

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 there are the only circumstances of saponaceous quality. I find this equally a property of every viscid fluid, and of certain degrees 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 certainly that there is either one or other; and indeed there is no

probability of either of them. Thirdly as 81 to the mixing oil and water perfectly there is no foundation for this in experiment. After a variety of trials upon saliva, 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

— Assimilation —
This is not perfected in the stomach but long after, in other parts, tho' it is begun there. To enter upon this I have said that the aliment of animal Bodies is properly vegetable substances. And I have alledged that vegetable substance in its nature and qualities differs

82 from that of animals. The change then
that produces the resemblance, we call
assimilation. Now it is doubtful ~~that~~
that we should begin with explaining this
difference, and enquire 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 oc-
cur in either way. The last is the more
secure method and we therefore prefer it.
Upon this plan then, I say a fermenta-
tion occurs. Here the ferment of the
Chemists and Cartesians, *vis brevis* max
and generally rejected. *Opinionum communis*
la delat dies. But it is universally agreed
that there is a fermentation; it is found

83
first upon the nature of the aliment
which is disposed to fermentation. I
advanced that a saccharine matter is
the chief foundation of our vegetable
aliment. Haller is wrong in rejecting
sugar as one of the condimenta. If the
experiments are true, the number of
men and other animals, that live upon
fruits of trees, figs and dates, where a
saccharine matter is fixed, must certainly
receive their nourishment from sugar.
But further the vegetable substances
which make the chief part of our ali-
ment are the farinacea; now if su-
gar is the basis of the fermentable
matter, it is a proof that the matter of
our aliment is of the saccharine kind.

84 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 inflammation;
the intestine motion of it extricates
the fixed air. And from our Digesting food
of vinous or acedous disposition there is
no doubt of fermentation. Now we can
say that none of the circumstances pre-
venting fermentation have that effect.
and first in this fermentation a degree
of heat has not. In ordinary fermenta-
tion the heat must be somewhat less than
that of animal Bodies. But tho' that be
the case in the fermentation of wine yet

we know that its fermentation will go 85
on, in countries of higher Degrees of heat;
and tho' they be in close vessels, yet if they
have a quantity of air included it is suf-
ficient to carry on the fermentation to
a certain degree. But there is air in
the stomach. I said that air contribu-
ted to solution from its making a gita-
tion to the place; but it is necessary to
extricate the fixed air. A third circum-
stance hindering fermentation was the
mixture of animal substance, which
resists the vinous or acedous. But the
experiments of Pringle and Mcbride
have shown that they bring on and ex-
pedite fermentation. The degree of heat,

86 the air and mixture of animal matter
there, all help fermentation in the sto-
mach. I maintain that it is by a pro-
cess of fermentation that our assimila-
tion takes place. From the general tenden-
cy of animal matters, it has been thought
they run on to the putrefactive forma-
tion. But I say the fermentation is
not directly putrefactive, as that fer-
mentation does not directly take place
without the other two preceding. The
vinous passes to the acetous, and that
to the putrefactive. Most Chemists admit
this, that it can not be determined to the
putrefactive fermentation without going
thro' the acetous, nor into the acetous

without going thro' the vinous. I main- 87
tain that in the human body this is the
case, and in fact we find it so from
experiments out of the body. If we put
them into proper circumstances with
regard to heat and air, we see that they
constantly run their course, of passing
from one fermentation to another. Obser-
vations in the body shew the same thing.
The vinous is perhaps not so evident,
but it is observable; and more certain-
ly is the acetous universally observed.
Most Physiologists are ready to admit
the vinous. Pringle and Mead maintain
the vinous to take place, and it
is established by the production of a sin-

88 galar. vapour, the gas silvestre. Boer-
haave called it the "bestrum fermentum"
It has all the qualities of mephitic air.
We have too many instances of this gas
silvestre produced in the stomach co-
horrificous to animal life. But the mix-
ture of animal fluid modifies the vi-
nous fermentation so that the gas sil-
vestre is prevalent. I rest here then that
either the vinous or acetous fermenta-
tion constantly takes place. A question
remains, to what length the vinous pro-
ceeds, if constantly to the acetous?

— Sect. LXIII. Feb. 9. 19th —

I had begun the means of assimilation &
said that it was a fermentation, from

the nature of the aliment, the phenomena 89
that occur, and the experiments that have
been made; and I added that none of the
experiments quoted to the contrary had any
effect. I said that vinous and acetous fer-
mentation takes place from Dr Pringle's
& Matraids experiments. A question may
be put whether the vinous passes always to
the acetous, or ends in the animal mix-
ture. It is extremely probable that our ali-
ment undergoes an acetous fermentation, &
it is the natural course of the vinous to
proceed to the acetous. Further no circum-
stance in the stomach should prevent the
vinous going the length of the acetous. I took
off the objections of heat and shewed it not
incompatible with the vinous, but only

20 of difficult management, and that it
hence is determined to run to the acetous.
Boerhaave concluded that the heat here
was incompatible with the vinous fer-
mentation. But the air here is compati-
ble with vinous fermentations, and still
more favourable to the acetous. The next
question to be put here is whether the fre-
quent acidity is natural, or always to be
considered as the effects of a morbid state.
undoubtedly it is a morbid state and is the
acetous fermentation either gone ^{to} too high
a degree or subsisted longer than the oeco-
nomy appears to shew. But we conclude that
there are means for covering the acidity
from hence that in innumerable ins-
tances where we know it takes place

it does not prove a disease; Many obser-
vations shew acidity to be almost con-
stantly present, as appears in ruptures
and vomits, where it shewed it self by no
morbid symptom. 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 acetous fermentation is
constant; you will find these proofs in
Haller and every other Physiologist. He
says there is no animal without an
acid. Not to speak of the rucous and
acid flavour of the breath, if there are,
as I believe there are, observations where
the acidity did not, and an alkaline qua-
lity did appear; this however is a very rare

92 case; and we must say tho' the acidity is produced it is covered again: And there are diseases that cover acidity and carry matters to the other extreme of alcali-
cency. Therefore the fermentation is uni-
versally and naturally acutous. What
is the use and purpose of it? I think
to answer this it is necessary to step
our present consideration, and to con-
sider the late and present state of the
theory of chylicification. After the theory
of menstrua contrived by the Chemists
and adopted by the Cartesianians, had been
rejected by the mechanical physicians,
they substituted another equally excep-
tionable. Hence Boerhaave gained just reputa-

tion by moderating both these theories. 93
He rejected the acid or alkaline menstrua
of the Chemists and Cartesianians and he
could not miss to perceive that no force
in the stomach could be equal to tritura-
tion, and therefore he reduced that to agi-
tation, the alternate action of the Dia-
phragm he upon the supposition of the
menstrua which he adopted. But in
both respects he fell short; he went no
farther than to account for solution;
the assimilation he did not attend to, &
he rejected our fermentation which did
not escape him altogether; he took notice
of the fermentescence incipiens, and contrived
the means by which he said it was checked.
but his theory does not account for the
change of vegetable to animal substance.

31. 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 the
120 P. after carrying the element to the
subclavian vein he says "ad assimilatio-
nem corporis" I say he insinuates that
the vegetable nature disappears in this mass
of animal nature. He does not show
us the means how it should be filled to
be blended with, or converted by them.

In the next Par: he shifts the question.
The pointing out the body contained in
these four elements has not in one in-
stance explained the matter. Let the ve-
getable be but 1000th part of the whole, and
the rest animal, yet as the animal matter

is constantly passing off & the vegetable con-
stantly taken in, the animal should lose
in time all its animal parts and be
entirely vegetable; there is therefore a change
of quality. Boerhaave, a man of so much
judgement, did not miss seeing this, and
endeavoured to account for it by a mode
of reasoning in his 400 P. which has pre-
vailed universally for 50 years past.

cc

cc

cc

relinquitur." In the
next he says "ut duobus

cc

cc

cc

cent". In the P.
he urges the same with regard to the

96 heart cc

cc

cc

„. and he concludes „sic
videtur &c.“ The whole conclusion is
that the particles of the Chyle are brought
to be of all the possible sizes; and because
they are brought to be perfectly round,
they are fitted to pass thro' every part of
the body. Forma implies figure, but we
use it for the complex sum any body.
you only learn that it is brought to cer-
tain size in N. 247 cc

cc

cc

cc

accommodatus“ all this
has been adopted by the schools of physio
for 40 or 50 years past. I have shown that
all the changes of the particular qualities

of Bodies are performed by separation &c
and combination. There are Bodies un-
changeable by any power in our system;
These are atoms or elementary particles.
all the other changes are in proportion
to the various proportions, and arrang-
ment of these as to one another, and all
is performed either by addition or subtrac-
tion; all by chemical combination. No
mechanical force can break down a mix-
ture; you may by that means break
down an aggregate into its integral
parts. so in an acid and alkali, you
may rub off a corner or make a hole
in another part. The whole of this Car-
tesian and mechanical philosophy seems
to be extremely ill founded and admissi-

is to be no where. We must seek for the
change in the chemical qualities and in-
quire what is added and subtracted. See
Gaubius's *Coop. P.* "

" *Blanditern creando*" Hear how
he answers it "*non satis constat*"

"

"

"

" *quadrate*". I wish he had
said it would neither apply to fluid nor
solid. he adds "

"

"

"

" *Indid Boerhaave does*
not entirely shift the question ^{of assimilation.} of ~~assimilation~~.
But what he says of it is neither admissi-
ble as means nor does it explain the mat.

ter. Haller in his *Sanguinis natura* & 33
indolis, has hardly touched the doctrine of
sanguification, but he has spoken of it in
N. 1769 in an ambiguous, undetermined
way "*Sanguini*"

"

this he hints at as the means.

" *nunc*"

"

"

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

"

"

"

" *Diluat*". That is point-
ing out effects as so many facts but does
not in the least shew the manner in
which they happen. Look at his greater