(whom he does not cite). Spencer surely read Lamarck, and so did Wallace. Darwin presumably read all, but did not see fit to divulge which ideas he got from whom.

All the above inferences may well be correct ; but on the other hand they may not be, and proof or disproof will in the nature of things be hard to come by. The writer A may have actually read and copied some ideas from the publication of B ; or A may have merely heard about these ideas from a third party, C, who may or may not have mentioned B's authorship; again, these ideas may have become lodged in A's brain long after the reading of B or the conversation with C have been forgotten ; and finally, A may have re-invented them independently of direct or indirect influence of B. It is a fact of life that man is conscious of only a part of his thinking processes; it requires effort to trace the sources of one's own political, philosophical, scientific, and other ideas, not to speak of those of anybody else. Finding out whether evolution has occurred, and if so, what brought it about, occupied Darwin understandably more than facilitating the task of the future historians of evolution theories. This does not mean, of course, that ". . . one of the greatest of our figures should not be dissected, at least by one of us ". By all means, let us dissect and study Darwin's work and personality; but in so doing, should we not hold him entitled to the benefit of doubt before we conclude that " Darwin was slippery " ?

THEODOSIUS DOBZHANSKY.

BLAKESLEE: THE GENUS DATURA. By A. G. Avery, S. Satina, and J. Rietsema. New York: Ronald Press. 1959. Pp. xli+289. $8.75.

When Dr A. F. Blakeslee died in 1954 he had devoted forty years largely to the study of *Datura*. Between 1915 and 1943 working at the Carnegie Institution and directing numerous collaborators, he had published 154 papers ; three also had been published by John Belling on the chromosomes of *Datura*. Since 1944, 71 other papers have appeared by various authors. It was, however, the work of Belling between 1920 and 1927 which inspired the whole of the later development. Abnormal " mutant " plants had been discovered by B. T. Avery between 1915 and 1920. Belling found that these were not due to gene mutation but to trisomic and other whole-chromosome variants. In 1921 he classified them and in the same year a haploid *Datura*, and the first haploid flowering plant, was discovered. The other work on the implications of unbalance, structural change and polyploidy followed.

Blakeslee's contribution to the following years was a discriminating judgment, forceful management and unremitting, repetitive, publication. He believed in keeping the work (and publication) entirely in his own hands : he would part with nothing. This policy of a closed shop his successors (headed by Dr H. H. Plough of Amherst College) are to be congratulated on reversing. They offer seeds of the *Datura* stocks to their colleagues throughout the world.

The present book represents an authorised version of the *Datura* work of the kind Dr Blakeslee would have liked. As such it is a period piece; but it is also a record of experimental versatility implemented with prudence and thoroughness. It reveals innumerable interesting details such as the chromosome chimaeras, the feeble hexaploids and octoploids, the new types of chlorophyll, the pollen-transmitted " quercina " virus, the methods of controlling pollen and embryo growth. And it is admirably illustrated and fully referenced.

We can now look back on the work as a whole. What is most striking about it is the deep freeze into which the chromosome work descended when Belling departed. Analysis stopped. Terminology congealed. Humps or frying pans remained. Chiasmata were never admitted. The number of plants examined increased by hundreds and thousands. For the study of trisomics in progenies alone I estimate that 99,243 plants are represented in table 4. Here are enumerated primary and secondary trisomics. But what are these secondary trisomics ? And how do they arise ? Not a word is said.

Yet if we examine the data we find them concealing a variety of novel and instructive indications. Following is an extract from table 4 (p. 96):

Trisomic parent	Selfed progeny : trisomics			
	Parental extra chromosome		Other extra chromosome	
	$2n+1$	$2n+iso$	$2n+1$	$2n+iso$
19·20 . . .	151	7	102	4
1·2 . . .	421	6	27	0
Other ten types .	6169	7	145	4
	6941	20	274	8

We see that when the 19·20 chromosome (and to a less extent the 1·2 chromosome) is the extra chromosome of the parent, it produces a higher frequency of trisomics in respect of *other* chromosomes than do the other types. Why ? This is what we expect if the 19·20 trisomic has its chromosome pairing upset, *i.e.* has fewer chiasmata and more univalents for all chromosome types. But we know that iso-chromosomes arise in other plants when univalents appear at meiosis. In fact 19·20 and 1·2 yield a proportionately higher frequency of " secondary trisomics " in their progenies also. This removes any doubt there could be that " secondary trisomics " arise by the formation of iso-chromosomes.

Thus this book on *Datura* might well prove a rich quarry of information for those who are inclined to excavate it. But perhaps none of those who are so inclined will know how to translate into modern speech the vivid phrases in which the Jimson Weed was first explained to the world.

C. D. DARLINGTON.

DREISSIG JAHRE ZÜCHTUNGSFORSCHÜNG. By Professor Dr W. Rudorf. Stuttgart: Fischer Verlag. 1959. Pp. 241. DM. 27.

Erwin Baur was the organiser of plant breeding and in some respects the organiser of genetics in Germany. Twenty-five years ago on 2nd December 1933 he met his death prematurely at the age of 58. The aim of this book is to commemorate his work by recording how it happened and what has sprung from it at the present day. Baur established the first great