This is no visionary, unpractical scheme. It has been realized, in one form or another, by most European states. The idea is slowly evolving in connection with our own government departments. The state department has in training a body of consular clerks. The navy details men for special study in Greenwich, Paris, and Baltimore. The war department has also allowed men to study in Baltimore laboratories. Mr. Trenholm, the comptroller of the currency, says he is going to select the brightest young men he can find, and train them for bank-examiners. The idea is in the air at Washington, and it will sooner or later find a lodgement in every department and bureau. You will probably hear of it next week from Col. Carroll D. Wright, commissioner of the bureau of labor, in his address on the study of statistics in American colleges, before the American economic association, at its meeting in Cambridge, May 24, 1887. Statistical science, finance, forestry, agrarian economy, consular duties, and diplomacy have never yet been taught, to any considerable extent, in our American schools and colleges. You might as well expect a corps of military engineers to evolve from the state militia as to suppose that the higher arts of administration can be acquired by either school or college training. Administration is one of the highest branches of scientific politics, and it seems to me that *Science* ought to recognize the fact. As to the diplomatic service, a Boston gentleman, who has had much experience in this connection, writes, "I have had a good deal to do with some of our diplomatic servants in Europe, and have often been put to the blush for their incompetency to perform their duties. Why should we not have a diplomatic service like other nations, and why should we not have a national institution in which the students should be taught, among other things, diplomacy?"

HERBERT B. ADAMS.

Johns Hopkins univ., May 16.

The occurrence of similar inventions in areas widely apart.

The leading idea of Otis T. Mason's writings on ethnology is his attempt to classify human inventions and other ethnological phenomena in the light of biological specimens. "They may be divided into families, genera, and species. They may be studied in their several ontogenies (that is, we may watch the unfolding of each individual thing from its raw material to its finished production). They may be regarded as the products of specific evolution out of natural objects serving human wants and up to the most delicate machine performing the same function. They may be modified by their relationship, one to another, in sets, outfits, apparatus, just as the insect and flower are co-ordinately transformed. They observe the law of change under environment and geographical distribution." This method of research is founded on the hypothesis that a connection of some kind exists between ethnological phenomena of people widely apart. Professor Mason is of this opinion, and expresses it as follows: "Anthropologists assign similar inventions observed in different parts of the world to one of the following causes: 1. The migration of a certain race of people who made the invention. 2. The migration of ideas — that is, an invention may be made by a certain race or people and taught or loaned to peoples far removed in time and place. 3. In human culture, as in nature elsewhere, like causes produce like effects. Under the same stress and resources the same inventions will arise." From this stand-point Professor Mason has arranged the ethnological collections of the national museum according to objects, not according to the tribes to whom they belong, in order to show the different species of throwing-sticks, basketry, bows, etc.

We cannot agree with the leading principles of Professor Mason's ethnological researches. In his enumeration of causes of similar inventions, one is omitted, which overthrows the whole system: unlike causes produce like effects. It is of very rare occurrence that the existence of like causes for similar inventions can be proved, as the elements affecting the human mind are so complicated; and their influence is so utterly unknown, that an attempt to find like causes must fail, or will be a vague hypothesis. On the contrary, the development of similar ethnological phenomena from unlike causes is far more probable, and due to the intricacy of the acting causes. As far as inventions are concerned, the disposition of men to act suitably is the only general cause; but this is so general, that it cannot be made the foundation of a system of inventions.

But from still another point of view we cannot consider Professor Mason's method a progress of ethnological researches. In regarding the ethnological phenomenon as a biological specimen, and trying to classify it, he introduces the rigid abstractions species, genus, and family into ethnology, the true meaning of which it took so long to understand. It is only since the development of the evolutional theory that it became clear that the object of study is the individual, not abstractions from the individual under observation. We have to study each ethnological specimen individually in its history and in its medium, and this is the important meaning of the 'geographical province' which is so frequently emphasized by A. Bastian. By regarding a single implement outside of its surroundings, outside of other inventions of the people to whom it belongs, and outside of other phenomena affecting that people and its productions, we cannot understand its meaning. The only fact that a collection of implements used for the same purpose, or made of the same material, teaches, is, that man in different parts of the earth has made similar inventions, while, on the other hand, a collection representing the life of one tribe enables us to understand the single specimen far better. Our objection to Mason's idea is, that classification is not explanation.

His method, as far as applied to objects which have a close connection with each other, is very good. The collection of moon-shaped Eskimo knives or labrets from North-west America has given us great pleasure, and enables us to trace the distribution of those implements; but even they do not fully answer the purpose of ethnological collections. Besides these, we want a collection arranged according to tribes, in order to teach the peculiar style of each group. The art and characteristic style of a people can be understood only by studying its productions as a whole. In the collections of the national museum the marked character of the North-west American tribes is almost lost, because the objects are scattered in different parts of the building, and are exhibited among those from other tribes.

Another instance will show that the arrangement

of similar implements does not serve the purpose of ethnological collections. From a collection of string instruments, flutes, or drums of 'savage' tribes and the modern orchestra, we cannot derive any conclusion but that similar means have been applied by all peoples to make music. The character of their music, the only object worth studying, which determines the form of the instruments, cannot be understood from the single instrument, but requires a complete collection of the single tribe. Here, however, it can be seen that each ethnological collection affords only very fragmentary instruction; that its real use is only to illustrate descriptions of the tribes. For a study of native art and its development, they are indispensable. For this purpose, duplicates, of which the superficial visitor of ethnological museums frequently complains, are absolutely necessary. They are the only means of determining what is characteristic of a tribe, and what is merely incidental.

Mason's method takes a place in ethnology similar to the former 'comparing method' in geography. A mere comparison of forms cannot lead to useful results, though it may be a successful method of *finding* problems that will further the progress of science. The thorough study must refer to the history and development of the individual form, and hence proceed to more general phenomena.

DR. FRANZ BOAS.

New York, May 13.

Explosions in coal-mines.

In *Science* for May 6, is a review of the report of the Atkinsons on explosions in coal-mines. One or two statements therein seem to convey an erroneous impression; notably, "At the working faces the dust is not often a serious evil," and, under remedial measures, that "watering the roadways . . . is of little avail as a means of preventing explosions, since the upper dust in every instance is left undisturbed."

The first quotation is manifestly an error, as dust-explosions can generally be traced to the firing of the dust in the working faces by blown-out shots, especially when such shots react against a tamping of coal-slack. In the main body of the article the argument seems to be in favor of the dust in the gangways as the proximate cause of explosion, while it is but the ultimate cause. The dust formed by cutting or breaking down coal has very little to do with the formation of an explosive mixture, because it is not impalpable enough. As the article states, the gangway dust is ground to an impalpable powder, and carried away by the air; but such dust would do little harm, did the return currents through the working places not lose their velocity and deposit this impalpable dust on the walls nearest the face, from the fact that the ventilating currents must sweep the faces free from smoke and foul air. In the only known American accident due to dust (the Pocahontas explosion), there was little evidence of initial explosive force along the gangways, but in the headings of dusty rooms there occurred a series of explosions that made the fact evident that fine dust in headings is the cause of so many accidents.

The Prussian commission showed that a certain percentage of volatile matter was necessary for an explosive dust, and experiments made at this place show that the temperature at which coals give up their volatile ingredients vary; so that a blown-out shot, from a hole tamped with coal slack, projected into a chamber whose walls are thickly powdered with fine dust, will have its flame prolonged by the dust of the tamping and the gases from that dust, and the case will be analogous to those stated by Professor Abel, where a small admixture of gas would render inert dusts explosive.

Finally, it has been found that watering dusty roads with brine at intervals of thirty days made the dusts less ready to rise in clouds, and stopped the formation of 'upper dust.' But the best way of all is to keep the roads clean by ballasting them at the outset with rock or shale free from carbon, and by picking off the coal shaken from cars. In the Pocahontas accident the explosive phenomena ceased as soon as the current left the region where the roads were ballasted with coal-slack, and the action in the parts ballasted with slate was a simple burning of the dust brought there by the current. In spite of an explosive coal, the parts of that mine last mentioned were comparatively free from dust, and the props nearly all standing, while in the former portion there was explosive action in nearly every working place, so that the tracks were torn to pieces and the props down. In mines of this nature, black powder should be avoided, and the coal should be wedged, or, if the coal must be shattered, the dynamite cartridge with water casing can be used with impunity.

EDWARD H. WILLIAMS, JR.

Lehigh univ., May 13.

Water-filtration.

It may be of interest to notice in connection with your note on the results of Dr. Swarts's experiments on the relation of water-filtration to bacterial development, that Dr. J. H. M. Munro, in his experiments on the nitrification of well-waters, discovered and called attention to the fact that a well-water nitrified more rapidly after filtration through a Lipscombe's charcoal filter in common use, than did an unfiltered sample of the same water (*Journ. chem. soc.*, 1886, p. 666).

WILLIAM FREAR.

State college, Penn., May 16.

The fact that an increase of micro-organisms would take place in a filter constantly in use, had already been demonstrated by Percy Frankland, England, and by several German investigators.

The series of experiments conducted by myself differed from theirs merely in using the filters found upon the local market, and in imitating as near as possible their use in ordinary family water-supply; cleansing in the first use by allowing the supply to pass through the filter to wash away detritus, and not by absolute sterilization, as in usual laboratory experiments. The filters used were variously packed with bone or animal charcoal, quartz, the two combined in layers, felt, and unglazed porcelain.

Such a result as that spoken of in *Science* (ix. p. 457) is to be expected, when we consider the mechanical work we have to do in filtration; for no amount of chemical change is expected except in Clarke's process, which is precipitation, not filtration. If the filter is a successful strainer, the suspended matters within the water are held back upon the surface of the strainer *and within* the interstices of the filtering media, whether it be gauze, asbestos, iron shavings, felt, or porcelain. The great mistake seems to be in believing, that by use of a current of water, or by removing the media and scrubbing the surface with a brush, all the filtrate is removed, for-