

OBSERVATIONS

ON

DR. RUSH'S ENQUIRY

INTO THE ORIGIN OF THE LATE

EPIDEMIC FEVER

IN PHILADELPHIA:

BY MATHEW CAREY.

PHILADELPHIA:

FROM THE PRESS OF THE AUTHOR.

December 14, 1793.

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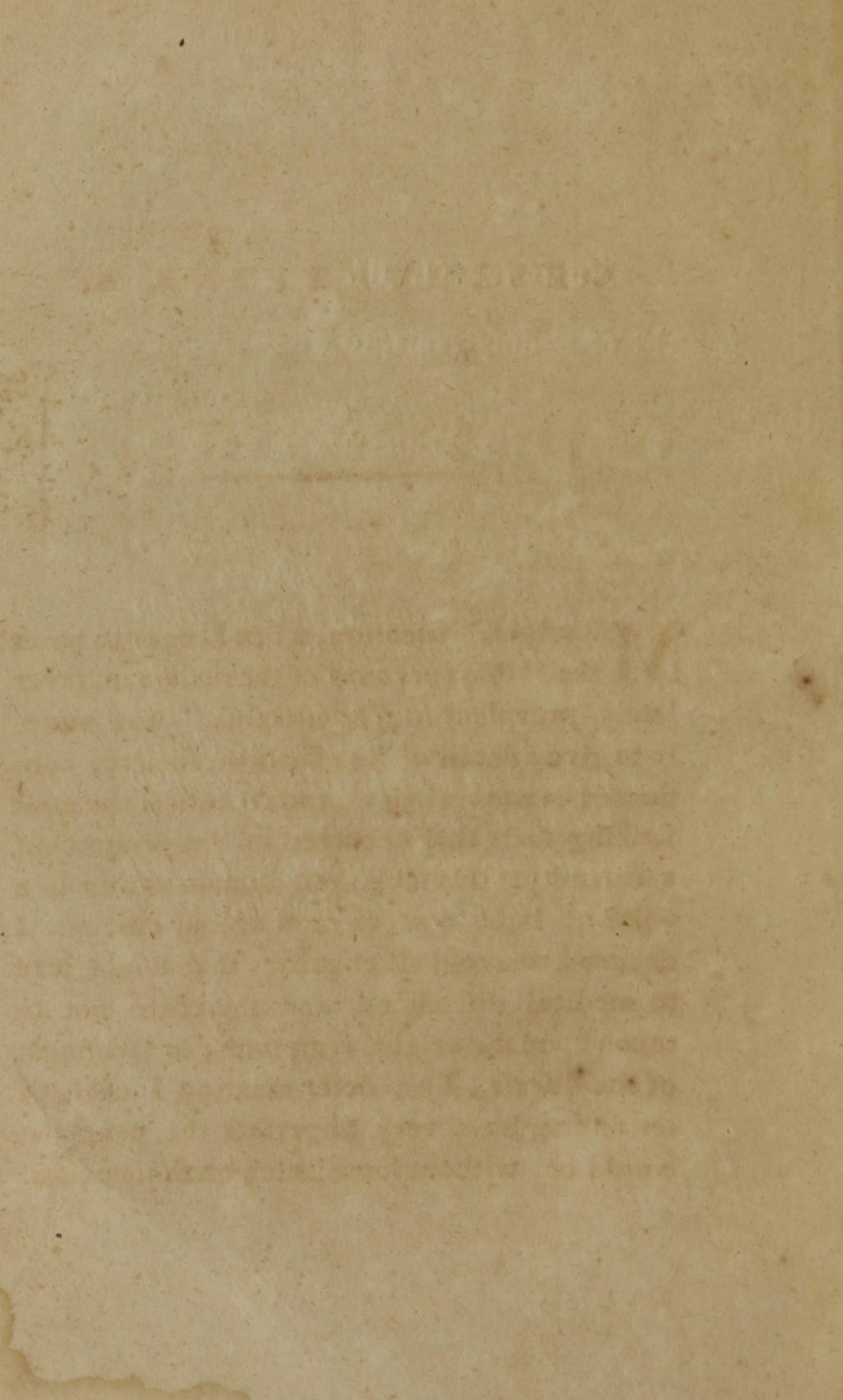
NUMBER XLVIII.

District of Pennsylvania, to wit—

(L. S.) **B**E it remembered, that on the thirteenth day of December, in the eighteenth year of the independence of the united states of America, Mathew Carey, of the said district, hath deposited in this office, the title of a book, the right whereof he claims as author, in the words following, to wit :

“ Observations on dr. Rush’s enquiry into the origin
“ of the late epidemic fever in Philadelphia. By Mathew
“ Carey.” In conformity to the act of the congress of the
“ united states, intituled, “ An act for the encouragement of
“ learning ; by securing the copies of maps, charts,
“ and books, to the authors and proprietors of such
“ copies, during the times therein mentioned.”

SAMUEL CALDWELL, Clerk of
the district of Pennsylvania.



OBSERVATIONS, &c.

MY original intention, when I began to write the “short account of the malignant fever lately prevalent in Philadelphia,” was merely to give a detail of its effects on society, considered in a moral view, and to collect the most striking facts that occurred in the progress of a calamity, that displayed human nature in a point of light new to most of our citizens. I designed to avoid all enquiries that might lead to medical discussions, and therefore not to touch upon the origin, symptoms, or treatment of the disease. This determination I changed on finding how very imperfect the pamphlet would be, without some sketch on those points.

With respect to the origin of the disorder, I was and am in favour of the prevailing opinion, that it was imported. I gave a statement of such arguments on both sides of the question, as had made their appearance in the papers. I was very desirous of being able to introduce those which dr. Rush had long before declared, he could advance in support of his opinion. But in the three editions of my little work which have appeared, it has been out of my power to gratify this desire; as the doctor's essay on the subject made its first appearance on Wednesday last.

As therefore I had no opportunity of animadverting on this essay in the former pamphlet, it will not, I hope, be judged improper to attempt a refutation of the doctor's arguments in a separate publication.

My reason for these preliminary observations, is, to apologize for (what might otherwise appear presumption) my interfering in a question which appears out of my sphere, and to belong exclusively to the gentlemen of the faculty.

The first position laid down by dr. Rush, is, “ that the yellow fever in the West Indies is “ always generated by vegetable putrefaction.” This is not well founded. Dr. Hillary makes no mention of vegetable putrefaction producing this disorder. He ascribes it to the peculiar temperament and habits of living of the subjects of it. “ It is very remarkable,” says he, “ that this fever most commonly seizes strangers, especially those who come from a colder or more temperate climate—and most “ readily those who use vinous or spiritous “ liquors too freely ; and still more readily “ those who labour hard, or use too violent “ exercise, and are at the same time exposed “ to the scorching rays of the sun in the day “ time, and soon after expose themselves too “ suddenly to the cool dews and damp air “ of the night.”* Dr. Lind gives the same account, and nearly in the same words.

Dr. Mosely, who is the latest and the best writer on this fever, makes no mention, as far

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* Hillary on diseases of Barbadoes, page 146.

as I could discover, and I examined his book with some care, of its arising from vegetable putrefaction in the West Indies, nor of any particular season in which it prevails. He speaks of it as one of the seasoning disorders to which new-comers are subject, especially those of a plethoric habit, and those who live irregularly. If it arose from the source to which dr. Rush ascribes it, dr. Mosely would not certainly have neglected to mention the fact.

But even admitting the position, that it arises from vegetable putrefaction, it by no means follows that it was generated here. Indeed, I cannot see that this argument is in any manner whatever applicable to the question.

Dr. Rush's second position is, that "similar
 " degrees of heat are capable of producing it
 " in every part of the world." This is, if possible, more unfounded than the first. Dr. Hillary expressly says: "It does not appear, from the
 " most accurate observations of the variations
 " of the weather, or any difference of the seasons,
 " which I have been able to make for
 " several years past, that this fever is any way

“ caused, or much influenced by them ; for I
 “ have seen it at all times, and in all seasons,
 “ in the coolest, as well as in the hottest time
 “ of the year.”*

To the same purpose, Dr. Lining observes,
 “ This fever does not seem to take its origin
 “ from any particular constitution of the wea-
 “ ther, independent of infectious miasmata, as
 “ dr. Warren has formerly well observed ; for
 “ within these twenty-five years, it has been on-
 “ ly four times epidemical in this town, name-
 “ ly in the autumns of the years 1732, 39, 45,
 “ and 48, though none of those years, (except-
 “ ing that of 1739, whose summer and autumn
 “ were remarkably rainy) were either warmer
 “ or more rainy, (and some of them less so)
 “ than the summers and autumns were in seve-
 “ ral other years, in which we had not one in-
 “ stance of any one seized with this fever ;
 “ which is contrary to what would have hap-
 “ pened, *if particular constitutions of the wea-*
 “ *ther, were productive of it, without infectious*
 “ *miasmata†*”

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* Ibidem.

† Essays and observations, political and literary, vol. II.
 page 406, 407.

The doctor's third division of his essay, contains these facts : " A quantity of damaged coffee was exposed at a time (July 14th) and " in a place (on a wharf, and in a dock) which " highly favoured its putrefaction. Its smell " was highly putrid and offensive ; so that the " inhabitants of the houses in Water and Front " street, which were near it, were obliged to " exclude it by shutting their doors and win- " dows." These circumstances are all true, but they do not answer the purpose intended. Whoever consults the ingenious and interesting experiments made by dr. Alexander, will see that putrid vegetable matters are so far from being the sources of putrid disorders, that they are very powerful antiseptics. As the book is scarce, I shall transcribe his account of one of his curious experiments.

" I reduced a quantity of cabbage leaves to " a pulp in a mortar, and set them in a warm " place to putrify. A bowl of strawberries " was likewise set along with them. When " both the cabbage leaves and strawberries had " undergone a fermentation, and were in that " state in which vegetables are said to be pu-

“ trid, I put a bit of mutton into each of them,
 “ and covered it over with the pulp. Another
 “ bit was put into a bowl of water for a stan-
 “ dard, and they were all set together in a warm
 “ closet. The piece of mutton in the water had
 “ evidently turned putrid, and tinged the water
 “ red in one night. Both that piece which
 “ was in the pulp of the cabbage, and that in
 “ the pulp of the strawberries, were kept three
 “ weeks, and continued sweet and hard, as if
 “ they had been salted. The bowls were then
 “ overturned, and their contents lost by acci-
 “ dent. From these experiments it appears,
 “ that neither the putrid substance, nor the
 “ watry infusion of vegetables, is septic*.”

In this division of his subject, dr. Rush pro-
 duces several instances of malignant disorders
 said to have been generated by putrid vegetable
 substances; and among the rest, quotes Lanci-
 sis. Of this author's want of penetration, dr.
 Alexander makes the following very judicious

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* Alexander's experimental enquiry, 53—4.

observation : “ Whoever reads the observa-
 “ tions of Lancifis with attention, will see that
 “ the greatest part of the putrid epidemics which
 “ he mentions, always followed remarkable
 “ inundations of the Tyber ; and several of
 “ them sooner than the water, left by those
 “ inundations, could have become putrid ;
 “ so that all, or the greater part of the pu-
 “ trid diseases ascribed by Lancifis to marsh
 “ miasmata, may be fairly attributed to the
 “ effect of moisture alone, independent of
 “ any mixture of putrid effluvia.”

The doctor proceeds : “ The rapid progress
 “ of the fever from Water street, and the
 “ courses through which it travelled into
 “ other parts of the city, afford a strong evi-
 “ dence, that it was at first propagated chiefly
 “ by exhalations from the putrid coffee.”

This argument applies with equal force ei-
 ther to the generation or to the importation of
 the disorder ; for as the vessels by which we
 have the strongest reason to believe it was in-
 troduced, were moored near the wharf where
 the coffee lay, it is obvious, that, whether im-

ported or generated, it must have been driven through every adjacent avenue according to the direction in which the wind blew.

The doctor's fifth position is, that “ many persons who had worked, or even visited in the neighbourhood of the exhalation from the coffee, early in the month of August, were indisposed afterwards with sickness, puking, and yellow sweats, long before the air of Water street was so much impregnated with the contagion, as to produce such effects.” This argument stands on precisely the same ground as the former one. For the existence of the disorder in that spot, must have produced the same effect in either case, whether imported or indigenous.

The sixth position is : “ The strictest enquiry, accompanied with the greatest solicitude for proofs, has not been able to discover any other cause of our late epidemic. Every account of the importation of the disease, has been discovered, upon examination, to be inaccurate, contradictory, and without foundation. The first cases of the

“ yellow fever have been clearly traced to
 “ the sailors of the vessel who were first ex-
 “ posed to the effluvia of the coffee. Their
 “ sickness commenced with the day on which
 “ the coffee began to emit its putrid smell.
 “ The disease spread with the increase of the
 “ poisonous exhalations.”

Amidst the confusion in which this subject is involved—amidst the difficulty of producing positive proof*—let us enquire into the evidence. Let us see whether the opinion of its generation be not much more improbable than that of its importation—let us see whether the presumptions in support of the latter opinion be not so extremely strong, as fully to satisfy every mind open to conviction.

That several persons arrived in this city in the course of the summer, sick with the malignant fever, is beyond question. There were,

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* Some of the persons who could throw full light on this subject, are dead—others removed away—and there are some who would not choose to reveal the truth from an unwillingness to engage in any controversy.

early in August, several sick people on board the *Flora*, as appears by the publication of drs. Cuthrie and Cathrall. Many persons have been landed out of vessels coming to this port, at different places on the banks of the Delaware, who afterwards died of the yellow fever; and several dead bodies have been buried along the shore.

About the end of July, mr. John Massey of this city, was at Marcus Hook, where a woman from Cape Francois had been landed, and had died of the yellow fever. Some French people who lived near the place of her death, and knew the disorder of which she died, burned a quantity of tar at the door, to purify the air.

About the same time in July, as this affair happened at Marcus Hook, captain Hamilton Sage, from St. Domingo, was landed in a very low state at Chester, in the evening, and died next morning at four o'clock. His sickness was of only five days. His skin had all the yellowness, and livid spots that characterised our late epidemic. Information of this latter fact I have had from a letter written by dr. William Mar-

tin, to a gentleman of the faculty in this city, who was kind enough to allow me to avail myself of the information it contained.

A single fact, of this kind, would give to the opinion of its importation that high degree of probability, which is nearly all the nature of the case allows ; for the clothes, or bedding of either of these persons would be sufficient to spread the disorder, if we reason from the experience of 1762.

The seventh position of dr. Rush, is : “ It
 “ has been remarked that this fever did not
 “ spread in the country, when carried there by
 “ persons who were infected, and who after-
 “ wards died with it. This, I conceive, was oc-
 “ casioned in part by the contagion being de-
 “ prived of the aid of miasmata from the pu-
 “ trid matter which first produced it in our
 “ city, and in part by its being diluted, and
 “ thereby weakened by the pure air of the
 “ country. During four times in which it pre-
 “ vailed in Charleston, in no one instance, ac-
 “ cording to dr. Liniug, was it propagated in
 “ any other part of the state.” This argument,

by which the doctor doubtless intended to support his hypothesis, if it answers any purpose, militates very strongly against him. The yellow fever of Charleston did not spread into the country—neither did ours. Yet the former was confessedly imported. If, therefore, any inference is to be drawn from this fact, it undubitably is, that ours was likewise imported.

The eighth position of dr. Rush, is : “ It is
 “ very remarkable that in the histories of the
 “ disorder, which have been preserved in this
 “ country, it has *seven* times appeared about
 “ the first or middle of August, and declined,
 “ or ceased about the middle of October—viz.
 “ in 1732, 1739, 1745, and 1748 in Charlestown—
 “ in 1791 in New York, and in 1762
 “ and 1793 in Philadelphia. This frequent
 “ occurrence of the yellow fever at the usual
 “ period of our common bilious remittents,
 “ cannot be ascribed to accidental coincidence,
 “ but must be resolved, in most cases, into the
 “ combination of more active miasmata with
 “ the predisposition of a tropical season.”

Of the seven times in which this disorder appeared in America, five have been unquestion-

ably by importation, viz. four times in Charleston, and the former time here, to say nothing of our late misfortune. These instances must have arisen from an "accidental coincidence;" and could not possibly have depended on "the predisposition of a tropical season." Moreover, the arguments advanced from Hillary and Lining, in page 8 and 9, effectually destroy this position, even if the facts were as stated.

"Several circumstances," continues Dr. Rush, "attended the late epidemic, which do not occur in the West India yellow fever. It affected children, as well as adults, in common with our annual bilious fevers. Dr. Hume tells us, it never attacked any person under puberty."

The yellow fever, even in the West Indies, is not always uniform in its symptoms or effects—and on its introduction here, may very readily be supposed to have changed its appearance. Dr. Lind, who, like Dr. Lining, is unexceptionable authority on this subject, after detailing the arguments for and against the

contagious nature of the disorder, says : “ We
 “ can only reconcile the facts that may be pro-
 “ duced on both sides of the question, by
 “ supposing that the yellow fever of the West
 “ Indies is sometimes of a mild nature, and al-
 “ together free from infection, while at other
 “ times it is more violent and highly infecti-
 “ ous*.”

One more observation of dr. Rush's, it may be proper to notice. He says : “ I am disposed
 “ to believe that the instances of the yellow
 “ fever being imported, are *very few*, compar-
 “ ed with those of its being generated in our
 “ country.” Five times out of seven, as I have already observed, it was confessedly imported—and in the other two instances of its appear-

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* The different appearance of the same disorder, in different persons and places, is noted by Lind : “ From
 “ one cause, from the same infection, I have fre-
 “ quently known to proceed what may be termed, from
 “ outward appearances, the yellow, petechial, and mili-
 “ ary fevers ; and while in a few the contagion assumed
 “ an intermitting form, and was mild, in others it appeared
 “ with a constant fever.” Lind on the health of fear page 265.

ance, viz. at New York, 1791, and here this year, its origination in this country is by no means admitted. In New York, it was attributed to the exhalations from some putrid mud—although the same kind of mud was at other docks, without producing any effect whatever. Surely then it is improper to assert that the instances of its importation are *few*.

I shall now close this subject. The reader has seen that Hillary and Lining declare positively that this disorder does not depend on the weather*—and Lind and Mosely imply the same thing by their silence—yet that dr. Rush declares it arises chiefly, if not wholly from that source—that our disorder assumed the same appearances as in former instances when incontrovertibly imported—that the yellow fever existed in the West Indies for many months before it appeared here—that a very

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* Dr. Bruce, an eminent physician of Barbadoes, quoted by Lind, is of the same opinion: “ In omni anni tem-
 “ pestate, sese effert hic morbus ; symptomata autem gra-
 “ viora observantur, ubi calor magnus cum multa humi-
 “ ditate conjungitur.” Lind on hot climates, 237.

great number of vessels arrived here from that quarter during the summer—that having hardly any idea of danger, we used no precautions, so that it would be more astonishing if we escaped, than it is, that we caught the disease—that many creditable people assure us of sundry sick persons and dead bodies being landed privately from some of those vessels—that people in a very advanced stage of the yellow fever have died at Marcus Hook and Chester, and, for aught we know, at other places—that the ships infected with the effluvia of the sick and dead came freely to our wharves*—that

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* Lind has some remarkable instances of the yellow fever being communicated by infected vessels, even after the removal of the sick. “ When the men from the
 “ Cambridge (one of the healthiest ships then in the fleet)
 “ became infected, their infection did not proceed from
 “ any person being sick on board the Neptune ; for upon
 “ the first complaint, or the appearance of any man be-
 “ ing there taken ill, he was immediately taken to the
 “ hospital. And the same is to be observed, with respect
 “ to the numerous infected patients, who were daily sent
 “ for five or six weeks from the North American ships ;
 “ that is, from their arrival at Spithead till their purifi-
 “ cation in the dock. During this period, no sick man was

there is not the smallest probability of their infected beds, bedding, or clothes, being destroyed, but on the contrary, that we have every reason to suppose they were brought to the city and sold here.—Let every candid man give these several circumstances a full consideration. Let him be actuated solely by a disposition to discover the truth, unbiaſſed by any consideration of the character of our city—let him allow his reason free ſcope—and I feel the utmoſt confidence, that he will pronounce a decided opinion, that the late malignant fever was, in the year 1793, as in 1762, imported from the Weſt Indies.

One word more. Every poſition the doctor has advanced, might be freely admitted, and ſtill they by no means prove that the diſorder was not imported. If it aroſe from the coffee—and that vegetable was, as it muſt have been, in a ſtate of putrefaction on its arrival here, the diſorder was as effectually imported by the

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“ kept for an hour on board any of them, if the weather permitted to ſend him aſhore.”

Amelia, in which the coffee came, as if a sick man had arrived in her, and spread it in the city, as was the case in Charleston, or as if a quantity of infected clothes had introduced it, as happened in 1762.

T H E E N D.

Med. Hist.

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