Should "Heredity" and "Inheritance" Be Biological Terms? William Bateson's Change of Mind as a Historical and Philosophical Problem

Gregory Radick*†

In 1894, William Bateson objected to the terms "heredity" and "inheritance" in biology, on grounds of contamination with misleading notions from the everyday world. Yet after the rediscovery of Mendel's work in the spring of 1900, Bateson promoted that work as disclosing the "principles of heredity." For historians of science, Bateson's change of mind provides a new angle on these terms at a crucial moment in their history. For philosophers of science, the case can serve as a reminder of the potential of Putnam's hypothesis of a division of linguistic labor for analyzing the semantic lives of scientific kind terms.

1. Introduction. Hilary Putnam proposed, in his famous slogan, that "'meanings' just ain't in the *head*!" (Putnam 1975, 144). His externalism about meaning (as the position came to be known) comprised two theses. One concerned the way that the physical world enters into the meaning of terms for natural kinds. To use Putnam's example, what you mean by the term "water" is in part fixed by that thirst-quenching liquid out there, over and above the list of properties you associate with water. The other thesis concerned the way that the human world—more precisely, an individual's linguistic community—figures in the meaning of such terms. Again to use one of Putnam's examples, you can think and speak meaningfully about "elms," even though you

*To contact the author, please write to: School of Philosophy, Religion, and History of Science, University of Leeds, Leeds LS2 9JT, United Kingdom; e-mail: G.M.Radick@leeds.ac.uk.

†Many thanks to Jordan Bartol, Dominic Berry, Mike Buttolph, Jon Hodge, Staffan Müller-Wille, and the referees for invaluable help of various kinds and to my fellow symposiasts and the participants at sessions in Leeds, Montreal (PSA 2010), and Salt Lake City (ISHPSSB 2011) for stimulating discussion along the way.

Philosophy of Science, 79 (December 2012) pp. 714–724. 0031-8248/2012/7905-0019$10.00

may not be able to distinguish an elm from a beech, because you belong to a community in which there are experts who can make the distinction or who at least busy themselves with the question of how to do so.

There is, of course, some overlap between these theses. On Putnam's picture, even if you never yourself have encountered any of that thirst-quenching liquid, your thinking and talking about "water" can be meaningful, thanks to your belonging to a community of people, some of whom have encountered it or who are at least in intellectual descent from people who encountered it. But, as was noted long ago, the theses are independent, in that one can be true and the other false (Zemach 1976/1996, 66–68). Certainly within the philosophy of science, the theses have had very different fates. The question of whether the physical world in part fixes meaning has been endlessly discussed, due especially to the promise it held out for answering Kuhnian worries about changes of theory bringing about radical, ultimately incommensurable, changes in the meaning of scientific terms.

By contrast, there has been little discussion of whether, as Putnam put it, there is a "division of linguistic labor" when it comes to scientific kind terms. Whatever initial interest the thesis (Putnam called it a hypothesis) may have excited did not long survive the publication of Dupré (1981). There John Dupré argued that, far from nonscientific folk deferring to expert scientific classifications about kinds of plants and animals, often those classifications are just ignored. Folk taxonomies divide up plants and animals into kinds according to the interests of ordinary folk, and scientific taxonomies, arising out of very different interests, do the dividing very differently. Dupré's was a picture not of a single epistemic and linguistic community, comprising lay people and scientific experts, with the latter doing all the linguistic and epistemic heavy lifting, but of two more or less separate communities, each deferring to the other depending on whose kinds are under discussion.

In making these points, Dupré never claimed to have overturned Putnam's hypothesis tout court. After all, even in biology, kinds of plants and animals do not exhaust the kinds of kinds there are. Nor is it difficult to find instances of scientific terms that are shared, in complex ways, between vernacular and technical cultures. In recent reflections on Putnam (1975), Ian Hacking gives the example of "cholesterol" (Hacking 2007, 10–11). We should want a philosophy of science that has something to say about such cases of common usage. To that end, I here take up an invitation that Hacking issued nearly 30 years ago, in recommending Dupré's critique to his readers (Hacking 1983, 90–91), to see in it a spur to the refinement of Putnam's account of meaning, above all through a more thorough study of the historical record.

2. "Heredity" and "Inheritance" Up to 1900: A Brief History. The kind terms that occupy me here are the English words "heredity" and "inheritance." Since about the middle of the nineteenth century, they have been used

more or less interchangeably to name a kind of likeness relation between parents and their offspring and, more generally, between ancestors and descendants. Below I examine a remarkable protest from the end of that century against the use of those words as names for that relation—a protest as remarkable for its content as for its later abandonment. Out of the examination will come a to-do list for the future revision of the Putnamian image of how the division of linguistic labor works within and beyond the sciences. But since the protest involves some polemical etymology, we should at the outset briefly consider, in an evenhanded spirit, the history of the terms.

"Inheritance" has by far the longer history. The *Oxford English Dictionary* (*OED*) teaches that, along with the constituent term "heir," "inherit" can be traced back through old French to late Latin. By the fifteenth century, the verb and its associated noun form can be found in English documents, in various spellings, with the meaning—to quote from the *OED*—of "to take or receive (property, *esp.* real property, or a right, privilege, rank, or title) as the heir of the former possessor (usually an ancestor), at his decease; to get, or come into possession of, by legal descent or succession." More colloquially, an inheritance was worldly stuff transmitted to whomever was next in line when the stuff holders died or otherwise lost ownership. By the end of the sixteenth century, a second, body-and-mind-directed sense of the word had been acquired: "To derive (a quality or character, physical or mental) from one's progenitors by natural descent; to derive or possess by transmission from parents or ancestry." An example from the early modern period of the worldly-stuff meaning is not far to seek. Recall the promise in the King James Bible: "But the meeke shall inherite the earth" (Ps. 37:11). For the body-and-mind meaning, Shakespeare's plays bear witness (as they do, and more copiously, to the worldly-stuff meaning). In *All's Well That Ends Well*, I.ii.22, the King of France, introduced to the young Count Bertram (who is the son of a good friend), declares: "Youth, thou bear'st thy father's face; . . . Thy father's moral parts / Mayst thou inherit too!"

To conceive of parent-offspring resemblances as arising from transmission is one thing. To group all such resemblances together within a kind, under a single name, is something else and requires explanation. Here is where the word "heredity" enters the story. In the early decades of the nineteenth century, French medical writers in particular became sufficiently interested in how diseases get transmitted within families that they came to describe their field of inquiry under a distinctive label, *hérédité*. Their efforts climaxed in the physician Prosper Lucas's two-volume *Traité Philosophique et Physiologique de L'Hérédité Naturelle* (1847–50). His title continued: *dans les état de Santé et de la Maladie du Système Nerveux*—natural heredity in the state of health and of disease of the nervous system. Parent-offspring transmission in the absence of disease thus came to fall under *hérédité* as the horizon of inquiry expanded beyond the initial interest in hereditary dis-

eases (López-Beltrán 2004; also Müller-Wille and Rheinberger 2007, 2012).

It was in the 1860s that English biological writers, most influentially perhaps Herbert Spencer, began to follow suit under the term "heredity," an Anglicization of the French term—although in fact the English word had been knocking around the etymological margins for centuries (López-Beltrán 1994). But "inheritance" and its cognates were by then also in use in connection with the same kind of bodily and mental transmission phenomena, as can be seen in the pages of Darwin's *Origin of Species* (1859). Shortly after affirming the authoritative status of Lucas's volumes on the topic, Darwin summarized the reasoning behind the recently expanded category: "Every one must have heard of cases of albinism, prickly skin, hairy bodies, &c., appearing in several members of the same family. If strange and rare deviations of structure are truly inherited, less strange and commoner deviations may be freely admitted to be inheritable. Perhaps the correct way of viewing the whole subject, would be, to look at the inheritance of every character whatever as the rule, and non-inheritance as the anomaly" (13).

The Englishman who more than anyone advanced and advertised heredity/inheritance in the latter decades of the nineteenth century, Francis Galton, used, in addition to the traditional medical adjective "hereditary" (in, e.g., the title of his 1869 book *Hereditary Genius*), both "heredity" (in, e.g., the title of his 1875 paper "A Theory of Heredity") and "inheritance" (in, e.g., the title of his 1889 book *Natural Inheritance*). Meanwhile, beyond the medical-scientific literature, the topic became an object of public fascination, with a particular surge of interest in the 1890s. Of the hundred plus entries cataloged under "heredity" in the SciPer index (http://www.sciper.org) covering science content within general British periodicals in the nineteenth century, more than half come from the final decade.

3. Bateson's 1894 Critique. We come now to the protest. It came in 1894. Its author, the English biologist William Bateson (1861–1926), is best remembered today as the great champion of Mendel, beginning not long after the famous "rediscovery" in 1900 of Mendel's 1866 paper on his experiments with hybrid pea varieties.[1] Bateson's major debut as a Mendelian came with the publication in early summer 1902 of a little book, *Mendel's Principles of Heredity: A Defence*. Note the presence in the title of one of our kind terms, "heredity." In Bateson's lifetime, the book, always with the same title, went through several editions.

Beyond the 1890s, then, Bateson outwardly went with the flow in using "heredity," and using it quite a lot. Paper titles from the 1900s include "Problems of Heredity as a Subject for Horticultural Investigation" (1900; his first

1. For a recent overview of Bateson's life and work, see Cock and Forsydke (2008).

paper discussing Mendel), "An Address on Mendelian Heredity and Its Application to Man" (1906), "Heredity and Variation in Modern Lights" (1909; his contribution to the Darwin centenary), and "Heredity" (1913). In 1919, in an address given not long after the end of the Great War, he expressed his hope that the "substitution of true ideas of heredity and of the biological structure of societies"—that is, the substitution of Mendelian ideas stressing how significant families are biologically and how insignificant nations are—would "weaken the conviction that national sentiment is a proper and natural extension of normal fraternal affection" (Bateson 1919/1928, 368).

At the start of the 1890s too, when he was well on his way to developing a vigorously dissenting saltationist perspective on evolution, Bateson showed little sign of wanting to depart from the by-then standard vocabulary. In a well-known letter to his sister Anna in September 1891, enthusing about a new "VIBRATORY THEORY of REPETITION of PARTS," which, he reckoned, promised to explain symmetrical, repetitive patterning in biology on dynamical principles, he added a postscript: "Of course, Heredity becomes quite a simple phenomenon in the light of this" (in Bateson 1928, 42–43). What he meant, roughly, was that, at the organismic level no less than at the organ level, the appearance of a new instance of a preexisting form—of, that is, a descendant animal or plant (the organismic level of "heredity") or an additional petal or vertebra or other plant or animal part (organ level)—could be explained using the same set of ideas about pattern formation through vibrations. The new theory, along with the use of "heredity" to identify its domain, recalled the work of the American zoologist W. K. Brooks, author of *The Law of Heredity* (1883) and Bateson's teacher in the summers of 1883 and 1884.

It is the more unexpected to find the following in the introduction to Bateson's mammoth 1894 tome, *Materials for the Study of Variation*: "In what has gone before I have as far as possible avoided any use of the terms Heredity and Inheritance. These terms which have taken so firm a hold on science and on the popular fancy, have had a mischievous influence on the development of biological thought. They are of course metaphors from the descent of property, and were applied to organic Descent in a time when the nature of the process of reproduction was wholly misunderstood" (1894, 75–76). He went on to say that the metaphors are problematic above all for a couple of reasons. First of all, such talk misleadingly "suggests the idea that the actual body and constitution of the parent are thus in some way handed on" (75). He named and shamed Darwin's discarded pangenesis hypothesis as a case in point. More generally, and in line with the new vibratory theory, what Bateson was driving at was what he saw as *the* major trend in the physical sciences of his day, away from explanations in terms of substance and toward explanations in terms of motion. Take heat: where it was formerly attributed to a substance, caloric, it was now understood to arise from systems of particles in motion. Similarly, Bateson reckoned, with patterns

of relation between parent and offspring, we should not rest satisfied with the invocation of some magic hereditary substance. Rather, we should seek to identify the forces that act in bodies to generate patterns dynamically. It was the motion of the matter, not its chemical identity, that was important—a concept that Bateson explained to himself and others by referring to Chladni patterns, waves in the sand on a windy beach, and other such phenomena (Radick 2011, 134–36).

The second problem that Bateson had with "heredity" and "inheritance" talk was that, to his mind, it pulled against the drift of new notions, from Galton and more recently from August Weismann, about the germ line: its segregation from the soma but also the soma's making manifest only a selection of what the germ line contains. As Bateson put it, "the metaphor of Heredity misrepresents the essential phenomenon of reproduction. In the light of modern investigations [he mentioned Weismann], . . . it is likely that the relation of parent to offspring, if it has any analogy with the succession of property, is rather that of trustee than of testator" (1894, 76). To spell out that last claim a little, on the Galtonian-Weismannian view, our bodies manifest a partial, contingent subset of character-trait potentialities passed on to us, some of which we in turn pass on. What you inherit is not the traits of your parents, and what you pass on are not your own traits. Rather, the traits exhibited by your parents represented a selection of the full set of potentialities contained within their germ cells, and likewise your traits represent a selection and so will your children's. What gets passed on is a set of potentialities, which are wider than the set of things visible in or on the passer-on—hence, in Bateson's view, the greater aptness of analogizing parents with trustees, who hold property for future passing on without its being theirs, than with testators, who pass on property that is theirs.

4. Explaining and Exploring Bateson's Subsequent Change of Mind. Sometime between 1894 and 1900, Bateson changed his mind about whether biologists should talk about "heredity" and, although he favored it less, "inheritance." Why? And how, if at all, did this change matter? To start with the first question, there may, for all historians know, somewhere be a document, published or unpublished, in which Bateson explained himself. Until it turns up (if it exists), we can only speculate, with lesser or greater plausibility. Here I shall try out three speculations, in increasing order of plausibility: that the return of "heredity" and "inheritance" talk shows that he changed his mind about his criticism of them, that their return came about through attempts to communicate to horticulturalists, and that it came about through attempts to communicate to researchers and students in biology.

There is no sign that Bateson ever abandoned his 1894-vintage views on how parent-offspring resemblance should and should not be explained. On the contrary, his steadfastness here made him, from the early 1910s, an in-

creasingly isolated figure. For it made him skeptical of the identification of Mendel's factors—"genes," as they eventually became known, in a back formation from his own 1905 coinage of "genetics"—with parts of chromosomes. He allowed that chromosomes undoubtedly played a role in the causal process that brought about like from like. But he thought the notion that biologists would eventually understand how that happened if only they had powerful enough microscopes, able to unlock the material secrets of chromosomes, or any other cellular part, utterly misguided. It was in the motion of the transmitted matter, not in the matter itself, or the mere fact of its transmission, that Bateson thought the answer lay (Radick 2011, 134–36).

What of his attempts to communicate with horticulturalists and other "practical men"? The critique-containing passage in Bateson (1894) ends with him saying that, although new terms are needed, what is needed even more are the breeding experiments that alone will throw light on the real nature of the phenomena (76). From the mid-1890s, Bateson not only undertook those experiments but started to interact with breeders, notably in the context of the Royal Horticultural Society, based in London. In a paper read at the society in July 1899, Bateson advocated a new program of experimental hybridization as something that "would in some five-and-twenty years make a revolution in our ideas of species, inheritance, variation, and other phenomena which go to make up the science of Natural History" (1899/1928, 161). His Mendel-introducing paper of 1900, "Problems of Heredity as a Subject for Horticultural Investigation," was published in the society's journal.

The trouble with supposing that Bateson reembraced talk of "heredity" and "inheritance" in the later 1890s for the sake of the breeders is that those terms do no seem to have figured all that much in their own communications with each other. A major difficulty that historians encounter in approaching this issue is the success of Bateson's subsequent campaign, from 1902 onward, to represent Mendelian analysis as a boon to breeders who, he suggested, had been desperate for a true theory by which to conduct their work rationally. For a corrective, one need only look at the agricultural lecturer John Percival's *Agricultural Botany*, published in 1900. It went on to become the standard textbook for generations of British plant breeders. And it had no section on "heredity" or "inheritance." Nor did it convey any sense of anxiety about that absence. When, in 1910, Percival introduced a discussion of Mendelian genetics, he did so by way of expanding coverage of reproduction, not breeding techniques. A major element of Bateson's success as a Mendelian advocate was his managing to persuade so many breeders that their prosperity depended on a theoretical understanding that, before 1900, they did not seem to crave and did just fine without (Charnley and Radick, forthcoming; Radick, forthcoming).

Before 1900, then, breeders as breeders did not, it seems, have much need for biologists' talk of "heredity" and "inheritance," notwithstanding wider

public interest in such talk. Biologists themselves, needless to say, were different. And in the later 1890s, Bateson found himself addressing and appealing to them, for various ends. The funding for his breeding experiments came about thanks to a November 1897 letter to Galton (as head of a Royal Society committee) "making application for a grant in aid of experiments in heredity."[2] The same period saw him lecturing to young biologists in the making at Cambridge University, as part of a course he gave on "the practical study of evolution"—that is, on the study of evolution pursued in a spirit not of airy abstraction but of research into concrete specifics, notably about variation. Among his papers at Cambridge is a printed syllabus from the 1899–1900 outing. "Heredity: phenomena and laws of" appears on a page indicating topics to be covered in the Lent term. Scrawled in pencil alongside is the word "Regression," suggesting that, under the heading of "heredity," Bateson taught his charges about Galton's innovations.[3]

However we explain it, did Bateson's change of mind about "heredity" and "inheritance" matter? One way of characterizing his critique is to say that, for him, and for other thoughtful commentators including J. Arthur Thomson and Wilhelm Johannsen, the terms "heredity" and "inheritance" had too much conceptual baggage to be scientifically useful.[4] In nevertheless adopting those terms, Bateson can be seen as winning the battle but losing the war. Packaged under a terminology that everyone understood, Mendelism got widely taken up and, indeed, became one of the great success stories of modern biology. But, against Bateson's wishes, Mendelian doctrine came to be tied very firmly to the substance explanation that, on his own analysis, "heredity" and "inheritance" talk invites. Mendelian genes became identified with chromosomal stuff, and ultimately with DNA. It is worth asking how much of what Susan Lindee and Dorothy Nelkin (1995) called "the DNA mystique" gets a boost, however subtle, from the etymological backstory that Bateson highlighted. For anyone who is today impatient with notions of DNA as the master molecule in understanding why organisms develop as they do, and frustrated by the persistence of such notions despite all attempts to dislodge them, Bateson's critique may well hold a clue.

5. Putnam's Hypothesis in the Light of the Bateson Case. As we have seen, the *OED* corroborates, in a soft-focus way, Bateson's backstory about "inheritance." But his point in presenting it—and his subsequent change of

2. W. Bateson to F. Galton, November 11, 1897, Royal Society Archive, London, MM/15/93; quoted with permission.

3. Papers of William Bateson, Cambridge University Library, Add8634 B.1.

4. See, e.g., Thomson (1908, 4) and Johannsen (1911, 129–30). See also similar observations from E. G. Conklin in 1908 and T. H. Morgan in 1910, as quoted in Sapp (2003), 134.

mind about using it and its medico-scientific spin-off, "heredity"—would be worth considering even if, one day, we discover that "inheritance" talk about the transmission of bodily and mental properties had emerged at the same time as, or even before, "inheritance" talk of the transmission of worldly property. (Again, recall the Shakespearean example. What could be more natural, and less inherently "theory laden," than to describe children as getting their looks, temperaments, etc., from their parents, even in cultures with no tradition of parents passing on worldly stuff to their children or anything much by way of worldly stuff to pass on?) For the student of the division of linguistic labor, what matters is that, having protested the use of "heredity" and "inheritance" as names for parent-offspring likeness relations, on the view that recent investigation had revealed those relations to arise from causes having little to do with the transmission of matter from parent to offspring, Bateson nevertheless went on to use those words precisely for that purpose, and without changing his mind about the underlying causation. Accusing biologists of taking over popular usage in a counterproductive way, he went on to readopt that usage—initially, I have suggested, while attempting to be an effective writer and talker to fellow biologists.

Let me conclude with three lessons that should bear on any future, fuller revision of Putnam's hypothesis. Each is tied to a different element of his picture of how the division of linguistic labor works.

Lay Deference to a Scientific Elite? In Putnam (1975), there are two categories of people, hierarchically ordered: experts on top; everybody else down below. The talk of those below is meaningful because they defer to the superior understanding of those above. That is not at all what happened in the story recounted here. Despite large and, it seems, never repudiated reservations, Bateson conformed his categorization to lay practices. One might think that such cases are so rare as to be uninstructive. Maybe so, but the Bateson case is certainly not a one-off. Think of Darwin's starting to use "survival of the fittest" and even "evolution" (Gayon 2009). The challenge such cases present is to integrate Putnam's insights with those of Ludwik Fleck, who noticed how deeply embedded popular language and concepts could be in the sciences and how acts of communication, teaching very much included, functioned to stabilize findings into facts (Fleck 1979, 111–25).

Elite Agreement? It is no part of Putnam's picture that experts might disagree with one another in a sustained way. Perpetual disagreement among experts is a threat to meaning. To put the point another way, meaning persists on the whole because there is consensus among the experts, albeit with regular, rapid readjustments as new information comes in. Again, the Bateson case presents us with almost a mirror image: order among the ordinary folk; chaos among the experts. Bateson was a contrarian, but so was just about ev-

erybody else studying heredity and inheritance (in and out of quotation marks). The period 1890–1910 saw huge disagreements among the experts about what was true concerning heredity and how to talk about it. An amazing range of proposals and considerations were on the table. It may be that, as Hasok Chang suggests elsewhere in this issue for "acidity," lay understanding was what ensured that the elite debate remained a debate about heredity.

Progressive Clarification of Kinds and Names? For Putnam, the experts' job is to investigate and so get ever better at sorting the world into natural kinds, ever more precisely labeled. And investigations can turn up surprising things (Putnam imagines a future in which cats turn out to be robots). Natural knowledge is cumulative, progressive—hence, in Putnamian science, there is no meaning incommensurability. How different is the Bateson case. As we have seen, he came to hold an intellectual position for which his own label was, on his own estimation, inapt. But perhaps such messiness can itself, sometimes, serve the cause of inquiry. As commentators including Evelyn Fox Keller (2002, 128ff.) have long stressed, ambiguity and multivalence are sometimes just what a term needs in order to become a scientific rallying cry.

REFERENCES

Bateson, B., ed. 1928. *William Bateson, F.R.S.* Cambridge: Cambridge University Press.

Bateson, W. 1894. *Materials for the Study of Variation*. London: Macmillan.

———. 1899/1928. "Hybridisation and Cross-Breeding as a Method of Scientific Investigation." In Bateson 1928, 161–70.

———. 1919/1928. "Science and Nationality." In Bateson 1928, 356–70.

Charnley, B., and G. Radick. Forthcoming. "Intellectual Property, Plant Breeding, and the Making of Mendelian Genetics." In "Owning and Disowning Invention," ed. C. MacLeod and G. Radick, special issue, *Studies in History and Philosophy of Science*.

Cock, A., and D. R. Forsydke. 2008. *Treasure Your Exceptions: The Science and Life of William Bateson*. New York: Springer.

Darwin, C. 1859. *On the Origin of Species*. London: Murray.

Dupré, J. 1981. "Natural Kinds and Biological Taxa." *Philosophical Review* 90:66–90.

Fleck, L. 1979. *Genesis and Development of a Scientific Fact*. Chicago: University of Chicago Press. German original published in 1935.

Gayon, J. 2009. "From Darwin to Today in Evolutionary Biology." In *The Cambridge Companion to Darwin*, ed. J. Hodge and G. Radick, 2nd ed., chap. 11. Cambridge: Cambridge University Press.

Hacking, I. 1983. *Representing and Intervening*. Cambridge: Cambridge University Press.

———. 2007. "Putnam's Theory of Natural Kinds and Their Names Is Not the Same as Kripke's." *Principia* 11:1–24.

Johannsen, W. 1911. "The Genotype Conception of Heredity." *American Naturalist* 45:129–59.

Keller, E. F. 2002. *Making Sense of Life: Explaining Biological Development with Models, Metaphors and Machines*. Cambridge, MA: Harvard University Press.

Lindee, Susan, and Dorothy Nelkin. 1995. *The DNA Mystique: The Gene as a Cultural Icon*. New York: Freeman.

López-Beltrán, Carlos. 1994. "Forging Heredity: From Metaphor to Cause; A Reification Story." *Studies in History and Philosophy of Science* 25:211–35.

———. 2004. "In the Cradle of Heredity: French Physicians and *L'Hérédité Naturelle* in the Early 19th Century." *Journal of the History of Biology* 37:39–72.
Müller-Wille, S., and H.-J. Rheinberger. 2007. "Heredity: The Formation of an Epistemic Space." In *Heredity Produced: At the Crossroads of Biology, Politics, and Culture, 1500–1870*, ed. S. Müller-Wille and H.-J. Rheinberger, 3–34. Cambridge, MA: MIT Press.
———. 2012. *A Cultural History of Heredity.* Chicago: University of Chicago Press.
Putnam, H. 1975. "The Meaning of 'Meaning.'" *Minnesota Studies in the Philosophy of Science* 7:131–93.
Radick, G. 2011. "Physics in the Galtonian Sciences of Heredity." *Studies in History and Philosophy of Biological and Biomedical Sciences* 42:129–38.
———. Forthcoming. "The Professor and the Pea: Lives and Afterlives of William Bateson's Campaign for the Utility of Mendelism." In "Owning and Disowning Invention," ed. C. MacLeod and G. Radick, special issue, *Studies in History and Philosophy of Science*.
Sapp, J. 2003. *Genesis: The Evolution of Biology.* Oxford: Oxford University Press.
Thomson, J. A. 1908. *Heredity.* London: Murray.
Zemach, E. 1976/1996. "Putnam's Theory on the Reference of Substance Terms." Repr. in *The Twin Earth Chronicles*, ed. A. Pessin and S. Goldberg, 60–68. Armonk, NY: Sharpe.