somewhat during the period of the duration of the war, because of the large numbers of biological chemists and physicians who have already entered, and who will enter the government service. However, we may look for a return to the highest point of the curve as soon as normal conditions exist again. It would seem that one of the lessons we must learn from this terrible conflict is the national value of scientific research.

TABLE V

Institutions which Have Published Ten or More Papers in Any One Periodical

	Am. J. Physiol.	*J. Biol. Chem.*	*J. Exp. Med.*	*J. Pharm.*
Armour and Co.		10		
California	13	89	12	
Carnegie Nutrition	17	10		1
Exptl. Evolution	7	5		
Chicago	70	40	13	11
Columbia	21	23	30	6
Conn. Agr. Expt. Station	30	26		
Cornell Medical	25	46	3	7
Harvard	38	95	10	9
Health Dept., N. Y. City		3	10	
Herter Lab.		77		
Illinois	12	20	10	
Johns Hopkins	34	39	37	51
Mass. Gen. Hospital	1	16	1	7
Michigan	2	3	2	10
Missouri	4	14	2	3
N. Y. Agr. Expt. Station		35		
N. Y. Post Graduate		14		
Montefiore Home	2	13	1	
Northwestern	10	3		13
Pennsylvania	8	53	20	10
Rockefeller	16	135	114	8
Roosevelt		13		
U. S. Fisheries	5	19		
U. S. Hygienic Lab.		7		10
Vanderbilt	2		12	
Washington Univ.	6	18		6
Western Reserve	24	15	15	37
Wisconsin	14	57	7	9
Yale	35	117	11	8

Table V. contains a list of those institutions which have published ten or more papers (biological) in any one of the four periodicals tabulated, with the number of papers published in the others. (Lack of time prevented a complete classification of all the periodicals). Table VI. classifies the articles in these four periodicals according to the scheme used in *Chemical Abstracts*.

These tables and figures, incomplete as they are, give us an idea of the large amount of biological material which is being published yearly. As Professor Vaughan remarks about medical literature, some is good, some is bad, and much of it is indifferent.[4] While the direct responsibility for the quality of the work published depends upon the investigator himself, some of this responsibility must be laid upon the teachers of biochemistry of this country. This responsibility may even be carried back still farther, as Dr. Hammett[5] has recently pointed out, and may in part be placed on the shoulders of the teachers of the fundamentals of chemistry. It is a hopeful sign, in view of this responsibility, to see so large a number of the leaders of biological chemistry gathered in this conference for the improvement of its teaching. May the inspiration which is gathered here send us back to our desks and our laboratories with the determination to do our best the coming year to build deep and strong the foundations upon which the future biological publications will be based.

TABLE VI

Classification of Articles

	Am. J. Physiol.	*J. Biol. Chem.*	*J. Exp. Med.*	*J. Pharm.*
Organic	34	213	2	4
General Biology	58	196	2	1
Methods	16	180	8	14
Botany	0	17	1	4
Bacteriology	257	121	61	2
Physiology	29	171	69	62
Metabolism	63	265	24	2
Pharmacology	40	6	33	144
Pathology	21	38	166	17
Zoology	3	10	2	3

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THE AGE AND AREA HYPOTHESIS

Professors Sinnott and Berry[1] express themselves unfavorably to my hypothesis of "age and area," which Professor de Vries

[4] Editorial, *J. Lab. and Clin. Med.*, 1915–16, 1, 59.

[5] Hammett, F. S., Science, 1917, 46, 504 (Nov. 23); *Medical Record*, 1916, 90, 503 (Sept. 16).

[1] Science, N. S., Vol. 46, p. 457, November 9, 1917; p. 539, November 30, 1917.

has done me the honor to endorse in two reviews[2] in the same journal. But I may perhaps be permitted to point out that, though they object to the hypothesis, they put nothing in its place, and make no effort to explain the figures upon which it is based, and which are so clear and so consistent that if age and area be abandoned, or a rival hypothesis be set up, they can not be left without an explanation, The hypothesis was originally based upon the results obtained from the flora of Ceylon, where distribution was only estimated, and though there were many irregularities, the figures came out clearly enough for it to be fairly safe to publish the hypothesis. Since then, actual measurements of area occupied have been used—for New Zealand, the islands round New Zealand, Jamaica, Hawaii and Australia —and have given results which are far more clear and decisive, following in the most extraordinary manner what is required under the hypothesis. Its applicability has also been extended from the angiosperms to the Coniferæ and ferns.

Professors Sinnott and Berry, it seems to me, have to some extent misapprehended my views. They certainly give them too wide an application, and do not take sufficient notice of the provisos with which I have hedged round the use of age and area. For example, as I have several times pointed out, age and area must not be applied to single species (as Professor Sinnott applies it), but only to groups of 20 allied forms. In other words, it is primarily a proposition in taxonomic plant geography; and though I think that its application may probably be extended, I have as yet only applied it to groups of allied species within a single country, and to genera (the closest of groups) beyond.

I believe that *in general,* when there is a genus covering the range of its family, that genus is the parent of the rest of the family. But of course not infrequently there are two or more genera covering the family range, or two or more dividing it between them (*e. g.,* one Old World, one New). In the solution of this problem, with which goes the parallel fact that, as Professor Sinnott points out, genera are often represented in a given country only by endemic species, and the similar fact that species may be only represented by endemic varieties, there lies, I believe, a very large step on the way to modern theory of evolution.

There is no doubt that many species have in past times died out, or been killed out, but that is no proof that the process is going on now, though *man* is no doubt responsible for a great deal. Age and area, however, refers to action under practically unchanged conditions, and the advent of man may completely alter them.

Professor Sinnott so far extends the applicability of age and area as to make it responsible for the deduction that herbs are older than trees. In the first place these are *ecological,* not taxonomic groups, and the fact that they are undoubtedly very polyphyletic complicates the matter; and in the second place I have not said that the rate at which a herb and a tree spread is the same, though both are governed by age and area, and one covering 1,000 square miles bears the same age relationship to one that covers 500, in each case.

The attacks by Professors Sinnott and Berry are upon my first three papers on age and area.[3] Since then I have published[4] three more. The first is controversial, dealing with the inapplicability of natural selection, and with the question of relative age (in one country) of endemic and widely distributed species. I have shown by two crucial cases that the former are the younger. In New Zealand the widely distributed species take no notice of Cook's Strait (between the chief islands), while many endemics are held up there, having evidently arrived too late to get across before the formation of the strait. Again, in the Tristichaceæ and Podostemaceæ, the primitive genera, which resemble ordinary water plants, are spread throughout the tropics, while the extraordinarily dorsiventral forms, which

[2] SCIENCE, N. S., Vol. 43, 1916, p. 785; 45, 1917, p. 641.

[3] *Phil. Trans.,* 1915; *Ann. Bot.,* 1916.

[4] *Ann. Bot.,* 1917.

must be younger, are all local, and are no more numerous. I have shown in another paper[5] that one can not speak of adaptation in these plants, other than the first adaptation which enabled the families to live in running water.

Neither of my critics makes any attempt to explain why under my hypothesis one can make numerous predictions about the composition and distribution of a given flora—predictions which as yet have always proved to be correct on verification of the facts. For example, in the paper on New Zealand, to which they object, I predicted that the number of endemics found in each zone of 100 miles from north to south in New Zealand would show a maximum at some point (sometimes two) and a regular tapering away from this. This proved to be the case for every one of the 91 families and 392 genera—a result hitherto quite unsuspected, and which alone was almost enough to establish age and area in a higher rank than that of a mere hypothesis. In the second of my recent papers mentioned above I predicted that the species which reach the islands outlying round New Zealand (Kermadecs, Chathams, Aucklands) would on the whole be the oldest in New Zealand, and therefore very widespread there. This proved to be the case, not only generally, but in detail, the most widespread being those reaching all three groups of islands, the next those reaching two, then those reaching one, and finally those that were confined to New Zealand, however far they might range in the world in general. In this paper I have likewise shown that the area covered in New Zealand by the species that also reach Australia, etc., goes, not with the area covered in the world in general, but with that covered in the archipelago of which New Zealand is the principal part. This result seems to me to exclude any explanation based upon natural selection.

In the same paper it was predicted that the species endemic to New Zealand and the islands round it would also be very widespread in New Zealand. This proved to be the case; they are even more widespread than the average of the species which range beyond New Zealand to Australia and the rest of the world. This also is a fact that is quite inexplicable by natural selection.

It seems to me that a hypothesis that shows itself thus capable of being used as a basis for successful prediction deserves at least a very careful investigation before being rejected. One of the best proofs of its general applicability is the fact that other workers are beginning to apply it to the solution of various problems. Breakwell has already worked out the distribution of the grasses of Australia, and shown that it agrees with age and area. Small, in work now being published on the Compositæ,[6] has found it to confirm the evidence of other lines of work in phylogeny, and a third worker is applying it to the Leguminosæ.

In the last of the three papers mentioned above I have shown that the orchids of Jamaica are least widespread in that island when endemic to it, more widespread when also found in Cuba, and most widespread of all when ranging yet further than Cuba. It is also shown that the flora of the Hawaiian Islands fits the age and area hypothesis, as does the distribution of *Callitris* (Coniferæ) in Australia. The ferns of New Zealand and Hawaii are then considered, and shown to obey the same law, while confirming previous work on the greater youth of endemic species.

Finally, in two further papers, as yet unpublished, I have applied age and area in more detail. In my first (a reply to the criticisms of Professor Sinnott, who objects that age and area will not explain the flora of New Zealand) I have shown that New Zealand was peopled with plants in all probability by two chief invasions, one northern from Indomalaya, one southern. In the second paper I have dealt with the flora of Stewart Island (the southernmost of New Zealand) and have successfully made no fewer than 15 predictions about its composition and geographical relations, bringing out a number of points hitherto unsuspected or unnoticed.

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[5] *Proc. Roy. Soc.*, 1914.

[6] See his review in *Science Progress*, January, 1918.