage workers," already very familiar with the end results of development, shifted attention to early cleavage stages of blastulation. They sought to identify the fate or prospective significance of cells that did not yet

> manifest the differentiated states of the kind of tissue or organ to be explained, whether epidermis or mesoderm, neural plate or lens, notochord or somite. (Griesemer 2007, 399)

The adult organism afforded anatomical decomposition, but the structures were not only defined as being different from other structures, with respect to position, shape, color or reaction to some intervention, but they also differed with respect to what brought them about or was transformed into the structure in question, while the embryo in this context afforded to contrast structures in terms of what they, in turn, give rise to or what they transform into.

Embryologists had to "track a process of cell division leading from a determined state to a visible embryonic differentiation." (Griesemer 2007, 402) Processes are transformations of structures in time. What are contrasted in the contrastive procedures of embryology are such structures over time, or what is called a lineage. The contrastive procedures can be characterized as visualizing differences in the fate of cell populations; they are decompositions, in that they decompose the whole process of development into partial processes. Classical embryologists did so by "introducing and then following 'marks' in order to establish the fate or prospective significance of *marked parts* of a dynamic process." (Griesemer 2007, 378, my emphasis)

Marking interactions can be one of the following activities or a combination of them (Griesemer 2007, 399). The first activity is mental marking, which basically relies on the fact that some notable features in a process can be followed with the eye, e.g. under the microscope, by focusing one's attention on them as opposed to others and track their transformation by literally "keeping and eye on them." The trajectories of structures will typically be captured by physical marks in a diagrammatic representation, e.g. a *camera lucida* drawing. These drawings allow to make a second observer understand which was the notable feature followed, since it omits many other equally notable observable features.

Griesemer illustrates this practice with the work of C. O. Whitman and E. B. Wilson in the late 19th century.

Secondly, marking procedures can make use of artificial substances that highlight some structures as opposed to others, just as in the case of anatomical staining, but are observed over time. The intervention again does not only introduce a difference for an observer, but allows her to share the observation. This strategy is illustrated by Walther Vogt's investigations in the 1920s.

A third way to track processes consists in heterospecific tissue hybridization, an approach most famously applied by Hans Spemann and Hilde Mangold in their work that led to the organizer concept. The strategy they followed not only made certain cell populations visible when the implanted cells had a different pigmentation from the host embryo, but furthermore, allowed them to infer the state of the implanted cells at the time of extraction, by following their fate in the new embryo.

In one case discussed by Griesemer, Edwin G. Conklin's (1863-1952) *The Organization and Cell-Lineage of the Ascidian Egg* (1905), among other procedures performed, a strategy is followed that lies between the first and the third way of tracking processes. Conklin "relied specifically on pigment markings of cells, which behaved as though the observer had introduced a persistent physical mark directly on the embryo." (Griesemer 2007, 404) Conklin describes his discovery as follows:

> The very first lot of the living eggs of *Cynthia* which I examined showed a most remarkable phenomenon and one which modified the whole course and purpose of my work; for there on many of the unsegmented eggs, which were of a slate-gray color, was a brilliant orange-yellow spot, which in other eggs appeared in the form of a crescent or band. Further observation showed that this crescent became divided into two equal parts at the first cleavage and that it could be followed through the later cleavages and even into the tadpole stage. (Conklin 1905, 7)

Apart from being an example for the open character of experimental systems and procedures for new phenomena and unplanned changes in strategy, that was emphasized by Rheinberger and Gooding, Conklin's approach shows how a contrast (gray/yellow) defines a phenomenon against what it is not, in this case makes a lineage stand out against the manifold process of development, made up of countless interdependent cell divisions. It allows to interpret characters as parts, at least in the tadpole, such as the tail muscles, as originating from certain regions in the embryo, and structures in the embryo as having a " prospective significance for future states." (Griesemer 2007, 399) Griesemer writes: "Presence of yellow pigment in a cell at a later time meant membership in the cell lineage tracing back to the original mark." (Griesemer 2007, 404)

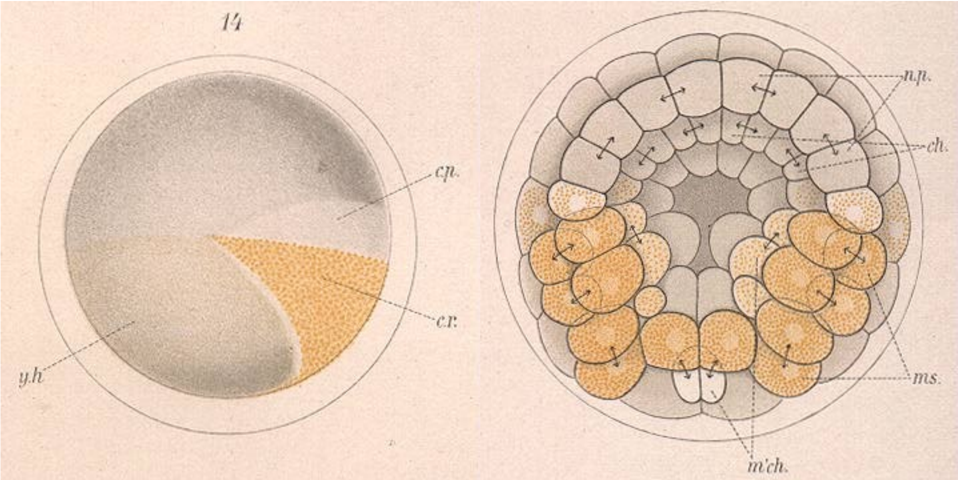
While, as argued above, the parts of embryos, larvae and adult organisms, if they become represented through decompositional contrastive procedures, exemplify in a dense system and afford further decomposition, in short, are thick phenomena, also processes have been classified as thick phenomena in Chapter 3. The following passage from Conklin's work illustrates how the processes that bring about the parts, that is, the structures transforming in time, themselves exemplify in a dense system when individuated by a contrastive procedure, and afford further distinguishing of sub-processes.

> While the divisions of the endoderm and chorda-neural-plate cells are occurring, the most anterior mesoderm cell [...] forming the point of the crescent on each side, divides, the spindle lying in a nearly dorso-ventral direction [...]. **The products of this division are nearly equal in size but are qualitatively dissimilar, the dorsal one [...] containing less of the yellow protoplasm and more yolk than the ventral one [...].** This difference between these daughter cells is plainly visible in the living condition, the dorsal cell being a fainter yellow than the ventral one (figs. 43-48, *et seg*.). [...] This difference in the constitution of these cells corresponds to a difference in their fate; the dorsal cells give rise to mesenchyme, while the ventral ones produce

some of the muscle cells of the tail of the tadpole. (Conklin 1905, 53-54, bold emphasis is mine)

The sentence in bold font shows that the process of cell division is resolved to a level below the cell, describing the distribution of cellular components in terms of more or less. For every sub-process such as cells dividing to give rise to daughter cells it is impossible to say that it belongs to one kind of sub-process rather than the other, because under a dense interpretation, the kinds are so ordered that between every two kinds there is a third with respect to the distribution of components. The system is thus syntactically dense. It is also semantically dense in that, for every sub-process, every manner of distribution of components, it is impossible to decide if it exemplifies one label or the other. The labels that describe the objects are so ordered that between every two there is a third. This is not to say that Conklin does actually resolve the process to an extent that every sub-process or resulting structure is different from every other. He obviously observes a pattern of distribution, and establishes a semantically differentiated scheme, where two distinct types of cells arise, the precursors of muscle cells and of mesenchymal cells. The point is that the system, the experimental as well as the symbolic, which after all is no difference, is in general open to further distinction.

Conklin's figures (see Figure 5.1.), that illustrate the structures in the embryo at different stages and in their spatial succession in the plates as snapshots also the process, in their reporting function must be seen as diagrammatic, but the system of representation is in principle open to differentiation. They operate with shades of yellow and indicate the presence of cellular constituents.



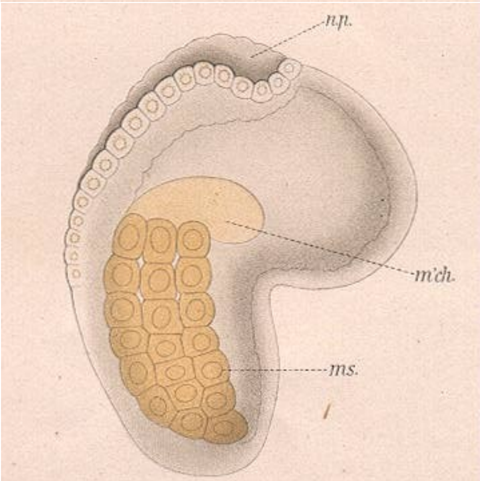


Figure 5.1. Drawings from Conklin 1905, 119 ff., Plate II, Fig. 14 (top, left) Plate IV (top, right), Fig. 48 and Plate V, Fig. 59 (bottom), showing the distribution of yellow pigment in the embryo that Conklin exploits to track cell fates.

To conclude, classical embryology was a science that operated with thick phenomena, concerning the developmental processes as well as the explananda, the resulting parts of organisms. This conception of characters as parts, with its dense systems of representations and its characteristic affordances of further decomposition is still present in developmental biology today, as I will argue below.

Chapter Conclusion

There are indefinitely many ways to decompose and an organism into parts, or to ascribe properties to it or its parts, in short to describe features of organisms. Some of them become recognized and important in some context of human practice and especially in scientific practices. Those might be called characters. In order to become recognized, characters must be represented by some kind of method, that delineates them from the whole organism, and which always includes some form of contrast and sometimes also physical manipulation. The delineated features will usually be put into some theoretical context by emphasizing some of their relational properties, such as their functional roles or similarity relations to other features. The methods and theoretical contexts are always at the end point of some historical tradition and their particular interaction constitutes a disciplinary perspective. I argued for pluralism concerning the way parts are delineated and suggested a symmetrical view of observation and theoretical goals in the sense that neither the contrastive procedures that represent characters, not the theoretical roles they play can be said to have epistemic priority. The ways to make things visible have a life of their own, but nevertheless their choice or a choice among the representations produced, will be guided by the theoretical goals, such as the construction of certain explanatory models or taxonomies.

Furthermore, I have shown that in those disciplinary contexts in which characters are investigated as parts (though the term "character" is not always used in these contexts), they exemplify analogously and afford further decomposition and are thus thick phenomena. One might ask now, whether, if characters in their materiality always exemplify in dense symbol systems, there are situations at all, in which they nevertheless are thin phenomena. What can exemplify in a dense system and can also exemplify in an articulated system, and the interests need not always aim at further decomposition. Thus there are contexts in which characters are thin phenomena. Systematics might be one of them, but I cannot investigate this part of Biology here. Another context in which characters are thin is classical genetics, although, the development of the discipline has to be described as a negotiation of thick and thin character concepts by different practitioners (see below). The illustration of this movement that results in a thin character concept, will centre around Wilhelm Johannsen's writings on heredity. The juxtaposition of the thick notion of characters as parts in embryology and the thin notion of characters as values of variables in genetics will allow me to address some controversial points about modern developmental genetics by way of conclusion.

Chapter 6. Variables and Values: The Phenotype Concept — Negotiating Thick and Thin Characters in Genetics

6.1. From Darwinian to Mendelian Variation

Variation

One of the roles that characters can play in biology is that of being the bearers of differences among members of the same or closely related species. In this case they mostly appear not as parts, but as variables that can take different values. Whether the differences are thought of as being between varieties, races or species depends on how these notions are applied. In any case, differences among closely related groups can also be expressed in terms of parts as we will see, and the variable/value scheme is also applied on higher taxonomic levels. But the thesis of this chapter is that in genetics, which deals with closely related groups, the variable/value scheme became dominant. This is because characters became mere data for genotypes. Also in systematics it seems that in those cases where functional considerations are put aside and characters are treated as pure data for phylogenetic relations the character concept moves from thick parts to thin variable/values, but this discipline and its history will not be covered in this thesis.

Apart from "character-state," which is more related to systematics, another term often used for these different values in characters as variables is "phenotype." It is used in genetics in a similar way to the term "trait." Being a neologism, it has a much shorter and specific history than "character" and "trait." It was introduced by Wilhelm Johannsen (1857-1927) in 1909 in the context of the disciplinary formation of genetics as the science of heredity. However, the more specific context of the introduction of the term is a clarification of the concept of variation as relevant for evolutionary theory. It is important, therefore, to understand what was at stake in debates on variation at Johannsen's time.

Peter Bowler, in tracking the concept of "*Variation from Darwin to the Modern Synthesis*" (2005) remarks that Darwin held the view of his predecessors that heredity and variation are two antagonistic processes. Whereas heredity guaranteed similarity between parents and offspring, variation was thought of as disturbance of the fidelity of the hereditary process caused by environmental influences. Nevertheless, variation was persistent. From this perspective, he did not distinguish between somatic and germinal variation, that is he allowed for acquired characters to be submitted to the offspring, although he thought, informed by breeders experience, that most variation was random, that is, the characters were acquired, but not as directed adaptations (Bowler 2005, 15). He also did not distinguish sharply between trivial, individual variation and large-scale monstrosities, or "sports of nature," but he thought that the latter are rarely beneficial and thus held a mainly gradualist view of evolution (Bowler 2005, 10).

August Weismann famously separated Darwinism from the possibility of the inheritance of acquired characters. The determinants of characters were passed on through the germ-plasm, which gave rise to the soma, but was not effected by it. A variety of determinants for one character were thought to exist in a population that were passed on to the offspring. Selection affected the proportion of determinants in a population. In this way variation and heredity were no longer antagonistic, but heredity maintained variation in a population (Bowler 2005, 21). However, Weismann followed Darwin in considering large-scale variations as unimportant for evolution. Francis Galton shared the new concept of variation as being passed on through heredity, and emphasized that there is a range of values present in a population for a given varying character, following a normal distribution.

Variation thus became a property of populations, instead of referring to independent deviations of individuals. Galton, as opposed to Weismann, thought, however, that even in the case of continuously varying characters, variation could only be evolutionarily relevant as long as it involved saltation, that is a major shift of the statistically normal type around which variation is distributed. Selection on individual variation would only skew the distribution temporarily, but the population would move back to its normal type when the environmental pressure disappears (Bowler 2005, 20-21). Early Mendelians like William

Bateson and Hugo De Vries also emphasized the role of saltations or mutations, but built their view of evolution on the role of discontinuous characters. Their view contrasted with that of biometricians like Pearson and Weldon, who appropriated the Galtonian notion of ranges of variation in normal distribution, but thought of selection as operating on the individual variations, gradually changing the distribution of values for a given character (Bowler 2005, 21). This was the position that Johannsen attacked when he introduced the term "phenotype," thus lining up with the Mendelians. The purpose of the concept was to assimilate continuous characters, the realm of the biometricians, to the discontinuous characters of the Mendelians. At the same time, as we will see, Johannsen distanced himself from the Mendelian character concept.

Following De Vries, in a chapter on genetics in the textbook on *General Botany* co-authored with Eugenius Warming, Johannsen distinguishes five forms of variation (Warming and Johannsen 1909, 632-633, the classification was already in place in the fourth Danish edition from 1900, on which the translation is based):

1) The varieties or polymorphisms within Linnaean, "large," or "good" species, that is, the sub-species or races.

2) The diversity typical for the progeny of hybrids.

3) Individual or fluctuating variability, that is, the (often quantitative) differences between offspring and parents or among siblings.

4) The differences that are (obviously) caused by different environments.

5) The differences that are (obviously) caused by mutations.

The fourth type of variation had not been distinguished by De Vries, who subsumed it under individual variation, which he sharply distinguished from variation due to mutation. Johannsen praised De Vries for separating statistical variation from mutation, but wanted to emphasize that from mere observation it is not always possible to distinguish fluctuation from mutation, especially concerning quantitative traits. Therefore, he classified those cases where differences are *obviously* caused by the environment (such as, for instance, when two plants grow under different light conditions) separately, just as the obvious cases of mutation were listed separately. He then focused on the analysis of individual, fluctuating variation. In this category the cause was not obvious, but it had to be either environmental factors, or mutations. Thus category 3) had to be dissolved into categories 4) and 5). His strategy of pure line breeding that consisted in producing offspring from individual plants by self-fertilization, allowed him to analyze individual variation accordingly (Roll-Hansen 2009, 477-479). Mendelian hybridization experiments instead were designed to account for variations in category 2). Those were the core problems and strategies of heredity[50].

Do Phenotypes Apply to Individuals?

There is dispute among historians concerning the meaning Johannsen gave to the terms "phenotype" and "genotype." Frederick Churchill (1974, 13) claims that Johannsen first uses the terms predominantly in a statistical sense and claims that this implies that populations have phenotypes, but not individuals. He then observes that Johannsen later in his life, in the light of the success of the chromosomal theory of heredity as advanced by the Morgan school, came to an understanding of individual phenotypes and genotypes. Nils Roll-Hansen (2009, 489) instead maintains that Johannsen from the beginning was interested in the hereditary types of individuals. This follows, according to Roll-Hansen, from Johannsen's application of the pedigree method (pure line breeding as Johannsen calls it), developed by the French breeder Louis de Vilmorin in the mid-nineteenth century, which focused on the selection of individuals in breeding instead of selecting large numbers of individuals exhibiting the desired traits, a strategy that Johannsen also knew from Microbiology (see also Gayon and Zallen 1998; Müller-Wille 2007a).

It seems inappropriate to dismiss those instances in the first edition of *Elemente der Exakten Erblichkeitslehre* (Johannsen 1909) where Johannsen uses "phenotype" as applying to individuals, just because these instances are "not common and they do not fit in

[50] Variation in the sense of Category 1) instead were the data of systematics.

with the general thrust of Johannsen's text" (Churchill 1974, 13). Instead one should ask under what interpretation the apparently different uses go together. I will show that the different occurrences of the term in the *Elemente* stand in a systematic relation that mirrors Johannsen's view on heredity.

While it is misleading to say that for Johannsen populations have a (or are of a) phenotype, but not individuals, it is true that phenotype is a statistical concept. It is defined as the mean value concerning a given variable in a studied population, the centre in a binomial distribution, around which variation occurs:

> Such Phenotypes are in and for themselves measurable realities: exactly what can be observed as typical; thus in variation distributions the centers about which the variants group themselves (Johannsen 1909, 123).[51]

Of course, one has to observe at least two organisms — and should observe much more — to arrive at a mean value concerning a quantity. In this sense only populations have a phenotype in that they possess a mean value. But this concerns just the way to construct the particular phenotype category. Once it is available it is possible to say of one individual in the population that it falls under the category, or that it has (or is of) that phenotype.[52] One might also say that every individual in this population has the phenotype and in this sense the population has the phenotype. Both formulations — the population has a phenotype in that it has a mean value for a quantity, and the population has a phenotype in that all of its members have the phenotype — are distinct. But both are not at odds with the statement that an individual has a phenotype in that it falls under the phenotype category,

[51] The translation follows Churchill's, except that he uses "characteristic" where I chose "typical." The original reads: "Solche Phaenotypen sind an und für sich meßbare Realitäten: eben was als typisch beobachtet werden kann; also bei Variationsreihen die Zentren, um welche die Varianten sich gruppieren."

[52] Usually, one would say that a token is of a certain type or belongs to a certain type. Here the question is whether the population or the individual is the token that is of a certain type. But since the type is defined by a property, I will say that the population or the individual has the phenotype, in the sense that one can say that if a token is of the red type, it has the property red.

which means that it is part of the population with that particular mean value for the quantity in question. The criteria for delineating the population of which the mean value is determined, and for counting the individuals in, can be different in different situations. Johannsen often starts from a population defined by all individuals on a field or from one package of seed and moves on to populations defined as generations of offspring of one (self-fertilised) individual or parental pair of organisms.

Mendel's Character Categories

The notion of phenotype as a constructed category can best be illustrated with qualitative characters of the type that Gregor Mendel (1822-1884) used in his experiments, and that were the model for Johannsen's approach to quantitative traits. Phenotype is a concept that allows to treat quantitative characters just as qualitative ones. Johannsen explicitly says that there are also qualitative phenotypes:

> [Phenotypes of qualitative nature] are almost always obvious, and it can be seen in any given individual without problems whether it belongs to one or the other qualitatively different phenotype. There are, for instance, brown, blue, yellow, violet etc., and furthermore differently marbled beans; and from a mixture one can easily sort the individuals according to such color- or pattern-types. Here the phenotypes are qualitatively different and easily recognizable (Johannsen 1909, 129, all translations of Johannsen's texts are mine, unless stated otherwise).

Here a phenotype such as brown color of beans obviously applies to individuals. A given individual *is* brown. One might also say of a population of brown beans that the phenotype applies to this population, without there being a conflict. But what about the statistical nature of the phenotype concept in this case? Note that brown is a "color-type." The point is that labels such as "brown" have individuals in their extension that show much variation in color. Thus the difference between qualitative and quantitative variation is not so sharp after all, but rather a matter of the scheme that is applied to organize a realm.

When Mendel first introduces the characters (*Merkmale*) he uses in his experiments the descriptions are quite rich. Here is the second and third of seven characters he investigates:

> 2. The *difference in the color of the seed albumen (endosperm).* The albumen of the ripe seeds is either pale yellow, bright yellow and orange colored, or it possesses a more or less intense green tint.
>
> 3. The *difference in the color of the seed-coat.* This is either white, with which character white flowers are constantly correlated; or it is grey, grey-brown, leather-brown, with or without violet spotting, in which case the color of the standards is violet, that of the wings purple, and the stem in the axils of the leaves is of a reddish tint. The grey seed-coats become dark brown in boiling water. (Mendel 1902, 45-46)

A character, for Mendel, is the differential value of a variable, which is a property of a part (the expression in italics). The part (e.g. seed albumen) is a category from plant anatomy. The property (color) is perceptually salient. I treat those categories (seed albumen, color) as given for the present purpose, but it should be kept in mind that not only their choice, but their individuation or definition might be effected by the context. Here I am concerned with the delimitation of the values. The actual appearance of the albumen of individual plants obviously showed some variation (from pale yellow to orange). But Mendel knew from his pre-experiments, which consisted in pure-line breeding, that offspring from plants with yellow albumen showed the same variability in albumen color (from yellow to orange) as did offspring from orange plants. These characters bred true, or, to put it differently, they did not segregate. And none of the plants exhibiting them ever produced offspring with green albumen (or any other color). Plants with green albumen showed variation as well ("more or less intense green tint") in their offspring, but never produced plants with yellow or orange albumen. Yellow-to-orange and more-or-less-intense-green where thus "constant differentiating characters" as Mendel calls them (Mendel 1902, 42). Realizing the importance of focusing on plants differing in singular characters (as opposed to overall appearance) has often been emphasized as a major achievement of Mendel (e.g. Müller-Wille and Orel 2007, 194). But an often-overlooked step in Mendel's approach is to define the two characters that are contrasted, such that they are constant. The characters *qua* values are delimited according to the criterion of true breeding. This criterion justifies to lump together a great variety of colors under one category.

Robert Scott Root-Bernstein (1983) shows that Mendel put considerable effort in constructing discrete character categories, which have to be seen as idealizations in the sense that scientific models always operate with idealizations. He diagnoses a difference between a biological and a statistical approach to characters, where Darwin's work is an example of the former, emphasizing the continuous differences between individuals with respect to single characters, while Mendel, the statistician, was looking for *kinds* of characters. This terminology is misleading, though, because Mendel also applied biological criteria (breeding true) and his approach must not be confused with that of the biometricians, who where gradualists. According to the analytic tools recruited in Part I of this thesis, it is possible to interpret the situation as follows. While gradualists applied a semantically dense scheme to variations in a given property (variable), Mendel applied a differentiated scheme. Individuals, accordingly, fall in groups that share an idealized character, or, in terms of symbol systems, they fall in one range of extension or appliance class. Mendel can be said to have introduced this approach in the study of variation among closely related organisms.[53] To be more precise, with respect to characters as variables (albumen color, seed size), individuals share characters on both accounts. Schemes of labels for variables are differentiated.[54] But according to the Darwinian or biometric account characters as values (yellow, 12,3mm) are not shared, the scheme is dense. They are individual appearances (this particular shade of yellow, this particular shade of orange),

[53] It must be said, though, that this notion of characters certainly existed with respect to higher taxonomic relations.

[54] But again, also variables have to be delimited. E.g., where does color end and brightness begin in the perception of a surface?

only constraint in the researchers ability to measure differences. For Mendel the values come in discrete categories (a category called “yellow” covers all shades from yellow to orange).

If we look at the description of characters in the above quotation, it seems that language is used as a dense symbol system and the organism affords ever more fine-grained descriptions. Characters are thick phenomena. But then Mendel applies his two factorial model, which requires the application of a differentiated scheme. Under this system, characters afford different sorting and grouping. But such a transformation of a dense system to an articulated one requires decisions and a process of adjustment to make the categories fit with the model.

With reference to J. Heimans’ (1971) analysis of a preserved note of Mendel, Root-Bernstein shows how Mendel carefully constructed these character value categories. It appears as if Mendel, with respect to the third character (color of the seed coat), has started with the assumption that there are six categories: white, violet, light violet, dark brown, light brown and violet brown, and, with an intermediary step supposing five categories, finally came up with a scheme that had three labels, “white,” “violet” and “brown,” that subsumed the different shades of violet and brown. He thus delimited the character categories from a (though already partly fragmented) continuum such that they fit the predictions of his two-factorial model. In this sense it is true that Mendel did not arrive at his model by induction, but that his observation was driven by theory (Root-Bernstein 1983, 288), but only in the sense that the theory suggested an interpretation of the observations, it suggested a scheme. On the other hand this does not exclude that he was led to his model by observation. Even if here function plays no role, the situation fits the analysis of Laubichler and Wagner discussed above. Observation as material practice has a life of its own, but the model drives the determination of character categories. We do not have enough information on Mendel’s reasoning to finally answer these questions of priority, but this seems to be an occasion where the model and the entities that figure in it are fixed by mutual adjustment.[55] The important point is that Mendel thought of characters as categories that partition individual variation in discrete chunks. Accordingly, when he proceeded he omitted the rich description and spoke, for instance, of the second set of characters as “yellow” and “green,” but these were only names he gave to the category, which still subsumes different shades of yellow and orange, or green, respectively.

Mendel chose only those character variables for his experiments where the constant differentiating characters were easily distinguishable:

> The various forms of Peas selected for crossing showed differences in length and color of the stem; in the size and form of the leaves; in the position, color, size of the flowers; in the length of the flower stalk; in the color, form, and size of the pods; in the form and size of the seeds; and in the color of the seed-coats and of the albumen [cotyledons]. Some of the characters noted do not permit of a sharp and certain separation, since the difference is of a "more or less" nature, which is often difficult to define. Such characters could not be utilized for the separate experiments; these could only be applied to characters, which stand out clearly and definitely in the plants. Lastly, the result must show whether they, in their entirety, observe a regular behavior in their hybrid unions, and whether from these facts any conclusion can be reached regarding those characters which possess a subordinate significance in the type (Mendel 1902, 45).

Here Mendel makes clear that also the delineation of the easily separable characters is evaluated by their behavior in crosses. If they do not conform to the laws then they are not the right categories in this context. If they do, then they are typical for that kind of plant. But also the less clearly separated and thus less typical characters can be evaluated according to their behavior in crosses.

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[55] Sara Schwartz (2002) gives a similar example from Bateson’s work, which she describes as “mutual adjustment” of the model and the entities that figure in it.

Even in the case of characters that allow for a sharp and certain separation, there are individual plants that do not fit easily in one of the categories.

> In counting the seeds, also, especially in Expt. 2, some care is requisite, since in some of the seeds of many plants the green color of the albumen is less developed, and at first maybe easily overlooked. The cause of this partial disappearance of the green coloring has no connection with the hybrid-character of the plants, as it likewise occurs in the parental variety. This peculiarity is also confined to the individual and is not inherited by the offspring. In luxuriant plants this appearance was frequently noted. Seeds, which are damaged by insects during their development, often vary in color and form, but with a little practice in sorting, errors are easily avoided (Mendel 1902, 53).

Root-Bernstein, answering to the long standing debate on whether Mendel's results were "too good" from a statistical point of view, argues that Mendel sorted these indefinable items according to the expectation. This does not qualify as fraud, because it is part of the very common process of fitting the data to the model. Characters are discrete values of variables in the model and as such idealizations (Root-Bernstein 1983, 289).

Johannsen's Phenotype: Mendelizing Continuous Variation

Whereas the existence of Mendelian characters, that is qualitative characters that are easily separated and that segregate, were a threat to the Darwinian-biometricians' view of selection operating on individual variation, it seemed that those qualitative characters were still rare phenomena, and as we saw, they were not so discontinuous after all. Most of the characters still seemed to be continuous. Johannsen's point in the first edition of the *Elemente* (1909) was to treat quantitative (continuous) traits just as qualitative traits as they are studied in Mendelism. His pure line research (1903) was the experimental basis of this strategy. We can now see how the phenotype definition is tailored for this purpose.

The discussion of Mendel's characters showed that there is no sharp distinction between qualitative and quantitative properties. Colors appear as qualitative characters when the constant differentiated characters (character categories) are clearly separated by gaps in the continuum, but otherwise are continuous. If they are differentiated, a differentiated scheme is applied and characters fall in the mutually exclusive ranges of extension of one of the labels in the scheme. Even height, which is almost the standard example for quantitative characters, can be treated as qualitative if long and short forms are easily separated like in the case of Mendel's seventh trait:

> The *difference in the length of the stem.* The length of the stem is very various in some forms; it is, however, a constant character for each, in so far that healthy plants, grown in the same soil, are only subject to unimportant variations in this character. In experiments with this character, in order to be able to discriminate with certainty, the long axis of 6 to 7 ft. was always crossed with the short one of 3/4 ft. to 1 and 1/2 ft. (Mendel 1902, 46-47)

"Long" and "short" are categories of character values that subsume variations (6 to 7 ft.) that are in this context "unimportant." The delimitation of characters is determined by the context, that is the interest and method. Here, the criterion for characterhood is to breed true under similar conditions.

The difference between qualitative and quantitative characters is thus just an epistemic matter. If there are two forms, say long and short, but they overlap — the longest of the short form are as long as the shortest of the long form — the researcher cannot distinguish them. Thus the case is treated as quantitative or continuous variation. If the forms are clearly separated through a gap in the continuum as in Mendel's seventh character, they appear as qualitative or discontinuous characters. But even if a gap in the continuum was necessary for Mendel in order to recognize qualitative traits, it is the treatment of the separated ranges as categories that makes them qualitative traits. Otherwise they would be continuous fragments of a range of values. On the other hand, once the notion of character categories is accepted, even uninterrupted continua can be broken down into discrete character categories. That is exactly Johannsen's goal when he introduces the phenotype concept. The pure line analysis is a way to find out if an individual, which has a certain value for a variable that is covered by both categories, falls under one or the other category.

If Johannsen approaches a population (*Bestand*) of beans (which are self-fertilizing), with an interest in a quantitative property such as seed length, he starts with an assumption of unity of type. And just as in the case of qualitative properties, he glosses over the individual differences, and defines the phenotype as the most typical, that is the mean value ("what can be observed as typical; thus in variation distributions the centres about which the variants group themselves"). If the mean value of the seed length is 14,1mm, the population has the phenotype 14,1mm[56]. It sounds odd however, to say that the population has this phenotype in that all its members have that phenotype, or to say of a particular individual plant, which has an actual seed length of 12,2mm that it has the phenotype "seed length 14,1mm." But in fact the case is not different from that of Mendel's characters, where he called the orange albumen "yellow," or seeds with light depressions "round." And the oddness goes away when we give a different name to the category. We can call the phenotype — defined as 14,1mm — simply "M," and say that all members of the population have the phenotype "M". The individual that has a seed length of 12,2mm still has the phenotype "M," just as the plant with orange albumen has the phenotype "yellow."

It becomes even less odd when the name is contrastive. If the population is subjected to a pure line analysis, and it is found that some plants give rise to offspring generations that has a mean seed length (that is phenotype) of 12,8mm and some give rise to a generation with a mean seed length (that is phenotype) of 15,1mm, then there will be two phenotypes.

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[56] The example is taken from Johannsen (1909, 122). In Johannsen's example the measurement procedure divides continuum segments of 1mm, so that every individual has a class value such as 12-13mm. I will pretend the seeds were measured to a tenth of a millimetre and I will round up the mean values. It has also to be noted that seed length is a property of the individual plant that grows from the seed, not of the plant that brings the seeds about, because the seed is the embryo.

Even if they are defined by the mean value, they might be designated as "short" and "long." To say that all individuals of the population consisting of the offspring of one bean have the phenotype "short," or to say of one individual that has an actual seed length of 10,7mm that it has the phenotype "short," does not sound odd at all.[57] And this is exactly the way Johannsen uses the term. The following passage, where Johannsen discusses some data from Bateson's *Materials for the Study of Variation* (1894) illustrates Johannsen's use of the term "phenotype." The distribution of the length of forceps in a population of earwigs shows two clearly separated peaks:

> As the distribution shows itself, divided in two almost independent parts, two phenotypes become obvious: Males with short — about 3,5mm on average — forceps and males with long forceps, about 7mm in the mean. Around both of the phenotypes variation of a very common nature can be observed (Johannsen 1909, 208).

Churchill is right; the phenotype "forceps length with a mean value of 3,5" is derived from a population. Its formulation depends on the observation of a population. But this does not keep the phenotype thus defined from being applied to individuals. Johannsen speaks of individuals here. There are male earwigs with "short forceps," where short is specified as on average 3,5mm long. To define the phenotype as the mean value is just a way to produce a robust and exact category.[58]

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[57] Not speaking of properties, but of types, one can also say, more correctly, that the individual is a token of the short type, and accordingly, that it is a token of the 12,8mm type, even if it has a seed length of 10,7mm.

[58] If one would define the phenotype as the range such that short would be specified as "between 3mm and 4,5mm," it would be less odd to apply the phenotype to an individual: "*A* has a forceps length between x and y," sounds unproblematic. But in this case a single individual would have too much influence on the construction of the phenotype category. An outlier of 2mm length, for instance, would change the definition of the phenotype, while it would not have much influence on the mean value if the sample size is big enough.

From the above said, it follows that even if every population has a phenotype in the sense of a typical value, Johannsen seems to imply that there are tentative phenotypes and phenotypes proper. The tentative phenotype would be the mean value concerning a given property in any population that happens to come under investigation (say, a package of seeds). The phenotype proper is the constant phenotype that is determined in a pure line breeding experiment; they are phenotypes of very specific populations, that is, generations. These are quantitative characters treated as Mendelian characters and they apply to individuals.

If we look again at the section chosen by Churchill (1974, 13, fn. 23) to show that Johannsen sometimes speaks of the phenotype of individuals, and of which he claims that these passages "do not fit with the general thrust" of the text, we can now see that it fits very well into the picture developed above:

> Especially here [in the context of quantitative properties] where it is not possible to recognise directly in every individual its phenotype, the difficulties for research were the largest and the sources of mistake were the most abundant (Johannsen 1909, 130-131).

One cannot read the mean value of the population the individual belongs to (or gives rise to) off the individual, but once we know the phenotype, the individual can be said to have the phenotype.

Phenotypes, Personal Characters and the Environment

We saw that it is wrong that the phenotype does not apply to individuals, as Churchill claims, if that means that it cannot be predicated about individuals. It can. Another thing is to observe that the non-idealized value of a character variable, that is, a value in a dense scheme, as exhibited by an individual is not called a phenotype. Johannsen indeed does not use the term in this sense in the first edition of the *Elemente*. Instead he speaks of "personal properties."[59] If an individual plant of a pure line has a seed length of 13,5mm and is part of a population of siblings that has a mean value for seed length of 12,8mm (and also the population consisting of its offspring will have roughly the same mean value), then it has the phenotype "short," specified by the mean value 12,8mm, but it does not have the phenotype "seed length 13,5mm." This particular value is a personal property of the individual.

It is not the case that the phenotype has to be separated from the genotype, because the phenotype is influenced by the environment, while the genotype is not. As a matter of fact the phenotype as typical (in the sense of the mean value of a property such as seed length or in the sense of the "yellow albumen") is construed in a way that it subsumes all environmentally induced variation under one type. The phenotype is exactly the property defined broadly enough to be robust against environmental influences. "Therefore, a statistical treatment of the observed groups becomes necessary, such that the more random influences of the local fluctuations in the environment can be eliminated." (Johannsen 1909, 101) The personal character, as opposed to the phenotype, is the result of genetic and environmental influences:

> The personal character of an individual is not only determined by its genes, which the gametes bring together in the zygote. Also the whole living conditions, the environment (the sum of all states that have an influence on development, all "factors of the surrounding"), plays a significant role (Johannsen 1909, 131).

The phenotype instead is only differentially influenced by the environment, if two populations of the same genotype are influenced by different environments, e.g. different fields, or different climatic factors in subsequent years, and thus have different mean values for a property (Johannsen sometimes speaks of "year-phenotypes,"1909, 216).

[59] "Persönliche Beschaffenheit," (Johannsen 1909, 131) "persönliche Abweichung," (p. 113) "persönlicher Charakter," (p. 167) or "persönliche Einzeleigenschaften" (p. 243).

The concept of a phenotype, as determined in pure line breeding, was not meant to account for the fact that the environment had an influence on the individual appearance and that therefore individuals with the same genotype had different phenotypes. It showed that the differences caused by the environment did not change the phenotype and therefore only changes in genotype could change the phenotype in an evolutionary relevant way.

The genotype-phenotype distinction was devised to cut off the characters of the parents and ancestors from the constitution of the zygote. Johannsen wanted to cut off the Darwinian-biometrician tradition that saw individual variation as the source of evolution, the material on which selection acted. He expressed this by cutting off a syllable from a word used by De Vries with reference to Darwin's pangenesis conception of heredity, to indicate the influence of the whole body on the formation of the gametes: Johannsen's critique culminated in cutting away the "pan" from "pangenes" and left us with the gene as an element of the zygote that comes from the gametes of the parents not from their body. The characters of the parents do not influence the gametes and the resulting zygote, and thus are not transmitted to the offspring. Instead the same genes influence the characters of the parents and offspring.

Accordingly, the purpose of the genotype-phenotype distinction was not to criticise preformationism as Lenny Moss claims:

> Johannsen coined the terms genotype and phenotype and distinguished between them in order to depart from this morphological (preformationist) legacy and establish, he felt, the grounds for a proper science of the inheritance of the genotype. It is precisely the conflation of the phenotype — a product of environmental-developmental interaction — with the inheritance of Mendelian units, which constituted a new brand of preformationism. (Moss 2003, 29)

As we saw, Johannsen did not think of the phenotype (the mean value of a variable) as being "a product of environmental-developmental interaction," but as being robust to environmental influences. Only personal characters were the result of such an interaction.

Johannsen's contemporaries were in a way right when they thought that his distinction served the same purpose as Weismann's germ-plasm/soma distinction. Johannsen explicitly credits Weismann for attacking the idea of the transmission of characters. Then, as Moss rightly says, Johannsen distanced himself from Weismann and his terminology because of Weismann's preformationism that Johannsen disagreed with. But this difference in their views is logically independent from the difference between their shared views and the biometricians' view. And making change in continuous characters dependent on saltation was actually very welcome for preformationism.

Accordingly, the genotype-phenotype distinction was not a device to overcome preformationism. As will be shown in the next section, Johannsen did that by employing several conceptual means: He made another distinction, namely that between characters as parts and characters as properties (values of variables), he stated a many-many relation between genes and characters, and a form of the difference principle (the latter two points are usually attributed to Morgan), and he made a further distinction between superficial characters and more fundamental features of organisms. Finally, he developed an epigenetic concept of development, which he formulated in the chemical idiom of reaction. The role of the environment was not important for the critique of preformationism. Even if the influence of the environment is acknowledged by preformationists (that the environment modifies the unfolding of the preformed character, a view that Johannsen grants Weismann, 1909, 320), the problem for Johannsen is the one to one relationship between genes and characters. Expressions such as "new preformationism," but also "genetic determinism" often do not distinguish between cases where genes are thought of as being related to particular characters and cases where the influence of the environment is neglected. Johannsen was clearly more concerned with the former case.

6.2. From Morphological Unit-Characters to Characters as Values

Johannsen's Critique of the Morphological Unit-Character Concept

Already in the first edition of the *Elemente*, Johannsen formulates his critique of morphological unit-characters. He cites Weismann and Bateson as allies in the rejection of the notion that characters are passed on from parents to offspring, that was implied in Darwin's "pangenes" and the biometricians' view that selection acted on individual variation. He acknowledges the value of the germ-plasm/soma distinction in this respect, but he strongly disagrees with Weismann's speculations concerning the nature of the components of the germ-plasm as small independent units that determine whole body parts:

> That Weismann's doctrine of the "determinants" is also wrong in that the "determinants" affect organs or tissue regions, becomes clear from the behavior of hybrid offspring; accordingly in this lectures we spoke of properties, with which genetics has to operate — whether these properties are local or distributed in the organism (Johannsen 1909, 323).

Apart from the fact that Johannsen tries to avoid any speculation on the nature of, what he would call genotype, — he introduces the term "gene" to have a term that is free of any previous speculations, — he relates genes to properties not parts. This is the way Mendel and Bateson perceive characters as determined by factors, and what the latter called "unit-characters". Johannsen endorses this view. He himself makes use of a quasi preformationist language of the unit-character when he says:

> The single gamete contains special, separable "genes" for different properties. The gametes of *Lychnis diurna* e.g. contain genes for hair generation, genes for red coloring, genes for chlorophyll generation etc. Every property that has a special gene (a gene of a special type) at its basis can be called a singular property. It is a matter of research to determine in every case, what is a singular

> character in this sense. Crossing experiments are an important procedure here. (Johannsen 1909, 125)

The term “phenotype” was introduced to apply this Mendelian notion of character that can be delineated through pure line and hybridization analysis to quantitative characters (and thus make changes in quantitative characters require changes in genes).

But at the same time he expresses doubts about the unit-character concept. Johannsen speaks of “single properties” (*Einzeleigenschaften*) as opposed to the whole organism. Bateson’s unit-character he interprets as a “simple” (*einfach*) property (Johannsen 1909, 389). While characters, observed as segregating in hybridization experiments are single characters (what is different when the rest of the whole organism is similar), Johannsen has doubts about the simplicity of characters, if this is to mean that they are influenced by only one gene.

When Johannsen discusses Bateson’s research on the combs of fowl hybrids, he emphasizes the relativity of genetic analysis. Bateson describes a certain form of comb (*walnut*) as a compound character, because it appears only if two factors are present, which in the absence of the respective other produce different characters (*pea* and *rose*). If both factors are absent, the property of the comb is described as *single*. If characters in the sense of Mendelian genetics are represented by one factor, *walnut* is not a proper character, but composed of two characters. Johannsen observes that this depends on the depth of analysis. If *walnut* would be crossed with only *pea* or *rose*, it would appear as a dominant unit-character. Only if crossed with *single*, the compound nature becomes apparent. But in the same way also *pea* and *rose* can be regarded as compound characters if it is assumed that there is a factor corresponding to *single*. If *pea* and *rose* appear as unit-characters, the only reason is that no mutation is known that allows for further analysis. The unit-character is an illusion that stems from the relativity of observation: “What appears in one cross as “single [read: simple] property” (here *walnut* — e.g. versus *rose* or versus *pea)* can turn out to be a complex matter. The relativity of hybrid crosses thereby becomes obvious." (Johannsen 1909, 390)

Thus Johannsen comes to the conclusion that every character is in the end influenced by many genes. The fact that one gene affects many characters was already mentioned by Mendel himself, when he stated that the difference in seed coat color is correlated with differences in many properties such as flower or stem color. Johannsen accordingly clearly states the many-many relation between genes and traits.

> What appears in segregation phenomena as one unit, can affect a whole series of properties; and conversely, a seemingly simple property, e.g. red color of sap in *Matthiola* require two different genes under given circumstances, or, as in the case of felted character of the mentioned plants, the simultaneous presence of three genes (Johannsen 1909, 483).

Bateson's version of the unit-character is closely linked to his presence-absence hypothesis, according to which in any pair of alternative segregating characters the dominant form is regarded a property present in the organism, whereas the recessive form is simply interpreted as the absence of the same property, and accordingly, the factor is present or absent in the zygote. Johannsen instead, sees any character, part or property, as the result of the (interdependent) activity of many genes. The whole organism, with all its parts that one might anatomically dissect, is a result of a reaction of the whole genotype with the whole environment. The alternative characters of genetics, which should be seen as properties *of* parts, indicate specific differences in the reaction, they can be singled out, but they are not simple. They are different outcomes of the reaction that reproduce under similar environmental conditions and that indicate different starting conditions, that is, differences in the composition of the zygotes. Johannsen calls the characters of the geneticists "differing points." He thereby comes very close to formulating the difference principle that is worked out in a much clearer from by Thomas Hunt Morgan (1866-1945) and co-workers in *The Mechanism of Mendelian Heredity* (1915). There are several places in the *Elemente,* where Johannsen makes the point, but his formulation in his first English language publication from 1911 is probably most telling in this respect:

> It may be quite impossible to indicate whether a particular reaction (character) is due to something positive or to the lack of a factor in the genotypic constitution. All that can as yet be determined in this regard by Mendelian analysis is the *number of differing points* between the two gametes forming a heterozygote. Such differences maybe termed "*geno-differences*." The well-known facts, that a "character" maybe dominant in some hybrids but recessive in others, and that segregation in different cases maybe very different, indicate that "characters" are complicated reactions. (Johannsen 1911, 148-149)

While the elements that compose the zygote can be independently recombined, characters as parts, are (genetically and developmentally speaking) not such independent bits, but outcomes of the reaction as a whole. Differences in the outcome, that is, differential properties of parts allow to track zygotic constituents.

Bateson instead interprets the notion of composition of characters quite literally and describes the morphology of the *walnut* comb in terms of the morphology of the *rose* and *pea* combs (that is, as combining papillosity and ridgedness which morphologically characterize *rose* and *pea*, respectively, where *single* is characterized as a comb showing none of these features). Therefore, Johannsen, later in his life, comes to include Bateson in the morphological tradition that first included the Darwininan scholars that saw selection operating on individual variation, then Weismann for thinking of germ-plasm constituents as determining whole body parts (tissue regions), and now Bateson for conceiving of characters as morphological properties, a view that still had single factors determining parts. One can also say that Johannsen constantly rejected a thick interpretation of characters when it comes to heredity, while he promotes a thick character concept as the proper explanandum of development.

By the time of the Johannsen's second English language publication in 1923 and the third edition of the *Elemente* (1926), Mendelism had succeeded and the Darwinian-biometric notion of individual variation that is transmitted to the offspring, was generally regarded as defeated, at least within genetics (though, of course, not biometrics as a method). Now Johannsen put much more emphasis on criticising character-preformationism (the view that one factor determined one character, where the character is understood as a part). He sees himself strongly supported, among other things, by the multifactorial hypothesis of Nilsson-Ehle[60], which states that many genes have the same effect on certain properties (as opposed to the compound case where many genes affected the same property differently). The one to one relation between genes and characters could not be maintained and it generally was not supported any more at this time. Again Johannsen cut off a tradition. The tradition of the morphological unit-character, comprising among others Weismann, Mendel, Bateson and De Vries.[61] While earlier he had cut off genes from characters in the production of the gametes, he now cut off the genes from characters on the side of the development of the zygote. He again signaled the intellectual step by cutting of a syllable: he suggests — successfully — to eliminate the "morph" from Bateson's term "allelomorph," leaving us with the alleles as the alternative forms of a gene, that do not determine morphological properties, but are indicated by *differences* in properties of organismic parts.

Johannsen introduced the term "phenotype", in an attempt to construct quantitative characters as Mendelian unit-characters, that is, as idealized property categories. The phenotype was therefore sharply separated from the individual expression of the character, the exact value measured, or "personal property." At the same time he had doubts about the one to one relation between genes (factors) and characters implied in the unit-character

[60] These results did not enter into the first edition of the *Elemente*, because they were published in the years from 1909 to 1913.

[61] I described Mendel's characters as abstract values as opposed to morphological properties, but at least in the influential Batesonian interpretation they appeared as morphological properties.

concept. Later, when his focus was more on the critique of the unit-character, in this sense, especially the morphological understanding of characters that was attached to it, he also used the term “phenotype” for personal properties:

> The word phenotype, however, is not only applied to statistically ascertained 'typical' averages but can simply be used as a designation of personal characters of any individual whatever. The phenotype of an individual thus comprises all of his expressed characters. The single organism, the individual plant, an animal, a man, — “What it is and what it does” — has its phenotype, i.e., it appears as a sum of traits which are determined by the interplay between “inherited Anlagen” and elements of the environment (Johannsen 1926, 163, modified translation from Churchill).

Churchill argued that Johannsen did not apply “phenotype” to individuals in 1909, but did so in 1926, citing this passage. But we have to distinguish between applying the term to individuals by sorting them into idealized character categories (e.g. Mendel’s *yellow*) and applying it to personal properties of individuals (e.g. the particular shade of orange a plant might exhibit). I showed that Johannsen did apply the term in the idealized category sense to individuals in 1909 and here we see that he used it for personal properties in 1926. But this cannot be the change that Churchill diagnoses, because he says Johannsen adjusts his use to the Morgan school’s use of individual characters. But these are idealized characters of individuals, as I will show below. If a fly has the phenotype *vestigal*, then it has short wings. There will certainly be differences in wing length among *vestigal* individuals. But no individual has a phenotype given by the exact length of its wings, but only *vestigal* or *normal*, which covers a whole range of possible lengths. (Though at first the Morgan school does not make much use of the actual term “phenotype,” at all, but speaks of characters or mutations.) When Johannsen in the passage above includes “personal properties” in the phenotype definition, this is because he speaks about the phenotype as the outcome of the reaction of the genotype with the environment, that is, as the outcome of development. This becomes obvious from the holistic perspective, because what is the more striking change in this amended phenotype definition is that Johannsen uses "phenotype" for the whole of the organism, comprising all characters, instead of single properties.

What we can observe is that Johannsen who criticized "simple properties before," now even has reservations about "single properties." This becomes apparent in the fact that when Johannsen — himself never shy to introduce new terms — rejects the possibility of using the term "phene" with a "*sit venia verbo*." (Johannsen 1926, 165) He does not deny that the organism can be dissected descriptively in many ways, into single morphologically or physiologically characterized parts or properties, but at the same time he emphasizes that in its development, as well as in its physiological function, the organism acts as a whole. The development of the parts in the context of the whole is a matter for developmental physiology (*Entwicklungsphysiologe*). As his discussion of Valentin Haecker's (1918) *Entwicklungsgeschichtliche Eigenschaftsanalyse* (*Phänogenetik)* (Haecker's term for developmental studies) makes clear he sees developmental physiology, studying the development of the organism with all its parts and properties from the zygote, as a separated field of research from genetics, which moved from pheno-analysis (Mendelism) to geno-analysis (what we might call Morganism). But the latter still relies on single phenotypic differences, the heuristic value of which Johannsen never denies (Johannsen 1926, 519-520). Only if the phenotype is thought of as the result of development, the individuation of individual units (characters as parts) becomes problematic. If "phenotype" is used for difference points as the data for geneticists, the difference between "phenotype" designating the whole organism or a single property becomes irrelevant, because the difference point is ideally the one point in which two organisms as a whole differ and to speak of "phenes" would give the property identified through the difference too much independence. We can see here already that there are two notions of phenotype, the value of a variable (the difference point against the background of otherwise, at least ideally, identical organisms) as data for the geneticist, and the organism as a whole, that can be dissected into morphological parts for many purposes, but whose parts do not develop independently from each other.

Johannsen's Chemical Metaphors

The impact of Johannsen's training as a pharmacist and the chemical knowledge he gained from this education on his thinking have often been mentioned (Churchill 1974; Müller-Wille 2007a; Roll-Hansen 2009). Two notions are central to his thinking, which we can understand as being imported to biology from chemistry: Analysis and reaction. Johannsen often speaks of the type of investigation the geneticist performs as analysis: "The variations can only be analysed through the hereditary ratios!" (Johannsen 1909, 440) This is the credo of the *Elemente*. Organisms are compounds that result from the reaction of the genotype with the environment. If a chemical compound is analysed, it is usually subjected to a test where it is brought in contact with another chemical element or compound or physical environments like heat to see what reactions take place. A particular reaction, for instance, a color in a flame test, can be ambiguous, because more than one metal can produce the same flame color. But several tests can narrow down the possibilities, so that as a result of a series of tests the elements that reacted to form the original compound are known. Similarly, an organism is subjected to tests in the form of self-fertilization or cross-fertilization and the result of the reaction is observed in form of the phenotypic make up of the offspring generation. A single test might be inconclusive, but if the appropriate series of tests is performed, the genotypic elements that reacted with the environment to form the organism are known. "Analysis" is an epistemic term; it is a procedure, a series of tests that result in a certain type of knowledge. "Reaction" instead is an ontological term; it designates an occurring process. But in the chemical example there are two kinds of reactions, which are ontologically the same, but epistemologically different: The reaction that gave rise to the compound of interest and the reaction that is part of a test to determine the elements that took part in the first reaction. In the case of biology the compound of interest is the organism. The reaction of the genotype with the environment that gave rise to the organism is development. The results of the test reactions are the phenotypes observed in the breeding experiments. Thus the phenotype of the organism resulting from the reaction of the genotype with the environment, we can call it phenotype-D(evelopment) (the whole organism, that has morphological parts, which do not develop independently), is epistemologically different from the phenotype as observed in breeding experiments (difference point), the phenotype-G(ene), that indicates a genotypic difference. They are of course both the result of development, just as the test reaction and the original reaction are both obeying the same chemical laws. But the phenotype-D is in a way the naturally occurring phenomenon that is analysed. The phenotype-G is the controlled reaction that, according to the contrast and the known background conditions, allows the geneticist to draw certain conclusions about the original constituents. Analysis aims at identifying the elements (in the case of genetics only the genotypic elements) that took part in the reaction that brought about the compound (organism). Chemical analysis in itself does not provide an explanation of the reaction. The reaction can be explained with the help of certain properties of the elements but this is another project. In the case of the organism this is the task of developmental biology (Entwicklungsphysiologie). The phenotype-G is an experimental phenomenon that is indicative with respect to the genotypic elements that entered into the production of the phenotype-D because it is a result of reactions that are planned, controlled and put in the context of other reactions.

In emphasizing the analytic value of single properties (*Einzeleigenschaften*), while at the same time battling the morphological unit-characters, Johannsen expresses the epistemic difference between phenotypes in breeding experiments and phenotypes as products of a developmental process. But in his writings the heuristic value and the ontological criticism of unit-characters still remain in a tension that is not fully resolved by Johannsen.

The clear difference Johannsen makes between the units of analysis and the analyzed whole is obvious in the following quotation. It also shows again that he draws analogies between chemistry and biology, but is at the same time conscious about their limits. Furthermore, it expresses the epistemic point that analysis of complex entities such as organisms does not automatically lead to an understanding of the functioning of the whole (as it would manifest itself in the ability to synthesize an organism).

> The Mendelian analysis of an organism through hybridization is in its restricted relativity of a rather primitive nature; the analytical reagents are other complex organisms, not simple pure bodies like in chemical analyses. Genes or hereditary units as elements of life are probably impossible to isolate — because "life" can manifest itself apparently only as complex appearance. Analysis leads to death — and we will as it seems not succeed in the synthesis of life (Johannsen 1909, 439).

Saying that Johannsen developed several notions that are usually attributed to Morgan, such as the many-many relation between genes and characters and the difference principle, is not meant to redirect credits or blame, but to show that influential geneticists at the time pushed in the same direction. The genotype was the subject of research and it was investigated through phenotypes(-G), which implied a construction of the phenotype as a idealized differential value of a variable, a measurement datum that informed the researcher about the genotype. This view was essentially there already in Johannsen's reasoning in 1909:

> The genes are not to be seen as "bearers" of hereditary properties. These properties should only be regarded as symptoms or reactions, which however are real and measurable — and they must be measured if one wants to proceed in exact research (Johannsen 1909, 482).

This is what Sara Schwartz called the minimizing of the trait concept (Schwartz 1998) and it can also be expressed by saying that characters were made thin in genetics. I will describe Morgan's versions of thin characters, in order to go one step further and argue that this implied to cut characters construed in this way completely from genes, with respect to the explanatory projects pursued, and treat characters, instead, purely as data. The causation expressed in the difference principle is part of an observational theory. It was acknowledged that genes do play a role in the explanation of characters as parts, but this was out of the scope of the experimental systems the Morgan group had at hand. So if we first saw a movement from thick characters to thin characters, we now see the completion of this movement in the change of the status of characters from explananda to data.

6.3.The Morgan Group and Mutations as Markers

Laboratory Phenomena: Mutations and Organisms as Instruments

Johannsen was never fully able to relegate phenotypes (even in the sense of difference points) to mere data (as I will show Morgan did), most likely, because he, coming from research on industrially and agriculturally important organisms himself, was well aware of the implications of genetics for practical fields. In practical contexts, characters (alternatives in the difference points) were always the focal point of interest and therefore not regarded as indicators for genes, but instead had to be seen as something that could be controlled through genetic knowledge. I will briefly come back to this point in the next section.

In the work of the Morgan school, the distinction between phenotypes-D and phenotypes-G becomes much clearer. The mutations observed in the lab are, as Kohler (1994) showed, a result of the laboratory culture, of the sheer mass of flies bred. And the frequency of mutations would soon be pushed even further by artificial means (especially X-ray mutagenesis). The phenotypes studied in *Drosophila* are clearly laboratory phenomena. They do not constitute elementary species that are sorted by natural or artificial selection, like Mendel's peas that represent two varieties, not a normal and an abnormal form (even if they can be interpreted as pairs of normal and abnormal forms).

Even if Morgan started to work with *Drosophila* in the context of artificial evolution and thus regarded mutations as possible points of origin for new species (Kohler 1994, Ch. 2), the mutants that were finally employed for genetic research are described as aberrations from the wild type, which is in itself an artificial product in the sense of an inbred strain. The mutants often would not survive outside the lab. The term "mutation" thus changed its meaning. In natural history "transmutation" meant the change in the form typical for a species that resulted in a new species. De Vries used "mutation" in this way, but applied it in a Mendelian vein to singular characters, which mark the difference between two varieties. In *Drosophila* genetics it designated deformations of an individual that was part of a strain that was carefully maintained for its value as a research tool. "Mutation" here designated the kind of phenomena that were called "monstrosity" in teratology and embryology. But since they were studied in the context of Mendelism they were called mutations. And we will see that this difference in terminology marked a difference in the disciplinary perspective on characters, monstrosity, one can say, is a thick concept, mutation a thin one. The important point is that *Drosophila* mutations, being artificial phenomena in an economically uninteresting organism, made Morgan's research free from the focus on characters that makes every gene look like the cause in the explanation or at least a handle in the manipulation of this character and thus a handle to manipulate agricultural or medical situations. Characters were artificial and uninteresting mutations and the corresponding normal form they defined by contrast.

This made it easier to conceive of characters as conveying information about genes and nothing else. It is in this particular sense that the model organism became an instrument, as Kohler has put it. Johannsen described organisms as the "analytical reagents" that are used to investigate other organisms. Whether one describes organisms in genetics as instruments or reagents, both are laboratory tools, applied for the purpose of analysis (Kohler actually gives analytical reagents as an example for instruments, Kohler 1994, 53). As Staffan Müller-Wille writes:

> Mendel's achievement was not so much that he discovered some empirical law, nor that he found out something about the evolution of hybrids. What Mendel achieved was nothing less than the invention of a powerful experimental system, in which plants not only served as ideal devices for recording the effects of manipulations, but also as the precision tools with which manipulations were carried out. (Müller-Wille, 2009, unpublished manuscript, 6)

If this tool-like character of organisms, with characters as the measured effects of manipulations was there from the beginning in Mendelism, the members of the Morgan school brought this approach to perfection, on the one hand by freeing genetic research from economically important characters and on the other hand by purifying the reagents through rigid standardization and control of strains and experimental conditions.

Of course, also the Morgan group, apart from the fact that there were different opinions among its members, changed its attitude towards things over time. Morgan himself, coming from embryology, at first had a developmental understanding of genetics. He had criticized the morphological unit-character concept, but he continued to see genes as causes of characters. Under the premise of a many-many relationship between genes and characters, he classified genes as affecting the same morphological characters in so-called "organ series." If single genes did not determine characters, multiple genes, in interaction with the environment could be causally responsible for the growth of form. Morgan clearly had a vision of a genetically informed embryology, but apart from constructing organ series, which actually led to some quite elaborate developmental hypotheses (Schwartz and Falk, 1993), he did not have an experimental approach to address the interaction of genes in bringing about characters. Instead another experimental approach emerged in his lab (Allen 1985, 112-113), an approach that used characters, to study structural and spatial relations among genes (an approach that according to Kohler was much more pushed by his students than by Morgan himself, Kohler 1994, 62-63). Genes were now no longer classified according to the organs they affected, but into linkage groups. Kohler interprets this change in focus and method as a break away of the Morgan group from Neo-Mendelism, “in which the boundaries between heredity, development and evolution were not yet well defined.” (Kohler 1994, 57) How strongly one wants to emphasize this break depends, of course, on who of the Neo-Mendelian researchers is chosen as contrast and how this researcher’s or group’s work is interpreted, but on the basis of the reading of Johannsen that was provided here, the move away from developmental or evolutionary explanations of characters towards using characters to study the genotype was inherent in Mendelism, because it was the only type of conclusions its experimental system warranted. The Morgan group, as opposed to Bateson, for instance, came to fully accept this limitation and by focussing solely on the informational content of characters with respect to the genotype, thereby abandoning any attempt to explain characters, they hugely expanded the range of types of information that could be gained from patterns of “inheritance of characters” (it would be more correct to speak of occurrences of characters) and combined it with cytological information about chromosomes. But the information gathered allowed inferences only about the inventory and structure of the genotype and finally established a material understanding of the genotype as being embodied in the chromosomes. The chromosomal theory of inheritance was a theory about the inheritance of chromosomes, not the inheritance of characters.

The Separation of Heredity and Development

It is usually said that Morgan separated heredity from development (Allen 1985; Gilbert 1978; Amundsen 2005, Ch. 7). Before, this implies, they were seen as two aspects of the same process, which we might call reproduction (see also Griesemer 2007). That Morgan himself held this unitary view of heredity is often illustrated by the following quotation (which is given in varying length by Gilbert 1978, 349, Allen 1985, 108, Amundsen 2005, 148):

> We have come to look upon the problem of heredity as identical with the problem of development. The word heredity stands for those properties of the germ-cells that find their expression in the developing and developed organism. When we speak of the transmission of characters from parent to offspring, we are speaking metaphorically; for we now realize that it is not characters that are transmitted to the child from the body of the parent, but that the parent carries over the material common to both parent and offspring. (Morgan 1910, 449)

But if we read carefully and place the quotation in the right context, it actually looks as if Morgan already makes a distinction here. The expression "We have come to look" is contrasting his view with a different view. This rejected view is the "transmission of characters from parents to offspring," a formulation, which, as he says, can now only be used metaphorically, but which was used literally before. It is the view that the body of the parents influence the germ cells, be it in form of the radical view of acquired characters, or in the mild version of the biometricians that allow for transmission of personal variation. This view was rejected so forcefully by Weismann and Johannsen that Morgan "hesitates" to state the new view, which is expressed in the quotation above, again. Heredity and development are the same on the new account as opposed to the old view where characters are transmitted *and* characters develop. There are not two processes that involve characters, but only one: development. Transmission instead concerns only the constituents of germ cells. This observation clearly does introduce a distinction: The transmission of hereditary material and its expression. And Morgan already reserves the term "heredity" for the "properties of the germ-cells," because they are the result of a transmission and this is what the metaphor taken from the legal context originally alludes to.[62] His distinction is

[62] Johannsen preferred to get rid of the term "transmission" altogether, but from his point of view Morgan's use of the term was harmless. He battled the "transmission of characters;" the transmission of germ cell constituents might be a metaphor as well, because it was not known if the hereditary material was passed on or copied, but as long as the body did not have an influence this did not matter. And as Roll-Hansen

a separation of one process in two linear steps (transmission and development). This is certainly different from the distinction made in the rejected view, of two parallel, or overlapping processes, the transmission of characters from parent to offspring and the development of the characters in the individual. If heredity and development are identical, it is because they are two steps in the same process (reproduction), but they are still distinct. Later, Morgan will emphasize the difference between the sub-processes, stating that they are processes of a very different kind:

> For purposes, then, of closer analysis, it seems desirable in the present condition of genetics and embryology to recognize that the mechanism of distribution of the hereditary units or genes is a process of an entirely different kind from the effects that the genes produce through the agency of the cytoplasm of the embryo. (Morgan 1917, 25)

Thus my interpretation of the original unity of heredity and development (at least in Morgan's reasoning) is quite different from that of Ron Amundsen who argues that, before the separation also for Morgan, "Heredity is the passing on of developmental processes." (Amundsen 2005, 148) But as said above, it is true that Morgan first invested the genetic analysis with the hope of achieving developmental explanations. What is suggested by most authors who discuss the separation of heredity and development is that Morgan and his co-workers came to abandon attempts for developmental explanations, but maintained a view where the presence of characters are explained through the presence of genes (where only genes and not characters are inherited), even if the intermediary steps of development are unknown. Sometimes this view is stated more carefully, acknowledging that what was explained was not the presence of single characters, but only patterns in the distribution of characters in the offspring, or at least differences in characters.

(2009, 470) pointed out, Johannsen used a Weismannist diagram throughout his career that showed the persistence of genotypic units (the transmission of the stirp, the genotypic unit in the Galton-Weismann view, which he shared to that extent).

I will argue instead that the Morgan group completed the separation of heredity from characters and left the explanation of characters solely to development. Scott Gilbert writes: “The study of inheritance became genetics, which Morgan defined as the discipline concerned with the transmission of nuclear genes.” (Gilbert 1996, 104) One might add that it was concerned also with the distribution of genes, but not with the transmission and distribution of characters. In Morgan’s words:

> The modern theory of heredity is derived from numerical data obtained by crossing two individuals that differ in one or more characters. The theory is primarily concerned with the distribution of units between successive generations of individuals. (Morgan 1926, 1)

The units here are genes. Characters only play a role as data. This minimizing of the character concept shows itself in the term “marker” as Schwartz rightly points out (1998, 2000), although the term was introduced relatively late and at first did not exactly have the meaning of one character indicating one gene. Muller and Altenburg use it in 1930 to replace the more complicated expression “identifying character” or “identifying gene.” (Muller and Altenburg 1930) This terminology was first used in 1920 for characters that indicate genes the hereditary behavior of which was well known, and that are used as evidence for other genes that are not related to these characters, but that did not have an easily identifiable phenotypic effect themselves. Through linkage relation inferences could be made about the gene in question (Altenburg and Muller 1920).[63]

It is true that Morgan and his co-workers separated heredity from development, but they did not separate heredity from development *in explaining characters*. By defining

[63] Morgan et al. in the 1922 edition of their textbook discuss the case and use the term “identifying marks”: “An analysis of the factorial composition of the truncate flies was then made by crossing them to flies containing in each of their chromosomes other mutant factors whose hereditary behavior was known. In the second generation of the cross (back-cross) these other factors served as identifying marks which disclosed just which chromosomes of the P_1 truncate fly each F_2 individual had or had not received.” (Morgan et al. 1922, 242)

inheritance as the inheritance of genes, they excluded characters from the things that are explained by the theory of inheritance. Accordingly, they excluded development from the study of inheritance. Later development was tackled again from within the *Drosophila* community, most notably in form of the work of Beadle and Ephrussi, but this work, even if it utilized genetic analysis, consisted in an expanded experimental system that integrated strategies from classical embryology as well as biochemistry, and thereby facilitated a re-morphologizing, or thickening of characters.

The Difference Principle and Characters as Data

The reason that some authors argue that heredity and development were separated, but that heredity still served as an explanation of characters, is that Morgan and other Drosophilists kept on talking about the causation of characters. The way in which genes are thought to cause phenotypes is given by Morgan et al. in a section "On the Relation Between Factors and Characters" as follows:

> In this sense we may say that a particular factor (p) is the cause of [the character] pink [eye color], for we use cause here in the sense in which science always uses this expression, namely, to mean that a particular system differs from another system only in one special factor. (Morgan et al. 1915, 209)[64]

Ken Waters describes this type of reasoning more formally as a difference principle:

> Differences in a gene cause uniform phenotypic differences in particular genetic and environmental contexts. (Waters 2007, 558)

[64] What this means is that causation is usually thought of as a relation between two states of a system as opposed to a relation among individual entities (a notion that many philosophers seem to have lost). The latter understanding is only warranted through the difference principle. A textbook on statistical method of the time that is also quoted by Johannsen illustrates the way that "science always uses the expression": "We start with the assumption that everything that exists, and everything that happens, exists or happens as a necessary consequence of a previous state of things. If a state of things is repeated in every detail, it must lead to exactly the same consequences. Any difference between the results of causes that are in part the same must be explainable by some difference in the other part of the causes." (Thiele 1903, 1)

It is usually acknowledged that this is a very modest form of claiming causal explanation. Nevertheless it is taken to indicate that the Morgan group was interested in explaining characters. Ron Amundsen for instance writes: “If a single allele can be regarded as the cause of pink eye color, then it is possible to causally explain characteristics without any reference to the embryological process that actually brought them about.” (Amundsen 2005, 150) For Amundsen it seems obvious that if genes are thought to cause characters, then this causal relation must be explanatory and therefore the explanations of characters must be on the agenda of the Morgan group. This shows in another reading of Morgan et al. by Amundsen that seems to me to be mistaken. Morgan and his co-authors write:

> The characters of the organism are far removed, in all likelihood, from these materials [factors=genes as chemical materials]. Between the two lies the whole world of embryonic development in which many and varied reactions take place before the end result, the character, emerges. [...] Although Mendel's law does not explain the phenomena of development, and does not pretend to explain them, it stands as a scientific explanation of heredity, because it fulfils all the requirements of any causal explanation. (Morgan et al. 1915, 226-227)

Amundsen seems to read “causal explanation” in the end of the paragraph as referring to the difference principle, that is, to the relation between factors and characters. But it is clearly stated that explaining characters is a matter of development and that Mendelian genetics does not explain development and thus does not explain characters. There is no justification to think that here it is said that Mendelian genetics does causally explain characters, just not in a developmental way, but in a different way rationalized through the difference principle. In which sense then is Mendel’s law a causal explanation? In the formulation that it has been given by the Morgan group, it causally explains the constitution of the hereditary material of the offspring from the constitution of the hereditary material of the parents. It provides an “explanation of heredity” and heredity is only about chromosomes as Morgan has clearly stated.

In Morgan’s later formulation of the *Theory of the Gene*, characters do not play an important role at all. What is said is that they “are referable” to genes, an expression Morgan uses several times and that can be read as “characters indicating genes.” In any case, characters are not mentioned as explananda here:

> We are now in a position to formulate the theory of the gene. The theory states that the characters of the individual are referable to paired elements (genes) in the germinal material that are held together in a definite number of linkage groups; it states that the members of each pair of genes separate when the germ-cells mature in accordance with Mendel's first law, and in consequence each germ-cell comes to contain one set only; it states that the members belonging to different linkage groups assort independently in accordance with Mendel's second law; it states that an orderly interchange — crossing-over — also takes place, at times, between the elements in corresponding linkage groups; and it states that the frequency of crossing-over furnishes evidence of the linear order of the elements in each linkage group and of the relative position of the elements with respect to each other. (Morgan 1926, 25)

Most authors would, nevertheless, agree with Amundsen that characters are somehow the explananda of Morgan’s theory of the gene, even if some are more careful in stating that only patterns in the distribution of traits are explained (Amundsen offers this as a less problematic reading of Morgan, Amundsen 2005, 150-151), or that only differences between traits are explained (Waters 1994).

Therefore, I provide further argument that characters do not figure as explananda on the Morgan school’s agenda. I will proceed in two steps, first I will show that even if characters are described as caused by genes in the sense of the difference principle, there is no sign that they are regarded as the explanandum of the “*Theory of the Gene.*” Then I will sketch an alternative role for the difference principle, namely as an observational theory.

It already became clear from the above statement of the *Theory of the Gene* that characters do not figure as explananda, but, instead, other phenomena concerning the behavior of and relations between genes in the transmission of genetic material. But it can also be argued that characters cannot be thought of as explananda, because explanations of, say, eye color through the difference principle would not be very interesting. The reason is not, of course, that no one would be interested in explaining something as unimportant as eye color in a fruit fly, because in model organism based research the hope always is that some generalizable principles are found, such that an explanation of the eye color in *Drosophila* could serve as a model to explain important characters in humans or economically relevant organisms, that is, it could help to explain characters in general. But what kind of generalization does the explanation of eye color through genes in the sense of the difference principle allow for? Probably the only generalization is the difference principle itself, or some general statement that genes do play some role in development or that more than one gene is involved in the production of a particular organ. But then the power of the system to generate generalizable explanations would soon have been exhausted.

Furthermore, it seems that the patterns in the inheritance of characters cannot be explained through patterns in the inheritance of genes, because genes are only identified through characters. If, for instance, it is observed that a character is inherited together with sex and it is concluded (justified by the difference principle) that the gene modifying this character is located on the sex chromosome, a subsequent explanation of the co-occurrence of the character and sex would be rather redundant, because we only know that the gene is on the sex chromosome through the co-occurrence. In terms of explanation, independence matters. We cannot explain a phenomenon through something of which the only evidence is the explained phenomenon. Genetics' explanatory structure has been accused of being circular. But this is not true of the work of the Morgan school. Not because they have independent evidence in form of microscopic cytology (this does not have the capacity to identify genes), but because the phenomenon that is used as evidence for the genes is not what is explained.

Another point that shows that the role assigned to characters cannot be that of an explanandum is that the characters used to identify genes can be switched in a way that an explanandum cannot be exchanged. Morgan keeps emphasizing that mutations in genes have many unrelated phenotypic effects. All of them are caused by genes (or rather alleles) in the restricted sense of the difference principle. In Morgan et al. (1915) a case is described of a mutant that was first identified through the phenotype of not-unfolded wings (it was called *club* because of the shape of the wings in that state). Since only 20% of the carriers of the mutation actually showed the phenotype, it was exchanged by another effect of the mutated gene that was discovered later: the absence of a pair of spines on the side of the thorax. Of course, sometimes the explanandum is exchanged in scientific explanations, if an explanation has been elaborated but it turns out that a phenomenon different from that which triggered the investigation in the first place is explained better by this explanations. But here it is obvious that it simply does not matter which phenotypic effect is looked at, because the goal of the investigation was to find the mode of inheritance of the gene and the result was that it was sex linked. If unfolding of the wing would be an explanandum in any sense, it could not just be replaced by something else. That it does not matter which effect of an allele was chosen becomes obvious in the following quotation from *The Physical Basis of Heredity*:

> Clearly then the character that we choose to follow in any case is only the most conspicuous or (for purposes of identification) the most striking or convenient modification that is produced. Since, however, these effects always go together, and can be explained by the assumption of a single unit difference in the germ-plasm, the particular difference in the germ-plasm is more significant than the character chosen as its index. (Morgan 1919, 240)

Even if Morgan uses the verb “to explain” here, this passage also clearly states that the interest focuses in the constitution of the hereditary material and that the role of characters is that of an “index.” Causation accordingly matters only to justify the indexicality of character, not to explain them. This term, similar to the term “marker,” gives a hint to the appropriate understanding of the difference principle as an observational theory. A character is explained only in the sense that an instrument’s readout is explained.

An index represents due to its causal relation to the object it represents. But pragmatically, an index is used to measure the object; its explanation is not necessarily a purpose of stating the causal relationship, but rather the justification of its use for measurement. A weather vane is an index for the direction of the wind. Its position is caused by and thus somehow explained by the wind direction. But if “explanation” is taken to express the epistemic interest of the observer, it is hard to imagine a situation where someone was interested in explaining the position of the vane. Nevertheless, the causal relation can be spelled out in terms of classical kinetics. Such a theoretical explanation would justify the use of the index, but this would usually not be the purpose of its use. The kinetic explanation would be an observation theory here: a theory that explains the connection between the measured object and the measurement instrument. In this context it has no epistemic value in itself, though it might have in other circumstances.

Are Characters Explained in Genetics?

It might appear as if I am preaching to the converted here. For sure, I am not claiming that anyone accuses Morgan and his followers of entertaining any naïve form of genetic determinism. Morgan rejected the unit-character concept as preformationist and emphasized the many-many relation between genes and characters. If genes explain characters, they do so only in the sense of the difference principle. It is also generally agreed upon that the Morgan school had various explanatory agendas and maybe some goals that are not well described as explanations at all, such as the mapping of genes, and that many of the goals were related only to genes, not characters. Waters, for instance, writes: "Laws of segregation and independent assortment and principles of genetic recombination and replication are used to explain and predict gene transmission." (Waters 1994, 165, see also his 2004) It is further acknowledged by most authors that in pursuing these goals characters were used as indicators for genes. But still the idea that one of the goals was to explain the inheritance of characters, or patterns in the inheritance of characters, or at least patterns in the inheritance of differences in characters dies hard.

Waters cites the passage where Morgan uses the term "index" (see above) and states that "the differences in phenotypic form identified by classical geneticists were not viewed as fundamental units of development; they were understood to be phenotypic quirks caused by differences in the real units of heredity, the genes" (Waters 1994, 174), thus making a distinction similar to the phenotype-G and phenotype-D distinction. He puts much emphasis on the difference principle and contests other philosopher's reading of classical genetics as identifying genes for characters. But still, he writes that in classical genetics the "inheritance of phenotypic characteristics can be explained by charting the transmission of genes and relating genotypes to phenotypes." (Waters 1994, 169) His critique of other accounts is that they seem to "relate genotypes to phenotypes" through development, ending up wrongly accusing classical geneticists of a naïve genetic determinism, while he maintains that classical geneticists related genotypes to phenotypes only through the difference principle, that is, strictly speaking, they explain differences in characters through differences in genes: "What were studied were character differences, not characters, and what explained them were differences in genes, not the genes themselves." (Waters 1994, 172)

It sounds, however, ontologically dubious, to say that differences cause differences. It was remarked above that earlier states of systems cause later states, and that differences in the resulting state indicate differences in the original state. If we move on this basis to individual causes, then it is still the individual alternative forms that "cause" the individual alternative "effect." Objects cause objects or values of variables cause values of variables, whatever the descriptive decisions taken, but differences do not cause differences, otherwise, the difference between smoking and non-smoking causes the difference between getting cancer and not getting cancer and smokers have nothing to fear. That is, it is still the allele that causes mutant phenotype. And another allele that causes the normal form (which is delineated from the whole organism in contrast to the mutation). It is the contrast (informed by counterfactual reasoning), that is, the difference that allows one to speak of the alleles as causes, but it is not differences that cause differences. The difference principle is more aptly expressed by Sara Schwartz:

> Thus, the differential concept of the gene can coexist with both one-to-one and many-to-many relationships between genes and traits because the former deals with the relations on a different ontological level than the two latter concepts of relations. The one-variate and multivariate functions deal with the relations between genes and traits, while the differential concept of the gene deals with the relations between their attributes, which are expressed in terms of alternative states of both genes (alleles) and traits (AATs [alternative appearances of a trait]). (Schwartz 2000, 31-32)

The relation is between alleles and alternative appearances of traits, not between differences. In a way already the one to one relation of the unit-character involved allelomorphs, but they could be thought of as singular entities. The difference principle requires that one sees them as pairs (or x-tuples). This is implied in the fact that characters are viewed as values of variables as opposed to morphological units. Like Colless, Schwartz remarks that the relation of traits (variables) and alternative appearances of traits (values) can be spelled out in terms of determinables and determinates. This is not possible for traits as parts, which follow a part-whole logic. Now it seems that a determinable cannot have only one single determinate. (If there would be only red things, there would be no color.) As Schwartz writes elsewhere:

> Variability guides the observer to the unifying element, which indicates the boundary of the character. In this way, for example, the color variability of mouse guides the observer to the character 'the color of mouse fur.' (Schwartz 1998, 4)

Schwartz also seems to share the view defended here that if classical genetics refers only to the work of the Morgan group, character differences were not the explananda in the sense of the object of interest of the investigation. They were pure data and as such they were the explanandum only of an observation theory, that was, however, in the absence of a developmental theory only a weak one. There are several reasons why the idea of characters (patterns of, differences in) are part of the explanandum of classical genetics persists in other authors' writings.

First, apart from statements about genes causing characters that have now been identified as statements specifying the observation theory, there is a particular feature of the observation theory that makes it difficult to identify it as such, and that is its relative lack of independence. Peter Kosso in analyzing "*Dimensions of Observability*," emphasizes the role of the independence of observation:

> Given that there is a theory T of the object x (a theory which the observation of <x,P> is likely intended to test), one can ask whether the physical laws, which account for the delivery of information in observation are independent of T. (Kosso 1988, 456)

The question is if the observation theory, the theory that accounts for the causal connection between the measured object and the measurement instrument and the theory of interest about the measured object are independent. In this case we would have to ask if the theory connecting genes and phenotypes (the observation theory), which is used to observe genes, is independent of the theory of the transmission of genes (the theory of interest). However, we have seen that there is no proper theory connecting genes to characters. This would be a developmental theory. Instead, there is the difference principle, describing a regular behavior that can be used to justify the observation. But this is done on the assumption that there could be a developmental theory assigning a, however remote, role to genes in the development of characters. Would such a theory be independent of the theory of gene transmission or not? From Kosso's analysis, I cannot see sufficient criteria to distinguish dependent from independent theories, but the intuition is clear. The observation of chromosomes under a microscope is, for instance, a case of relative independence. The optical theory accounting for the relation between the chromosomes and the microscopic image is not affected if the theory concerning the chromosomes that is investigated with the help of the microscope is modified.[65] Instead Morgan and his co-workers clearly thought that a complete theory of the gene would cover its behavior in transmission as well as its role in development. Thus even if the theory of development as part of a complete theory of the gene did not yet exist, a change in the theory of the gene as a unit of transmission must be seen as affecting the theory of development. The difference principle does not explain traits in the sense that optical theory explains the microscopic image. It is a proxy for a developmental theory that, apart from being of interest in itself (as optics is for physicists), would fully justify the evidence gathered about the transmission of genes through phenotypes, just as optical theory justifies the evidence gathered with the microscope.

Being a proxy for a developmental theory, being connected to it through the phenotype, which is after all explained only through development, the difference principle has to be regarded as dependent as well. Kosso regards independence of observation theories as a criterion for evaluating observations. Dependent observations are compromised; they are

[65] One could argue that it is the same physical properties that are responsible for the reflection of light that are responsible for the genetic properties of the chromosomes, but this would mean to break down every phenomenon to physics. But this is not done in the situations we are talking about, where theories of the domain of interest are employed. Here the theory of the gene and the optical theory are clearly separated.

laden by the same theory they help to test. Independent observations are also theory laden, but since the theory in question is not the one tested, this is less threatening. Kosso writes in the vein of philosophical debates on theory justification. In the actual practice of science, however, where phenomena of interest and theories are developed in parallel (as opposed to ready made theories that are tested and justified), dependent observations play a particular role in identifying and operationalizing the objects of interest in the first place. Also independent observations can operationally define objects, for instance, if an object is defined as the particle visible under this and that condition under the microscope. But it is theoretically dependent observations that identify objects as important functional players. Genes are not only indicated by phenotypes, but they are also operationally defined as those bits of the chromosomes that have an effect on the phenotype, because this shows that they must have some distinct functional role (even if not directly in bringing about the phenotype). I claimed that the Morgan school was only interested in explaining the transmission of genes and not in the explanation of characters, but still they were interested in the transmission of these units *because* they had an influence on the phenotype. This is what made them interesting units to study, because, no doubt, the whole organism is what biologists are interested in. The fact that characters identified genes as interesting units seems to be one of the reasons that people think the investigation of the transmission of genes by Morgan and other Drosophilists must have had the immediate goal to explain characters.

Another reason is the role that the genotype-phenotype relation played in other fields of investigation, contemporary to classical genetics, or later. For instance, genes will be successfully implemented in developmental explanations of characters some decades later, as discussed in the conclusion, and this strategy makes use of the difference principle. This might give the impression that genes have been referred to in the explanation of characters also before. But here I want to focus briefly on the above-mentioned role of characters in practical contexts. The main reason that one might think that the Morgan school provided an explanation for the “inheritance of characters” is probably that in most contexts people are interested in the characters, and not in the genes and chromosomes. I said before that in practical contexts characters were the focus of interest and every application of transmission genetic knowledge depicted them as what is controlled through genes rather than as mere data for genes. Transmission genetics was seen as being instrumental for controlling characters in agricultural breeding, and actually funded for this purpose (Allen 1985, 113-115), instead of characters being instrumental in investigating the transmission of genes. But the immediate goals of *Drosophila* genetics and agricultural genetics were, nevertheless, different and I emphasized the role of the emancipation of the Morgan school from practical contexts for the direction their work took. I am not intending to reinforce a sharp distinction between applied and basic research.[66] But attempting to create an organism with particular features and giving an explanation of the principles that underlie successful breeding, are still logically separable activities and while the latter is not concerned with the explanation of characters, the former is interested in characters but not concerned with explanation at all. People might be interested in the inheritance of characters and not only the inheritance of genes. But in a practical context causal explanation is not what is looked for,[67] but rather a recipe of how to set up a breeding regime to achieve a certain result. This is based on prediction, but prediction and explanation are not symmetrical (Hanson 1959).

[66] Research and the application of genetic knowledge, are certainly not separable as agricultural research stations such as Svalöf illustrate. And often it was particularly the challenges that arose from manipulating a certain desired traits that did not easily fit the known principles, which triggered new knowledge, where laboratory scientists would just have dropped the trait and worked with a more tractable feature instead. Nillson-Ehle’s multiple factor hypothesis might be a case in point (Müller-Wille 2005).

[67] Although, a specific notion of causation becomes important in these fields, where the environment is not controlled as in laboratory genetics, when the degree of genetic and environmental influence has to be separated. This is not the notion of causation expressed in the difference principle, because the latter relies on constant environmental conditions.

Morgan acknowledges the predictive power of the results his version of genetics produces, but he leaves no doubts where his interest lies, namely in the structure of the chromosomes:

> Today we arrange the genes in a chart or map. The numbers attached express the distance of each gene from some arbitrary point taken as zero. These numbers make it possible to foretell how any new character that may appear will be inherited with respect to all other characters, as soon as its crossing-over value with respect to any other two characters is determined. This ability to predict would in itself justify the construction of such maps, even if there were no other facts concerning the location of the genes; but there is today direct evidence in support of the view that the genes lie in a serial order in the chromosomes. (Morgan 1933, 315)

The interests of the kind of genetics Morgan and his followers pursued where different from the interests of agricultural geneticists. And as has been argued before, they were also different from that of embryologists. Embryologists at the time, were equally aware of the difference between parts and variables, explananda and data, and, commenting on genetics, left no doubt they in turn were interested "more in the back than in the bristles on the back and more in the eyes than in eye color," thus in thick parts (E.E. Just quoted in Harrison 1937).

Chapter Conclusion

Today, Johannsen is taken to have rejected the connection between characters of parents and the genes they pass on and between the genes received and the characters of the offspring at the same time, through the introduction of the genotype-phenotype distinction. But these were two separable intellectual steps. To realize these steps and thus to see how the notion of character in genetics was sharpened and at the same time diversified in the different sub-branches of genetics that developed from Mendelism, it is important to understand the specificity of the different character notions. I argued that Johannsen first attacked a naturalist notion of heredity that for him goes from Hippocrates to Darwin and the biometricians. He saw Galton and Weismann as allies in his battle against this notion. This was a battle against the view that the body of parents or more distant ancestors had any influence on the structure of the gametes, an idea that was clearly dismissed by Weismann's germ-plasm/soma distinction. The introduction of the phenotype was meant to bring even continuously varying characters in line with the Mendelian characters concept, which implied that only mutation can facilitate evolutionary change.

Nevertheless, Johannsen strongly disagreed with the particular conception of germ-plasm as containing determinants for body parts. Weismann helped to establish that "Heredity is the presence of the same genes in offspring and ancestors," (Johannsen 1909, 488) but he still felt the need to explain why there are the same body parts in parents and offspring, through the inherited units. This is after all the same notion of body parts that the naturalists equipped with an influence on the gametes. Even if Weismann cut this connection he kept the notion of body parts and he did not cut the connection between genes and body parts in development. Therefore, Johannsen later came to think of Weismann (and also Bateson) as being part of the naturalist tradition, which Johannsen characterized as a morphological tradition. Johannsen interestingly connected the morphological view to specific observational practices: collections, anatomical dissection and histology.[68] It would be absurd to think that Johannsen dismissed any notion of body

[68] Already when he criticized the notion of inheritance of the ancestor's body parts, which was for him implied in the notion that selection operates on individual (non genetic) variation, he mentioned that this view was influenced by the apparent "continuity of museums." This is a quite modern sounding observation about the epistemic role of collections (see e.g. Griesemer 1990; Müller-Wille 2007b; Strasser 2010), that goes back to a metaphor of Galton (Johannsen 1909, 328, 1911, 134, 158; Galton 1889, 33), and which at least with respect to the concept of variation as relevant to heredity and evolution, for Johannsen was something that we could today describe with Bachelard (2002) as an epistemic obstacle, although not in the first place a conceptual, but rather an obstacle that is embodied in the institutional and material practice. Weismann overcame the obstacle of apparent continuity, but he still thought in terms of parts. This why in 1923, Johannsen added anatomy and histology next to collections as the practices that

parts composing the organism. As a botanist he could clearly see the usefulness of anatomic and physiological partitionings that were described in detail in the textbook co-authored with Warming (Warming and Johannsen 1909). But with respect to the relation of genotype and phenotype, even if the genotype consisted of units that segregated independently, which could be observed in differences in characters as values of variables, the genotype as a whole (in the actual composition that happens to make up one zygote) in reaction with the environment brought about the individual organism as a whole (Johannsen 1911, 133). Weismann's mistake was not that he maintained a morphological character notion, in principal, but to describe heredity in terms of this character concept. Mendelian genetics had developed a new observational system that implied a different character concept (characters as changes in values of a variable that indicated changes in genes)[69]. As long as there was no observational system that allowed researchers to address the connection between genes and morphological characters every statement of a connection was pure speculation. A practice with which Johannsen just as Morgan strongly disagreed; it was against their scientific ethos. But as opposed to Johannsen, Morgan was much more optimistic that such an observational system could be constructed.

However, Morgan and his students did not have a system to address the development of characters as parts that would allow them to include the action of genes in the picture. Therefore, they had to follow Johannsen in treating characters as indicators for the constitution of the genotype and they did so very successfully. Including also cytological information about chromosomes, they managed to describe many new structural features of the genotype that explained how the constitution of the offspring's genotype was influenced by the parent's genotypic constitution.

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defined characters as parts (Johannsen 1923, 133), and are thus inadequate for defining characters in genetics, although they can be instrumental, of course, because after all, thin character concepts are abstracted from thick observations.

[69] Even if they looked at properties not parts, Johannsen also thought of Mendel and Bateson as having a morphological property concept.

If we return for a moment to the historical narrative of a separation between genetic and developmental explanations of characters, we can now see what went wrong. Amundsen illustrates this view with the help of two diagrams (see my Figure 6.1. after Amundsen 2005, Ch. 7, Figures. 5 and 6, 156-157). One shows the older, broad view of heredity. We see an arrow, which symbolizes the flow of hereditary units and the structure of explanation, accordingly. It connects the phylogenetic ancestors and the remote conspecific ancestors with the parents of an individual. The parents are connected with the "heredity passed to the offspring," that is whatever goes from the parents in the zygote, and this is connected to the ontogeny of the offspring (the individual in question), which finally is connected to the "phenotype of the offspring". This is contrasted with an image (representing the new, narrow view of heredity) in which the arrow connects only the parents with what is inherited, which here is characterized more specifically as "material" and this is connected with the "phenotype of the offspring", bypassing "the black box of development." The connection of the hereditary material with the remote ancestors has indeed been cut off by Johannsen. The hereditary material of the offspring is the result of the merging of hereditary material from the parents, whatever is the reason for the constitution of the parental material. It is also true that what is passed on from parents to offspring has been conceptualized more clearly as "material" that is organized in chromosomes.

What concerns me here is the bypassing of development by the explanatory arrow from "heritable material" to the "phenotype of the offspring." The point is that the expression "phenotype of the offspring" in the two diagrams actually should be taken to designate very different things. In the broad view diagram, it stands for thick characters as parts in the sense of embryology, that is, for morphological structures that are conceived as products of a growth process (phenotypes-D). In the narrow view diagram instead, it represents characters as values of variables (phenotypes-G).

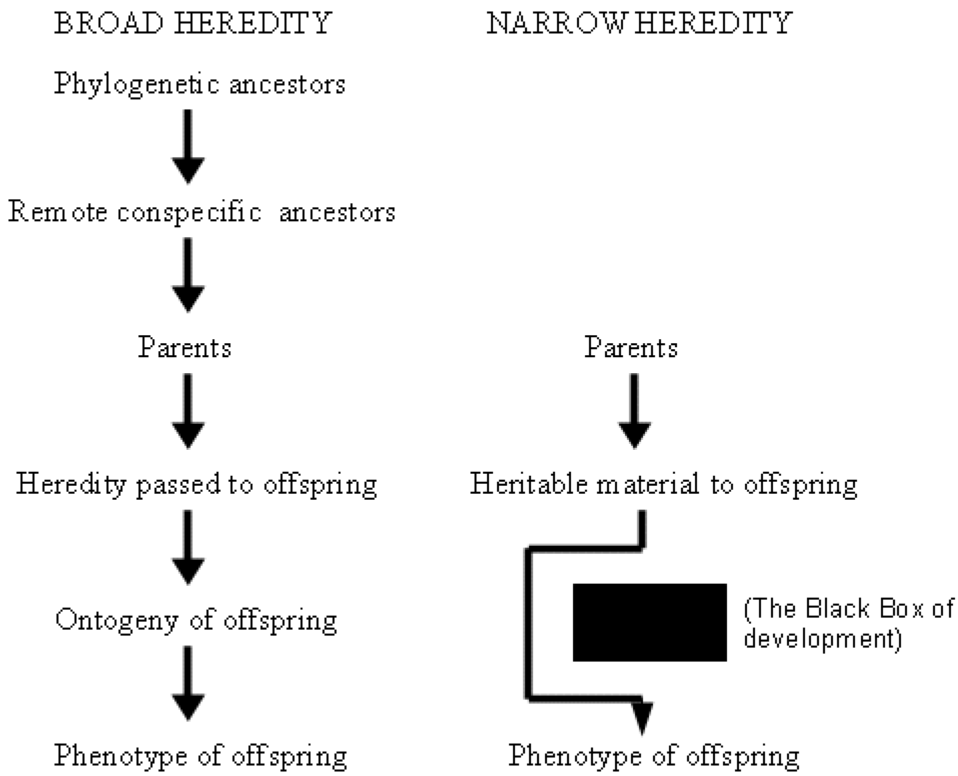

Figure 6.1. The broad and the narrow conception of heredity. (After Amundsen 2005, Ch. 7, Figs. 5 and 6, 156-157.)

Characters as values are indeed caused by genes in the sense of the difference principle. But first, this does not imply that they are explained, because causation in this sense is not sufficient for proper explanation (even if "explanation" is sometimes used in this weak sense), and their explanation is also not a goal of the investigations performed by the Morgan group. And secondly, development does not explain characters as values (phenotypes-G), but characters as parts as products (phenotypes-D). Therefore the arrow is neither an arrow of explanation, nor can it bypass development, because development does not lie on the way from genes to phenotypes-G.

Instead, it would be appropriate, to draw a reverse arrow, connecting the phenotype-G with the "heritable material," an arrow of inference[70]. Together with

[70] It is an inference from data to phenomena, where genes are phenomena, that is, "stable, repeatable effects or processes that are potential objects of prediction and systematic explanation by general theories and which can serve as evidence for such theories," while phenotypes are data, that is, "public records [...] produced by measurement and experiment, that serve as evidence for the existence of phenomena or for

phenotypic information about the parents, the constitution of the "heritable material" of the offspring can then be explained from the constitution of the "heritable material" of the parents. To come back to the cutting away of the remote ancestors: of course, the constitution of the genotype of the parents is somehow a result of the genotype of the ancestors. The reason for Johannsen to cut it off was exactly that there was no arrow of inference going from the observed organisms to the ancestors. This inference is possible in phylogenetic analysis, but this is a practice with a different data set and inferential structure. It starts from the phenotype or genotype of closer or more distantly related varieties or species to infer the phenotype or genotype of remote ancestors.

So much for the historical narrative, but it should at this point be clarified again conceptually what a character as value or phenotype-G is as opposed to morphological parts or properties. This can probably best be achieved through a small thought experiment. If we get back to Bateson's research on the combs of fowls, we can say that he gave the morphological interpretation of the *walnut* comb as showing the morphological features of *rose* and *pea* combs after he found that it was genetically a compound character by segregation analysis. His perception of form was biased by what he already knew to be a compound. Segregation analysis of course also requires careful morphological inspection, but only in order to find differences. We can now imagine a strict division of labour. One person does the morphological inspection, but does not give any morphological information to the second person, but only letters that indicate the differences (a notation that was actually used). The other person sets up the breeding regime and does all the

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their possession of certain features." (The definitions, though exemplified with other examples are from Woodward 2000, S163) Woodward continues: "When data play this role they reflect the causal influence of the phenomena for which they are evidence but they also reflect the operation of local and idiosyncratic features of the measurement devices and experimental designs that produce them." One can say that in this case they reflect environmental influences, but in virtue of the way they are represented through the contrastive procedure, they refer to genes.

genetic inference. It will be perfectly possible in this arrangement to come to the same conclusion, namely that *walnut* occurs in the presence of two factors that alone give *rose* and *pea*, respectively. Inference to the constitution of the genotype does not require any morphological interpretation, just differences in values. This shows that in genetic analysis, a thin character concept is employed.

Conclusion: Thick and Thin Characters in Developmental Genetics

What has been achieved?

The starting point for this thesis was the observation that some concept of character, trait or phenotype, or, even if these terms are not used, at least some form of recognition of parts and properties of organisms, plays a role in every branch of the life sciences and in many other contexts of human practice as well. A superficial look at different disciplines and different times in the history of biology makes clear that there are very different character concepts in use that play different roles and warrant different ways of decomposition of organisms into parts or discrimination of organismic properties. This diagnosis of plurality required taking a position on scientific pluralism. In Chapter 1 I have, therefore, analyzed philosophical accounts of the issue and found that, although pluralism became something like a consensus in the philosophy of the life sciences, the prevailing view is that the plurality of decompositions, classifications and explanations that is found in these sciences is due to the complexity of the living world. Complexity is construed here as some fundamental property of the domain that is studied by these sciences. I have argued, instead, that the plurality stems from the principal openness of human practice, in terms of how to represent, what to contrast, how to manipulate the world, and furthermore in terms of the ever new creation of cultural concepts that interact with empirical ones.

To describe which role characters play and how they become individuated in specific situations, accordingly, requires some tools to describe human practice in general and scientific practice in particular. Scientific achievements, whether they are theories, models, explanations or taxonomies come in some semiotic form, be it linguistic text, mathematical formulas, images, e.g. diagrams, drawings, photographs or films, or material models of various kinds. Accordingly, an account of representation was needed that is compatible with pluralism, can explain in which way representations are about the world and is open to all kinds of semiotic forms. Furthermore, an account of action was needed that can describe representation as an activity, that is, describe how representations are brought about and at the same time can show how action is informed by representations.

In Chapter 2 I have employed Saussure, in order to show that signs, verbal or other, have a systemic nature. Signifiers, the material sign-vehicles (or the impressions thereof) can fulfill their function in that they are different from other signifiers and the signified, which for Saussure is a concept, is delimited in its scope by the other concepts in a system. Such a view supports pluralism, because various systems can segment a conceptual space differently. I discussed Putnam, to argue for the necessity of an extensional theory of meaning that construes sign-vehicles as denoting not only concepts, but also things in the world. But given the systemic nature of signification, these things should not be understood as pre-delineated. Things become individuated within the symbol system, just as Saussure's concepts, hence the systemic interpretation of extensionalism supports pluralism. I found in Goodman's theory of symbols a theory that combines the systemic thinking of structuralism (even if he does not count Saussure among his ancestors, probably because of Saussure's mentalism, or because of some socio-cultural boundaries) with an extensional approach. Furthermore, Goodman (like most structuralists but unlike Putnam) does not restrict himself to verbal language, but develops analytic tools to investigate all kinds of symbol systems that include sounds, images, diagrammatic and pictorial, models, and all kinds of artifacts. His main contributions are the distinction between denotation and exemplification that allows for a description of things as symbols that are not sign-vehicles in the narrow sense, as well as the distinction between dense and articulated symbol systems.

I sketched an account of practice that fits with this view on symbol systems in Chapter 3. That is, an account of how actions bring about symbol systems, and an account of how symbol systems regulate actions. Since symbol systems *qua* systems are coordinated sets of alternatives of things and labels that generate the value of their members from the differences between them, actions that act on, or react to things must come in sets of alternatives as well, just like the labels that denote things. I used Gibson's concept of affordance to describe the relation between things and actions or behaviors, analogously to exemplification, but gave it a systemic interpretation in accord with the promoted view of symbol systems. Actions answer to things as individuated in symbol systems and are themselves part of these systems.

If things appear in symbol systems, then actions that result in the recognition of things, as many scientific actions, must be understood to create such systems, that is, they must be described as introducing differences between things relative to a given realm, which is delimited in another system. To introduce differences, such an action, or procedure as I called it in order to acknowledge its complexity, must be contrastive. I showed that such contrastive procedures can be distinguished according to their aim and focus, according to whether they aim at attributing parts or properties, classify individuals or partition them as well as according to their interest in causal relations. They can be further distinguished according to their use, which is either concept forming or concept demonstrating. A special case that is important for the pluralism defended here is the introduction of systems by fiat. Once systems are established, effects, labels and behaviors can refer to things and *vice versa*.

Finally, I argued that phenomena as represented in the material constellation brought about by the contrastive procedure can be interpreted as exemplifying in an articulated or dense symbol system, and accordingly afford different things in different situations. Most notably, phenomena exemplifying in a dense system (structures and processes) afford further decomposition, that is internal contrast, while in an articulated exemplificational scheme phenomena (objects and events) are taken as units that can figure in external contrasts. I spoke of thick and thin phenomena to subsume their syntactic, semantic and affording aspects.

Applying these analytic concepts to the case of the plurality of character concepts in biology, I was able to show that, at least in the few fields and periods I looked at, differences can be seen concerning what is contrasted (e.g. parts of organisms with other parts, organisms of one species with organisms of another, organisms with mutated conspecifics). I was further able to show that in some contexts characters figure as thick phenomena, while in others a thin character concept is used. This observation maps on the distinction provided by Colless (discussed in Chapter 4), who, mainly speaking about systematics and evolutionary biology, finds that characters are sometimes taken as parts and sometimes as variables that can take different values. Parts can be interpreted as thick concepts and the values of variables as thin concepts and these two conceptualizations are found also outside of systematics and evolutionary biology. In particular, I found in Chapter 5 that the anatomical and physiological disciplines, including embryology, a thick character concept is predominant, while in Chapter 6 I interpreted the history of genetics as a move from a thick to a thin character concept, as well as a move from characters as explananda to characters as data.

Mendel on my account introduced a thin character concept, but characters were still what was explained by his model. In the appropriation of his model by his re-discoverers confusion arose, because they interpreted the model in terms of thick parts. The confusion can be described with Goodman as a lack of fit of the character concepts, such as Bateson's, which therefore did not become entrenched. Johannsen analyzed the misfit and resolved the issue by proposing thin values of variables as the proper subject of a science of heredity that stand in a thin causal relation to genes that is expressed in the difference principle, while thick parts result from thick epigenetic processes, that are studied by developmental physiologists. Johannsen never fully rejected thin characters as the explananda of genetics, which I take to be a result of his intimate connection with practical contexts such as agriculture and the brewing trade, but after all, even in these contexts characters are not explained, but only predicted and manipulated through genes. Morgan and his followers completed the move to a thin character concept and relegated thin characters to mere data in the investigation of gene transmission processes. They agreed with Johannsen in thinking that thick characters were neither caused nor explained by genes and were the subject of developmental biology.

Integration, Reductionism and Cellular Phenotypes in Developmental Genetics

The analytic concepts developed in Part I should be useful to investigate the relevant contrasts and the resulting character concepts predominant in other branches of biology, such as phylogenetics and evolutionary ecology, as well as those in applied contexts, such as medicine or agriculture. Here, however, I restrict myself to pointing out that the analytic tools and the historical results achieved through their application in Part II should be helpful in addressing some important questions about contemporary biology and its interpretation. The questions I have in mind concern the issues of disciplinary integration and reductionist strategies in modern developmental genetics in the context of functional genomics.

Even though Morgan has separated genes from the explanation of thick characters, he did so in lack of an experimental system that allowed him to address the role of genes in development. But very soon attempts were made to create an appropriate experimental approach and developmental genetics took shape. With the rise of molecular genetics, “regulation” became the leading metaphor for the role of genes in development and physiological processes in general, and recombinant DNA technology provided new means for manipulating and detecting components of molecular regulatory mechanisms. While regulatory pathways were successfully dissected by forward genetic screens, the study of gene function through the alteration of gene expression gained new significance in the light of the data provided by the human genome project and the accompanying sequencing of model organism genomes. Novel genes detected in the sequence by computational means could now be targeted in the organism. Whether genome sequencing is considered a tool for the study of development and other physiological processes, or whether developmental and physiological genetics are seen as serving genomics in investigating gene function is a matter of taste (and funding opportunities).

In any case, if the explananda of developmental biology are thick characters, and genetics links genes to thin characters, how can developmental genetics bridge this gap that Morgan as well as contemporary embryologists expressed from either side of it? This is the problem of integration. How is the thin character concept of genetics integrated with the thick character concept of embryology? Integration in science has often been characterized as theoretical integration. Instead, if thick and thin concepts are characterized by different representational practices and afforded actions, then one shall ask how the practices that give rise to the different concepts are integrated.

The second issue I want to address is that of genetic reductionism[71]. If genes play an explanatory role in developmental biology and the explanandum of developmental biology is still the outcome of developmental processes, that is, thick characters, do genes ultimately explain thick characters? There are some philosophical commentators of biology that generate the impression that biologists give genes undue weight in the explanation of characters. I will argue that these authors usually do not distinguish between thick and thin characters and their different roles as explananda and data, respectively. Above I have also addressed thick and thin characters in the context of development and genetics as phenotypes-D and phenotypes-G, where the former refers to phenotypes as the outcome of a developmental process (a thick concept), while the latter describes phenotypes as

[71] I use the term in a very broad way here to designate debates on the nature of the relation between genes and characters. Two main strands can be distinguished. There are those debates in which it is discussed in which sense genes determine characters (genetic determinism). Everyone agrees that characters are the result of internal and external factors, but the question is if genes somehow play a special role in the determination of characters. Another strand discusses the value of centring on genes on the expense of other factors (gene centrism), which can be supported on the grounds that genes are suitable handles for manipulating organisms or decried on the assumption that such a focus, if it exists, will yield a biased view on the organism. Often accusations of reductionism are mixed in that they claim that there is an undue focus on genes and it is motivated by an alleged special role of genes, assumed by the proponents of reductionism (who usually are taken to implicitly have a reductionist view rather than explicitly defending it).

differential values of variables that serve as data to indicate genes (a thin concept). This echoed a terminology employed by Lenny Moss.

Moss introduced a distinction between Gene-P and Gene-D (Moss 2003). Gene-P has its roots in classical genetics. It is defined by the phenotype[72]. By tracing the hereditary transmission of phenotypes, a gene associated with the phenotype can be identified. A gene-P is unspecific with respect to DNA sequence, because the observed phenotype is mostly the result of the absence of a product derived from DNA. There are many ways this product can be absent, in that many modifications of the part of DNA that gives rise to the product can lead to a situation where the product is not or only incompletely produced, or where it is dysfunctional. Nevertheless, a gene-P can be located in a chromosome. And particular versions of the absence of the normal form of the sequence, that is particular alterations of the sequence at this locus, can be used to predict the phenotype, as it is done in genetic screening.

The sequence that gives rise to a functional product is instead a gene-D. "D" stands for developmental resource. A gene-D is defined by its sequence, but it is unspecific with respect to the phenotype. Gene products are usually active in a great variety of tissues and at various times and situations in the life of an organism, and not only in the structure that is most affected if they are dysfunctional.

Accordingly, BRCA1 is a gene-P for breast cancer in the sense that mutations in that gene allow to predict breast cancer. But the sequence of normal BRCA1, the gene-D, is not a gene for healthy breasts, but instead is a gene only for a protein that is present in almost every cell in the human body and which has various cellular functions.

Moss argues that it is the conflation of these two gene concepts that gives rise to the idea that genes contain the information for making a phenotype, which is also expressed in

[72] In Moss text the "P" stands for preformation, because the strategy of predicting phenotypes from genes-P is a form of instrumental preformationism, in that it treats the trait as being determined by the gene (and thus as somehow present in the gametes). I prefer, however, as warranted by Moss, to read the "P" as designating phenotype.

the metaphors of „genetic programme“ and „genetic blueprint.“ It is, however, not clear who holds this view. It is certainly, as Moss remarks, part of public discourse about genes. But within science it will be hard to find researchers who express a naive and simplistic view on the relationship between genes and phenotypes. However, in evolutionary biology, a straightforward relation between genes and normal phenotypes seems to be presupposed sometimes.[73] Another field, where straightforward relations between genes and phenotypes seem to be proposed is in agricultural and epidemiological genetics. But most people would agree that if Moss' distinction is applied, and these disciplines are seen as relating genes-P to phenotypes for pragmatic predictive rather than for explanatory purposes, the proposed relation is unproblematic.

Developmental geneticists certainly aim at understanding the development of the phenotype by investigating gene function. The main strategies employed, forward and reverse genetics, both rely on relating mutant phenotypes to genes that have lost their function due to mutation or knock-out, respectively. Does this imply that researchers in these disciplines grant a particularly prominent role to genes in the explanation of characters?

Sylvia Culp, in discussing the strategy of targeted gene knock-out, questions the assumption that this strategy can illuminate genotype/phenotype relationships, thereby assuming that this is what is intended. She describes the strategy as follows:

[73] For instance, Maynard- Smith (2000) argues that a gene represents a trait, since it has been selected for because of its contribution to the development of a trait, which is an adaptation to the organism's environment. The gene has a function in producing the trait, which in turn has a function in sustaining the live of the organism and lets it reproduce itself. A function, here, is the effect something is selected for. If the trait has a function, a gene responsible for that trait has a function too, even if there is no direct causal nexus between single genes and certain traits. It is sufficient that the gene contributes to the development of a trait.

> Gene targeting allows precise, predetermined changes to be made in a chosen gene. Once molecular biologists make this kind of change in a genotype, they look for change(s) in a phenotype. From these changes in a phenotype they infer a causal relation between genotype and phenotype (Culp 1997, S268).

Culp observes that studies of that type frequently meet the problem of identifying the changes in the phenotype. She distinguishes three situations. First, the phenotypic effect of the knock-out is different from the hypothesized (Culp 1997, S272). This point is not relevant, because sometimes loss-of-function studies are performed without any prior hypothesis concerning phenotype. It depends on how much is known beforehand about the gene-product. And after all hypotheses are meant to be put to test and to be rejected if necessary. Second, there is no phenotypic effect detectable (Culp 1997, S272-273). This point indeed came as a surprise to a certain extent when gene targeting was introduced. It seems to be often the case that the loss of function of one gene is compensated by the system, that is, that there is a certain amount of redundancy in genetic networks. But from this perspective such results actually provide important insights in the architecture of such genetic networks and thus rather add information to the way genes contribute to the phenotype than being an obstacle. If no phenotype is detectable, it is also possible that a more fine-grained screen for phenotypes is necessary. This leads to Culp's last point. Phenotypes might only be visible under highly artificial conditions. She writes "It is disturbing, however, that [the scientists] could identify an unexpected phenotypic change only because they placed the gene-targeted mice in a particular circumstance." (Culp 1997, S274)

This observation, however, points out exactly the problem with Culp's criticism. She conflates the phenotype as something that undergoes changes when a gene loses its function, and the phenotype as the outcome of the developmental process that is explained only indirectly through the causal function of genes. If the knock-out of a gene has an effect in any structure of the organism, this does not mean that the gene in question is a gene for that structure or even for the normal development of that structure. Therefore, the phenotype can be something as obscure as performing well in some laboratory behavioral test. Such a test is just a contrastive procedure, employed to identify and classify an entity by its effect. What she fails to distinguish properly is the two concepts of phenotypes involved. One is the mutant phenotype and its wild type pendant, defined by contrast, the phenotypes-G. The other is the phenotype-D as the actual anatomical or physiological part or capacity that is the outcome of a developmental process.

Sandra Mitchell also discusses the problem of redundancy in interpreting gene knock-out experiments (the lack of phenotypes due to compensation). Her point is that in robust gene networks, it is problematic to make causal claims on the basis of interventionist reasoning. But it seems that Mitchell as well does not see the phenotypes in question as thin data for gene interactions, but as the explanandum concerning which such experiments collect bits of an explanans. At least, she sees such experiments as aiming at formulating gene function in terms of phenotypes. She writes:

> What is the causal relationship between genes and phenotypic traits? Undoubtedly this is an extremely difficult question to answer, yet technological developments in genetic engineering continue to provide new tools of intervention.

She continuous by briefly explaining what a gene knock-out is, to go on in characterizing the alleged conclusions drawn from such an experiment.

> Differences between the normal organism and the double knockout mutant, in particular the presence in one and absence in the other of the expected phenotype, indicated the function of the normal gene.
>
> The logic of these experiments is typical of controlled experiments. That is, if the gene that is knocked out is a component cause of the phenotypic trait, successfully eliminating its contribution should issue in a change in the trait.

The change reflects the contribution or function of the knocked out gene. (Mitchell 2008, 700)

Developmental biologists certainly made use of the notion of a genetic program, that Moss criticized, but this already implies a complex interaction among genes instead of a relation between genes and phenotypes. The critics of gene centered explanations of phenotypes certainly do not accuse biologists of proposing one-one relationships between genes. They acknowledge that biologists are aware of the complexity of developmental process and its many causal influences, genetic and environmental (who else should be – after all, philosophers can only know about the complexity from the biologists). But still, these authors seem to claim that biologists present genes as making causal contributions to phenotypes and thus as having some explanatory force with respect to phenotypes, or, to put it differently, that bringing about these phenotypes is part of the function of the gene in question. Another author who exemplifies this view is Jason Scott Robert, who after correctly pointing out that some form of simplification is a necessary heuristic in the study of causal factors in complex systems, characterizes developmental genetics as following the premises listed below (Jason Scott Robert 2002):

> Genes by themselves are not causally efficacious, as genes and environments (at many scales) interact (differentially, over time) in the generation of any phenotypic trait. (p. 978)
>
> We decide to focus on the causal agency of genes against a constant background of other factors, for pragmatic or heuristic reasons. (p. 979)
>
> A trait *x* is caused by a gene *y* only against a constant background of supporting factors (conditions), without which *x* would not be present (even if *y* is present). (p. 979)

This is correct, if it describes the way that phenotypes-G are conceptualized as valid indicators of genes, that is, as a description of the observation theory that guarantees the validity of phenotypes as data for genes and their interactions. But the author seems to suggest that by this reasoning biologists present genes as causes of phenotypes-D. He continues his reconstruction of developmental geneticists' reasoning like that:

> Therefore, organismic development is a matter of gene action and activation, as particular alleles have their specific phenotypic effects against standard environmental backgrounds. (p. 980)

In the following, I will use as a last case study an example from recent zebrafish developmental genetics[74] to illustrate that thin phenotypes-G are used only to study causal relations *among* genes and that the causal relations between genes-P and phenotypes-G is only relevant in the sense of an observation theory, just as in the case of the Morgan school's study of gene transmission. The observation of phenotypes-G clearly does not establish causal relations between genes (-P or -D) and thick phenotypes-D. Phenotypes-D, instead, are explained by cellular dynamics. The genes-P identified through phenotypes-G can be interpreted as genes-D, but then they lose their connection with phenotypes-P. Instead they can enter mechanisms either as thin objects in thin causal dependence relations with other genes or as thick structures, that give rise to other structures (RNA, proteins) that are involved in thick processes. Such mechanisms can contribute to the explanation of cellular dynamics. It is important to note again, that genes-D are usually active in all kinds of tissues, not only those related to the phenotypes-G.

There is an event that has an almost mythical character in the zebrafish community, as becomes apparent in the label people use to refer to it: The Big Screen. It refers to two coordinated large-scale mutagenesis screens performed in the mid-1990s (Haffter et al. 1996; Driever et al. 1996). Christiane Nüsslein-Volhard, who already had eminent success with this strategy before, in studying Drosophila development, initiated the screen. While

[74] On the history of zebrafish as a model organism and the general role of model organisms in constructing phenomenological models of developmental mechanisms, see Meunier (unpublished manuscript).

in the *Drosophila* screens, the mutants selected were limited to those affecting very early embryonic patterning, the zebrafish screens also targeted early development, but up to a stage where organs are formed in the larvae. The strategy was to screen for mutants on a large scale, but to select only those mutants that affect the process of interest, such that with respect to this process all relevant genes are detected, without relying on any preceding idea concerning which genes or how many this might be. This is what is called a saturation screen for a specific phenomenon, in this case embryonic development. Analyzing not only the effect of the loss of function of a single gene on a phenomenon, be it the architecture of the nervous system or the patterning of early embryos, but instead identifying all genes affecting one phenomenon when mutated, allows geneticists to hypothesize interactions among genes and thus molecular level explanations. The researchers reporting the screen write:

> There are a number of cases where small groups of genes are responsible for the same or a very similar phenotype. [...] The similarities in phenotype suggest that the genes within the group participate in the same developmental process. [...] The molecular analysis of most of them has now complemented the phenotypic analysis. In the majority of cases, genes producing similar, but not identical, phenotypes are participants in the same pathways and often interact. (Haffter et al. 1996)

Similarity among effects here is a heuristic to construct hypotheses on the interaction of causes. The classification of genes by the temporal order of their activity and their effects on the patterning already provide a basic structure for a model of their interactions. Thus material collections of mutants constitute proto-models of molecular pathways.[75]

With respect to the heuristic role that phenotypes-G play in the practice of developmental genetics, it is important to notice here that they do not indicate genotype-

[75] James Griesemer's study on Joseph Grinnell makes a similar point concerning collections of appropriately annotated specimen of the local *fauna* in California as first steps towards theoretical models, in this case of ecological relations (Griesemer 1990).

phenotype relations, but are used to identify causal relations *among genes* by way of similarity of phenotypes-G that are found to have different genotypes by complementation analysis. It is the *genetic interactions* identified that figure in more complex explanations of phenotypes-D, but as we will see only via the phenotypes of cells that harbor them. Only in that sense the phenotype-G selects the elements that figure in the explanation of phenotypes-D, but these are usually more phenotypes-D than those directly related to the phenotypes-G in question (by relation I mean what can be called the *locus* of the phenotype-G in the organism that can be associated with a phenotype-D).

To show how models of genetic interaction and cellular dynamics are built, I will briefly follow one of the mutants identified in the screens: *one-eyed pinhead* (*oep*), thus named because of its cyclopic phenotype.[76] Different alleles of the *oep* gene were identified in Tübingen and Boston. The mutant was, as most others, assigned to several classes of mutant phenotypes, according to the different structures or processes affected at different stages during development. *oep* was described as being involved in midline/notochord formation (Brand et al. 1996, 133), dorsal-ventral patterning and brain development (Schier et al. 1996, 168) and as "affecting morphogenesis during gastrulation" (Hammerschmidt et al. 1996, 143,145). Many of the mutant categories were motivated by an interest in the study of the patterning of the very early embryo, which lies at the heart of embryology. During gastrulation, the cells generated during the cleavage period form the three primary germ layers. These give rise to different tissue types and their organization along the embryonic axes instantiates the early body plan.

[76] According to the convention in the field, gene symbols are italicized and lower case, whereas the protein symbol is non-italic with the first letter upper-case (https://wiki.zfin.org/display/general/ZFIN+Zebrafish+Nomenclature+Guidelines, accessed September 26, 2011).

Subsequently, the labs of Alexander Schier and William Talbot, in cooperation, published a study further elaborating the functions of Oep (Gritsman et al. 1999). The main result was that Oep is an essential component of the Nodal signalling pathway. Nodal is a signalling molecule discovered in mouse that plays a critical role in the early patterning of the embryo. Its name is derived from the node, a region in the mouse embryo that corresponds to the Spemann organizer in Amphibians[77]. The two genes *cyc* and *sqt* were found to be orthologs of *nodal* in Zebrafish and their products are, therefore, collectively referred to as Nodal signal. A guiding observation for the experiments in question was that the phenotype of the double mutant for the *sqt* and *cyc* genes is very similar to the *oep* mutant. This similarity and the fact that the *oep* mutation affects embryonic processes associated with Nodal signaling, together with the knowledge that the protein is membrane associated, but acts cell-autonomously — i.e., that it does not transfer the mutant phenotype to cells with a wild type genotype, as determined by mosaic development studies — led to the hypothesis that "Oep is required for cells to receive Nodal signals" (Gritsman et al. 1999, 125). It becomes clear how carefully comparing and relating different mutants from the large collection available already provided the basic outlines of a model. To test whether Oep is essential for Nodal signaling and where it is located in the pathway, two experiments were performed:[78]

1) Phenotypic analysis already showed that Oep is necessary for Nodal signalling, but it might have been just a signal-amplifying element. In this case overexpression of the Nodal signal (through injection of *sqt* and *cyc* mRNA) should rescue the *oep* mutant phenotype or even lead to dorsalization (excessive growth of dorsal tissues). The result, however, was

[77] In 1924 Hans Spemann and Hilde Mangold published their experiments in newt on the induction of a secondary embryo in a host embryo upon transplantation of cells from a particular region in a donor embryo, named the organizer, into the host embryo shortly after gastrulation (Spemann and Mangold 1924).

[78] Though the whole study as published consisted of more experiments.

that, whereas overexpression of these genes, alone and in combination, leads to dorsalization in wild type embryos, there is no rescue effect or even dorsalization observable in *oep* mutants. The conclusion was that Oep is indeed essential for Nodal signaling during embryogenesis (Gritsman et al. 1999, 125).

2) There was evidence available that Nodal signaling in the cell is mediated by a pathway involving the ActRIB receptor and the Smad2 transcription factor. Another experiment then consisted in injecting mRNAs of the genes coding for these factors in the *oep* mutants. Here the result was that the *oep* mutant phenotype was rescued. The conclusion was that the Nodal signal is indeed transduced by this pathway and that this requires Oep, which acts upstream of these components (Gritsman et al. 1999, 125).

These experiments thus "identify Oep as a novel and specific component of the Nodal signaling pathway" and localize it as an extracellular co-factor that is required for Nodal to activate the downstream elements in the pathway (Gritsman et al. 1999, 128). By integrating further mutants or reagents to the model, further elements have been added to the pathway (e.g. Bisgrove et al. 1999). What clearly indicates that here the phenotype is thin data, is that it does not play any role for the investigation of the pathway that the phenotype is cyclopic. Indeed, it does not matter where in the embryo a phenotype is observed. Some changed expression pattern of a marker gene in earlier stages serves the same purpose. And, as we will see, when it comes to explaining phenotypes-D, the pathway's contribution to different processes than only the formation of eyes can be investigated.

Pathways have to be related to cellular phenotypes, which in turn explain broader developmental or physiological processes. Another study in which the Schier lab was involved illustrates how the genetic interactions investigated in the previous study are put in the context of cellular phenotypes that figure in thick morphogenetic processes that enable explanations of characters as parts in developmental biology, or phenotypes-D. In this case the study aims at heart morphogenesis, especially concerning the asymmetric morphology and position of the organ (de Campos-Baptista 2008). The example also shows that for such an approach the same elements, involving *oep* mutants that have been used to construct the phenomenological model of the Nodal signalling pathways are now used to construct a model of the Nodal pathway's role in heart organogenesis. The point is that the model is extended by adding contrastive tracking procedures that allow the researchers to represent morphogenetic processes. In particular, the model now includes transgeneic animals that express the fluorescent protein GFP in all cardiomyocytes throughout cardiac morphogenesis and *in vivo* 4D confocal time lapse imaging, aided by the appropriate analysis software. Movements of cell populations are contrasted with other populations by means of the GFP marking and with cell movements in mutant animals. The representation is dense and taken to be open for further decomposition. The authors establish a scheme to analyse cellular movements when they quantify speed, displacement, and meandering of a cell: "Speed was defined as distance moved/time interval, displacement rate as distance from first to last location/ entire time interval, and meandering index as total distance traveled/ displacement." (de Campos-Baptista 2008, 3627) The analysis of processes yields sub-processes or parameters. The parameters used are partly established standards in studying cell dynamics, but the observation is still open for all kinds of peculiarities of the process. In short the process and the part it brings about are thick phenomena. The results are formulated like this:

> Our quantitative analysis of LZ*oep* mutants shows that loss of Nodal signalling affects the direction of movement of individual cardiomyocytes during early heart development. Anterior movement is reduced and leftwards migration is abolished. In addition, our results show that loss of Nodal signalling also affects the speed of cardiomyocyte movement. Cardiomyocytes move more

slowly and meander more, resulting in strongly reduced displacement. The differences between anterior/posterior, left/right, and atrial/ventricular cells are reduced or absent. Taken together, these results suggest that Nodal signalling provides a directional cue and promotes the speed of heart cells. (de Campos-Baptista 2008, 3631)

When, above, I gave the impression that mutant phenotypes (and their wild type counterparts) are exclusively interpreted as thin indicators of genes and their interactions, I have to point out now that mutational phenotypes can also be thick phenomena. As such they are amenable to detailed distinctions in terms of their cellular components. Certainly, developmental biologists sometimes pick sub-collections of mutants from a screen (or screen only for specific effects), because they are interested in the affected parts or capacities as phenotypes-D, and not only in order to find out something about molecular pathways that actually operate in many tissues. But when mutants are interpreted as thick phenomena, they offer information about cellular processes, not genes. Looking at a deformation such as cyclopia, might, for instance, suggest some problem with cell migration at certain stages in development. This sets the cellular level explanandum that can be related to molecular mechanisms.

The examples show that while phenotypes-G are used to establish causal relations among genes, the genetic mechanisms thus established are related to cellular behaviors that in turn constitute thick processes that can explain thick characters (e.g. parts like the heart, or their structural properties such as the hearts asymmetrical structure and position). The link between genes and thick phenotypes must go via the cell, because we saw above that it is part of the thick character concept that characters are composed of cells. And complementarily, Brian K. Hall calls the cell the gene's "locus of operation" (Hall 2001, 226). This role of cellular phenotypes mediating gene action and organismal phenotypes-D has not always been fully acknowledged by philosophical commentators of experiments in developmental genetics and gene function studies. The relation between molecular mechanisms and cellular phenotypes is itself not straight-forward, but it is much less remote than that between genes and organism level phenotypes. I suggest that an answer to the question of how molecular mechanisms can be connected to cellular phenotypes lies in the flexible character of mechanistic models with respect to containing thin and thick components. Some thick entities and interactions will be known while others in the vast network of interactions are black boxed by thin causal dependence relations such that mechanisms including thin components can explain thick phenomena. But this idea has to be developed elsewhere.

Concerning the question of integration, it can be said that the thin character concept of genetics has been integrated with the thick character concept of embryology, 1) by employing representational practices of both fields[79] and 2) by moving from thin objects (phenotypes-G) to thin mechanisms (gene interactions) that do not explain these objects but that can successively be turned into thicker mechanisms (protein interaction can be thin or thick) and that can explain some thick medium level of organization (cellular phenotypes and the processes they are involved in), which finally have some explanatory power concerning thick phenomena on the same level as the thin phenomena the investigation started with (organismal form). This might instantiate a generalizable pattern that applies also to other sciences like economics and sociology, where there are similar conflicts between thick and thin concepts.[80] This, however, is another intuition that has to be developed elsewhere.

[79] Rheinberger describes integration as the merging (hybridization as he calls it) of experimental systems and their modes of representation (Rheinberger 1997, Ch. 9). And even if integration does not result in a new field with its own experimental system, but only consists in more cooperation between fields, this happens among other things through the exchange of representational practices (Griesemer 2006).

[80] Indeed, the notion of thin phenomena can be traced back to Marx' discussion of political economy (Marx 1903) as has been pointed out to me by Staffan Müller-Wille.

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