ratios. Others may be satisfied with an elastic view that allows of profound interactions of factors upon one another with their material modification in the "melting pot of cross breeding."

The lengthy fourth part is an examination of the chief new species of De Vries's cultures as to their behavior in crosses, with special reference to an explanation of this behavior on the theory of intracellular pangenesis. An immense amount of detail is presented, well sifted, however, by the summary and conclusions. It is interesting to note that of these new species *gigas* alone is considered as progressive; *brevistylis, rubrinervis* and *nanella* are regarded as retrogressive, *lata* and *scintillans* as degressive, and *oblonga* as anomalous.

Finally a fifth part on the cause of mutation gives us the latest statement of De Vries's position. This part consists of discussions of a number of topics related to other portions of the book or to earlier publications of the author, and constitutes a general summary. "Gruppenweise Artbildung" results from a gradual accumulation of mutations on the part of a species, and hybridization to De Vries includes a very much wider range of phenomena than the types interpreted by Mendelian analysis.

A comprehensive bibliography of *Œnothera* experimental literature, a full and very valuable citation of the crosses that De Vries has personally made among the Œnotheras, and an excellent index complete the volume. The 121 text-figures throughout the book are of an unusually high grade, and the 22 colored plates admirably executed. It is greatly to be hoped that the author and publisher will promptly arrange for an English translation.

BRADLEY M. DAVIS

Monographia Uredinearum seu specierum omnium ad hunc usque diem cognitarum descriptio et adumbratio systematica. By P. and H. SYDOW. Volumen III., Fasciculus I.: Pucciniaceæ, cum 7 tabulis. Lipsiæ, Fratres Borntræger. 1912. 8vo. Pp. 1–192.

The appearance of the first part of the third volume of the "Monographia Uredinearum" by P. and H. Sydow has been of especial interest to mycologists because it has given the first bit of information concerning the classification which the authors are following, or propose to follow, in this work. The two earlier volumes (Vol. I., Genus *Puccinia,* 1902–4; Vol. II., Genus *Uromyces,* 1910) were entirely taken up with the treatment of two genera, *Puccinia* and *Uromyces,* without the slightest hint as to how they were to fit into any general arrangement. It seemed evident from the beginning that these two genera were given preferences on account of their size and popular importance and not because they might appear in that order in any scheme of classification. The correctness of this surmise is now well shown. The third volume is begun with a key to the genera of the family Pucciniaceæ, a total of twenty-five being recognized. In this key *Uromyces* is number 8 and *Puccinia* number 10. In the preparation of a work of this nature there are many obvious advantages in not being hampered by the publication of a key at the start, before all of the genera are fully studied, which must thereafter serve as a guide. The freedom with which these authors began their task they have deliberately relinquished, for they are herewith publishing a key to twenty-five genera although the descriptive accounts to date only cover fully the first sixteen of them.

To do the monographic work first and follow it with a key will, however, evidently not succeed in eliminating difficulties, as is evidenced by an examination of the present part. For example, one finds that the genera *Uropyxis* and *Diorchidium* are recognized in the key as valid, although they have been treated already in previous parts as synonymous with *Puccinia.* With the exception of these four genera, which have been treated previously, the present part takes up the genera in the order of the key and proceeds as far as the generic description of *Uromycladium,* which is the sixteenth genus.

The genera in the order of their appearance are as follows, *Gymnosporangium, Hamasopra,*

Gymnoconia, Phragmidium, Phragmopyxis, Blastospora, Rostrupia, Triphragmium, Hapalophragmium, Sphœrophragmium, Anthomyces, Uromycladium. These are distributed among the three subfamilies, Phragmidieæ, Uropyxideæ and Pucclnieæ, into which the family is divided. The limitation placed on these subfamilies has not been very rigid, for the genus *Triphragmium* is included in two of them, the Phragmidieæ and the Pucclnieæ. The authors, however, state that they are uncertain regarding the place which *Triphragmium, Hapalophragmium* and *Sphœrophragmium* should occupy in their key. The remaining genera of the family Pucciniaceæ are *Dicheirinia, Gerwasia, Hemilea, Ravenelia, Neoravenelia, Kuehneola, Pucciniostele, Skierka,* and presumably we may expect the next part to deal with them in the order given.

The classification shows conservatism on every hand and especially in the selection of the characters upon which it is founded. The old idea of the importance of the teleutospore is maintained. Such a method can be made to work very well as long as only the common things are considered from a "practical standpoint," but when all forms are considered from a scientific standpoint it can not be said to have much in its favor. The result in the present key is uncertainty and lack of uniformity. The attempt to arrange the key in such a way as to show relationships of the genera is highly desirable, but is not attained when the boundaries for genera are so loose that species contained within them admittedly indicate relationships to different subfamilies. But the composite character of genera can not be avoided with the one character scheme as a basis for grouping. Neither can the segregation of closely related forms be prevented as long as this system is maintained. The purely artificial character of number of cells in the teleutospores will throw forms which are of undoubted relation into different genera. Numerous examples illustrative of this feature have already been cited in the literature[1] and more are constantly being found as careful comparisons are made. An arrangement which places the peach and plum rusts in the genus with *Puccinia graminis* and then separates a few forms from *Puccinia* into the genus *Rostrupia* may be "practical," but if it is any way natural it must be accidental. If *Rostrupia* which differs from *Puccinia* only in having the teleutospores with 3 or 4 cells should be maintained, it is not clear why several of the species of *Gymnosporangium* which have more than 2 cells should not be separated into a genus by themselves, or why the old genus *Phragmidium* should not be broken up into several genera, since the number of cells in the teleutospores in this group is highly variable. If the forms on Rosaceous hosts which have 2–several cells are worthy of generic standing outside of the ordinary *Puccinia* and *Rostrupia* genera, then it is not clear why the 1-celled forms on these hosts should not be separated from *Uromyces,* but such has not been done.

The bulk of the part is taken up by the genera *Gymnosporangium* and *Phragmidium,* 136 out of 192 pages being devoted to them and divided nearly equally between them. The monograph of *Gymnosporangium* is of interest in comparison with the one published by the writer about a year earlier as a Bulletin of the New York Botanical Garden.[2] The order of arrangement of the species, the plan of the keys, and the form of the descriptive accounts are the same as introduced in the writer's bulletin. Three additional species are included in the Sydow monograph, all described since the appearance of the writer's publication and founded on material not seen by him. As to the validity and relationship of the species there has not been the slightest disagreement. With one or two exceptions it is also to be noted that the specific treatment of the *Phragmidium* group is identical, so far as North American species are concerned, with Arthur's account in the Uredinales, "North American Flora." The latter, however, refers some of the species to other genera, *Earlea, Kuehneola,* while the Sydows refer all to the genus *Phragmidium.*

As regards certain nomenclatorial questions,

[1] Arthur, *Mycologia,* 4: 54–56, 1912, and Orton, *Mycologia,* 4: 194–204, 1912.

[2] Vol. 7, No. 26, 1911, pp. 391–483.

it is to be regretted that no such agreement of opinion can be recorded. When a full list of synonyms is included, as in the monograph, no great difficulty is likely to be experienced even when different authors choose to select different names as the one to be maintained, and yet instances will arise which are deplorable. The very first species in this third volume of the Sydow monograph raises some questions. They reject for it the writer's combination, *Gymnosporangium Blasdaleanum* (Diet. & Holw.) Kern, although that specific name is without question the oldest, presumably because it was founded on an æcidial stage, *Æcidium Blasdaleanum* Diet. & Holw. They also refuse to admit for this species another combination of the writer, *G. Libocedri* (P. Henn.) Kern, although this is founded on the "all important" teleutosporic stage and is the oldest specific name thus applied. Henning[3] used the combination *Phragmidium Libocedri,* cited his specimen in full, and accompanied it with an adequate description. He was in error in suggesting that *Gymnosporangium Libocedri* Mayer was the same as his plant, and we can not say that he transferred the Mayer name to *Phragmidium,* but he nevertheless very evidently did intend to apply the name *Phragmidium Libocedri* to his plant and he characterized it accordingly. To reject this specific name because he did not specially propose it *as new* seems to be a motive which is contestable and of little import. Such a procedure not only seems illogical, and is not only not followed by most botanists, but in many similar cases not by the Sydows themselves. They accept *G. Sorbi* Kern, *G. Harknessianum* Kern, *G. Photiniæ* Kern, *G. hyalinum* Kern, *G. tubulatum* Kern, *G. transformans* Kern and *G. bermudianum* Earle, although not one of these was proposed, as a new name in the genus *Gymnosporangium!* They were all transfers of æcidial names as is evidenced by the inclusion of the original authors' names in parenthesis or the words *comb. nov.* Why not say that in these instances the establishment of names has not been accomplished if one refuses to recognize æcidial names and there has been a failure to propose them in *Gymnosporangium* as new? In the case of *G. Amelanchieris* Ed. Fisch. the matter is somewhat different, for although *Amelanchieris* is a name proposed by de Candolle for the æcidial stage of the plant to which it is now applied, Fischer distinctly stated that he was not transferring de Candolle's name, but proposing another just like it as new. This appears to satisfy the conditions which it seems that the Sydows would like to impose and yet in this very instance they have given evidence that they did not regard Fischer's name as a new one but as a transfer, for their Uredineen No. 2287 was issued as *Gymnosporangium Amelanchieris* (DC.) Ed. Fisch., with *Æcidium Amelanchieris* DC. given in parenthesis as a synonym. To go back again to the first species it is to be noted that the authors after rejecting the specific names *Blasdaleanum* and *Libocedri* for it see fit to retain their own name *aurantiacum,* although there is a *Gymnosporangium aurantiacum* of Chevallier[4] published in 1826, seventy-eight years earlier.

As regards the standing of names applied to stages other than the teleutosporic, it seems evident now that these authors will accept them in cases where there is no teleutosporic name. A number of instances have already been cited in a foregoing paragraph. However, in previous parts it is not clear that they have been willing to do this; for example *Uromyces Silphii* Arth. founded on a specific name, which was originally applied to an æcidial stage, has been renamed *Uromyces Junci-tenuis* Syd. nov. nom.[5]

As already suggested, unusually full and accurate lists of synonyms have been included, but the arrangement of these is not always uniform or of such a nature as to make them most usable. It is not a straight chronological arrangement such as used by many authors, but the names are grouped according to the genera so that specific names belonging to the same genera come together. If the order of the

[3] *Hedwigia,* 37: 271–72, 1898.

[4] *Fl. Env. Paris,* 1: 424, 1826.

[5] See Sydow Monog. Ured., Vol. II., fasc. II., p. 289, 1910.

names under a genus were chronological it would facilitate matters, and while it seems to be thus in many cases, it is by no means uniformly so. Under *Gymnosporangium clavariæforme* four synonyms in the genus *Æcidium* are cited, dated as follows, 1801, 1808, 1801 and 1905; four are also given here under *Roestelia* in this order, 1849, 1887, 1880, 1815.

In the matter of illustrations the present part shows a considerable improvement over the preceding parts. The drawings of the spores show more accuracy in preparation and do not look so diagrammatical. The fact that other structures aside from the teleutospores, such as peridial cells, have had representation in these illustrations is a matter worthy of favorable comment. The printing of the plates on the regular paper makes them somewhat difficult to find. Since not all species are illustrated it is not always possible to tell from the plate and figure number in which direction from the description one should turn to find the illustration. This could be avoided by including the page number of the plate (they all have page numbers although they are not printed upon them) along with the plate and figure number where the reference is given at the end of a description. It is also very difficult to find the description of a figure if one sees an illustration and desires to look it up. Aside from the figure number there might also be given the number of the page where the description occurs. These items would increase the amount of labor in preparation, but would enhance the value of the work sufficiently to warrant it.

The authors are to be praised for the great amount of valuable work they are doing with this difficult group of fungi, and mycologists in general must be exceedingly glad that the preparation of this large monograph has proceeded so steadily. With the appearance of the present part the larger and more important genera have received treatment. The world-wide treatment of such complex plants must necessarily entail an enormous amount of labor and must necessarily involve the inclusion of forms concerning which first-hand information may be meager. These authors must be commended for the use which they make of the work of other specialists.

That they have drawn freely upon the observations of others is especially apparent in the arrangement of the keys, the form of descriptive accounts, the synonymy, and in the preparation of illustrations in this third volume. A deplorable feature is that the works of other writers and investigators may receive only slight or even no credit for the parts which are adopted by the authors or followed closely, whereas in minor portions, where there may be a difference of opinion they see fit to call attention to them in such a way as often to bring discredit upon the works which are really so largely utilized.

Frank D. Kern
Pennsylvania State College,
State College, Pa.

The Primitive Family as an Educational Agency. By James Arthur Todd. G. P. Putnam's Sons, New York and London, 1913.

"The Primitive Family as an Educational Agency" is frankly a brief against "the family superstition" in education, a brief, let one in turn be frank enough to say, that is hardly needed by the ethnologist and that will not be heeded, I venture to predict, by the sentimentalist.

To him or her Professor Todd has undertaken to show that the past of the family is not all it is supposed to be, that monogamy, for example, is an acquired predilection, that in primitive circles kinship may be an uncertain notion and that the "natural bond" between parent and child is merely a latter day figment.

So sympathetic am I with Professor Todd's main undertaking, the cornering of the sentimentalist, and so much in agreement with his general contention that non-familial agencies may have been or may become much more efficient in education than the family that I am reluctant to criticize his method and regret having to question several of his minor arguments.

As to his method, it may be enough to merely describe it as the method of illustra-