Descended from Darwin

Insights into the History of Evolutionary Studies, 1900–1970

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Chapter 14

Revisiting the Fisher-Wright Controversy

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Introduction

Between 1918 and 1932, R. A. Fisher, J. B. S. Haldane, and Sewall Wright ushered in the field of theoretical population genetics. In so doing, they accomplished a formal, or mathematical, synthesis of Darwin's mechanism of natural selection with the principles of Mendelian inheritance. Thus began the historical period of evolutionary biology called the "evolutionary synthesis," spanning roughly the 1920s to the 1950s.

The formal synthesis achieved by the architects of theoretical population genetics was not without controversy. Fisher and Wright debated the conceptual foundations of population genetics theory from 1928 until Fisher's death in 1962. Wright continued his contributions until his own death in 1988. Just over 20 years ago, Provine (1985) published his important paper laying out the crucial issues of the Fisher-Wright controversy, calling this controversy "central, fundamental, and very influential" in modern evolutionary biology (Provine, 1985, pp. 197, 217).

The controversy is persistent, too (Skipper, 2002). In this chapter, I revisit the Fisher-Wright controversy in the spirit of Provine's seminal paper, paying special attention to the direction it has taken in the past 20 years. In addition, I put a philosophical gloss on the structure of the controversy in the hope of explaining its persistence.

I begin with an overview of Fisher's and Wright's general evolutionary theories, then analyze the same three controversies Provine (1985) discussed: (a) the controversies over the evolution of dominance, (b) their general evolutionary theories, and (c) evolution of the Scarlet Tiger moth, *Panaxia dominula*. Provine was prescient in choosing these debates from the many in which Fisher and Wright engaged. Each has seen considerable continued work, and I consider some of these contributions. It is my view that disagreements about relative significance (Beatty, 1995) form a crucial point of contention in these debates. Recognizing this dimension helps explain the

persistence of disagreement. I turn first to Fisher's and Wright's general theories of evolution.

FISHER AND WRIGHT ON EVOLUTION

Fisher's Natural Selection Theory

In 1922, Fisher published the first paper adumbrating a mathematical synthesis of Darwinian natural selection with the recently rediscovered laws of Mendelian heredity. As he himself writes, Fisher discusses "the distribution of the frequency ratio of the allelomorphs of dimorphic factors, and the conditions under which the variance of the population may be maintained" (Fisher, 1922, p. 322). His aim was to respond to a set of criticisms about Darwinian natural selection, especially that it cannot be the correct explanation for the modulation of genetic variation in populations, because the genetics of populations are such that there is not enough variation available for selection to act on. In his response, Fisher considered the interaction of natural selection, random survival (genetic drift), assortative mating, and dominance. As Provine (1985, p. 199) makes clear, Fisher showed, among other things, that for a single locus with two alleles, natural selection acting to increase one homozygote would lead to the elimination of the other alleles. But if selection favored the heterozygote, the result would be a stable equilibrium of the distribution of the alleles in the population. Fisher also showed the survival at low frequency in a population of a rare mutant depended more on chance than selection. Hence, in large populations one would expect, as Darwin ([1859] 1964) had, a higher genetic variability.

Fisher (1922) and other work, for example, on the evolution of dominance and mimicry, culminated in his book, *The Genetical Theory of Natural Selection* (1930b). Here, Fisher's aim was to establish particulate inheritance against the blending theory of inheritance, then demonstrate how plausibly Darwinian natural selection may be the principal cause of evolution in Mendelian populations (*cf.* Gayon, 1998; Provine, 2001). The first seven (of twelve) chapters of *The Genetical Theory* set out Fisher's synthesis of Darwin's mechanism of natural selection and Mendelian genetics. Fisher considered the first two chapters, on the nature of inheritance and the "fundamental theorem of natural selection," the most important of the book. These two chapters accomplish the key piece of the reconciliation. By and large, the middle chapters of *The Genetical Theory* are (mainly theoretical) explorations of cases, such as dominance, sexual selection, and mimicry, to support and extend the preceding theoretical work. Nevertheless, in the fourth and fifth chapters, Fisher expands his theoretical discussion to more general issues concerning the causes of genetic variation. The last five chapters of the book explore natural selection in human populations, particularly social selection in human fertility.

The first chapter of *The Genetical Theory* consists of a consideration of the implications of a synthesis of natural selection with, alternatively, blending and Mendelian inheritance. He demonstrates natural selection may be the main cause of a population's variability. This demonstration resolved a persistent problem for Darwin's theory of descent with modification, a problem that had led biologists to abandon natural selection as an evolutionary cause: Darwin's acceptance of blending inheritance required him to imagine special causes controlling mutation, because of enormous mutation

rates demanded by the blending theory. Because Mendelian heredity did not demand such enormous mutation rates, Fisher was able to eliminate these controlling causes and, so, revive natural selection as an important evolutionary cause.

Fisher's second chapter develops, mathematically, his genetical theory of natural selection. Three key elements may be distilled from Fisher's "heavy" mathematics therein. The first is a measure of average population fitness, Fisher's "Malthusian parameter," that is, the reproductive value of all genotypes at all stages of their life histories. The second is a measure of variation in fitness, which Fisher partitions into genetic and environmental components. The third is a measure of the rate of increase in fitness, namely, the change in fitness due to natural selection. For Fisher, "*the rate of increase of fitness of any organism at any time is equal to its genetic variance in fitness at that time*" (1930b, p. 37, emphasis in original). This third element is Fisher's "fundamental theorem of natural selection," and the centerpiece of his natural selection theory. Under the rubric of the fundamental theorem, Fisher argues *cumulative evolution is primarily the result of low pressures of natural selection on mutations of small effect in the context of large (> 10^4) populations.* This understanding of Fisher's natural selection theory persisted for the past 70 years (*cf.* Gayon, 1998; Hodge, 1992; Provine, 1985, 1986, 2001; Turner, 1985, 1987).

Fisher compared the exploration of the balance of evolutionary factors and the "laws" describing them to the theory of gases and the second law of thermodynamics, respectively. Of the 1922 investigation, Fisher says,

> the investigation of natural selection may be compared to the analytic treatment of the Theory of Gases, in which it is possible to make the most varied assumptions as to the accidental circumstances, and even the essential nature of the individual molecules, and yet to develop the natural laws as to the behavior of gases, leaving but a few fundamental constants to be determined by experiment. (Fisher, 1922, pp. 321–322)

He continues the analogy in 1930, adding that

> the fundamental theorem . . . bears some remarkable resemblances to the second law of thermodynamics. Both are properties of populations, or aggregates, true irrespective of the nature of the units which compose them; both are statistical laws; each requires the constant increase in a measurable quantity, in the one case the entropy of the physical system and in the other the fitness . . . of a biological population. . . . Professor Eddington has recently remarked that "The law that entropy always increases—the second law of thermodynamics—holds, I think, the supreme position among the laws of nature." It is not a little instructive that so similar a law should hold the supreme position among the biological sciences. (Fisher, 1930b, pp. 36–37)

The received view of these comparisons is this: Fisher's interests in physics and mathematics led him to look for biological analogues (e.g., Provine, 2001; Gayon, 1998). No doubt this is part of the story. However, I think a more complete story is that Fisher built his genetical theory of natural selection piecemeal, or from the bottom up. That is, Fisher worked to justify the claim of his fundamental theorem by constructing plausible arguments about the precise balance of evolutionary factors. Thus, his piecemeal consideration of the interaction between dominance, gene interaction, genetic drift, mutation, selection, and so forth, led to his theorem. It was not,

at least not primarily, the search for biological analogues to physical models and laws underwriting the theorem.

Wright's Shifting Balance Theory

Wright (1931, 1932) are his early, classic, published work on evolution in populations under Mendelian inheritance. Often forgotten, Wright produced a steady stream of papers on evolution throughout his life, including a four-volume *magnum opus*: *Evolution and the Genetics of Populations* (Wright, 1968, 1969, 1977, 1978a). Even in the last year of his life, Wright (1988) revisited what he took to be his life's work in evolutionary biology. The paper was a (mostly) favorable reaction to Provine (1986).

In 1931 and 1932, Wright was looking for the ideal conditions for evolution to occur, given specific assumptions about the relationship between Mendelian heredity and the adaptive value of gene complexes (Wright, 1931, p. 158; 1932, p. 163). Ideal conditions, for Wright, are those producing the fastest rate of evolution to the highest "adaptive peak." By the last volume of his *Evolution and the Genetics of Populations*, Wright (1978b, p. 1415) claimed his Shifting Balance Theory (SBT) described the principal processes by which cumulative evolutionary change occurred in nature. Wright did not carry such a strong claim through all of his work on evolution. He vacillated throughout his career. In his final statement (1988) he backed off the strong claim substantially, restating his problematic from the work in 1931 and 1932 of determining the ideal conditions for evolution to occur (Wright, 1988, p. 122; *cf.* Provine, 1986).

According to Wright, accurately representing the population genetics of the evolutionary process requires many thousands of dimensions (Wright, 1932, pp. 161–163). This is because the field of possible gene combinations in the field of gene frequencies of a population is vast (approximately 10^{1000}) (Wright, 1932, p. 161). Wright used a two-dimensional graphical depiction of a metaphorical adaptive landscape as a way of intuitively conveying what can only be realistically represented in many thousands of dimensions. Wright's two main figures are reproduced in Figure 14.1.

The contour lines in the diagram represent the field of gene combinations in combination with their adaptive value. The surface of the adaptive landscape is very "hilly," according to Wright, because of epistatic relations between genes the consequences for which are that genes adaptive in one combination are likely to be maladaptive in another (Wright, 1931, 1932). Given Wright's view of the consequences of epistasis and the vastness of the field of gene combinations in the field of gene frequencies (10^{1000}), Wright estimates the number of adaptive "peaks" separated by adaptive "valleys" at 10^{800} (Wright, 1932, p. 163). Peaks are represented by "+"; valleys are represented by "–" (Figure 14.1A).

In the explicit context of the 1932 paper, Wright used the adaptive landscape to argue for his "three phase shifting balance" view of the evolutionary process (F in Figure 14.1B). Of the six windows in Figure 14.1B, Wright argued for window F. His argument was essentially a demonstration of the ways in which the conditions expressed in windows A–E solved or did not solve the problem of peak shifts. According to Wright, the ideal circumstances for a global population to reach the optimal adaptive peak requires three stages, or phases. These three phases, working in a shifting balance from one to the next, describe the ideal conditions for evolution

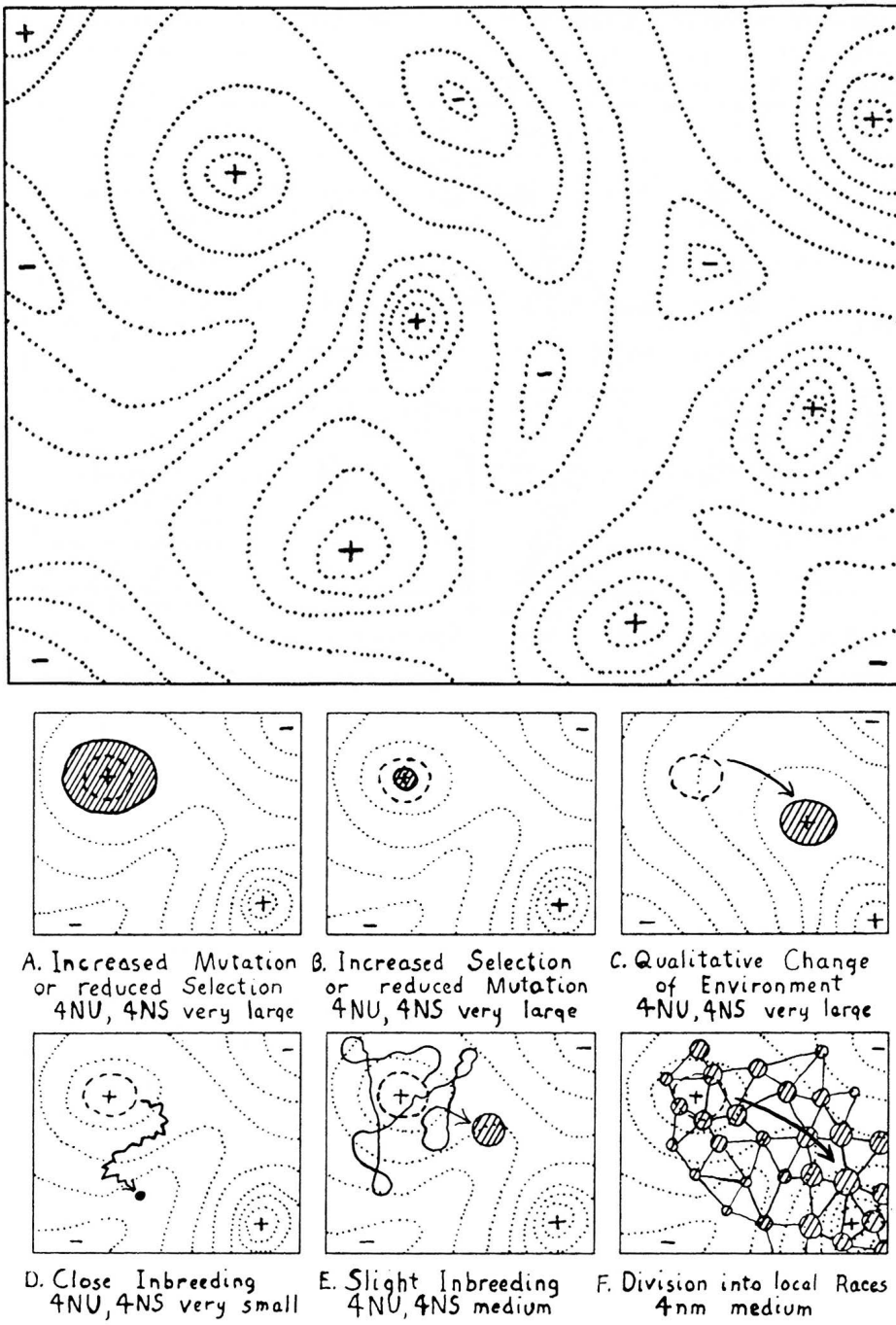

Figure 14.1. Wright's two main adaptive landscape diagrams. (A) represents Wright's view of gene combinations graded for their adaptive value. (B) represents evolution on the adaptive landscape given the surface in (A) and under alternative assumptions. Windows A–E vary selection intensity (*s*) and mutation rate (*u*) in populations of varying sizes (*N*, *nm*). Window F represents Wright's view as expressed in the SBT. Figures from S. Wright, "The Roles of Mutation, Inbreeding, Crossbreeding and Selection in Evolution," 1932, *Proceedings of the Sixth Annual Congress of Genetics, 1*, pp. 356–366, figures 2 and 4. © Genetics Society of America. Reprinted with permission.

to occur. In the first phase, random genetic drift causes gene frequencies to change and pull subpopulations semi-isolated within the global population into adaptive valleys, because random fluctuations in gene frequencies are almost always maladaptive. In phase two, mass selection will then act within subpopulations and increase their fitness, dragging them from adaptive valleys to adaptive peaks. In the third phase, selection between subpopulations, which Wright called interdemic selection, driven by differential dispersion (migration of organisms from more fit subpopulations to less fit subpopulations) would then enable the global population to be raised to its optimal peak.

Wright's SBT is summarized basically as I have described it here. Evolution proceeds via a shifting balance process through three phases:

> *Phase I*: Random genetic drift causes subpopulations semi-isolated within the global population to lose fitness.
> *Phase II*: Mass selection on complex genetic interaction systems raises the fitness of those subpopulations.
> *Phase III*: Interdemic selection then raises the fitness of the large or global population.

The understanding of the SBT I have just provided has, like that of Fisher's natural selection theory, remained unchanged for the past 70 years but for Wright's change in view of the importance of random genetic drift in the evolutionary process (Gayon, 1998; Hodge, 1992; Provine, 1985, 1986, 2001).

THE CONTROVERSIES

Provine's (1985) seminal paper examined three key controversies among the many in which Fisher and Wright engaged. These considered (a) the evolution of dominance, (b) their general theories of evolution, and (c) the evolution of the moth, *Panaxia dominula*. Introducing his discussion, Provine remarked, "here I will be able to examine briefly only three of the many controversies in which Fisher and Wright engaged, but these should give sufficient insight to see their significance" (Provine, 1985, p. 206). Provine made excellent choices. Each continued well beyond even Wright's lifetime. In what follows I revisit each.

Controversy 1: Evolution of Dominance

Beginning in 1928, Fisher's and Wright's debates over dominance were the first major public debates of the broader Fisher-Wright controversy (Provine, 1985, p. 206). Fisher and Wright each constructed a theory of dominance firmly grounded in the core assumptions of their respective theories. They believed if one or the other were shown to be incorrect, disastrous consequences for the corresponding evolutionary theory were sure to follow (Provine, 1986, p. 303). Fisher's evolutionary theory of dominance has been, if not outright falsified, shown to be obsolete. Wright's physiological theory of dominance has been shown to be at least the foundations of the correct theory (even if the evolution of dominance is a continuing puzzle). Continued work on dominance holds no substantive consequences for either Fisher's or Wright's

general evolutionary theories. Ultimately, any corroboration that the extendibility of the general theories to dominance might have afforded has been trumped by independent empirical evidence for or against the general theories.

Genetic mutations are almost always recessive to their wild-types. Fisher proposed, in 1928 and again in 1931, this phenomenon was to be accounted for evolutionarily (1928a, 1928b, 1930a, 1930b, 1931; see also 1958, 1999). In other words, Fisher claimed the dominance of the wild-type allele is not an inherently physiological phenomenon. Rather, alleles are not dominant initially, or normally; dominance evolves. Wright criticized Fisher's view in 1929 and, later in 1934, proposed his own account of the phenomenon of dominance (Wright, 1929a, 1929b, 1934a, 1934b). Wright argued, in polar opposition to Fisher, dominance is primarily physiological. Although there are a handful of alternative theories of dominance or its evolution, or both (e.g., Haldane, 1930; Muller, 1932; Plunkett, 1933), Fisher's and Wright's remain the main ancestors in the ongoing evolutionary controversy (Bagheri & Wagner, 2004; Charlesworth, 1979; Mayo & Bürger, 1997; Orr, 1991).

Fisher proposes his evolutionary account of dominance as directly connected to his general theory of evolution. It is grounded in assumptions that populations are large, that selection acts on the average effects of single allele changes, that selection pressures are small and accumulate over a long period of time, that the mutation rate is low, and that mutations are usually deleterious (see Provine, 2001, pp. 147–152). The more specific foundation of Fisher's theory of dominance is his claim that heterozygotes for rare deleterious alleles are at a much higher frequency than homozygotes in a large and randomly mating population at equilibrium (Fisher, 1928a, p. 115). Fisher believed the heterozygotes are maintained by recurrent mutation from the wild-type allele at the locus. Such maintenance, he thought, would be most likely to occur in large populations (Provine, 2001).

Fisher's theory of the evolution of dominance is as follows (Fisher, 1928a, 1930a, 1931). Mutant alleles are initially semidominant. Most mutations are observed to be recessive or nearly so to the wild-type due to selection for modifiers at other loci and this increases the fitness of the heterozygote. (Note: Fisher here ignores any intrinsically genetic effect of the modifiers; selection plays the crucial role in accounting for any such contextual effects.) Fisher referred mainly to studies on *Drosophila melanogaster* to substantiate this claim (Fisher, 1928a, p. 115; he relies on Morgan, Bridges, & Sturtevant, 1925). Selection to improve the fitness of the mutant homozygotes is ineffective because these mutants are rare relative to the heterozygotes, and also because the wild-type in the heterozygote makes them more susceptible to modification toward the wild-type. The frequency of the mutants is, over time, reduced to zero while the selection pressure remains constant. Fisher initially had no empirical evidence for his prediction of the relationship between selection and heritability when he published his evolutionary account of dominance. But soon after he was able to follow up with two cases—a case involving cotton plants and a case involving poultry—which he was certain supported his theory (Fisher, 1928b). Fisher's 1931 paper synthesizes and extends the work he did on the topic in 1928 and 1930 (Fisher, 1931; see also Fisher, 1958, 1999).

Fisher's theory has been criticized since he first proposed it. Wright's criticism was immediate (1929a; see also Haldane, 1930; Ewens, 1967). Wright showed mathematically that because the mutant heterozygotes are so rare, selection for a gene

modifying them could only be as strong as the mutation rate for mutants at the locus undergoing modification. Dominance could, thus, not assert itself. Wright also argued a weak selection pressure would be unlikely to overcome the effects of random drift or the selection consequences of pleiotropic effects of the dominance modifier. Fisher (1929) thought Wright's criticisms of his evolutionary account were weak. (Provine, 1986, pp. 243–250 provides a detailed discussion.)

Wright believed the function of most gene loci was to specify enzymes, and most mutations cause a decrease in enzymatic activity (1934a). Accordingly, dominance is explained primarily as the outcome of the normal allele at a locus being a highly active one and producing enough gene product for normal viability. This is not to say Wright thought dominance could not be modified by evolutionary processes; he did (Wright, 1927). Rather, Wright believed dominance was mainly biochemical and Fisher's evolutionary account belied that fact. Wright predicted, in contrast to Fisher, there would be a negative relationship between selection and the fitness of the mutant heterozygote. Fisher (1934) published a venomous attack on Wright's (1934a) theory, but it is hardly substantive. Wright defended himself (Wright, 1934b). The debate is thoroughly documented, and so I will not recapitulate it here (see especially Provine, 1986, pp. 299–303).

The next major development in the dominance debates came in 1979, when Brian Charlesworth published a paper crucial to furthering the critical work on Fisher's theory of dominance (Charlesworth, 1979; Charlesworth & Charlesworth, 1979). Charlesworth infers that there is a statistical pattern from a previous review of large amounts of collected data, which is inconsistent with Fisher's evolutionary account of dominance (1979, p. 848; the review is Simmons & Crow, 1977). On Fisher's view, the strength of selection pressure on the mutant heterozygotes depends only upon the mutation rate. This is what is the basis of Fisher's prediction that the semidominance of the mutant in the heterozygote should be reduced at the same rate for alleles with large effects on fitness as for alleles with small effects on fitness (i.e., the selection rate is constant while the level of the mutants is lowered to zero). If Fisher's prediction is correct, there should be no observable relationship between the fitness of the heterozygote and selection pressure (Charlesworth, 1979, p. 848). According to Charlesworth, however, experimental work on the genetics of *D. melanogaster* indicates a statistical pattern that shows an *inverse* relationship between heterozygote fitness and selection pressure (1979, p. 849). Charlesworth concludes the experimental evidence suggests either dominance is primarily a physiological phenomenon as Wright suggests (Wright's theory predicts the inverse relationship Charlesworth uncovered), or it has evolved due to factors other than those suggested by Fisher (and also Haldane).

H. Allen Orr (1991) argued he had an outright falsification of Fisher's theory of the evolution of dominance. At the time of Orr's paper there had been few tests of Fisher's, or anyone's, theory of dominance. Orr cites Charlesworth (1979) as the strongest evidence against Fisher's theory, but points out correctly that it did not amount to a direct test. Orr's paper reports his test of Fisher's theory and its results. He studied the dominance of mutations in the alga *Chlamydomonas reinhardtii*, according to him an ideal organism for such a test (1991, p. 11413). Orr reported most mutations in *C. reinhardtii* were *initially*, or *normally*, recessive (1991, p. 11414). Exceptions were semidominant mutations (Orr, 1991, p. 11414). Fisher predicted all mutations are initially semidominant. Orr takes his results to falsify Fisher's claim that dominance is

fundamentally an evolutionary phenomenon (Orr, 1991, p. 11415), plus all similar claims (see also Turelli & Orr, 1995). Orr further asserts his results are evidence for Wright's claim that dominance is mainly physiological (Orr, 1991, p. 11415).

Henrik Kacser and James Burns (1981) had already extended Wright's physiological theory of dominance. They showed any individual enzyme in a metabolic network will vary substantially in activity without necessarily affecting flux through the pathway. They conclude dominance need not have evolved. As Wright suggested, it could be simply the result of genes interacting. In other words, dominance is essentially physiological. To be clear, this view opposes Wright, who allowed for the possibility of dominance modification by evolutionary processes (Wright, 1927; see also Haldane, 1930, 1956). Savageau and Sorribas (1989) criticized Kacser and Burns, and a rather heated debate ensued (Kacser, 1991; Savageau, 1992). It should be noted, briefly, that Savageau's complaint is *not* that dominance is not physiological. Rather, it is that Kacser's theoretical framework is inadequate for understanding the physiological nature of the phenomenon. Savageau and Sorribas (1989) propose a biochemical approach they think more adequately deals with the complexity of molecular genetic properties than does Kacser's thoroughly physiological genetics approach. Currently, many biologists agree Kacser and Burns' theory is too simple, although there is less agreement concerning the best approach to capture the complexity of the phenomenon (Bagheri & Wagner, 2004; Mayo & Bürger, 1997).

In their review of work on dominance, Mayo and Bürger (1997) agree with Orr that Fisher's 1928 and 1931 theory of the evolution of dominance has been falsified. But they caution against the more general claims that dominance never results from evolution. Mayo and Bürger argue there is good reason to think room exists for both fundamentally evolutionary and fundamentally physiological theories of dominance (Mayo & Bürger, 1997, p. 105).

Mayo and Bürger (1997) also is an attempt to garner support for earlier work Bürger has done on the evolution of dominance (Bürger, 1983a, 1983b; Wagner & Bürger, 1983). The accomplishment Mayo and Bürger tout is Bürger's development of a model of the evolution of dominance that is generalized to handle multiple loci (Mayo & Bürger, 1997, pp. 106–108). Fisher's model does not do what Bürger's does. What Bürger's model shows is that when there is an advantageous allele sweeping through the population, as would be the case in visual predation, a positive relationship between the fitness of a mutant heterozygote and selection pressure may exist (Mayo & Bürger, 1997, pp. 99–100, 106–108). Thus, although Wright's (1929a), Ewens's (1967), and Charlesworth's (1979) theoretical objections to Fisher's model remain, they do not also apply to Bürger. However, Bürger's model does nothing to dismiss Orr's (1991) empirical findings (see also Turelli & Orr, 1995).

Mayo and Bürger draw heavily, but not solely, on Kettlewell's famous studies of industrial melanism in the moth *Biston betularia* to ground their claim dominance evolves according to Bürger's model (Mayo & Bürger, 1997, pp. 99–101). However, Rudge (1999) has recently argued there are problems with the standard interpretations of Kettlewell's data. Furthermore, Mayo and Bürger give no evidence or argument to substantiate the claim that dominance is ever *primarily* evolutionary. Thus, at best, Mayo and Bürger can only claim the evolution of dominance is a special case. Even then, there is no reason to think it is mainly anything but a physiological phenomenon. More recently, Bagheri and Wagner (2004) argued an insight due originally to Wright

(1929a, 1929b, 1934a, 1934b; see also Haldane, 1930) captures what is currently the best work on the topic: dominance is primarily biochemical. Questions remain about which is the right approach to extend Wright's physiological theory. These arise out of criticisms of the Kacser and Burns (1981) model. Importantly, however, dominance made be modified evolutionarily. Both Haldane and Wright adumbrated what Bagheri and Wagner think is at least the foundations of the correct model: Modifier alleles could have fitness effects that could be manifested independently of their dominance modification effects; natural selection may lead to a factor of safety against mutations (Bagheri & Wagner, 2004, pp. 1714, 1729).

In Provine's (1985, 1986) treatment of Fisher and Wright's original debates over dominance, he rightly argued they believed their work was a direct reflection of their evolutionary theories. After all, each viewed his theory of dominance as part and parcel of the assumptions that grounded his evolutionary theory. Thus, if one or the other theory of dominance were falsified or shown to be obsolete, Fisher and Wright would have taken the evidence as against the assumptions that are the basis of their evolutionary theories. As the debates have turned out, it appears Fisher would be forced to admit of a severe defect in his evolutionary theory: Its extension to the phenomena of dominance is either false or obsolete. Interestingly, biologists have not considered the implications that the recent work on dominance has for the assumptions of Fisher's and Wright's evolutionary theories. Rather, their main concern has been to criticize, or substantiate, the respective theories of dominance. In my view, there is a sense in which this lack of consideration is problematic, but another sense in which it is not.

Extending Fisher's general theory to account for the phenomenon of dominance misrepresents the phenomenon. That is, if Fisher's main assumption, i.e., there is no need to represent gene interaction in his models, is extended to cover dominance, then the phenomenon is characterized as inherently evolutionary and not, as the theoretical and experimental evidence suggests, dependent primarily upon interaction effects in biochemical pathways. In one sense, this is a problem for Fisher's assumption. The problem is Fisher's lack of emphasis on gene interaction in his models leads to a specific application of his theory obscuring the nature of the phenomenon and, moreover, leads to faulty predictions. Comparatively, Wright's emphasis on gene interaction in his models of the shifting-balance process leads to a specific application of them that reveals the genetic nature of the phenomenon to which they are applied. Successful extensions of a theory (here, a theory's assumptions) to disparate problems may corroborate the theory (or its assumptions) (see Darden, 1991).

In another sense, however, the lack of extendibility of Fisher's general evolutionary theory is not a problem. A reason to ignore the implications of the extendibility of theories in their assessment of them is the presumption that Fisher's general theory has *otherwise* much observational or experimental support and Wright's is thought to be descriptive of an evolutionary process that may only occur rarely in nature (Coyne, Barton, & Turelli, 1997, 2000). The message is straightforward: empirical evidence (direct or indirect) amassed for a theory can trump an assessment of extendibility.

Controversy 2: General Theory of Evolution in Nature

To be sure, Fisher's and Wright's debates over dominance between 1928 and 1934 revealed underlying differences between their general evolutionary theories. But it

was the publication of their major works between 1930 and 1932 that crystallized for them their apparently alternative understandings of the balance of evolutionary causes that explained cumulative evolution (Fisher, 1930b; Wright, 1931, 1932). Interestingly, Fisher and Wright agreed on the quantitative details of mathematical population genetics (Provine, 1985, p. 198). They disagreed about how to interpret those details. As we saw above, Fisher advocates natural selection as the primary explanation of cumulative evolution (Fisher, 1930b). Wright, however, thought natural selection alone was not able to solve what he took to be the key problem of evolution, which for him is the problem of peak shifts (Wright, 1932). Instead, a shifting balance of evolutionary factors was required, including genetic drift.

According to Provine (1985, p. 210), the conflict over Fisher and Wright's understandings of evolution in Mendelian populations concerned Fisher's panselectionism and Wright's apparent emphasis on the role of genetic drift. He further suggests the conflict was driven primarily by the failure of each to appreciate the full sophistication of the other's view. Provine is surely right. Fisher and Wright misunderstood the core, qualitative claims of each other's evolutionary theories. Nevertheless, there is more to their conflict during the 1930s than mutual confusion. I think the locus of the conflict is Fisher and Wright's disagreement over Wright's view that the central problem of evolutionary theory was his problem of peak shifts, that is, the problem of traversing a surface of selective value replete with adaptive hills and valleys toward the highest adaptive peak.

Fisher argued, in correspondence with Wright and in print, that Wright's problem of peak shifts was confused. Yet, Wright never bent to criticism. In fact, Wright's view of the centrality of the problem of peak shifts is what underwrote his claim that his Shifting Balance Theory (SBT) described the principal processes of evolution in nature (Wright, 1978b, p. 1415). The problem of peak shifts arises for Wright because of his emphasis on epistatic interactions between genes. As Wright understood the field of gene frequencies comprising a population, epistatic gene interaction means genes that are adaptive in one combination will be maladaptive in another. Given Wright's view of the epistatic gene interaction and the vastness of the field of gene frequencies, the surface of selective value, namely, Wright's adaptive landscape, will be replete with adaptive peaks and valleys. The problem of peak shifts follows: for a population to reach the highest adaptive peak, a way of moving from one adaptive peak, through a valley, and toward the highest adaptive peak is required. Wright's SBT solves the problem of peak shifts.

But according to Fisher, Wright's understanding of the adaptive landscape in multiple dimensions is flawed, because, as the dimensionality of the field of gene frequencies *increases* the number of stable peaks on the surface of the landscape *decreases* (Fisher correspondence to Wright, May 31, 1931, cited in Provine, 1986, p. 274). Thus, claims Fisher, representation of the adaptive value of populations in multiple dimensions is not a *hilly* landscape. Because the peaks on the landscape become unstable as the number of parameters of the landscape increases, the adaptive landscape is actually a single peak with ridges along it; there are no valleys. Evolution on the adaptive landscape does not require the complex of evolutionary factors of Wright's SBT; natural selection *alone* can carry a population to the apex of the peak.

Fisher's criticism of Wright's problem of peak shifts, mostly verbal and not mathematical in the 1930s, has persisted. There is a long line of criticism of the problem of

peak shifts based on criticism of Wright's adaptive landscape. Provine himself thought Wright's adaptive landscape was mathematically incoherent (Provine, 1989; *cf.* Skipper, 2004). During the 1960s and 1970s, theoretical biologists such as Moran (1964) and Edwards (1971) developed their own critiques of Wright's problem of peak shifts along the line of Fisher's. Most recently, Coyne, Barton, and Turelli (1997) raised this problem for Wright's problem of peak shifts. Coyne and his colleagues developed a thorough critique of Wright's Shifting Balance Theory, of which one line of attack was directed at the problem of peak shifts. Coyne, Barton, and Turelli further argued, on both theoretical and empirical grounds, that the individual phases of the SBT are theoretically and empirically problematic, and that the SBT as a whole describes a complicated evolutionary process which on its face is unlikely to occur in nature and which lacks empirical support anyway—particularly relative to Fisher's natural selection theory (Coyne, Barton, & Turelli, 1997, p. 643).

With their critique, Coyne, Barton, and Turelli rekindled the controversy over Fisher and Wright's general evolutionary theories. Between 1997 and 2000, two teams debated the theoretical foundations and empirical evidence for Fisher and Wright's general evolutionary theories (Coyne, Barton, & Turelli, 1997, 2000, on one side; Wade & Goodnight, 1998; Goodnight & Wade, 2000 on the other). There has been no resolution. In my own analysis of their debates, I argued in support of Wade and Goodnight's (1998) conclusion: it is premature to dismiss either Fisher or Wright's general evolutionary theories (Skipper, 2002; *cf.* Plutynski, 2005).

Coyne, Barton, and Turelli (1997, pp. 646–653) take their criticism of Wright's problem of peak shifts further than Fisher and others. A key flaw, they suggest, is that Wright's adaptive landscape is based on the assumption that the mean fitness of the population will not change, meaning the environment of the population is held constant. Such an assumption is highly unrealistic (as Fisher, 1930 demonstrated). Environmental changes, for instance, are common, and they may plausibly change the mean fitness of the population. If that happens, the population may be dislodged from an adaptive peak and forced into an adaptive valley. In other words, say Coyne and his colleagues, peak shifts may occur in ways alternative to those Wright envisioned. In particular, genetic drift may not be required to move a population from an adaptive peak into an adaptive valley. Other factors may change the mean fitness of the population, such as frequency-dependent selection, according to which the fitness of a particular gene combination changes because of its frequency relative to others in a population. The change in fitness may shift the peaks on the landscape. Coyne, Barton, and Turelli's conclusion is that Wright's understanding of the problem of peak shifts is confused.

The second set of challenges Coyne and his colleagues raise is directed at the first and third phases of the SBT individually, plus the three phases conjoined. They argue that phases I and III are improbable (phase II is just natural selection). For instance, phase I, genetic drift acting to decrease the fitness of small populations, is unlikely because chances are greater that the small population will go extinct rather than survive to move on to the phase II. And we have already seen their critique of the transition from phase I to phase II. In addition, Coyne, Barton, and Turelli argue that phase III, interdemic selection, is unlikely to occur in nature, because it relies on the subpopulations having an unlikely low level of gene flow between them. Empirically, Coyne and his colleagues think there is little support for the claim that phases I and

III occur in nature. And they think there is very little empirical support for all three phases working together, in other words, for the SBT. Ultimately, Coyne, Barton, and Turelli are convinced the SBT is too complex and delicate to occur often in nature (Coyne, Barton, & Turelli, 1997, pp. 664–665). Their view is "there are few empirical observations explained better by Wright's three-phase mechanism than by simple mass selection" so that "it seems unreasonable to consider the shifting balance process as an important explanation for evolution of adaptations" (Coyne, Barton, & Turelli, 1997, p. 643). With that recognition, Coyne and his colleagues opt, by appeal to parsimony, for Fisher's natural selection theory (Coyne, Barton, & Turelli, 2000, p. 314). For Coyne, Barton, and Turelli, if cumulative evolution can be explained adequately via a theory that postulates a small economy of entities and processes, then there is no need to invoke a theory with a larger economy of entities and processes.

Wade and Goodnight (1998) question Coyne, Barton, and Turelli's parsimony reasoning, and rightly so in my view (Skipper, 2002; see also Goodnight & Wade, 2000). Consider what Wade and Goodnight (1998) say about Coyne and his colleagues' appeal to parsimony, as a way of rejecting Wright's SBT:

> Coyne et al. . . . echoing the early group selection literature, advocated Occam's razor . . . as grounds for dismissing the SBT. They argued . . . that "there are few empirical observations explained better by Wright's three-phase mechanism than by simple mass selection" and that "it seems unreasonable to consider the shifting balance process as an important explanation for evolution of adaptations." (Wade & Goodnight, 1998, p. 1537)

But how does parsimony figure into generality of scope of applicability of a theory? But how does parsimony figure into generality in Coyne, Barton, and Turelli's argument?

According to Levins (1968, p. 7), it is not possible to maximize at the same time the generality, realism, and precision of a model (or theory, as a cluster of models) during model building in population biology. For instance, generality might be sacrificed for realism and precision. In such an instance, a model builder might construct a model that includes as many of the real features of the system being modeled in a way that precisely captures the system's dynamics. A fruitful way of reading Wade and Goodnight (1998) it seems to me is by reading them as arguing that Coyne and his colleagues are sacrificing realism for generality and, allegedly, precision. One can make such a sacrifice by simplifying the model, that is, by constructing a model that captures the apparently essential aspects of the system under scrutiny while removing apparently distracting aspects or those introducing only small changes to modeling results, and by introducing patently false assumptions facilitating study (Levins, 1968, pp. 6–7).

In my view, Fisher's natural selection theory exemplifies a fairly extreme form of the above approach to model construction. Consider the following as an illustration of the point. A crucial (mathematical) assumption that Fisher makes in constructing his theory is that populations are infinitely large and panmictic (randomly mating). Fisher had argued earlier that natural populations are rarely smaller than 10,000 individuals (and that gene flow was sufficient to treat populations as if they are effectively infinite) (Fisher, 1922). The evolutionary consequences of the assumption that populations are large is important for assigning evolutionary importance to genetic drift, migration, epistasis, and other factors. In other words, by assuming populations are infinitely large, a model builder is able to treat genetic drift, migration, epistasis, and

so forth, as elements introducing only small changes to the modeling results and, so, is able to treat such factors as unimportant in modeling evolution. Take genetic drift, for instance. In evolutionary terms, genetic drift is efficacious in populations that are smallish (and certainly not infinitely large and panmictic). If a population is too small, drift will take it to extinction. But if the population is very large (e.g., infinite), the effects of drift are negligible. So, assuming populations are infinitely large allows a modeler to discount the evolutionary importance of random genetic drift. Fisher used the assumption that populations are infinitely large to great effect in his book, *The Genetical Theory of Natural Selection*, allowing him to set aside such things as effects of drift and migration on the evolution of populations and assign considerably little importance to any evolutionary consequences of epistasis (Fisher, 1930b).

As I understand Coyne, Barton, and Turelli's critique of the SBT and advocacy of Fisher's theory of natural selection, they are claiming the simplifying assumptions endemic to Fisher's theory enable them to explain, in a way that preserves precision of modeling results (i.e., so the results stay well within standards of error). Yet, for Wade and Goodnight, the paring down of the models based on such assumptions is a matter of debate. Consider the following comment from Wade and Goodnight:

> It is common place to reify additive effects and treat them as properties of genes, independent of genetic and ecological context. Perhaps the fault lies not so much with Fisher's LST [natural selection theory] as with the uncritical application of it to evolutionary problems it was not meant to solve, such as speciation, or to ecological and genetic contexts in which it does not hold, such as evolution in metapopulations. For the reasons discussed above [previously in Wade and Goodnight 1998], accepting the LST [natural selection theory] over the SBT on the grounds of parsimony . . . does not seem warranted to us. (Wade & Goodnight, 1998, p. 1549)

On Wade and Goodnight's view, considerable evidence shows it is not always the case that results are precise when the infinitely large population size assumption is made in a model. Thus, reasoning based on parsimony is problematic.

Ultimately, Wade and Goodnight agree with Coyne, Barton, and Turelli that Wright's claim (1978b) is mistaken; the evidence and modeling cannot support that the SBT describes the principal processes of cumulative evolution. However, they think appealing to parsimony to reject the SBT altogether is a mistake. There are serious challenges for both the SBT and Fisher's natural selection theory. And the task at hand is to determine how, when, and where to apply the theories of Fisher and Wright to evolutionary problems (Goodnight & Wade, 2000, pp. 317, 322; Wade & Goodnight, 1998, pp. 1537, 1548). In any event, it seems clear from the preceding analysis that the theoretical issues that Fisher and Wright debated in the 1930s continue to attract the attention of researchers.

Controversy 3: *Panaxia dominula*

The debates between Fisher and Wright during the late 1920s and 1930s were largely theoretical. However, in 1947, Fisher published, with the ecological geneticist E. B. Ford, an experimental paper aimed at discrediting Wright's SBT and substantiating Fisher's natural selection theory (Fisher & Ford, 1947). Fisher and Ford's paper describes and analyzes data from what was at the time a fairly novel field experimental

technique, the capture and release protocol, used in populations of the moth *Panaxia dominula*. Fisher and Ford argued via their experimental results that even in small(ish) populations (between 1,000 and 10,000)—Wright's assumed norm—genetic drift, Wright's most important evolutionary factor according to Fisher and Ford—was evolutionarily inefficacious. Fisher and Ford argue further that natural selection, *even in smallish populations*, is the driving factor of evolution.

The capture and release protocol that Fisher and Ford describe in their 1947 paper was carried out between the years 1939 and 1946. Moths, mainly in Cothill, Oxfordshire, England, were captured (and later recaptured), marked inconspicuously with paint (or not so marked if recaptured), scored for phenotype of interest (or as a recapture), and released if unharmed. The purpose of Fisher and Ford's capture and release protocol was to collect data over time for fluctuations in frequency of genes of interest by scoring particular phenotypes, here wing coloration patterns. The Scarlet Tiger moth is easily identified by its wing coloration. The forewing is black with iridescent green structural coloring and a pattern of green or white spots. The hind wing is usually bright scarlet with black markings. In Fisher and Ford's *Panaxia* study, form *dominula*, f. *medionigra*, and the very rare f. *bimacula* refer to particular patterns of wing coloration that were assumed to correspond to dominant homozygous (AA), heterozygous (Aa), and recessive homozygous (aa) genotypes, respectively (color plates can be found in Fisher & Ford, 1947). Breeding studies had been done by E. A. Cockayne (1928), Ford (1940), and H. B. D. Kettlewell (1942), which provided the genotype-phenotype correspondence.

Fisher and Ford argued their data showed the fluctuations in the frequencies of the f. *medionigra* genes were too large from year to year to be due to genetic drift. Essentially, their specific argument was that even though population size was sufficiently small for genetic drift to be effective, drift nevertheless was not a factor (because the gene frequency changes were too high to be due merely to chance). For the years that the population was studied, the average population size was in the range of 3,200–4,000 moths, with approximately 11% overall being f. *medionigra*, and a total gene frequency change of approximately 6% (Fisher & Ford, 1947, pp. 150, 164). Fisher and Ford ultimately inferred, by elimination, that because changes in gene frequencies in the moth populations were not due to random genetic drift, they must be due to natural selection.

In 1948, Wright published a critique of Fisher and Ford's study (Wright, 1948). Wright objected on several grounds. First, Fisher and Ford had misinterpreted the role Wright had assumed for random genetic drift. They attributed to him more of a role than he himself had attributed. Second, their inference that selection must be the cause of the changes in gene frequencies in the populations of the moths was not justified experimentally. Fisher and Ford provided no direct evidence that selection is the cause; they only infer it after rejecting drift. Wright's paper drew an acerbic attack from Fisher and Ford published in 1950 (Fisher & Ford, 1950). Wright (1951) again responded. The substance of the disagreement, after Wright (1948), is the problem of interpreting Wright's view of the role of genetic drift in evolution. These exchanges have all been thoroughly discussed by Provine (1986). What I am about to discuss has not been discussed (at least not the work since 1962; but see Provine's 1986 brief discussion of Ford's summary of the work in the 1964 and 1975 editions of his *Ecological Genetics*, Ford, 1964, 1975).

The experimental work on *Panaxia* has continued in the spirit of Fisher and Ford's pioneering study. In fact, the work on *Panaxia* comprises one of the longest-running ecological genetics field experiments, lasting around 60 years, in the history of scientific study, with the most complete scientific review having been completed by L. M. Cook and David A. Jones for the years 1939–1995 (Cook & Jones, 1996). Basically, the ongoing *Panaxia* work has been, by and large, taken as 60 years of replication of Fisher and Ford's 1947 findings. (Certainly Cook and Jones agree with this assessment.) Interestingly, the only *direct* observational evidence of selection (e.g., observation of a long-term environmental perturbation such as predation, see, for example, Endler, 1986, in which chapter 3 discusses methods of detection) acting to change gene frequencies of the f. *medionigra* genes in the *Panaxia* work is due to P. M. Sheppard working with Cook and with Ford (Ford & Sheppard, 1969; Sheppard, 1951; Sheppard & Cook, 1962). The selectionist interpretation of the *Panaxia* work has largely been based on the same eliminative inference (i.e., the inference from observed gene frequency fluctuations independent of a perturbation) that Fisher and Ford made in 1947 (in spite of Wright's 1948 criticism of it).

Between 1993 and 1997, however, Cyril A. Clarke, David Goulson, and Denis F. Owen published a series of experimental and review papers criticizing the broad-ranging *Panaxia* work (Goulson & Owen, 1997; Owen & Clarke, 1993; Owen & Goulson, 1994). Ironically, the biologists started out with the intention of replicating Fisher and Ford's (1947) results. However, each of the biologists argue ultimately there are good reasons to reject the selectionist interpretation of the data: Selection is not the cause of the changes in gene frequencies; neither is genetic drift the cause.

Owen and Clarke in 1993 reported and analyzed data from a combined reared and wild capture and release protocol with *Panaxia* in 1991–1992 in Cothill and two other Oxfordshire fens (specifically, Dry Sandford, North Hinksey). During their protocol, they noted f. *medionigra* was extremely variable. Owen and Clarke were suspicious of this variability, prompting them to change their scoring technique. Rather than scoring the moths simply for the three forms, following Fisher and Ford (1947), they complicated their technique, scoring the phenotype by more specific patternings of wing coloration (Owen & Clarke, 1993, pp. 396–397). On the standard model, moths with a small or absent spot combined with either a yellow, black, or absent hindwing spot are scored as f. *medionigra*. Owen and Clarke scored only moths with a small or absent forewing spot in combination with a black hindwing spot as f. *medionigra*; the others were scored as f. *medionigra*-like. Using the revised scoring model, the frequency of the apparent f. *medionigra* gene frequencies that Owen and Clarke reported were, they claim, some of the largest on record (Owen & Clarke, 1993, p. 398). Depending on which phenotypes are scored as f. *medionigra*, with the low frequency scoring only f. *medionigra* and the high frequencies all-inclusive, the frequency estimates were 0.4%–49% for their 1991 reared population, 0.4%–2.7% for their 1992 wild population, and a remarkable, so they claim, 2.7%–41.9% for their 1992 reared population. This last is four times as high as any population since 1939.

Owen and Clarke found the variability in their data, tied directly to the variability in the form of the moth, impossible to reconcile without looking more closely at the causes of the variability. Although they could not definitively posit an explanation, they suggested the extreme variability of wing coloration phenotypes was due to an environmental factor. Temperature fluctuations, which would cause an overall

darkening in the wing color pattern, would make f. *dominula* look more like f. *medionigra* and f. *medionigra* look more like the rare f. *bimacula*. Owen and Clarke believed such an environmental effect would account for the exaggerated representation of f. *medionigra* on the standard scoring model. Owen and Clarke raised the issue that Fisher and Ford's original study, as well as the subsequent studies, may be compromised due to probable scoring errors related to temperature effects on the expression of the wing coloration phenotypes in the moths. That is, the gene frequency data may be corrupted, because some phenotypes may have been scored as the wrong genotype. Such scoring errors, argued Owen and Clarke, would skew the results and their analysis significantly. Owen and Clarke also noted something interesting: Kettlewell had done temperature-controlled breeding experiments in the lab in 1943–1944 (Kettlewell, 1943–1944). He had shown temperature fluctuations caused differences in expression of the wing coloration phenotype. Now, Kettlewell's experiments were done before Fisher and Ford's 1947 paper. Yet, Fisher and Ford, at least according to the published record, did not mention Kettlewell's findings. (The paper of Kettlewell's that Fisher and Ford do cite *is not* his temperature effects paper.)

In 1994, Owen and Goulson published a paper demonstrating temperature fluctuations cause changes in the expression of wing coloration during pupal development of the moth (Owen & Goulson, 1994). Genes that under mild or normal temperatures express to look like f. *dominula* will under more extreme temperatures express to look like f. *medionigra*. To show this, Owen and Goulson had done lab-breeding experiments in controlled-temperature environments on *Panaxia* (larvae) they had brought in from Cothill and North Hinksey. The wings of moths raised in temperatures below 12°C and above 24°C darken. They then argued there is no reliable way of tracking (in nature) changes in gene frequencies in *Panaxia* by observing and scoring phenotypes: temperature fluctuations significantly affect the phenotype, belying the genotype. Ultimately, Owen and Goulson concluded the *Panaxia* work is not good support for the prevailing selectionist interpretation of the *Panaxia* data.

There were no temperature data records in Fisher and Ford's studies; similarly for the subsequent ones. So, there is really no way to know whether temperature fluctuations really played any role in the variability of wing coloration in the moths during Fisher and Ford's or subsequent studies. Cook and Jones (1996), supporters of Fisher and Ford, claim in their review of the *Panaxia* work that there is nothing to worry about: They statistically analyzed weather data, independently recorded, and concluded temperature fluctuations likely did not affect expression of the wing coloration trait (Cook & Jones, 1996, p. 1625). So, the long-standing support for natural selection by the *Panaxia* work, according to Cook and Jones, goes undiminished by Owen and Goulson's 1994 temperature work.

By 1997, Goulson and Owen had done additional temperature experiments and had re-scored museum specimens of *Panaxia* originally scored by Fisher, Ford, their workers, and subsequent workers (Goulson & Owen, 1997; *cf.* Clarke, Clarke, & Owen, 1991). Goulson and Owen found errors in scoring *had* occurred in the *Panaxia* capture and release studies according to the Owen-Clarke revised scoring technique. Cook and Jones, in 1996, never comment on this possibility. Yet, it had been argued by Owen and Clarke (1993) that such errors were likely. Further, Goulson and Owen's newer temperature experiments further substantiate their claim the expression of wing

coloration phenotypes in *Panaxia* is all but controlled by fluctuations in temperature. Goulson and Owen's ultimate assessment of the *Panaxia* work and its contribution to the Fisher-Wright controversy is this (Goulson & Owen, 1997, pp. 616–617): Due to the severe problems with the *Panaxia* experimental methodologies, the results of the work must be rejected as a way of settling Fisher's and Wright's disagreement over the role of drift in evolution. As I see matters, Wright's (1948) original critique of Fisher and Ford's (1947) argument that the elimination of drift is sufficient to establish selection, ignored from the moment Wright raised it, is substantiated.

Jones, in 2000, published a paper that weakens Goulson and Owen's (1997) conclusion (Jones, 2000). What Goulson and Owen lacked was a capture and release study of gene frequency fluctuations in the moth that included the relevant experimental check for temperature fluctuations. Jones (2000) published that study. Jones captured and released moths in Cothill, Oxfordshire, during the years 1995–1999. Jones followed Owen and Clarke's (1993) scoring model. And, importantly, Jones monitored temperatures in precisely the locations in the Cothill Fen that the larvae of the moths pupate. For an average year-to-year population size in the range of 3,100–5,000 moths, Jones recorded gene frequency fluctuations in f. *medionigra* in the range 0.73% to 2.62%. Jones further found air temperature reached lows and highs exceeding the boundaries at which Owen and Goulson (1994) reported darkening of the wings in the laboratory. Temperatures in the Cothill Fen litters ranged from 4°C to 33°C (Jones, 2000, p. 580). However, the larvae and pupae never experienced those extremes for a prolonged period of time during any month between 1995 and 1999. The monthly mean was well within the 12°C to 24°C range for normal wing development (Jones, 2000, p. 584).

Jones's (2000) gene frequency fluctuation data is consistent with the data collected since 1939. However, Jones never explicitly attributes the fluctuations he recorded between 1995 and 1999 to natural selection. Rather, Jones's purpose was to cast doubt on Owen and Goulson's (1994) criticism of the historical *Panaxia* work. My view is that Jones has done just that. But he has not managed to remove all doubt cast upon the selectionist interpretation of the historical work. Again, Wright's (1948) original criticism of Fisher and Ford's (1947) argument for selection by elimination of drift stands. The Panaxia work is not a strong case to adjudicate between the relative roles of selection and drift in evolution.

Jones's (2000) paper is, so far as I am aware, the most recent in the ongoing *Panaxia* work. It is hard to say whether his assessment of the work will conclude the debates. Approximately 60 years have been devoted to the *Panaxia* work in efforts to resolve Fisher and Wright's original disagreement over the role of random genetic drift in evolution. The first ten years of work were hotly debated by Fisher, Wright, and their immediate associates (e.g., Ford, Sheppard). About 40 years hence have been taken up with the assumption that the continued work has successfully replicated Fisher and Ford's demonstration of the primacy of selection in (even smallish) populations of *Panaxia* in England. It has only been in the last 15 years that work has been done to confute the selectionist interpretation. And that work did not start with that intention; Owen and Clarke (in particular) thought they were just going to replicate the long-standing selectionist results (Owen & Clarke, 1993, p. 393).

Where Are We?

Provine (1985, pp. 197, 217) called the Fisher-Wright controversy "central, fundamental, and very influential" to modern evolutionary biology. The three debates Provine chose to demonstrate his point were precisely that. But, moreover, revisiting them shows they are also persistent. I think the persistence of the Fisher-Wright controversy is, at least for the most part, unsurprising. Fisher and Wright were, with Haldane, responsible for what was at the time a new evolutionary theory, one that, as Fisher put it, made for "a clearing of the ground of the debris of anti-Darwinism" (Fisher, [1932] 1983; *cf.* Provine, 1985, p. 215; 1989). The conflicts between Fisher and Wright concerned the very foundations of Darwinian evolution. And, it seems to me, in the mere seven decades since the publication of their major works on evolutionary theory, it is unsurprising to see those foundations continue to spark debate. But beyond the progress and problems of the preceding analyses of the Fisher-Wright controversy in the two decades since Provine's important paper, what, philosophically, can be said about it?

The three debates considered in detail in this essay show, among other things, there is ample room in the domain of evolutionary genetics for both Fisher's and Wright's evolutionary views. Differently put, each of the debates considered here is what Beatty (1995) calls a relative significance controversy. Such controversies reflect a theoretical pluralism, or the notion that the domain under scrutiny is fundamentally heterogeneous, so there can be no expectation that a single theory will explain the entire domain. Thus, in such a controversy, the aim is to establish the extent of applicability of some theory or set of theories as opposed to determining the one theory that explains the entire domain. That is, the aim is to determine the significance of a theory relative to another (or others) in the domain.

The continuing debates over the evolution of dominance, Fisher and Wright's general evolutionary theories, and the Scarlet Tiger moth are examples of relative significance controversies. In the late 1920s and early 1930s, Fisher and Wright disagreed about the biology of dominance. Fisher thought it was purely evolutionary whereas Wright thought it was primarily physiological. Today, biologists are coming to the conclusion that Wright's physiological theory provides the foundations for the correct view, but that dominance can evolve (an idea Wright suggested). During the 1930s, Fisher and Wright disagreed about whether natural selection or the shifting-balance process explained cumulative evolution. And while there is continued disagreement, many in evolutionary biology agree there is room in the evolutionary domain for both Fisher's and Wright's theories, among others. Finally, in the late 1940s and early 1950s, Fisher (with Ford) and Wright debated the significance of genetic drift in the evolution of the Scarlet Tiger moth. Continued work on the ecological genetics of the moth suggests quite forcefully that developmental mechanisms must be considered as an explanation of the changes in gene frequencies of the moth's *medionigra* form.

Perhaps the most vocal biologists supporting the view that the Fisher-Wright controversy is a relative significance controversy are Wade and Goodnight (1998; Goodnight & Wade, 2000). In their response to Coyne, Barton, and Turelli's criticism of Wright's Shifting Balance Theory, Wade and Goodnight made it plain the aim in assessing Wright's and Fisher's theories is not to determine which is the correct theory, but rather to determine when, where, and how the two, and others, apply, given some evolutionary problem at hand (Goodnight & Wade, 2000, pp. 317, 322; Wade

& Goodnight, 1998, pp. 1537, 1548). And in spite of Coyne and his colleagues' assertions to the contrary, Wright held a view similar to Wade and Goodnight's. A decade after Wright claimed in 1978 the Shifting Balance Theory describes the principal process by which cumulative evolution proceeds, the claim Coyne, Barton, and Turelli attack, Wright said, of his own, Fisher's, Haldane's, as well as Motoo Kimura's neutral theory, "all four are valid" (Wright, 1988, p. 122).

Biologically, recognizing that the Fisher-Wright controversy is a relative significance controversy may help to focus the goals of evolutionary research from searching for the correct theory to determining the extent of applicability of a theory, as I have argued elsewhere (Skipper, 2002). Historically and philosophically, recognition of the fact that the Fisher-Wright controversy is a relative significance controversy sheds some light on what a so-called "resolution" of the controversy might look like. The resolution of the controversy will likely not come in the form of determining which biologist got evolution right and which got it wrong. Rather, resolution will come in the form of understanding just how much each got correct.

Conclusion

This chapter revisited the Fisher-Wright controversy in the spirit of Provine (1985), paying special attention to the direction the controversy has taken in the more than 20 years since Provine wrote it. Provine concluded his discussion of the Fisher-Wright controversy saying, "the controversy between Fisher and Wright was in my opinion so central to modern evolutionary biology that it has become invisible to young people in the field" (Provine, 1985, p. 217). I take it Provine means the issues Fisher and Wright debated are so fundamental to population genetics (and evolutionary biology more broadly) that they belong to the field itself rather than just two of its founders. I think, however, the debates led by Coyne and Wade in the late 1990s made the controversy between Fisher and Wright visible again. The conflict between Coyne and Wade generated similar intensity to that between Fisher and Wright. But perhaps that is, as I suggested, unsurprising given the youth of modern evolutionary genetics.

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