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Reinventing Richard Goldschmidt: Reputation, Memory, and Biography

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Abstract. Richard Goldschmidt was one of the most controversial biologists of the mid-twentieth century. Rather than fade from view, Goldschmidt's work and reputation has persisted in the biological community long after he has. Goldschmidt's longevity is due in large part to how he was represented by Stephen J. Gould. When viewed from the perspective of the biographer, Gould's revival of Goldschmidt as an evolutionary heretic in the 1970s and 1980s represents a selective reinvention of Goldschmidt that provides a contrast to other kinds of biographical commemorations by scientists.

Keywords: Richard Goldschmidt, hopeful monsters, evolutionary synthesis, Stephen J. Gould, punctuated equilibrium, biography

Richard Goldschmidt is one of the most controversial and enigmatic figures in twentieth century biology. During a career that stretched from 1900 to 1958, Goldschmidt became known as one of the top biologists in the world by producing groundbreaking studies of sex determination, gene action, evolution, and geographic variation. After he was forced to leave Nazi Germany in 1936, however, he also produced incredibly controversial theories denying both the existence of the gene and the possibility of gradual evolution of new species (Dietrich, 1995, 2003; Richmond, 1986). Like most scientists and their work, Goldschmidt faded from the scene after his death in 1958. Citations to *The Material Basis of Evolution* fell to zero in 1968 (see Figure 1). Citations after 1975 suggest a different story. Rather than fade into obscurity after his death, Goldschmidt's work and reputation have persisted, even flourished, in the last half of the twentieth century. Taking their cues from his contemporary opponents, prominent biologists in the last 30 years have

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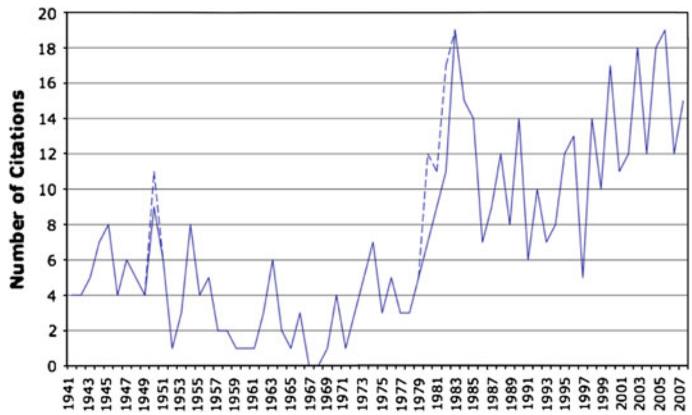

Figure 1. Citations for Richard Goldschmidt's *The Material Basis of Evolution* (1940), 1940–2008. Citation analysis drawn from Web of Science cited source search. *Dotted lines* represent author's inclusion of known sources not included in the Web of Science database

reinforced and reinvented Goldschmidt's reputation as a biological heretic (Bush, 1982; Gould, 1982a). Indeed, adopting the label of "Goldschmidtian" was a crucial aspect of Stephen Jay Gould's strategy for advocating the novelty of his own theory of punctuated equilibrium in the 1980s (Gould, 1982b, 2002). Scientific Creationists have likewise rallied around Goldschmidt's heretical reputation as evidence for an oppressive evolutionary orthodoxy ("Monster Mutation Theory", 2010). In the last 10 years, as evolutionary developmental biology has experienced a resurgence, Goldschmidt's reputation and even his theories of "hopeful monsters" have become a common gauntlet thrown down as a rhetorical flourish, a scientific challenge, and a signifier of non-conformity (Ronshaugen et al., 2002).

Where traditional biographies center the life of a person, I would like to consider how reputation and its uses extend that narrative beyond a subject's death. More specifically, I will consider how Stephen Jay Gould's efforts in the 1970s and 1980s perpetuated and shaped the reputation of Richard Goldschmidt as a scientific heretic. Reputations, as shared judgments about some aspect of a person, usually have some basis in the facts surrounding the person in question. What then is the relationship between biography as history and reputation as scientific memory?

When biologists decide to commemorate one of their own, they are actively fashioning a public memory (Abir-Am and Elliot, 1999;

Glassberg, 1996). The public goal of this activity is to celebrate the work of a fellow scientist deemed worthy of such an honor. Among molecular biologists, Max Delbrück was famously celebrated in this fashion (Cairns et al., 1966/2007). As historians and biologists have noted, this festschrift was not neutral, instead it legitimated the place and priority of the "informational school" in molecular biology (Abir-Am, 1987, 1999; Creager, 2010). For Abir-Am, these public commemoration are driven by three kinds of social pretexts: a disciplinary pretext aimed at articulating the public imagery of that discipline, an institutional pretext aimed at establishing organizational support within a field, and a genealogical pretext aimed creating a link to a prominent ancestor in order to legitimate claims of that ancestor's descendents to influence and authority (Abir-Am, 1999, p. 326). The commemoration of Delbrück was driven by exactly these forms of social pretext by biologists, such as Gunter Stent, who located himself in a lineage from Delbrück, as he offered an image of molecular biology as originating from phage research (Abir-Am, 1985, 1987, 1999). That Delbrück's origin story was contested by John Kendrew and others at the time reveals both that biologists themselves are often well aware of the social pretexts motivating such commemorations and that commemoration within a community is a process of negotiation (Abir-Am, 1999; Smocovitis, 1999).

Gould's commemoration of Goldschmidt is a bit odd, because he is celebrating an anti-hero, not a hero as is more commonly found in scientists' historical narratives (Abir-Am, 1982, Sapp, 1990; Cantor, 1996; Smocovitis, 2005, p. 46). "Hero myths" and origin stories are a well recognized form that typically loosely resemble a biography, but frequently are directed at legitimating some aspect of contemporary science (Abir-Am, 1982, 1999). In this sense, they are similar to the kinds of public myths that Nathanial Comfort uses to describe the reception of Barbara McClintock (Comfort, 2008), or the "different lives" that Jan Sapp describes when he analyzes the wide ranging accounts of Gregor Mendel's life (Sapp, 1990). Narratives of the scientist-hero often isolate the hero from their non-scientific life, isolate the hero from collaborators and supporting institutions, and isolate the hero from their cultural, social, and political contexts. The result is an individualist and triumphal narrative that supports an image of science as a privileged and value neutral pursuit (Abir-Am, 1982). Consider the stories of Gregor Mendel's long neglect. In these accounts bemoaning the long gap between Mendel's work in the 1860s and his rediscovery in 1900, Mendel is portrayed as "a creative genius clothed in monastic virtues pursuing the truth undauntedly on the frontiers of knowledge,

unappreciated by his colleagues'' (Sapp, 1990, p. 140). Of course, Mendel as lone genius is eventually vindicated, and in the process, science itself as a just and fair enterprise is vindicated. Casting the heroic Mendel as the founder of genetics portrays the field as just and free from corrupting influences both from without and from within.

Anti-hero narratives are much rarer in scientists' repertoires of stories, but, when they are told, they too are laced with moral overtones and motivated by social pretext. Indeed, narratives casting Goldschmidt as a heretical anti-hero resonate with neglect narratives as the anti-hero is pitted against the scientific establishment. In the end though, where the hero triumphs, the neglect of the anti-hero is justified and the impersonal and impartial image of science is upheld.

Ironically, Richard Goldschmidt laid the foundations for his own reputation as a heretic. He cultivated an iconoclastic flair throughout his career, but misjudged how his colleagues would receive him and his work after his immigration to Berkeley in 1936. In a new cultural and institutional context, Goldschmidt's pronouncements were not appreciated by a much more pragmatic American scientific establishment. After his death in 1958, his status as a heretic was sustained by his much longer lived opponents until it was reinvigorated by Stephen Jay Gould, who explicitly constructed Goldschmidt as an anti-hero. In doing so, Gould appropriated a public memory within evolutionary biology. That memory was in dialogue with the biographical details of Goldschmidt's life, but selectively constructed the image of a scientific life that supported Gould's standing and research more than it did Goldschmidt's. This is not to say that a scientific biography should be uncritically supportive of its subject, or that biographies are not also selective and culturally embedded, as Janet Browne's survey of Darwin biographies so ably demonstrates (Browne, 2010). Rather, the object of this essay is to illustrate how scientific biography diverges from the perpetuation of a scientific reputation as a public memory.

Richard Goldschmidt's ''Heresies''

Richard Goldschmidt was both controversial and accomplished. This combination gave even his most iconoclastic hypotheses some credibility. This combination also makes it easier for Goldschmidt to be reinvented or revived as an unappreciated innovator or a scientific heretic who may have been on to something. In this section, I will review Goldschmidt's scientific career and its reception by his contemporaries,

before turning to Stephen Jay's Gould revival of Goldschmidt in the next section.

Born in 1878, Richard Goldschmidt began his scientific career at the University of Heidelberg, taking his doctorate under the supervision of the zoologist Otto Bütschli. As an Assistant in Richard Hertwig's laboratory at the University of Munich, he spent years carefully describing the development of nervous systems in the nematode *Ascaris*. While this intricate morphological work earned him praise in the biological community and a promotion within Hertwig's lab, his interests began to shift toward the study of heredity in 1909. Perhaps because Hertwig's lab was heavily focused on questions of fertilization or because of the association of sex chromosomes with Mendelism, Goldschmidt decided to concentrate on the genetics of sex determination using the Gypsy moth (*Lymantria dispar*) as his experimental system. Gypsy moth females have white wings with dark bands, while males are smaller and have brown wings. Goldschmidt exploited the fact that when moths from different regions were mated the offspring can sometimes have intermediate sexual forms. He called these sexual intermediates "intersexes" (Dietrich, 2000, 2003; Richmond, 1986).

From the 1910s to 1930s, Goldschmidt developed a program of genetic research based on the controlled mating of gypsy moths that he had collected from around the world (Goldschmidt, 1911, 1931, 1934). Where geneticists in the tradition of Thomas Hunt Morgan tended to focus on distinct traits, such as the presence or absence of color or bristle numbers on *Drosophila*, Goldschmidt was interested in the spectrum of sexual intermediates from male to female that he could produce in his laboratory. In order to explain the range or variability of sex in his moths, Goldschmidt turned to the developmental tradition in which he was trained and articulated theories of gene action that would contribute to the foundations of developmental and physiological genetics (Goldschmidt, 1938).

Goldschmidt's physiological genetics emphasized the dynamics of the production of gene products, whether they be enzymes, hormones, or inducing substances. The link between a gene and its phenotype depended on the quantity of substance produced and the timing of its production. Goldschmidt believed that timing during development was especially important and argued for critical periods where threshold amounts of determining substances were required to produce the phenotype in question.

Goldschmidt's temporal approach to genetic regulation was foundational. For present purposes, it demonstrates his abiding confidence

that developmental processes must play a central role in the production of phenotypes. Where transmission genetics narrowed its attention to situations where genes unambiguously produced phenotypes, often in a one to one relationship. Goldschmidt valued more complicated expression patterns for what they might reveal about the underlying developmental mechanics that were so often erased or taken for granted in transmission genetics.

Goldschmidt conducted his research on sex determination in *Lymantria*, as a laboratory director at the Kaiser Wilhelm Institute for Biology in Berlin, where he was free from teaching duties and could devote his full energy to research. Because different geographic varieties were required to produce intersexes in gypsy moths, Goldschmidt made three trips to Japan to collect Asian varieties between 1914 and 1934. Scientifically, these trips provided the basis for one of the first geographic studies of genetic variation, as Goldschmidt's collections and subsequent experiments allowed him to chart strong and weak varieties of gypsy moths from Korea to Okinawa (Goldschmidt, 1960).

By 1934, Goldschmidt brought his research on gypsy moths and intersexuality to a close with the publication of a monograph on *Lymantria* (Goldschmidt, 1934). Coming 1 year after the purge of Jewish scientists by the Nazi Nuremburg Codes, Goldschmidt was preparing to leave Germany. Although he initially believed that Max Plank could protect him, Goldschmidt was forced to resign in 1935 and began a Professorship at the University of California, Berkeley in 1936.

During the 1930s, Goldschmidt was laying the intellectual groundwork for his two most controversial claims: that there was no particulate gene and that the evolution of new species occurred suddenly by a novel genetic mechanism. Goldschmidt was never shy with his opinions. In Germany he had become known for his passionate defense of his bold claims and theories (Richmond, 1986). His decision to attack the gene and the emerging neo-Darwinian synthesis upon arriving in the United States was probably exacerbated by his change in status from Director of a prestigious research institute to a Professor at Berkeley teaching introductory biology to sometimes less than deferential students. It is possible that Goldschmidt was trying to restore some of the renown he had lost when he was forced to leave Germany by creating controversy. Or it may be, as he himself wrote to an American colleague, a matter of "genetic makeup", which encouraged him to "run ahead of the facts with conclusions" and immediately assign new facts "their place within the whole" (Richard Goldschmidt to L. C. Dunn,

May 27, 1940. L. C. Dunn Papers, American Philosophical Society Library, Philadelphia, PA.).

The classical gene concept had been articulated beginning in 1915 by Thomas Hunt Morgan and his group of *Drosophila* researchers. In a nutshell, the classical gene concept asserted that the gene was simultaneously a discreet unit of structure, function, mutation, and recombination. As discreet particles, these genes were often represented as beads on a string, with the resulting chain forming a chromosome (Carlson, 1966). Goldschmidt's doubts about the classical gene began to crystallize in 1932 after Theodosius Dobzhansky confirmed A. H. Sturtevant's 1927 arguments for position effects using the Bar Eye mutant in *Drosophila* (Goldschmidt, 1944). Position effects were changes in phenotype resulting from recombinations in chromosomes that rearranged the order or position of genes. According to the classical gene concept, a gene's neighbors should have no influence on a gene's function. Position effects revealed that in fact adjacent genes did have influence. This called into question the idea that genes had the sharp kinds of boundaries that Morgan had postulated.

Throughout the 1930s a number of researchers actively investigated the phenomena of position effect (Dietrich, 2008). From the Soviet Union, future Noble laureate H. J. Muller and his colleagues carefully mapped three mutants, yellow, scute, and acheate to the tip of the x chromosome in *Drosophila*. By carefully documenting patterns of chromosomal inversion that rearranged that part of the x chromosome, Muller and his coworkers demonstrated that scute seem to be extended over a large area, that rearrangements in many different parts of that area produced the mutant effect, and that yellow, scute, and acheate even seemed to overlap with each other (Muller et al., 1935; Raffel and Muller, 1940). From Goldschmidt's perspective Muller's research suggested that mutations were in fact rearrangements, which could occur in different sizes, from large visible inversions to invisible rearrangements that would not even alter the newly discovered chromosomal banding patterns. Moreover, while mutations and breaks may be specifically located at a point on a chromosome, Goldschmidt argued that Muller's results revealed that the location of the gene on the chromosome was certainly much more extended. Mutation then was not a reliable guide to the structure and function of a normal gene (Dietrich, 2008).

In his biographical memoir of Goldschmidt, Curt Stern, who had begun his career as Goldschmidt's assistant in Germany, remarks that Goldschmidt's reception in the birthplace of the gene was certainly tainted by his announcement in a funereal voice that "The theory of the

gene is – dead!'' (Stern, 1967, p. 83). Goldschmidt's rejection of the gene raised the hackles of the Morgan group. Others, such as Muller and L.C. Dunn, were more sympathetic, but unwilling to accept Goldschmidt's sweeping generalizations that all mutations were rearrangements or that the gene should be replaced with a hierarchy of genetic units of structure and function (Dietrich, 2000).

Goldschmidt's rejection of the gene was a prelude to his rejection of the gradual evolution of new species. First delivered as the Silliman Lectures at Yale University in 1939, Goldschmidt's *The Material Basis of Evolution* was described by him as the phylogenetic consequences of his view of the gene (Goldschmidt, 1940). While the connection to his rejection of the classical gene is unmistakable, in fact Goldschmidt' treatise drew on years of research in evolutionary biology especially his earlier work on the geographic distribution of Lymantria. As Goldschmidt told L. C. Dunn at the time, *The Material Basis of Evolution* would be ''typical Goldschmidt with everything I like about him, and some others dislike'' (Richard Goldschmidt to L. C. Dunn, May 27, 1940. L. C. Dunn Papers, American Philosophical Society Library, Philadelphia, PA).

At the heart of *The Material Basis of Evolution* is a claim that the available evidence equally supports two distinct alternatives: the neo-Darwinian theory recently articulated by Theodosius Dobzhansky in *Genetics and the Origin of Species* that speciation is a result of the gradual accumulation of small genetic variations, and Goldschmidt's saltational alternative that postulates different mechanisms for evolution above and below the species level (Dobzhansky, 1937). Goldschmidt's aim was to layout the saltationist alternative as a challenge. In doing so he was reviving a tradition of evolutionary thinking that distinguished microevolution from macroevolution and championing the claim that neo-Darwinism had only addressed the mechanisms of microevolution (Goldschmidt, 1940).

The Material Basis of Evolution is a masterful review of the evolutionary literature that is even more impressive when we realize that it was written in spiral notebooks almost without revision as Goldschmidt sat in the garden of his Berkeley home recovering from a heart attack. After reviewing the evidence for microevolution, Goldschmidt argues for two possible distinct mechanisms of macroevolution: systemic mutations and developmental macromutations.

Based on his claim that all mutations were chromosomal rearrangements, systemic mutations were hypothesized rearrangements of chromosomes on a massive scale. These large genetic changes could

possibly create an original and stable phenotype. This new species-level phenotype would appear suddenly as the individual rearrangements became coordinated and produced a set of reactions capable of crossing their expression thresholds. The genotypic rearrangements underlying systemic mutations could accumulate over a long period of time, but their subsequent phenotypic change would appear quickly. Moreover, the phenotype produced by a systemic mutation would be of a new kind of trait, instead of a change in some aspect of the same kind of trait. This ability to create a novel trait made systemic mutations the kinds of genetic changes capable of creating a new species – it made them distinct macroevolutionary mechanisms (Dietrich, 2009).

The chief drawback of system mutations was that no one had ever observed one. Goldschmidt admitted as much, but his goal was to argue that such a mechanism was possible and had not been ruled out by the neo-Darwinian approach. To bolster his case for systemic mutation, Goldschmidt offered an argument by analogy to a second macroevolutionary mechanism based this time on small genetic changes to developmentally important genes that would then have large phenotypic effects. These developmental macromutations relied on a more traditional understanding of the gene and were the mechanism for producing what Goldschmidt called "hopeful monsters." In Goldschmidt's words, "a single mutational step affecting the right process at the right moment can accomplish everything, providing that it is able to set in motion the ever present potentialities of embryonic regulation" (Goldschmidt, 1940). Hopeful monsters then represented the integration of evolution, development, and genetics. Unlike systemic mutations, Goldschmidt believed he had ample evidence for hopeful monsters in the homeotic mutations of *Drosophila*, which are characterized by the appearance or transformation of body parts. Dramatic homeotic mutants transforming two winged flies into four winged flies, and antennae into legs, had been documented in *Drosophila* since 1918. For Goldschmidt, the homeotic mutant bithorax mirrored the macroevolutionary transformation of dipterans from four wings to two. A small, single genetic change then could produce profound morphological differences that, in Goldschmidt's words, "demonstrate that it is possible, and even probable, that macroevolution takes place without accumulation of micromutations under pressure of natural selection" (Goldschmidt, 1940, p. 331).

In retrospect, Goldschmidt described the reaction of Theodosius Dobzhanksy, Ernst Mayr, and G. G. Simpson, the architects of the evolutionary synthesis, as savage (Smocovitis, 1996). In his words,

he "had struck a hornet's nest" (Goldschmidt, 1960). While reviews of *The Material Basis of Evolution* were negative, they were not dismissive. Important figures such as Dobzhansky and Sewall Wright argued that Goldschmidt's challenges had to be taken seriously (Dobzhansky, 1940; Wright, 1941). Nevertheless, Goldschmidt's systemic mutations were quickly rejected. They simply pinned too much on the idea of chromosomal rearrangements on a scale that had never been observed. Hopeful monsters, however, would gradually find a place in evolutionary biology.

Goldschmidt forcefully advocated for homeotic mutations as examples of developmental macromutations in a series of papers throughout the 1940s and 1950s (Goldschmidt, 1945, 1946, 1952a, b; Davis et al., 2009). Goldschmidt's evolutionary claims for homeotic mutants met with some initial resistance from architects of the synthesis. Theodosius Dobzhanksy, who had earlier done research on the homeotic mutants in Drosophila, was not convinced that the large phenotypic changes could be considered fundamental in part because the famous four winged fly produced by bithorax could not survive in the wild (Dobzhansky, 1941). Sewall Wright and G. G. Simpson worried that Goldschmidt had considered the population dynamics necessary for a new mutant to succeed and spread. In Simpson's words, "the appearance of a mutant individual is not evolution" (Simpson, 1944, p. 53). Goldschmidt later persuaded Wright to consider how large mutations could spread through a population, and by 1950, Wright included large-effect mutations as a part of his shifting balance theory of evolution (Wright, 1950; Dietrich, 2000). Goldschmidt eagerly embraced Wright's model of the evolution of large-effect mutations (Goldschmidt, 1952a, pp. 101–103). However, Goldschmidt still seemed wary to invoke the large number of modifying mutations that Wright invoked to fine tune a major mutation into a viable and selected trait. According to Goldschmidt, these modifiers were not necessary because the regulatory and integrative processes of development could produce the needed modification between genotype and phenotype. From Goldschmidt's perspective, "evolution is not only a statistical genetical problem but also one of the developmental potentialities of the organism" (Goldschmidt, 1952a, p. 103; Dietrich, 2000).

The most far reaching criticism of Goldschmidt's interpretation of homeotic mutations as developmental macromutations was articulated by both Dobzhanksy and Simpson when they claimed that homeotic mutations were merely mutations of large effect. They did not deny that homeotic mutations acted in development to produce significant

phenotypic differences, but they did not accept that these changes result in new species. Instead, homeotic mutants were assimilated into the neo-Darwinian synthesis as another means for producing variability. In his third edition of *Genetics and the Origin of Species*, for instance, Dobzhansky argues that the genetic and morphological changes that homeotic mutations create are subject to the same process of selection and speciation as any other type of mutant (Dobzhansky, 1951). Simpson even agreed that Goldschmidt's model for gene expression appeared to be "at least a possible physiological mechanism for the production of the observed variations" (Simpson, 1953, p. 76). However, Simpson separated this bit of physiological genetics from Goldschmidt's further evolutionary interpretations (Davis et al., 2009).

The tempestuous reception of his evolutionary ideas and his persistent defense over the next 15 years helped secure Goldschmidt's reputation as a controversial figure. Goldschmidt was aware that his views were not widely accepted and believed that eventually he would be proven correct. In a letter to Ernst Mayr in 1945, Goldschmidt described himself as a "hopeless case" because "instead of following the crowd I still believe in my impossible ideas." Rather than see the matter settled by the arguments presented by Mayr, Dobzhansky, and Simpson, Goldschmidt continued to look for ways in which new evidence could support some of his views. Always confident, he wrote, "I expect the day to come when I shall hear in my grave somebody saying, "That son of a gun Goldschmidt was right after all" (R. B. Goldschmidt to E. Mayr, September 20, 1945, Richard B. Goldschmidt Papers, Bancroft Library, University of California, Berkeley). While Goldschmidt never convinced Mayr of this, his controversial views did not prevent others from celebrating his accomplishments, electing him to the National Academy of Science, the Genetics Society of America, which he declined, and the Presidency of the International Congress of Genetics. Indeed, at the end of his career, Goldschmidt was celebrated by his friends and colleagues as a masterful biologist, a magnanimous mentor, and a courageous innovator (Comments at Goldschmidt Memorial by Curt Stern and Richard Eakins, n.d., Goldschmidt Family Private Papers in possession of the author).

Gould's Revival

Beginning in 1977, Goldschmidt was revived and reinvented by Stephen Jay Gould. In an article on Goldschmidt's hopeful monsters, Gould

claimed that, as "a Darwinian, I wish to defend Goldschmidt's postulate that macroevolution is not simply microevolution extrapolated, and that major structural transitions can occur rapidly without a smooth series of intermediate stages…" (Gould, 1977, p. 24). These postulates formed the core of the idea of punctuated equilibrium that Gould and Niles Eldredge were championing at the time. Indeed, punctuated equilibrium itself becomes an object of controversy as Gould began to associate it with Goldschmidt's views in the late 1970s and early 1980s (Sepkoski, 2008, 2009). Gould's writings on Goldschmidt presented him as a heretic, and that reputation as a heretic forms a useful association as Gould sought to mark his new views as innovations. In the face of criticism, Gould refined his relationship to Goldschmidt, but his reinvented heretic remained. Gould's revived Goldschmidt's memory, but his recollection was partial and partisan, when contrasted to expectations we might have of a scientific biography.

Gould's revival began with his column in *Natural History* heralding the return of hopeful monsters (Gould, 1977). Likening Goldschmidt to George Orwell's character Emmanuel Goldstein from *1984*, Gould at once offered Goldschmidt as the object of biologists' "two minute hates" during the 1960s and a "famous geneticist" who would be "largely vindicated in the world of evolutionary biology" in the decade to come. In this account, the neo-Darwinian synthesis was presented as the "reigning, if insecure, orthodoxy." In Gould's words, Goldschmidt "broke sharply with the synthetic theory, however in arguing that new species arise abruptly by discontinuous variation, or macromutation." These macromutations produced the hopeful monsters that could lead to new species, to macroevolution. Gould was careful to note that he does not endorse everything Goldschmidt claimed, after he argues that "defenders of the synthetic theory made a caricature of Goldschmidt's ideas in establishing their whipping boy." Instead, Gould champions "Goldschmidt's postulate that macroevolution is not simply microevolution extrapolated, and that major structural transitions can occur rapidly without a smooth series of intermediate stages." In the remaining article, Gould discussed the limits of gradualist thinking and the nature of microevolution, but when he discusses Goldschmidt's mechanisms, he mentioned only developmental macromutations, leaving out the systemic mutations at the heart of Goldschmidt's book. In particular, Gould focused his attention on small genetic changes in genes controlling the rate of developmental processes. In Goldschmidt, Gould found someone he could cast as a like mind; someone who was enamored with timescales and the relationship between the two

historical processes of development and evolution. As such, Gould's essay shamelessly plugged his 1977 book *Ontogeny and Phylogeny* and makes a point of saying so. In setting Goldschmidt against an evolutionary orthodoxy, Gould made his case for pluralism in evolutionary biology while marking himself as both an innovator and an underdog pitted against orthodox evolutionary "dogma".

In 1980, Gould joined the ranks of evolutionary biologists who proclaimed the advent of a new or improved or completed evolutionary synthesis. These narratives typically take the form of detailing the shortcomings of the synthesis and then describing how these shortcomings can be overcome by some new theory or field of research. Like Gould's account of Goldschmidt against the orthodoxy, these narratives posit an evolutionary orthodoxy to be overcome or reinvented. In his "Is a new and general theory of evolution emerging?," Gould coins the term "The Goldschmidt Break" to mark the shortcomings of the neo-Darwinian synthesis that sought a unified explanation of evolution above and below the species level (Gould, 1980; Smocovitis, 1996).

Following Goldschmidt's advocacy of the bridgeless gap between micro- and macroevolution, Gould argued that "there is a discontinuity in cause and explanation between adaptation in local populations and speciation; they represent two distinct, though interacting, levels of evolution." As before, Gould added the caveat that he did not accept all of Goldschmidt's arguments about "the nature of variation." Instead, Gould wanted Goldschmidt to mark a discontinuity of levels and presumably mechanisms of evolution. To this end, Gould returned to Goldschmidt's mechanisms and explicitly rejected systemic mutations while championing mutations in regulatory regions, especially those controlling timing. Regulatory mutation is, in his words, "a major focus in the study of heterochrony (effects, often profound, of small changes in developmental rate upon adult phenotypes); it is also implied in the emphasis now being placed upon regulatory genes in the genesis of macroevolutionary change (King and Wilson, 1975) – for regulation is fundamentally about timing in the complex orchestration of development."[1]

At the same time Gould was trying to reinvent evolutionary biology, he was convincing Yale University Press to reprint Goldschmidt's *The Material Basis of Evolution* with a new preface that Gould would

[1] King and Wilson's earlier paper had also invoked regulatory mutations, but not in connection to speciation. Instead, King and Wilson use regulatory mutations to explain a disjunction between the scales and rates of change at the molecular and morphological levels. See Dietrich, 1998, Gould, 1980, see also Gould, 2002, p. 68.

contribute. According to Jean Black at Yale University Press, the 1982 edition sold 1,647 copies over roughly a 25 year period (Jean Black, personal communication, 7/17/2007). Citation analysis for *The Material Basis of Evolution* (see Figure 1) reveals a dramatic spike in citations at the time of this reissuing. Combined with Gould's popular and scientific writing, this reissuing of Goldschmidt's book brought him back into scientific circulation (Gould, 1981/1994, 1982a, b, c, 1983). However, despite a more extensive biographical treatment of Goldschmidt by Gould in his introduction, "The Uses of Heresy," the Goldschmidt that was being revived was the controversialist, the evolutionary heretic (Gould, 1982a).

Gould's revival of Goldschmidt invited controversy of its own. Russ Lande, a population geneticist and evolutionary geneticist, defended the neo-Darwinian perspective against Gould and by extension Goldschmidt. Lande and later Lande with Brian Charlesworth and Montgomery Slatkin presented a much more detailed analysis of the genetic mechanisms and evolutionary dynamics at play in speciation and proposed developmental macromutations (Lande, 1980; Charlesworth et al., 1982). In response, Gould began to make his relationship to Goldschmidt's ideas much clearer. "Punctuated Equilibrium," he claimed, "is not a theory of macromutation (Lande, 1980 notwithstanding); it is not a theory of any genetic process …" (Gould, 1982b). Instead, Gould just retained the "Goldschmidt Break" between microevolution and macroevolution. He was willing to concede the evolutionary dynamics to Lande and others, but pleaded with geneticists for "mutual acknowledgment of interests" (Gould, 1982b).

In retrospect, Gould's revival of Goldschmidt is incomplete in two important ways. First, Gould did not champion all of Goldschmidt's evolutionary views. He selectively advocated Goldschmidt's proposal for developmental macromutations and the distinction between micro and macroevolution. Second, Gould typically reduced Goldschmidt's work to a single book, *The Material Basis of Evolution*. This narrow focus facilitated Gould's reinvention of Goldschmidt as a heretic. Only brief mention was given to Goldschmidt's career and his genetic views were incorrectly represented (Gould, 1980). Gould's Goldschmidt, therefore, lacked the intellectual trajectory that we would expect in a biographical treatment. But Gould was not acting as a biographer. He was invoking Goldschmidt's reputation in a particular controversy to make a set of claims about the history and structure of the field of evolutionary biology. Gould's intent here was not simply to use Goldschmidt to cast himself with him against the orthodoxy. While Gould seemed to enjoy

railing against the establishment, his interest in Goldschmidt went beyond self-promotion; Gould was genuinely interested in developmental timing and the role that changes in timing could play in evolution, especially macroevolution (Gould, 1977).

Conclusion

When scientists find out that I'm working on Richard Goldschmidt, they will often ask if I am seeking his vindication. At first, I found this kind of question perplexing, because I don't see history as something capable of vindicating or justifying a scientific research program. A biography can seek to explain the rise and reception of a research program by providing careful historical contextualization, but even the best history will not scientifically justify a theory or hypothesis.[2]

If you take this question of vindication as one addressing Goldschmidt's reputation, however, the place of history shifts. Can my or any historical research on Goldschmidt's life and work serve as a corrective to accounts such as those proposed by Gould? My account of Goldschmidt's life and reputation is neither objective nor definitive, but it is significantly different from the account provided by Gould in terms of the evidence that it considers and the different aspects of Goldschmidt's life and work that it can explain. Gould's reinvention of Goldschmidt as an evolutionary heretic does not touch on Goldschmidt as a physiological geneticist, sex determination researcher, journal founder, Institute Director, advisor, mentor, teacher, art collector, spouse, or parent as a full biography might. Goldschmidt as heretic abstracts away from these biographical details to a reputation that becomes a shared memory. Gould is by no means alone in this construction. Remembering Goldschmidt as a heretic has appeal to both scientists and historians. I myself am guilty of placing a great deal of emphasis on Goldschmidt's evolutionary research and its controversial reception (Dietrich, 1995). My rationalization was that I wanted to explore the dynamics of scientific controversy, even though I used the label of heretic freely. In doing so, I may have contributed to the life of a heretical reputation that I am now seeking to more carefully historicize.

Gould's promotion of Goldschmidt as evolutionary heretic is not a condensed biography. It does not follow Goldschmidt's personal or

[2] Pnina Abir-Am insightfully discussed how history is used by molecular biologists as a form of secondary legitimation, but this use is distinct from the "primary" legitimation I refer to here. Abir-Am, 1987.

scientific trajectory, tells us little about him as a person, and deliberately seeks to make use of his ideas in a new scientific context. The issue of reputation explicitly focuses on assessments by others. Of course, biographers do not ignore these elements of how a person or their work were received by the public or within a scientific community. Indeed, in his biography of Francis Crick, Robert Olby deliberately begins his narrative with an account of the events surrounding Crick's winning the Nobel Prize. The rest of the biography is then written as either prelude or postlude to this moment of public recognition and personal glory (Olby, 2009). Goldschmidt's reception by his peers is likewise absolutely important to our understanding of his person, his decisions, and his research. Goldschmidt's reception has outlived him. The life of a reputation does not end with its subject's life. Gould's assessment and use of Goldschmidt's work on evolution will tell us nothing about Goldschmidt's character, because he will not react to it. It will not influence Goldschmidt's future research, because he will not be doing any.

Gould's revival of Goldschmidt's reputation as evolutionary biology's anti-hero is doing a different kind of work from that of a scientific biography. Where the hero struggles and triumphs, the anti-hero struggles and fails. The root of that failure though is crucially important: it could be the result of a tragic flaw of character or overwhelming opposition from without. Gould's narratives of Goldschmidt cast him as an innovator opposed by an emerging orthodoxy. In offering these anti-hero narratives, Gould suggests an identification with Goldschmidt that casts both Goldschmidt and himself as outside the same evolutionary orthodoxy, as scientific innovators, and as unjustly unappreciated. Gould's narratives provide a way for him to place himself in an unorthodox tradition of evolutionary biology that marks his own contributions as original and deserving of recognition. Casting Goldschmidt as an unjustly maligned anti-hero is a means of generating sympathy for him as a historical figure that Gould wishes to extend to his ideas and to the dynamics of recognition within evolutionary biology. Because we empathize with Gould narrative of ostracism and neglect, we become favorable disposed toward offering a "second chance."

Gould's narrative does not function as "secondary legitimation," especially since he backed away from the content of Goldschmidt's science rather quickly when pressed (Abir-Am, 1987). Indeed, when Gould arranged for *Paleobiology* to run a set of reviews of the reissued edition of *The Material Basis of Evolution*, Alan Templeton forcefully made the case that the book should remain unread (Templeton, 1982). In the wake of such reviews, too close an association with Goldschmidt

would create secondary delegitimation. Instead, invoking Goldschmidt borrowed notoriety and drew attention to Gould's work as boldly innovative.

Invoking Goldschmidt as heretic is rarely intended to make a historical or biographical claim: it is intended to make an argument that evolutionary biology is divided into an orthodoxy and a heterodoxy. Seen in its best light, Gould used Goldschmidt's heresies to make a plea for pluralism against a perceived dogmatic orthodoxy. However, as Gould and other have noted, Goldschmidt's bad reputation could also be used as a moral lesson to reinforce the evolutionary orthodoxy. In either case, conveying Goldschmidt's reputation to others is not intended as condensed biography, but as a way of communicating information about the political terrain of evolutionary biology as well as where one stands and how one wishes to be considered. Becoming a purveyor of Goldschmidt's reputation is more about fashioning a scientific self-image than crafting Goldschmidt's biographical narrative.

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