80. If we consider the foundation for such a supposition in the composition of the animal fluid, we can perceive no such thing, much less can we see it in the power of dissolving oil, and yet these are the only circumstances of no phenomena quality. I find this equally a property of every viscid fluid, and of certain days diffused in water. The viscid fluids by their adhesion will give an opportunity of diffusion. Next we see no foundation for it in the composition of animal fluids. Salt and oil are in animal fluids, but it is not without other ingredients. It may be an acid, an alkali, or a neutral soap. There is no certainty that there is either one or other, and indeed there is no probability of either of them. Lastly as 81 to the mixing oil and water perfectly fine is no foundation for this in experiment. After a variety of trials upon calcar mucus, and bile no diffusion is obtained, much less mixture. What union takes place is just in proportion to their viscosity; we shall speak of the more intimate union of oil afterwards.

Dissolution

This is not perfected in the stomach, but long after, in other parts, as it is begun there. Hunter upon this I have said that the aliment of animal bodies is properly vegetable substance. And I have alleged that vegetable substance in its nature and qualities differs
from that of animals. The change that produces the resemblance, we call 
fermentation. How it is brought about, we should begin with explaining this 
difference, and inquire how the change is produced, or leave out of sight the chan-
ges produced, and first consider the means employed in changing, and from these 
determine the effects. Difficulties occur in either way. The last is the more 
secure method, and we therefore prefer it. 

Upon this plan then, I view a fermentation occurs. Here the ferment of the 
Chemists and Botanists, mere true manure, and generally rejected. Opinion un.
common and delit dies. But it is universally agreed that there is a fermentation; it is four.

I'd first upon the nature of the albumin which is disposed to fermentation, I 
advanced that a saccharine matter is 
the chief foundation of our vegetable 
element. Haller is wrong in rejecting 
chegan as one of the condiments. If the 
operations are true, the number of 
men and other animals, that live upon 
fruits of trees, figs and dates, where a 
saccharine matter is found, must certainly 
receive their nourishment from chegan. 

But further the vegetable substances 
which make the chief part of our ele-
ment are the farinacea; now if ch-
gar is the basis of the fermentable 
matter, it is a proof that the matter of 
our element is of the saccharine kind.
Therefore we presume a fermentation of the aliment, so fermentable. Further we say it actually does in all cases ferment on the human stomach. Our aliment suffers diminution, the intestinal motion of it expedites the first air. And from our digesting of vinous or weasel disposition, there is no doubt of fermentation. Now we can say that none of the circumstances preventing fermentation have that effect, and first in this fermentation a degree of heat has not. In ordinary fermentation on the heat must be somewhat less than that of animal bodies. But this that be the case in the fermentation of wine yet we know that its fermentation will go on, in countries of higher degrees of heat, and the sky be inclose vessels, yet if they have a quantity of air included it is sufficient to carry on the fermentation to a certain degree. But there is air in the stomach. I said that air contributed to solution from its making agitation in the place; but it is necessary to expedites the first air. A third circumstance hindering fermentation was the mixture of animal substance, which resists the vinous or weasel. But the experiments of Wingle and Motbrad have shown that they bring on and expedite fermentation. The degree of heat,
The air and mixture of animal matter therein, all help fermentation in the stomach. I maintain that it is by a process of fermentation that our assimilation takes place. From the general composition of animal matter, it has been thought they run on to the protoreactive fermentation. But I say the fermentation is not directly protoreactive, as that fermentation does not directly take place without the other two preceding. The vinous passes to the acetous, and that to the protoreactive. Most chemists admit this, that it can not be determined to the protoreactive fermentation without going thru' the acetous, nor into the acetous.
88 guafo vapors, the gas silvestre. Been have called it the "deorman fermentation." It has all the qualities of mephitical.
We have too many instances of this gas silvestre produced in the stomach co-homoeous to animal life. But the mixture of animal fluid modifies the vinous fermentation so that the gas silvestre is prevalent. I was here then that either the vinous or acelous fermentation constantly takes place. A question remains, to what length the vinous proceeds, if constantly to the acelous?

--- LXXIII. Feb. 19th. ---
I had begun the means of fermentation & said that it was a fermentation from the nature of the element; the phenomena that occur, and the experiments that have been made, and I added that none of the experiments quoted to the contrary had any effort. I said that vinous and acelous fermentation takes place from Dr. Ringle's & Mortands experiments. A question may be put whether the vinous always to the acelous, or vice versa in the animal mixture. It is extremely probable that our element undergoes an acelous fermentation, it is the natural course of the vinous to proceed to the acelous. Further, no circumstance in the stomach should prevent the vinous going the length of the acelous. I took off the objections of heat and showed it not incompatible with the vinous, but only...
it does not prove a disease; Many other ob-
ervations shew acidity to be almost con-
stantly present, as appears in rustication
and somnambulism, where it showed it self by no
marked symptoms. Every body will allow
that there is a provision in the economy
itself, for correcting acidity. But these in-
stances are so frequent that they lead to
believe that the acclous fermentation is
constant; you will find these proofs in
Ramsay and every other Physiologist. Be-
cause there is no animal without an
acid. Not to speak of the vulcanus and
acid flavour of the Broth, if there are,
us I believe there are, observations where
the acidity did not, and an alcaline qua-

city did appear; this however is a very rar
92 case; and we must say that the acidity
is produced it is cured again. And there
are few cases that cover acidity and every
matter to the other extreme of allac-
deneity. Therefore the fermentation is uni-
versally and naturally acetic. What
is the use and purpose of it? I think
to answer this it is necessary to adopt
our present consideration, and to consi-
ider the late and present state of the
theory of chylification. After the theory
of menstrua contrived by the chemists
and adopted by the bacterians, had been
rejected by the mechanical physicians,
they substituted another equally excep-
tional. Hence Boerhaave gained just repu-

gation by moderating to the mere theories. He
rejected the acid or alkaline menstrua
of the chemists and bacterians and he
could not wish to perceive that in those
the stomach could be equal to tritura-
tion, and therefore he reduced that to ac-
tation, the alternate action of the De-
phragm or the supposition of the
menstrua which he adopted. But in
both respects he fell short; he went no
further than to account for solution;
the assimilation he did not attend to, &
he rejected our fermentation which did
not escape him at all; he took notice
of the fermentative encephalium, and contro-
led the means by which he said it was child.
but his doctrine does not account for the
change of vegetable to animal substance.
He indeed, on the other hand, considered the vegetable nature not changed, but that it disappeared by the addition of more and more of the animal nature. In his 1207, after carrying the element to the subclavian vein he says "as of imitation of corporis, I say he convinces not the vegetable nature disappears in this way of animal nature. He does not show us the means how it should be grafted to be blended with, or converted by them. In the next year, he shifts the question, pointing out the body contained in these four elements has not in one instance explained the matter. Let the vegetable be but 1000 part of the whole and the rest animal, yet as the animal matter is constantly falling off, the vegetable can be slowly taken in, the animal should lose in time all its animal parts and be entirely vegetable; there is therefore always of quality. Boerhaave, a man of so much judgement, did not misf. seeing this, and endeavoured to account for it by a mode of reasoning in his 1007, which has been vailed universally for 50 years past. "

"ut ducibus... cunt". In the next he says...
of Bodies are performed by separation or combination. There are Bodies unchangeable by any Force in our System; these are atoms or elementary particles. All the other changes are in proportion to the various proportions, and arrangement of these as to one another, and all is performed either by addition or subtraction, all by chemical combination. No mechanical force can break down a mixture; you may by that means break down an aggregate into its integral parts. So in an acid and alkali, you may put off a corner or make a hole in another part. The whole of this Cartesian and mechanical philosophy seems to be extremely ill founded and absurd.
ter. Haller in his Sanguine nature & 29 indecisive has hardly touched the doctrine of liquefaction, but he has spoke of it in 1768 in an ambiguous, undetermined way "Sanguine."

"This he hints at as the mean.

"nunc.

"not a word more; it is changed, but except repeating circulation he "mutatur ita"

"Vilcat." That is point

"ing out effects as so many facts but only not in the least; they happen. Cook at his greater