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Epistemic Styles in German and American Embryology

The Argument

This paper argues that different epistemic styles exist in science, and that these make up an important unit of analysis for studying science. On occasion these different sets of commitments to ways of doing and knowing about the world may fall along national boundaries. The case presented here examines German and American embryology around 1900 and shows that differences in goals and approaches make up different epistemic styles.

In particular, the Germans sought causal mechanical explanations of as many phenomena as possible, guided by strong theories which achieved confirmation when they fit with as much of the available data as possible. The Americans, in contrast, sought definitive facts, as many as possible, which might be quite specific or narrowly based. These facts could lead to empirical generalizations which, in turn, could guide the generation of new knowledge in the form of new facts. Thus, the two epistemic styles emphasized different goals, processes of investigation, and standards of evidence.

Introduction

Near the end of the nineteenth century, something had changed in embryology. It was more than that it had become a progressive, productive science. It was more than that Americans were joining the leaders in the field, though that was a part of the change. The epistemic style in the United States was somehow importantly different from that in the Old World. To see what these changes and differences were and why they were important will require first a brief overview of the field and then a clarification of what is meant by "epistemic style." Only then can we look more closely at the character of German and of American research in embryology to see why the differences between them deserve to be called epistemic styles.

By the end of the nineteenth century, embryology had in fact become a progressive, productive science. Prior to the 1870s it had achieved some successes but had become self-consciously stalled by the lack of available new information. The problem was that embryologists wanted to know how the initial germ gives rise to a complexly functioning, highly differentiated adult organism of the proper sort. Yet after a series of studies on the relatively accessible chick's egg and a few other species, by the 1860s embryologists had no scientific way of gaining significant further information. They could peer at their embryos through microscopes and see changes, but not in sufficient detail to reveal how development occurs. Furthermore, in the most familiar and readily available species of birds and farm animals, there was no way to see what happens during the earliest developmental stages, since the initial germ (which by the mid-nineteenth century meant egg) remains inside the mother, invisible to the outside observer.

What is that initial egg like? they asked. Is it already differentiated or not? What role does the father play in initiating or directing development? What processes drive development, and do they primarily lie within the egg itself or outside? What causes the embryo to turn into the right sort of adult rather than into a monstrous aberration or hybrid? Their metaphysical convictions about the way the world is structured might provide answers for them. Thus they might respond, with Aristotle, that the organism assumes the right form because of some directive formal and final causes. Or they might insist that the father provides the formal cause, which crucially directs development of the passive material egg. They might appeal to some sort of preformationism whereby the future adult is assumed, for logical or metaphysical reasons, to lie already preexistent in the germ. Embryologists raised these questions and suggested alternative theoretical responses, but they had no way to answer them scientifically until the late nineteenth century.

The 1870s and 1880s brought major advances. Improved microscopes, a host of advanced microscopic techniques, and the availability of new organisms changed the conditions of research. With the 1880s came the production of apochromatic lenses, using different types of glass with different optical properties. This caused the different colors to focus at the same or very close points, an improvement that eliminated chromatic aberrations and allowed observers to use higher-power objectives. This in turn made possible the effective use of photomicroscopical recording of results. Combined with the oil-immersion lens developed in the 1870s, which increased the resolution of images, the microscope facilitated observations of very small organic structures and processes (Bradbury 1968, esp. 166–78, 185–92). Microscopic techniques also improved about the same time. Throughout the 1870s and 1880s, researchers worked at improving the microtome so that it could make regular, very thin serial sections of a single organism. They devised methods for fixing the specimens in as close to normal conditions as possible and for embedding organisms so that the edges did not curl, for example. Staining also received considerable attention, since organic tissues are usually nearly transparent or homogeneous in color; differential staining of tissues and parts thus revealed details not otherwise observable (Bracegirdle 1978, chaps. 4 and 5). New organisms helped too. Marine organisms, in particular, provided eggs that were often large, observable from a very early stage of development, and fairly fast developing. This was true also of frogs' eggs, which achieved wider use in the 1880s.

Revised epistemic convictions altered the situation even further. The general assumption had persisted that one can only do science (i.e., study nature) if one does not do violence to the natural subject under consideration. In biology that assumption had seriously restricted the sorts of investigations possible as long as living organisms were thought to be special, vital phenomena distinct from inorganic phenomena and as long as the organism was considered as an integrated, interactive whole. If each organism represents such a vital whole, then it cannot be studied using laboratory methods without doing violence to its very nature. Nondisruptive scientific study of developing organisms appeared to have reached its limits. Yet by the 1880s these assumptions about the limitations of proper investigation had begun to change. First, the materialistic implications of the Darwinian theory of evolution and current medical advances had dissolved the supposed dividing line between organic and inorganic phenomena. Second, researchers increasingly accepted a reductionistic program that separated the organism into its various parts for purposes of scientific study; one no longer needed to study the organism solely as an indivisible organic whole.

By the 1880s, then, researchers had begun to accept cytology (the microscopic study of cells) and histology (the microscopic study of tissues) as legitimate and productive scientific approaches to understanding living phenomena. Cytology rejected vitalism and treated the cell, including the egg cell, as material that obeys basic physical and chemical laws. Histology assumed that the study of parts can reveal valuable information about the nature of living structures and functions. Both can yield knowledge in ways that holism and vitalism cannot. A growing number of researchers, though not all, accepted the validity of these scientific approaches by the 1880s.

By that time, the general embryological assumption was that development begins when the spermatozoon cell from the male parent and the egg cell from the female parent unite to produce a new cell, which constitutes the starting point for the offspring. Each cell was thought to contain inherited tendencies from the past, which serve to provide stability and to preserve continuity between generations. In addition, each cell holds the potential to develop and to produce variations away from the past. The fertilized egg cell, as a consequence, holds a mixture of past adaptations and future possibilities.

In addition, by the early 1880s many embryologists had come to assume that the first important stage of development occurs when the germ layers have been established. Whereas earlier development merely involves growing bigger, germ layers hold the key to later development and differentiation. Only with the germ layers, it was assumed, is the egg sufficiently fixed as to its later fate, and only at that stage is it subject to true adaptation. Such an assumption made it less interesting to examine the earliest stages of cell division and development. Only as this germ layer doctrine came to be widely called into question in the course of the 1880s did considerable attention focus on those earliest stages. This meant that new equipment, techniques, organisms, and questions converged during the 1880s. As we shall see, they also coincided with the coming of revised epistemic styles, facilitating the generation of productive alternative research programs.

Epistemic Styles

By epistemic styles, I do not mean something quite like Ian Hacking's "styles of scientific reasoning." As Hacking realizes more than most philosophers, science consists of experiments as well as reasoning. Science is not just thinking or just theorizing. It also involves doing. Therefore, part of an epistemic style is the way in which scientific work is done at any given time for any given individual or group of individuals; and sometimes it is also localized in a given place. But it is more than that, too. An epistemic style involves commitments as to what one seeks as the outcome of science, as to what count as appropriate procedures for gathering knowledge, and as to how to know when one has achieved knowledge at all. Sometimes these are treated as independent categories of scientific goals, process of discovery, and process of justification, as if these were really separable and distinct aspects of science. Or they are lumped together as tacit assumptions made by scientists, with the view that they cannot be investigated further. Yet they clearly are all interactive and mutually necessary parts of science. One cannot carry out a process of discovery without some understanding about the goal for that discovery and what counts as having discovered something. These sets of commitments are plainly epistemic, and they cluster into different ways of doing science.

The differences are epistemic because they deal with ways of coming to know, what counts as knowing, and what count as the objects of knowledge. Moreover, they characterize patterns of practice (or the doing of scientific work) rather than the content of theories. As patterns of practice they may persist through time for a particular group in a particular place. Thus, styles overlap with traditions. Larry Laudan's definition of traditions certainly may include what I am calling style. He claims that research traditions do all of the following: include assumptions about what counts as noncontroversial background knowledge, help indicate which parts of a theory are in trouble, establish rules of data collection and theory testing, and pose conceptual problems (Laudan 1981, 151). He also recognizes that they reflect a great deal more directly than is traditionally acknowledged the social and intellectual norms of the culture in question. But such traditions concentrate, for Laudan, around theories and their revisions. As such, they incorporate dominant metaphysical convictions that may pervade a culture and that form far-ranging and long-lasting patterns of thinking. Instead of focusing on theories, I am much more concerned with the actual, practical, empirical process of doing science: the styles of carrying out scientific work.

Different localized schools may coexist within a style, I think, just as different styles may coexist within a tradition. Such schools refer to localized groups of individuals sharing a general set of concerns and doing similar sorts of work. Individuals within a school will most often share epistemic convictions and will also carry out research projects that address the same sorts of questions and follow very similar lines. Individuals within a school will generally talk with one another and work together, comparing results and projects in a much closer way than will participants in different schools. Their concrete everyday research practices will generally exhibit considerable overlap. Schools also tend to be shorter-lived, as new problems, new approaches, and new leaders emerge and generate new schools to replace the old. Styles of work may tend to persist longer, as traditions also do.

A standard definition (from various editions of *Webster's*) suggests that a style is a "manner or method of acting or performing especially as sanctioned by some standard." In contrast, a tradition has a historical component and acts as an "inherited pattern of thought or action." Meanwhile, a school consists of "persons who hold a common doctrine or follow the same teacher." Such definitions seem more or less accurate. They make the tradition seem somehow larger and the school more localized, but this is not just a hierarchical layering. By analogy, we talk comfortably in art about, say, the *tradition* of German Romanticism, the *style* of Botticelli, and perhaps the *school* of Rubens. Similarly, we might speak of the *tradition* of German biology, the *style* of reductionist physiology, and the *school* of Carl Ludwig. The progression is qualitative more than quantitative. The categorizing allows for different sorts of traditions, styles, and schools as well.

Different epistemic styles exist, then, as historians and philosophers of science have finally begun to recognize. The myth of the perfectly unified science, with its one scientific method, has finally begun to give way in the face of historical evidence to the contrary. Yet many still insist that at any given time a particular way of doing science prevails. They want to preserve the idea of a uniform science and the assumption that there exists a proper, or at least a dominant, scientific method, even if only temporarily for a given time and/or place. Thus there is Renaissance science, eighteenth-century science, or what have you. Some have pointed to the generation of new styles, especially by Galileo or the "experimental philosopher" Robert Boyle. Some have made claims about, say, French or Italian science, suggesting that there are national sciences rather than just one science for all. But few have directly considered whether such differences involve different ways of doing science: are there epistemic styles for science? I shall argue that at least sometimes there are, although I do not claim that these epistemic differences must necessarily be national or even localized, or that they must persist for long.

Because this paper was originally written for a conference on national styles, it concentrates on a case that represents an apparent national difference in styles. I would certainly not go further to say that the national unit is always basic in determining style. Insofar as a different national cultural or historical tradition shapes the epistemic commitments for science, national boundaries will coincide with different styles. Yet so many other factors also influence epistemic convictions that purely national styles may be relatively rare. For present purposes, I am convinced that German embryologists — and not just the few famous individuals considered here — did see different goals and approaches as proper for embryology than did their American counterparts. I provide empirical examples to establish that claim. What is needed next is a larger study of more people in more places, to determine the national character of the styles in question, as well as to extend understanding of those styles and the way they derive from and influence scientific practice. Only then could one begin to explicate what is peculiarly "Germam" or "American" about the styles the embryologists adopted.

Something can, of course, be a national style without every individual within the country in question adopting it; and there can, of course, be variations within a style. Moreover, a style can exist outside its initial home. Thus we can speak of Victorian architecture in Chicago, for example, or Italian baroque music in the twentieth century. Or, I think, we can consider a German embryological style carried out by an American. Styles also do not fall into neat dichotomies. Therefore the common tendency to categorize thinkers into distinct categories — ancients versus moderns, inductive versus deductive thinkers, lumpers versus splitters, materialists versus idealists (or vitalists), theorists versus experimentalists, or epigenesists versus preformationists — necessarily fails to capture the nature of the style I seek here. There are, it might seem, two types of people: those who divide people into two opposing categories and those who do not. I do not. (Holton 1978, 148–51, discusses the tendency to dichotomize.) In actuality, elements of styles may overlap.

I wish to demonstrate that German and American embryology, especially of the decades following the 1880s, manifested different styles. First, however, let me extend the caveat. It is not necessarily the case that all individual embryologists of either Germany or the United States, respectively, agreed on all aspects of their epistemic styles. Nor is it the case that the style necessarily persisted through all time, since different boundaries, commitments, and allegiances exist at different times and places. What I mean is simply that the probability is considerably greater that an American researcher in embryology around the turn of the century will have adopted a particular epistemic style — that is, will have done science in a way that conforms to what I have termed the American style. Similarly, the German researcher will with a great degree of probability have adopted a German style. This does not preclude a single embryologist, such as the black American Ernest Everett Just, from having adopted a German style. Nor a German such as Theodor Boveri from occasionally doing science or speaking in a way that sounds more like his American contemporaries. Such apparent discrepancies do not cause problems for the suggestion of the coexistence of different dominant styles as long as most remain within the "right" camp.

One writer of a *Science* article in 1883 identified what he (presumably he, the article appeared anonymously) saw as larger national traits in science. "The scientific writings of each nationality are characteristic," the author wrote, "and, taken as a whole, offer in each case distinctive qualities" ("National Traits in Science" 1883, 455). German science, the writer acknowledged, was then dominant and distinctive, emphasizing the value of original empirical investigation. Yet the German scientist can neither write well nor think clearly; his "profundity is mysticism," and therefore "German science is the professional investigation of detail, slowly attaining generalizations."

In contrast, the critic asserted, American science suffers from a lack of sufficient attention to investigation. Americans are much busier "professing" what is already known than generating new and original knowledge. This is the primary problem with

American science and the principal need for change. According to this writer, the "little band of men" in the United States who emulate the German model are leading the way for "the intellectual elevation of their country." This assessment holds a great deal of insight but also a limited view of the situation, which at any rate began to change rapidly in the course of the 1880s and 1890s. Yet it illustrates that national styles of doing scientific work were perceived to exist even a century ago.

Rather than attempting to assess all of science, as the above writer did, or even all of biology, I wish to focus on embryological study. The 1880s brought a great flurry of activity in this general area of research in Germany, followed shortly by a similar excitement and activity in the United States. Let me explore the evidence that epistemic styles existed, describe the nature of the differences in German and American styles, and offer suggestions about what follows.

A German Style of Embryology

German embryology (the interconnected study of both heredity and development) had taken on its post-Darwinian form through the suggestions of Ernst Haeckel and others. They maintained that the embryo reflects its evolutionary history and in fact records the history of adaptive changes. Thus, the ontogenetic development of the individual in some admittedly imperfect way recapitulates its phylogenetic past. According to Haeckel and most of his contemporary evolutionary morphologists, the germ layers of the embryo are the material of adaptation, and they thus give the earliest clues to phylogenetic history. Earlier changes simply involve the vegetative proliferation of living material.

Yet not everyone accepted this latter point. Some, including German anatomist Wilhelm His, sought a proper place for the embryological study of individual development from the egg stage onward, with every stage holding significance for later developmental stages. It might, he pointed out, be mechanical rather than evolutionary causes which dictate that the particular embryo will develop as it does.

In a published book of letters ostensibly addressed to his nephew, His first made his case for a mechanical causal explanation of development, which was what he felt embryologists should be seeking (His 1874). He insisted that evolutionary history actually *explains* nothing about what *causes* development. In the explanatory account he offered, each organism begins as a set of organ-forming germ regions, each of which then "grows up" to become its proper predetermined part. According to His, fertilization stimulates the processes of "form-producing growth." A complex series of foldings of the egg material into tubes as well as other bendings and rollings then turn the initial germ regions into their designated parts in accordance with a theory of "transmitted movement." The process, as His told his nephew, is rather like that of relaying a telegraph message: we cannot see the process at all, but we do witness the end result and infer what the process must have been.

What His sought in his science was an explanation of life processes in terms of mechanical causal actions. He offered a bold theory, buoyed by its consistency with the data of embryonic development. It was certainly not the case that he based this theory on the direct observation of any organ-forming germ regions; he had only theory to suggest that such regions exist. Nor was it the case that he had observed the rollings and foldings of embryonic tissue; again, he had only theory to indicate that they must occur. The theory offered an explanation of how each individual organism develops from its initial unformed state to a highly differentiated adult. There was, in His' view, no alternative explanation that worked. Haeckel's appeal to an evolutionary past offered no explanation at all, for it could not explain the physiological process of differentiation and change. His ridiculed Haeckel's pretensions to be doing science, saying that Haeckel was only playing a word game. "All these words, which are capable of strengthening a heart thirsty for knowledge, come into use: parental material, molecular movements, life quantities, protein, form and protoplasm. 'Misce, fiat explicatio!' so runs the enlightening formula of our clever Doktor, and with this stroke he opens his eyes to all secrets of generation and life" (ibid., 144). For His, the goal of science must be to provide a causal explanation, in terms of localized, or proximate, mechanical action.

Not many accepted His' particular theory. Nor were many convinced that his organ-forming germ regions even exist in the terms in which he presented them. But he stimulated many researchers to think about whether they did actually wish to seek explanations through their science, about what counts as an explanation, about how one knows when one has achieved knowledge by way of an explanation, and about what counts as confirming a particular theoretical explanation. In other words, His' posing of a strong and definite theory stimulated considerable epistemological interest in what science should be like.

Others responded with similar attempts to deal with embryological phenomena in ways they considered appropriately scientific. Wilhelm Roux, August Weismann, and Oscar Hertwig provide particularly interesting examples for our purposes. Roux and Weismann each believed that science must proceed through posing and assessing broad, bold theories. Roux came, in the course of the 1880s and 1890s, to worry more explicitly about what constitute legitimate approaches for gaining scientific knowledge. He argued with increasing ardor for experimental approaches to the mechanics of development, or what he termed *Entwickelungsmechanik*. Embryology, he said, should follow physics and chemistry — namely, with interventionist experimental approaches for which the researcher poses questions and manipulates the phenomena to get answers. The ultimate goal is causal explanation, meaning an explanation of each phenomenon in terms of prior causes (always mechanical for Roux), which always seeks earlier and earlier causes until the search reaches the intrinsically unknowable first causes.

Roux felt that embryology had passed through its proper path to knowledge — its inevitable descriptive, observational, and comparative stages — and needed now to move on to an experimental attack on development. Roux's understanding of experimentation corresponded with traditional "experimental philosophy," in which the experiment is seen to produce new information, to extend knowledge, and to serve in testing hypotheses or judging existing interpretations. "The experiment is the artificial production of conditions of phenomena, the artificial combination of factors, in order to see what will happen because of them and in order to gain consequently a clarification of their influence" (Roux 1912, 140). But scientific application of experimentation also requires theory, according to Roux. Experimentation provides data; theory uses those data to build up knowledge about causes. Thus theory, for Roux, is the proper goal of science. Even speculative theory is justified, if it is consistent with known facts and simple in nature — not unnecessarily complicated (Roux [1894] 1986).

In accordance with his commitments, Roux postulated the occurrence of qualitative nuclear division, according to which the inherited material in the nucleus undergoes a division that separates differential information into separated parts of the dividing egg — starting during the earliest stages of cell division (Roux 1883). For Roux, there are no predestined or morphologically differentiated germ regions, as His had suggested. Rather, the initially homogeneous cell material undergoes separation and specialization in response to the mechanical causes carried somehow in the nuclear material.

Weismann held a similar view. Somehow the inherited nuclear material becomes distributed to the different body parts. Weismann put forth an ingenious and intriguing theory about how the particulate determinants of heredity are organized as ids, idants, determinants, and biophores (which he saw as corresponding to chromosomes and their parts). These were all nonobservable entities at that time. The postulated qualitative nuclear division was likewise unobserved. That did not matter to either Roux or Weismann. For what one seeks in science is causal explanation. The goal for embryology is to discover the causes of living phenomena and to produce one explanation that provides an account of all those causes. Never mind if the result is a general theory that does not grow directly out of a plethora of facts, as long as the theory is consistent with the facts. Never mind that the theory cannot be unequivocally tested to provide a clear verification or falsification, as long as the theory is consistent within itself, is as simple as possible, and provides the sort of explanation sought. A theory may be testable or capable of disproof, but it need not be. Thus Weismann said that putting forth a bold and definite theory was justified, indeed necessary for scientific progress. Consequently,

> the ceaseless activity of research brings to light new facts every day, and I am far from maintaining that my theory may not be disproved by some of these. But even if it should have to be abandoned at a later period, it seems to me that, at the present time, it is a necessary stage in the advancement of our knowledge, and one which must be brought forward and passed through, whether it proves right or wrong, in the future. In this spirit I offer the following considerations, and it is in this spirit that I should wish them to be received. (Weismann 1889, 176)

Bold theory leads to knowledge, according to Roux and Weismann, and is a proper part of science.

Roux and Weismann developed a full-scale theory to explain heredity and development, based on the assumption that the egg cell begins with only material passed on from the parents. There must be morphological units, in the form of chromosomes, that carry the important hereditary material, they said. Given the fact that as development proceeds the individual parts (cells) of the organism become differentiated according to their proper patterns, that differentiation must occur because the hereditary material is progressively divided up into the individual cells during cell division. The total of hereditary material is therefore eventually distributed throughout all the body cells. This would certainly explain all the available phenomena, they insisted. And it was the only possible such explanation they could see. The alternative idea of the epigenesists, that the egg cell and its cell products gradually undergo differentiation because of some combination of unspecified internal and external factors, simply was not satisfactory. It was too vague and only asserted the obvious results of direct empirical observation rather than explaining the phenomena of differentiation. Explanation and causes are what science must seek.

When Roux's experiments showed that he could kill one of the two cells after the first division of a frog's egg and the half remaining would develop into the form of a half embryo, he felt that this provided great substantiation for the theory (Roux 1888). Development occurs as sort of a mosaic, whereby each part accordingly takes on its proper form and functions within the whole, all guided by the separations of units of hereditary information. Each part acts essentially independently and is capable of self-differentiation. When Hans Driesch showed with sea urchins' eggs that separating the blastomeres after the first or second division produces perfectly normally formed but smaller larvae, this did not provide disconfirmation or even a problem for Roux, as it might seem it would (Driesch 1891–92). Roux simply added the auxiliary hypothesis that each cell retains some reserve idioplasm (or inherited material in the form of chromosomes), which it calls into action in cases of emergency. Thus regeneration of cells and of body parts is possible. His reasoning went thus: regeneration occurs; it demands an explanation; furthermore it demands the same explanation as other developmental phenomena, since our goal for science is to bring all living phenomena under the same explanatory rubric; we can provide an explanation with an auxiliary hypothesis; therefore the mosaic theory of development by qualitative nuclear division is actually further strengthened by its ability to account for regeneration as well as for normal development.

We seem to have an epistemic style that characterizes the work of His, Roux, and Weismann at least. They all saw knowledge as lying in achieving causal, mechanical explanations, with the best explanatory theory accounting for the greatest number of living phenomena. They all saw strong and general theories as producing knowledge. They also all asserted that they counted a theory as confirmed or justified when it held up against all the empirical data, even though providing such data was not the central focus for any of them. In fact, however, Roux and Weismann quite willingly invoked auxiliary hypotheses at various times to save the theory in the face of apparently problematic data. So it is not quite clear what counted for these three as evidence for having gained knowledge or for justification. Yet they seem to have held similar views about the primacy of a general theory that explains all the available phenomena. It seems that we do have here a coherent epistemic style. It may also be that this constitutes a German epistemic style for the time.

At first glance, Oscar Hertwig appears to complicate the case. Although German, Hertwig rejected the Roux-Weismann mosaic theory of development, and he ridiculed Weismann's unscientific ways. Hertwig suggested that Weismann was raving off the deep end of speculative philosophy. He saw Weismann's theory of germ plasm and the efficacy of the hereditary material in causing (and thereby explaining) development as not good science. Indeed, "it merely transfers to an invisible region the solution of a problem that we are trying to solve, at least partially, by investigation of visible characters; and in the invisible region it is impossible to apply the methods of science. So, by its very nature, it is barren to investigation, as there is no means by which investigation may be put to the proof" (Hertwig [1900] 1977, 140).

At first, then, Hertwig might appear to demand a revision in any effort to articulate the character of a German epistemic style in embryology around the turn of the century. We could provide a weak salvation of the case by identifying Hertwig as one of those individuals who simply did not conform to an otherwise typical style. Yet if we look closely at Hertwig, it turns out that he does share the German style. What he really objected to was Weismann's *particular* theory and not to the bulk of Weismann's epistemic assumptions. Instead, he thought that Weismann failed to provide "proof" because Weismann, in order to explain development, relied too heavily on appeal to heredity and its logical rather than observable implications.

Weismann had held that his "actual proof of the reality" of preformationist development came in the form of the following argument: If development takes place epigenetically, then differentiation must begin in the egg cell in the form of a collection of tiny biophores (defined as the units that would yield varied characteristics through development). Each separate body part is represented by one of these biophores. But, we here reach an absurdity. For satisfying the latter condition that *every* body part has its own biophore requires such a vast number of biophores that the germ material could not possibly hold them all. Therefore the alternative theory, namely that of preformation, must hold (Weismann 1893). Thus Weismann provided what he considered to be a "proof" of his preformationist theory.

Having insisted that Weismann had not really proved his theory at all, Hertwig rejected Weismann's metaphysical introduction of hypothetical entities. As Hertwig complained in an enthusiasm of mixed metaphors:

> When, to satisfy our craving for causality, biologists transform the visible complexity of the adult organism into a latent complexity of the germ, and try to express this by imaginary token, by minute and complicated particles cohering into a system, they are making a phantasmal image which, indeed, apparently may satisfy the craving for causality (to satisfy which it was invented), but which eludes the control of concrete thought, by dealing with a complexity that is

> latent, and perhaps only imaginary. Thus, craftily, they prepare for our craving after causality a slumberous pillow, in the manner of the philosophers who would refer the creation of the worlds to a supernatural principle.
>
> But their pillow of sleep is dangerous for biological research; he who builds such castles in the air easily mistakes his imaginary bricks, invented to explain the complexity, for real stones. He entangles himself in the cobwebs of his own thoughts, which seem to him so logical, that finally he trusts the labour of his mind more than Nature herself. (Hertwig [1900] 1977, 3).

It was not the seeking for causes that Hertwig opposed. Nor was it the attempt to explain as many phenomena as possible. Nor was it Weismann's other epistemic assumptions, with which Hertwig largely agreed. Instead he objected to the metaphysical appeal to theoretical entities and to Weismann's particular theory.

Specifically, he rejected Weismann's division of the complex organism into small hypothetical hereditary units. Instead, he believed that the organism is necessarily an interactive whole in which each part and what happens to each part is influenced by the whole (ibid., 105–6). In addition, Hertwig tells us that he began with two assumptions that differed from Weismann's. For Hertwig, the germ plasm is already highly and specifically organized, and that inherited material is gradually transformed into the adult by an epigenetic process. To understand this process, present (proximate) physiological processes remain more important than any information about past, evolutionary and invisible inherited traces. Hertwig said that he could "not regard the development of any creature as a mosaic work. I hold that all the parts develop in connection with each other, the development of each part always being further dependent upon the development of the whole" (ibid.). So far, then, it is the particular theory and its metaphysical underpinnings that differ from those of Weismann.

The two also differed as to the scope of their theories. Hertwig tells us that he does not attempt or pretend to explain all phenomena of development and heredity, while Weismann does. Weismann, he says, presents us with a "closed system," one that offers an explanation for everything or is capable of providing one. Instead, Hertwig wanted to offer a less all-encompassing theory, one consistent with all the known data, but acknowledging that it perhaps cannot explain everything all at once. Weismann's far-reaching theory, Hertwig felt, really only "explains by signs and tokens that elude verification and experiment, and that cannot encounter concrete investigation" (ibid., 140). And for Hertwig, experimentation and empirical verification played a more important role in science than they seemed to for Weismann. In matters of justification, specifically with respect to what might count as a verification or a disconfirmation of a theory, they did not agree absolutely. For example, Hertwig insisted on a much higher degree of support from empirical evidence than Weismann did. So they did disagree about epistemological details. Yet they differed in degree rather than significantly in kind.

On the whole, then, the cluster of epistemic commitments that Weismann held, and that His and Roux generally shared, overlapped very closely with Hertwig's convictions. Instead of providing a counterexample, Hertwig actually supports the claim for a German epistemic style. Certainly, these Germans agreed with each other much more than with their American counterparts: they all sought general theories and felt it more important to have some explanation for all the natural phenomena than to have a better "confirmed" or more "justified" theory for a particular (narrower) phenomenon. That is, each of them worried much more about generating knowledge than about how to determine that they had, in fact, achieved it. And the sort of knowledge they sought involved discovering causes, specifically mechanical causes, to explain developmental phenomena. Thus there is a high degree of probability that we can tell by looking at this embryological work of the time that it was German. It seems reasonable to consider the cluster of commitments to which they subscribed as a style. In fact the style persisted, as Boveri, Driesch, Gustav Born, and Hans Spemann shared the same basic epistemological commitments for embryology well into the twentieth century.

An American Style of Embryology

The Americans, in contrast, had different goals for science, different processes of investigation, and different standards of evidence, although they differed from their German contemporaries in some of these respects more than in others. First, they disagreed about the basic goals for scientific investigation. Instead of theories that can explain all conceivable phenomena of development (as Weismann and Roux wanted) or as many empirically observed phenomena as possible (as Hertwig allowed), the Americans sought to accumulate as many definitive facts as possible. Only on such factual bases could they develop working hypotheses, which then moved them toward the gathering of further empirical data. This, in turn, led to empirical generalizations that eventually took on the status of laws or "fundamental principles." Understanding of developmental phenomena came with the application of those principles and a working out of the mechanical and chemical factors directing the developmental process.

Both the Germans and the Americans found the experimental production of half embryos interesting. Roux's half frogs and Driesch's half-sized sea urchins provided a stimulus for American extensions of the technique, especially by Edmund Beecher Wilson and Thomas Hunt Morgan. Roux had concluded that the production of half embryos showed the preformationist mosaic nature of the essentially mechanical process of development. Driesch had, in contrast, concluded that the small whole embryos revealed an epigenetic regulating ability of all the cells acting together as a whole during the essentially mechanical process of development.

Working with sea urchins, Wilson obtained the same results as Driesch. But he regarded it as valuable to extend the experiment to provide a greater pool of empirical phenomena. He felt it necessary to work on other organisms as well, in order to provide a proper comparison before drawing any conclusions. He opposed drawing general conclusions from a single case. Thus, he studied a variety of marine invertebrates and found variations in their adaptive responses. Some acted in a more regulatory manner, others in a more mosaic manner. Since Wilson believed that the same basic fundamental principles must underlie all of nature, he concluded that development must exhibit a range of abilities to be more or less regulatory. More regulatory organisms, or those that can better recover and develop more normally after experimental intervention, include those whose cells remain relatively undifferentiated at early stages of development. More mosaic organisms are differentiated early and cannot adapt to external changes in the environment. This Wilson knew also from extensive comparative studies of cell lineage.

Morgan performed the half embryo experiments with frogs, getting some results similar to those of Roux. But he realized that Roux had actually obtained very few cases in which the embryo with the punctured cell had gone on to divide further. He worked at gaining a greater number of cases, and under varied conditions. In doing so, he found that not all the cases exhibited the sort of neat mosaic pattern that Roux believed he saw. Instead, some of the specimens recovered and went further toward compensating for the lost part than others. This suggested to Morgan a more complicated sort of chemical and mechanical developmental compensation that called for further study.

Both Wilson and Morgan concluded that development must be more epigenetic than preformationist, that the sort of strict self-differentiation and mosaic development that Roux claimed did not fit the data. But neither did Driesch's version of what he called the "harmonious equipotential system." Rather, the egg is neither predetermined nor completely undetermined, and not all eggs of all species experience exactly the same degree of differentiation. The empirical evidence demonstrated that some early differentiation occurs in some cases, but with notable differences among species. What the embryologist must do is draw together the evidence and reach empirical generalizations that fit the range of data and, in effect, grow out of the data. It is useful to gather new data to fill in the gaps, so to speak. It is necessary to explore alternative hypotheses, even multiple working hypotheses, and generate temporary working hypotheses to explain the range of varying data and guide the search for new data (Chamberlin 1890). The goal is to "understand" and to "apply the general principles" in order to account for particular phenomena. There is relatively little concern with generating large-scale theories to "explain" or "discover the causes of" all living phenomena at once.

The way that these people did science, or what counted for them as gaining knowledge, is primarily available to us through published papers and letters. Yet their public lectures about the nature of science and implicit assumptions also help. We see that on the face of it Driesch's and Wilson's experimental manipulations and published reports of laboratory work are very much alike. It might therefore seem that they participated in the same style of doing work. Except that Driesch stated his conclusions very differently from Wilson or Morgan. Whereas Driesch, like his fellow German biologists, was much more likely to assert that he had actually achieved knowledge about the particular theory in question through the experimental discovery process being reported, Wilson and Morgan were more likely to claim that they had definitively demonstrated individual facts of development. Only at the end of their papers did Wilson and Morgan offer what they saw as temporary best working hypotheses to explain the phenomena. Further, the Americans did not write the sort of larger speculative articles or theoretical and philosophical books that the Germans did.[1] Wilson objected to wild speculative hypotheses that either revealed their "quasi-metaphysical character," as the Roux-Weismann theory did, or to theories that did not have a firm basis in empirical fact.

Yet some hypotheses were, Wilson felt, sufficiently grounded empirically to serve as useful guides for research. Thus, as he worked to explain the role that cells play in development and inheritance, he concluded that

> it should be clearly understood that when we attempt to approach these deeper problems we are compelled to advance beyond the solid ground of fact into a region of more or less doubtful and shifting hypothesis, where the point of view continually changes as we proceed. It would, however, be an error to conclude that modern hypotheses of inheritance and development are baseless speculations that attempt a mere formal solution of the problem, like those of the seventeenth and eighteenth centuries. They are a product of the inductive method, a direct outcome of accurately determined fact, and they lend to the study of embryology a point and precision that it would largely lack if limited to a strictly objective description of phenomena. (Wilson 1896, 296)

Wilson certainly did not reject the role of theory for science, but he felt that theory must be solidly grounded in empirical fact. Theory works as part of the creative process of gaining knowledge in science, stimulating the production of new definitive facts. Thus theory simultaneously is built on facts and directs the production of new facts.

For the Americans, then, the process of gaining knowledge was more gradual and cumulative. They did not assert that their empirical data in themselves yielded knowledge about causes or explanations of phenomena. To the extent that it can be gained at all, that causal or explanatory knowledge must come from further work to verify the results and from comparisons among different organisms to verify the generality of the results. It is not the immediate goal of scientific inquiry. The process of producing knowledge involves instead an inductive process of moving from the empirical facts at hand to the sort of empirical generalization often referred to as a law. This does not mean, however, that the Americans adopted a sort of empirico-inductive

[1] The Americans, in fact, did write books, but of a much different sort than the speculative and theoretical works by Weismann, Roux, Hertwig, Driesch, and others. The American works, especially those by Wilson and Morgan, are more like extended research reports, summarizing the best work of the day and offering tentative suggestive generalizations. Morgan felt that a book should be out of date by the time it was published, since it would provide a summary of work to that time while progress carried science inexorably onward.

approach while the Germans endorsed an alternative hypothetico-deductive approach. This distinction does not work, since both embraced the importance of hypotheses and empirical facts. Neither saw science as a one-way move toward either theories or facts; rather each sought both. Instead they had different emphases and different priorities, and they therefore sought different balances of observation and theory. They held different goals for science and different procedures for achieving and knowing that they had achieved those goals. These commitments might seem to lead to something not so terribly different in the long run, but there were considerable differences in how they saw the process of doing science at the moment.

As Wilson pointed out, scientists did not have to rely on passive observation to gain knowledge. Controlled interventionist experimentation could help to move beyond the "wanderings through the scholastic maze" that traditional zoology had brought. Yet Wilson did not agree with Roux and those others who maintained that experimentation was the *only* way to achieve progress in modern science. Rather, the wider range of results as well as the precision that can come from properly designed experiments could supplement the methods of observation, comparison, and inductive hypothesis formation (Wilson 1901; Maienschein 1987). Teams of researchers cooperating, working on different organisms, with varied experimental approaches to the same problems, can achieve the greatest "positive knowledge" of definite facts and can thereby generate the most productive and reasonable working hypotheses — the goal of science.

In order to establish that one has in fact achieved the sort of positive knowledge they sought, the Americans believed it must be possible to reproduce the phenomenon in question. Observable, repeatable, verifiable — all facts must satisfy these qualifications. Any interpretation must hold for all the available facts, including those spanning a range of organisms and circumstances. Any hypothetical generalization must fit all the facts, must hold for all living nature, and must serve as a guide for the production of further positive facts. Justification therefore lies in the fit with data already established and in the ability to produce new data — not in the ability to explain or to find the causes of new data, as the Germans would have it.

The Americans rejected Roux's and Weismann's speculative generalizations, even while they found them suggestive as possible working hypotheses. For the generation of American embryologists under discussion this did not mean that they had taken the hypothesis out into the world to test it and found it lacking. They did not start with an explanatory hypothesis and hold it up continually to the world to verify or refute it. Instead, they gathered data about a particular phenomenon. They generated experimentally derived additional data in order to gain new knowledge, since definitive facts are knowledge. They then considered whatever available working hypotheses might exist and measured them against their data to see which fit best for the moment, which had problems and of what sort, and which could guide the production of further knowledge. Some failed altogether, others measured up. This course of action might falsify a particular theory that did not fit the data but could only rarely verify one that did. It showed that some theories, such as the Weismann-Roux mosaic theory, simply failed to be about anything useful since they invoked hypothetical and, it seemed, inaccessible entities. The American way of doing science generated knowledge in the form of facts and remained with temporary suggestive hypotheses rather than seeking larger explanatory theories.

Having argued that there was a different American epistemic style, at least for the generation of embryologists considered here, we can also ask whether it was self-consciously so. Were the Americans purposefully trying to develop a way of doing work different from their German counterparts? There is considerable evidence that American biologists in the 1860s and 1870s had become entranced with German science and with English science. They had traveled abroad and had sought to enter the community of the world's best research biologists, as they saw them. They then made some efforts to import that biological science to the United States and to give it a uniquely American stamp. We can see this in the rhetoric and actions of Charles Otis Whitman, for example, as he founded the Marine Biological Laboratory and the research-oriented programs at Clark University and the University of Chicago. The Americans wanted to establish laboratories and research facilities even better than those of their European counterparts. It might also be the case that they sought to do their work better by adopting a more effective epistemic style.

I have found no evidence, however, that they self-consciously sought to effect a uniquely American style of work. In the 1870s and early 1880s, some Americans had tried to develop American science and American institutions that would import European traditions and improve upon them. By 1890 the rhetoric and even the implicit moves along those lines had given way to discussion of how to do the best available science, not the best American Science. Clearly, the American embryologists were skeptical of the theoretical approach and the emphasis on explanation with which their German friends felt comfortable. Clearly, also, the Americans felt that their careful empirical approach would produce more positive and reliable knowledge. They rejected Roux's and Weismann's theories, even while they pursued some of the same research problems with the same organisms and techniques.

It seems, therefore, that they had a sense of doing science more carefully and more effectively. But by the 1890s they were not referring to this as a preferred American style in any self-conscious way. Since they seemed more secure in their own abilities to do quality science and to produce valuable work, they did not have to measure up to Europe as closely. They could join Europeans in an international community of scientists, where different groups carried out different projects and carried them out differently. They did not so much see an American way of doing science as a better way of carrying out scientific work.

Significance of the Different Styles

Having detailed some of the arguments offered by two groups of embryologists, I would now like to step back and draw some conclusions about the possible implications. What significance does the coexistence of different national epistemic styles have for our understanding of the history of science?

One might begin by asking whether this particular case is unique or an instance of a more general phenomenon of coexisting yet contrasting styles of doing science. Obviously, it is impossible to document a full answer to that question here. But with considerably more evidence, spanning a larger number of researchers spread across a wider section of each country and each discipline, and even across more time, the existence and persistence of other differences in American and German ways of doing science could at least be demonstrated. At times, more than one style probably existed in each country, embodied in different research schools and in widely scattered different individuals. So the evidence seems strong that epistemic styles exist, even though one might argue at length about the exact character of such styles. It might turn out that other, perhaps metaphysically based, styles exist in science as well. Often they may be either local or international rather than nationally different styles. But clearly epistemic styles exist. It is less clear that we can agree on an explanation of the phenomenon or what significance its recognition holds for understanding the history of science.

One possible explanation might begin with the fact that the Americans entered biological science later than their German counterparts. Individually, the American embryologists were also quite young in the 1880s and 1890s. Another consideration might be the fact that the American community of embryological researchers was quite small. By the early part of this century, nearly all the leading American embryologists had studied at Johns Hopkins (with William Keith Brooks), at Clark University, at the University of Chicago, at the Marine Biological Laboratory (with Charles Otis Whitman), or at Harvard (with Alexander Agassiz or E.L. Mark) — or with a student of one of these programs. They made up a small and relatively interactive community, most of which gathered each year at meetings or at the various marine laboratories, such as the Marine Biological Laboratory in Woods Hole or the Naples Zoological Station, for summer research. They knew of each others' sometimes apparently conflicting results and consequently worked hard at coordinating their interpretations.

At least this was true for the generation of American embryologists trained in the late nineteenth century; embryologists who began to emerge in the 1910s and later were more willing to specialize and began to work in different ways than before. At the same time the Germans experienced some changes as well, since different groups at different universities and research institutes gained dominance. Yet even those changes seem to have fallen into patterns that reflected the persisting differences between German and American epistemic styles. The Germans remained concerned with developing theories, with explanations, and with causes; they also continued to be interested in a wide range of related phenomena at the same time. Americans, in contrast, eagerly specialized and thereby sought definitive facts and positive knowledge. Changes came, of course, and an increase in the number of biologists in both Germany and the United States brought divergence within each tradition, so that a greater number of epistemic styles could coexist and overlap than during the period at the end of the nineteenth century. Yet the early patterns of distinct epistemic styles apparently persisted through these changes well into this century.

Perhaps one cause of the appearance and persistence of such styles lies in local, proximate factors. Historian Jonathan Harwood argues for that explanation, maintaining that it is proximate sociological causes, in the form of different social and institutional settings, that explain the existence of national styles in science (Harwood 1987). Others offer a less concrete cause, suggesting that some sort of "miasma" prevailed at different times and places, and that a particular viewpoint was "in the air."

Certainly, each of these factors makes some difference and contributes to the existence of styles. But equally certainly, none fully explains what causes the styles to exist. Surely some more historical factors must be involved. There must be something about the evolving nature of national and other, larger traditions that predisposes people to accept certain ways of doing and acting and thinking. We can point to many examples of styles in the sciences and elsewhere that are too widespread to appeal only to local explanations. For a full account, then, we must look more deeply and further into the past, probably at a mixture of social, economic, institutional, practical, and intellectual factors. Deciding just where and how far to look is a task for future study. It is worth such further exploration to inform both the history and the philosophy of science.

Why does all this matter, one might ask? What if there are epistemic styles? What follows from that fact? First, if there are different styles of work, each of which is accepted as perfectly reasonable and good science in its context, then science is not unified or uniform in any straightforward way. Certainly, there is no one scientific method for all of science for all times and places, but neither is there any one way of doing science for any one given time. Any philosophical view that assumes one unique rational approach to science must therefore be misguided.

That there are styles also suggests that there is something worth exploring and explaining that extends beyond individuals. The historian who feels more comfortable working with the single scientist or research team, and who does not explore what sorts of choices that individual or team is making because of participation in a larger style or tradition, is missing an essential part of the story of science. Researchers fall into clusters of people who agree about how to go about doing science, clusters that conform to epistemic styles and that — at least in the case of embryologists around 1900 — fell within national boundaries. Therefore, to understand the nature of science and its historical patterns and processes of change, historians must make a broader sweep than just an individual biographical chronicle or case study.

Finally, the fact that styles involve varying patterns of practice that shape the science done shows that most philosophical inquiry has focused too narrowly on theory change. The commitments that American embryologists shared were far broader than any set of theoretical claims. Similarly, the German way of doing scientific work stressed commitments other than those to particular theories; individual researchers could disagree about theory and agree substantially about how to do science. In order to understand the nature of science, we must recognize that what is important in science is not always theory. Nor is it limited to individuals. Nor is it always the same from age to age or nation to nation. In order to capture what is important in science we must cast our historical and philosophical nets more widely. Indeed, we must come to understand both the nature and significance not only of schools and traditions in science but also of styles, and in particular epistemic styles.

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