
Specious Speciation: The Myth of Observed Large-Scale Evolutionary Change

A Response to TalkOrigins’ “Observed Instances of Speciation” FAQ

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by Casey Luskin

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PART I: EXECUTIVE SUMMARY

The TalkOrigins Speciation FAQ, titled “Observed Instances of Speciation”¹ (herein “FAQ”), claims it “discusses several instances where speciation has been observed.” For years, this FAQ has been cited by pro-Darwin internet debaters as allegedly demonstrating that neo-Darwinian evolution is capable of producing significant biological change. However, an analysis of the technical literature regarding many of the examples discussed in the FAQ² reveals that such claims are clearly incorrect. This assessment finds:

- **NOT ONE of the examples demonstrates the origin of large-scale biological change.**
- **The vast majority of the examples do NOT even show the production of new species, where a “species” is defined according to the standard definition of a “reproductively isolated population.”** Only one single example shows the production of a new species of plants via hybridization and polyploidy, but this example does not entail significant biological change.
- Only one of the examples purports to document the production of a reproductively isolated population of animals—*however this example is overturned by a later study not mentioned in the FAQ.*

- **Thus, not a single bona fide example of speciation in animals—e.g., the establishment of a completely reproductively isolated population—is given in the FAQ.**

I should note from the outset that my purpose is not to deny that speciation can occur in nature, especially when speciation is defined merely as a reproductively isolated population. When trying to assess the creative power of the Darwinian mechanism, that definition is trivial. Rather, my purpose is to test the FAQ’s claims. In that regard, if the FAQ is correct that “Many researchers feel that there are already ample reports [of speciation] in the literature,” then an analysis of the literature cited in the FAQ suggests those researchers are wrong.

While most of the FAQ’s discussions of the papers it cites are reasonably accurate, these papers amount to citation bluffs if one is claiming to “discus[s] several instances where speciation has been observed.” People who believe this FAQ demonstrates that Darwinian processes can produce large-scale biological change have been badly misled. The examples in the FAQ are ultimately used to make inaccurate claims, and the FAQ’s title, “Observed Instances of Speciation,” is unwarranted.

PART II: “SPECIATION?” IT’S ALL IN THE DEFINITION.

The TalkOrigins “Observed Instances of Speciation” FAQ, claims to discuss “several instances where speciation has been observed.” The most important question is whether these examples show significant biological change has occurred. But this analysis shows:

- **(1) As a primary finding, none of the examples demonstrate that Darwinian evolution is capable of causing large-scale evolutionary change.**
- **(2) As a secondary finding, the vast majority of the examples do not even meet the standard definition of “speciation.”**

To understand why both (1) and (2) are the case, we first need to understand the implications of how evolutionary biologists typically define “species.”

Evolutionary biologists typically define “species” as a reproductively isolated population of individuals. For example, the FAQ quotes the great neo-Darwinian evolutionary biologist Ernst Mayr defining a species as “groups of actually or potentially interbreeding natural populations which are reproductively isolated from other such groups.” This classical definition is called the biological species concept. Under this standard definition, speciation entails the origin of such a reproductively isolated population. But does it entail anything else?

Not necessarily. Such definitions say nothing about the degree of morphological, behavioral, or genetic change that has evolved. Thus, *such a definition of “species” does not necessarily imply that significant biological change has taken place between the two populations.* In many cases, two populations may be termed different “species” under the biological species concept, but yet the differences between the populations are small-scale and trivial. Indeed:

- One of the papers cited by the FAQ (Dodd, 1989) clearly states that speciation is reduced to mere reproductive isolation, “According to the biological species concept, speciation is basically a problem of reproductive isolation.”³ Another paper (Schluter and Nagel, 1995) cited by the FAQ notes that under this definition species are “defined by the criterion of reproductive isolation rather than morphological criteria.”⁴
- Yet even the notable evolutionary biologist Theodosius Dobzhansky (1972) admits that under this view, “speciation may occur without rearrangement of the genetic materials in the chromosomes” and “Reproductive isolation evidently can arise with little or no morphological differentiation.”⁵
- Putting these quotes, claims, and definitions together, papers cited by the FAQ admit that under the biological species concept, “speciation” does not require any morphological change.

As noted, the FAQ is often cited to explicitly or implicitly claim that Darwinian evolution is capable of producing significant biological change. But the FAQ’s definition of “speciation” seems contrived when used to demonstrate the grander claims of Darwinian evolution that fundamentally new biological structures, body plans, and higher taxa can evolve. Even if we do find reproductively isolated populations that document “speciation,” that might provide virtually no evidence that Darwinian processes can produce new complex biological features or large-scale change. **Indeed, the primary finding of this analysis is that *the examples in the FAQ do not report the kind of change which shows Darwinian processes can produce fundamentally new types of organisms, new complex biological structures, or higher taxa.***

What is more, the vast majority of the examples in the FAQ don’t even document “speciation” under the biological species concept. One paper cited by the FAQ (Rice and Hostert, 1993) notes that “Once pre- and/or postzygotic isolation is complete, speciation has occurred.”⁶ But in the vast majority of the instances cited by the FAQ, pre- or postzygotic isolation was not complete, and thus speciation did not occur. **Thus, a secondary finding of this report is that only one single paper analyzed in the entire FAQ actually reported complete reproductive isolation, and thus “speciation” under the biological species concept.**

This raises an irony: the title of the FAQ is “Observed Instances of Speciation,” yet the vast majority of the examples analyzed show that complete reproductive isolation was not achieved. Thus, the FAQ is overselling the evidence, not just for significant morphological change, but also for true speciation (e.g., complete reproductive isolation). If these are some of the best examples for “speciation” that evolutionists can muster, then the evidence for Darwinian evolution must be meager indeed.

Before discussing this analysis of the FAQ, I must again reiterate that my purpose has never been to deny that speciation can occur in nature, especially when speciation is defined by the trivial definition of a mere reproductively isolated population. Rather, my purpose is to test the FAQ’s claims.

PART III: ANALYZING THE FAQ

Section 5.0 of the FAQ purports to provide “examples of observations of speciation,” making this the section that needs to be studied to confirm if the evidence backs the FAQ’s claims.

Many of the references cited by the FAQ are old and were difficult to obtain. I downloaded whatever papers I could find online from my local university library and analyzed those examples.

A. Summary of Findings

The following table summarizes the findings of this review for the examples analyzed:

FAQ Section:	Summary:
5.1.1.3, “Tragopogon”	Two plant species could hybridize into populations showing small-scale changes compared to the “parent species”—the greatest of which is color changes of the kind well known within plants. Since hybrids are “extremely sterile,” it does not seem that speciation has occurred.
5.1.1.5 “Hemp Nettle (Galeopsis tetrahit)”	Two highly similar species of flowering plants within the same genus were crossed in the laboratory to produce a polyploid plant virtually identical to a known species in nature. This confirms the long-known fact that plants can hybridize to form new polyploid forms. But speciation by polyploidy does not produce new morphological characteristics, and the tetraploid daughter species showed only small-scale changes—the greatest of which is color changes of the kind well known within plants—from the parent species. Speciation by hybridization and polyploidy is not a viable mechanism for the vast majority of evolution because: (1) it occurs only within flowering plants, (2) it does not produce new morphological characteristics, and (3) polyploid hybrids cannot arise without pre-existing parent species, meaning it entails a collapse—not gain—of existing diversity.
5.1.1.8, “Maidenhair Fern (Adiantum pedatum)”	An abnormal sporophyte ultimately led to tetraploid, rather than normal diploid sporophytes in a species of fern. No morphological change was reported and tetraploid sporophytes were “less vigorous” than normal. At best this evidence only “provided the opportunity to characterize the first step of one possible route to polyploid formation” as speciation was merely “incipient.” Complete speciation was not observed and large-scale morphological change was not observed.
5.1.2, “Animals”	While hybrids can occasionally occur between very closely related animal species, generally speaking animal hybrids are “rare organisms” because hybridization is not a viable

	<p>mechanism for animal diversification. Animal hybrids are typically unisexual, where part of the genome is not heritable. For example, in female fish hybrid clones, the parent species is always required to provide the male portion of the genome, meaning a truly new independent species is not formed. Darwinian evolution requires heredity, but this does not entail the origin of anything new that is heritable. Asexual animal hybrids are thus often called “evolutionary dead ends” where they do not produce new diversity and instead “[e]xtant asexual ‘species’ are little more than scattered twigs at the tips of major phylogenetic branches.” Moreover, hybrid asexual females are so similar to the parent species that “sexual males of the progenitor species are unable to distinguish hybrid females from those of their own species,” implying little morphological change arises in this process. This mechanism is largely “irrelevant” to sexually reproducing animals.</p>
<p>5.2.2, “Maize (<i>Zea mays</i>)”</p>	<p>A new breed of corn was produced through artificial hybridization of two “varieties” of corn within the same species. There was only “almost complete reproductive isolation” but a new species was not claimed to have emerged. The partial reproductive isolation results from a pre-mating mechanism—changes in flowering timing—not large-scale change which can produce fundamentally new types of organisms. The hybrid was produced via controlled artificial selection; whether this could be achieved in the wild is not established.</p>
<p>5.2.3, “Speciation as a Result of Selection for Tolerance to a Toxin: Yellow Monkey Flower (<i>Mimulus guttatus</i>)”</p>	<p>Two populations within the same species of a flowering plant “developed partial postmating isolation between some races” where “total postzygotic reproductive isolation between two populations, in the sense that inviable zygotes are formed, can be produced by a comparatively simple genetic mechanism.” The cause of the reproductive isolation is thought have “a simple genetic basis” entailing changes in “a single gene.” Large-scale differences have not evolved and speciation is only claimed to have been “initiated,” not complete.</p>
<p>5.3.1, “<i>Drosophila paulistorum</i>”</p>	<p>This study showed that if you start with “semispecies” within a fruit fly species which are “indistinguishable morphologically,” and then subject the strains to artificial breeding experiments, “in none has anything like complete isolation been achieved.” Moreover, there is no suggestion that the populations were no longer “indistinguishable morphologically” after the experiments. At best, only a “new race or incipient species” was created. Some authorities have challenged even the partial isolation, claiming the results “may have been due to contamination of cultures by other subspecies.”</p>
<p>5.3.2, “Disruptive Selection on <i>Drosophila</i>”</p>	<p>Artificial selection for the number of chaetae (hairs) on a population of fruit flies produced partial, though not complete,</p>

melanogaster”	reproductive isolation. The extent of morphological variation is small-scale changes in the number of chaetae. Later attempts to reproduce these results were unsuccessful. The authors explicitly state that natural speciation has not been demonstrated. This experiment does not show complete reproductive isolation, speciation, or significant morphological change.
5.3.3, “Selection on Courtship Behavior in <i>Drosophila melanogaster</i>”	This experiment sought to induce changes in the mating preferences of two strains of fruit flies. Only “partial” reproductive isolation was achieved, and the extent of change observed was small changes in courtship initiation behaviors (e.g., licking and vibrations). The two strains were “similar” before the experiments, and apart from slight changes in mating behaviors, remained very similar after the experiments.
5.3.4, “Sexual Isolation as a Byproduct of Adaptation to Environmental Conditions in <i>Drosophila melanogaster</i>”	This fruit fly study found partial reproductive isolation after selection experiments on fruit flies. No significant morphological change was reported, and any reproductive isolation which did exist stemmed from premating factors. This paper thus serves as a good example of how speciation need not entail significant morphological or genetic change.
5.3.5, “Sympatric Speciation in <i>Drosophila melanogaster</i>”	After two populations of fruit flies were selected for various food-finding behaviors, incomplete reproductive isolation was observed. The populations could still produce “fertile offspring” and speciation was only claimed to be “incipient. No significant morphological change arose.
5.3.6, “Isolation Produced as an Incidental Effect of Selection on several <i>Drosophila</i> species”	Three fruit fly studies were reported: they showed “slight” or “incipient” or “not complete” sexual reproductive isolation, but none showed complete reproductive isolation or speciation. None showed significant morphological change.
5.3.7, “Selection for Reinforcement in <i>Drosophila melanogaster</i>”	Again, fruit fly experiments found only “partial” reproductive isolation and did not report significant biological change. One paper boasted that “[t]he evidence here presented shows ... that natural selection can act to strengthen isolation.” But since the ‘destroy the hybrid’ experiments simulated processes that would never occur in nature—the artificial destruction of all hybrid flies for no biological reason other than experimental curiosity—it obviously confused natural selection with artificial selection.
5.3.8, “Tests of the Founder-flush Speciation Hypothesis Using <i>Drosophila</i>”	Three papers testing the founder-flush model of speciation using fruit flies failed to produce complete reproductive isolation. Reproductive isolation was called “partial” and / or “weak,” and no significant morphological change was reported.
5.4.1, “A Test of the Founder-flush Hypothesis Using Houseflies”	Experiments testing the founder-flush model of speciation using houseflies found only “marginal significance for positive assortative mating.” Biological changes observed amounted to loss of certain courtship behaviors which would probably not be advantageous in the wild, and is not evidence that Darwinian evolution can produce significant biological change.

<p>5.4.2, “Selection for Geotaxis with and without Gene Flow”</p>	<p>Mating experiments between races of houseflies produced only “incipient” reproductive isolation. The only biological change detected was the insignificant behavioral question of whether the fly chose to fly upward or downward in a tube. Reproductive isolation was not complete and speciation was not claimed to have occurred. Significant biological change also was not observed.</p>
<p>5.5.1, “Apple Maggot Fly (<i>Rhagoletis pomonella</i>)”</p>	<p>The FAQ suggests a new species evolved when parasitic flies on hawthorn trees invaded a new type of tree (apples). The two populations form viable hybrids in the lab and thus postzygotic isolation is not apparent. Moreover, the studies leave open the live possibility that the flies “represent a single panmictic population,” where both groups interbreed in nature. The populations of flies are called “races” that are only “partially reproductively isolated”; speciation is not established. While some change in allele frequencies is observed, significant morphological change is not claimed to have occurred. The FAQ calls this case “very exciting” but the technical literature it cites is more measured and objective, calling this example “controversial.”</p>
<p>5.5.2, “Gall Former Fly (<i>Eurosta solidaginis</i>)”</p>	<p>Populations of the gall former fly live on different species of host plants, leading some to wonder whether they have formed different species. The evidence shows the flies are mere “races” which only have “partial reproductive isolation,” and thus are not members of separate species since “both the genetic data ... and the behavioral data presented here suggest that there is gene flow between populations.” The most significant differences amount to “a preference for mating on the host plant and different emergence times,” which correspond to the host plant life cycle. Complete reproductive isolation is not established, and only small-scale biological is observed.</p>
<p>5.6, Flour Beetles (<i>Tribolium castaneum</i>)</p>	<p>Experiments which selected for high, and low weights within flour beetles managed to increase the mean weight in various lines by about a milligram. This is not significant biological change. Some assortative mating was found but reproductive isolation was not complete.</p>
<p>5.7, “Speciation in a Lab Rat Worm, <i>Nereis acuminata</i>”</p>	<p>Initially the investigators thought they had discovered a completely reproductively isolated population of polychaete worms that had been subjected to phases of bottlenecks and population growth in the lab. However, a later study found that these conclusions were wrong, since “the Lab population was already a species different from P1 and P2 at the time when it was originally sampled in 1964.” Thus, what happened was the investigators sampled a naturally occurring independent species of polychaete worms and mistakenly concluded that a new species had formed in the lab. The original paper which</p>

	originally reported this example stated: “the entire process of speciation has rarely been observed.” This paper did not remedy that problem.
5.9.2, “Morphological Changes in Bacteria”	The FAQ claimed bacteria “underwent major morphological change” but the technical paper it cites does not claim the change was “major.” The change entailed a growth in bacterial cell size—from about 1.5 μm in length to up to 20 μm—which allowed larger bacteria to escape predation. However, the change also involved a fitness cost, where the bigger bacteria faced “a selective disadvantage” when competing with smaller cells in a predator-free environment. Fitness costs in bacteria often limit the ability of new forms to persist, or evolve further. The investigators never claim that a new species of bacteria has evolved. This probably represents the most significant example of morphological change reported in the FAQ, but it was in bacteria which are known to vary widely in response to selective pressures, and the change involved a significant fitness cost. After this study was published, the British bacteriologist Alan Linton stated: “Throughout 150 years of the science of bacteriology, there is no evidence that one species of bacteria has changed into another.” ⁷ This study does not claim to contradict Linton’s conclusion.

B. Full Responses to Selected Sections of the FAQ

Response to Section 5.1, “Speciations Involving Polyploidy, Hybridization or Hybridization Followed by Polyploidization”

Response to Section 5.1.1.3, “Tragopogon”

Summary: Two plant species could hybridize into populations showing small-scale changes compared to the “parent species”—the greatest of which is color changes of the kind well known within plants. Since hybrids are “extremely sterile,” it does not seem that speciation has occurred.

The FAQ states that “Owenby (1950) demonstrated that two species in this genus were produced by polyploidization from hybrids.” Again, the notion that plants can hybridize is nothing new. And it is noteworthy that in this case, we’re hybridizing two species that are already within the same genus—in other words, they were already thought to be highly similar and closely related.

But do hybrids show the production of a “new species”? During plant hybridization, the genomes of two plants merge. Even under an evolutionary paradigm, such hybridization would likely be interpreted as two species which recently diverged coming together to form a hybrid. If anything, it would seem to entail collapse, loss, and decrease of diversity rather than the generation of new diversity.

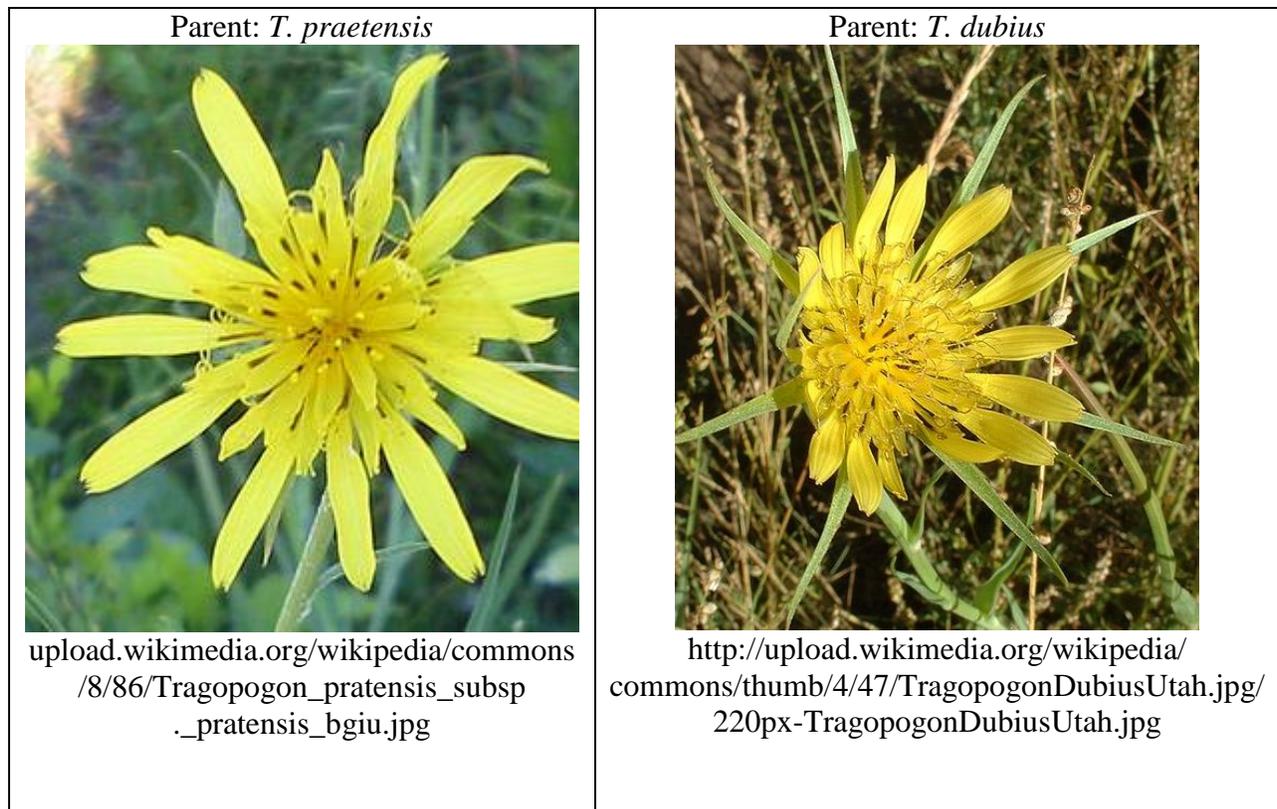
Indeed, Owenby (1950) indicates that new traits are not necessarily being generated. Thus, Owenby finds that the hybrids simply contained a mix of the dominant traits from the two parent species:

“They combine certain dominant characteristics derived from the parents involved, and on this basis form three additional classes. In most features, they are not intermediate, but display a re-combination of the characteristics which mark their parents.”⁸

Such examples of hybridization don’t necessarily show that something “new” has been created, but rather seem to show that pre-existing traits are perpetuated.

Indeed, in this case, Owenby (1950) also observed that “All three hybrid combinations are extremely sterile,”⁹ leading to questions about their viability. The sterility of the hybrids implies that functionality is being lost in this process of hybridization leading to questions about whether this is a viable mechanism for speciation.

The FAQ boasts that “[e]vidence from chloroplast DNA suggests that *T. mirus* has originated independently by hybridization in eastern Washington and western Idaho at least three times (Soltis and Soltis 1989).” But this mundane point was also obvious from Owenby (1950)’s paper, which stated “Wherever any two of the three introduced diploid species grow together, natural hybrids can be expected.”¹⁰ The ease with which these species hybridize implies they are already closely related, and what we’re seeing is loss of pre-existing diversity. To understand just how similar these species are, consider the pictures below:



Hybrid: *T. mirus*



http://calphotos.berkeley.edu/cgi/img_query?enlarge=6249+3022+3908+0030

Given that it is widely known that cross-breeding among plants can cause small-scale changes, like changes in petal color, size, and shape, this study reports only small-scale variation that is no greater than that which is regularly achieved by breeders.

It is noteworthy that when Tate *et al.*'s chapter on "Polyploidy in Plants" states that "Polyploid species of independent origin may also differ morphologically," their centerpiece example is that populations of *T. mirus* can "differ in floral coloration."¹¹ If the floral coloration is one of the most significant observed morphological changes resulting from polyploidy then this certainly is not an established mechanism of macroevolution.

Response to Section 5.1.1.5 "Hemp Nettle (*Galeopsis tetrahit*)"

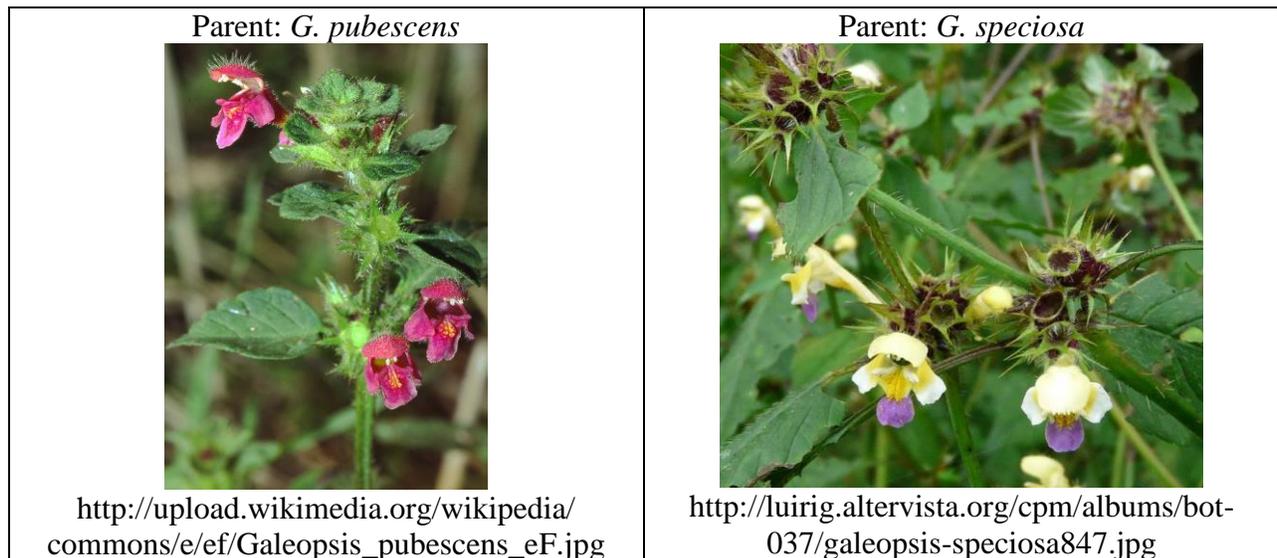
Summary: Two highly similar species of flowering plants within the same genus were crossed in the laboratory to produce a polyploid plant virtually identical to a known species in nature. This confirms the long-known fact that plants can hybridize to form new polyploid forms. But speciation by polyploidy does not produce new morphological characteristics, and the tetraploid daughter species showed only small-scale changes—the greatest of which is color changes of the kind well known within plants—from the parent species. Speciation by hybridization and polyploidy is not a viable mechanism for the vast majority of evolution because: (1) it occurs only within flowering plants, (2) it does not produce new morphological characteristics, and (3) polyploid hybrids cannot arise without pre-existing parent species, meaning it entails a collapse—not gain—of existing diversity.

In this example, the FAQ discusses an experiment by Müntzing (1932) where two species within the same genus of flowering plants in the mint family (*Galeopsis pubescens* and *Galeopsis speciosa*) were crossed to produce a new polyploid species, *Galeopsis tetrahit*.¹² The notion that flowering plants can be crossed to produce polyploid hybrid species is nothing new. It's long

been known that polyploidy occurs commonly in flowering plants. But duplicating a chromosome doesn't necessarily produce new genetic information, and polyploid plants generally have small-scale differences from their haploid counterparts. As Jonathan Wells observes regarding this example:

“There actually are some confirmed cases of observed speciation in plants—all of them due to an increase in the number of chromosomes, or ‘polyploidy.’ In the first decades of the twentieth century, Swedish scientist Arne Muntzing used two plant species to make a hybrid that underwent chromosome doubling to produce hempnettle, a member of the mint family that had already been found in nature. Polyploidy can also be physically or chemically induced without hybridization. Observed cases of speciation by polyploidy, however, are limited to flowering plants. **According to evolutionary biologist Douglas J. Futuyma, polyploidy ‘does not confer major new morphological characteristics . . . [and] does not cause the evolution of new genera’ or higher levels in the biological hierarchy.**”¹³

This example thus shows the crossing of two highly similar species without the production of new morphological characteristics. These similarities can be seen in the photographs, below, where both the parent species, and the daughter species, have similar leaf shapes and snap-dragon-like flower shapes. Many of the same points that are made about the case of *T. mirus* can be made here:



Hybrid: *G. tetrahit*



http://upload.wikimedia.org/wikipedia/commons/thumb/a/a0/Galeopsis_tetrahit_bloemen2.jpg/450px-Galeopsis_tetrahit_bloemen2.jpg

Speciation by hybridization and polyploidy implies flowering plants may be designed to evolve by forming hybrids, and doesn't necessarily show unguided or unplanned evolution. Jonathan Wells explains why this mechanism—which entails the joining of two lines—cannot explain much diversity:

“Darwinism depends on the splitting of one species into two, which then diverge and split and diverge and split, over and over again. Only this could produce the branching-tree pattern required by Darwinian evolution, in which all species are modified descendants of a common ancestor.”¹⁴

Speciation by hybridization and polyploidy thus cannot be a viable mechanism for the vast majority of evolution because:

- (1) it occurs only within flowering plants,
- (2) it does not produce new morphological characteristics,
- (3) polyploid hybrids cannot arise without pre-existing parent species, meaning it entails a collapse—not gain—of pre-existing diversity.

Since this species cannot arise without pre-existing parent plant species, obviously this mechanism cannot be responsible for all plant species. As another paper cited by the FAQ (Dobzhansky and Pavlovsky, 1971) states: “Though widespread and important in some plant families, species formation by allopolyploidy is uncommon in the living world at large.”¹⁵

Response to Section 5.1.1.8, “Maidenhair Fern (*Adiantum pedatum*)”

Summary: An abnormal sporophyte ultimately led to tetraploid, rather than normal diploid sporophytes in a species of fern. No morphological change was reported and tetraploid sporophytes were “less vigorous” than normal. At best this evidence only “provided the opportunity to characterize the first step of one possible route to polyploid formation” as speciation was merely “incipient.” Complete speciation was not observed and large-scale morphological change was not observed.

In this case, the FAQ cited a paper (Rabe and Haufler, 1992) that found a naturally occurring fern sporophyte which produced diploid spores rather than normal haploid spores. The FAQ notes that these diploid spores “germinated normally and grew into diploid gametophytes,” which later produced a new generation of tetraploid sporophytes. Since gametophytes are normally haploid rather than diploid, and sporophytes are normally diploid rather than tetraploid, this case was abnormal.

Despite that abnormality, the paper cited by the FAQ, reports that unreduced (2N) spores are not wholly uncommon within ferns, as “Normal diploids may also produce unreduced spores.”¹⁶

The question is: are there any interesting evolutionary implications from this mutant form? The FAQ doesn’t identify any in fact the paper reports that when they tried to grow these tetraploid sporophytes that they were “less vigorous”:

“The few tetraploid individuals of *Adiantum pedatum* produced in the lab appeared to be less vigorous than diploids growing under the same conditions. More importantly, in the mutant *A. pedatum* discussed here it is likely that any individuals achieving reproductive maturity would be unable to follow a normal series of meiotic divisions given the synaptic mutation that they would inherit.”¹⁷

Reproductive problems might also result from the fact that Rabe and Haufler (1992) observed that “Although these gametophytes produced archegonia, no antheridia were observed”¹⁸ (antheridia are the plant structure in gametophytes that produce male gametes, or sperm).

In the end, the paper itself probably reports the biggest (and not very incredible) implication of this study: “The discovery of diploid sporophytes producing unreduced spores provided the opportunity to characterize the first step of one possible route to polyploid formation.”¹⁹ As the paper’s title suggests, any speciation was merely “incipient” and complete speciation was not observed

So they have found one potential route in just the first step of generating polyploid forms of plants in nature. But complete production of a new species did not occur. And in this specific case, we haven’t found that the mutant tetraploid sporophyte offspring are significantly different from the normal form—if anything they are less viable. This example shows very little of use to those seeking the evolution of novel advantageous traits through natural processes.

Response to Section 5.1.2, “Animals”

Summary: Animal hybrids are “rare organisms” because hybridization is not a viable mechanism for animal diversification. Animal hybrids are typically unisexual, where part of the genome is not heritable. For example, in female fish hybrid clones, the parent species is always required to provide the male portion of the genome, meaning a truly new independent species is not formed. Darwinian evolution requires heredity, but this does not entail the origin of anything new that is heritable. Asexual animal hybrids are thus often called “evolutionary dead ends” where they do not produce new diversity and instead “[e]xtant asexual ‘species’ are little more than scattered twigs at the tips of major phylogenetic branches.” Moreover, hybrid asexual females are so similar to the parent species that “sexual males of the progenitor species are unable to distinguish hybrid females from those of their own species,” implying little morphological change arises in this process. This mechanism is largely “irrelevant” to sexually reproducing animals.

The FAQ suggests animals may also speciate through hybridization, though one primary paper cited by the FAQ (Vrijenhoek 1994) admits that animal hybrids are “rare organisms.”²⁰ Gregory and Mable (2005) concur, observing that “recent polyploidy is far less common in animals than in plants.”²¹ Indeed, there are good reasons why animal hybrids are much rarer than plant hybrids: the problems encountered with animal hybridization appear far more severe than those raised by plant hybridization.

Darwinian evolution can only operate when there is variation, selection, and heredity. Vrijenhoek (1994) discusses hybridogenesis within fish but finds that the hybrid genome is not entirely heritable since, “Paternal B genes are expressed in the hybrids but are not heritable. Only the A’ ‘hemiclinal’ genome is transmitted between generations.” This is why hybrid fish like *Poecilia formosa* are often called “clones,” because the heritable portion of the genome is simply cloned from generation to generation, and males from the parent “species” are always required to maintain the line. Thus, this “species” cannot be maintained without the constant presence of the parent line, meaning the hybrids are not a truly independent species. Since hybridization in vertebrates typically involves asexual clonal reproduction, this poses a problem for those who would cite hybridization as a mechanism of animal evolution.²² As the Monterey Bay Aquarium Research Institute website explains regarding hybrid fish:

“In hybridogenesis, the female mates with a male, forming a female offspring with both the maternal and paternal genomes. When that female offspring produces eggs however, the male genome is discarded.”²³

Again, in other words, these hybrids cannot persist without both species present. These hybrids always require the parent species to be present in order for them to originate and persist. The review by Vrijenhoek (1994) explains that such animal hybrids are typically “evolutionary dead ends”:

“Asexual species are often considered evolutionary dead ends because of their presumed genetic inflexibility. Among vertebrates and insects, only 0.1% to 0.2% of species are strictly asexual. This rarity suggests a ‘mutation/selection-like’ balance. New asexual lineages arise infrequently and go extinct rapidly. **Extant asexual ‘species’ are little more than scattered twigs at the tips of major phylogenetic branches.** Except for bdelloid rotifers, asexual lineages have not speciated and diversified into rich asexual clades.”²⁴

If anything, animal hybridization would seem to entail collapse, loss, and decrease of pre-existing diversity rather than the generation of new diversity. Indeed, in this case these asexual animal hybrids are not genetically viable in the long term:

“Genetic decay provides a final challenge to the persistence of clones. Muller suggested that mutations would accumulate like a ‘ratchet mechanism’ in asexual lineages. Recombination in sexual lineages produces offspring with higher and lower mutational loads than the parents and purifying selection effectively maintains a low load. An asexual population cannot reduce its load below that of the ‘least loaded’ clone. If by chance that clone is lost, the load has increased one step. Excluding back mutations, it cannot be reduced.”²⁵

It’s not clear that this can be a viable long-term mechanism for evolution in vertebrates because this new “species” could never exist on its own in the wild without the parent species constantly available to continually generate it. In the long term, animal hybridization appears to be a byproduct of existing species, not a mechanism for producing new species.

Little more is said in the FAQ regarding hybridization in animals, but it is clear that hybrids result from crossing highly similar species within the same genus, and nothing new is created that is heritable. In fact, regarding *Poecilia formosa*, Gregory and Mable (2005) observe that “sexual males of the progenitor species are unable to distinguish hybrid females from those of their own species,”²⁶ implying that little morphological change has occurred. This is not a viable mechanism for evolution in animals, as Dobzhansky recognizes: “Sudden emergence of new species by allopolyploidy is ... irrelevant to *Drosophila* and most bisexual animals.”²⁷

Response to Section 5.2 “Speciations in Plant Species not Involving Hybridization or Polyploidy”

Response to Section 5.2.2, “Maize (*Zea mays*)”

Summary: Corn breeders produced “almost complete reproductive isolation” between two “races” or “varieties” within the same species but a new species was not claimed to have emerged. The partial reproductive isolation results from a premating mechanism—changes in flowering timing—not a large-scale change which might produce fundamentally new types of organisms. The isolation was produced via strong artificial selection; whether this could occur in the wild is not established.

According to the paper cited by the FAQ (Paterniani, 1969), humans have bred the corn species *Zea mays* into many races over the past 4000 years, and “all races cross readily one with the other giving progenies with normal fertility.”²⁸ Through selective breeding that destroyed hybrid crosses, this study sought to achieve reproductive isolation between two species of corn. However, even at the end of the experiment some intercrossing between the varieties still occurred. As Jonathan Wells writes, “Paterniani noted ‘an almost complete reproductive isolation between two maize populations’ but did not claim that a new species had been produced.”²⁹

So what exactly generated the reproductive isolation? It turns out that by destroying hybrids, the experimenters were selecting for individuals that flowered at different times and did not produce hybrids. Reproductive isolation was probably achieved by little more than slight changes (a few days) in the timing of flowering in the two varieties:

“Data on days to flowering show that some change occurred, thus indicating that this mechanism is playing a role in the isolation obtained. Both original populations flowered in the same days with identical averages for days-to-tassel and days-to-ear flowering. Cycle IV of the two populations, already showing a great degree of reproductive isolation, have a marked difference in days to flowering. The white flint maize became about 5 days earlier, for tassel and ear, than the corresponding original population. The yellow sweet maize changed less, but in the opposite way; it became, on the average, 2 days later. As a result, the two cycle-IV populations have about 1 week of difference in flowering time. This difference is of sufficient magnitude to explain most of the reproductive isolation obtained.”³⁰

The paper concludes: “The data show that the number of days from planting to flowering was probably the main factor.”³¹ Thus, what the paper shows is that the plants are flowering at different times and thus don’t have the opportunity to pollinate one-another.³² Other than that, there’s no indication of biological changes. This does not amount to the kind of large-scale change which can produce fundamentally new types of organisms. And it was done by controlled artificial selection; whether this could be achieved in the wild is not established.

Response to Section 5.2.3, “Speciation as a Result of Selection for Tolerance to a Toxin: Yellow Monkey Flower (*Mimulus guttatus*)”

Summary: Two populations within the same species of a flowering plant “developed partial postmating isolation between some races” where “total postzygotic reproductive isolation between two populations, in the sense that inviable zygotes are formed, can be produced by a comparatively simple genetic mechanism.” The cause of the reproductive isolation is thought have “a simple genetic basis” entailing changes in “a single gene.” Large-scale differences have not evolved and speciation is only claimed to have been “initiated,” not complete.

According to the paper cited by the FAQ (Macnair and Christie, 1983), the yellow monkey flower, *Mimulus guttatus*, has “developed partial postmating isolation between some races.”³³ While offspring can be produced between the two populations, they are inviable, as the paper

later reports “total postzygotic reproductive isolation between two populations, in the sense that inviable zygotes are formed, can be produced by a comparatively simple genetic mechanism.”³⁴ The investigators do not fully understand the genetic cause of reproductive isolation but believe it may be linked to pleiotropic effects caused by the gene responsible for copper tolerance/intolerance. They suggest that the isolation has “a simple genetic basis” and is caused by changes to “a single gene.”

On the one hand this shows that reproductive isolation may be achieved. On the other hand the fact that offspring can be produced shows large-scale biological differences have not evolved. In fact, the paper suggests that speciation was only “initiated” and not complete.

In this case, we have seen two races within the same plant species are essentially identical apart from one being tolerant to copper and other intolerant. The two races are so similar that they can produce offspring, but those offspring are not viable. Speciation is not complete and significant biological change is not observed. The exact genetic mechanisms which are causing such reproductive isolation are unknown, but they might result from “a simple genetic basis” entailing changes in “a single gene.” This does not show significant biological change.

Response to Section 5.3, “The Fruit Fly Literature”

Response to Section 5.3.1, “*Drosophila paulistorum*”

Summary: This study showed that if you start with “semispecies” within a fruit fly species which are “indistinguishable morphologically,” and then subject the strains to artificial breeding experiments, “in none has anything like complete isolation been achieved.” Moreover, there is no suggestion that the populations were no longer “indistinguishable morphologically” after the experiments. At best, only a “new race or incipient species” was created. Some authorities have challenged even the partial isolation, claiming the results “may have been due to contamination of cultures by other subspecies.”

In this example, the FAQ discusses whether reproductive isolation has been achieved between various strains, or “semispecies,” of the fruit fly *Drosophila paulistorum*. The paper cited by the FAQ (Dobzhansky and Pavlovsky, 1971) states that in the wild, “The semispecies are indistinguishable morphologically,”³⁵ and “not different enough” to be considered “fully differentiated species.” They observed that after a certain amount of time breeding in captivity, crosses between two particular strains only produced sterile males. (Female hybrid crosses were apparently still fertile.) The investigators claim that contamination was “ruled out” but Jonathan Wells notes that those claims may be incorrect, for “Coyne and Orr wrote in 2004, however, that Dobzhansky and Pavlovsky’s result ‘may have been due to contamination of cultures by other subspecies.’”³⁶

After conducting artificial breeding experiments where hybrid crosses between the strains were destroyed, the authors produced some assortative mating. (This process does not mimic natural conditions.) The authors admit that the proportion of hybrids merely “decreased” and “in none has anything like complete isolation been achieved.”³⁷ Jonathan Wells thus notes that in this example: “Dobzhansky and Pavlovsky reported only a ‘new race or incipient species,’ not a new

species.”³⁸ Another paper in the FAQ (Halliburton and Gall, 1981) lists this study among various studies where “none has succeeded in establishing complete sexual isolation.”³⁹ In any case, speciation is not claimed to have occurred, and there is no suggestion whatsoever that the semispecies are no longer “indistinguishable morphologically.” In fact, after reviewing this example, Dobzhansky concludes that sometimes “reproductive isolation and speciation precede differential adaptedness,”⁴⁰ suggesting the populations had not diverged.

Response to Section 5.3.2, “Disruptive Selection on *Drosophila melanogaster*”

Summary: Artificial selection for the number of chaetae (hairs) on a population of fruit flies produced partial, though not complete, reproductive isolation. The extent of morphological variation is small-scale changes in the number of chaetae. Later attempts to reproduce these results were unsuccessful. The authors explicitly state that natural speciation has not been demonstrated. This experiment does not show complete reproductive isolation, speciation, or significant morphological change.

In this example, the paper cited by the FAQ (Thoday and Gibson, 1962) reports experiments with a small population of wild fruit flies (*Drosophila melanogaster*), which over a series of generations selected those with both the highest and lowest numbers of chaetae, or hairs. Through artificial selection over the course of successive generations, they were able to select for flies with more, or less chaetae than in the original population. This is the extent of the variation bred by this experiment.

Even after multiple generations, the flies with high or low numbers of chaetae could still hybridize. However, hybrids did become less common as the experiment progressed, although the reason for this was not known. The experimenters proposed that perhaps it “arises from mating preferences or from an inability of hybrid flies to compete as larvae.”⁴¹ The ability for the high and low chaetae populations to hybridize, although rare, was not impossible. Reproductive isolation was thus not complete. Additionally, since *artificial* (rather than natural) selection was used to create and maintain the two populations, the authors warn:

“We do not, however, wish it to be thought that we regard this as a demonstration that sympatric speciation occurs in Nature, for such a conclusion cannot be drawn from the results of laboratory selection experiments.”⁴²

At best, this experiment shows there is pre-existing variation among fruit flies for numbers of chaetae, and artificial selection for this trait in a non-natural laboratory setting can result in partial reproductive isolation. But changes in the numbers of hairs was all that was produced: it does not show anything close to large-scale evolutionary change.

Thoday and Gibson (1962) open their paper by admitting that “the key demonstration that a single wildtype population can be converted by selection into two populations that are mutually isolated in the conditions in which they have to maintain themselves has not hitherto been made.”⁴³ But Jonathan Wells observes, “Not only did Thoday and Gibson not claim to have produced a new species, but also other laboratories were unable to replicate their results.”⁴⁴ The

FAQ likewise admits, “In the decade or so following this, eighteen labs attempted unsuccessfully to reproduce these results.” The summary of Halliburton and Gall (1981) is striking:

“Several attempts to repeat these results have failed (e.g., Scharloo *et al.*, 1967a; Chabora, 1968; Barker and Cummins, 1969). Similar experiments, selecting for other quantitative characters or using other organisms, have usually failed to produce assortative mating (Scharloo, 1964; Robertson, 1966; Scharloo *et al.*, 1967b; Grant and Mettler, 1969; Bos and Scharloo, 1973), but a few have succeeded (Coyne and Grant, 1972; Soans *et al.*, 1974).”⁴⁵

As far as this example goes, it would seem this “key demonstration” of speciation has not been made.

Response to Section 5.3.3, “Selection on Courtship Behavior in *Drosophila melanogaster*”

Summary: This experiment sought to induce changes in the mating preferences of two strains of fruit flies. Only “partial” reproductive isolation was achieved, and the extent of change observed was small changes in courtship initiation behaviors (e.g., licking and vibrations). The two strains were “similar” before the experiments, and apart from slight changes in mating behaviors, remained very similar after the experiments.

This experiment took two pre-existing strains of fruit flies from within the same species—*Drosophila melanogaster*—and sought to determine whether changes in mating preferences could be induced. This included artificially killing hybrids between the strains (a process that does not necessarily mimic nature). Incomplete reproductive isolation was observed, which one paper cited by the FAQ (Knight *et al.* 1956) called only “[p]artial sexual isolation.”⁴⁶ Another paper in the FAQ (Halliburton and Gall, 1981) lists this study among various studies where “none has succeeded in establishing complete sexual isolation.”⁴⁷ The most biological change that this example documented was small-scale behavioral differences pertaining to courtship, specifically changes in the amount of “licking” that males do to females to initiate mating. One paper cited by the FAQ (Crossley, 1974) showed just how unimpressive the sort of change observed in this experiment was:

“Quantitative analysis of male and female behavior revealed the underlying causes of changed mating preferences and faster mating. In the LS experiment male courtship became more stimulating because percentage licking of both males and percentage licking plus vibration of males increased.”⁴⁸

Thus, all that was observed were changes in the courtship initiation behaviors (licking and vibrations) between the strains. This is small-scale change. The two strains were “similar” before the experiments, and apart from slight changes in mating behaviors, remained very similar after the experiments.

These experiments were conducted in the laboratory, but Crossley (1974) observed why laboratory experiments do not match natural conditions: “One difficulty in relating these results to selection against hybrids in nature is that in the laboratory, selection against hybrids was total

but in the wild some hybrids would survive to breed in spite of their disadvantages compared with pure bred offspring.”⁴⁹

Response to Section 5.3.4, “Sexual Isolation as a Byproduct of Adaptation to Environmental Conditions in *Drosophila melanogaster*”

Summary: This fruit fly study found partial reproductive isolation after selection experiments on fruit flies. No significant morphological change was reported, and any reproductive isolation which did exist stemmed from premating factors. This paper thus serves as a good example of how speciation need not entail significant morphological or genetic change.

In this experiment, the investigators changed temperature and humidity conditions for populations of fruit flies (*Drosophila melanogaster*) in the laboratory. Originally the populations came from the same genetic stock, but after about 5 years of artificially exposing them to different environmental conditions, the experiment found that some reproductive isolation was established. According to the paper cited by the FAQ (Kilias *et al.*, 1980), “The highest isolation index detected was 0.388 ± 0.108 ,”⁵⁰ meaning reproductive isolation was far from complete.

But was any significant morphological change found? None was reported, and in fact Kilias *et al.* (1980) said the reproductive isolation resulted simply from pre-mating factors:

“Since, females from either population mate equally, the reproductive isolation detected in the present investigation seems to be due to changes in behavior (different mating preferences or discrimination) of our populations. ... In the present study we failed to detect significant postmating isolation.”⁵¹

The change that was observed pertained to the sexual isolation and changes in ovipositional rhythms, and thus they observed that “speciation” (meaning mere reproductive isolation) “may occur with relatively little genetic change in structural genes.”⁵² This led them to the final conclusion that genetic divergence between populations often occurs after “speciation” (again, mere reproductive isolation) because such divergence was not observed here: “The genetic distance observed between species probably results from post-speciational divergence.”⁵³ This paper thus serves as a good example of how speciation need not entail significant morphological or genetic change.

Response to Section 5.3.5, “Sympatric Speciation in *Drosophila melanogaster*”

Summary: After two populations of fruit flies were selected for various food-finding behaviors, incomplete reproductive isolation was observed. The populations could still produce “fertile offspring” and speciation was only claimed to be “incipient. No significant morphological change arose.

This is another study where partial reproductive isolation was established between populations of *Drosophila melanogaster*, and the differences between the populations were minor, and of ambiguous importance.

The experimenters forced fruit fly pupae to navigate a maze to find food where they might choose to go towards the light/dark, up/down, or choose between two different scents. Flies which made opposite choices were then separated, allowed to breed, and then subsequent generations were selected according to which flies made the same choices. When the fly populations were then allowed to mix, partial reproductive isolation was achieved.

Complete reproductive isolation was not found. As one of the papers cited by the FAQ (Rice and Salt, 1988) reports:

“It might be argued that incipient speciation has not occurred in this experiment for two reasons. First, trace amounts of gene flow occurred between the population using habitats SE and 4L, since a small fraction of flies switched habitats between generations. ... Second, forced matings between the two populations produced fertile offspring in the F1 and F2, and thus reproductive isolation was mediated only by habitat-preference behavior. ... [I]rreversible reproductive isolation did not occur in this experiment.”⁵⁴

The fact that gene flow between the populations occurred, and that they could be forced to produce fertile offspring, shows how similar the strains remained. They claim that any speciation is merely “incipient.”

But what evolved? At most they might have selected for preferences for when seeking food. As they conclude: “In these experiments, the only barriers to gene flow were gaps that gradually developed in the distribution of spatiotemporal habitat preference.”⁵⁵ It’s important to realize that again, we have seen no significant morphological change. As Jonathan Wells observes, “Within thirty generations the flies had sorted themselves into two populations that did not interbreed, but Rice and Salt claimed only ‘incipient speciation that we believe to have occurred.’”⁵⁶

Response to Section 5.3.6, “Isolation Produced as an Incidental Effect of Selection on several *Drosophila* species”

Summary: Three fruit fly studies were reported: they showed “slight” or “incipient” or “not complete” sexual reproductive isolation, but none showed complete reproductive isolation or speciation. None showed significant morphological change.

In this example, the FAQ discusses a paper (del Solar, 1966) which reported experiments that artificially selected “positively and negatively geotactic” and “positively and negatively phototactic” strains of both *Drosophila melanogaster* and *Drosophila pseudoobscura*.⁵⁷ The paper reports that this produced what was called “slight” sexual isolation, or “incipient reproductive isolation,” due to “changes in sexual behavior.”⁵⁸ The paper thus reports that complete reproductive isolation was not found:

“Whether selection for geotaxis and phototaxis always and necessarily produces a change in the sexual behavior, and whether continued selection may carry the sexual divergence *anywhere near complete isolation*, can only be decided by further experiments.”⁵⁹

Not only was “anywhere near complete isolation” not achieved, but significant biological change was also not achieved. As the paper reports: “The geotactically and phototactically positive and negative strains appear to be *indistinguishable* in external morphology.”⁶⁰

Another example discussed by the FAQ in this section pertains to Dodd (1989) which reported experiments on populations of the fruit fly *Drosophila pseudoobscura*. Four populations were given a starch-based medium, and the other four were given a maltose-based medium. The paper reported that individuals raised on the starch medium preferred to mate with other starch-fed fruit flies; likewise flies fed maltose preferred to mate with other maltose-fed individuals. Interestingly, these traits arose independently in each of the four populations in each medium.

Because the experiments controlled for food source rather than mating behavior, they concluded that the sexual isolation was “a pleiotropic by-product of the adaption of the populations to the two media” but “The mechanism of the isolation in this system is as yet unknown.”⁶¹ Indeed, it was not that the two populations were incapable of interbreeding or never interbred, it was just that they did so less than would be expected under normal random mating. Another paper cited by the FAQ (Schluter and Nagel, 1995) described the findings by stating that only “some premating isolation evolved,” and “Reproductive isolation between divergent lines was not complete.”⁶² Speciation was also not said to have occurred.

Aside from mating and food preferences, there were no claims of biological change between the populations. Again, we see that not only has reproductive isolation not been demonstrated, but significant biological change did not evolve.

Despite the aforementioned underwhelming results, the FAQ then discusses another paper which it says reported “Less dramatic results.” According to the paper cited by the FAQ (de Oliveira and Cordeiro, 1980), different populations of *Drosophila willistoni* were given food at different pH levels. Like the other studies in this section, some individuals preferred to mate with other individuals fed food at the same pH. But the different populations were still capable of interbreeding. When the offspring were fed alkaline food, “their hybrids are not less fit.”⁶³ However, the paper reported that “on the acid substrate the hybrids are inferior to their parents which are adapted to this food.”⁶⁴ The paper thus only claimed to find “incipient isolation,”⁶⁵ not complete reproductive isolation.

As for the degree of morphological change, aside from a preference for a certain pH level in food, no significant biological change was reported. In fact, the paper notes that among three long-standing natural races of *D. willistoni*, “These flies are morphologically indistinguishable.” This study certainly did not change that observation: Once again we have not seen complete reproductive isolation, nor have we seen significant biological change.

Response to Section 5.3.7, “Selection for Reinforcement in *Drosophila melanogaster*”

Summary: Again, fruit fly experiments found only “partial” reproductive isolation and did not report significant biological change. One paper boasted that “[t]he evidence here presented shows ... that natural selection can act to strengthen isolation.” But since the ‘destroy the hybrid’ experiments simulated processes that would never occur in nature—

the artificial destruction of all hybrid flies for no biological reason other than experimental curiosity—it obviously confused natural selection with artificial selection.

In this section, the FAQ acknowledges that Rice and Hostert (1993) do not find evidence for the reinforcement model of speciation, where, according to the paper, “the physical barrier breaks down before complete reproductive isolation has evolved in allopatry” but yet “matings between previously separated subpopulations are presumed to produce low-fitness hybrid offspring, and this selects for positive assortative mating.”⁶⁶ If hybrids cannot survive and reproduce, then arguably the two populations are reproductively isolated.

The FAQ then discusses two older papers which purportedly support the reinforcement model. Keep in mind again that the most important question here is not whether two populations can fail to produce viable hybrids, but whether the two populations show some non-trivial degree of evolutionary change.

The first study cited by the FAQ (Ehrman, 1971) took two strains of *Drosophila melanogaster* and sought to test for sexual isolation. The paper never claims that significant morphological change evolved, but it does note that this experiment had results similar to Knight *et al.* 1956, a paper which only found “[p]artial sexual isolation,” and never claimed significant biological change arose. Likewise, Ehrman (1971) reports that after breeding experiments, in males there is only “some sexual isolation,” and the author thus hopes that “the degree of reproductive isolation evolved to be enhanced by the passage of time.”⁶⁷ The paper cites no significant changes in the fly populations after the experiments. The experiment did not find cross-mating is impossible, placing limits on the degree of change which arose. Not only was complete reproductive isolation not achieved, but there is no report whatsoever that significant biological change emerged. Another paper in the FAQ (Halliburton and Gall, 1981) lists this study among various studies where “none has succeeded in establishing complete sexual isolation.”⁶⁸

In a similar study, another paper cited by the FAQ (Koopman, 1950) mixed two similar species of *Drosophila*, *D. pseudoobscura* and *D. persimilis* in an attempt to induce reproductive isolation in the lab. Complete reproductive isolation was not established.

Normally one would think that if the two populations are already classified as members of different species then perhaps they are already completely reproductively isolated; but this is not the case here, as the two species are “closely related” and in fact *D. persimilis* was “formerly known as *D. pseudoobscura*, race B.”⁶⁹ Complete reproductive isolation between the two species does not exist in the lab: the two groups can form hybrids, and “Hybrids seem to have the same viability as the pure species,” although hybrid males are sterile and females, when backcrossed with parent species, tend to have eggs with “poor viability.”⁷⁰ While they form hybrids in the lab, however, “in nature, not a single hybrid has been found, even from localities where both species occur together.”⁷¹

To help investigate whether complete reproductive isolation could emerge, the experimenters used a tactic that would not be present in nature: they artificially killed any hybrids. As the paper stated:

“The experiments herein described were made in order to determine whether, in artificial populations consisting of the two closely related species ... an increase in the reproductive isolating mechanisms could be detected if in each generation the hybrids between the two species were systematically eliminated.”⁷²

The paper concluded that “[t]he evidence here presented shows ... that natural selection can act to strengthen isolation between species.”⁷³ But was it “natural selection” or artificial selection? The paper shows astonishment at how “in a surprisingly short time,” increased reproductive isolation was established. But they really should not be so surprised since they recognize that “This change, of course, was aided by the practice of removing the hybrids entirely each generation, in this way simulating complete hybrid inviability.”⁷⁴ Thus, natural selection was not at work; rather artificial selection caused these changes.

In any case, complete reproductive isolation did not arise as hybrids still formed, albeit at a “low level.” So the experiments started off with two similar populations of flies that have partial reproductive isolation, and they ended with two highly similar populations of flies that have “partial”⁷⁵ (though slightly more) reproductive isolation. The paper makes no report of significant morphological differences between the populations at the beginning of the experiment, and the end, so this experiment once again shows that (1) complete reproductive isolation was not achieved, and (2) significant biological change did not arise.

Response to Section 5.3.8, “Tests of the Founder-flush Speciation Hypothesis Using *Drosophila*”

Summary: Three papers testing the founder-flush model of speciation using fruit flies failed to produce complete reproductive isolation. Reproductive isolation was called “partial” and / or “weak,” and no significant morphological change was reported.

The first paper cited by the FAQ in this section (Powell, 1978) investigated a hypothetical “founder-flush” mechanism of speciation where a small number of individuals found a new population, which then goes through various cycles of expansion in population size (“flush”), followed by a “crash,” where “[a]t each crash the bottleneck population is small and genetic drift is strong.”⁷⁶ After the crash, another small group of individuals found the new population, and the cycle repeats. Powell (1978) attempted to simulate this process for strains of fruit flies within a population of *Drosophila pseudoobscura*.

The paper reported that “neither isolation nor inbreeding by themselves lead to reproductive isolation. Only among populations which were inbred (four founder events) and allowed to flush did reproductive isolation evolve.”⁷⁷ When reproductive isolation did evolve, it was called “partial”; at one point the author claimed only “some degree of reproductive isolation.”⁷⁸ He claimed to only have observed the “first stages of speciation,”⁷⁹ not complete speciation.

What is also significant is the type of reproductive isolation that evolved. Powell (1978) reports that “no post-mating factors were detected,”⁸⁰ indicating that when crossbreeding between the populations did occur in the experiment, it produced viable and fertile offspring. This implies that significant biological change between the populations did not arise over the course of the

experiments. The only type of reproductive isolation that was observed was “pre-mating (ethological) isolation,”⁸¹ where behavioral factors reduce cross-mating.

Finally, the paper observes the extreme degree of repeated flushes and crashes simulated in the experiment would require “rather special circumstances” not necessarily common in nature. Either way, once again neither complete reproductive isolation nor significant biological change was observed.

Another paper cited by the FAQ (Dodd and Powell, 1985) repeated this type of experiment and found very similar results to Powell (1978). Reproductive isolation was called “significant” but was far from complete. The overall finding was that: “in general, it appears that some weak ethological isolation exists.”⁸²

Also like Powell (1978), all reproductive isolation that did arise was due to behavioral (pre-mating) mechanisms and “no post-mating isolation could be detected.”⁸³ This implies that significant morphological or genetic change did not arise between the separated populations over the course of the experiment, because fertile and viable offspring could be produced. The paper thus notes that these results counter the common evolutionary presumption that speciation occurs because populations diverge biologically:

“Many scenarios for the formation of new species envision post-mating isolation factors to evolve before pre-mating isolation evolves (e.g., Dobzhansky, 1940). Pre-mating barriers are thought to be secondary, reinforcing mechanisms of isolation. Here they appear to be primary; that is, they have evolved in the apparent absence of post-mating isolation.”⁸⁴

Like the others, this experiment did not report that significant biological change evolved.

A final paper cited in this section of the FAQ (Ringo *et al.*, 1985) used populations of *Drosophila simulans* test the founder-flush model (which fosters genetic drift) against the classical model of speciation, where certain traits undergo selection, gradually leading to a new species. The lines experiencing selection were artificially selected for various “arbitrary traits.”⁸⁵

While some “partial reproductive isolation” in the various lines did arise, they found it was “much weaker than that typically found between sibling species of *Drosophila*.”⁸⁶ They thus hope that:

“More-complete reproductive isolation might be established by the same forces at work over a longer time span, perhaps reinforced by direct selection for pre-mating barriers to gene flow.”⁸⁷

They thus lament that “A large gap lies between the degree of isolation between any experimental populations and the degree of isolation observed between species.”⁸⁸

As far as pre-mating isolation goes, the paper reports that “Weak sexual isolation was observed between BASE and the drift lines, for the experiment as a whole.”⁸⁹ However, the paper did find

some postmating isolation. Interestingly, “reproductive isolation was stronger in drift lines than in selection lines” and “postmating isolation increased over time in drift lines but not in selection lines” and overall “there was only a 5% reduction in hybrid fitness.” This implies that even artificial selection for certain traits did not produce sufficient biological change to prevent viable and fertile hybrids from forming between the selected lines and the original base population. Significant biological change is not reported.

Even the FAQ admits regarding this study that “only weak isolation was found and that there was little difference between the effects of natural selection and the effects of genetic drift.” In other words, even when certain traits are selected for in the laboratory, only weak isolation arises viable and fertile offspring between crossed lines can still be produced. Once again, incomplete reproductive isolation (which the FAQ admits is “weak”) and only very limited small-scale change observed, even when there is artificial selection for many traits.

Finally, it’s worth noting that Ringo *et al.* (1985) and Powell (1978) “have been criticized (Charlesworth *et al.* 1982) because the base populations were derived from geographically diverse stocks, ‘so it is not clear whether their results are representative of what might happen in a natural population.’”⁹⁰ Other critics (Meffert and Bryant, 1991) observed that in Ringo *et al.*’s experiment, “Because the populations contributing to the base population exhibited differences in mating activity that were presumably genetic, the experimental protocol created an artificially high genetic variance for traits affecting mating behavior and exaggerated the divergence among experimental lines.”⁹¹

The response from the authors to such criticisms is that they wanted to study “the extent and the speed of establishment of reproductive isolation under optimal conditions; that is, we hoped to maximize reproductive isolation among lines by maximizing genetic variation”⁹² in the initial population. They did this because when more realistic natural conditions are modeled in experiments, the results might have “[n]egative.”⁹³ In other words, even when they gave speciation mechanisms their best shot—better than would likely exist in nature—complete reproductive isolation was not achieved and significant biological change was not observed.

In any case, Meffert and Bryant (1991) observe that due to weaknesses in Ringo *et al.* 1985:

“Hence the critical issue in the founder-flush theory of speciation has not been addressed: can bottlenecks in a natural population cause permanent alteration of courtship behavior in founder lines that would lead to premating isolation.”⁹⁴

Would Meffert and Bryant thus argue that the papers cited in this section of the FAQ don’t even establish what the FAQ claims they do?

Response to Section 5.4, “Housefly Speciation Experiments”

Response to Section 5.4.1, “A Test of the Founder-flush Hypothesis Using Houseflies”

Summary: Experiments testing the founder-flush model of speciation using houseflies found only “marginal significance for positive assortative mating.” Biological changes observed amounted to loss of certain courtship behaviors which would probably not be advantageous in the wild, and is not evidence that Darwinian evolution can produce significant biological change.

The paper cited in this section of the FAQ (Meffert and Bryant, 1991) established six lines of houseflies to purportedly test the founder-flush model. After modeling five founder-flush cycles, “[o]nly two cases of significant assortative mating were detected.”⁹⁵ One of those two cases entailed negative assortative mating where individuals prefer mates from other lines, which obviously would not preserve isolation between those two particular lines in nature. Positive assortative mating was observed between lines 1a and 4b, although they called this only “marginal significance for positive assortative mating.”⁹⁶

The most change they reported in this experiment was premating isolation, pertaining to changes in courtship behavior. However, this also included “loss of specific courtship behavior”⁹⁷ in various lines. It’s not clear that the observed changes would be advantageous in the wild. In particular, it has been suggested that the new forms would be less fit, as “Kaneshiro (1980, 1983) proposed that bottlenecks may cause the loss of specific courtship behaviors such that derived males *would be discriminated against* when competing with ancestral males for ancestral females.”⁹⁸ They find that “[t]his case would appear to support Kaneshiro's hypothesis such that the control females discriminated against 1a males which had a reduction in courtship element utilization.”⁹⁹ Such loss of courtship behaviors thus might not be preserved in nature. In any case, loss of function is not a compelling evolutionary mechanism for speciation; loss-of-function examples are not good evidence that Darwinian evolution can build complexity

In fact, it's not even clear that the ability of males to perform these courtship behaviors was lost. They observe that the observed loss might have been an artifact of more rapid mating: “The mechanism for this unidirectional pattern may have been that females of the bottleneck lines accepted males at an earlier point in courtship such that the last behaviors to be executed in courtship, KICK and WING OUT, were omitted.”¹⁰⁰

In any case, no postmating barriers were reported and no significant biological change was reported.

Finally, Meffert and Bryant warn that a major obstacle to the founder-flush model of speciation is that during the “founder” phase where the population goes through a bottleneck, “[i]f founder lines are to be successful in nature they must overcome the initial effects of inbreeding depression, otherwise their establishment as a viable population may be seriously hindered.”¹⁰¹ Interestingly, they suggest that the one observed example of positive assortative mating in their experiments may have resulted from such an inbreeding depression:

“Because slow mating in 1b and 4a was associated with some degree of inbreeding depression in egg-to-adult viability (Bryant *et al.*, 1990), the positive deviations along the first principal axis may represent a trend for coordinated inbreeding depression across suites of traits (egg-to-adult viability, general mating propensity, and complexity of courtship).”¹⁰²

So it’s not entirely clear that the one example of premating isolation observed in the experiment entailed advantageous biological change. In any case, the rarity of reproductive isolation observed in this experiment, and the low degree and potentially deleterious nature of biological change observed do not support the claim that evolutionary mechanisms can produce significant biological change.

Response to Section 5.4.2, “Selection for Geotaxis with and without Gene Flow”

Summary: Mating experiments between races of houseflies produced only “incipient” reproductive isolation. The only biological change detected was the insignificant behavioral question of whether the fly chose to fly upward or downward in a tube. Reproductive isolation was not complete and speciation was not claimed to have occurred. Significant biological change also was not observed.

This section of the FAQ cites a paper (Soans *et al.*, 1974) which tests a model of speciation which proceeds first by “formation of races in subpopulation,” and then second, by “the establishment of reproductive isolation.”¹⁰³ Reproductive isolation is said to arise when “selection began to operate against the hybrids.”¹⁰⁴ The experiment used strong artificial selection for flies which chose to fly either up or down in a vertical tube. Four races of houseflies (*Musca domestica*) were then established:

Race A: 50 flies that flew upward (i.e. pure selection of upward flies)

Race B: 50 flies that flew downward (i.e. pure selection of downward flies)

Race C: 35 flies that flew upward and 15 that flew downward (i.e. 30% gene flow; partial selection of upward flies)

Race D: 35 flies that flew downward and 15 that flew upward (i.e. 30% gene flow; partial selection of downward flies)

All four populations showed assortative mating where they preferred to mate with members of their own race. However, reproductive isolation was not complete and was called merely “incipient.”

Thus, the most that was observed was partial premating isolation. The only biological change that was observed was the insignificant behavioral question of whether the fly chose to fly upward or downward in a tube. But since viable and fertile offspring could still be produced, it’s not clear that this entailed any significant form of biological change. Thus, there is only selection “against the hybrids” due to partial premating isolation, not because the two populations had diverged to the point that interbreeding was impossible.

The authors acknowledge, however, that since only partial reproductive isolation was achieved, “results of our experiments are far from conclusive in demonstrating speciation via either sympatric or allopatric conditions.”¹⁰⁵ Once again, we see only partial reproductive isolation and insignificant biological change arising.

Another paper cited by the FAQ (Hurd and Eisenberg, 1975) performed a similar experiment except they allowed for 50% gene flow in races C and D. They found similar results but asked why selection for a geotactic response (e.g., upward or downward flying flies) would cause such reproductive isolation. They speculate that “It is more likely that by selecting for geotactic response, some other (e.g., behavioral) response which served to differentiate mating types was responsible for the degree of reproductive isolation observed here.”¹⁰⁶ This makes it clear that the exact behavioral mechanism which caused reproductive isolation in these experiments is unknown, making it difficult to claim that these studies have established conclusively that significant behavioral change evolved.

Response to Section 5.5, “Speciation Through Host Race Differentiation”

The FAQ states that “differentiated host races may represent incipient species,” but as we will see in the following two examples, complete reproductive isolation is not observed and low levels of biological change have arisen.

Response to Section 5.5.1, “Apple Maggot Fly (*Rhagoletis pomonella*)”

Summary: The FAQ suggests a new species evolved when parasitic flies on hawthorn trees invaded a new type of tree (apples). The two populations form viable hybrids in the lab and thus postzygotic isolation is not apparent. Moreover, the studies leave open the live possibility that the flies “represent a single panmictic population,” where both groups interbreed in nature. The populations of flies are called “races” that are only “partially reproductively isolated”; speciation is not established. While some change in allele frequencies is observed, significant morphological change is not claimed to have occurred. The FAQ calls this case “very exciting” but the technical literature it cites is more measured and objective, calling this example “controversial.”

In what the FAQ calls “a very exciting case,” it discusses claims that the apple maggot fly (*Rhagoletis pomonella*) has invaded new trees, and “may represent the early stages of a sympatric speciation event.” Clearly then, the FAQ admits that it does not show a full speciation event.

Previously, the fly was only known to invade hawthorn trees, but it is now known to invade other trees—all of them also within the family Rosaceae—including apples, cherries, roses, and pears. Questions have arisen as to whether flies that live on apple trees are forming a new species compared to those that live on hawthorn trees. If some populations prefer one tree over another, then reproductive isolation could occur. But the evidence in this case is far from clear.

One paper cited by the FAQ (McPherson *et al.*, 1988) notes that “speciation by the formation of host races (parasite populations associated with different plant or animal hosts) has been the

subject of great controversy”¹⁰⁷ because “it has been difficult to demonstrate the existence of host races, much less prove that host races are evolving toward species status.”¹⁰⁸ Likewise, another paper cited by the FAQ (Prokopy *et al.*, 1988) repeatedly calls claims that incipient reproductive isolation is arising “controversial,” since previous studies have been inconclusive:

“Two previous studies comparing behavioral responses of female *R. pomonella* assayed in groups hinted at small differences in the pattern of host fruit acceptance between hawthorn and apple origin flies ... On the other hand, Prokopy *et al.* (1985) found no differences in pattern of acceptance of a variety of fruit types among populations of *R. pomonella* originating from apple in Nova Scotia, Massachusetts, Michigan, and Oregon.”¹⁰⁹

Prokopy *et al.* (1988) reported experimental results that were unexpected if sympatric divergence is occurring. Female apple maggot flies preferred to lay eggs in the fruit of hawthorn trees over apple trees regardless of whether they originated on hawthorn or apple trees, leading the paper to conclude that “[t]he results of this experiment again strongly indicated that hawthorn is a significantly more acceptable fruit than apple.”¹¹⁰ Likewise, “Males of both larval origins remained on hawthorn fruit significantly longer than on apples.”¹¹¹ In both cases, apple-born females preferred apples more than hawthorn-born females, and apple-born males preferred apples more than hawthorn-born males. Similarly, “Survival from egg to pupal stages was significantly higher for flies of both host origins in hawthorn than apple fruit.”¹¹²

This example therefore does not show that somehow apple maggot flies have evolved an affinity for apple trees over hawthorn trees. If both populations can accept and even prefer hawthorn trees, this raises the question of whether there has been any significant evolution. Clearly complete reproductive isolation does not exist in nature. They speculate that there is only “some degree of restriction in gene flow” between the two types, but they note that it’s hard to explain why this exists: “Of particular interest to us is an explanation of how gene flow is restricted in face of the ability of both apple and hawthorn origin flies to accept hawthorn to an equal degree.”¹¹³ Neither reproductive isolation nor significant biological change has been established by this paper.

In fact, another study cited by the FAQ (Smith, 1988) notes that “direct genetic evidence of biologically meaningful differentiation among putative host races has been lacking.”¹¹⁴ This paper found that there may be genetic differences pertaining to timing that wild flies emerge, which may be “fine-tuned to coincide closely with fruit maturation.”¹¹⁵ However, this paper notes that such differences “do not even signify the existence of a reproductive barrier among populations” and the fly populations on various types of trees “could still represent a single panmictic population,”¹¹⁶ where all individuals can interbreed. It also acknowledges that “the specific genetic nature of the developmental trait investigated here awaits elucidation,”¹¹⁷ so it’s not clear what degree of genetic change has occurred. At the very least, Smith (1988) shows that developmental timing may have changed so that flies emerge when fruit matures, but reproductive isolation does not exist as a result of this small-scale change.

Indeed, another paper cited by the FAQ (Feder *et al.*, 1988) called the populations mere “races” of the same species because they are “partially reproductively isolated.”¹¹⁸ This paper did find

differences in frequencies for six alleles in apple and hawthorn fly populations, but it also noted that these were not the result of an inability to hybridize leading to postmating isolation, stating: “Hawthorn and apple flies readily mate in the laboratory and produce viable F₁ progeny.”¹¹⁹ The paper further notes that “the likelihood of reproductive incompatibility between these flies is remote.”¹²⁰ Thus, whatever genetic differences do exist, they are insufficient to produce anything less than viable offspring between the populations.

Because fertile hybrids are readily produced, Feder *et al.* (1988) proposes that any restriction in gene flow between the two groups is the result of premating factors. But the paper finds that any isolation that does exist is not sufficient to warrant calling the populations different species: “We consequently believe that it is inappropriate to state definitively that hawthorn and apple races represent ‘incipient’ species.”¹²¹ Yet this is the example which the speciation FAQ author called “very exciting.”

The FAQ states that “Hawthorn and apple ‘host races’ of *R. pomonella* may therefore represent incipient species. However, it remains to be seen whether host-associated traits can evolve into effective enough barriers to gene flow to result eventually in the complete reproductive isolation of *R. pomonella* populations.” At present what we know is this: there is only partial reproductive isolation, and the populations readily produce viable offspring, indicating that only limited biological change has emerged. If this is “very exciting” then the evidence for speciation must be limited indeed.

In fact, it’s important to note that some have suggested that we’re in fact not even witnessing the origin of a new species. Another paper cited by the FAQ (Barton *et al.* 1988) states:

“Evolutionary biology is often an attempt to reconstruct history: even for the recent past this is always difficult. In *Rhagoletis*, for example, it is hard to be certain that the apple race is not an existing sibling species which became common only after it invaded apples.”¹²²

Though this is an interesting example, in the final analysis it does not demonstrate the evolution of complete reproductive isolation, nor does it show significant biological change has evolved.

Response to Section 5.5.2, “Gall Former Fly (*Eurosta solidaginis*)”

Summary: Populations of the gall former fly live on different species of host plants, leading some to wonder whether they have formed different species. The evidence shows the flies are mere “races” which only have “partial reproductive isolation,” and thus are not members of separate species since “both the genetic data ... and the behavioral data presented here suggest that there is gene flow between populations.” The most significant differences amount to “a preference for mating on the host plant and different emergence times,” which correspond to the host plant life cycle. Complete reproductive isolation is not established, and only small-scale biological is observed.

This example discussed by the FAQ studied flies of the species *Eurosta solidaginis* that reproduce in host plants of different species. (The host plants are of the same genus; they are

Solidago altissima and *Solidago gigantea*.) One paper cited (Waring *et al.*, 1990) studied 21 genetic loci and found that six showed variation between fly populations on *S. altissima* and *S. gigantea*, and it suggests that the cause is “limited gene flow.”¹²³ The likely cause of isolation is plant host choice, as those flies which were found on *S. altissima* preferred *S. altissima* in lab experiments, and those flies which were found on *S. gigantea* preferred *S. gigantea* in the same.

Another paper cited in this section by the FAQ (Craig *et al.* 1993) notes that a “host race is defined as ‘a population of a species that is partially reproductively isolated from other conspecific populations as a direct consequence of adaptation to a specific host.’”¹²⁴ It cites a definition which defines host race as populations that are “restricted solely or primarily because of different host preference.”¹²⁵ The paper found that this example fits these definitions. Thus, it’s noteworthy that these populations entail mere “races”—not separate species—where there is only “partial reproductive isolation” between the races, which “is maintained only through association with the host plant.”¹²⁶

To be more specific, the partial reproductive isolation between the populations is thought to be “maintained by a combination of a preference for mating on the host plant and different emergence times.”¹²⁷ But those premating isolation mechanisms do not imply that the populations cannot or do not interbreed. In a breeding experiment without host plants, “38% of the matings took place between host-associated populations” and thus “[v]ery weak assortative mating exists in the absence of host plants.”¹²⁸ Even in the wild complete reproductive isolation does not exist, since “both the genetic data ... and the behavioral data presented here suggest that there is gene flow between populations.”¹²⁹

Finally, it is noteworthy that the paper reported “crosses between the *gigantea* and *altissima* fly populations produce viable and fertile offspring.”¹³⁰ Thus, significant change has not emerged between these populations.

Given that the populations are “incompletely reproductively isolated” and that viable and fertile hybrid offspring can be produced, it seems that once again, complete reproductive isolation is not observed and only low levels of biological change have evolved.

Response to Section 5.6, Flour Beetles (*Tribolium castaneum*)

Summary: Experiments which selected for high, and low weights within flour beetles managed to increase the mean weight in various lines by about a milligram. This is not significant biological change. Some assortative mating was found but reproductive isolation was not complete.

This study cited by the FAQ (Halliburton and Gall, 1981) took a collection of flour beetles, divided them into 4 lines, and in each line selected for those with the heaviest, and lightest weight at the pupal stage over the course of successive generations. At the beginning of the experiment, mean weight of the lines was a little over 2 milligrams. By the end, the mean weight of all groups had increased; the group with the largest increase saw a mean weight of about 3 mg. They thus report “selection for high pupa weight was more effective than selection for low pupa weight.”¹³¹ As far as morphological change goes, a 1 mg increase in mean weight of the

pupae was the most that was observed; there are apparently impediments to significantly decreasing the weight of these pupae. The most this experiment achieved was to select beetles of slightly differing weights.

Two lines did not show any change in mating preferences, while two showed assortative mating, leading to a lack of “intermediate” pupa weights.¹³² This however was not due to inviability of hybrids, but due to mate choice: “Clearly, the offspring of heterogamic matings did survive at least as well as the offspring of homogamic matings, and were intermediate in weight. Any deficiency of intermediate weight pupae must, therefore, be due to a deficiency of heterogamic matings.”¹³³ In the lines that showed assortative mating, reproductive isolation was incomplete. Indeed, such changes in body size alongside changes in mating preferences are nothing new as another paper cited by the FAQ (Schluter and Nagel, 1995) states that various studies show “size is important in premating isolation.”¹³⁴

These results are consistent with those of many other studies discussed by the FAQ: complete reproductive isolation was not established, and meager biological change was observed.

Response to Section 5.7, “Speciation in a Lab Rat Worm, *Nereis acuminata*”

Summary: Initially the investigators thought they had discovered a completely reproductively isolated population of polychaete worms that had been subjected to phases of bottlenecks and population growth in the lab. However, a later study found that these conclusions were wrong, since “the Lab population was already a species different from P1 and P2 at the time when it was originally sampled in 1964.” Thus, what happened was the investigators sampled a naturally occurring independent species of polychaete worms and mistakenly concluded that a new species had formed in the lab. The original paper which originally reported this example stated: “the entire process of speciation has rarely been observed.” This paper did not remedy that problem.

The FAQ cites a paper (Weinberg *et al.*, 1992) which purports to have discovered the establishment of complete reproductive isolation among animals—i.e., speciation under the biological species definition. As this section of our response will discuss, while initially they felt confident they had found evidence of speciation in the laboratory, later evidence overturned this claim.

Three populations of polychaete worms of the species *Nereis acuminata* were collected from the coastline around the Long Beach, California area. One population (“Lab”) went through “two bottlenecks, each followed by exponential population growth.” The other two populations (P1 and P2) were collected directly from the field and crossed with the Lab population. While P1 and P2 could produce viable offspring when crossed, crosses between Lab and P1, and Lab and P2, could not. Some premating isolation between Lab and P1/P2 due to mating preferences was also found. They suggest that a difference in chromosome 9 between Lab and P1/P2 might be responsible for the death of hybrids. Thus, the populations are so similar that they can produce offspring, but those offspring are not viable. However, they are not exactly yet sure why the populations are reproductively isolated:

“In particular, we can not say whether the alleged speciation, reported here, resulted from selection in the novel laboratory environment (adaptive radiation) or from a stochastic process such as genetic drift or founder effect ... Testing these competing hypotheses and determining the genetic basis of each form of reproductive isolation represent difficult challenges for the future.”¹³⁵

In any case, in this one instance it is claimed that they found establishment of complete reproductive isolation. But as the notable evolutionary biologist Theodosius Dobzhansky (1972) reminds us, “Reproductive isolation evidently can arise with little or no morphological differentiation.”¹³⁶ That seems to be the case here as the paper reports no morphological change between the populations. Perhaps some slight change in the karyotype of the Lab population is responsible for the reproductive isolation, but no apparent morphological change was reported.

There’s a very important epilogue to this story, however. Four years later, in 1996, the lead author of the original study co-published a follow-up study which essentially retracted and refuted claims of speciation in the lab. The follow-up paper states:

“A critical assumption in Weinberg's experiment is that the P1 and P2 populations are, in fact, representatives of the natural population from which the Lab population hypothetically had diverged and speciated in the laboratory. We have tested this hypothesis by assaying 18 electrophoretic gene loci in the Lab, P1 and P2 populations and in an Atlantic population of a different species, used as a reference control. If the Lab population had speciated from P1 or P2, we would expect that randomly selected electrophoretic markers should be largely similar between the Lab and P1 or P2 populations. However, no common alleles between Lab and P1 or P2 are found in 13 (725) loci, and at two more loci the alleles fixed in Lab are at low frequencies in P1 and P2. The genetic distances between Lab and P1 or P2, are 1.75 ± 0.51 and 1.76 ± 0.52 , larger than between most pairs of congeneric species in many sorts of organisms; and roughly similar to the distance between P1 or P2 and the reference population from the Atlantic ($D=1.36 \pm 0.40$). The Lab population is genetically depauperate, most likely as a consequence of the founder event, but this reduced variability contributes only trivially (about 1%) to the genetic differentiation between the populations. **We conclude that the Lab population was already a species different from P1 and P2 at the time when it was originally sampled in 1964.**”¹³⁷

Thus, this is not an example of speciation in the laboratory, but the original investigation had simply sampled two naturally occurring separate species. Unfortunately the Speciation FAQ has not been updated to accommodate these findings, reported 15 years ago in 1996.

The initial Weinberg *et al.* (1992) paper which originally reported this alleged example of speciation stated: “the entire process of speciation has rarely been observed.”¹³⁸ This example did not remedy that problem.

Response to Section 5.9.2, “Morphological Changes in Bacteria”

Summary: The FAQ claimed bacteria “underwent major morphological change” but the technical paper it cites does not claim the change was “major.” The change entailed a growth in bacterial cell size—from about 1.5 μm in length to up to 20 μm —which allowed larger bacteria to escape predation. However, the change also involved a fitness cost, where the bigger bacteria faced “a selective disadvantage” when competing with smaller cells in a predator-free environment. Fitness costs in bacteria often limit the ability of new forms to persist, or evolve further. The investigators never claim that a new species of bacteria has evolved. This probably represents the most significant example morphological change reported in the FAQ, but it was in bacteria which are known to vary widely in response to selective pressures, and the change involved a significant fitness cost. After this study was published, the British bacteriologist Alan Linton stated: “Throughout 150 years of the science of bacteriology, there is no evidence that one species of bacteria has changed into another.”¹³⁹ This study does not claim to contradict Linton’s conclusion.

In this instance, the FAQ claims that a bacterium “underwent a major morphological change when grown in the presence of a ciliate predator.” The change was that the bacteria got longer: Normally this species grows as rods of about 1.5 μm in length (type S), but they observed cells of various sizes up to 20 μm in length (type L) after an apparent selective response to escape predation. The paper cited by the FAQ (Shikano *et al.*, 1990) do not claim the change is “major.”

Bacteria of many sizes and shapes are known, and bacteria are well-known to evolve resistance to selective pressures. However, resistance to a selective agent often involves a fitness cost. In this case, the changes also involved a significant fitness cost. As (Shikano *et al.*, 1990) reports:

“The type L population was at a selective disadvantage in predator-free competition with type S or the parental strains.”¹⁴⁰

A paper published in *Environmental Toxicology and Chemistry* observed that “[t]he topic of fitness costs is a central theme in evolutionary biology” because “fitness costs constrain the evolution of resistance to environmental stress.”¹⁴¹ Thus, the observed fitness costs could place constraints on the ability of the type-L cells to persist, or evolve further.

Since bacteria reproduce asexually it is difficult to define what constitutes a “species” within bacteria, and whether this should be considered a new “species.” Konstantinidis *et al.* (2006) suggest that strains with 95% average nucleotide identity (ANI) should be considered part of the same species. The paper cited by the FAQ (Shikano *et al.*, 1990) does not elucidate the genetic basis for the change, and there may be no other changes in the bacterium apart from its change in size. While Shikano *et al.* (1990) does not explore the ANI between the two types, it would seem likely that they would meet this definition of “species.” In any case, it is significant that Shikano *et al.*, 1990 never makes any intimation that a new species of bacteria has evolved.

It’s noteworthy that this probably represents the most significant example of morphological change reported in the entire FAQ, but it was in bacteria which are known to vary widely in

response to selective pressures, and the change involved a significant fitness cost. In 2001, over ten years after this study was published, the British bacteriologist Alan Linton stated: “Throughout 150 years of the science of bacteriology, there is no evidence that one species of bacteria has changed into another.”¹⁴² Shikano *et al.* (1990) does not claim to contradict Linton’s conclusion.

PART IV: DOES THE EVIDENCE FOR SPECIATION COME FROM NATURE OR GROUPTHINK?

The FAQ claims “the biological community considers [speciation] a settled question. Many researchers feel that there are already ample reports in the literature.” But this is contradicted by the very literature cited by the FAQ:

- For example one paper cited by the FAQ (Weinberg *et al.*, 1992) admits that “the entire process of speciation has rarely been observed.”¹⁴³
- Another paper cited by the FAQ (Thoday and Gibson, 1962) states: “Though speciation is one of the more striking features of evolution, direct experimental evidence concerning the origin of species is limited.”¹⁴⁴
- Yet another paper cited by the FAQ (Dobzhansky and Pavlovsky, 1971) provides the striking admission that: “we are in a situation today similar to that experienced by Darwin more than a century ago: differentiation of species is inferred from copious indirect evidence, but has not actually been observed.”¹⁴⁵

Again, my purpose has never been to deny that speciation can occur in nature, especially when speciation is defined by the trivial definition of a mere reproductively isolated population. Rather, my purpose is to test the FAQ’s claims. In that regard, if the FAQ is correct that “Many researchers feel that there are already ample reports in the literature,” then the quotes above, and this analysis as a whole, suggest those researchers are wrong.

Perhaps this is an instance where many Darwinian biologists take speciation on faith, an assumption which needs no proof; *someone else* has explained speciation. Ironically, the FAQ’s author reports an informal survey which seems to document such groupthink regarding the evidence for speciation:

“I asked about two dozen graduate students and faculty members in the department where I’m a student whether there were examples where speciation had been observed in the literature. Everyone said that they were sure that there were. Next I asked them for citations or descriptions. Only eight of the people I talked to could give an example, only three could give more than one. But everyone was sure that there were papers in the literature.”

In other words, “everyone was sure” that the literature contained documented examples of speciation, but only 1/3 could provide an example of such, and only 1/8 could provide more than one example.

Presumably the references provided by these graduate students and faculty members provided the basis for many of the references in the FAQ which, as we have seen, almost entirely fail to document speciation.

In his book *The Politically Incorrect Guide to Darwinism and Intelligent Design*, Jonathan Wells also analyzed some of the examples in the FAQ. His analysis states:

“Anyone who takes the time to plow through the references cited in these essays finds that most of the alleged instances of ‘observed’ speciation are actually analyses of already existing species that are used to defend one or another hypothesis of how speciation occurs.”¹⁴⁶

After taking that time and plowing through the references, I believe that Dr. Wells is correct. As we have seen, at most only “reproductive isolation” was observed—but that is **very different** from observing significant biological change. **In fact, in most instances: (1) complete reproductive isolation was not even observed so the examples fail to meet the biological species concept definition of “species.” And (2) even when reproductive isolation was observed, only very small amounts of biological change were observed, trivializing the importance of the example.**

Those examples which fit into category (2) show that the claims of “speciation” often sound impressive, but in reality evolutionists are hiding behind impressive-sounding terminology in order to make it sound like significant biological change has evolved, when in reality virtually nothing of interest happened.

PART V: CONCLUSION

The TalkOrigins “Observed Instances of Speciation” FAQ claims it “discusses several instances where speciation has been observed.” After scrutinizing much of the technical literature cited by the FAQ, however, we see claims of mere “incipient” speciation, where reproductive isolation is “initiated” but only “weak” or “partial” and “not complete,” or merely “the first step of one possible route to” speciation. Moreover:

- In plant hybridization studies, there was only one example where a new viable species was demonstrated (Section 5.1.1.5). This species does not show significant morphological change from its parent species. Moreover, speciation by hybridization and polyploidy is not a viable mechanism for the vast majority of evolution because: (1) it occurs only within flowering plants, (2) it does not produce new morphological characteristics, and (3) polyploid hybrids cannot arise without pre-existing parent species, meaning it entails a collapse—not gain—of existing diversity. This cannot be a major mechanism in animal speciation.
- **In all of the other examples analyzed, there was not a single example found where complete reproductive isolation, and thus speciation, was demonstrated. There were also no examples of significant morphological change.**

This study has thus arrived at similar results as the assessment by Jonathan Wells in his book *The Politically Incorrect Guide to Darwinism and Intelligent Design*:

“So except for polyploidy in plants, which is not what Darwin’s theory needs, there are no observed instances of the origin of species. As evolutionary biologists Lynn Margulis and Dorion Sagan wrote in 2002: ‘Speciation, whether in the remote Galápagos, in the laboratory cages of the drosophilosophers, or in the crowded sediments of the paleontologists, still has never been directly traced.’ Evolution’s smoking gun is still missing.”¹⁴⁷

The TalkOrigins “Speciation FAQ” gives no evidence that anything has changed significantly since Dobzhansky stated that “we are in a situation today similar to that experienced by Darwin more than a century ago: differentiation of species is inferred from copious indirect evidence, but has not actually been observed.”¹⁴⁸ **Those who believe that this FAQ provides evidence for significant morphological change or even speciation (e.g., complete reproductive isolation) have been badly misled.**

¹ See <http://www.talkorigins.org/faqs/faq-speciation.html> (downloaded July 27, 2011).

² This response responds to as many examples in the FAQ as possible where the original papers cited in the FAQ could be downloaded at a local university library. Some of the examples cited in the FAQ refer to very old papers that were not easily accessible. This rebuttal thus responds to 21 out of 30 total sections in the FAQ.

³ Diane M.B. Dodd, “Reproductive Isolation as a Consequence of Adaptive Divergence in *Drosophila pseudoobscura*,” *Evolution*, Vol. 43 (6): 1308-1311 (September, 1989).

⁴ Dolph Schluter and Laura M. Nagel, “Parallel Speciation by Natural Selection,” *The American Naturalist*, Vol. 146 (2):292-301 (August, 1995).

⁵ Theodosius Dobzhansky, “Species of *Drosophila*,” *Science*, Vol. 177 (4050):664-669 (August 25, 1972).

⁶ William R. Rice and Ellen E. Hostert, “Laboratory Experiments on Speciation: What Have We Learned in 40 Years?,” *Evolution*, Vol. 47 (6):1637-1653 (December, 1993).

⁷ Alan Linton, “Scant search for the maker,” *Times Higher Education Supplement* (April 20, 2001):29.

⁸ Marion Ownbey, “Natural Hybridization and Amphiploidy in the Genus *Tragopogon*,” *American Journal of Botany*, Vol. 37 (7):487-499 (July, 1950).

⁹ Marion Ownbey, “Natural Hybridization and Amphiploidy in the Genus *Tragopogon*,” *American Journal of Botany*, Vol. 37 (7):487-499 (July, 1950).

¹⁰ Indeed, Soltis and Soltis (1989) acknowledge that “Previous morphological, cytological, and electrophoretic analyses indicated that *T. mirusa* rose independently at least three times.” Douglas E. Soltis and Pamela S. Soltis, “Allopolyploid Speciation in *Tragopogon*: Insights from Chloroplast DNA,” *American Journal of Botany*, Vol. 76 (8):1119-1124 (August, 1989).

¹¹ Jennifer A. Tate, Douglas E. Soltis, and Pamela S. Soltis, “Polyploidy in Plants,” p 395 in *The Evolution of the Genome* (T. Ryan Gregory ed., Elsevier Academic Press, 2005).

¹² Arne Müntzing, “Cyto-genetic Investigation on Synthetic *Galeopsis tetrahit*,” *Hereditas*, Vol. 16:105-154 (1932).

¹³ Jonathan Wells, *The Politically Incorrect Guide to Darwinism and Intelligent Design*, p. 53 (Regnery, 2006) (emphasis added) (quoting Douglas J. Futuyma, *Evolution*, p. 398 (Sinauer Associates, 2005)).

¹⁴ Jonathan Wells, *The Politically Incorrect Guide to Darwinism and Intelligent Design*, p. 53 (Regnery, 2006).

¹⁵ Theodosius Dobzhansky and Olga Pavlovsky, “Experimentally Created Incipient Species of *Drosophila*,” *Nature*, Vol. 230:289-292 (April 2, 1971).

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- ¹⁶ Eric W. Rabe and Christopher H. Haufler, "Incipient Polyploid Speciation in the Maidenhair Fern (*Adiantum pedatum*; Adiantaceae)?," *American Journal of Botany*, Vol. 79 (6):701-707 (June, 1992).
- ¹⁷ Eric W. Rabe and Christopher H. Haufler, "Incipient Polyploid Speciation in the Maidenhair Fern (*Adiantum pedatum*; Adiantaceae)?," *American Journal of Botany*, Vol. 79 (6):701-707 (June, 1992).
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