Chapter 6

Competing Epistemologies and Developmental Biology

JANE MAIENSCHEIN

In 1926, American embryologist Herbert Spencer Jennings reflected on developments over the previous decade. He recalled the 1890s, when, in a spirit of great enthusiasm for experimentation, one after another embryologist did experiments, got results, and drew different conclusions. Indeed, they often drew *quite* different conclusions. It reminded him, he reported, of a Gilbertian comic opera [particularly the *Mikado*] in which all the characters claim success. They happily sing, "For I am right, and you are right, and he is right and all are right." Despite their apparent disagreements and contradictions, all are perceived as "right." (Jennings 1926: 99) The issues are not always about the concrete details of evidence or the niceties of theoretical interpretation. They are not even always about having one "winner" in a given case. Instead, the issues often hinge on rightness in a different sense. Often the issue concerns what is to count as evidence or how much certain evidence is to count for or against a given argument. In short, the question often concerns what counts as the "best science" within the constraints and context of the community at the time.

At root, these are issues of epistemology. Claims of rightness or bestness carry with them views about knowing: what does it mean to be right? What counts as evidence in favor of claims to being right? How do we know? This is not to say that the cases Jennings was recalling did not also involve disagreements about precisely what the researchers saw, or about how best to acquire data or evidence. Nor did the cases exclude arguments about theoretical interpretations or metaphysical convictions. Indeed, historians as well as the biologists themselves have often emphasized the empirical evidence

or the theories or details of the experimental design. Philosophers and historians have considered whether theory or the scientific practice that produces data takes precedence, and how scientists have negotiated the relations between theory and practice, while sociologists have considered the ways that communities allow selected theories and practices to become conventional and thus established for that group.

What biologists have generally missed, and what historians and philosophers have often tended to misunderstand, however – even during the recent resurgence of enthusiasm for thinking about such things as "social epistemology"–is that epistemology actually can drive the science. Epistemological issues of rightness, and the coexistence of competing sets of epistemological values, often strongly direct the scientific discussions and underlie the controversies involved in particular cases. It is the epistemic norms, after all, that say how scientists should select their data, evaluate their experiments, and judge their theories. It is epistemic convictions that dictate what will count as acceptable practice and how theory and practice should work together to yield legitimate scientific knowledge. It is epistemology that underlies consideration for a given group in a given time and place of who is right. And competing epistemologies can coexist in a scientific community – and fruitfully coexist.

Let us begin with three parallel examples from what we now call developmental biology and genetics, and move on to other types of cases. I will present each case in stark outline, extracting key features of the discussion and ignoring much of the potentially rich contextual discussion. That detail appears elsewhere, and what I want to do here is to draw on texts to show how epistemology matters. After discussing the cases themselves, I will consider what they tell us about how science, and scientists, work.

PREFORMATION AND EPIGENESIS I: WOLFF AND BONNET

Enter Caspar Friedrich Wolff (1759) and Charles Bonnet (1769). Participants in the lively discussions about embryonic development during the mid eighteenth century, this Russian-turned-German and the Swiss scientist provide clear alternatives. They looked at the same thing, and at some basic level they agreed in their description of what they saw, yet they drew quite different conclusions. They provide an apparently clear case – but a case of what?

Sometimes their story is presented as a case of alternative theories. Wolff was an epigenesist, who held that development of each individual embryo proceeds from an unformed and basically homogeneous state to the fully formed adult stage. Development, for Wolff, was gradual and progressive. By contrast, Bonnet and his counterparts held a preformationist view. Form does not emerge gradually from nonform. Rather, the form must have been there all along in some preexistent, preformed state. On the face of it, epigenesists and preformationists were arguing about a particular theory.

Yet on another axis lies metaphysics, and they also disagreed about that. In general, though not necessarily, epigenesists were vitalists during the eighteenth century and for most of the nineteenth century as well. It was very difficult for an epigenesist to explain the emergence of form if it did not come from somewhere. Vitalism provided a source. Therefore, those who began with epigenesis tended to move also to vitalism: epigenesis first, then vitalism. By contrast, materialists were led to preformation. How else to explain the existence of form – highly differentiated and highly specific form?

Shirley Roe has beautifully developed this discussion of materialism and vitalism, epigenesis and preformation, for a range of eighteenth-century principal players, and I need not repeat that discussion here or discuss all the myriad other players in the debates (Roe 1981). Rather, let us note something important. While the two positions differed on metaphysics and theory, they agreed on the goal of achieving an explanation of the phenomena. They even agreed on the phenomena. But they had much different views about the epistemic value of their observations.

Both Wolff and Bonnet studied chick development, and both looked closely at the twenty-eight-hour stage – shortly before the heart becomes clearly visible and clearly beating. They had no way to observe the process of development, no special secret window into the egg to observe every moment of the progress. They had to take what were, in effect, snapshots frozen in time and extrapolate by making assumptions about what happened in between. Furthermore, they had to do this with different individuals, since once they had taken the picture that particular embryo no longer existed.[1]

Wolff looked at his twenty-eight-hour chicks and said, basically, I don't see the form or all the parts, I don't, for example, see a beating heart. Therefore it is not there. It could be that our senses just aren't good enough, but then we should see more parts with a more powerful microscope lens. That does not happen, however; the lens just makes the existing parts clearer and bigger. For Wolff, he does not see it, and therefore it does not exist. Seeing is believing – and not seeing is believing not. This is a powerful epistemological conviction about how we should go about believing – and knowing – things.

Bonnet had equally strong epistemological convictions. He looked at his twenty-eight-hour chick and could not see the form of all the parts either. He agreed that all the parts were not yet visible. Unlike Wolff, however, he insisted that they *must* be there. We know that form exists later, and we know that we need an explanation of how it got there. Furthermore, that explanation must be in terms of matter and motion, or materialism.

Note that the need for a valid explanation was as important as the need for materialistic roots or for a preformationist result. Indeed, if Bonnet had been willing to start with materialism and the later existence of form and to say, "I do not know how the form arose. Hypotheses non fingo," he would not have needed preformation. If Wolff had been willing to start with empirical evidence about that form and to say, "I do not know how the form arose. Hypotheses non fingo," he would not have needed to invoke a vitalistic something that he could not directly see. The insistence on having an explanation is the epistemological conviction that the possession of such an explanation is what constitutes knowing and produces good science. Bonnet would conclude that not seeing is not determinate. The issue is, in part, how much we know and what we know from what we see. The epistemological forces leave Wolff with his epigenesis and vitalism and Bonnet with his materialism and preformation. Each believed he was right. And there was room for the coexisting competing epistemologies, even though eventually the scientific community, while endorsing a general empirical disposition to think that seeing should be believing, decided that accumulating evidence favored Wolff's epigenetic interpretation and Bonnet's materialistic metaphysics.

EPIGENESIS AND PREFORMATION II: ROUX, DRIESCH, AND FRIENDS

The second example takes us to the end of the nineteenth century with a parallel debate. Two German embryologists, Wilhelm Roux (1888) and Hans Driesch (1891), play the lead roles here. In this case, rather than starting with the same observations and developing different theories, they began with the same theory, performed what they regarded as fundamentally the same experiment, made different observations, and drew quite different conclusions. They each quite confidently drew those divergent conclusions. And they diverged for epistemological reasons.

Roux laid out the theory: embryonic development is mosaic-like, that is, each cell division divides the originally inherited complement of "determinants" that cause cell differentiation into the separate cells (Roux 1888). This was essentially a preformationist, or rather a modified predeterminist, view. Roux predicted that if a researcher could remove one of the two cells after the first cell division, the result should be a half-embryo. Only half of the original determinants would persist to yield this half-embryo. One and a quarter centuries after Wolff and Bonnet, Roux recognized that passive observation alone would not get the embryologist very far and urged the use of experimentation to produce additional observations and to control and test ideas.

To test his prediction experimentally, Roux took the frogs' eggs that were readily abundant in his area and killed one of the two cells (blastomeres) after the first cell division. He used a hot needle and observed that this blastomere failed to develop further; it just stayed there as an undifferentiated lump. As predicted, Roux concluded that the remaining blastomere developed as it would have under normal conditions and yielded, in effect, a half-embryo. Therefore: a brilliant confirmation of the original hypothesis using a well-designed test. He triumphantly declared the mosaic theory to be correct.

The slightly younger Driesch was inspired by Roux and believed that Roux was right – in his interpretation and in his approach (Driesch 1891). He resolved to take the same mosaic theory, to make the same prediction, and to perform the same experiment. But while Roux had abundant frog eggs, Driesch worked at the Naples Zoological Station and had abundant sea urchin eggs. It also occurred to him that sea urchin eggs might even be better for getting clean results, since the Hertwig brothers, Oscar and Richard, had shown at Naples that it is possible fully to separate the blastomeres by shaking sea urchin embryos after the first or even second cell division. Thus, Driesch could obtain isolated cells where Roux had had to settle for killing half the material, which remained inertly attached to the still-living cell.

Driesch's theory, prediction, experiment, and basic approach were all the same as Roux's. And he recorded, "I must confess that the idea of a free-swimming hemisphere or a half gastrula open lengthwise seemed rather extraordinary. I thought the formations would probably die." (Driesch 1891: 46) But not so. "Instead, the next morning I found in their respective dishes typical, actively swimming blastulae of half size." Indeed, what Driesch got was two small forms. According to prediction, this was not at all right.

Driesch therefore concluded that the predeterminist mosaic theory must be wrong. Instead of cell differentiation because of the partitioning of inherited determinants at cell division, Driesch postulated that the cells must each retain some "totipotency" (or potential of the whole). They have the ability to undergo internal regulation in response to the needs of the whole. The cells, therefore, behave by working together as a "harmonious equipotential system." Driesch accepted his new observations with sea urchins as constituting new evidence and positive knowledge, which led him to reject the proposed theory and to develop a new alternative explanation. That, after all, is how the experimental approach, much acclaimed by Roux himself, is supposed to work.

It might seem that Roux must surely follow Driesch and admit Driesch's rightness in the face of such a powerful counterexample to his own theory. He did not. Nor did Roux develop an alternative theory of his own. Rather, he stuck with his mosaic theory, saying that it was still right. Surely that was bad science on his part.

But no, Roux invoked additional values. Science seeks explanations, he urged, in the form of explanatory theories. The mosaic theory is such a valuable theory, he persisted, explaining so many different things and providing the best theory available. All we need is a little auxiliary hypothesis to fix things. To this end, he postulated the existence of a "reserve germ plasm." Aha: so the original germ material and its determinants get divided up into cells in the normal division process. Yet in some cases for some organisms, there is a backup set of determinants to step in and carry on the process. Roux did not actually physically see such a reserve; he had no direct empirical evidence for its existence. Yet he believed that postulating its existence was justified, and indeed necessary, since the theory was so clearly important for producing knowledge in the form of materialistic explanation – which is what he valued most, and what made his science right. He sought to save the theory even in the face of apparently contradictory phenomena.

On the face of it, Driesch's is a clearer experiment, since it actually separates the blastomeres completely. Driesch's epistemology told him to accept those experimental results and to revise the theory – even though this later pushed him toward a vitalistic theory for reasons like those that had made Wolff a vitalist, namely, the need to provide an explanation of the emergence of form from the unformed. Roux's epistemology told him to go with the good theory apparently capable of explaining so much and so many different kinds of phenomena of heredity and development. Each believed he was right.

Yet these two were not alone. Others joined the discussion and sought still further evidence, with further experiments and more reflection on the interpretation of results. The American cytologist Edmund Beecher Wilson was one of these.

Wilson was intrigued by the difference between Roux's and Driesch's results and sought to understand how such apparently different results could occur and how best to interpret them. Wilson said, quite reasonably, that we need to seek answers with additional experimental evidence – with different organisms and different situations to control more factors. He used nemertine eggs (*Cerebratulus lacterus*) and others, since "[I]t is obvious . . . that this question is one not for speculation but for further experiment" (Wilson 1906: 265–266). The result was some of both patterns, which led Wilson to

conclude that development is more complex than either Roux or Driesch had recognized. For Wilson, any satisfactory explanation, anything that could possibly be accepted as knowledge, must take that complexity and diversity into account. The researcher should move carefully from observation to conclusion. Wilson realized that seeing does not simply lead to believing, nor does a great theory carry the day simply because it can apparently explain more or has more immediate promise.

In retrospect, Wilson's approach and his cautious conclusions make a lot of sense. Yet note the underlying assumptions. Wilson rejects an emphasis on any one model organism and its clear and compelling theory, or on any one crucial experiment. His approach requires waiting for more evidence – and how much is enough? He has no theory to tell us that. He deemphasizes the role of theory and of explanation in favor of accumulating more data. Roux says to follow the theory and its explanatory and predictive power. Driesch calls for following the compelling experimental discrepancy to a new theory. Wilson calls for continuing to accumulate more evidence.

They were arguing not just about form and what causes organization of form, but also about how to study it. These are, at root, fundamentally different epistemological values addressing what matters most in science. And there is no way any of these man could have persuaded the others of his rightness, though Wilson and Driesch certainly tried in an extended correspondence. Interestingly, in retrospect Roux is often praised by biologists for his invoking of the "modern experimental method" for biology, even though Driesch's epistemological approach and his following the empirical evidence conforms better to the description of the modern approach. And Wilson, the careful researcher, has been forgotten by all but a few historians and older biologists (Maienschein 1991).

EPIGENESIS AND PREFORMATION III: NERVES

A third epigenesis–preformation case, also from the late nineteenth and early twentieth centuries, shows the variety of ways that superficially similar debates can involve quite different underlying issues. The question centered on how nerves develop. Since nerve fibers play an obviously important role in making complex functional neural connections, researchers began to ask how they do that. How do fibers "know" how to make the proper connections? Or do they "learn"? In other words, is the connection predelineated or pre-established in some material way, or does it emerge and find its path only gradually, epigenetically over time and in response to whatever formative forces are operating? Further, do the individual neurons and nerve fibers act and grow independently, or do they make up an integrated nerve net, in which the cells may even interconnect into a reticulum? The Italian researcher Camillo Golgi and the Spanish investigator Santiago Ramon y Cajal played central roles in this debate. Golgi argued for reticular nets, Ramon y Cajal for autonomous neurons.

Golgi and Ramon y Cajal both looked at killed and prepared neural material. They used essentially the same methods and, indeed, some of the same specialized techniques including "Golgi preparations" with silver nitrate impregnations and advanced staining. At least early in their work, they apparently respected each other's technical abilities and even referred to each other's preparations as "evidence." (Their later battles may have had more to do with establishing themselves as deserving primary credit and ultimately the Nobel Prize – which they eventually shared – than with their deeper convictions about how best to do science.)[2]

What differed significantly from the beginning was which examples they regarded as important for understanding the development of form. They had different views about which phenomena were really "data" and "evidence," about which observations should "count," as well as about how and when the nervous system is organized. Yet they each kept gathering more of the same kinds of observations and largely ignoring or discounting the other's. Golgi selected examples and worked to create more examples that clearly show the apparent interconnections and the nets, while Ramon y Cajal selected and developed examples to reveal apparently separate cells. They could have spent more effort commenting on why their own selections made better material, or on what was wrong with the other's selections, but at least at first they largely made this an implicit matter. Each made his selections and argued, dancing around with increasing vehemence and eventually with significant vitriol, that his own selections were right.

The situation seemed irresolvable, with fundamentally incompatible assumptions, until other participants entered with alternative approaches, different focuses, and still further competing epistemologies. American embryologist Ross Harrison saw the difficulties both in selecting which material to count and in interpreting the dead and manipulated preparations that were far removed from their original living state. He sought a way to achieve what could be generally accepted as "definitive knowledge." Inspired by the experimentalism, with its promise of control and respectability, of Roux, Gustav Born, and others, Harrison worked to devise an experimental test of the theories about how nerves develop. He maintained that performing an experiment with actual living, developing neural material would be better than relying only on the preserved specimens of Golgi and Ramon y Cajal – better for producing reliable knowledge in the form of both observations and explanations (Harrison 1910).

He first assumed that since nerve fibers are the outstretching parts of nerves that connect with other nerves, it would be legitimate to focus on nerve fiber growth. Next he assumed that a key question was whether nerve fibers can develop independently and separately. If they are capable of doing so, he argued, then it is reasonable to assume that they can do so in normal conditions – and that they normally do. If they can do so, there can be no legitimate argument that it is necessary to have preexisting nerve nets to guide or make possible normal nerve development. Thus, using small nerve cells transplanted into drops of frog lymph in the first successful tissue culture, he devised what he regarded as a "crucial experiment" to determine whether fibers can grow independently by protoplasmic outgrowth. They did, therefore they can.

Harrison's approach assumes that his artificial, experimental, highly contrived conditions will yield information useful for understanding normal development. It assumes that what happens in this artificial controlled setting parallels what happens in the living organism. It assumes that such an experimental approach yields reliable and warranted knowledge.

Perhaps astonishingly, others agreed. Even many earlier critics of the neuron theory with its independent nerve cells came to agree that Harrison had provided "proof" of the theory, with its epigenetic implications. This did not happen overnight, and it required a campaign over several years and with increasingly sophisticated experimental design, but it worked. Though we know in retrospect that proof is a complicated thing, at least for his contemporaries Harrison was regarded as right. And he seems to have carried the day by pursuing a set of epistemological values and an experimental approach that won over the scientific community that came to endorse similar views.

VARIATIONS ON A THEME: MORGAN

Turning to yet a different case, we find the American Thomas Hunt Morgan apparently at odds with himself. This is a case where an essentially consistent epistemology underpinned quite distinct theories and research approaches, and a case where one researcher shifted from an epigenetic to a more preformationist position, for epistemological reasons.

Prior to 1910, Morgan had been studying development, especially focusing on regeneration in a wide range of organisms (which he saw as a kind of "natural experiment"). As he wrote to a friend in 1908, "my field is experimental embryology." He viewed heredity as much less interesting, basically serving to insure stability by making offspring much like parents, as more conservative and more preformationistic than he could accept. Development, in contrast, was for Morgan an interesting and creative process that produces variation and brings the process of developmental mutations in a more epigenetic way that he felt best fit the facts.

Morgan was looking for de Vriesian mutations in various organisms, including the fruit fly *Drosophila*, as a source of variation. He wrote to a friend that he was about to give up on the messy annoying flies (and the requirement to provide rotting bananas for them to feed on), but he did not. He found a peculiar white-eyed male, where all others had red eyes. The famous resulting Mendelian ratios of offspring's sex-related traits suggested strongly that heredity both operates in a Mendelian way and is connected with the chromosomes. Morgan did change his emphasis and enthusiastically took up heredity and *Drosophila* as his primary research program (Maienschein 1991, chapter 8; Morgan 1910).

That is a familiar story, and it is often recounted to show how Morgan changed his mind on the face of empirical evidence. It is suggested that, in effect, the previously misguided Morgan herewith saw the error of his ways and corrected his approach. This is offered as a story about the triumph of empiricism and reduction, and of experimentalism, over his former less defensible developmental views. It is also often offered as a tale of the triumph of the Mendelian-chromosome theory, with its message of genetic determinism and its first step toward our present enthusiastic search for "the gene for [whatever trait]." It was partly that – but only partly.

Yes, Morgan did change his mind and embraced a Mendelian-chromosome theory as a strong interpretive framework from which to explore further the actions of heredity and development. Yet Morgan insisted that science does not work in such a black-and-white way. It is not that he had seen the error of his former ways and, in a grand revelation, finally embraced the truth. Instead, he explained, "I beg to remind the reader, and possible critic that the writer holds all conclusions in science relative, and subject to change for change in science does not mean so much that what has gone before was wrong as the discovery of a better strategic position than the one last held." (Morgan 1913: iv).

The particular theory was not very important to Morgan, and served effectively as the temporarily best working hypothesis instead of as some capturing of reality. The experimental evidence was important, of course, but of only passing interest. What was most important about the evidence was the way it weighed in favor (or against) a theory. Morgan's deepest commitments were neither evidentiary nor theoretical but epistemic. His only enduring commitment was to his epistemological standards for what counts as good science.

Morgan did *not* change his mind about how to do science. All along he said that researchers should pursue the best theoretical interpretation most consistent with available data and most capable of producing further knowledge. He embraced an experimental approach, and he was fundamentally – and productively – an opportunist who pursued the "better strategic position" for any given time and context. This led him, quite naturally and logically, to change his mind about the Mendelian-chromosome theory of heredity.

Morgan's rightness lay in his solid and persistent epistemological convictions – even while that led him to different research questions, approaches, and interpretations over time.

COMPETING EPISTEMOLOGIES

There are many more examples that show the way individual disagreements play out, but let us look at a few types of cases to illustrate the range. One centers on what has been termed the naturalist-experimentalist or field versus laboratory debate. Though these are different debates, what is at issue in both is how much we can learn in the laboratory – by extracting life from its natural ecological setting and seeking to contrive and control conditions, by preparing, slicing, dicing, poking, measuring, and such. Or can we learn more – and perhaps learn better–by studying the messy, muddled life-in-its-context? Assuming that biology is the study of life, which approach is better? Which allows researchers better to know about life? Which epistemological conviction about how to do good science wins? Which is right?

One of my colleagues, Douglas Chandler, uses electron microscopy to study cells. He has developed a very useful freeze fracture technique that freezes the cells, then fractures them in a way such that they break along the "fault lines" corresponding to internal structures within the cell. This method has proven invaluable for gaining insight into the internal workings of the cell. With time, he and others have developed revised approaches including ways of freezing the cell more quickly so as to produce less damage and distortion, such as with deep etching techniques. Yet the entire approach requires killing the cell and observing it under artificial and experimental conditions. Those who insist on studying life in its living, functioning, active form reject the entire approach and its epistemological assumption that we can indeed learn about nature from such controlled and contrived conditions. Meanwhile, Chandler insists that there are things that we can learn from such techniques, things which constitute important contributions to knowledge, that we cannot learn otherwise. Which approach is right? And which should receive the funding, given limited resources and intense competition for the rewards?

In the laboratory, then, what techniques and approaches provide the best science? With cytology: is it better science to study cells physiologically, actively, to study processes of development? Or is it epistemically acceptable to take a snapshot of killed and preserved materials, seeking to gain knowledge about the morphological structure of the cell? Often the different approaches produce different results and conflicting interpretations. Which is right?

Is it the systems ecologist who looks at the dynamically interacting components of a system who is right, or the evolutionary ecologist who insists on the historical, evolutionary features of each adaptive unit? Is it the geneticist who seeks genetic determinants as providing knowledge about the cause of traits, or the developmental biologist who insists on historical process in any explanation of biologist traits? Each of these positions brings a competing view about appropriate epistemological values, and there are innumerable parallel examples that illustrate the range of ideas about "rightness."

Perhaps this is a case of Gilbertian rightness, and everyone can dance about singing happily of his own rightness. After all, diversity is considered a virtue these days. Why not accept all the competitors; what possible criteria can there be for adjudicating among the various competing claims? Perhaps none definitively, but we should try. Some science will be selected – for example by NSF, NIH, or those doing hiring – as better than other science. Some will be funded and some will be published, but much will not. The stakes are high. It is worth thinking about why, about who is more right at any given moment and according to what standards.

The cases show that epistemology matters and that competing sets of epistemological norms can coexist and make science lively, exciting, and perhaps even more productive as more research appears attempting to resolve the debates. We have seen a range of cases involving views about epigenesis and preformation, about heredity and development, about materialism and vitalism, about different theories and different practices, and about how and where best to study life. We have different views about what we are looking for in science, about what will count as data and evidence, as legitimate patterns of inference – in other words, as ways of knowing and of gaining knowledge.

We have cases of seeing the same thing differently, of weighing the evidence differently, of seeking different bits of evidence and counting theory more heavily than empirical evidence, of counting experiment or field observations as most important, of valuing explaining or more data or experimental testing as most important, of preferring "natural" to "artificial" or controlled conditions.

These are cases where epistemological views are central, cases that together illustrate the richness of science, the value and legitimacy of coexisting competing views, and the way such debate can be good for science. They tell us a lot about how science works and about the range of what should count as "right" or "good."

Furthermore, developing such examples and their implications gives us something desirable: a better picture of how science works and a way of discussing what should count as "good science" that is research-based and data-driven, that is intellectually and historically defensible, and that is useful. We can leave it to others to develop this usefulness, or we can do it ourselves by trying to sort through ways to authenticate, even if not to adjudicate, the competing claims to rightness.

Yet this does not mean that "anything goes," nor that there is no basis for judgment. There are constraints on the values of any community, and these come from conventions of the community. The rational epistemologist would allow discussion and debate. Such an epistemologist would smile at the dancing in the square as all are convinced that they are right, and would then allow us to work at adjudicating the competing claims based on reason and the existing values of science. Those in the scientific community who embrace the reason and logic of the enlightenment should agree. And once we agree, we can comfortably endorse the coexistence of competing valid epistemologies within the scientific community at any given time and place, and for any group of researchers. Then we can get down to the work of understanding how to make justifiable demarcations and just how much and in what ways epistemology matters.

ACKNOWLEDGMENT

Thanks to the National Science Foundation for support of research for this project.

NOTES

1. For an accessible look at Wolff's and Bonnet's central ideas, see Hall (1951), pp. 371–372, 377–381.
2. There are many discussions of Golgi and Ramon y Cajal's works. For useful overviews see Clarke and O'Malley (1968), pp. 91–96, 109–113; Brazier (1988), pp. 143–144, 145–146; Maienschein (1991), pp. 268–293.

REFERENCES

Brazier, Mary. 1988. *A History of Neurophysiology in the 19th Century.* New York: Raven Press.

Clarke, Edwin and C. D. O'Malley. 1968. *The Human Brain and Spinal Cord.* Berkeley: University of California Press.

Driesch, Hans. 1991. "The Potency of the First Two Cleavage Cells in Echinoderm Development." Translation in Willier and Oppenheimer, 38–59.

Hall, Thomas S. 1951. *A Source Book in Animal Biology.* Cambridge: Harvard University Press.

Harrison, Ross. 1910. "The Outgrowth of the Nerve Fiber as a Mode of Protoplasmic Movement." *Journal of Experimental Zoology* 9: 787–846.

Jennings, Herbert Spencer. 1926. "Biology and Experimentation." *Science* 64: 97–105.

Maienschein, Jane. 1991. *Transforming Traditions in American Biology, 1880–1915.* Baltimore: Johns Hopkins University Press.

Roe, Shirley. 1981. *Matter, Life and Generation: Eighteenth-century Embryology and the Haller-Wolff Debate.* Cambridge: Cambridge University Press.

Roux, Wilhelm. 1888. "Contributions to the Developmental Mechanics of the Embryo." Translation in Willier and Oppenheimer, 2–37.

Willier, Benjamin H. and Jane M. Oppenheimer. 1964. *Foundations of Experimental Embryology.* Englewood Cliffs, N.J.: Prentice-Hall.

Wilson, Edmund Beecher. 1904. "Experimental Studies on Germinal Localization." *Journal of Experimental Zoology* 1: 1–72, 197–268.