and fifty-eight, three-lobed; and seventy-nine, ' mitten form.'

The first leaves of spring were invariably entire, and a lobed leaf was rarely found until the fourth leaf was passed in counting from the base of the branch toward the tip. No regular order was discovered. In one case the arrangement was as follows: three entire, four three-lobed, one ' mitten,' one three-lobed, one ' mitten,' one three-lobed, one ' mitten,' one three-lobed; on another branch, four entire, one ' mitten,' five three-lobed, one ' mitten,' three three-lobed, three ' mittens.' The leaves on short spurs of old trees were nearly all small and entire; when the branches were somewhat longer, and the leaves larger, there were one or more three-lobed or ' mitten' leaves in the middle of the stem. A number of branches taken from slow-growing trees gave the following aggregate: entire leaves, seventy; ' mittens,' six; three-lobed leaves, three. A vigorous young sprout gave twenty-seven three-lobed leaves, one ' mitten ' near the middle of the stem, and no entire leaves. Another had two entire blades at the base, and twelve three-lobed leaves above. A number of these rapidly-growing young trees together gave twenty-seven entire leaves, fourteen ' mittens,' and eighty-one three-lobed leaves.

The entire and smaller leaves are in the majority on slowly-growing trees; while, on the young sprouts, larger three-lobed leaves predominate. The ' mitten ' form is mostly found with the entire leaves. This form of leaf is probably about equally divided between the ' right-handed ' and ' left-handed; ' though, of the number found (seventy-nine), those with the ' thumb ' to the left, when held with under side upward, exceeded the other sort by half. About every thirteenth leaf is a ' mitten,' — a form not found mentioned in the botanical description of the sassafras.

There seems to be no order in the arrangement of the three forms upon the branch. Leaves from the buds were examined, and all of the three forms were found. Each kind is distinct, from a very early state; and there is no indication that one ever passes into the other. No intermediate forms have been found. The venation of the three forms is very much the same. There is a midrib running lengthwise through the leaf, and a strong lateral vein on each side, which runs from near the base to beyond the middle of the leaf. Smaller veins form the framework of the middle and upper parts of the leaf. The portion of parenchyma absent in a lobed leaf is midway between the strong lateral veins. This is very clearly shown in a ' mitten,' where one side is lobed, and the other entire. It would seem as if the lobing is a failure to fill up the framework, and apparently due to a too vigorous growth of the veins, and a lack of a sufficient amount of the soft, filling tissue. In the formation of leaves the sassafras is certainly ' at loose ends,' but in this it is not alone.

Fig. 1 shows an entire sassafras-leaf; fig. 2, a three-lobed leaf; and fig. 3, a ' mitten.' Fig. 4 shows the young leaves of the three forms. All the illustrations are drawn from nature. BYRON D. HALSTED.

New York, July 2, 1883.

THE UNITS OF MASS AND FORCE.

IN the original definition of the gram it was regarded as a weight, and therefore a force, being the weight at the level of the sea, and at the latitude of 45°, of one cubic centimetre of water at its maximum density. It was thus virtually defined as a force. But as we shall soon see, although defined as a unit of force, it has become in practice a unit of mass. In the C. G. S. system of units this change is accepted, and the definition is modified accordingly; that is, one cubic centimetre of water is taken as the unit of mass, and this mass is called the gram without reference to its weight.

In volume i., *Cours de physique*, M. Jamin criticises this change. The high standing and character of this great work, as well as the eminence of its author, entitle his views to respectful consideration, especially as the question involves the fundamental elementary conceptions of physics in a way to render it of interest to the general student.

We set out with the proposition that what we commonly consider units of weight, such as the *kilogram* and *pound*, practically become units of mass in all the ordinary affairs of life. The reason is, that in practice bodies are weighed by balancing them against pieces of metal, and not by means of a spring balance. A pound weight is indeed heavier the farther north we go; but then, whatever we weigh with it is heavier in the same ratio. Accordingly, if by means of a weight we weigh a pound of tea at the equator, at the poles it will still weigh the same as a pound weight, although in reality heavier than at the equator. This is obviously a great practical and commercial convenience; because the quantity or mass of the tea is the important question to those who deal in it, while its gravitating force is of secondary importance. Were a perfect spring balance used which measured absolute weight, the dealer who should purchase tea at one latitude, and sell it at another, would be subject to a gain or loss, depending upon the difference in the force of gravity.

It is not, however, on merely commercial grounds that the change rests. For scientific purposes a unit is used as a term of comparison between different quantities of the same kind, and must be so defined and chosen as to fulfil this function with the greatest convenience. Now, a unit of force which shall furnish a direct and convenient standard of comparison between forces or weights at different places is entirely impracticable. At any one place the weight of a given mass of metal may be taken as a convenient unit; but this unit will change when we go to any other place, owing to the difference in the force of gravity. Indeed, every student of physics knows that the measure of the force of gravity at any one place is one of the most delicate and difficult problems in physics. In the definition which refers to the latitude of 45° it is assumed that the force of gravity is the same at all points on this parallel. We now know that this is not the case, and that if we adopt such a unit we shall have to define the exact spot on the earth's surface which is taken as the standard. Reference to such a standard would be impracticable. Hence a unit of force must be subsidiary to the unit of mass. The most convenient way of fixing it is to take the unit of mass as known, and to determine the force of gravity at the place of observation. The combination of the two gives a standard by which weight may be expressed in force. To be more explicit: if we have a piece of metal the mass of which we know to be one gram, and if we determine the force of gravity at the place to be n, then the gravitating force of that piece of metal will be known to be n units of force. In practice this must be the method used in physics, if an accurate measure of forces is really required.

Let us now consider M. Jamin's objections. He says that the mass of a body is not susceptible of direct determination; for to measure it we must commence by determining its weight in a balance, and afterwards dividing by the number which expresses the acceleration of gravity at the latitude of 45° and at the level of the sea. It is difficult to attribute this remark to any thing but inadvertence, since the division by g at 45° is necessary only on the French system. If we measure it by means of a balance having grams as weights, the resulting weight is at once the mass on the C. G. S. system, no matter where the weighing is made, and therefore needs no division whatever.

He then adds, "Suppose, on the contrary, that we have to measure a force: we determine it directly by means of weights at the place of observation. Afterwards we apply to these weights the corrections relative to the latitude and the altitude, to have an expression of the force as the function of a normal gram. We must remark that we cannot avoid these corrections to taking mass as the fundamental unit; because it is always weights that we measure, and the course followed in the experiments is necessitated by the nature of things." This is quite true, but it does not prove that one system affords any more convenient unit of force than the other.

Simon Newcomb.

STANDARD RAILWAY TIME.

The problem of simplifying the system of time standards used by the railways of this country seems to be near solution. The representatives of various railway-lines, who are to-day in session at Chicago, will receive the report of the secretary, Mr. W. F. Allen, and, it is expected, will take final action. For some years past, committees of various scientific bodies, as the American metrological society, the American association for the advancement of science, and the American society of civil engineers, have called attention to the urgent need of reform in the standards of time in use, and suggested plans for action. The railways, which are naturally most interested in the movement, have recently taken hold of the matter in earnest. The plan which has met with the most favor is that in which five standards of time, differing by consecutive hours, are proposed for the whole territory occupied by the United States and Canada. These are based upon the meridians from Greenwich, but receive other names for purposes of convenience. It is proposed by the railways that in Canada the standard shall be known as *intercolonial time*, and shall coincide with the local time on the meridian four hours, or 60°, west of Greenwich. In the United States the standards will be known as *eastern*, *central*, *mountain*, and *Pacific time*, and coincide with the local times on the meridians five, six, seven, and eight hours, or 75°, 90°, 105°, 120°, respectively, west of Greenwich. The advantage of this system is, that the standards will differ from the true local times of the various parts of the country by amounts not greater than thirty minutes, if the divisions are made rigidly according to longitude, and no one will be inconvenienced