Results. Using the technique described above, 30 human sera obtained from adult patients and normal subjects were tested for the presence of inhibitory substances against *Str. dysgalactiae*. Six bovine and two rabbit sera were tested also. No inhibition was demonstrated by any of the sera. In fact, the organism grew more luxuriantly in the tubes containing 1:2.5 serum dilution than in the tubes containing less or no serum.

TABLE 2

PENICILLIN BLOOD LEVELS OF PATIENTS RECEIVING PENICILLIN INTRAMUSCULARLY

(Test organism: *Streptococcus dysgalactiae*)

Patient	Dosage	Administration intervals	Blood levels		
			1 hr.	2 hr.	3 hr.
	Penicillin units	*Hr.*	*Penicillin units/ml. serum*		
1	20,000	3	0.25	0.06	0.016
2	30,000	3	0.13	0.03	0.016
3	40,000	3	0.13	0.016	
4	20,000	4	0.03	0	0
5	20,000	3	0.06	0.016	0
6	30,000	3	0.13	0.06	0.016
6	30,000	2	4.0*	2.0	
7	30,000	3	0.06	0	0
7	30,000	2	1.0	0.25	
8	30,000	2	0.25	0.25	

* This patient developed uremia with urinary retention.

This procedure was also applied in eight human hospitalized cases undergoing penicillin therapy. The results of the penicillin blood levels are shown in Table 2. Although the number of cases reported in this series is small, it will be seen from these results that, in the cases reported here, the penicillin blood levels are not therapeutically adequate when 20,000 units are given intramuscularly at three-hour intervals.

Summary. A strain of *Str. dysgalactiae* was found to be an effective test organism for penicillin blood-level determinations. This organism, although inhibited by penicillin in concentrations of 0.006–0.008 unit/ml., is resistant to the natural inhibiting substances of blood sera. The latter characteristic is very significant, since the test organisms (*Str. pyogenes* C-203 and *B. subtilis*) that are now employed for penicillin assay of body fluids are inhibited by a large percentage of human sera.

The method described can detect penicillin blood levels in concentration of 0.016 unit/ml. of blood sera. Since blood levels above 0.03 unit/ml. are considered to be therapeutically effective, this method of assay is adequately sensitive for routine clinical application.

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Letters to the Editor

Taxonomy and the Biologists

Carleton R. Ball's recent communication (*Science*, 1946, **103**, 713) states the grievances of the nontaxonomists so cleverly that they are apt to be accepted at full face value. These statements, however, are only partially valid. Systematists of today are not primarily interested in describing new species, or in erecting new names to replace old ones merely for the purpose of having their names attached to these supposedly new forms. Their primary motive is a sincere desire to place before the other workers in biology as full and complete a record of the forms living in the world as is possible with our present support and opportunities.

That he is doing as good a job in his field as the workers in any other field is a challenge that must stand until someone produces reliable statistics to the contrary. The mere listing of the mistakes made by the taxonomists will not override the challenge, because mistakes are made in all fields. The chief difference is that the taxonomist is the only worker who embalms his mistakes and erects them like totem poles along the highway, so that each succeeding generation of taxonomists must do obeisance as they pass by. Unfortunately no one has proposed a real remedy for this burdensome process.

Changes in generic names are due chiefly to five things: First is the fact that some earlier systematist has described the genus under another name. With the present survey of literature nearly complete, this cause for name-changing is almost a thing of the past. Second is the discovery that two authors have used the same name for two different organisms. With the recent publication of Neave's *Nomenclator Zoologicus*, most of the previous duplications can be cleared up. The number of duplications in the future should be small with such world-wide reviews of current literature as are now being published. The third cause of confusion is due to present and past methods of type selection. However, with strict enforcement of a rule which would prevent publication of new generic names without clear type designation, such confusion should be reduced to a minimum. Fourth is the division of a genus into two or more genera. This process has continued since the beginning

of binomial nomenclature, and we are apparently as far from the end of it as we are from its beginning. The fifth cause of confusion is due to misidentification of old genera and misinterpretation of previous descriptions. Changes in specific names are caused by all these mistakes except the third.

No one seems to have a very clear conception of the enormous number of species of animals living in the world today. An actual count of the number of genera and species of Homoptera in the card catalogue of this order of insects in my laboratory shows that there are approximately 3,100 genera and 30,000 species recorded. Perhaps from these counts of the number of species of Homoptera we may be able to get a real estimate of the number of species of animals that have been described. From various counts and estimates, I believe that the Homoptera represent from 1/100 to 1/150 of the Animal Kingdom. This would give us an estimated total of 2,500,000 species of animals, already described, of which 1,500,000 are insects.

There is, of course, no such thing as stable nomenclature—certainly not until the last organism is fully described, illustrated, and catalogued. The discovery of any new species or new genus is apt to upset all our present notions about phylogeny or evolutionary principles. How poor our present knowledge is of even fairly well-known groups needs no demonstration. Certainly stable nomenclature is a will-o'-the-wisp, no more to be desired than a stable chemistry or physics, embryology or morphology. Anyone who thinks that we must still continue in nomenclature on the basis of the names that he learned 40 years ago is thoroughly unscientific.

My earnest plea is for support for taxonomy from all biologists—not alone for financial support but also for a sympathetic understanding of its problems, limitations, and mistakes, and above all for a realization that all taxonomists are making a sincere effort to advance their branch of biology for the assistance of all biologists.

A colleague recently called my attention to this advice by Mephisto to the Student:

> Gebraucht der Zeit, sie geht so schnell von hinnen,
> Doch Ordnung lehrt Euch Zeit gewinnen.

Freely translated so that he who runs may read:

> Time flies so swiftly bye, use it,
> Only systematics can teach you, do not abuse it.

Z. P. METCALF

State College of Agriculture and Engineering
University of North Carolina, Raleigh

Dormant and Adventitious Buds

An attempt is made here to distinguish more precisely between dormant and adventitious buds. It is probable, however, that an accurate nomenclature on the entire subject of buds will be possible only in the distant future.

Stone and Stone (*Science*, 1943, 98, 62) state: "It would be profitable to restrict the use of the term *dormant* or *latent* to buds formed in the axils of leaves (including scales) on the young annual shoots" and adventitious buds to those that "arise outside the normal phyllotaxy." They mention, however, that "adventitious buds, once formed, may also remain dormant."

I propose to classify buds as trace and adventitious buds. The concept of the dormant bud as a structure with a trace to the pith, and the capacity to remain dormant, is not valid, because buds in roots become dormant.

The trace bud has a trace to the pith and develops in the elongating region of the shoot. Primary trace buds develop in the axils of leaves. Secondary trace buds arise in axils of scales of other trace buds. A primary trace bud can become the ancestor of many secondary trace buds, with its trace branching and extending to them.

An adventitious bud lacks a trace to the pith and can appear wherever elongation has ceased. Adventitious buds can be found in roots, shoots, leaves, hypocotyls, epicotyls, and callus. They also develop in axils of scales of other adventitious buds and are connected by branching bud traces. Adventitious buds can be mistaken for trace buds when the traces begin near the pith.

Any bud can develop into a shoot, either immediately or after a quiescent period. With the terms dormant and quiescent having the same meaning, quiescent adventitious buds are dormant buds, and the distinctions between them disappear. There are merely trace and adventitious buds.

These distinctions, now made, are hereby projected into tree culture. Root suckers come only from adventitious buds. Sprouts arise from both trace and adventitious buds. Coppice consists of trees whose boles come from trace and adventitious buds and arise as sprouts and root suckers. It is not to be expected that all shoots from a stump develop from buds of the same kind and that their traces begin in tissues of the same age. If all buds on a stump are trace buds, no shoots arise below the root collar.

ISADOR AARON

Laboratory, P. J. Oesterling & Son Company, Inc.
Butler, Pennsylvania

Aseptic Cultivation of Excised Plant Embryos

The cultivation *in vitro* of excised embryos of seed plants presents certain practical difficulties, some of which have been only partially overcome. One of these difficulties has to do with the growing of embryos in solid medium in such a way as to prevent contamination, at the same time reducing to a minimum the rate of drying of the medium and the number of transfers required to maintain the cultures in a healthy condition.

We are using an extremely simple device to accomplish these ends. When embryos of *Oenothera* are large enough to be transferred from liquid to solid medium, they are placed in shell vials (70×21 mm.) containing a suitable amount of medium, and another sterilized shell vial (60×25 mm.) is inverted over the first vial, thus serving as a lid. No cotton plug or other material is used. Sufficient gas exchange to maintain health is permitted between container and lid, since the edges and bottoms of the vials are not absolutely flat. The container and its lid fit tightly enough together, however, to reduce evaporation to a minimum, thus allowing transfers to be maintained for long periods. In no case have we transferred oftener than once a month, and in some cases the interval has been as long as three months. Cul-