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## Arguments for Experimentation in Biology<sup>1</sup>

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### 1. Introduction

"An experiment," the Oxford *Dictionary of the History of Science* records, "unlike an experience, is a designed practical intervention in Nature; its upshot is a socially contrived set of observations, carried out under artificially produced and deliberately controlled, reproducible conditions. At the experiment's core is the notion that the conditions for producing a given effect can be separated into independently variable factors, in such a way as to demonstrate how the factors behave in their natural (i.e. the non-experimental) state." (*Dictionary* 1981, p. 136). Around 1900, some biologists would have acceded to such a definition, yet many would have offered alternatives. Some would have denied that the conditions in question must be "designed" or artificially produced; nature may also provide experiments. Others would have emphasized the use of experimentally-derived observations for hypothesis testing; experimentation goes beyond the mere experiment itself, that is. A variety of additional definitions would have been advanced as well, illustrating the lack of one such orthodox interpretation as the *Dictionary* now offers.

Notwithstanding the variety of definitions, the vast majority of biologists around 1900 would have agreed that experimentation was a proper method for biological science, indeed that it was a turn toward experimentation that had brought the recent rapid progress. American cytologist Edmund Beecher Wilson, for example, wrote in 1915 that:

"It is our privilege to live in a time of almost unexampled progress in natural science . . . because not long ago naturalists began to turn aside from historical problems in order to learn more of organisms as they now are. They began to ask themselves whether they had not been over-emphasizing the problems of evolution at the cost of those presented by life-processes everywhere before our eyes today. They awoke to the insufficiency of their traditional methods of observation and comparison and they turned more and more to the method by which all the great conquests of physico-chemical science had been achieved, that which undertakes the analysis of phenomena by deliberate control of the conditions under which they take place -- *the method of experiment.*" (Wilson, 1915, p. 1).

Wilson writes of the experimental method as providing leavening in all fields, so that zoology could move beyond its earlier restrictions on method and subject matter to

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experience a "day of rapid obliteration of conventional boundary lines; of revolt from speculative systems toward the concrete and empirical methods of the laboratory; of general and far-reaching extension of experimental methods in our science." (Wilson 1915, pp. 3-4). Such a claim seems clear enough, so that historians of biology have confidently discussed and sought to explain this revolt from speculation toward experimental biology (see Allen 1978, introduction; Cravens 1977). And yet the very variety of meanings of experimentation suggests that we must be more careful if we really expect to understand what the move toward experimentation entailed.

Wilson certainly did *not* mean that biology had rejected the earlier methods of observation and comparison. Instead he saw experimentation as essentially a refined extension of traditional empirical methods, a move away from speculative science, from excessive theorizing and from what he regarded as the earlier pernicious intrusion of metaphysics into science. Good scientific work in the life sciences should embrace the traditional methods, "plodding along the old hard beaten trail blazed by our scientific fathers -- the way of observation, comparison, experiment, analysis, synthesis, prediction, verification." (Wilson 1915, p. 6). Experimental methods could help to accelerate the formation of new hypotheses and thus to advance science. But even with those tools and even when embracing experimentation, progress comes only slowly, as the mountainous problems seem continually to recede from us even as we rapidly approach them. Wilson is *not* saying, then, that experimentation is *the* scientific method or that a modern experimental, analytical, and materialistic approach is *the* answer for all of biology. Rather, the addition of manipulative experimental methods to aid observation and comparison had brought significant advances for biology in the late nineteenth and early twentieth centuries, just as for physics earlier, and it promised further successes. Wilson might have accepted our *Dictionary's* definition, but he would have embedded it within a larger matrix of traditional observation and comparison as together constituting proper scientific method.

What I intend to do in this paper is to explore several views of experimentation for biology around 1900, that time when historians have agreed that biology became strongly experimental (see Coleman 1977, Allen 1978). For several cases, I will consider what an experiment was, what experimentation was, and what experimentation was intended to accomplish. I have suggested that the different views represent French, German, and American alternatives, but I do not wish to argue at this point that they constitute distinct continuing national traditions, especially since the arguments belong to different chronological slices. Nor do I wish to suggest that this survey is complete or comprehensive. Rather the ideas discussed here were all among representative important contributions to the discussion of experimentation in biology around 1900.<sup>2</sup> And this paper hopefully will lead to other, more detailed examinations of the nature and role of experimentation for biology.

## 2. A French Call for Experimental Zoology

As John Lesch and Frederic L. Holmes have demonstrated, physiology had come to embrace experimental methods in France by the mid-nineteenth century (Lesch 1984; Holmes 1974). The program of experimental physiology, culminating with Claude Bernard's proposal that physiology was *the* proper experimental life science and that zoology should continue to pursue careful observation and description of facts, had gained considerable reknown by the 1870s. This relegation of zoology to what seemed an inferior position -- given that Bernard regarded experimentation and physiology as the highest forms of life science -- annoyed Frenchman Henri de Lacaze-Duthiers. As Professor of Zoology at the Sorbonne and director of the national French marine zoological station at Roscoff, Lacaze-Duthiers exerted considerable influence on French zoology and became a spokesman for a French program of experimental zoology.

In an essay of 1872 which introduced his new journal devoted to experimental zoology, Lacaze-Duthiers developed a response to what he clearly regarded as Bernard's unfair and limited role for zoology (Lacaze-Duthiers 1872). In relegating zoology to contemplation and description, Bernard unreasonably arrogated for physiology alone the highest form of proper experimental work, Lacaze-Duthiers felt. After discussing the history of zoology to date, Lacaze-Duthiers proceeded to demonstrate that Bernard himself did not always hold a consistent view of what constituted experimentation and what did not, that physiology itself was not always experimental, and that zoology often could be. He developed his argument into a strong plea for incorporating experimentation into zoology.

Lacaze-Duthiers objected to the physicists' definition of an experiment as a "prepared observation." A prepared or controlled observation might not be an experiment if it leads nowhere, and an observation might be an experiment even if not prepared by man, if only it is properly embedded in a larger intellectual understanding. To be an experiment, then, an observation must be part of an experimental procedure. He gives the example of a dropped apple. An apple might fall and serve as an experiment for the prepared mind, if it leads to an understanding of the cause of motion. In contrast, a man might prepare his observation by purposefully dropping an apple and yet learn nothing, in an action which thus does not constitute an experiment. In fact, "the nature of experiment is alone found in an association of manipulation, fact, observation, and psychological activity." Thus, observation is not sharply distinguished from experimentation but instead "leads confidently to experimentation." (Lacaze-Duthiers 1872, pp. 136, 140).

The role of proper experimentation begins with doing experiments but goes on to require the combination of observation of facts (either naturally and spontaneously or artificially produced) within an intellectual endeavor which leads beyond the experiment itself. Epistemologically, experimentation guides the researcher at first to limited deductions, then to more general deductions, and finally on to the goal of experimental science, namely a "new or higher truth and one different from the fact from which the whole process began." In other words, experimental procedure should also serve the more traditional Baconian or Millian creative inductive role of leading to general principles and first causes. In addition, experimental procedure moves beyond basic empiricism to a method of verification and control, thereby allowing the scientist "to avoid error." The experimental method "appears now as always to play a considerable role in the search for principles and for the truth." (Lacaze-Duthiers 1872, pp. 137, 150).

For Lacaze-Duthiers, then, experimentation was more than simply manipulative or controlled observation. The recognition of what is being controlled, and how, and what conclusions are being sought are also parts of the experimentation itself. Experimentation helps to produce certainty and truth or knowledge. Lacaze-Duthiers' own work reflects his convictions as he searched for those generalities emerging out of consideration of collected facts. Following Lacaze-Duthiers, other French researchers such as Lucien Chabry pursued experimental work in developmental biology, often influenced also by the strong teratological tradition in France and gradually moving away from Lacaze-Duthier's search for general principles toward more narrowly-defined goals (Churchill 1973; Fischer and Smith 1984).

### 3. The German Program in Experimental Physiology

In Germany, experimental work in physiology gained prominence with the analytical work of the reductionist physiologists at mid-century (Cranefield 1957; Lenoir 1982). Physicist/physiologists such as Hermann von Helmholtz, Emil du Bois-Reymond, and Carl Ludwig demonstrated the value of experimental methods in the life

sciences for understanding such phenomena as nerve and muscle action or respiration. Traditional zoological or morphological work remained largely distinct from this pursuit of physiology, just as had Lacaze-Duthier's zoology from Bernard's medical physiology.

This schism dissolved only in the 1880s and 1890s, when the work on development and on individuals, which had been considered morphological, began to change. In part that happened as new researchers entered the field and as they began to break down old boundaries. Thus, for example, anatomist Wilhelm His began to discuss the mechanical processes experienced by a developing embryo (His 1874, 1901). His's approach depended on the traditional strongly manipulative methods of histological fixing, staining, slicing, and then observing and led the move to study of the physiology of development carried out at Breslau by physiologist Eduard Pfluger and embryologist Wilhelm Roux, among others. There, Pfluger transferred the traditional physiological sorts of isolation experiments to work on embryos, specifically of frogs, hoping to discover the mechanical processes of development (Lenoir 1986). He thereby introduced a self-consciously experimental approach into developmental study.

Pfluger turned, in a series of experiments in the 1880s, to questions about the causes of sex, or of sex production, specifically in frogs (Pfluger 1883). He then moved on to other developmental questions with his much-cited gravity experiments, with which he examined the extent to which environmental conditions outside the egg direct the course of individual development. When he reoriented the embryo within its gravitational field, a new cleavage plane developed in a different place than it would have under normal conditions. It seemed that his result must mean that the embryo was not lying preformed in the egg, but that its development was instead directed by the environment. Pfluger's experiment stimulated a flurry of controversy, with other researchers (especially Roux and Gustav Born) immediately carrying out the same or similar manipulative experiments on a variety of organisms. Was the position of the embryo already predelineated in some way in the egg, or did it become oriented only later in response to environmental conditions? Experiments -- or appropriately manipulated observations -- could provide the answer to such questions by controlling the relevant factors, the experimenters believed.

Another innovation came in 1884 with the success of Pfluger's and Roux's experiments on development of single blastomeres. These experiments, in which one or another blastomere was destroyed, began to stimulate wider interest. Roux decided that the egg's particular orientation did not affect development in any significant way (Pfluger 1884; Roux 1885). As he rotated eggs about an axis, he found no change in development, which proceeded normally in both speed and detail. He concluded that eggs are self-differentiating, respond to factors within the egg, and do not require external formative influences to guide them to normal differentiated development.

Roux maintained that only direct manipulative experimentation could definitely establish the fact that development acts as a sum of separate mosaical (or independent) developments, in which each part self-develops because of internal factors (Roux 1895). Those direct experiments which he regarded as so critical in this case consisted of puncturing with a needle one of the two blastomeres in frogs' eggs after the first cleavage, and in a few cases puncturing one or two of the four blastomeres after the second cleavage. He did not remove the punctured cells. Roux concluded that "In general we can infer from these results that each of the two first blastomeres is able to develop independently upon normal circumstances." (Roux 1888, pp. 25-26). In short, the early developing embryo acts as a mosaic of independent parts. And probably the mosaic is effected by "Qualitative separation of materials," including separation of materials in the cells' nuclei. Proper experiments lead, then, to justified conclusions about such phenomena as the internal working of the processes of development. Hans

Driesch, Gustav Born, and others followed with similar experiments and similar goals, though with varying results.

Controlled experimental manipulations, it seemed to these young enthusiasts, could help to uncover the processes of development which are normally obscured by the complexity of the intricate interactions. They assumed that organisms are essentially mechanical beings, and that their workings are thus subject to analysis in physical and chemical terms. Biology, like the physical sciences, could analyze systems into component parts and dynamic processes, these experimenters assumed. Here sceptics often disagreed, not accepting that organic processes could be chopped up into separate parts which would still function in anything like the normal way. Not necessarily vitalists, a variety of organicists urged that there is something about the organism as a whole that defies simple experimental analysis. Many of the critics were American, but they gained support also from Hans Driesch after he moved away from an analytical program toward an even more sceptical wholistic vitalism. The varying degrees of acceptance of an analytical approach help explain the diversity of interpretations of the validity of experimental manipulation as well.

A second, related major motivation to embrace manipulative experiments for German morphologists was the desire to provide causal explanations in science. Starting with Pflüger and Roux, those dissolving the old boundaries of physiology and morphology by exploring morphological problems with physiological methods (*Entwicklungsmechanik* or *Entwicklungsphysiologie*) argued that true science must provide what they regarded as adequate causal explanations. Given that what we see today in the developing organism is an effect of past causes, what are those causes, they asked; how can we get back to the forces and factors that actually cause the organism to develop as it does, following certain processes, and resulting in particular defined patterns. This search for causal accounts of development for Roux required an analytical, or reductionistic, approach since we cannot apprehend or discover the causes of the whole organism at once. And the search must also lead to mechanistic causes rather than to some more vague or distant first principles. For Roux and his fellow *Entwicklungsmechaniker*, the doing of experiments became embedded in a wider experimental approach and the epistemological drive toward experimentation comes more clearly into focus (Churchill 1966; Nyhart 1986).

By the 1890s Roux had become the most vociferous and insistent of propagandists for an experimental science of development. In particular, his influential essay introducing his journal *Archiv für Entwicklungsmechanik der Organismen* served as a manifesto for the program and for experimentation (Roux 1895). Exact science, Roux maintained, must study the underlying causality in development; because of the complex chain of causality, only direct experimentation can produce certain or definitive knowledge about causes. His argument held that since organisms are so complex, since we normally see so many things happening at once, and also since causes are often temporally separated from their effects, we cannot determine by simple observation alone what causes what and how. Experimenting ideally allows the researcher to vary one known component at a time (and perhaps associated observed factors) while controlling all other factors. Ideally the informed doing of controlled experiments -- or experimentation -- yields exact knowledge of the true causes of the all important phenomena of developing form. (Since individual development brings form into being, it is study of development that will be the most basic science.)

In reality, Roux admitted, one may not achieve certain knowledge immediately or easily but may have to repeat experiments and to modify various factors to reach knowledge about those basic causes (Roux 1895, pp. 162-163). Using as many different methods as possible when experimenting on the same subject will maximize certainty in the understanding of cases.

In addition, because experimenting does not always produce immediate or certain results, biologists must also rely on the use of hypotheses, just as physicists and chemists do. "And just as in those sciences," Roux conceded, "we shall have to regard those assumptions as approximating most nearly to the truth which explain the most facts and permit of the successful prediction of new facts; and *ceteribus paribus* we shall prefer that explanation which appears to be the 'simplest,' not forgetting, however, that we may easily fall into error on this point . . ." (Roux 1895, p. 168). Choosing the simplest theory which explains the most facts and makes the most successful predictions will also produce near certainty about the developmental phenomena and their causes, assuming that observations are accurate and ideas and hypotheses clear. Though developmental mechanics must rely on hypotheses and on other methods as well as on experimentation, Roux acknowledged, nonetheless "direct proof" and "complete certainty" about those all important causes can come *only* with controlled, manipulative experimentation -- whether performed artificially by man or serendipitously by nature (Roux 1895, pp. 173, 162). Experimentation, then, involves carrying out appropriately controlled manipulative experiments in order, eventually at least, to uncover the truth and thus achieve certainty about underlying and invisible processes and their (mechanical) causes.

Hans Driesch argued in similar manner for the efficacy of experimentation. As for Roux, for Driesch experimentation meant controlling all possible factors in order to assess the effects of the condition which was either allowed to, or artificially induced to, vary. The fact that Driesch performed an experiment apparently similar to Roux's but obtained different results suggested insufficient control -- at least at first that is what Driesch concluded. He initially expected to support Roux's experimental results and thus to underscore the value of experimental work for advancing biology. He also expected to support Roux's mechanism and analytical studies. Instead, he discovered that two separated sea urchin egg cells can produce two separate, miniature sea urchin larvae. No self-differentiation here. No mosaic development. Indeed, development seemed decidedly epigenetic and not independent, with form emerging only gradually and relying heavily on environmental conditions external to the egg (Driesch 1892). Despite his different results and different interpretations, Driesch nonetheless continued to agree with Roux that proper experimentation is the only path -- though perhaps a longer path than Roux had at first hoped -- to certainty about results and knowledge of underlying causes in biology.

In an essay of 1899, Driesch developed his arguments most explicitly. Pointing out that a new field of study often boasts a new method, he explained that developmental physiology was a new field and that some people had consequently labelled experimentation in developmental study as new. Driesch denied its newness. Study of development, traditional in morphology, was simply coming late to achieve a sort of renaissance of experimentation begun in physics, chemistry and physiology (Driesch 1899, p. 50).

Driesch agreed with Roux that morphology, including developmental physiological morphology, must rely on a range of methods. Careful observation and comparative study produce useful and legitimate results (Driesch 1899, pp. 42, 47). But in order to achieve definite and reliable *knowledge* of causes, the researcher must rely on controlled experimentation. Describing and cataloging and classifying organisms, for example, are activities preliminary to science rather than science themselves. Proper science is analytical, since it is necessary to break the complex whole organism into subphenomena, and experimental, since only experimentation can produce true knowledge, (Driesch 1899, p. 45).

Oscar Hertwig joined the chorus for experimentation during the 1890s, even while he urged caution in moving too far in an analytical mechanistic direction. The question he saw as basic for morphology and biology more generally was 'what is a particular organ for?' rather than Roux's or Driesch's question 'what causes form?' This led him to concentrate more on the whole functioning organism and to avoid the analysis into parts which concentration on production of form encouraged. Yet Hertwig agreed about the role of experimentation. True science must seek explanations based on empirical data since only thus could science achieve the certainty about results which he also sought. "Observation alone will afford a very insufficient insight into the mode of working of a particular organ, and in many cases none at all," he concluded (Hertwig 1901, p. 469). The physiologist of development must employ a variety of tools "by which alone he can draw any conclusion from what he has observed." Specifically he must rely on "systematically conducted experimentation" as his basic tool in order to avoid merely spinning speculative theories.

Here Hertwig emphasized the use of empiricism to achieve 'explanations' and hence productive results, though not necessarily in causal terms. He argued much less directly that Roux or Driesch for the doing of controlled, manipulative experimentation. Yet his goal of achieving reliable facts and explanations about which he could feel certain remained similar to Roux's and Driesch's. Hertwig explicitly disagreed with August Weismann, who found experimentation useful but not always conclusive or even the "safest means of discrimination." (Weismann 1893, pp.137-138). Weismann, who sought to achieve generalizations that applied to a range of specific phenomena, found it more prudent to work from general facts to those general conclusions. Experiments on individual specimens and on limited cases often could not provide reliable enough information to reach those important general conclusions which Weismann sought. For Hertwig Weismannian generalization involved spinning fancy theories without sufficient basis in all important reliable empirical detail (Hertwig 1900, pp. 11-12).

Different goals led scientists to endorse different methods, but this growing and vitally active group of young German biologists sought certainty or reliability in their results and explanations through at least some level of analysis. Experimentation in the form of carrying out controlled observation and drawing informed conclusions from the results provided the best road to what this group saw as solid or definitive knowledge about causes and explanations.

#### 4. American Reactions

The Americans offered a variety of reactions to experimentation, ranging from wild enthusiasm to scepticism. Though familiar with traditional zoological and even manipulative techniques well before 1890, most of the Americans really first encountered explicit discussion of experimentation in biology when they visited Germany or read the polemics of Roux, Driesch, Hertwig, and others. In the course of discussion, several of the leading American zoologists concentrated on the role of hypotheses in the experimental process, and they tended to stress experimentation neither as *the* road to certainty nor as the search for causes in quite the same way as the Germans did.

In examining a selection of American attitudes, let us first return to Wilson, an influential figure in American science, whose positive attitudes toward experimentation of 1915 were mentioned earlier. Actually, Wilson's opinion evolved. Whereas Wilson had provided the leading example of non-experimental, old-fashioned descriptive and comparative phylogenetic morphology for Driesch (in his paper of 1898 discussed above), Wilson increasingly became an advocate for experimental approaches as well as experimental techniques in zoology in the twentieth century. This reflects a changing

attitude of the leading American biological community which came together each summer at the Marine Biological Laboratory in Woods Hole, Massachusetts and which included many of the most ardent advocates of experimental biology. Throughout the 1890s, that community developed an increased awareness of the German successes in developmental physiology and of the experimental programs behind the success (Maienschein 1986).

In 1900, following Driesch's critical reaction to Wilson's comparative phylogenizing (or constructing theoretical phylogenies on the basis of comparative studies of development and the appearance of homologies), Wilson acknowledged that experimental methods and the doing of experiments were also appropriate for zoological work (Wilson 1900). Yet he still argued that the organism is a functional whole and must be understood as such, in relation to its external environment. Thus individual cells do not have a fully independent life, and as a result experimental analysis of individual cells has limits. Experimental methods are useful, then, but excessive analytical application of those methods is not legitimate.

By 1901 Wilson endorsed experimentation even more positively. Morphology, he recognized, had passed through a stage in which the conclusions had remained very vague. The older guiding theory of recapitulation had led to so many difficulties and confusions "that investigators have in a measure wearied of their wanderings through the scholastic mazes of ancestral and secondary characters, of palingenesis and cenogenesis, of primary and adaptive forms and the like, and have sought for new interests and fresh motives of study." (Wilson 1901, p. 17). Yet even more important than reaction against such imperfect and inconclusive results of those wanderings was the growing sense that real solutions lay too far distant and that thus "we would better turn for a time to the study of questions that lie nearer at hand and are, to say the least, of equal interest and importance." (Wilson 1901, p. 18). This had brought morphologists specifically to cell theory and to experimental methods.

Rejecting what he saw as the excessive claims for experimentation by some such as Driesch, Wilson insisted that "seriously to maintain that the non-experimental comparative study of nature is not science is an efflorescence of enthusiasm at which one could hardly repress a smile did it not involve so serious a blunder (Wilson 1901, p. 20). Experimental methods do help bring the researcher to the "limits of scientific analysis," but "Observation and experiment give us our materials" and it was "comparison and correlation of those materials that first built them into the fabric of science." (Wilson 1901, p. 21).<sup>3</sup>

Experimentation is a tool that may very well produce greater certainty about results than the older methods. But, as Wilson recognized and articulated even more clearly by 1915, experimentation provides the "test of experimental verification" for working hypotheses (Wilson 1915, p. 1). As he illustrated in many of his articles and in his masterpiece *The Cell in Development and Inheritance*, experiments help make a working hypothesis more or less "highly improbable" or more or less likely. Experimentation does not in itself produce the truth or knowledge about facts or causes that Roux or Driesch ascribed to it (Wilson 1896). Rather experimentation moves toward verification and thus toward greater certainty than most of the older comparative genealogical programs could for example. It can give clear answers and productive results but not certainty or truth about underlying causes. As such, Wilson recognized that the importation of that age old method of experimentation from physiology into morphological work represented a great advance for biology, but not such a panacea as the German advocates suggested.

Indeed, this theme of experimentation as more or less confirming one or another working hypothesis is central for many of the American discussions of experimental

biology. An experiment is a controlled observation of some sort and at some level, which brings new and importantly useful information to bear on working hypotheses. In fact, the late nineteenth century had brought considerable discussion by scientists of proper scientific method, and the dominant view in the United States was that science should proceed through the examination of working hypotheses. Geologist Thomas Chamberlin urged the value of multiple working hypotheses rather than depending on one leading hypothesis. Yet he acknowledged that having even one hypothesis was better than the inevitable dogmatism which results when a theory is taken as established rather than seen as the best available working hypothetical alternative (Chamberlin 1890).<sup>4</sup>

The ongoing assessment of the value of working hypotheses and the role of evidence in supporting one or another took place outside the later discussion of experimentation, but it may help to explain the different emphasis that the Americans placed on the nature and value of experimentation. With the Americans we find more discussion of testing hypotheses, for example, or of the value of experimentation for comparing alternative hypotheses -- an aspect of experimentation scarcely mentioned by Lacaze-Duthiers and very little by the Germans.

This move by American biologists to embrace hypothesis testing as a valuable and legitimate scientific function was perhaps influenced by Thomas Henry Huxley, if not also by Chamberlin's discussion. Thomas Hunt Morgan cited Huxley as having written that "All science starts with hypotheses -- in other words, with assumptions that are unproved, while they may be, and often are, erroneous; but which are better than nothing to the seeker after order in the maze of phenomena. And the historical progress of every science depends upon the criticism of hypotheses -- on the gradual stripping off, that is, of their untrue or superficial parts -- until there remains only that exact verbal expression of as much as we know of the fact, and no more, which constitutes a perfect scientific theory." (Morgan 1905, p. 26).

Aside from Wilson, Morgan probably most clearly stated the case for giving up the claim to be able to achieve certainty and moved toward understanding experimentation as a method for bringing well-established facts to bear on working hypotheses. In his *Experimental Zoology*, he outlined that experimentation's several required steps: one must formulate a working hypothesis, while presupposing knowledge of relevant conditions; carry out an observation, while carefully controlling conditions; then use the data thus gathered to support the hypothesis in question or to formulate a new one (Morgan 1907, Chapter I; Manier 1969, p. 191, especially pp. 9-10). Confirmation or disconfirmation of the working hypothesis might be relatively weak or strong. Morgan did not expect his experimentation to solve all problems or to achieve certainty easily. Indeed, he pointed out that experimental results are subject to constant revision within the context of the hypotheses. Experimentation itself produces data in a controlled way that helps to judge an hypothesis, yet it does not always produce truth or certainly or even unquestionable facts, according to Morgan. Because it allows control, however, experimentation is more useful than more passive observation or the inconclusive results of comparative phylogenizing, for example (Morgan 1898, p. 158). In different ways or in less clearly articulated or less confident form, this recognition of the limits of experimentation ran through many of the Americans' writings.

Edwin Grant Conklin reflected a somewhat more sceptical position, for example. He saw experimentation as essentially a set of methods for obtaining data, with both observation and experiment having their proper places and their limitations (Conklin 1898, p. 17). In other words, experimentation was primarily the doing of experiments to achieve data and productive results and not anything magically more likely to uncover the truth than older methods.

For Herbert Spencer Jennings, as he reflected some years later on the introduction of experimentation to embryology, experimentation had become problematic. He related how biologists had come to a point at which their theories and explanations disagreed radically. They had wanted to determine "which, if any, of the tales were correct" since they had wanted to achieve certainty and definitive results. And "Henceforth, they said, we must so work that our results and conclusions can be tested; can be verified or refuted. We must be able to say: Such and such things happen under such and such conditions, and if you don't believe it you may supply the conditions, you may try it for yourself, and you will find it to be true. But that is precisely experimentation; and so they flocked with enthusiasm to experimentation." (Jennings 1926, p. 98). That confident definition of experimentation had proved ephemeral, Jennings recognized, since as Roux and Driesch continued to reach different conclusions they showed -- even if they did not themselves immediately admit -- precisely that experimentation did *not* give the promised "clear, definite and verifiable answer." Experimentation as productive of certainty, as providing crucial and decisive evidence does not work, he concluded (Jennings 1926, p. 99). In order to be successful, experimentation must remain more modest and recognize these fundamentals: 1. Experimentation must analyze and control the environment in question; 2. Experimentation must study in detail the chemistry and physics involved; 3. But experimentation must also study in detail the role of the physical arrangements of the material, the organization or structure (Jennings 1926, p. 101).

This latter sense of system and of the whole organism had been rejected by experimental physiologists, Jennings lamented in a concern reminiscent of Weismann's but with different goals. The goal of science should be to achieve general answers which hold for all organisms. The enthusiastic style of experimentation carried out by Roux and Driesch could not realize that generality because each sought to conclude too much from his limited particular cases, each assigned too much generalized certainty to his isolated results. Comparison of experimental results in the context of evolutionary understanding and of relevant theories is required to achieve valid general answers, Jennings insisted.

William Morton Wheeler also acknowledged the progress brought to biology by experimentation, but with reservations like Conklin's or Jennings'. He too thought that perhaps the enthusiasm had been overblown. In his wonderfully irreverent address on "The Dry-Rot of Our Academic Biology," which he hoped Harvard's librarians would mis-catalog with fungi, Wheeler pointed out that the descriptive work of natural history remained the "perennial root-stock or stolon of biological science." With time, buds of biological specialties grow on that root-stock, some experimental, others not. As he said, some of those buds, such as genetics, had been too "constricted at the base." (Wheeler 1923, p. 67). Other buds will be less constrictive, some will be non-experimental, and others not even acceptable to the professional biologist. Such "older men, trained during the later Victorian morphological boom," as this leading experimental scientist pretended to be, have trouble assuming the attitude held by some of the younger folks that only a modern dynamic and experimental approach to science can be productive. Encourage amateur interests and even the rambling explorations of natural history, Wheeler urged, concluding that:

We should all be happier if we were less completely obsessed by problems and somewhat more accessible to the esthetic and emotional appeal of our materials, and it is doubtful whether, in the end, the growth of biological science would be appreciably retarded. It quite saddens me to think that when I cross the Styx, I may find myself among so many professional biologists, condemned to keep on trying to solve problems, that Pluto, or whoever is in charge down there now, may condemn me to sit forever trying to identify specimens from my own specific and generic diagnoses, while the amateur entomologists, who have not been damned

professors, are permitted to roam at will among the fragrant asphodels of the Elysian meadows, netting gorgeous, ghostly butterflies until the end of time. (Wheeler 1923, p. 71).

Experimentation, and the drive for results, was not the obvious, or even always the best choice for all of biological study for Wheeler -- who was, after all, himself one of the leading American biologists.

Alongside such reserved American responses, we find also the contrasting enthusiasm of researchers such as Ross Harrison. Significantly, Harrison had received a medical degree in Germany as well as an American Ph.D. in biology. In Germany, he had examined the symmetry of teleost fins and had become interested in nerve fiber development, using techniques of embryonic transplantation borrowed from Gustav Born.

Harrison at first saw experimentation as the way to provide a crucial decision between two alternative theories of nerve fiber development, and to achieve that certainty which Roux and Driesch and Born also sought from their experiments. Yet he soon realized that experiments alone would not convince people who did not already agree with his conclusions (no "working hypothesis" here, Harrison *knew* which was the right answer from the beginning). He had to provide detailed information about normal development as a control, thus going beyond what Roux had meant by controlled experimentation. Only with detailed descriptions and comparisons of both normal and experimentally-derived abnormal cases could he arrive at conclusive evidence to support his preferred theory (Harrison 1912; Maienschein 1983; Witkowski 1985). But when properly carried out and placed in the wider context which also demonstrated why the chosen theory was otherwise more productive, experimentation could prove decisive for practical purposes, even though perhaps not logically decisive or "crucial." Despite his greater faith in the reliability of experimental results, Harrison too stressed the value of experimentation for weighing and choosing the best among alternative theories. Not perfect or absolutely productive of truth by any means, "Experimentation is simply a means for varying conditions purposefully, and is usually a far more effective one than nature unaided affords." (Harrison 1913, p. 407). For Harrison, biology should therefore follow physiology and pathology to useful experimental methods (Harrison 1912, p. 53).

## 5. Conclusion

To review and expand: What did that experimentation mean to biologists around 1900, and with what expectations did they embrace it? Most importantly, it should be obvious by now, it meant different things to different people. For some, experimentation meant simply employing experimental techniques. This is the simplest form of experimental work, in which the experimenter manipulates the conditions to produce interesting results: the typical what-happens-if-you-kick-the-dog type of experiment (Wilson 1915, p. 53). Nearly all biologists accepted by 1900 that some forms of manipulative work could be useful for some types of study, though a few diehards rejected any significant manipulation as too destructive and as producing such artificial conditions that nothing could be learned about nature, an uncertainty problem of sorts.

In certain cases only minimal manipulation would be appropriate: for example, if one were interested in examining the patterns through which the nucleus changes from one developmental stage to the next, then basic manipulations would be appropriate. It would be legitimate to preserve and fix the specimens, then to observe them, with that old style of technical "section cutting" skill as Wilson called it when describing Charles Otis Whitman's talents with the technique. But one should maintain those specimens in conditions as close to normal as possible and not manipulate further. We might call this the kick-the-dog-in-a-certain-way-to-see-what's-underneath approach. This type of

manipulation teases out new information about conditions close to normal. Those traditional types of experimenting with manipulations lie at one end of a spectrum of possible experimental approaches.

At the other extreme comes a broader experimental approach in which the experimental manipulation is used to seek underlying causal explanations of presumably mechanical phenomena or to select which of several alternative working hypotheses best fits a given phenomenon. Such an experimental approach tends, but needs not always, to accept the legitimacy of analyzing the whole organism into parts.

Some cited the experimental approach as dating from Francis Bacon, with his 'Instances of the Fingerpost' or crucial experiments to decide among available hypotheses. But several of the leading American biologists explicitly acknowledged the weakness of the popular idea of a crucial experiment, which would decisively determine which of several alternatives was the true one. The crucial experiment was something of a "bogey," as Morgan called it (Morgan 1927, p. 12). Instead, the experimenter often contrives an experiment with a larger question in mind, then uses manipulations to produce new data relevant to that particular question (for example, if I subject the egg cells to a higher than normal concentration of salt, will they develop abnormally and thus support the working hypothesis that salt concentration plays an essential role in directing development?). A new theory or several working hypotheses may follow.

For some Americans at least, the experiment thus took on an important creative role in the discovery process. The working hypotheses were refined, expanded, and otherwise developed in light of the full range of evidence -- both experimental and non-experimental. The experiments would come closer to Lacaze-Duthiers' function in identifying general principles than to Roux's and Driesch's emphasis on generation of definite facts and resulting truth or knowledge of causes, since the former admits a more inductive and creative role while the latter stresses analytical deduction.

Yet above all the message remains that the American reaction to German experimental work and the rhetoric supporting it was mixed, as German and French attitudes themselves varied. There were political reasons as well as specific scientific reasons for endorsing experimentation in different ways. Though I will not discuss the political factors here at any length, most are fairly familiar influences on the development of science. The desire to identify one's field as successful, as progressive, as scientific, and as professional strongly influenced biological rhetoric especially after 1900. New universities and research laboratories, expansion in the sciences generally, successes in medicine following the germ theory of disease: all these factors worked together to provide resources for those researchers and those programs which could convince others of their professional and scientific success and progress. To some extent, the emphasis on experimentation and its definite repeatable results became more vociferous retrospectively, for political as well as strictly scientific reasons. Only later did Wilson recognize that the scales had earlier fallen from biologists' eyes as they endorsed experimentation (Wilson 1915, p. 4). Yet in the 1890s, those biologists had felt the excitement and the heady sense of progress brought by experimentation and its promise of results. The actual research done changed only slowly through the 1890s for most biologists, but by 1910 the leaders had embraced experimentation of some sort -- some version of experimental techniques and an experimental approach -- as a very productive addition to traditional methods. Intellectual and political factors during that period dictated that biological experimentation was an excellent approach, an obvious choice.

Yet it was not *the* obvious choice for all biologists. That too had become clear in the few decades after the 1890s. Weismann had expressed his doubts. Wilson himself urged the importance of non-experimental methods within an experimental approach.

Charles Davenport acknowledged the rush to experimentation, but also recognized the value of work done on the old problems in old ways, or on new problems in old ways, or on old problems in new ways. In 1900 he predicted that inexorable scientific progress would bring more experimentation but also more quantitative work and more specialization into a diversity of acceptable sorts of work (Davenport 1900). Whitman agreed with Davenport that not everyone should adopt the same methods or problems or approach (Whitman 1891). Different areas of biology require different tactics, and biologists must not pour out biology with the bath water of older methods, Wilson reminded his readers. Specifically, successes with experimentation in development and heredity must not lead biologists to forget about either evolution or whole living organisms.

Experimentation was to be used where appropriate and where it was helpful in answering questions and obtaining productive or useful data. It was not to become *the* scientific method. And experimentation could involve more than "designed practical intervention in Nature," and even more than "a socially contrived set of observations, carried out under artificially produced and deliberately controlled, reproducible conditions." Experimentation could, in combination with traditional observation, description, and comparison, serve as a way to develop and assess working hypotheses, perhaps to move toward certainty, and thus to advance science according to the latest interpretation of what science should be.

### Notes

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<sup>2</sup>For discussion of the distinctness of French and German traditions, see Fischer, 1980.

<sup>3</sup>Wilson is referring here to Driesch 1899, pp. 40 and 42, though he does not say so.

<sup>4</sup>I thank Michele Aldrich for bringing Chamberlin, 1890 and related materials to my attention.

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