1-1-1996

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Throughout the 1930s two of Germany's most senior geneticists were caught up in controversy as they tried to enter the distinctly American culture of Drosophila *genetics. When Richard Goldschmidt and Victor Jollos were forced by the Nazis to leave Germany in 1936 and 1933, respectively, this type of conflict intensified. The experiences of Goldschmidt and Jollos as émigré scientists are interpreted in terms of a conflict of scientific styles of thought. Their Americanization, I claim, involved the modification of their scientific styles and consequently the ways in which they conceived of and presented their scientific work.*

Introduction

Of the 18 geneticists dismissed from their positions in Nazi Germany, Richard Goldschmidt and his colleague Victor Jollos were among the most senior (Strauss and Roder 1983).[1] Before they were forced into

I am indebted to a number of people for their comments and criticisms of various stages of this project. Aloha Hannah Alava, Richard Burian, Margaret Dietrich, Scott Gilbert, M. M. Green, James Griesemer, Jonathan Harwood, Nick Hopwood, Paolo Palladino, William Provine, Paul Teller, the anonymous referees for this journal, and audiences at Virginia Tech, the University of California, Berkeley, and the 1995 meeting of the International Society for the History, Philosophy, and Social Studies of the Biological Sciences. I am especially indebted to Laura Lovett for constructively challenging my understanding of historical material and providing the support and encouragement to see this and other projects through to their completion. Research for this paper was supported by the University of California, Davis Research Fellowship program, the Mellon Fellowship Program of the American Philosophical Society, and a grant from the National Science Foundation (SBER 94–12384).

1. List of refugee of geneticists also compiled from "Displaced German Scholars Available for Academic Positions: Biology," April 1936, Academic Assistance Council, L. C. Dunn Papers, American Philosophical Society Library, Philadelphia, Penn.

Perspectives on Science 1996, vol. 4, no. 3
 1063-6145/96/0403–0003$01.00

exile, both were affiliated with the elite Kaiser Wilhelm Institute for Biology in Berlin-Dahlem. At the time of his dismissal in 1933, Victor Jollos was a 46-year-old biologist teaching at the University of Berlin and researching at the Kaiser Wilhelm Institute (Brink 1941; Goldschmidt, n.d.). When Richard Goldschmidt was forced to flee in 1936, he was 57 years old and had been a director at the Kaiser Wilhelm Institute for Biology for 23 years (Goldschmidt 1960). Jollos went first to Edinburgh, then to Madison, Wisconsin. After a long search, Goldschmidt secured a position at the University of California at Berkeley.

On July 5, 1941, Victor Jollos died. Goldschmidt feared that Jollos had taken his own life as he had threatened (Richard Goldschmidt to Tracy Sonneborn, July 31, 1941. T. M. Sonneborn Papers, Lilly Library, Indiana University, Bloomington, Ind.). This was not so (Tracy Sonneborn to Richard Goldschmidt, August 4, 1941. T. M. Sonneborn Papers, Lilly Library, Indiana University, Bloomington, Ind.), but L. C. Dunn, who had worked tirelessly on Jollos's behalf, had to admit that Jollos had "not much to live for" and probably "welcomed death" (L. C. Dunn to R. A. Brink, July 15, 1941. L. C. Dunn Papers, American Philosophical Society Library, Philadelphia, Penn.). He had no job, no prospects for employment, little respect in America at large and was strongly disliked by many at the University of Wisconsin (see Lowell E. Noland to Victor Jollos, February 23, 1940. T. M. Sonneborn Papers, Lilly Library, Indiana University, Bloomington, Ind.). Goldschmidt fared much better personally, but professionally, by the time of Jollos's death, Richard Goldschmidt had become one of the most controversial geneticists and evolutionary biologists of his time and as a result had alienated himself from the majority of the genetics community.[2]

The fate of both Jollos and Goldschmidt as new immigrants was bound up in a clash of scientific styles. This clash of styles began before Jollos and Goldschmidt were forced to immigrate, as they began to experiment with the fruit fly *Drosophila* and tried to enter the distinctly American domain of *Drosophila* genetics. As émigrés, both men were trying to adapt personally and professionally, but both were also striving to regain their status as cultural and scientific leaders in a new culture and a new scientific hierarchy. The dramatic changes in setting and circumstances resulting from immigration made these differences in style much more important. As they Americanized, both Jollos and Goldschmidt altered fundamental aspects of their practices as scientists, especially the presentation and articulation of their experimental

2. Goldschmidt, himself, died of natural causes in 1958.

research programs. Examining and comparing the experiences of Jollos and Goldschmidt in terms of scientific style provides a means for understanding style as a dynamic category and for understanding immigration and acculturation as a process of negotiated adjustment (Ash and Söllner 1996; Fischer 1996).

The Migration of American *Drosophila* Genetics

In the early twentieth century, *Drosophila* flies were found in the wild all over the world, but *Drosophila* as a genetic instrument were not (Kohler 1993). The specially constructed stocks of flies essential to the study of transmission genetics found their way to Berlin-Dahlem with Curt Stern in 1926. Stern had been working in Goldschmidt's department at the Kaiser Wilhelm Institute for Biology since 1923. In his first year there, Stern did something quite extraordinary: he directly questioned the views of his professor, Herr Professor Doctor Richard Goldschmidt. Stern had written a paper offering an interpretation of crossing-over that opposed Goldschmidt's interpretation and favored the interpretation proposed by Thomas Hunt Morgan's *Drosophila* group.

Goldschmidt had been at odds with Morgan's group before, in 1917, when he questioned the reality of crossing-over and linearly organized chromosome maps by positing that genes disassociated from each other and then reassociated under some type of force of attraction. Variable forces produced by recessive genes, according to Goldschmidt, sometimes allowed genes to not reassociate at their proper site, thus producing what appeared to be a crossing-over event (Goldschmidt 1917). Goldschmidt's hypothesis was sharply rejected by A. H. Sturtevant, on the grounds that mysterious forces could not account for double crossing-over events (Sturtevant 1917; Carlson 1966). At the time, Morgan and his students, A. H. Sturtevant, H. J. Muller, and Calvin Bridges, were articulating the chromosomal theory of heredity and in the process making *Drosophila* the premier organism for genetic research.

Goldschmidt never mentioned Stern's paper to him but in 1924 offered him fellowship money from the Rockefeller Foundation's International Education Board to study *Drosophila* genetics with Morgan's group (Stern 1974; Neel 1987). When Stern returned in 1926, he effectively extended the *Drosophila* genetics network to Berlin-Dahlem and provided a crucial link to the Morgan group. Not long after Stern's return, Goldschmidt began experimental work on *Drosophila* himself.

Goldschmidt's scientific reputation up to this time was based primarily on his contributions to the study of sex determination. Sex de-

termination had been a major issue at the turn of the century and provided a proving ground for the young science of genetics as biologists from Carl Correns to Thomas Hunt Morgan tried to give various Mendelian interpretations of chromosomal and cytoplasmic material (Gilbert 1978; Maienschein 1984; Richmond 1986). Goldschmidt's work stood apart from typical Mendelian accounts by providing a quantitative interpretation of sex determination. For Goldschmidt, sex was the result of a complex relation between quantities of different types of chromosomal material. The material basis of Goldschmidt's work was the gypsy moth, *Lymantria dispar. Lymantria* are usually sexually dimorphic: females are large with dark bands on white wings, while males are small with brown wings. When two geographic varieties of *Lymantria* are mated, however, the offspring are not dimorphic. Instead they produce a high proportion of intersexes, which show intermediate sexual characteristics. This sexual plasticity helped orient Goldschmidt toward a more complex model of sex determination capable of explaining these intersexes.

Underlying Goldschmidt's work on sex determination from 1911 to 1934 was a theory of genetics that was both quantitative and physiological. Where many American geneticists during this time period, especially *Drosophila* geneticists under the influence of Thomas Hunt Morgan, emphasized gene transmission, Goldschmidt emphasized gene function or gene action.[3] In other words, Goldschmidt thought that a vital aspect of genetics was the integration of the gene with the physiological processes of development (Allen 1974; Richmond 1986; Gilbert 1988). Using *Drosophila,* Goldschmidt continued to develop a genetics that emphasized gene action. Many of his early *Drosophila* papers were on genes and development and in particular mutations affecting wing development (Goldschmidt 1931, 1935).

In 1929, Goldschmidt's first published a paper on *Drosophila* genetics received worldwide attention as he announced that he had been able to induce massive mutations by using sharp changes in temperature during the course of development (Goldschmidt 1929). At this time X-ray radiation was known to induce mutations, but the effects of temperature were not clear.[4] Goldschmidt's paper set the Drosophilists into a frenzy as they tried to replicate his results. At six different labora-

3. This is not to deny the important contributions of American geneticists, such as Sewall Wright or R. A. Emerson (Provine 1986; Kimmelman 1992), to physiological genetics nor to ascribe a scientific style to an entire nation.

4. Standfuss (1896) and Fischer (1895) had argued for the modification of butterfly wing patterns using temperature shocks and Goldschmidt was, in part, responding to their work.

tories, Alfred Sturtevant, Jack Schultz, Helen Redfield, Harold Plough, Milislav Demerec, H. J. Muller, A. S. Serebrovsky, L. Ferry, N. Shapiro, and B. Sidoroff all followed Goldschmidt's procedures as closely as possible but were not able to produce the effect (M. Demerec to A. H. Sturtevant, November 14, 1929. J. Schultz Papers, American Philosophical Society Library, Philadelphia, Penn; also Ferry, Shapiro, and Sidoroff [1930]).

In a fashion typical of Morgan's *Drosophila* group, the data from three of the replications were funneled via Sturtevant to Jack Schultz, who was given the job of writing up the results (M. Demerec to A. H. Sturtevant, November 14, 1929. J. Schultz Papers, American Philosophical Society Library, Philadelphia, Penn.). Schultz's paper was short and to the point. After describing the pains taken to replicate the experiments and the negative results, Schultz concluded that Goldschmidt's experimental mutations were in fact experimental errors (Schultz, n.d.).

The concern with these failed replications was not expressed to Goldschmidt directly but to Curt Stern (Jack Schultz to Curt Stern, October 16, 1929. Curt Stern Papers, American Philosophical Society Library, Philadelphia, Penn.). Schultz kept Stern well informed of the progress or lack of progress of the American Drosophilists but as he was preparing his rebuttal to Goldschmidt in January of 1930 a telegram from Stern arrived. It read: "Goldschmidt mutations proved withhold manuscript" (Curt Stern to Jack Schultz, January 1, 1930. Jack Schultz Papers, American Philosophical Society Library, Philadelphia, Penn.). Victor Jollos had replicated Goldschmidt's results (Jollos 1930). Schultz did withhold the manuscript but replied a few days later with a detailed defense of why he believed Goldschmidt's results to be the product of contamination. Schultz's doubts were based, in his words, on "the locus specificity, the low mortality in the culture that produced the mutant individuals, and the high fertility of these same flies," as reported by Goldschmidt (Jack Schultz to Curt Stern, February 21, 1930. Curt Stern Papers, American Philosophical Society Library, Philadelphia, Penn.)

Frustrated with his inability to replicate the results, Schultz pleaded for more information on how their culture methods could be different—did they use the same kind of molasses, how accurately was temperature regulated, how were the thermometers calibrated? Stern never received this letter, and it was not until July that Schultz heard from Ernest Everett Just that Stern was still in the dark as to Schultz's manuscript. Schultz immediately sent off another letter explaining his views (Jack Schultz to Curt Stern, August 2, 1930. Curt Stern Papers,

American Philosophical Society Library, Philadelphia, Penn.).[5] While Schultz was apologetic about the delayed communication and remained friendly with Stern, according to Franz Schrader, a cytologist close to the fly group and a close friend of Stern's, not all of the Drosophilists appreciated Stern's warning. In Schrader's words, "it has become known that you warned Schultz not to publish his anti-Goldschmidt findings in re the temperature mutations. The general opinion at Woods Hole last summer [the summer of 1930] was that that was not exactly the right thing to do, in view of the loudly voiced conviction of the Pasadena people that Goldschmidt has done very bad work on it. I think myself that you should have let them go right ahead and make damn fools of themselves. They have come around to so many of Goldschmidt's views after laughing at him that I think a good fall is just about coming to them" (Franz Schrader to Curt Stern, March 14, 1931. Curt Stern Papers, American Philosophical Society Library, Philadelphia, Penn.).

Schultz never published his manuscript, but Demerec, who had sent his data to Schultz, did arrange for all of the data to be exhibited (probably as posters) at the International Congress of Genetics held in 1932 in Ithaca, New York (M. Demerec to J. Schultz, January 29, 1932 . Jack Schultz Papers, American Philosophical Society Library, Philadelphia, Penn.). Undoubtedly, the overwhelming number of negative replications created the impression that Goldschmidt's original experiments were questionable as were Jollos's replications. This is precisely what Schultz and the Drosophilists wanted, although even Schultz hesitated to say so directly. Schultz's reply to Demerec's query about exhibiting the results ends with the statement, "From what you write, I suspect that there will be a representative collection of negative experiments . . . in this field." Schultz had crossed out his original conclusion, which read, "From what you write, I suspect that there will be a representative collection of negative experiments against which the others will seem wr[ong]"(Jack Schultz to Milislav Demerec, n.d. Jack Schultz Papers, American Philosophical Society Library, Philadelphia, Penn.).

Up to this point, Jollos was probably best known in the United States for his work on *Dauermodifikation* in protozoans (Jollos 1924; Sapp 1987). *Dauermodifikationen* are changes resulting from environmental stimuli that persist in the cytoplasm for up to 1,000 cell divisions. These modifications were considered semiheritable, since a simple cytoplasmic modification should dilute and disappear in about ten

5. Goldschmidt and Morgan had had previous disagreements, such as the one concerning crossing-over that Stern entered into. See Allen (1974).

cell divisions. Moreover, *Dauermodifikationen* appeared to be directed and adaptive. According to historian Jan Sapp, Jollos "led the theorizing" on the inheritance of environmental modifications and "assigned the modifications to the cytoplasm instead of the nucleus" (1987, p. 61). The discovery of these kinds of modifications intensified the debate over directed mutation, Lamarckian inheritance, and parallel induction and contributed to the larger controversy over cytoplasmic versus nuclear control of heredity (Sapp 1987; Harwood 1993). Indeed Goldschmidt's 1929 paper used the results of his temperature shock experiments to argue that environmentally induced modifications do not lead to genetic mutations. Cases of so-called parallel induction were, according to Goldschmidt, the result of the chance coincidence of modification and mutation (Goldschmidt 1929, p. 448). In light of his earlier work on *Dauermodifikation*, it is not surprising that Jollos set out to test Goldschmidt's 1929 results.

Jollos had studied with Richard Hertwig in Munich when Goldschmidt was Hertwig's assistant and later had worked at Robert Koch's Institute for Infectious Disease under Max Hartmann. Shortly after Hartmann was appointed to the Kaiser Wilhelm Institute for Biology, Jollos joined him and became an assistant in 1919 (Curriculum vita for Victor Jollos, n.d. Max Hartmann Nachlass. III.47.705. Archiv zur Geschichte der Max-Plank-Gesellschaft, Berlin-Dahlem). Although Jollos and Goldschmidt had worked on the same floor and probably in the same laboratory in Berlin, there was some rivalry between Goldschmidt's and Hartmann's groups. Jollos took pains in 1934 to make clear that he was not working with Goldschmidt (Jollos 1934). Hartmann also urged Jollos to continue to publish on the differences between mutation and modification, because he knew that Goldschmidt was continuing his own work on mutation and too often passed too quickly over Jollos's results on this distinction (Max Hartmann to Victor Jollos, 17 April 1934. Max Hartmann Nachlass. III.47.706. Archiv zur Geschichte der Max-Plank-Gesellschaft, Berlin-Dahlem). Nevertheless, prior to 1933, Jollos and Goldschmidt were lumped together in the eyes of the American *Drosophila* community (Jack Schultz to Curt Stern, 12 February 1930. Curt Stern Papers, American Philosophical Society Library, Philadelphia, Penn.).

For present purposes, this early confrontation is significant because it set up Jollos and Goldschmidt against the American Drosophilists and it did so on an issue of experimental competence. Because Schultz felt he could rule out the possible sources of difference in the methods followed by himself, Goldschmidt, and others, he felt justified in claiming that the results must be errors and could be attributed to the exper-

imenters, i.e., Goldschmidt. While the charge of experimental incompetence could be interpreted as simply a matter of replicability in this case, I think that the reaction to Stern's telegram indicates that issues of authority and control of the *Drosophila* network were also at stake.

This incident reveals the regulated nature of the *Drosophila* network in the United States during this period. From 1912 to 1936, Morgan's fly group distributed the specialized stocks of flies needed to do experimental work, the distribution of problems, and the distribution of credit. Thomas Hunt Morgan was the group's founder and maintained control of the group well into the 1930s when it was handed over to Sturtevant. With a few famous exceptions, the fly group shared ideas and mutants freely. Indeed historian Robert Kohler argues that this openness and cooperation were a result of the "unspoken rules of etiquette" among the Drosophilists. These rules mandated reciprocity, "the privilege of receiving stocks entailed the obligation to reciprocate." They mandated disclosure, "recipients of stocks were expected to tell donors what experiments they planned to do and to keep them informed of what the results were, especially if the results came a little too close to the donor's own line of work." Lastly, they governed ownership, problems could be temporarily owned but tools could never be owned. In short, there were to be "no trade secrets, no monopolies, no poaching, no ambushes" (Kohler 1994, pp. 143–45). This system of etiquette enabled the Drosophilists to make such progress in genetics that by 1920 they dominated the field. This hierarchical network was what Jollos and Goldschmidt had tried to enter with their experiments on temperature effects.

The Migration of German Geneticists

Of course, in the mid–1930s, Jollos and Goldschmidt did more than enter the American *Drosophila* network; they entered America itself. In 1933, the Nazis passed the first legislation dismissing Jews from university positions throughout Germany. Jollos lost his position at this time and began to search for a position abroad. He was offered a place with F. A. E. Crew's group at the University of Edinburgh and, with the help of L. C. Dunn, was offered a position at the University of Wisconsin–Madison. What Jollos did not understand at the time was that the offer from Wisconsin was not a job offer. It was a relief gesture, an offer of a place to work while Michael Guyer was on sabbatical. Jollos's paycheck came from a grant from the Emergency Committee in Aid of Displaced German Scholars, and funding was limited to two years. By the time the situation was made clear to Jollos, he had been in Madison for 18 months, and the job at Edinburgh had been filled

by H. J. Muller, who was fleeing Stalin's Soviet Union (Victor Jollos to Lowell E. Noland, February 25, 1940. L. C. Dunn Papers. American Philosophical Society Library, Philadelphia, Penn.).

Terrible economic problems befell Jollos and his family when the Emergency Committee grants ran out and no jobs were to be found. They raised money to pay rent and heating bills with lecture tours, short-term employment, and grants from various aid societies, as well as gifts from friends, the sale of their oriental rugs, and Mrs. Jollos's income from giving piano lessons (Harwood 1993, p. 126; Victor Jollos to L. C. Dunn, May 3, 1937. L. C. Dunn Papers. American Philosophical Society Library, Philadelphia, Penn.). These difficulties are crucially important for understanding the fate of the Jollos family, but Victor Jollos's difficulties with his new American colleagues started well before his economic troubles. Much of Jollos's professional difficulty, I think, can be explained in terms of a conflict of scientific styles between himself and his colleagues at Wisconsin and in American genetics at large.

Both Goldschmidt and Jollos were bearers of what historian Jonathan Harwood calls a *comprehensive style of scientific thought*. A style of thought, for Harwood, is a fairly stable and coherent set of attitudes. In the interwar German genetics community, a comprehensive style of thought was manifest by most geneticists "in their broad approach to the problems of genetics, their attitudes toward breadth of biological knowledge, and their cultivation of artistic sensibility, the recurring theme of striving for an all-embracing conceptual synthesis, occasionally manifest in sympathies for holism" (Harwood 1993, p. 270). This certainly holds true for Goldschmidt, who was recognized as a broadly based zoologist but with an appreciation for Goethe, a modest ability as a violinist, and discriminating taste as a world-class collector of Chinese sculpture (Goldschmidt 1960; Stern 1967 [1980]). Jollos was similarly comprehensive. Goldschmidt describes him as "a man of rarest broadness of knowledge in Zoology as well as otherwise. I know of very few among the outstanding zoologists who could beat him. He is in addition a trained bacteriologist (M.D.) and a great linguist" (Richard Goldschmidt to T. M. Sonneborn, April 17, 1940. T. M. Sonnebron Papers, Lilly Library, Bloomington, Ind.).

The minority of German geneticists in this period were what Harwood calls *pragmatics*. For a pragmatic, "the aim of science . . . was to develop not a unified picture of nature, but rather a tool for prediction and control" (Harwood 1993, p. 270). In contrast to Goldschmidt and Jollos were, for instance, Erwin Baur and Hans Nachtsheim who had both worked their way up from the Berlin Agricultural College to the

Kaiser Wilhelm Institute for Breeding Research and the Kaiser Wilhelm Institute for Anthropology, Human Genetics, and Eugenics, respectively. Baur and his group were focused on specific problems, geared toward producing results, and very much tied to the Ministry of Agriculture and private industry (Harwood 1993, pp. 210–18).

It is significant that Curt Stern is also classified by Harwood as a pragmatic (Harwood 1993, p. 248). To some of his contemporaries, Stern appeared more American than German in his approach to genetics (Harwood 1993, p. 309). According to geneticist Bentley Glass, Stern fit "perfectly from the beginning" with "the spirit of American science" (quoted in Harwood 1993, p. 309). Certainly, Stern's early training with Morgan's group introduced him to this distinctly American style of experimental genetics. Indeed, Stern was so well thought of by the American Drosophilists that he was offered a job in Morgan's department at the California Institute of Technology in 1928 (Harwood 1993, p. 308). Stern did not take this offer and, like Goldschmidt and Jollos, was forced to immigrate in 1933. Stern found permanent positions first at the University of Rochester and, with Goldschmidt's retirement in 1948, at the University of California, Berkeley. Unlike Goldschmidt and Jollos, Stern had a very successful career in the United States. Stern's success was in part due to the large network of contacts he had with American biologists and his early exposure to *Drosophila* genetics. It is important to note that at the time of his immigration, Stern was 31 years old, much younger than either Goldschmidt and Jollos, and much more entrenched in a research program grounded in an American tradition of genetics. Like the majority of Morgan's *Drosophila* group, Stern was a pragmatic.

The roots of these two different styles are located in Germany's changing social and cultural context. The difference between comprehensives and pragmatics in German genetics is explained in terms of different responses to modernization and industrialization. Comprehensives embraced an educational ideal that emphasized the development of the whole person by cultivating the highest cultural values. This cultivation was based on achievement and had enormous appeal to the middle class. Moreover, the promise of personal unity and harmony gained through this cultivation was extended to cultural unity and eventually political unity. In the last half of the nineteenth century, the educated middle class, who defended this educational and cultural ideal, was gradually being replaced by industrialists in the German government. In response to the changing social and economic conditions that were driving academics from positions of power in German society, a crisis of education was invoked. Industrialization was selling

out the high values of *Kultur* for the inferior values of modernization. The orthodox comprehensive embraced *Kultur.* The orthodox pragmatic embraced modernization (Harwood 1993, pp. 274–83; also Ringer [1969] 1990; Pueckert 1987).

It is important to note that during the early twentieth century this crisis of values was actively debated in German academia, and the rhetoric used was that of Americanization. *Amerikanisierung* was equated with modernization. It was associated with increasing industrialization, increasing emphasis on the practical, increasing fragmentation of knowledge, and increasing specialization. Especially among those identified as comprehensives, there was a deep dissatisfaction with the trend toward "narrow expertise" and "conscientious but pedestrian" work (Harwood 1993, p. 281). There seemed to be no coherent overview within many traditional academic disciplines.

Goldschmidt was a very strong defender of *Kultur.* His semipopular and professional writings both reinforce the value of a broad understanding of biology and target for criticism the increasing authority claimed by specialists, such as the sexologist Magnus Hirschfeld (Goldschmidt 1916, 1922). While I have found no direct evidence of similar sentiments from Jollos, it is clear that Goldschmidt considered Jollos to be a soulmate when it came to *Kultur* (Goldschmidt, n.d.).

In light of the attitude toward modernization and Americanization that informed comprehensive attitudes, it should not be surprising that Jollos and Goldschmidt would find their own Americanization on immigration difficult. This difficulty was made even more acute by the extremely pragmatic disposition of most of the members of Morgan's *Drosophila* network. The American *Drosophila* network was geared for production with its careful distribution of materials, topics, and credit. While Americans did not experience the same debate over *Kultur,* they did experience a deep anti-intellectualism that did not give knowledge and academics the kind of respect or power they received in Germany (Hofstadter 1962; Harwood 1993, pp. 189–90). American sciences like genetics developed in an atmosphere that emphasized practical results and experimental progress. Recent work on the strong connections between genetics, eugenics, and agriculture speak to this kind of emphasis (Kimmelman 1983, 1992). Similarly, Morgan's explicit decision to orient his group's work toward transmission problems, which could be addressed experimentally with decisive results, instead of pursuing problems of gene action, which required an understanding of the very complex processes of development, speaks to the dominance of a pragmatic style in American *Drosophila* genetics (Maienschein 1991, p. 259; Harwood 1993, p. 49–50).

In his biographical memoir of Goldschmidt, Curt Stern remarks that Goldschmidt waited until he arrived in America, the birthplace of the gene, to announce in a funereal voice: "The theory of the gene is—dead!" (Stern 1967, p. 83). Goldschmidt's pronouncement did not play well in the *Drosophila* network.

Goldschmidt's rejection of the particulate gene is usually associated with his strong belief in the power of position effects. A position effect occurs when the location of a piece of genetic material alters the effects of that material. The particulate theory of the gene saw genes as indivisible beads on a string. In the mid–1930s, the particulate gene was, in L. C. Dunn's words, showing "some signs of disappearing in a cloud of position effects" (L. C. Dunn to Richard Goldschmidt, November 15, 1937. L. C. Dunn Papers, American Philosophical Society Library, Philadelphia, Penn.). Past historiography has duly emphasized the impact of position effects on Goldschmidt's position but has not examined how Goldschmidt supported his position with experiments of his own (Carlson 1966; Allen 1974).[6] Goldschmidt's opposition to the particulate gene was grounded in a series of experiments on spontaneous mutation or mass mutation. These were done with *Drosophila* and were, in general, poorly received.[7]

As Goldschmidt toured the United States in 1935, desperately trying to find a job and raise money with lecturing fees, he began to articulate his doubts about the particulate gene. At the time, his views were almost exclusively based on the experiments of others on position effects and on the ability of X rays to produce minute rearrangements in chromosomes. The reaction was overwhelmingly negative (Goldschmidt 1960). In the summer of 1936, however, as the Goldschmidt's sailed for Berkeley and a new life at the University of California, Demerec, Plough, and Holthausen announced at the meeting of the Genetics Society of America their independent observation of a high frequency of spontaneous mutation in an inbred Florida stock of *Drosophila*. Just before his departure from Germany, Goldschmidt had also observed a

6. Historian E. A. Carlson's discussion of Goldschmidt's views refers to one paper on Goldschmidt's spontaneous mutation experiments (Goldschmidt 1937), but the bulk of his account is based on Goldschmidt's more synthetic and polemical papers (Goldschmidt 1932, 1938*b*, 1940*a*, 1940*b*, 1944*a*, 1946). Historian Garland Allen's discussion of Goldschmidt's views refers to only synthetic works (Goldschmidt 1938*a*, 1944*a*, 1946). No mention is made of Goldschmidt's papers reporting results of spontaneous mutation experiments (Goldschmidt 1937, 1939, 1944*b*; Goldschmidt et al. 1945).

7. M. M. Green has also suggested to me that another reason for the poor reception of Goldschmidt's views by the genetics community in general could be that in the 1930s position effects were known to occur only in *Drosophila*.

higher than normal number of spontaneous mutations in the same Florida stock. Demerec, Plough and Holthausen, and Goldschmidt all published accounts of their experiments in 1937 (Demerec 1937; Goldschmidt 1937; Plough and Holthausen 1937). Goldschmidt's article was significantly different from the others, however.

In his short article published in the *Proceedings of the National Academy of Sciences*, Goldschmidt used his observations of increased spontaneous mutability to argue against the existence of the gene. In a letter to L. C. Dunn, Goldschmidt admitted that the article would make him "an outcast in genetics," since he "now stated in writing, as before only orally, that there is no such thing as a gene. Horror!" (Richard Goldschmidt to L. C. Dunn, November 3, 1937. L. C. Dunn Papers, American Philosophical Society Library, Philadelphia, Penn.). Goldschmidt's view was that the burst of mutations he and others had witnessed were the result of chromosomal rearrangements, not gene mutations. He also suggested that his very-difficult-to-replicate results with temperature were actually products of spontaneous rearrangements that happened to coincide with the heat treatment. In 1938, Goldschmidt submitted the full report of his experiments to *Genetics* for consideration. In his accompanying letter, Goldschmidt admitted that the experiments were not yet completed but wanted to publish in parts starting with general comments followed by detailed analysis of particular mutants. L. C. Dunn, acting as editor of *Genetics*, was not willing to go along with this plan. Dunn recognized that Goldschmidt's paper would be of much interest because of its attack on the gene, but the assumption that all mutations were rearrangements needed to be buttressed. Dunn thought that "the data could best be judged if presented as a whole, with the pedigrees accompanied by the analytical data from locations of new changes, and salivary or other proof of the actual nature of the 'mutants'" (L. C. Dunn to Richard Goldschmidt, May 31, 1938. L. C. Dunn Papers. American Philosophical Society Library, Philadelphia, Penn.). With such a radical interpretation, Dunn thought that there had to be evidence of careful experimentation to back it up.

The quality of Goldschmidt's manuscript bothered Dunn so much that he passed it on to Curt Stern, "Not as referee, but as someone who knows the data and is his friend." Dunn was worried that the paper could do serious damage to Goldschmidt's reputation. In his reply Stern agreed. In his words, "It is indeed a very painful experience for me to read this paper and it makes one strongly doubtful of many things. I am quite of your opinion that the publication of this paper

would harm the author very much and I agree with you in trying, at least, to avoid this. . . . My first reaction regarding this paper was to write a frank letter to Goldschmidt and tell him how he made people wonder about his recent work. However, I agree with you that the style of this paper shows so definitely that outside criticisms would not be effective that I feel much at a loss and don't know what to do" (Curt Stern to L. C. Dunn, June 6, 1938. Curt Stern Papers, American Philosophical Society Library, Philadelphia, Penn.). So reinforced, Dunn held firm and refused to publish Goldschmidt's results until he had provided the analytic results from the study of the specific mutants and their representation on the salivary chromosomes.

Goldschmidt took Dunn's advice and stepped up research, but he also made his views known. The next year, 1939, Goldschmidt's article "Mass Mutation in the Florida Stock of *Drosophila melanogaster:* Details of an Old Experiment Reinterpreted" appeared in the *American Naturalist* (Goldschmidt 1939). Dunn was not at all pleased (L. C. Dunn to Richard Goldschmidt, May 27, 1940. L. C. Dunn Papers. American Philosophical Society Library, Philadelphia, Penn.). Despite his serious warnings, Goldschmidt had published something that was evidently very similar to the manuscript rejected from *Genetics.* Goldschmidt pleaded to Dunn that the *American Naturalist* article was not a report of his new experiments, merely the reinterpretation of his old temperature shock experiments from 1929. Dunn felt that Goldschmidt was promoting useless controversy at great personal risk.

While in Germany, Goldschmidt had been no stranger to controversy, but in the United States his forceful denial of the existence of corpuscular genes seriously threatened his hard-earned reputation (See L. C. Dunn to Curt Stern, May 28, 1938 and Stern to Dunn, June 6, 1938, Curt Stern Papers, American Philosophical Society Library, Philadelphia, Penn.). Undoubtedly Goldschmidt's plunge into what would be a very hotly contested dispute was partly the result of his radical change in status (Stern 1967, p. 83). It was clear to some readers of Goldschmidt's equally controversial evolutionary work that he was engaged in "a too-obvious striving for priority and credit, for a place in the scientific sun" (Hubbs 1941, p. 273).[8] Controversy may have acted as a means for Goldschmidt to try to regain some of the prestige he had lost when he was forced to leave Germany. Or it may be, as Goldschmidt says it was, a matter of "genetic makeup" that encouraged him to "run ahead of the facts with conclusions" and immediately assign new facts "their place within the whole" in order to satisfy

8. For more on Goldschmidt's controversial evolutionary views, see Dietrich (1995).

his sense of "national normalness."[9] With characteristic faith in his eventual vindication, Goldschmidt concluded this bit of introspection by declaring that "it makes me happy to have a complete picture of a field in which I am interested and I prefer being wrong to a terribly cautious agnostic. I work after all because I love to do it and because I thoroughly enjoy galloping around the field of ideas. If others enjoy teaching my horse to draw a bus, let them have their fun. Time will tell who goes farther" (Richard Goldschmidt to L. C. Dunn, May 27, 1940. L. C. Dunn Papers, American Philosophical Society Library, Philadelphia, Penn.). In this same letter to his close friend and colleague L. C. Dunn, Goldschmidt warned that his upcoming Silliman Lectures, published as *The Material Basis of Evolution,* would soon appear and were an exploration of the phylogenetic consequences of his view of the demise of the gene. In Goldschmidt's words, it would be "typical Goldschmidt with everything I like about him, and some others dislike." Goldschmidt knew that his tendency to generalize first had been the norm for him in Germany. In the United States, Dunn, Stern, and others were trying to give him the message that experiments came first.

From 1939 to 1945, Richard Goldschmidt's experimental program was devoted to analyzing completely the spontaneous mutations in *Drosophila*. During the same time period, the Drosophilists lined up their evidence against Goldschmidt's views: most notably at the 1941 Cold Spring Harbor Symposium, where Plough and Demerec set the record straight—as they saw it, the spontaneous mutations they observed were not associated with any rearrangements and were only occasionally associated with small deficiencies (Demerec 1941; Plough 1941). When Goldschmidt published his complete analysis, it was not in a top-rated journal like *Genetics* but was a 549-page monograph in the *University of California Publications in Zoology* series (Goldschmidt et al. 1945).[10] This monograph is painfully detailed: Dunn's advice was taken to the extreme. Curiously though, Goldschmidt does not end with a full-blown attack on the particulate gene. Instead, the conclusion is more careful and less speculative. Goldschmidt does not hold that his results bode ill for the particulate gene but claims that the end of his monograph is no place for a survey of the vast literature on mutation needed to fully make his case. The discussion and conclu-

9. It is not clear that this comment indicates that Goldschmidt recognized a difference in German and American styles. Scott Gilbert has suggested in correspondance that "national normalness" may refer here to Goldschmidt's intellectual heritage as a Jew.

10. A short announcement concerning spontaneous production of plexus-blistered mutants also appeared in *Proceedings of the National Academy of Sciences* (Goldschmidt 1944*b*).

sions are summaries of facts pertaining to spontaneous mutations and a list of 29 important points covered in the monograph. This is uncharacteristic. Where is the typical Goldschmidt running ahead of the facts? Why is this monograph not used to drive the last nails into the coffin of the particulate gene?

After 1945, Goldschmidt continued his research on *Drosophila,* focusing on the nature of heterochromatin and homeotic mutation. Both of these lines of research were an extension of the position he staked for himself in opposition to the particulate gene in that both rearrangements involving heterochromatin and homeotic mutants addressed higher levels of genetic organization than the classical gene (Goldschmidt 1944*a*). Significantly, although these lines of research supported Goldschmidt's opposition to the gene, they were not in themselves taken to be as controversial. The significance of homeosis for evolution was in fact widely embraced by the late 1940s (Dietrich 1995). Many of Goldschmidt's papers on homeotic mutations had strongly interpretive elements, such as the implications of homeotic mutations for evolution, but they were backed up by extensive experimentation and morphological analysis that was presented in detail in the same publication or in publications prior to these interpretations (Goldschmidt 1945, 1952*a*, 1952*b*).

Before 1939, Goldschmidt used his experimental results and other people's experimental results to reject the existence of the particulate gene. The pattern of connecting experimental results with broader biological interpretation or theory is, I claim, a typical comprehensive strategy. Both Goldschmidt and Jollos followed this pattern in their publications on *Drosophila* while in Germany and to an extent in the United States as well.

In Jollos's case, his work on temperature effects was initially motivated by the problem of directed mutation and the relationship between environmentally induced modification and mutation, as well as issues of nuclear versus cytoplasmic control (Sapp 1987, pp. 60–65). Although he distanced himself from Goldschmidt, Jollos's work on temperature effects was in direct response to Goldschmidt's 1929 paper (Jollos 1930, 1931*a*, 1931*b*, 1931*c*, 1932 1933*a*, 1933*b*, 1934). Jollos was interested in induced changes that formed an orthogenic series, and he thought his heat treatments produced a series of stepwise mutations such that consistent application of high temperatures would always produce the same series of mutational steps. Such a series led in a particular direction and so was orthogenic. In contrast, when Harold Plough confirmed Jollos's temperature results in 1934, he reported his findings but barely suggested their implication (Plough and Ives 1935).

In Jack Schultz's words, "Jollos's orthogenic stuff seems somewhat beside the point" (Jack Schultz to Curt Stern, August 4, 1931. Curt Stern Papers, American Philosophical Society Library, Philadelphia, Penn.).

Once in the United States, Jollos did what many *Drosophila* geneticists did; he spent the summer at Woods Hole. There he met and discussed his research with Thomas Hunt Morgan and Harold Plough. According to Jollos, Morgan was kind, although it was clear that he and Jollos had their differences (Victor Jollos to Max Hartmann, n.d. Max Hartmann Nachlass. III.47.706. Archiv zur Geschichte der Max-Plank- Gesellschaft, Berlin-Dahlem). Jollos's conversations with Plough were probably more significant, in light of the similarity of their research. It is clear that Plough shared all of the notes of his temperature experiments with Jollos (Jollos 1934), but without funding for equipment and assistance at the University of Wisconsin Jollos could not continue this line of research. Instead of improving and standardizing his temperature experiments as he wanted to, Jollos, with the help of Dr. Cole at Wisconsin, began research on the effects of cosmic-ray radiation on *Drosophila*. While Jollos downplays the significance of this research in correspondence with Hartmann (Victor Jollos to Max Hartmann, July 2, 1937. Max Hartmann Nachlass. III.47.706. Archiv zur Geschichte der Max-Plank-Gesellschaft, Berlin-Dahlem), the results were received well enough that two of the three articles that resulted from this research appeared in *Genetics* (Jollos 1937, 1939*a*). The third was an early report published for the National Geographic Society, which had taken stocks of *Drosophila* aloft in its high-altitude balloon, *Explorer II* (Jollos 1936). All followed a traditional format of reporting methods and results with a minimum of grand theorizing (in fact, no theorizing beyond the experiment).[11] Jollos could have sought to put this cosmic-ray research into a broader context, as he had done with his other studies on mutation and modification, but he chose not to do so.

Despite these publications, it cannot be said that Jollos enjoyed the high esteem of most American geneticists or his colleagues at Wisconsin. In a candid letter to his friend Tracy Sonneborn, Jollos admitted

11. Jollos also published a major monograph on mutation, *dauermodifikation*, and modification in 1939, but this was a project that he had agreed to do for Max Hartmann's *Handbuch der Vererbungswissenschaft* before he was forced to leave Germany (Jollos 1939*b*). The long delay in its publication is in part a result of Jollos's financial situation. Jollos chose to devote his time to writing semi- popular histories of protozoology and biology to try to raise money (Victor Jollos to Max Hartmann, December 2, 1937. Max Hartmann Nachlass. III.47.706. Archiv zur Geschichte der Max-Plank-Gesellschaft, Berlin-Dahlem; Brink 1941).

that he had made social mistakes and had incurred, perhaps for racial reasons, the wrath of Michael Guyer, in whose lab he worked. In Jollos's words,

> It is very difficult to avoid mistakes in academic and social life, when you are suddenly transferred into a new environment with traditions and customs which differ in many respects from those you were used to. Thus I might have hurt [the] feelings of some people without suspecting it, and decidedly without the slightest intention to do so. For instance I tried in semminaries [*sic*] or scientific discussions to correct evidently wrong statements or opinions of graduate students at once. This is almost always done in German Universities. It saves time, and the students learn much in this way. They don't resent being corrected in the presence of their fellow students, if it is done without malice. I learned only after some time that it is not done here, and that you have to be much more cautious in disagreeing with the views of others, at least in this university.
>
> I have to admit that my "Americanization" in this respect, and in general, took probably more time than in the case of some other European scientists; but I imagine that it is sufficiently advanced during the last years, and that all objections on this basis belong to the past (Victor Jollos to T. M. Sonneborn, February 23, [1940]. T. M. Sonneborn Papers, Lilly Library, Bloomington, Ind.).

The Jolloses did pay the price of small town life as they generated gossip by keeping German meal times and allowing Mrs. Jollos to take care of the furnace, shovel snow off the walk, or help wash glassware and prepare media in the lab (Lowell E. Noland to Victor Jollos, February 23, 1940. L. C. Dunn Papers. American Philosophical Society Library, Philadelphia, Penn.). The Jolloses also alienated the local Jewish community by accepting aid from their relief organizations but not following Jewish traditions, even though Jollos was very forthright and made it clear that, while he was of Jewish descent, he was raised Protestant and did not know Jewish traditions (Victor Jollos to Lowell Noland, February 25, 1940. L. C. Dunn Papers. American Philosophical Society Library, Philadelphia, Penn.). Nevertheless, they did acculturate.

It speaks to their Americanization that, professionally, Jollos and Goldschmidt altered the way of presenting themselves in print by minimizing connections to broader issues and contested theories. Jollos was better at this than Goldschmidt and as a result actually had articles published in *Genetics*. In fact, Goldschmidt's predilection to theorize

and stimulate controversy often kept him out of the top journals and almost out of the *Drosophila* network itself. According to Aloha Hannah Alava, a graduate student and research assistant of Goldschmidt's during the 1940s, the Cal Tech fly group would not accept stocks of flies from Goldschmidt's lab, and Sturtevant used Goldschmidt's presence at Berkeley as an excuse to never visit (Aloha Hannah Alava to the author, October 14, 1994). Ever protective of his friends, L. C. Dunn was deeply worried in 1938 as Goldschmidt tried to publish his spontaneous mutation results in *Genetics* that he would endanger his future in the United States. Dunn's letters advising Goldschmidt often contain news of Jollos's difficulties, and Goldschmidt quickly got wrapped up in doing what he could to find Jollos a position (L. C. Dunn to Richard Goldschmidt, November 15, 1937. L. C. Dunn Papers. American Philosophical Society Library, Philadelphia, Penn.). I think that the fate of Victor Jollos served as a moral lesson to Goldschmidt, as did the storm of controversy produced by his rejection of the gene and his views on evolution. The result was that Goldschmidt resisted the temptation to tie his spontaneous-mutation experiments with a new theory of the gene and in other publications actually conceded several points to his adversaries.

Goldschmidt made a serious attempt to adapt to his new scientific environment, but the pressures were not strong enough to lead him very far from the comprehensive style of thought to which he had become accustomed in Berlin. Goldschmidt had come to the United States with a fairly big reputation and had managed to secure a permanent post at a good research university. While he did suffer a loss in status, on immigration, he was relatively secure scientifically and financially. In contrast, Victor Jollos's situation was much more dire, and the pressures much more acute. Jollos left Germany with a much smaller reputation for what turned out to be a temporary position. Jollos's scientific success was crucial for his and his family's economic survival. As a result, I believe he more fully altered the presentation and conceptualization of his research to conform to the norm for the journal *Genetics*.[12] This norm reflected the pragmatic character of ge-

12. In order to confirm that this was the norm for *Genetics*, I surveyed all of the articles from 1930 to 1939 (vols. 15–24). Three hundred forty-eight of the 365 articles described original observational or experimental results with a minimum of speculation or theorizing beyond the results reported. Seventeen of the 365 articles did not fit this pattern. Thirteen of the 17 articles concerned mathematical aspects of genetics, such as Sewall Wright's famous "Evolution in Mendelian Populations," *Genetics* 16 (1931): 97–159. The remainder theorized well beyond the results presented, such as in I. J. Agol's "Step Allelomorphism in *Drosophila melanogaster*," *Genetics* 16 (1931): 254–266.

netics and geneticists influenced by Thomas Hunt Morgan and the tremendous success of his Drosophilists.

Conclusion

How scientific work is presented is not fully determined by either the nature of the underlying scientific activity or factual content. That there is some flexibility in how scientific work is presented makes the nature of that presentation and changes in modes of presentation significant sources of evidence with regard to scientific cultures (Holmes 1988; Sinding 1996). Examining the experiences of Jollos and Goldschmidt as émigrés clarifies how the presentation of their work changed in response to a new scientific culture and in doing so marks the significance of modes of scholarly presentation for understanding processes of acculturation during the intellectual migration from Nazi Germany.

Interpreting immigration through the lens of scientific styles helps to highlight the fact that, in both Jollos's and Goldschmidt's cases, the need to assimilate or Americanize ran contrary to the ideal of *Kultur* and the status and recognition it supported among the German professoriat. This tension between the need to Americanize and the need to retain the status and comprehensive values of a German Mandarin had a significant impact on how both Goldschmidt and Jollos conceived and reported their experimental research in the United States.

In Goldschmidt's case in particular, this approach to his experience as an émigré provides a much richer understanding of his rejection of the particulate gene. Previous attempts to characterize this aspect of Goldschmidt's thought have reconstructed his opposition to the particulate gene as a synthetic effort: i.e., Goldschmidt is seen as following one set of consequences from H. J. Muller's research on position effects and the production of small chromosomal deficiencies and rearrangements (Carlson 1966; Allen 1974). Some of Goldschmidt's publications concerning the theory of the gene bear this out, but this interpretation overlooks 10 years of *Drosophila* experimentation on spontaneous mutation, as well as the simultaneous pressures to Americanize and to retain a comprehensive style.

By focusing on émigré experience in terms of scientific styles, it is possible to begin to understand how the pressure to continue to do science in a style that had won both Goldschmidt and Jollos recognition in Germany ran contrary to the pressure to Americanize culturally and scientifically as they tried to establish careers in *Drosophila* genetics. Comparing Goldschmidt's and Jollos's experiences reveals that differences in level of recognition in Germany and professional secur-

ity in America profoundly affects the balance of these pressures. In Goldschmidt's case, his extremely controversial stance on the particulate gene was an attempt to retain the level of recognition he had enjoyed in Berlin, while his subsequent modification of his bold claims reflects an effort to Americanize the presentation of his research. Even though Goldschmidt toned down his radical rhetoric, his position at the University of California allowed him to remain an iconoclast. Victor Jollos could not have enjoyed the same privilege. The terrifying uncertainty of the Jolloses' economic situation forced Victor Jollos to adopt an American style of scientific presentation in an attempt to establish his full acculturation and worth as a scientist. In both cases, Americanization was a process of negotiated adjustment between old styles and new.

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