Creator. Accordingly, He formed some obligate seeders (like certain species of *Ceanothus*) for vigorous survival after intense fires where resprouters may have largely died. Conversely He formed some resprouters to restore vegetation efficiently after fires that come with greater frequency. In this case fewer seeds have been produced (because of lack of time) and the fire has been of lower temperatures so the resprouters flourish. It reminds one of a failsafe engineering system whereby regrowth will occur no matter what fire conditions prevail.

What at first seemed to be a dull study of two groups of little southern California shrubs has turned out to be crucial ground for testing creation research against evolutionary idealogies. It appears at this writing that the scientific creation model is superior.

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DOES CHROMOSOMAL REORGANIZATION REALLY LEAD TO THE ORIGIN OF NEW SPECIES?

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Some people have appealed to chromosomal reorganization as a way of getting large changes in a hurry, as is needed according to the notion of punctuated equilibrium. It is shown here that there is no evidence that such changes will really lead to anything which is new, viable, and improved. Punctuated equilibrium is still a hypothetical process, for which no mechanism can be found.

Stephen J. Gould in his article entitled: "Is a New and General Theory of Evolution Emerging?[1]" states that "the most exciting entry among punctuational models for speciation in ecological time is the emphasis, now coming from several quarters, on chromosomal alterations as isolating mechanisms." Then he quotes the work of Carson with great approval. So let us see what Carson proposes in his paper entitled: "The Genetics of Speciation at the Diploid Level."[2] He states that "Indeed, the origin of the genetic basis of species differentiation appears as an important unsolved problem of evolutionary biology." (introductory paragraph.) After pointing out that often it is difficult to decide where the species line can be drawn between two gene pool communities, he then proposes new criteria for the determination of species boundaries and how they might have originated. Contrary to the usual basic assumption of neo-Darwinism that the effects of mutation and recombination by meiosis and syngamy can reach into every corner of the genome (or gene pool of the species) and shake it up, Carson maintains that part or even most of it is essentially closed to that process in natural populations. In fact he claims that in every species there are two systems of genetic variability, the "open" and the "closed" system.

In the open system mutants are either codominants or simple recessives. Both homozygotes tend to be fully viable. He is of the opinion that these genes tend to have a superficial or possibly even a trivial effect, at least in their individual action. These he believes respond readily to either artificial or natural selection. Quantitative traits would come under this category. However, as I have pointed out in various articles, there has been as yet very little proof that any mutations are of value to the organism as regards survival and actually most are actually defective under the usual environment of the species. This has been fully discussed in my article: "Mutations Reflect the Glory of God's Handiwork.[3] As to their responding to natural selection we have yet to see any demonstration. Thus even the classical case

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always referred to in college text books, that is, the melanic vs. the gray moth *(Biston betularia)* did not result in a uniform true breeding race of melanic moths. Even when the chimneys of England manufacturing districts made the bark of the trees the blackest that they have ever been, about 1% of the moths remained gray! Investigators found that this was because the normal gray type had a higher reproductive rate. And when environmentalists forced the manufacturers to eliminate most of the smoke, and the trunks of the trees became lighter again, the percentage of gray moths incresed. To really prove the case for natural selection it should have been possible to observe the establishment of a completely uniform population of black moths free of any gray ones, and also incapable of interbreeding with them! Then indeed we would have had a clear cut demonstration of the origin of an incipient species by natural selection.

However, Carson's point is well taken that in order to have distinct species we must have closed variability systems of internally balanced blocks of genes or supergenes locked into strong epistasis. By this he means a non reciprocal interaction between nonalternative forms of genes in which one gene suppresses the expression of another affecting the same part of an organism. He believes that though inversion is not necessary for the establishment of these blocks, a genetic climate is created for them and also for chiasma localization. Now most inversions are of lower viability since they involve breakage of the chromosome. So it is unlikely that they would be established. In fact Carson does not believe that even linkage is needed if the epistasis is strong.

Mainly this type of system differs from the open system as regards its failure to yield easily to Mendelian analysis. Recombinational events occurring within the blocks result in individuals with reduced vitality or fertility, and often the recombinations are even lethal. Since the individual elements cannot be separated such regions cannot be mapped. Now most certainly in my breeding work I have come across such behavior; in fact it is the rule in rose breeding.

Now according to Carson certain genes or gene systems may be homozygous in closed systems but most are balanced heterozygotes which confer extra vigor. The stringency of natural selection in his opinion prevents the release of the genetic elements which are in this closed system as individual genotypes. These genes then are locked into a sort of obligatory epistasis. Now in my opinion though this sort of system undoubtedly exists and is typical of the differences between most species, it most certainly did not come about by natural selection but rather was built into the genetic system of the species at its creation.

Carson goes on to say that speciation is usually viewed as a gradual microevolutionary process. The genetic events which lead to speciation are thought to be individually simple, and so are merely the accumulation of advantageous mutations. Contrary to this Carson proposes that speciation is accomplished by a series of catastrophic events. It is initiated by an unusual forced reorganization of the epistatic supergenes of the closed variability system. This is accomplished by a population condition in which natural selection is temporarily relaxed. It is then one in which the population greatly increases in size. This sort of thing might well occur following a season of unusually heavy rainfall, mild winter and early spring weather such as was experienced in the spring of 1969, when there was such a great profusion of wild flowers in California. Carson calls this the "flush" phase and the major genetic effect is that of a very great expression of genetic variation. No increased quantity or changed quality of recombination need occur. Due to the great number of individuals and ideal conditions recombination types are now able to survive, which under the usual adverse conditions would be eliminated by natural selection. These population flushes are inevitably followed by a crash in population size. Just as the flush phase is non selective so also in the crash phase survival is a random one which is not influenced by any natural selection. The permissive or ideal conditions (such as the high rainfall and warm weather in the spring of 1969) are suddenly removed entirely, forcing the scattering or death of millions of organisms without regard to their detailed genetic constitutions. Following this event only a single plant or animal called a "founder" may leave descendents. Such an individual may well have a most unusual recombination of genes from the usually closed variability system. In the absence of competition and in the newly improved environment this founder may very well survive, reproduce, and be the start of a radically different and quite new closed variability system. This might well be accomplished in a few generations. When there are sufficient numbers of individuals for natural selection to again become operative, it will have quite a different set of genetic materials to work on. Accordingly it is unlikely that the new closed variability system will precisely reconstitute the earlier one.

Carson then goes on to say that this new system may show a basic incompatibility when tested against the diploid genotype from which it came. Just how such a species level incompatibility would arise in the founder population is not made clear by Carson.

In our plant succession studies reported in 1974[4] Howe and I found the sort of survival postulated by Carson. A most unusual wooly, cylindrical leaf type appeared among the population of *Salvia carduaceae* in the ideal spring of 1969. The usual leaves of this species are pinnate with 6-8 pairs of well indented pinnae each having long spines. The cylindrical leaf type was rounded and very wooly, without any spines. It did not show up at all in the dry springs of 1970, 1971, and 1972. But in the very wet season of 1973 the only surviving plants in the plot were the cylindrical leaf type! So then the sort of survival postulated by Carson can occur. Unfortunately we did not test this cylindrical leaf type against the typical form in order to see if there was any incompatibility. Also it was not possible to continue the studies after 1974 in order to see if it continued to be the only surviving type.

Though Carson does not make clear just how incompatibility between the new and the old genetic system arises, I am of the opinion that many of our so called species arise in just the way that Carson postulates. As to whether inversions and translocations ever can be established in such incipient species and so lead to complete intersterility remains to be demonstrated. C.R. Burnham[5] reports that an interchange or translocation between chromosome 8 and 9 in maize when selfed gave rise to plants homozygous for the interchange and also normal plants without it. He does not report as to whether these homozygous translocation plants grew as vigorously as the normal type, but does state that they show ten bivalents at meiosis and have no more ovule or pollen abortion than do normal plants. Burnham has spent most of his life studying translocations in corn and has published a monograph on his finding.[6] Though many translocations are lethal when homozygous, certainly some are not. There is still the problem of how such translocations could become established under natural conditions. Thus the plant in which the translocation originated would be semi-sterile of course, and so at a disadvantage as regards reproduction compared to all the normal type plants around it. The relatively few homozygous translocation plants among its offspring would at maturity be pollinated mostly with pollen from normal type plants in a cross pollinated plant such as corn. Accordingly most of the third generation plants would be semi-sterile and relatively speaking there would be even fewer translocation homozygotes. It is therefore necessary to postulate an isolation factor such that the homozygous new chromosome type would somehow only be pollinated by sister plants of the same chromosome constitution. Presumably this might occur by this translocation being tightly linked to a factor for later flowering than the normal type. There is then the possibility of a build up of translocations so that when the new founder type comes into contact with the original one, all of the hybrids would be quite sterile.

Obviously the same line of reasoning applies to inversions and the possibility of their establishments. It should be pointed out that in animals homozygous translocations usually have a significant lowering of viability and fertility. Thus in Drosophila fruit flies Dobzhansky reports that out of 332 tested only 14 had 100% viability, the others varying from 33% to 71% viability, and out of 120 of these tested for fertility only 32 were fully fertile.[7] It is therefore not as easy evidently to establish new chromosome translocation types in the fruit fly.

This whole field of study is one which might well be undertaken when we have our research laboratory established and funds available for research workers. Mainly we need to know more about the problems involved in establishing translocation homozygotes under natural field conditions. Though papers dealing with this sort of study may have been published, I am not able to find them, and it seems that our evolution minded geneticists have mainly been interested in showing that they occur, calculating linkage with marker genes, as well as crossing over percentages, and of course studying their behavior at the reduction division, particularly at the mid-prophase stage, These studies are very necessary of course, but do not answer the question as to whether or not they could become established under the usual field conditions.

My present feeling is that these chromosome reorganizations and in fact the very flush-crash-founder cycle postulated by Carson involved supernatural knowledge as to just how to effect the various gene system modifications following the catastrophe of the Flood and the later related and more localized ones. But should it be possible to show that this could all come about "naturally", it would of course make it much easier for creationists to convince their skeptical evolution minded colleagues that the remarkable species variation and adaptation we now see could actually have come from the relatively few plant and animal "kinds" which survived the Flood. Especially difficult is the problem we face in trying to explain how all of this could have happened in the relatively short time since this event. At least Carson removes the concept of such minute micro-evolution-progress due to mutation that the very idea of any speciation in less than hundreds of thousands of years is unthinkable.

Now what about polyploidy as an important factor in evolution? Stebbins and Ayala[8] make the statement that "polyploidy is the limiting case of rapid speciation—requiring only one or two generations—through chromosomal change. "However, Stebbins is far from as enthusiastic about the value of them when he discusses artificially induced allopolyploids in his chapter on polyploidy, its occurrence and nature in his book entitled "Variation and Evolution in Plants.[9] Thus he lists more than 25 allopolyploids which have been produced artificially and states that many of them such a *Crepis rubra-foetida* (Poole 1931), *Layia pentachaeta-platyglossa* (Clausen, Keck and Hiesey, 1945a, and *Allium cepa-fistulosum* (Jones and Clarke 1941) have been so sterile in both the original and later generations that they would have been complete failures under natural conditions. Others such as *Primula kewensis* (Skirm 1942) and *Nicotiana glauca-langsdorfii* (Kostoff 1938b) have either been fertile from the start or have yielded highly fertile and in some instances constant types after a number of generations of selection. Now as regards *Primula kewensis* careful cytological studies have shown that pairing of the chromosomes is not entirely regular, and so it shows considerable variability and would hardly survive under natural conditions.

Karpechenko's Raphanobrassica hybrid at first claimed to be a perfect example of a successful amphidiploid has been shown by Richaria and Howard to be considerably less than that. The F_2 plants were only partially fertile and even in the F_4 fertility varied from only 5 to 42%! This and also Arne Muntzing's *Galeopsis artificial Tetrahit* are quite fully discussed in my article on "Discoveries Since 1859 Which Invalidate the Evolution Theory."[10]

There I indicated that his artificial Tetrahit most probably originated by pollination of his triploid plant with a diploid pollen grain of the species *G. Tetrahit* which was growing nearby and so actually was the species rather than any synthetic hybrid. The main evidence for this conclusion is that the F_1 hybrid between *Galeopsis pubescens* ($N=8$) and *G. speciosa*

($N = 8$) shows five to eight pairs of chromosomes at the reduction division. Accordingly one would expect some quadrivalents if his triploid plant had actually been pollinated by *G. pubescens*. (His F_2 triploid had 16 *G. speciosa* and 8 *G. pubescens* chromosomes.)

The problems involved in the presumed origin of our modern wheat varieties, that is the hexaploid *Triticum aestivum* have been discussed in detail in the article on the origin and distribution of cultivated plants by Howe and Lammerts in the June 1980 issue of the *Creation Research Society Quarterly*.[11] The conclusion was drawn that the various complex crosses involved in deriving our modern wheat from the basic diploid species could only have been accomplished by ancient and very skilled plant breeders! One of the lines of evidence is simply that wheat is obligately and tenaciously self-fertile. Accordingly it is very hard to imagine a situation in which for example the cross of the tetrapoloid wheat, previously derived from the two postulated diploid species, would cross with the grass *Aegilops squarrosa*, the species contributing the D genome. This cross is difficult enough to make even when the flowers of tetraploid wheat are first emasculated and then pollinated with *Ae. squarrosa*. So just how the normally self fertilized wheat would ever set seeds from such stray pollen of a different genus is not as easy to imagine as Stebbins would lead us to believe.

In conclusion it seems that polyploidy is far from the proven limiting case of rapid speciation in only a few generations. As pointed out in my article on discoveries since 1859 referred to above it is obvious that for any amphidiploid to qualify as an incipient species the original F_1 hybrids should show no pairing, yet give a reasonable percentage of diploid gametes. The experiments with them should be conducted in such a way that only self-fertilization can occur, and the fertility and vigor of the F_2 should be at least comparable to that of the diploid species. Few if any of the reported amphidiploids qualify as regards all of these requirements.

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THE GENETIC INTEGRITY OF THE "KINDS" (BARAMINS): A WORKING HYPOTHESIS†

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Modern evolutionary theory is based on the assumption that all of heredity has its basis in the DNA genes. Evidence is presented here to show that that assumption is false. There are, in fact, two major interacting systems of heredity. The implications of these facts for Creationist research are discussed.

A. Introduction

Every schoolboy learns of Mendel's famous experiments, which showed that all heredity has its basis in the genes and that these genes are "particles".[1] The mutation of these genes is now regarded as the basic evolutionary process.

The appeal of these conclusions is obvious: if species are essentially genes, then by genic change species can be changed and the continuity of evolution can be explained. Evolutionary genetics would have impeccable scientific credentials.

However, what most never learn is that *neither Mendel nor any later scientist has shown any such thing.*

B. Mendel's Theory of Genetics

After the epochal work of Galileo and Newton, nature was generally regarded as a great *mechanism* whose workings could be described mathematically. If we knew the positions and motions of all the atoms at a given instant, then, in principle, all would be understood; and both past and future would lie open to our view. Mendel undoubtedly imbibed these optimistic ideas during his study of physics and mathematics at the University of Vienna (1851-1854); for he set his own experimental results in an atomistic context.

Mendel showed that observed *differentials* (e.g., round or wrinkled) among the features (e.g., shape of

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