Lehmann schlägt schließlich vor, wenn Lotsy den Artbegriff Linnés durchaus nicht als solchen bezeichnen wolle, ihm den früher von Lotsy selbst vorgeschlagenen Namen Linnéon zu geben.

Was schließlich die reine Linie anbetrifft, so muß man unterscheiden zwischen der reinen Linie sensu stricto und der, die uns als solche erscheint, die aber heterozygotisch sein kann, wie durch Kreuzungen plötzlich herauskommen kann. Lehmann hat dies beides verwechselt und in seiner ersten Arbeit von reinen Linien geredet, die noch hochgradig heterozygotisch sein könnten. Als Beispiel führt er zwei Sippen von *Linaria maroccana* an, die beide weiß einer reinen Linie anzugehören scheinen, aber miteinander gekreuzt durch verschiedene Reaktion des Zellsaftes bunt ergeben können. Selbstverständlich kann man das keine reinen Linien nennen. Lotsy weist auf die Definition Johannsens hin: Eine reine Linie ist der Inbegriff aller Individuen, welche von einem einzigen absolut selbstbefruchtenden homozygotischen Individuum abstammen. Mit diesem Irrtum, den Lehmann auch zugibt, werden seine Behauptungen hinfällig, daß eine reine Linie keine Einheit des Systems und daß sie ein realer experimentell bestimmbarer Begriff wäre im Gegensatz zur isogenen Einheit, die ein theoretischer Begriff sei. Die Verschiedenheit beider bleibt davon unberührt. Eine isogene Einheit kann auf ein homozygotisches Individuum zurückgehen und ist dann eine reine Linie, sie braucht es aber nicht, da für sie nur genotypische aber nicht genetische Gleichheit gefordert werden muß.

G. v. Ubisch, Berlin.

Lotsy, J. P. Evolution by means of hybridization. The Hague (M. Nijhoff) 1916.

Dr. Lotsy has in several papers advanced the theory that the origin of species (Evolution) is based upon hybridization. This theory is worked out more in details in the present book in which he defends his views very logically and with much ability. The book is read with much pleasure, as it is written in an easily understood language and with much smartness, sometimes even defiant. One does not always become convinced of the correctness of his arguments, but even if the reviewer cannot agree with him everywhere, there is no doubt that Lotsy's hypothesis is one of the ways, and probably the most important one, by means of which we shall overcome the dead-water with regard to the understanding of the origin of species, resulting from the necessary clearing out of the older evolution theories, a clearing which we owe to the pioneers of the new science of heredity (Bateson, Johannsen etc.).

In order to get a clear start, Lotsy begins with definitions of the terms to be used and he does it very well, only that I do not agree with him as to the names of the terms. The vague term which we usually call a species, is baptized a Linneon, and defined as "a group of individuals which resemble one another more than they do any other individuals". Morphological comparison is sufficient to create a linneon.

His next term is a Jordanon, a species in Jordan's sense, i. e. about the same as has been named "petite espèce", "microspecies" or "Elementar-Art", and he defines it by the following words: "a group of externally alike individuals which all propagate their kind faithfully under conditions excluding contamination by crossing with individuals belonging to other

groups". Both morphological comparison and breeding are necessary to create a Jordanon.

The third term is Lotsy's Species, defined as "a group of individuals of identical constitution, unable to form more than one kind of gametes". Besides morphological comparison and breeding an analysis by means of crossing is necessary to create a "species", as we know that two morphologically alike organisms which by breeding give offspring like the parents, in some cases when crossed by one and the same other organism give different offspring.

As hybrids he designates "all individuals able to produce more than one kind of gametes", and as modifications "the non-transmittable effect of external circumstances".

I do not think that the choice of names for the three first categories is lucky, as I consider it both incorrect and inadequate to use the word "species" in Lotsy's sense. It is better to let this word keep its old-fashioned meaning, synonymous with Lotsy's linneon. This is in accordance with the historical development; and it would cause endless confusion, if, in every paper we read, we have to find out in which meaning the word species is used. We may, namely, be sure that systematists, plant-geographers etc. will continue to use it in the old sense. We owe to the genetic researches the possibility of clearly defined terms, but even therefore we must create new names for them, and not restrict old names into special meanings. I propose to continue to use the term species instead of linneon and the term microspecies instead of Jordanon, and to place the term genospecies instead of Lotsy's species. The word "genospecies" is created by Raunkiaer¹⁾ and defined as follows (translated from Danish): a genospecies is a form which by selffertilisation or fertilisation between individuals within the same "pure line" produces an offspring, genotypically alike and like the parent. I think this definition covers in all essential parts Lotsy's definition of his "species". If we admit the three terms: species, microspecies and genospecies, we avoid any confusion and remain in accordance with common practice.

In the following I shall try to give an abstract of the main topics dealt with in Lotsy's book.

Variability (i. e. inheritable variability) upon which all the usual theories of evolution really are based, is, according to Lotsy, never proved and does not exist for the simple reason that genospecies do not vary. The only possibility for proving variability is to demonstrate that a homozygotic individual becomes heterozygotic without any crossing, or, in other words, a monogametic individual becomes polygametic. This is what we call mutation, but mutation must "for the present be discarded as a factor in evolution", as the classical object *Oenothera Lamarckiana* is not a genospecies, but "a mixture of heterozygotes of different constitutions throwing rogues (the pretended mutants) by a process of mendelian segregation". (Also the theory of inheritance of acquired characters is not at all proved, and is in reality not different from the mutation theory, as mutation is inconceivable without some kind of inheritance of acquired characters). The only kind of mutants which, possibly, exist, are those arising from a loss of a factor; but

¹⁾ In Salmonsens, Konversations Leksikon, 2 Udg., Bd. II (Köbenhavn, 1915), p. 157.

even if we admit this possibility, it would not assist the mutation theory as an evolution theory, because an evolution by repeated losses is inconceivable. The mutation theory requires progressive mutants, and "there is not a particle of proof for such an occurrence".

Mendel has shown us the manner in which new forms arise, viz. by hybridization, as, by crossing two monogametic individuals of different constitution, we get a polygametic hybrid. Hybrids are the only known source of the origin of new forms, some of which are heterozygotes, others of which are homozygotes, e. g. new *genospecies*; and in this respect it is indifferent if the parents are "pure species" or hybrids because the result has nothing to do with the origin of the gametes, but only with their constitution.

The question: how does a new form perpetuate its constitution by transmitting it from parent to offspring — the conception which we call heredity —, is answered by Lotsy in an completely agnostic manner: We know "absolutely nothing" about heredity, and cytology does not learn us anything in this respect. Perhaps future researches into haploid organisms, especially mosses, will give us a key to understanding; but for the present we must stick to known facts and work with the origin of diploid organisms as far as possible.

The fact that the individuals which we include in a species (Linneon) resemble each other more than they resemble other individuals and so to say simulate a *genospecies*, requires an explanation.

In order to get at this, Lotsy calculates what will happen (1) if self-fertilisation follows a cross between two different organisms, and (2) if free intercrossing follows such a cross. In the first case we have Jennings's calculation which tells us that in course of many generations nearly all individuals become homozygotes, as the relative number of heterozygotes diminishes and at last becomes minimal; thus a strictly selffertilising species consists of a number of different pure lines. In the second case the theoretical calculation (by Baur and Reimers) gives the result that the number of homozygotes and heterozygotes is about equal; but the real result is another, as is evident from the fact that species within which free intercrossing occurs, mostly have a rather uniform aspect. The cause to this phenomenon is the great rôle played by "intra-linneontic" selection. Based upon interesting considerations and upon examples, Lotsy, therefore, concludes that a species (Linneon) is "a vestigial group of a once much larger group of differently constituted types, born from a cross, which is apt to simulate a species (*genospecies*) by the overwhelming majority of the dominant types it contains, as a result of free intercrossing, combined with a favoring of the dominant by a process of selection, weeding out the weaker or more conspicuous recessives; this uniformity being more apparent than real because pure dominants are indistinguishable, in most cases, from dominant hybrids".

We find in nature that the species are, mostly, rather distinct, and the reason for this is that while free intercrossing within a species is the rule, it is the exception between different species, because such crossing is made difficult in different ways (aversion, differentiation in space, etc.). Our old species "are thus something more than mere conceptions of the human mind; they are natural intercrossing communities of differently constituted types". They owe their origin to an occasional cross and their persistence to the bars which nature normally keeps closed to prevent crossing, but exceptionally opens. In badly limited species the normally

existing bars have lost their effect, as is the case in genera where crossing between species is common. The latter is, as a rule, the case between the microspecies (Jordanons) of a species, the so-called "varieties".

In these rules we find also the explanation of the effect of domestication upon the raising of new forms. The two factors, isolation and selection of the recessive types, are sufficient to explain the effect of domestication, but in many cases crossing with other forms and then segregation have added to the polymorphy of domesticated animals and plants. Using Darwin's book on "Animals and Plants under Domestication" as reference, Lotsy shows that a mixed origin is the rule amongst the domesticated organisms. In many cultivated plants the "races" are really heterozygotes which can only be propagated vegetatively. But in all cases a cross is "at the bottom of is"; it is not the mere alteration of the outer circumstances which has called forth the "variability".

It was said above that crossing between species was an exception, but it does occur both in the animal and the vegetable kingdom. A. Kerner has first emphasized its value with regard to the evolution of plant species; but as Lotsy himself says in the preface, he has perhaps not laid enough stress upon Kerner's importance as the first to advance hybridization as the main factor of the origin of new species. According to Lotsy hybridization is not merely the main factor, but the only known factor; and when it has been argued, that in this way we do not get new species with new characters, i. e. characters not present in the two parents, this is not correct. By crossing different species of *Antirrhinum* Lotsy has himself got types so much diverging from the parents that they ought really be taken as new genera.

We can resume Lotsy's reasoning in the following three sentences: hybridization is the cause of the origin of new types, heredity keeps them persistent, and selection is the cause to their dying-out.

He does not flinch from explaining even the origin of the great systematical classes of organisms by means of his theory. From the palaeontology we know that new classes appear suddenly with numerous, often highly differentiated types, but the more we remove in time from the origin of a class, the more the number of types diminishes and the remaining types become "reduced". This is in good agreement with the theory that from a cross between two species, the offspring of the hybrid are at first very numerous and varied, but little by little the number gets smaller, until only a few types revive, or total extinction follows.

"In the present time we live in a period, in which the extinction of many classes is almost completed and in which no new classes are formed". (I do not think we are able to perceive if a new class is in statu nascendi). As an example Lotsy quotes Scott's system of the vascular plants of which the majority of the divisions are either dying out (*Equisetales*, *Psilotales*, *Lycopodiales* and *Gymnospermae*), or completely extinct (*Pseudoborniales*, *Sphenophyllales* and *Pteridospermae*), only two divisions (*Filicales* and *Angiospermae*) still flourishing.

From his whole manner of argumentation it follows that he has not much good to say about relationship, phylogeny and homology. As to migration he does not deny that it has some value, but means it has been much overrated, as one has taken the monotypic origin of species for granted, while Lotsy means that the origin may be polytopic as well as polyphyletic.

C. H. Ostenfeld.