London Pharmacopœdia of 1783 under the name of Pulvis Antimonialis. It was accordingly used by apothecaries as a succedaneum on account of the high price of the real James's powder; but it never obtained the confidence of practitioners; and hence the origin of the adjunct used in prescriptions, *verus*. Indeed, it never deserved their confidence, being, as directed in the Pharmacopœia, an almost inert substance.

Dr. Pearson informs us that all the parcels of James's powder, that he had seen would be called white powders, but no two of them were white in the same degree; they had either "a shade of yellow, or stone colour, and none were perfectly white, or so white as some specimens of Pulvis Antimonialis of the shops. Some parcels had a brassy taste, others no taste. Dr. Pearson having formed a powder from bone-ashes and crude sulphuret of antimony possessed of properties similar in kind to every one of those ascertained to belong to James powder, with scarcely any difference in the degree of them, considered that they were the same. Beside this synthetic proof, he adduced the evidence of analysis, and made experiments in proof before competent judges." He says, "it is very probable that no degree or duration of fire applied in open or close vessels alone can produce a calx of the same kind as that in James's powder, nor, perhaps can such a powder be composed by fire applied in close vessels to calx of antimony mixed with calcined bone; but if calx of antimony, duly calcined, be mixed with calcined bone, and exposed to air, in a due degree of fire, for a sufficient length of time, and then in a still greater degree of fire be applied to it in close vessels, such a compound may be formed as James's powder. . . . No such white powder is formed by a mixture of any calx of antimony and bone ashes, exposed to any degree of fire in close vessels, without previous exposure to fire and air."

Pearson concludes from all his experiments that James's powder consists of phosphate of lime and a peculiar calx of antimony, different from all others, composing a triple compound in the proportion of about 57 parts of calx of antimony and 43 of phosphate of of lime, or a double compound of the same elements.

The admitted medical efficacy and the high price of James's powder induced the various colleges of physicians to introduce into their pharmacopœias a process for imitating it. They took for their guide the investigations of Pearson, and dictated formulæ which apparently did not much differ from the prescription of that accomplished physician. This preparation called Pulvis Antimonialis, proved an utter failure, having neither the composition nor the medical effects of the powder of James. In the manipulation of the manufacturers, the chief object seemed to be the production of a powder as white as snow,—the very quality which it ought not to possess if intended to resemble the powder of James, which at that time was always slightly yellow, or cream coloured, or even stone-coloured, as we learn from Pearson.

I made a number of trials of the process of the three British Pharmacopœias (1816), but could not obtain the powder white like the Pulvis Antimonialis of the druggists, or like the James's powder then in use. The roasted materials introduced into the skittle-pot, with another inverted, both luted together, were maintained at a white heat in an air-furnace for two hours. When cold, the included matter was found converted into a dense, close-grained, buff-coloured mass, as hard as limestone and very heavy. Being again heated to whiteness, it became a deep olive-brown, harder than before.

I repeated the process on new materials, heating them similarly in a different air-furnace, and obtaining an olive-brown semivitrified mass with dark streaks, harder than the former mass, a small portion of a white enamel appearing on the side of the skittle-pot.

It was plain, therefore, that the heat was too high, and that the use of the air-furnace originally directed by Pearson, and adopted in all the pharmacopœias, was an error. I, therefore, repeated the process, and placed the skittle-pot containing the powder in a common fire grate, heaping coal round and and over it. In due time the skittle-pot became red-hot, and in this state was kept for an hour and a half or two hours. When cold, it was found to be a snow-white powder, covered by a congeries of crystals a quarter of an inch thick. Thus one important fact was acertained.

On repeating this method several times, and using an iron ladle in a common coal fire, the resulting powder, instead of being uniformly white, proved in some instances to be buff-coloured; but occasionally the snow-white powder was obtained. As the failure was not due to the final heating, it must have originated while the materials were in the iron ladle. Various experiments convinced me that the heating in the ladle is the most important part of the whole process; and at length it became evident that when the heat, accompanied by continued stirring or raking, was maintained until the powder changed from dark brown to a light yellowish-grey, the final heating in a skittle-pot brightened it, or the greater part of it, to a perfect white. The light yellowish-grey colour here mentioned will be best understood by comparing it to the dust of a Bath brick, often used for cleaning dinner knives, but a little paler.

But to heat the powder while in the ladle fully to this colour, but not beyond it, was the difficulty.

During these experiments I perceived that when the quantities of the two ingredients were as large as ten ounces of each, the resulting powder when taken from the skittle-pot never proved white, but generally dark grey, interspersed with a deep yellow-colored portion. The fact pointed to the conclusion that the ladle was too small for that quantity of materials, that due raking during the heating was impeded, and that the desulphuration was accordingly imperfect. A hemispherical ladle capable of holding a gallon being procured, a charge of ten ounces of each was placed on the fire and continually raked for several hours, at first without any intermission, and at length with short intervals of rest, until the proper colour was attained. This matter, being finely powdered was introduced into a proportionately large skittle-pot and exposed to a well-built coal fire in a common grate, and kept red-hot for three hours. When cold, the top portion proved to be a thin cake of dark-coloured matter; under that was a small quantity of yellow portion; and the remainder snow-white.

On trying so large a charge as sixteen ounces of each ingredient in the large ladle it proved to be unmanageable; the carbon at an early period ignited; the mass softened, collected into dark-coloured lumps, which could not be raked notwithstanding much effort. Finding it impracticable, I took out the charge when cold, and being powdered, it was returned into the ladle in four different portions, each of which was separately raked while heating, until the proper colour appeared to be attained. The whole of the powder being charged into a very large skittle-pot, was heated in a well-built and well-supplied fire for several hours. The powder, when cold, was found to be yellow throughout; for the proper proportion between the quantity of matter and the containing iron ladle had not been observed, the necessity of which was thus amply proved. It is a certain fact that a large quantity in a small ladle will never afford a white powder.

By reversing the conditions of the process, that is, by acting with due care on a small quantity of materials in a very large ladle, we are pretty sure of bringing the charge safely through its first stage of danger. Thus when four ounces of hartshorn-shavings and the same weight of sulphuret of antimony were well raked in a ladle of the capacity of a gallon, until the requisite colour was attained, and when heated in the skittle-pot for an hour or more in the usual manner, the powder almost always turned out white, generally snow-white, but sometimes with the cream-coloured tinge noticed by Pearson. Under the condition of small charges in a very large ladle, the snow-white colour was sometimes produced by a very hot fire in fifteen minutes after the skittle-pot had become red-hot but with a fire not so hot, a much longer time was necessary.

After following up these experiments for some time, I found that much trouble and anxious watching would be saved by raking the bone-shavings, without the sulphuret of antimony, until the ammoniacal fumes, the sulphur, and the extremely fetid gases had been expelled; and making proper allowance in subsequently apportioning the antimony.

Adopting this method, six ounces of calcined hartshorn-shavings mixed with four ounces of sulphuret of antimony, were raked over a graduated fire, in my largest ladle, until the powder had assumed the usual yellowish-grey hue. It was then transferred to a small skittle-pot, which, being placed on a stand in a large fire grate, coals were built round and over it, and a cover applied. The skittle-pot was kept red hot for six hours. When cold, it was cautiously examined. No part of the partially cohering powder was white; it was almost all dark grey, but much darker towards the top; the portion at the very top was full of particles of metallic antimony, and even small masses of it which had assumed a somewhat rounded form. The dark grey colour of the whole mass seemed to be caused by intermixture of thousands of minute shining particles of the metal with the phosphate of lime. Round the mouth of the skittle-pot and on its cover was a small accumulation of white powder, some of which was minutely crystallized, and was deposited by the dense white smoke which issued from the skittle-pot every time the cover was removed, and ceased when it was replaced. At the bottom of the skittle-pot was a small quantity of yellow powder. It was remarkable that although many processes had been conducted in this fire-grate in all respects in the same manner, except that the fire had been maintained for two hours only, the pow-